TYPES, CATEGORIES AND SIGNIFICANCE

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ABSTRACT

In this dissertation, I confront a problem in the philosophy of language from an historical and systematic standpoint. The problem consists in explicating a concept of nonsignificance which can apply, <u>inter alia</u>, to the appraisal of philosophical assertions as category-mistaken or typeabsurd. Such appraisals often take the form:

"To say 'a is F' is nonsignificant, because a is not the

type of thing which can be F or not-F."

Accordingly, the thesis begins in an exmination of the historical and philosophical basis for Russell's theory of <u>logical types</u>, with its concommittant classification of propositions into true, false or nonsignificant.

In Part I, I seek to remedy a failing in past exegeses of the development of Russell's type theory which ignore Russell's demand that his "proper" solution to the paradoxes--the ramified theory of types-- not simply provide a consistent logicist system; but should also be recommended by his other philosophical doctrines. I remedy this failing by showing that:

(i) the source of inconsistency in Frege's logicism lies in his underlying semantic doctrines: complete definition and the treatment of extensions as objects;

(ii) the genesis of Russell's ramified theory lies in his logic, epistemology and theory of meaning--viz: the connections between his Vicious Circle Principle, his Multiple Relation Theory of judgement, and his doctrine of incomplete symbols.

(iii) in particular, the Multiple Relation Theory provided Russell with a foundation for the ramified theory which was undermined when Wittgenstein subjected it to two "paralysing" objections (hitherto, only partly reconstructed), within Wittgenstein's ongoing critique of the logical doctrines of PM. I reconstruct these criticisms and survey, in general, the critical background to the ramified theory resulting in the changes from the first to the second editions of PM.

In concluding Part I, I anticipate the constructive enterprise of Part II in arguing that previous attempts to extend the application of

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type theory to meaningful predication as a whole have often failed through their insensitivity to contextual relativity and linguistic creativity. Nonetheless, I discuss two accounts--Wittgenstein's theory of formal concepts and Ryle's theory of categories--having features which I preserve in Part II. In addition, I argue against construing category-mistakes as ungrammatical or as false.

My general contention through Part II is that category-mistaken significance-failures are best explicated within a theory of linguistic acts (broadly Austinian). I support this contention by considering the circumstances of an utterance failing to yield a statement in context through its failure to express 'content' to an audience. This notion of 'content' is developed by recourse to those techniques of formal semantics which provide an articulation of structural and algebraic features of contexts, utterances and speech-acts in the interaction of which significance is appraised. The interpreted formal languages I develop borrow features from significance and context logics given in Routley and Goddard's <u>The Logic of Significance and Context</u>, (<u>1973</u>); though my approach to the semantics diverges markedly from theirs.

The semantic structures I develop are recommended by their exhibiting systematic relations between utterances, contexts and significance without demanding that category-mistaken predications be diagnosed on the basis of <u>a priori</u> allocations to categories. They represent a category-mistaken predication in terms of a conflict between conditions for successfully talking <u>about</u> items of a type or sort, and for making a statement <u>of</u> such items, in context. Only in this way, it is argued, can a philosophical theory of meaning accommodate fully the richness, creativity and diversity of linguistic acts in context.

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DEDICATIO. (:

A T family reunions (Weddings, funerals, The joint junketings of Christ and Saturn) Blood is so much thinner than whisky and water That consanguinity drinks procrastination, Postponing the ineluctable anacoluthon In the polished Jamesian discourse of Uncle Fred (That prosy participial biped) When he and Uncle Arthur (Literal, inarticulate, A man transposed wholly into the key of A flat) Discover That there are impediments to the marriages of mind Certainly more than kin, as certainly less than kind.

M. Beardsley, Accthetic . 143.

INTRODUCTION

A: Preamble.

It is a typically philosophical ascription to dub another's utterances as 'meaningless' or to appraise sentences as nonsignificant. In this century, a number of philosophers have argued, at different times, that whole classes of sentences are meaningless, nonsignificant, absurd, incoherent or logically odd. It has been claimed, for example, that utterances expressing self-referential, analytic, contradictory, evaluative, and metaphysical propositions lack descriptive or cognitive meaning.¹ These claims have been extensively debated in recent philosophy. Frequently, support for them has been drawn from consideration of a number of examples of which almost any English speaker would judge that if any sentences lack significance, then the following do:

| (1) | This stone is now thinking of Vienna. | (Carnap) |
|------|--|---------------|
| (2) | Quadruplicity drinks procrastination. | (Russell) |
| (3) | Virtue is blue. | (M. Shorter) |
| (4) | My kangaroo is the fifth day of the week. | (Passmore) |
| (5) | Caesar is a prime number. | (Reichenbach) |
| (6) | John frightens sincerity. | (Chomsky) |
| (7) | Saturday is in bed. | (Ryle) |
| (8) | There is a loud smell in the drawing room. | (Strawson) |
| (9) | His fear of flying is three inches to the | |
| | left of his belief in ghosts. | (Strawson) |
| (10) | The number seven is indifferent to tomato | |
| | soup. | (R. Routley). |

These examples, it is supposed, highlight a kind of oddity or anomly which is overtly linguistic, yet which appears quite different from a breach

of grammatical rule or a syntactically ill-formed string. The purpose of the examples--perhaps their only purpose--has been to delimit in a noncontentious manner a species of significance failure or meaninglessness under which the more disputed philosophical cases can be subsumed. That is, a general theory proscribing examples like (1) to (10) as senseless may provide elucidation and justification for the claims of philosophers that less perspicuously nonsignificant utterances exhibit a similar deviance.

The critical intent of this essay is to investigate various accounts of the abnormality of sentences like (1) to (10), to identify the species of absurdity such sentences purport to exemplify, and to appraise arguments which ascribe a similar absurdity to philosophical utterances. In this critique-which occupies Part I of the essay -- my intention is not to decide, in specific cases, whether those several classes of utterances which have been proscribed as senseless warrant the charge. For example, it is not my intention to argue for or against Ayer's claim in Language, Truth and Logic that the propositions of ethics lack descriptive significance. My concern is rather with the prior question of whether there is a distinctive kind of nonsignificance exhibited by utterances to which philosophers may appeal in making such claims. That is, my enquiry is conceptual rather than historical. For, unless just such a generic notion of absurdity can be clearly identified, then one philosopher's rejection of various kinds of philosophical utterance as nonsignificant bears as little weight as his report that he is unable to understand or interpret the utterances of another.

The constructive enterprise--undertaken in Part II of the thesis-seeks to build upon the critique of Part I a general account of the various

kinds of linguistic anomaly into which the central notion of significance failure can be embedded. To accomplish this requires a detailed examination of the logical relationships involved in the analysis of utterance-significance, and in the appraisal of the inferential support for significance claims (claims that utterances succeed or fail to be significant). It is in carrying through this examination of the <u>logic</u> of significance claims that it proves necessary to re-appraise the generic notion of utterance-meaning and; in particular, to assess the extent to which the significance of an utterance is tied to its context of utterance, to the individual physical, social and cognitive circumstances in which it is uttered. For this reason I describe the formal semantic structures introduced in Part II as logics of <u>contextual</u> significance.

Following this theme, it is assumed, therefore, throughout the thesis that communication between individuals by means of speech involves a complex series of <u>actions</u> which take place in a context, against a background environment and on the basis of shared beliefs, experiences and customs. I assume in addition, of course, that verbal communication between individuals requires shared knowledge of a language. I do not, in general, describe what it is for an individual to know a language. To the extent that successful communication involves actions of various kinds, I take it to be evident that some of these acts will be <u>linguistic</u>. That is, an essential constituent of the description of the act of uttering a sentence will be some reference to the language to which the sentence belongs. Similarly, the understanding of that utterance--also an <u>act</u>, of interpretation, however immediate--involves the language of speaker and audience in an essential way. The object which

expresses meaning to an audience and whose meaning is apprehended by an audience is a linguistic object--namely, the sentence. Nevertheless, I offer no systematic answer to the question whether our knowledge of a language should be said to consist in the accumulation of those abilities necessary to the performance of linguistic acts--a species of 'knowing how'-or in the acquisition of facts--a species of 'knowing that'--knowledge of which is revealed in the successful performance of linguistic acts. It is a theme of the thesis, however, to argue that acts of expressing and apprehending meaning are so enmeshed in the complex of circumstances, context and custom that accompanies speech-acts, that to seek a <u>distinctive</u> kind of knowledge comprising linguistic knowledge, alone, amongst the heterogeneous beliefs, experiences, sensations, perceptions and thoughts shared by speaker and audience, may be to misrepresent, through oversimplification, the notion of meaning required.

With this thematic concern in mind, then, it is a first priority to enquire into the general conditions that an account of significance failure and linguistic anomaly has to satisfy. Such an account has not only to avoid oversimplification, but to function successfully in the diagnosis and analysis of significant discourse, also. It has to demarcate between what, in context, and subject to appropriately delineated background conditions, successfully communicates significant content to an audience and what does not. I devote the next section of this introduction to a brief description of these methodological requirements.

In the concluding section of this introduction, I proceed to survey the historical background to the thesis, to identify several of the problems

which the thesis confronts, and to extrapolate to the importance of those problems in the wider context of contemporary logic and philosophy of language. Of necessity, this "setting the scene" is severely condensed--touching only upon those issues which relate most readily to the subject-matter of the thesis, itself. Further extrapolation to the wider implications of the thesis is deferred, naturally, to its concluding sections.

B: Methodological Requirements.

The two most general criteria that an account of the nonsignificance of utterances like (1) to (10)--in any suitably ordinary context--must meet are those of 'explanatory relevance' and of 'recursive application'.

The former criterion of adequacy, though readily admissable, is difficult to state precisely. We have to recognise at outset that a theory explaining the anomalousness of sentences like (1) to (10)--one which captures the sense in which one would ordinarily reject them as meaningless-will not, of itself, account for all kinds of linguistic anomaly. A requirement upon the theory I propose, and upon those I criticise, therefore, will be that they be embeddable, in a natural way, in a systematic account of the ways language can go wrong. For this purpose I introduce in Part II a putative taxonomy of linguistic anomalies into which the nonsignificance of utterances-in-context is to fit. The taxonomy is neither exhaustive nor strikingly original--embracing as it does the classification of objects of linguistic investigation into "syntactic, semantic and pragmatic" components; a schema which originates in C.W. Morris' early works on the theory of signs.

The taxonomy serves only heuristic purposes, whereas the argument required to establish the explanatory relevance of the account I develop occupies a more central role. Indeed, that the account I develop can be related systematically to a generic account of linguistic anomaly, to the questions of typical concern to the philosopher of language, is established only through the course of the thesis. It appears in the discussion of views of precursors of the account I develop, and in the rival theories of nonsignificance I criticise. Nevertheless, some more general remarks upon the task of 'fitting' where possible, the account I develop into a wider framework are not out of place at this point.

Amongst the questions of typical concern to the philosopher of language are such as have to do with what a language is, what sort of description a description of a language should be, and with what relations obtain those notions we classify pre-theoretically as belonging to <u>linguistic</u> knowledge, abilities and experience and those belonging to human knowledge, abilities and experience as a whole. We can take it as given that language speakers have a discriminatory ability to grade utterances in respect of their significance. The behavioural symptoms of a breakdown of communication between speakers--through significance failure--are of interest to the psycho logist, but are not at issue, here. To the extent, however, that it is an acknowledged task for the philosopher of language to describe and explain what is involved in one speaker <u>understanding</u> what another has said, then it is <u>part</u> of that task to investigate the conditions in which the phenomenon of understanding is blocked. In particular, it is part of that task to analyse and elucidate the conceptual connections between the notions of

unintelligibility, meaninglessness and nonsignificance to which we often appeal when understanding is blocked.

I have used for the first time, here, two terms marking an important distinction between the <u>intelligibility</u> of what is said and its <u>significance</u>. I employ the distinction to separate classes of questions prompted by investigation into the breakdown of communication. Of these two classes of question--related as genus to species--I will be concerned almost exclusively with those of the second kind. Certainly, an audience may judge any utterance, by a speaker, in a context, to be <u>unintelligible</u> when, in their judgement, his speech fails to convey his meaning. Yet if his utterance fails to be intelligible, it need not be because his utterance lacks significance; for, he may have mumbled, spoken in a strange dialect or the physical conditions may have been unfortunate. Merely to appraise what another has said (or written) as unintelligible is to signal a breakdown in communication, not to explain it.

Within this generic notion of intelligibility, as I use it here, there are many complex questions which arise in asking how and why an audience judges a speaker's utterance to be intelligible or otherwise. There are questions in the purview of the linguist concerning variations in dialect and idiom; questions for the psychologist concerning behavioural and cognitive cues and signals in verbal acts; questions for the physiologist of speech production and reception and for the ethnologist concerned to identify customs, convention and gesture in speech behaviour. In addition, of central importance to the epistemologist are questions concerning the conceptual status of the notion of intelligibility, its use as a term of appraisal and

as qualifying an object of understanding. Yet we can separate off from this heterogeneous class of questions the more specific subclass of questions which have to do with occasions when what is said is unintelligible through the failure of a speaker's utterance to be significant. That is, as I shall maintain below (Part II, Section E), significance failures comprise that subclass of unintelligible utterances in context which is formed by considering only those occasions when the failure of an audience to understand what is said is directly attributable to some failing of the speaker's utterance, itself, in virtue of which it fails to express a significant proposition. In short, nonsignificance results from a failure to satisfy some essential condition for an utterance to be meaningful in context—in contrast to what may be termed the 'accidental' circumstance of what is said being unintelligible through some extraneous feature of the speaker, context or audience.

It may be objected immediately that to separate intelligibility from significance in this manner is to evade the important question whether nonsignificant utterances may be made intelligible through their expressing something other than their literal meaning. On many occasions, it might be argued, what is said is <u>literally</u> meaningless but is readily intelligible as a result of some other non-literal construal. Typically, metaphors, literary devices, codes and colloquialisms are cited as paradigms of intelligible, nonsignificant discourse.

I accept the objection and examine in some detail, below, whether this evasion is problematic for the analysis of significance failure I propose. Let it suffice, for the moment, to observe that the requirement of explanatory relevance--that an account of significance failure be embedded

in a natural way in a generic account of linguistic anomaly--is invoked primarily to ensure that this heuristic separation of 'significance' from 'intelligibility' does not isolate significance failure as a unique variety of anomaly unrelated to other ways in which language goes wrong.

The second adequacy requirement upon an account of significance failure---that of <u>recursive application</u>, as I shall call it--is prompted by the formal nature of the thesis' subject-matter: language. Any theory of nonsignificance has to yield analyses of a potentially infinite variety of cases. That is, to accommodate the capacity of a language in use to generate indefinitely many syntactically well-formed (i.e. "grammatical") sentences, a general account of utterance-meaning must adopt a 'recursive'--or, as Chomsky has termed it, a 'generative'--framework of description.³ Since there is no reason to suppose the subclass of well-formed sentences which, uttered in context, are meaningless, to be any less numerous, then a theory of significance failure has to be built upon a similar framework.

To make these criteria of adequacy more precise, consider three possible 'models' or frameworks in which we might choose to embed a theory of nonsignificance. I have specified that significance failures are to be classed as a sub-species of the class of unintelligible utterances. Thus, to determine this subclass more precisely, suppose we consider an idealised native speaker of a language and a suitably large, heterogeneous sample of utterances which, in a normal (or rather "unexceptional") context, the native speaker marks as deviant, odd, anomalous, unintelligible, or nonsensical. Our idealisation of the speaker allows us to pare down the size of this class somewhat: first, by discarding all those utterances whose

unintelligibility to the speaker is directly attributable to the speaker's vocabulary deficiencies or ignorance of syntax. That is, we endow the speaker with an impossibly good lexicon and total linguistic competence. Secondly, we discard utterances whose unintelligibility is solely derived from the fact that they are either infinitely long, or because their finite length prohibits our speaker from interpreting them during his lifetime.

Next, we discard members of the class consisting of <u>word-salad</u> utterances--those whose unintelligibility is the product of a violent breach of the syntax of the language; i.e. technically, those whose phrase-marker is not generated in the transformational-generative component of the language.

(This selective procedure, unfortunately, is not entirely unproblematic. There are several discussions--in Chomsky (1965)⁴ and Ziff (1964)⁵-of examples of apparent "word-salad" sentences which, on syntactic grounds, are arguably well-formed. Chomsky's "Colorless green ideas sleep furiously" is one such well-known example. Similarly, Ziff (loc. cit.) has argued that if we espouse the seemingly unobjectionable syntactic principle that expressions of the same syntactic category, having the same syntactic function, are interchangable without loss of grammaticality, then each of the following "gobbledy-gook" strings can be transposed, by substitution of like for like, into idiomatic English:

- (Z1) * It may have were \longrightarrow He may have been.
- (Z2) * Smith although Jones spoke until not Brown ---- > Smith and Jones spoke but not Brown.

Such problems, I believe, stem from an insufficiently articulated notion of syntactic category--one which, with further refinement, would reveal that Ziff's substitution-principle is, on its own, simply false. I do not intend to consider these problematic cases further).

We have pared down the corpus of utterances our idealised speaker finds unintelligible by discarding, in general, those which we can definitely classify as syntactically anomalous. Similarly we can discard as uninteresting utterances whose unintelligibility results from (i) variations in dialect or idiom; (ii) breach of phonological rule; (iii) accidents of morphophonemic context (parapraxis, speech-loss, etc.).

Finally, albeit arbitrarily, we confine attention to that subclass of the remaining utterances which are overtly declarative assertions--at least in respect of syntactic form (NP + VP). Of course, when an utterance is unintelligible to a speaker, he may be unable to identify it as declarative, imperative, optative, hortatory or whatever. This is an unfortunate consequence of the fact that the <u>mood</u> of an assertion is not uniquely determined by its syntactic form. Nevertheless, for the sake of this introductory illustration, I shall gloss over this indeterminacy of mood completely.

What remains, then, of the class of unintelligible utterances? Which of the following schematic diagrams, or 'models', best depicts the kind of account which our idealised speaker might employ to justify marking the remaining utterances as anomalous? Of several possibilities, the following three schematic models seem most likely to fit our intuitive picture of the structural properties of the class of unintelligible, nonsignificant utterances:

<u>I</u>: First, we may be inclined to suppose that there is a continuum along which we can locate declarative utterances which are progressively less and less intelligible to our idealised speaker. That is, we might suppose that there are degrees of diminishing significance with respect to which he may be disinclined to say of <u>any</u> utterance that is grammatical, but anomalous, that it is definitely <u>meaningless</u>, but of which he can judge that it is less understandable than other utterances.⁶ Such a supposition seems best depicted by a model which grades utterances along a line:



In the following, I shall refer to this schema as "model I".

<u>II</u>: An alternative to model I is to suppose that there is a family of intersecting sets of conditions which bear upon the significance of utterances within each of which nonsignificant utterances may be located and which exhibit features similar to sets in their immediate neighbourhood but which bear little or no similarity to remote sets. Such a supposition seems best depicted by the following schematic diagram:



In this schema, nothing is intended by the number and kind of intersecting sets--save that the general picture is one of nonsignificance being the product of several different kinds of anomaly which are nonetheless related. I shall refer to this framework of description as "model II".

III: Finally, a simpler schematism is provided by the supposition that there is some very general feature that all significant utterances share and nonsignificant utterances lack (or <u>vice versa</u>) in virtue of which we can partition the class of utterances into disjoint sets, thus:



Various ways of partitioning subsets within these disjoint sets may be proposed depending upon the manner in which the boundaries between syntactic, semantic and pragmatic anomalies are drawn. The general character of model III, however, is to endorse a theory of nonsignificance as a determinate and relatively permanent attribute of sentences uttered in context.

One can summarise the views which models I to III represent in noticing that:

(i) in model I, "significant" is a grading adjective ranging over utterances which, like "intelligent", ranging over persons, admits degrees of difference, without there being some one decisive test for separating significance from nonsignificance (or intelligence from unintelligence).

(ii) in model II, again, "significant" is an adjective of grading which admits degrees of significance. In this case, though, significance is a generic notion or 'family concept' embracing several qualitatively separable notions which are related by resembling conditions for the significance of what is said. In this respect, "significant" might be said to be like "healthy" in so far as this latter may encompass a group of interrelated qualities (of feeling, physical fitness, mental state, and so on), each of which contributes to the generic concept and is connected to others by resembling conditions for the well-being of a person, yet which are separable, and admit differences of degree.

(iii) in model III, "significant/nonsignificant" are being considered as polar adjectives--like "good/bad", "true/false" with respect to which there is some determinate feature the presence or absence of which, for any object over which the adjective is defined, establishes whether the adjective applies. (This does not exclude, of course, borderline cases, or cases where it may be extraordinarily difficult to discern whether an object possesses or lacks the determinate feature).

In all three models, "significance" is being regarded as a term of appraisal---for, any account of significance failure must admit the evident fact that a significance-claim (a judgement that an utterance is, or fails to be significant) has an evaluative, perhaps even a normative, content. To reject a declarative utterance as nonsignificant or meaningless is to dismiss the meaning it was intended to convey, to disqualify the speaker, albeit momentarily, from the ongoing discourse, and his utterance from making a statement which can be true or false. This is only to insist upon the commonplace that for an utterance to yield a true or false statement, it must express a significant proposition, i.e. say something meaningful. Such appraisals frequently carry connotations of considerable evaluative weight.

It remains to make a preliminary assessment of these three models--I do not claim that they are the only three possible--in the light of the methodological requirements delineated.

In general, the schema we adopt as best fitting the role of significanc claims in diagnosing and analysing linguistic anomaly will reflect the account

we espouse of utterance-meaning and of the semantic structure of a language. If, for example, we opt for model III---in some respects the simplest framework of description--then, presumably, we shall look for some definitional principle to demarcate between significance and nonsignificance. That is, our account of utterance-meaning should yield some definition of the form:

An utterance of a sentence S is <u>nonsignificant</u> if and only if S has (or lacks), on the occasion of its utterance, some property \emptyset ...

Relative to the determinability of \emptyset , then, the presence (or absence--we can always take \emptyset to be the lack of some property) of \emptyset will be necessary for a sentence S to be meaningful, in context; its absence being sufficient to declare S meaningless and, hence, unintelligible. To discover such a property \emptyset , then, would establish an important link between the meaning of a sentence S (the object of a semantic description) and the assertibility of S (that S is pragmatically successful in context). For, having identified \emptyset , should an utterance of S be unintelligible to an audience, in its context, its failure is directly attributable to its having \emptyset --whatever it may be-thus, to S's failure to be significant.

In contrast, if either model I or model II is a more correct schema for the account of significance failure, then it would be inappropriate to look for a <u>definition</u> of 'nonsignificance' of the above form. One has, rather, in the case of model I, to cite conditions justifying the grading of utterances along a continuum of significance--conditions which account for differences in degree of significance, but which need not yield some generic property which all significant utterances possess and nonsignificant utterances lack (or <u>vice versa</u>). Similarly, by adopting model II, one may have to abandon the search for necessary and sufficient conditions for an utterance, in a context, to be significant, and investigate, instead, the functional dependencies between the lower and upper bounds of the intersecting sets of utterance-types envisaged in this schema. (One has to assume, of course, some ordering of differences between adjacent sets can be discerned).

On either a model I or a model II-type account, it does not follow immediately that the link between "being semantically acceptable (<u>qua</u> sentence of the language)" and "being assertible (pragmatically successful)" is lost. Rather, it suggests that the link between semantic and pragmatic may be more complicated than has been thought, hitherto⁷. For, both models suggest that semantic features of the sentence uttered and pragmatic features of context and speech-act intermesh in the diagnosis and analysis of nonsignificance. This difference between model III and model I and II-type accounts is investigated further in the concluding section of this introduction.

In the critical arena of Part I, it is my aim to reject models I and III as inappropriate and inadequate in accounting for anomalies like examples (1) to (10) above. Such anomalies have been called "category-mistakes", "type-crossings", "semantically incoherent sentences" or "type violations", in past discussions of significance failure. For the moment, I shall refer to them as "category-mistakes" and focus upon them as core instances which a theory of significance failure has to explain. Models I, II and III are, of course, extremely general representations of the kind of approach best suited to an explication of category-mistakes. In view of this generality, arguments for or against each approach would be empty unless particularised to fairly representative exponents of the approaches I consider. To this end, the critical enterprise of Part I must fulfill two functions: that of showing that each account under consideration is indeed represented by one of models I to III, and that of showing that some of the criticisms of each account are the product of essentially problematic features of models I and III, themselves, which lead me to reject them. In addition, though I endorse model II and seek, in Part II, to develop a theory of significance failure within this framework, there are numerous pit-falls in that approach which are discussed in Part I in the exegesis of model II-type accounts of categorymistakes. For reference purposes, then, I list here, by author only, the classification of accounts to which I have referred (I have not discussed all of these in detail in the thesis):

| MODEL I | MODEL II | MODEL III |
|--------------|----------------------|----------------------------|
| Ewing (1937) | Ryle (1937-8) | Frege (1893) |
| Prior (1954) | Strawson (1952) | Russell (1908,'18,'40) |
| Quine (1960) | | Wittgenstein (1922) |
| | Goddard (1964,'68) | 3 |
| | R.Routley (1966,'73) | Shorter (1956) |
| | | Pap (1960) |
| | | Passmore (1961) |
| | | Katz (1967) & Fodor (1963) |
| | | Drange (1966) |

So far, I have identified two methodological requirements upon a theory of nonsignificance--those of 'explanatory relevance' and 'recursive application'. Together these set an upper bound to the application of the theory we seek. They do so by setting the theory within a context (that of a generic account of linguistic anomaly) and a closure condition with respect to the subclass of nonsignificant, but syntactically well-formed sentences. The

lower bound to the domain of application of the theory lacks such specificity. It would be rash to suppose that some limit could be set <u>a priori</u> below which all utterances could be classed unintelligible or nonsignificant, without further enquiry. In part, finding such a limit will depend upon whether we take the significance of utterances to be a <u>permanent</u> feature of the sentences uttered, i.e. as uniquely determined by semantic description of those sentences, unencumbered by pragmatic features of context and speech-act. It is argued in the thesis, however, that there is little plausibility in a view which rigidly classifies sentences as either 'meaningful' or 'meaningless'; that sentential significance is not to be construed as an enduring feature of semantic units independent of the occasion of their use.

One detects an immediate tension in making such a claim as the above. On the one hand, one accepts the commonplace that, for an utterance to be intelligible (here, I mean "interpretable in a context"), in addition to its satisfying minimal criteria of grammaticality, it has to consist of expressions of known significance combined in semantically acceptable ways. On the other hand, it is equally a commonplace that sequences of expressions proscribed as non-sentences, on syntactic grounds (not even meeting minimal criteria) are often capable of (non-literal) construal which renders them readily intelligible. The connection, then, between <u>literal</u> significance and intelligibility is, fortunately, not so trivial as equivalence. At most, one might demand a one-way entailment: that everything significant, in some context, is in principle, intelligible to some audience—though even this entailment falls foul of awkward counter-examples involving coded messages or

ineffable subject-matters (c.f. the discussion of Wittgenstein's doctrine of showing in Part I).

This tension is dissipated by the observation that neither of the above commonplaces is true in any simple sense. Utterances without number can deviate from grammatical norms and remain readily understandable. Contrastingly, the most gnomic allusion or distantly esoteric metaphor may only evoke an image when rooted in a surrounding context---heavily parasitic upon familiar usages and associations (though only the most penetrative critical analysis may reveal that root). The point can be made, however, without delving into the linguistically neglected domain of metaphor and non-literal interpretation. To reject the view that significance is a uniquely semantic, permanent feature of sentence-types, it suffices to reflect upon the numerous aspects of utterance-meaning which are, preanalytically, context-dependent; and, as is argued at length, later, remain so despite efforts of paraphrase and translation purporting to <u>free</u> meaning from context. An utterance like:

(11)* I will be angry, yesterday.

exhibits a variety of tense-anomaly which, <u>prima facie</u>, resists classification in terms of context-free semantic deviance. It is possible, of course to provide reductive analyses of token-reflexive operators like tenses which remove their context-sensitivity. Arguments against such efforts, and against other efforts to provide context-free semantic descriptions, are adduced, below. At this point, though, one can raise questions as to the motives behind such efforts. From whence stems the demand of the semanticist for context-free descriptions of the semantic component of a language?⁸ And from what source is the demand of the logician for a wholesale, extensional

reconstruction of the familiar egocentric and context-dependent features of ordinary language derived? In part, one can answer these questions by appeal to the methodological precepts of simplicity and ease of formalisation. On their own, however, such appeals are out of place when the <u>focus</u> of one's attention is upon the contribution of pragmatic features to the significance of utterances. More interesting answers to these questions involve consideration of the historical impetus given to semantic reconstruction and formalisation, by advances in the unrelated fields of mathematical logic and foundational studies in mathematics. These answers will be more carefully considered in the historical survey which concludes this introductory chapter.

I have identified two criteria of adequacy for a theory of the nonsignificance of category-mistakes. Relevance to existing semantic theories and generative applicability are the two most general conditions. Further requirements upon the theory will come to light along the way--to be noted as they arise. That the theory I propose meets them must await the conclusion of the enquiry.

PART I: THE THEORY OF TYPES

Section A: (I) Survey of Background:

The second task of an introduction is to "set the scene" for the critical account of theories of category-mistakes undertaken in Part I. This exposition of theories, itself adopts a historical perspective--tracing the origins of recent theories of nonsignificance in the logical and linguistic doctrines of Frege, Russell and Wittgenstein. It traces, also, the related, but separate development of theories of utterance-meaning and sentential significance in the two dominant schools of recent philosophy of language: the positivist and, subsequently, formal semantic approaches represented by Carnap and Tarski; and the "ordinary language" approach of the Oxford philosophers Ryle, Austin and Strawson.

In twentieth century logical theory, the absence (with certain noted exceptions⁹) of even semi-formal treatments of the logic of context-dependent significance is perhaps surprising when one notes that the most celebrated proponent of a three-valued logic--the 'true/false/meaningless' trichotomy in the type theory of <u>Principia Mathematica</u> (hereafter: <u>P.M.</u>)--is Russell. Yet, Russell's writings have also contained the staunchest advocacy of that conception of logic--derived from the "ideal language view"--which is apparantly least sympathetic to non-bivalent, non-classical formalisations of logic; so our surprise may be unwarranted. The contrast here, though--classical versus non-classical logic--does not match the distinction in Russell between an "ideal" or "logically perfect" artificial language and an ordinary language, replete with philosophically misleading expressions and logically opaque grammatical constructions. It is worth pausing,

therefore, to examine these conceptions of logic, on the one hand, and of language, on the other. For, they have constituted the background and historical source to many different approaches to contemporary problems in the philosophy of language.

One should begin by noting that an "ideal language view"--the doctrine that many problems of philosophy stem from the grammatical imprecision, semantic ambiguity and vagueness of ordinary spoken languages, which can be obviated by reformulation in a logically perspicuous, artificial language capable, in principle, of expressing any cognitively meaningful, fact-stating proposition--originates, probably, only in Frege's introduction of the Begriffschrift (1879) as a logically perspicuous system of notation. The analogous doctrine--which I shall call the "universal language proposal"-has a far longer history, going back, at least, to Descartes' proposal (1644: Principles of Philosophy) for a "mathesis universalis" as a deductive system of reasoning for natural philosophy. This latter doctrine received its fullest exposition in Leibniz' efforts to construct both a "characteristica universalis"--a universal language comprising few primitive symbols in terms of which all other symbols could be defined -- and an "ars combinatoria" for deriving complex concepts by combination of relatively few simple, primitive concepts.¹⁰ The two doctrines arc inter-linked yet separable. There is no implication in the latter that philosophical problems stem from, or, at least, are aggravated by the vagaries of ordinary language. Nevertheless, they became identified--subsequent to Russell's espousal of an ideal language view (1918) -- in the proposal for the unification of logic, mathematics and science, based upon a uniform system of notation and

primitive psychological concepts, which was adopted by the logical positivists of the Vienna Circle (especially Schlick, Carnap and Neurath between 1922 and 1930).

I do not intend to discuss the validity of the "ideal language view" nor the fruitfulness of the "universal language proposal", alone. It is rather my aim to show how the <u>particular</u> claim that classical logic (epitomised in Russell's <u>P.M.</u> formulation) is an appropriate basis for a logically perfect language for philosophical purposes has constituted a pervasive negative influence upon subsequent approaches to problems of meaning and significance.

The claim of classical logic as providing the basis for a logically perfect language can be articulated, in brief, as that conception of the logician's role as one of constructing an 'ideal', artificial language, along the lines of a formal, first-order language (with supplementary nonlogical signs) in which to reconstruct propositions of philosophy, mathematics and natural science:

"the fact that natural languages allow the formation of meaningless sequences of words without violating the rules of grammar, indicates that grammatical syntax is, from a logical point of view, inadequate. If grammatical syntax corresponded exactly to logical syntax, pseudo-statements could not arise. ... In a correctly constructed language ... considerations of grammar would already eliminate (nonsensical sequences of words) as it were automatically. ... It follows that if our thesis that the statements of metaphysics are pseudo-statements is justifiable, then metaphysics could not even be expressed in a logically constructed language. This is the great philosophical importance of the task, which at present occupies the logicians, of building a logical syntax."¹¹

In such a constructed language as Carnap here envisages, all well-formed

formulae (wffs) are recursively defined; sub-wffs are univocally interpreted; the sense and reference of every denoting expression is entirely context-independent; illocutionary force coincides with truth-conditions and meaning is independent of occasion of use. So, it is claimed, all closed wffs of such a language express complete propositions--nonsignificant sentences are excluded on syntactic grounds, as also are empty names, intensional, token-reflexive and attitudinal operators (except such as may be extensionally re-interpreted). Intersubstitutivity <u>salva veritate</u>--hence, Leibniz' Law for the indiscernibility of identicals--holds for the interpretation of all singular terms; whilst 'linguistic' predicates like "wellformed", "derivable", "true", "meaningful", "valid", along with quotation of expressions and denotation postulates (meaning-rules) are relegated to a meta-language in terms of which the logically constructed language is described.

I have specified the claim in its extremest form, above, and it is not clear that, in this form, any particular philosopher or school of philosophy has espoused all aspects of it. There are elements, nonetheless, of just such an extreme claim in several accepted practices of contemporary formal logic--some of which I shall discuss, below.

Historically, Russell's advocacy of at least parts of this claim appears first in his review of MacColl's Logic,¹² where he criticizes MacColl's introduction of "unmeaning" as a separate class of proposition, corresponding to nonsignificant sentences. This introduction, says Russell, "again illustrates the fact that his (MacColl's) system is concerned only with verbal expressions, not with what is expressed. For what is unmeaning

is only a phrase; it is, by no means, nothing, on the contrary, it is a definite form of words. In logic, we ought to adopt such a language, and such rules for its employment that unmeaning phrases shall not occur." (Russell, <u>1906</u>, p. 253). This early attitude appears subsequently as a much more elaborate doctrine of Logical Atomism (Russell, <u>1918</u>; and Wittgenstein, <u>1921</u>). Here, a suitably augmented version of the logic of <u>Principia</u> <u>Mathematica</u> is entertained as a "logically perfect language" whose rules of syntax will prevent nonsense:

"(Wittgenstein) is concerned with the conditions for accurate Symbolism, i.e. for Symbolism in which a sentence 'means' something quite definite. In practice, language is always more or less vague, so that what we assert is never quite precise. Thus, logic has two problems to deal with in regard to Symbolism: (1) the conditions for sense rather than nonsense in combinations of symbols; (2) the conditions for uniqueness of meaning or reference in symbols or combinations of symbols. A logically perfect language has rules of syntax which prevent nonsense, and has single symbols which always have a definite and unique meaning...not that any language is logically perfect, or that we believe ourselves capable, here and now, of constructing a logically perfect language, but that the whole function of language is to have meaning, and it only fulfils this function in proportion as it approaches to the ideal language which we postulate." (Russell's Introduction to Wittgenstein, 1921, p. x).

Russell is commenting here upon Wittgenstein's Tractarian doctrine that philosophy "is a 'critique of language'" (Wittgenstein, <u>1921</u>, <u>Tractatus</u> <u>Logico-Philosophicus--hereafter T.---4.0031</u>). That is, to the extent that language disguises the proper logical form of meaningful propositions and, hence, generates philosophical problems through our misunderstanding of the logic underlying language, so the philosopher's enterprise is to reveal, by analysis, this underlying logic: "Most of the propositions and questions to be found in philosophical works are not false but nonsensical.... (they) arise from our failure to understand the logic of our language." (T. 4.003)

It is nonetheless a subtle and highly debatable point whether Russell is correct to impute to Wittgenstein the view that the practice of philosophy should be to postulate an ideal language to which philosophers' analyses and reconstructions of philosophical assertions should aspire. In this connection, it is intriguing to draw attention to the final sentence of Russell's comment upon Wittgenstein, quoted above; for it appears to <u>deny</u> the claim of the classical logic of <u>P.M.</u> to be the basis of a logically perfect language ("...not that any language is logically perfect...")--a claim that Russell himself apparently did not make, though it has been attributed to him (see, for example, Urmson, <u>Philosophical Analysis</u>, Oxford: Clarendon Press, 1956, p. 97).

Leaving on one side this historical question, I am more concerned to show how several accepted practices in contemporary logical investigations stem from aspects of the ideal language claim. In general, the practices I discuss are examples of <u>idealisations</u> which have the effect of divorcing the formal reconstruction or reformulation of a philosophical problem from its source in thinking and reasoning in ordinary language. For the moment, all I shall be concerned to do is to indicate how the practices considered embody an idealisation. Fuller discussion of the effect the practice has upon logical investigations into meaning and significance is deferred until the exposition of particular theories of significance in Part I. For the sake of clarity, though, I rehearse, first, the main steps in the argument
I am to develop through Part I against the influence of the ideal language

view:

(a) that, contemporary with the rapid development of mathematical logic in the period 1879-1930, three of the leading contributors to this development--Frege, Russell and Wittgenstein--espoused some form of the ideal language view, described above, as a theory of the source of (some) philosophical problems and a proposal for their solution.

(b) that, subsequent to formulations of the symbolic languages of <u>P.M.</u> and independent of their use in Frege and Russell's logicist philosophy of mathematics, the logical positivists of the Vienna Circle adopted the ideal language view, supplemented by the claim for classical logic--viewed as a modified version of Frege and Russell's systems--to be the basis for a <u>universal</u> language for science and philosophy, and to be instrumental in the elimination of metaphysical and non-empirical propositions from science and philosophy.

(c) that, despite the failure of the positivist's elimination of metaphysics and unification of the sciences and philosophy, the practice of 'reconstructing' philosophical problems in formal languages--especially those central to the philosophy of language: problems of meaning, truth, reference, and necessity--remained influential in the works of the natural successors to positivism: the logical empiricists (Sellars, Feigl, Quine, Goodman).

(d) that the influence of the vestiges of the ideal language view was to make the fact of being able to fit theories of meaning, truth, reference and necessity to existing systems of classical logic an adequacy requirement upon such theories. This requirement disinclined logicians from considering theories based upon alternatives to the classical Fregean and Russellian systems with their supplementary formal semantic apparatus supplied by Tarski and Carnap.

(e) that, in addition, positivist arguments (especially those of Carnap) to the effect that acceptance of one or another framework of formal logic has no philosophical implications outside the formal interpretation of the symbols of the logic--save for the pragmatic considerations of efficiency and simplicity--have undermined attempts to argue against existing practices of formalisation from the basis of the epistemological and conceptual demands of ordinary language investigations.

(f) that the historical source of several contemporary practices of idealisation in using formal logic in the formulation of theories of meaning, truth, and so on, no longer justifies these practices. In particular, arguments against revising classical logic or introducing alternative formal frameworks which are grounded solely upon pragmatic considerations--that, for example, such revisions or alternates are unnecessary for logical theory, conceived as the enterprise of providing a logical foundation for the sciences and mathematics--are based upon a misconception of the utility of formal logic and of its relation to natural language. (I give two illustrations of such arguments, below).

(g) that, finally, the divergence between formal and natural languages which is encouraged by overemphasising the role of idealisation and simplification of the subject-matter of logical theory serves only to diminish the relevance of formal logic and semantics to the traditional problems of philosophy. Thus, it becomes crucial to re-appraise those idealisations and to reject those which inhibit the formalisation of theories motivated by efforts to remove this divergence of focus.

Having drafted the main steps of this thematic argument of Part I, I turn to the specific practices which I claim are illustrative of the influence of the ideal language view. It is a common practice amongst logicians to adopt a convention or stipulation to avoid the problems which arise in interpreting formally (assigning truth-values to) sentences whose subject terms fail to refer to an existing object, or whose subject terms refer to an object outside the accepted range of significance of the predicate of a sentence. This practice derives originally from Frege's doctrine of complete definition--the doctrine that predicates should be defined (be assigned values) for all arguments in the domain of interpretation of a language.¹³ As is discussed in detail in Part I (Section B), Frege was concerned to give positive arguments for this doctrine; though, in the main, his arguments are nowadays ignored. The practice of modern formal logic gives little save an ad hoc justification for either adopting a stipulation that sentences with vacuous subject terms are uniformly assigned the value 'False', or so interpreting such subject terms (including those falling outside the accepted range of significance of a predicate) that they are assigned the null-class as designation. The ad hoc character of such

stipulations is most clearly revealed in the custom of referring to the results of applying either stipulation as "don't care" values of the interpretation. Thus, for example, the formal interpretation of the sentences:

(12) Pegasus is a winged horse.

(13) Julius Caesar is a prime number.

would make (12) false (through the stipulation that vacuous singular terms refer to the null-class) and (13) also false (through the similar stipulation to regard "x is a prime number" as arbitrarily defined over all objects in the domain of interpretation--merely yielding a false statement for nonnumerical argument expressions like "Julius Caesar").

A vivid illustration of how this practice is sustained by the historical influence of the ideal language view is provided by Quine in his critical review of Strawson's <u>Introduction to Logical Theory</u>, (New York: 1952):

"There is a recurrent notion among philosophers that a predicate can be significantly denied only of things that are somehow homogeneous in point of category with the things to which the predicate applies; or that the complement of a class comprises just those things, other than members of a class, which are somehow of the same category as members of the class. This point of view turns up on pages 6, 112, and elsewhere (of Strawson, 1952). It is part and parcel of this doctrine that "This stone is thinking of Vienna" (Carnap's example) is meaningless rather than false. This attitude is no doubt encouraged by Russell's theory of types... It is well, in opposition to this attitude, to note three points: the obscurity of the notion of category involved, the needlessness of any such strictures on negation and complement, and the considerable theoretical simplifications that are gained by lifting such bans." (Quine, "Mr. Strawson on Logical Theory", Mind, vol. 63, 1952, p. 450, my emphasis).

The passage is vivid in its illustration of the practice of idealisation in

the following respects: Quine identifies, first, a traditional philosophical problem--our conviction that "This stone is thinking about Vienna" is somehow senseless, because category confus.d, and, hence, different from an ordinary falsehood. Having identified the problem, however, Quine notes its difficulty ("...the obscurity of the notion of category involved...") and appeals immediately to <u>pragmatic</u> reasons for avoiding the problem in formulating an adequate logical theory. These reasons are:

(a) that <u>classical</u> truth-functional negation (that a negated sentence always be assigned the opposite truth-value to the unnegated sentence) leaves no room for a <u>denial</u> of significance (i.e. 'not-true' is simply 'false', and not 'false or nonsignificant');

(b) that the conventional formal practice of interpreting a (oneplace) predicate by assigning it a sub-class of objects in the domain of interpretation requires negation and set-theoretic complementation (<u>a</u> is in the complement of a class X iff <u>a</u> is <u>not</u> in X) to be semantically correlated;

(c) that reasons (a) and (b), together, show that <u>revision</u> of classical practices of formalisation to accommodate the traditional problems involving nonsignificant sentences or referential failures is 'needless';

(d) and that to consider alternatives to the classical conventions would lose the "considerable theoretical simplifications" of adopting (a) and (b).

In sum, then, Quine's criticisms of Strawson's advocacy of proper consideration of this traditional problem in logical theory are that (i) it complicates the classical formalisation of logic and semantics; and (ii) such complications are needless and pragmatically objectionable when, ideally, a way can be found of <u>avoiding</u> the problem for the purposes of the logician's enterprise. What Quine takes 'the logician's enterprise' to be can be clearly discerned in a second illustration of the influence of the ideal language view, from the same source (loc. cit. p. 447-8):

"Actually the formal logician's job ... may be schematised as follows. To begin with let us picture formal logic as one phase of the activity of a hypothetical individual who is also physicist, mathematician, et al. Now this overdrawn individual is interested in ordinary language, let us suppose, only as a means of getting on with physics, mathematics, and the rest of science; and he is happy to depart from ordinary language whenever he finds a more convenient device of extraordinary language which is equally adequate to his need of the moment in formulating and developing his physics, mathematics and the like. ... He makes... shifts (in departing from ordinary language) with a view to streamlining his scientific work, maximising his algorithmic facility, and maximising his understanding of what he is doing. He does not care how inadequate his logical notation is as a reflexion of the vernacular, as long as it can be made to serve all the particular needs for which he, in his scientific programme, would have otherwise to depend on that part of the vernacular."

Such a conception of the logician's enterprise can only be sustained by a conception of philosophy which is derived almost exactly from the original positivist's programme for the unification and formalisation of science and mathematics. This, indeed, is the conception of philosophy to which Quine avers almost immediately (loc. cit. p. 448):

"Philosophy is in large part concerned with the theoretical, non-genetic underpinnings of scientific theory; with what science could get along with, could be reconstructed by means of, as distinct from what science has historically made use of. If certain problems of ontology, say, or modality, or causality, or contrary-to-fact conditionals, which arise in ordinary language, turn out not to arise in science as reconstituted with the help of formal logic, then those philosophical problems have in an important sense been solved: they have been shown not to be implicated in any necessary foundation of science. Such solutions are good to just the extent that (a) philosophy of science is philosophy enough and (b) the refashioned logical underpinnings of science do not engender new philosophical problems of their own."

To the extent that Part I takes exception to the treatment of the philosophical problems of meaning and significance as corollaries to the philosophy of science, it is argued there that to conceive philosophy thus is to misconceive the utility and value of logical investigations in relation to philosophy.

Quine is a current exponent of the logical empiricist's inheritance of an ideal language view. Tracing the influence of the view backwards through time, one comes upon a more severe advocacy of the view in the semantic theories of Tarski. The practice of 'idealisation', for the sake of preserving classical logic, is given theoretical support in Tarski's Wahreitsbegriff¹⁴ in his refusal to countenance, as part of the syntax of a formal language, a syntactic device which is a natural constituent of the syntactic apparatus of ordinary language: namely, quotation-functions or operators which enable us to employ expressions (with quote-marks) to denote (other) expressions of a language. His objections to such syntactic operators are two-fold: the first is a technical objection that the use of quotation-functions (together with other assumptions) generates an inconsistency in ordinary language -- this technical objection is discussed in detail in Part II, Section C. His second objection is more general, and stems directly from an espousal of classical syntactic canons as 'ideal' laws:

"I should like to draw attention, in passing, to other dangers to which the consistent use of...quotation marks exposes us, namely to the ambiguity of certain expressions. ... Further, I would point out the necessity of admitting certain linguistic constructions whose agreement with the fundamental laws of syntax is at least doubtful, e.g. meaningful expressions which contain meaningless expressions as syntactical parts. ... "14

What is being argued, here, is that, if a given expression is meaningless

(e.g. 'mimsy'), then it is doubtful whether the appearance of that expression in the language, even inside a quotation-expression which mentions it, (e.g. "'mimsy'"), is in agreement with the "fundamental laws of syntax". But what are the 'fundamental laws of syntax' to which Tarski appeals, and what determines whether an expression is in agreement with them?

It is certainly demonstrable (indeed, Tarski, himself demonstrates it--loc. cit. pp. 161-2) that if quotation-functions, interpreted in one way, are employed in a classical formalisation of logic which embraces a basic principle of unrestricted substitution of co-referential expressions, then an inconsistency similar to the Liar Paradox results. Yet, since quotation of expressions--including meaningless expressions--occurs readily in ordinary language (especially in such sentences as report the meaninglessness of an expression, e.g. "'mimsy' is meaningless."), then such 'fundamental laws of syntax' cannot be syntactic principles of ordinary language. Tarski's inference from the proof of an inconsistency in the use of quotation, interpreted in one way, in a formal language, is to declare ordinary language inconsistent. Notice, however, how peculiar this inference is: it proceeds by way of a conflict between a natural usage of quotation in ordinary language and its use in a classical formalisation of logic (together with auxiliary assumptions) to conclude that ordinary language is inconsistent, and a formalised alternative is necessary, if classical logic is to be consistently formulated.

In appealing to a disagreement with 'fundamental laws of syntax', then, Tarski's appeal is to some (unspecified) ideal language whose recursively characterised set of formulae excludes meaningless combinations of

expressions at outset, on syntactic grounds. It remains mysterious, however, how such a language is to justify using <u>syntactic</u> canons which exclude meaningless sentences. For, our initial intuition must be to suppose that a sentence is meaningless because it fails to convey any meaning. And that-except for the most obvious cases of "word-salad" strings--one would suppose to be the product of violations of semantic, and not syntactic, canons.

Whatever we make of Tarski's appeal to laws of syntax, his stronger claim--that <u>formal</u> languages which contain quotation-functions are liable to inconsistency--is simply <u>too</u> strong. For, it is not true, in general, that formal languages with a quotation-function are automatically inconsistent (see, for example, Smullyan's system S_p in Smullyan, <u>J.S.L</u>. 1957; and the system L_{qu} developed below in **II**, Appendix (B)). In addition, it is not true of sentences of such formal languages which contain (quoted) nonsignificant expressions that they are themselves nonsignificant. Whence, then, derives the 'fundamental' character of Tarskian syntax, if not from a prejudice to preserve classical syntactic principles?

I have concentrated upon two illustrations of the negative influence of the positivists' claim for classical logic to provide the basis for a logically perfect language: the practice of assigning "don't care" values in formally interpreting a logic, and the arbitrary exclusion from formal languages of natural syntactic features, like quotation, because they conflict with classically accepted syntactic principles (in this case, of substitution). There are certainly further examples of practices which illustrate the extent of this historical influence. I do not propose to discuss them in detail because they are not germane to issues which arise in the

thesis, proper. I shall restrict my comments, here, to a passing mention.

Firstly, some controversy arises over how permissable are the logicians idealised versions of syncategorematic expressions of ordinary language--the syntactic connectives and operators: "and", "or", "not", "if... then", "some", "all", "the". It is conceded in most elementary logic texts that the truth-functional re-interpretation of these expressions, and the standard Fregean construal of quantifiers and variable-binding operators, is a distortion, more or less, of their customary usage in everyday contexts. Here, one supposes, the demands of methodological simplicity and economy of symbolism override considerations of the fidelity of formalisation to ordinary usage. Yet, the idealisation is carried through despite the lack of clear criteria demarcating between what is and what is not a logical operator. This lack of clear criteria becomes problematic precisely at that point where elementary logic is supplemented by (i) the introduction of identity "=" which is not obviously a syncategorematic expression, nor does it apparently stand for a relation (between what objects?), and (ii) the introduction of the epsilon 'E' for set-membership which brings with it a wholly abstract domain of interpretation for formal logic (variables ranging not only over unspecified objects, but also over sets of, and sets of sets of objects, and so on).

There has been, perhaps until recent logical studies, less controversy over the predilection of logicians to treat sentences, rather than propositions or statements as the bearers of meaning and truth-values. Whilst the focus of formal logic has been upon mathematics and the relatively permanent, context-free assertions of natural science, there has been little

reason to differentiate between a <u>sentence</u> (which, one supposes, is of a type, a token of which is uttered in a particular context, as a specific speech act), a <u>statement</u> (which, one supposes, is what the declarative utterance of a sentence in a context yields) and a <u>proposition</u> (which is what a sentential utterance expresses). All three can, for ease of formalisation, be treated as the same--the substitution-class of sentential variables in a classical formulation of sentential (or "propositional") logic. The differences between bearers of truth-values, bearers of meaning and bearers of grammaticality become more crucial, however, when the focus of one's logic shifts to the problems of meaning and significance.

In the semantic investigations of contemporary logical empiricists--Quine, Davidson, Kripke, for example¹⁵--the practice remains a convenience, however,--occasionally disclaimed in a footnote¹⁶--to retain the classical identification of truth-bearers with meaning-bearers and conflate both with the objects of syntactic description. This remains a convenience despite the evident problems which arise in regarding contextually-sensitive sentences as truth-bearers. For example, one and the same sentence "I am hot", uttered on different occasions, can be at one time true, and, at another, false (bearing in mind that, <u>qua</u> syntactic object, reference to a sentence is exhausted by mention of its concatenated syntactic parts, e.g. letters and spaces, together with specification of the language concerned). Of itself, this convenient practice of idealisation remains innocuous except when it comes into conflict with important distinctions to be drawn at the level of logical principles. It cannot easily accommodate, in a classical formalism, for example, the necessary separation of the semantic principle of bivalency (that every statement, <u>qua</u> truth-bearer, is either true or false) from the syntactic principle of univocal negation (that every sentence has a unique negation, which is true just in case the sentence is not). It is at least conceivable that one of these two logical principles should be abandoned as false in some formulations of sentential logic--yet this requires complicated adjustments to the classical, Tarski-style semantics for sentential logic if the traditional conflation of sentences with statements is maintained. The ensuing complications are discussed in more detail in Part II, Section B.

I have concluded here the illustration of the negative influence of the historical developments following the ideal language view. It is intended only to provide the background to more detailed argument against this influence, throughout Part I, where my concern will be with its effect upon theories of category-mistakes and type-violations. It remains, however, to consider the historical background to the formulation of those theories, and to note the gradual shift in focus in formal semantic investigation away from its roots in the foundations of mathematics and towards its current attack upon the perennial problems in the philosophy of language.

In the history of logical theory the primary occasion when a need to consider 'nonsignificance' or 'meaninglessness' as a value of some formulae which are otherwise syntactically impeccable arises as a result of a technical difficulty in the foundations of mathematics. The appearance of paradoxes in the logical and set-theoretical analyses of mathematical concepts threatened the enterprise of Russell's logicist programme. Indeed, it is only because Russell espoused the logicist conception of mathematics--

that logic, construed as including set theory, and mathematics are identical in respect of conceptual content and deductive power--that Russell came to regard his solution to the technical threat of the paradoxes--type theory-as a contribution to logical theory.

This historical circumstance constitutes the starting-point for Part I of the thesis. There, I seek to explain the relationship between type theory as it appears in P.M. and has appeared in subsequent technical works on the foundations of mathematics by logicists, and type theory as a philosophical doctrine having to do with predication, propositional significance and meaning. It is argued early on that the historical connection between these two aspects of type theory lies in the general Russellian conception of propositions and propositional form, and in the particular notion of 'impredicativity' which Russell develops in appealing to the vicious-circle principle. This "Vicious Circle Principle"--hereafter VCP--was the name given to the principle advocated originally by H. Poincaré as responsible for the paradoxes of logic and set theory, and given various formulations by him and by Russell to the effect that any specification or definition of a totality (a class or the domain of values of a propositional function) in terms of elements which are themselves only definable by reference to the totality itself is 'impredicacive' because viciously circular. The connection between 'impredicative' definitions and type theory was for Russell the restriction of propositional functions to those which satisfied restrictions as to the "type" of their arguments, and, hence, involved totalities none of whose elements could only be determined or specified by reference to the totality of arguments itself. The immediate corollary to such restrictions

upon impredicative totalities was to confine the arguments to a propositional function to those of which the function could, in some sense, be significantly asserted or denied.

Both aspects of type theory have been assailed recently, together with their associated grounds--impredicativity and VCP. Thus, Myhill has concluded a recent article that:

"The Ramified <u>P.M.</u> (theory of types)...apparently does not correspond to any coherent philosophy of mathematics, certainly not to any philosophy that makes mathematics possible." (Myhill, <u>1974</u>, p. 27).

whilst Ramsey, Quine and Gödel have each argued that VCP is false and, hence, the notion of 'impredicativity' based upon it spurious. Though it is argued in Part I that some of these objections are either misconceived or depend upon upon a misinterpretation of Russell's views--one which, in the case of Ramsey, Russell himself endorsed!--it is not my concern, in general, to vindicate Russellian type theory as a philosophy of mathematics in the form in which Russell presents it. I shall concentrate, instead, upon the use made of type theory in providing the base-structure for a theory of the nonsignificance of category-mistakes and as a model of significant predication outside particular formal theories of mathematics.

The second aspect of type theory--as a philosophical theory of significant predication--develops out of type theory as a solution to the paradoxes of logic and set theory and a resolution of the notion of 'impredicativity'. There is, thus, a gradual move in Russell's thought from a limited appeal to nonsignificance in terms of a formal theory designed to eliminate contradictions, to a more general, philosophical appeal designed to eliminate spurious propositions outside mathematics. A summary of this enlargement of the scope of type theory introduces issues which occupy the latter sections of Part I.

An essential step in the logicist redefinition of mathematical concepts in terms of purely logical concepts is the identification of numbers with classes of a certain kind. But, it is the notion of a class that the Russellian paradox threatens first. It is only through Russell's introduction of classes by means of propositional functions--not only in the P.M. contextual definition of classes in terms of propositional functions, but earlier in the Principles (1903) introduction of classes as the extensions of class-concepts--that the paradox threatens an account of predication. The paradox seems to entail that there is an inconsistency in the notion of a class that infects the notion of a 'predicable', derivatively. Russell's eventual solution was to claim that the argument leading to the paradoxical conclusion contains a meaningless premise: thus, "no class can be significantly said to be or not to be a member of itself"¹⁷. Since the argument of the paradox requires us to consider the class comprising all classes which are not members of themselves, and infers that if this class is a member of itself, then it is not self-membered, and if it is not self-membered then it is a member of itself, then, when the original supposition is declared meaningless, the argument can no longer be significantly stated and the paradoxical conclusion evaporates.

This claim is supported by the technical development of type theory which, first in the <u>Principles</u> (1903), divides individuals and classes into a heirarchy of types. Heterotypical predication (or heterotypical membership conditions)--when arguments to a propositional function (members of a class) are from the same or higher type than the propositional function (class) itself--are ruled nonsignificant. From this it follows that assertions of self-membership and predications of a predicable of itself are meaningless. Consequently, a type theory requires that we proscribe as meaningless certain syntactically impeccable sentences (impeccable, that is, in the non-type-structured syntax of <u>P.M.</u>), which appear to be significant and, thus, appear capable of being true or false. So, from its inception, type theory conflicts with the classical view that every syntactically well-formed sentence of a formal language expresses a true or false proposition.

Now, the formal treatment of types in <u>P.M.</u> is ambiguous. It is nowhere clear whether type-differences are features of individuals, properties and classes, or of the expressions standing for these. This is not the only difficulty in the <u>P.M.</u> conception of types. There are several others: some involving the reconstruction of mathematics from a type theoretical base logic, and some involving Russell's appeal to the "range of significance" of a propositional function. These difficulties loom large in the critical enterprise of Part I. In spite of them, however, the thrust of type theory seems to ensure that nonsignificant sentences could be systematically excluded from logic and mathematics. This, in turn, suggested to Russell, and to his pupil Wittgenstein, a much more general theory of nonsignificance.

Suppose, as Russell did, that the whole of mathematics can be derived from primitive assumptions belonging to logic; and that the formal language of P.M. constitutes a symbolism adequate for the expression of these

assumptions and of rules of deduction necessary to derive the theorems of logic and mathematics therefrom. It seem reasonable to Russell to suppose that this formal language could be supplemented by non-logical (descriptive) terms and operators to embrace the language of the natural sciences and, thereby, all significant descriptive discourse. So Russell conceived, as had Frege before him with respect to the Begriffschrift (1879), that the formalism of P.M. with an augmented vocabulary would approximate towards a logically perfect language. It is at this point -- in Russell and Wittgenstein's development of the philosophy of logical atomism between 1913 and 1918--that the move from construing type theory as limited to the problem of the paradoxes in the foundations of mathematics to a more general doctrine of significant predication in language as a whole takes place. Contemporary with this move in Russell's thought is the espousal of the ideal language view discussed above. That is, Russell came to conceive of the augmented formalism of P.M. with the theory of types as forming the basis of a highly structured, logically perspicuous form of expression in which everything descriptively significant could be expressed and what could not be expressed was excluded as nonsignificant. This conception of the logician's role as one of revealing in the analysis of philosophical and scientific propositions their proper logical form lies behind Russell's espousal of the doctrines of logical atomism. And it is within logical atomism that the theory of types -- as a theory of significant predication and, ultimately, as a determination of the limits of descriptive significance--comes to the fore.

In the enlarged programme of analysis and reduction of philosophical

and scientific propositions proposed by logical atomism, the limited type theory of P.M. was insufficient as a general theory of significant predication. A more comprehensive account of meaning and significance had to be devised. Russell, in his 1918 Lectures on the Philosophy of Logical Atomism, simply took over the Picture Theory of Meaning of Wittgenstein, modifying it somewhat to accommodate an epistemology peculiar to Russell's own atomistic ontology. The picture theory, very briefly, required that a fully analysed proposition be meaningful if its logical structure pictures the structure of a fact (expresses agreement or disagreement with states of affairs). In this respect, as proposed by Wittgenstein, type theory becomes a special case of the picture theory. Since the logical form or structure of a proposition which violates type-rules does not embody a possible form of fact (every proposition, in so far as it is a logical picture, is also a fact--a nexus of names: T. 2.141, 3.14) then such a proposition cannot depict a possible fact, i.e., in essence, it is not a proper proposition because it is not a logical picture of a fact. Since the type theory of P.M. was limited to a certain range of indicative sentences -- n-adic predications, truth-functions and quantificational closures thereof -- it could not exhaustively represent all sentence-forms of natural language immediately. So, logical atomism required that the structural picturing or logical form of complex sentences could only be revealed by analyses of them into component sentential structures which could be expressed in the symbolism of P.M. For, given that all those formal sentential structures (elementary propositions) depicting atomic facts are expressible in the logically perfect language to which P.M. approximates, then the analysis of any given

sentence of natural language must reveal whether it can be expressed in terms of them--specifically, as a truth-functional combination of them. If it can, it finds its place in the perspicuous language and is significant; if not, it is meaningless. As such, type theory becomes a theory of correct symbolism for the perspicuous language in which the proper logical form of sentences is revealed, and the limits of descriptive significance are set by the possibility of analysing sentences into the symbolic forms of this language.

The shortcomings of the logical atomist's doctrine of significant predication, and of its generalisation to a full theory of what can be significantly said and, contrastingly, of what can only be shown by the logical forms of a language are discussed in Part I. My concern, here, is but to trace in outline the gradual enlargement of type-theoretic views to encompass significance claims in philosophy generally, outside their application in mathematical logic. The first general claim of a typetheoretic view, then, is as noted: that sentences which overtly are neither gibberish nor strictly ungrammatical (though that remains to be argued) can be disqualified as nonsignificant on other grounds. This disqualification, of course, can only be justified in terms of a developed account of the difference between grammaticality and significance; and it is precisely this justificatory account which makes consideration of categorymistakes as being violations of type restrictions philosophically interesting. For, the claim is that within natural language such sentences as those listed at the opening of this Introduction (p. 1) are not the only instances of nonsense resulting from violations of type. Perhaps the patent absurdit:

of the listed sentences (1) to (10), under any literal reading, will support few inferences to the possibility of further, less obvious cases of disguised nonsense--which, though apparently plausible, unlike (1) to (10), lead to nonsense. Yet, just this claim is a consequence of the generic theory of significance which the doctrines of logical atomism entail. And, from this claim, the historical successors to Wittgenstein's and Russell's atomism--the logical positivists--derived a full-blown criterion of cognitive significance which seemed to make it possible for the apparently plausible assertions of theologians, meta-physicians and moralists to be dismissed as just such (disguised) nonsense.

The claim that the relatively trivial examples of category-mistakes, or violations of type listed in Section A are not alone in falling short of significance, but share this feature with many assertions typical of philosophical theories in metaphysics, theology and ethics is most evident in the philosophy of logical positivism. It is unfair to cite examples of sentences which positivists have rejected as category-mistakes or pseudostatements without noting that, on the one hand, considerable exegesis and analysis is needed to bring out the sense in which some of these sentences have been used. On the other hand, care must also be taken to note that the positivists' own rejection of such sentences has, in many cases, appeared only as the conclusion of a long and fairly rigourous attempt to make sense of them. Two examples will have to suffice:

(i) Though G.E. Moore was not a logical positivist, his practice of subjecting the statements of past philosophers to rigourous analysis in order to determine whether they were meaningful is illustrative of the process whereby, as the conclusion of an analysis, a philosopher may come to reject another's utterances as nonsignificant. A natural example of this

in Moore's own work appears in his article "The Conception of Reality" (1917) where he sets out to make sense of Bradley's thesis of <u>Appearance</u> and <u>Reality</u> that though time is unreal, yet it exists, and only after the most painstaking analysis of the thesis has to conclude that it is mean-ingless.

(ii) In the challenge to traditional metaphysics posed by the positivists' verifiability criterion of meaningfulness, the grounds for rejecting metaphysical theses as pseudo-statements were often in the form of arguments to diagnose the linguistic or logical confusions behind the theses, rather than overt attempts, through critical exegesis, to make them intelligible. Carnap, particularly in The Logical Syntax of Language, supported many of his anti-metaphysical claims with the conviction that philosophers said the odd things they did because they did not understand the logico-grammatical form of their examples. For instance, he suggests that Heidegger was led to ask questions like "Does the Nothing exist only because the Not, i.e. the Negation exists?" because he did not realise that although the syntax of "Nothing is outside" apparently parallels that of "John Smith is outside", the logically perspicuous form of the latter was "F(john Smith)" whilst that of the former was " \sim (Ex) F(x)" revealing that any answer to Heidegger's question would be literally nonsignificant, because syntactically ill-formed. Similar diagnoses of nonsense were made by Ayer and Carnap with respect to sentences like

"The nothing nothings itself." (M. Heidegger) "The Idea reveals itself in history" (G. Hegel) "Physical objects strive towards perfection" (supposedly, B. Spinoza) "Perfection entails existence" (from the Ontological Argument)

The positivists' verifiability criterion of meaningfulness is derived from, though simpler than, Russell and Wittgenstein's atomistic view. Its consequences are correspondingly similar but proportionately more extensive. Briefly, the positivists proposed that a non-mathematical assertion is descriptively significant if, <u>and only if</u>, it is empirically verifiable.¹⁸ At least part of the greater scope of the verifiability criterion--and certainly its most problematic feature--in contrast to the logical atomists' view, is attributable to the addition of the necessity-clause "and only if" to what, in the interpretation of the atomist view, constitutes only an entailmentclause. That is, the picture theory of meaning provides only the condition that when a fully analysed proposition fails to express agreement or

disagreement with elementary states of affairs, then it is not descriptively significant. So, the analysability of a proposition into a truth-function of elementary propositions is sufficient for its meaningfulness. On the other hand, for the positivists, the verifiability criterion yields both sufficient and necessary conditions for the descriptive or 'factual' significance of a proposition. If we take it, as some positivists did, that a proposition is empirically verifiable if there is some fact in terms of which its truth or falsity can be determined,¹⁹ then the similarity to the atomists' view is clear--though it strengthens the view in making the empirical availability of some verifying or falsifying fact necessary for the proposition to be meaningful. In addition, the positivist view achieves greater scope than the atomist view, at the loss of specificity, through the omission of any detailed requirement of structural isomorphism ('picturing') between proposition and fact. Thus the demand for a complete analysis of a proposition to reveal its expressibility in a logically perspicuous truth-functional extension of P.M. is removed also. (One should except the positivism of R. Carnap's Logical Syntax of Language, (1934) from this final observation). Consequently, the conception of an ideal language changes to become that of a suitably refined language of science. Determinacy of truth-conditions in this language becomes the test of significance, because it is in the natural sciences that empirical procedures of verification are most highly developed. Thus, it became possible for the positivist to dismiss as nonsignificant domains whose descriptive resources lay outside the expressive power of the language of science.

In the rubric of the positivist, type theory, when annexed to the verifiability criterion acquires the status of an antimetaphysical thesis. For, it enables the positivist to excise whole classes of assertions as cognitively meaningless. In this juxtaposition, the technical articulation of the formal theory of types of \underline{PM} is condensed to yield a general schema more readily applicable to the analysis of non-logical assertions. For example, from Ayer's account of verifiability.

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"A complete philosophical elucidation of any language would consist, first, in enumerating the types of sentence that were significant in the language, and then in displaying the relations of equivalence between sentences of various types two sentences are said to be of the same type when they can be correlated in such a way that to each symbol in one sentence there corresponds a symbol of the same type in the other; and ... two symbols are said to be of the same type when it it is always possible to substitute one for the other without changing a significant sentence into a piece of nonsense."

> (A.J. Ayer, Language, <u>Truth</u> and <u>Logic</u>, London: Gollancz, 1936, p. 62).

The verifiability criterion has been the focus of much criticism; but it is not my concern in the thesis to attack or defend it. In summary, it has been criticised on the following grounds:

(i) It is oversimplistic and insensitive to the variety of kinds of linguistic meaning. $^{\rm 20}$

(ii) The criterion, when applied to itself, is selfrefuting: if every assertion which does not belong to mathematics or to the empirical sciences (not translatable into verifiable statements which are either tautologous or empirically testable) is meaningless, then the assertion of the verifiability criterion is, itself, meaningless. That is, the positivists' own theses, being neither tautological nor empirical (being <u>about</u> procedures of verification, hence not subject to them), suffer the same fate as the nonverifiable assertions of metaphysics.

(iii) Practical use of the criterion requires what positivists have no where given successfully -- a unified, context-invariant, unambiguous and logically perspicuous language for the statement and verification of scientific truths.²¹ Ayer's appeal to a general criterion for type-sameness, in particular, suffers from several flaws. First it appears to be too strong, in so far as, without qualification, intersubstitutivity of expressions <u>salva significatione</u> in sentences invites refutation by counter-example. Almost any trio of sentences involving intentional or attitudinal predications will give conflicting answers to the question "Are (expressions) e_1 , e_2 of the same type?".

For example, let $e_1 = "Ayer's book"$, $e_2 = "An eristic argument"$, then:

- 1. Ayer's book is 7cm thick
- 2. An eristic argument is dull
- 3. Ayer's book is dull

is just such a trio. Intersubstitutivity of e_1 and e_2 in 2. and 3. seems to make e_1 of the same type as e_2 ; whereas the nonsense of "An eristic argument is 7cm. thick" makes e_1 , e_2 of different types. Then "Ayer's book is as dull as an eristic argument" though plausible, must be nonsense. Similarly, the nonsense of 4. "Continuity is the birthplace of Kant" yields a symbol type-difference between "Continuity" and "Königsberg" which is contradicted by the meaningfulness of:

- 5. Kant is thinking about continuity.
- 6. Kant is thinking about Königsberg.

and even seems to rule out compound assertions like:

7. Kant is thinking about continuity in Konigsberg.

These counter-examples are no doubt contrived and might be overcome by restrictions upon the kind of substitution-context Ayer will permit. Yet they indicate the insensitivity of the account to the variety of contexts in which expressions may function meaningfully in different ways.

Ayer's account also suffers from a circularity of intention. He is maintaining both that the "proper elucidation of a language" would enumerate sentence-types which are <u>significant</u>, and that matters of significance will be, in part, determined by considerations of type. Failure of substitution <u>salva significatione</u> is to serve as a test for symbol-type sameness--which presupposes that significance can be determined independently of type-classifications. Enumeration of significant sentence-types, however, requires that we correlate symbols according to sameness of symbol-type, which, in turn, requires substitutions between sentences without loss of <u>significance</u>. The procedure cannot get started unless some separate test for significance can yield an enumeration of significant sentences prior to classification of symboltypes.

Just such an independent test, of course, is provided by the verifiability-criterion--except that sentences rejected as nonsignificant because non-verifiable must now be excluded from the enumeration of sentence-types for the purposes of type-classification. We are left wondering why a <u>further</u> test for significance based on symbol-type sameness is needed (or whether a <u>different</u> kind of significance is involved in correlations of symbol-type). This circularity is not necessarily vicious--vet it diminishes the explanatory value of Ayer's account.

The reaction of philosophers of language to the simplistic approach of positivism and the artificiality of ideal language conceptions led inevitably to that preoccupation with piecemeal analyses of natural language which is characteristic of recent Oxford philosophy. Yet, if the approach to questions of meaning and significance changed, the aim remained the same-namely, that the enterprise of the philosopher of language is to reveal nonsense masquerading as sense, not only in the esoteric works of metaphysicians and theologists, but now, in the familiar everyday contexts of ordinary language.²²

The essence of this approach is captured in Wittgenstein's oftquoted:

"My aim is to teach you to pass from a piece of disguised nonsense to something that is patent nonsense."²³

and its propaedeutic value for subsequent theories of meaning and significance has been to dispel the illusion of adequacy in ideal language approaches---an illusion sustained by a disregard for the context-sensitivity of meaning and by unwarranted extrapolations from small samples of linguistic practices, disguised as methodological simplifications. Yet the fragmentary nature of the Oxford approach is inimical to the formulation of general theories--even though one of its most celebrated exponents, Ryle, did propose a general theory of category-mistakes, based upon a much liberalised type theory; and used it, in characteristic fashion, as a literal <u>reductio ad absurdum</u> of philosophical theses--particularly, of Cartesian dualism as a philosophy of mind.

Ryle's theory of category-mistakes receives much discussion in Part I, since, though it suffers from flaws similar to those which vitiate the positivists' use of type restrictions, it introduces a novel technique in formulating significance theories which is not subject to those criticisms which cast doubt upon the explanatory utility of the original Russellian approach. I shall conclude this introductory survey of the background to the views I consider with an outline of the difference between Russell and Ryle's techniques, because it focusses, in a preliminary way, upon the central issues to be debated in Part I.

If the therapeutic aim of discussions of significance and nonsignificance is to pass from 'disguised' to 'patent' nonsense, there must be instances of the latter which are implied by overtly plausible and seemingly significant propositions expressed in natural language. Typical instances of such propositions, considered by modern philosophers, have been: "The mind is in the brain", "Mental events can cause physical events", "God is an all perfect Being", "The Real is the Rational". Here, it is said, we have insidious cases of nonsense which purport to be sense. If <u>overtly</u> nonsensical propositions are to be implied by these, the propositions, themselves, cannot be mere gibberish or strings of words radically violating syntactic canons--if only because it is unclear how a relation of implication

could hold between a grammatically well-formed sentence and a word-salad string.²⁴ Though obvious nonsense, the implied sentences must fulfill minimal criteria of grammaticality. Such are the paradigm-cases which Ryle instances as category-mistakes.²⁵ In sentences like those listed in Section A, at the start of this Introduction, we have, Ryle maintains, paradigms of the absurdity which results from combining a subject which belongs to one logical type or category, with a predicate which is undefined or nonsignificant over that category. Thus, in one of the examples which Ryle considers, days of the week are not of the kind that can be in bed or not in bed; again, numbers are not the type of thing that can be or fail to be virtuous. This is a type theory: a partition of the domain of discourse into ranges over which predicates are defined, and for some of which predicates are significant and others not. An immediate difference between this type theory and Russell's is that, for Russell, all individuals (referents of proper-names) belong to one type, whereas, for Ryle, individuals divide sortally according to the significance of predications over them.

More importantly, there is a difference in technique, also. Russellian type theory is formulated as a system of general rules which legislate for nonsignificance in the language as a whole; yielding, thus, an algorithm for deciding whether any given sentence violates type-restrictions. Ryle, however, relies on a procedure demonstrating whether doubtful cases of significance are reducible to paradigm-cases--by analysis of each case as it arises (defining constituent expressions, drawing inferences from the expanded proposition, hoping to generate a patent absurdity, thereby). If it can be shown, argues Ryle, that an apparently meaningful thesis--of the kind listed above--can be reduced to, or shown to entail, a paradigm-case of nonsense--a category-mistake--then the thesis is nonsignificant. It is precisely this kind of literal <u>reductio</u> which forms the thrust of the main argument of Ryle's <u>Concept of Mind</u>, (London: Hutchinson, 1949), against dualism.

The chief advantages of Ryle's technique, over Russell's, reside in the removal of three shortcomings which beset the attempt to apply formal type theory to natural languages. I conclude my historical account with a discussion of these.

Russell's motivation for type theory--primarily, the removal of impredicative violations of VCP in the introduction of classes and the corresponding definitions of propositional functions--led him to characterise type-rules as semantic constraints upon the meaningfulness of formulae in P.M. Nonetheless, because of the oft-remarked ambiguity in Russell's notion of a propositional function-between functions as forms of expression and as what expressions denote--type-rules can appear as syntactic constraints upon the well-formedness of formulae. Indeed, for the purposes of mathematical logic, it is simpler to construe type restrictions in this way--as supplements to the syntactic formation-rules of the language of type theory.²⁶ For, sentences violating type-rules are then eliminated at outset as illformed, reducing considerably the débris of the formalism, unwanted in its intended interpretation. A formal language of this kind, however, is least suitable as an explanatory model of significant discourse in natural language. Quite deliberately--with some methodological justification--the formalist disregards features of ordinary discourse which are not relevant to the

precise symbolisation of mathematical theories; in the informal language of mathematics, of course, the formalist is <u>already</u> presented with a standing vocabulary and limited syntax shorn of the variability and apparent imprecision of ordinary usage. It is not surprising, then, if formalisms constructed with the representation of mathematical theories in mind prove wholly inadequate when applied in non-mathematical domains. On the other hand, in its attention from the outset to paradigms of type absurdity as they appear in <u>ordinary</u> discourse, Ryle's technique avoids the criticism that attempting to apply formal devices to non-mathematical contexts begs crucial questions involving the relations between formal and natural languages. This leads naturally to the second advantage of the Rylean approach-one which proves more difficult to characterise.

Even if one can bridge the gap between formal and natural languages which is a consequence of the ideal language views discussed above, a second difficulty threatens the attempt to transplant a Russellian approach to type theory into ordinary language. For, it may be objected, a theory which legislates against nonsignificance by means of general <u>rules</u> of sentence formation (be they semantic or syntactic) represents simply the wrong approach to problems of meaning and significance in natural language. If we suppose type restrictions to be linguistic rules in any prescriptive sense, their application to particular utterances invokes the spectre of that general repudiation of large areas of significant discourse which so many philosophers found inimical in the logical positivists' criterion of meaningfulness. Briefly expressed, except when the application of a philosophical theory turns inwards--to philosophy, itself--it does not seem part of the philosophic enterprise to set prescriptive limits to the creative capacities of language users. If, on the other hand, we suppose the theory of types to consist of general rules which are, in some sense, descriptive of the limits of significant discourse, then the theory runs grave risk of refutation by counterexample. Any utterance in the language which stands in violation of type rules, yet which is readily intelligible to speakers of the language, in a given context, is <u>prima facie</u> a possible counterexample to those rules—until some alternative explanation of its significance is given, or the general theory is adjusted to accommodate it.

The point may be clarified if put in the following way: in the limited context of mathematical philosophy, Russell's appeal to the nonsignificance of sentences which yield semantic or logical paradoxes is supported by their sharing a common feature; i.e. their impredicativity, or as Russell sometimes called it "their peculiar self-reference". However, impredicativity pertains only to the introduction of classes and their defining propositional functions. There seems no obvious way in which the notion can be generalised to every kind of predication (or propositional function) which may appear in non-mathematical language. Consequently, the bare appeal to the nonsignificance of type violations in ordinary discourse must either derive support from our common judgements as to the unintelligibility of such utterances, or from some more general theory of utterance significance, specifically framed for descriptive discourse as a whole (or for some substantial, readily characterisable part). In espousing logical atomism, the picture theory of meaning afforded Russell just such a vehicle for the transfer of type theory from mathematical to non-mathematical

discourse. Yet, ultimately, the picture theory, itself, only made sense against a background view requiring every significant utterance to be analysable into the formal ideaography of a logically perfect language (not that every utterance could be so analysed, but that this idealisation, in principle, supported the picture theory). Without this requirement, the theory of types as a theory of significant predication lacked once more a supporting theory--other than simply language users' agreement as to the intelligibility of utterances in context. But, if the discrimination between significant and nonsignificant utterances on the basis of general type rules can only be sustained by appeals to a consensus amongst speakers as to the intelligibility or otherwise of those utterances, then the need for a general type theory vanishes. For, one could short-circuit the whole approach simply by grounding significance claims directly upon some criterion involving speakers' agreements as to intelligibility. Type-rules, or principles, might still describe the rational consensus behind speakers' agreements, but would not longer justify any philosophical appeal to the nonsignificance of non-obvious type-violations or category-mistakes. (Just such a theory of category-mistakes--based upon the concept of the "unthinkability" of category-confused propositions--has been proposed by T. Drange²⁷).

It is not immediately clear, of course, how Ryle's approach to type theory as an account of category-mistakes avoids this objection. His appeal to paradigm-cases appears to be nothing more than appeal to speakers' intuitions into the nonsense of Ryle's favoured examples. Ryle's approach is sufficiently different, however, from the Russellian approach, to contain an answer--though Ryle, himself, may not have seen it. For, Ryle later came to abandon the view which he had held in his 1938 article on "Categories"²⁸, that there could be a clear and general theory of sameness and difference of category explicable in terms of types. In 1938 he had maintained, perhaps audaciously, that "category propositions (assertions that items belong to certain types) are always philosopher's propositions...<u>the</u> <u>converse is also true</u>." (Ryle, <u>1938</u>, p. 189, my emphasis). The same robust view appears in the Introduction to the <u>Concept of Mind</u>: "Philosophy is the replacement of category habits by category disciplines", and in his intention, in that work, to refute the 'official' dualistic theory of mind as "one big mistake...namely, a category-mistake."²⁹ Later, the robustness of these claims is qualified: he remarks in <u>Dilemmas</u>, (<u>1954</u>), that talk of sameness and difference of category "can be helpful as a familiar mnemonic with some beneficial associations, but it lacks an exact and professional way of using it."³⁰

Examination of Ryle's view in detail is vital before it can be shown that it can escape the objection of principle, above, against the formal, type-theoretic approach. Such an examination finds its proper place in Part I. Yet, some indication of the response to be gleaned from Ryle is needed here. Notice, first, that for Ryle the instrument <u>par excellence</u> for the exposure of type absurdities is the <u>reductio ad absurdum</u> argument. Ordinarily, <u>reductio</u> proof (or 'indirect proof') in formal logic relies on demonstrating <u>not</u> that an 'absurd' proposition is derivable from the negation of the thesis to be proved, but that a contradiction is so derivable. Contradictions, of course, are not 'absurd', but necessarily false--indeed,

the type homogeneity of their constituents ('p' and 'not-p') is required for the derivation to yield a genuine contradiction³¹. Ryle is claiming, however, that there is a distinctive kind of absurdity which is involved in arguments which seek to prove, not the falsity of a negation, but the nonsignificance of an apparently plausible thesis, by reducing it to (showing that it entails or presupposes) palpable nonsense. And this kind of absurdity, claims Ryle, is to be explained in terms of type violations. Ryle's own attempt to give criteria for this kind of absurdity is unfortunately inadequate -- as is shown in Part I. Yet, by his frequent appeal to a number of vivid examples of this kind of absurdity, as paradigms, Ryle does more to indicate how a general theory might develop, than he does by his own abortive efforts to formulate general tests. His classical article of 1938 ends with an admission of despair: "What", he demands, "are the tests of absurdity?" which will serve to circumscribe nonsense of the kind he seeks. Nevertheless, there is in that article and elsewhere in his writings the beginnings of an approach which will yield a non-circular general account of category mismatch through type-violation in predication.

In holding that there is a variety of nonsignificant utterance which, though not overtly nonsensical, yields palpable nonsense when its constituent expressions, implications and presuppositions are examined, Ryle is proposing that there are logical principles governing significance claims in accordance with which we can assess them. Such principles will govern the manner in which we draw the implications, and presuppositions, and carry out the analysis of <u>all</u> discourse, descriptively significant or not. They will provide structures in which we can exhibit the variety of ways in which

the significance or otherwise of utterance can be determined. The logic of such principles of significance must take seriously the possibility that the significance of utterances is never finally decided by their grammaticality--even if the notion of grammar is extended beyond syntax, and into the semantic component of a language-description. For, it is a possibility--one which Ryle's attention to paradigm-cases brings into focus--that there may be no limit to be set a priori--by means of a formal semantic theory--to the amount or kind of information which may be relevant to determining the significance or nonsignificance of an utterance.³²

The objection above, then, which required that an account of typerules be embedded in a general theory of utterance-significance, and which an approach like Russell's derived from mathematical logic, finds so difficult to parry, is avoided if the logic of type-restrictions is included as part of a logic which has as its function the delimitation of varities of nonsignificance amongst which type violations are to fall. Examination of paradigms reveals that such a logical investigation must not only adumbrate the general principles we employ in assessing significance claims; it must show also how the employment of those principles is intimately related to the contexts with respect to which significance claims are made. For, as has already been noted in Section A, that an utterance of one of the listed paradigms of category-mistakes can be shown to be nonsignificant depends upon our being unable to assign an interpretation to it in the context in which it is ordinarily taken. Frequently, in cases where nonliteral, metaphorical, colloquial or idiosyncratic interpretations of the utterance can be invoked, the absurdity, even of paradigm cases, evaporates

(though, that a metaphorical reading is invoked, say, is often a consequence of our being unable to assign a literal reading to the utterance in its context). The conclusion--which will be drawn towards the end of Part I-is to be that a logic of significance and nonsignificance must also be a logic of context. That such an approach does justice to Ryle's own manner of inferring 'patent' absurdity from the 'disguised' nonsense of philosophical theses must also be discussed at that point.

It is precisely the lack of attention to the context-sensitivity of utterance-meaning which comprises the third shortcoming of previous attempts to mould type theory into an account of category-mistakes, based upon the Russellian approach. The formulation of type theory either as syntactic constraints upon the well-formedness of sentences, or as semantic constraints upon the significance-ranges of predicates, requires that significance be construed as a permanent, context-independent feature of sentences. Ryle's approach need not involve such a committment--though Ryle, himself, nowhere mentions this point expressly -- since the entailments, presuppositions and analyses which Ryle's account employs to demonstrate the category absurdity of some thesis may all be construed as more or less context-relative--even if the context involved is minimally 'standard'. That the alternative--to construe significance as a context-invariant, permanent feature of sentences -- is misconceived in principle, is the aim of the final sections of Part I. For, it is only when this aim has been achieved that the logical apparatus of Part II--the description of logics of contextual significance for the appraisal of category-mistakes and other varieties of linguistic anomaly--can be given a justification. The historical
background to this last claim is therefore best introduced in the preamble to the constructive enterprise of Part II.

(II) Strategy of the argument:

Whatever else is responsible for the failure of an utterance to be significant, it is certain that, in many philosophical arguments, the term "category-mistake" has been regarded as applying to philosophical utterances which fail to be significant. Whether one coins a trivial example, from the list which opened the Introduction, or attends to the particular claims of philosophers--which one hopes to be non-trivial--a judgement that a particular utterance is category-mistaken is a judgement to the effect that things have been misclassified. That is, allocation of things (expressions or objects) to categories is, at least in part, a classifying activity. So, it should not be surprising that an investigation into the nonsignificance of categorymistakes should begin with discussion of problems arising in the theory of classes.

It should not be thought, however, that 'categorising' and 'classifying' are the same activity. To take an example from Passmore¹, it is a mistake of <u>classification</u> to be shown a kangaroo, a koala, a wombat,...in an Australian zoo, and then to ask to see a marsupial. In contrast, it is a category-mistake (similar to one diagnosed by Frege in <u>The Foundations of</u> <u>Arithmetic</u>, transl. J.L. Austin, New York, <u>1960</u>, 40e-41e)) to argue from the fact that "Thales was wise" and "Solon was wise" entails "Thales and Solon were wise" to the absurdity that, since "Thales was one (person)" and "Solon was one", then "Thales and Solon were one". The conclusion is inadmissable, yet the premises appear to be formally identical. What must be wrong, then, is the supposition that "is one" and "is wise" are predicates belonging to the same category.

There seems, then, to be some relation between the category to which particular things (expressions or objects) belong and the kind of logical inferences which can be carried out with the expressions referring to such things. That is, the question of how to account for the absurdity of categorymistakes seems bound up with the logical differences between what we take to be an ordinary standing falsehood, such as:

(1) My desk is green.and what we take to be an absurdity, such as:

(2) My desk is virtuous.

The former we feel inclined to reject merely as <u>false</u> (my desk is brown); but to the latter we respond that desks are not the <u>kind</u> or 'logical type' of thing which can be, or fail to be, virtuous. So, to elucidate this question, we must accordingly begin upon an elucidation of the theory of logical types.

The theory of logical types originates with Russell--notwithstanding that some evidence of formal distinctions of a type-theoretic kind can be found in both Schröder's <u>Vorlesungen über die Algebra der Logik</u> (1890) and in Frege's <u>Grundgesetze der Arithmetik</u> (1893 and 1903). Evidence from the latter will receive some attention, below. The theory Russell puts forward as "a possible solution to the contradiction" (i.e. Russell's paradox) and it is in this context that my enquiry into type theory begins. It begins, then, with examination of a technical difficulty in Frege's logicist philosophy of mathematics, the focal point of which is the introduction of classes (or, rather, what, for Frege, does the work of classes). Its immediate connection with matters of significant predication outside of the formal language of mathematics is made through Frege's doctrine of <u>complete</u> <u>definition</u> (that concepts are defined for all objects as arguments), and his insistance that Fregean classes (the courses-of-values of functions) are objects on a par with individual referents of proper names.

I shall argue that it is the paradoxicality of these two doctrines of Frege's semantic theory which issues, eventually, in Russell's paradox. Secondly, I shall connect this argument to Russell's own diagnosis (in conjunction with Poincaré) of the source of the paradox in 'impredicative' definitions which violate the Vicious Circle Principle. This second argument raises the more general question of what justifies our locating the source of Russell's paradox in predication (i.e., in the notion of a propositional function), rather than, simply, in an insufficiently wellarticulated theory of classes. In turn, this prompts the still larger question of what, in general, our response to the discovery of the logical and set-theoretic paradoxes in the foundations of mathematics should be. Though I devote some space to this final question below, I do not intend to argue that any one "diagnosis" of the paradoxes is necessarily 'correct', nor even that any one solution is required, for mathematical purposes. Nevertheless, I shall defend Russell's demand that a solution to the paradoxes be "inherently consonant with common sense" and not merely an ad hoc adjustment to the axioms of set-theory, or substitution principles of higher-order logic, against attacks upon type theory and upon impredicativity

and VCP from Ramsey, Gödel and Quine.²

It is in examining more closely Russell's justification for type theory that the gradual move in Russell's thought--described in the Introduction (p.37-42) -- from applying type theory solely to the technical problem of the paradoxes, to its application outside of mathematical philosophy, requires attention. The use Russell makes of type theory in his writings on Logical Atomism, and Wittgenstein's influence upon Russell at that time, introduces the more pertinent question of whether type distinctions have a role in the diagnosis of nonsignificance, and in setting the limits to descriptive significance imposed, first, by the doctrines of logical atomism and, subsequently, of logical positivism. At this point, my enquiry becomes particularised to the individual accounts of nonsignificance and of categorymistakes--each embracing a schematic view of significance-failure--which were listed in the Introduction (p. 17). The conclusions drawn from this examination of particular accounts, at the end of this Part, will be essentially negative: that few of the accounts considered are sufficiently sensitive to the flexibility and variety of significant discourse, and to its contextual dependence, to avoid the criticisms and counterexamples to which they have been subjected.

Since my enquiry begins with the technical problem of the paradox confronting Frege and Russell's logicist programme, it would be well to conclude these prefatory remarks with a brief sketch of the paradox and the type theory to which it gave rise. (I do not discuss the other paradoxes which arose in the foundations of mathematics at the turn of the century; though several of my general remarks about Russell's paradox apply equally to them).

A class (or set), as Cantor introduces the notion³, is simply "a collection into a whole of definite, distinct objects of our intuition or of our thought...the objects are called the 'elements' (members) of the class." Amongst classes, most will not be members of themselves--the class of short-haired gibbons is not itself a short-haired gibbon. Some will be members of themselves. For, the class of abstract objects is itself an abstract object. Reflecting upon this intuitive division between selfmembered and non-self-membered classes, we can form the collection of all those classes which are not members of themselves. Let us call this class 'W'. If we suppose, now, that W is a member of itself, then W is one among those classes which are not members of themselves. Our supposition, then, must be wrong; W is not self-membered. Then, W is among those classes which, not being self-membered, form the membership of W, whence W is a member of itself. Both suppositions, then, entail their respective negations. We have to conclude, therefore, that W is a member of itself if and only if W is not a member of itself--which is a contradiction.

Russell's response, first after believing there was some "trivial error in the reasoning" responsible, and after five years of trying various alternative responses,⁴ was to claim that the specification of a class like W was viciously circular and, thus, that the suppositions that a class either is or is not a member of itself are not significant. So, since the premises of the argument are nonsignificant, the argument cannot be meaningfully stated and the paradoxical conclusion evaporates:

"All our contradictions have in common the assumption of a totality such that, if it were legitimate, it would once be enlarged by new members defined in terms of itself. This leads us to the rule: 'Whatever involves all of a collection must not be one of the collection'; or, conversely: 'If, provided a certain collection had a total, it would have members only definable in terms of that total, then the said collection has no total'. And when I say that a collection has no total, I mean that statements about all its members are nonsense." (Russell, <u>1908</u>, repr. in Russell, 1956, p. 63)

If the premises of the paradoxical argument are to be proscribed as meaningless, the theory which is to support this claim must constitute a part of the theory of meaning --- which, for Russell, when he first propounded type theory, in 1903, is a theory of denoting.⁵ I shall discuss this further below. For the moment, it suffices to observe that Russell first states the theory of types with reference to propositional functions, and thence derives the restrictions upon membership-relations between elements and classes required to eliminate the paradox as stated. Propositions are the basic entities of Russell's denotational semantics--they are the truth-bearers of Russell's logic and the constituents of implications. From the notion of a proposition, we can explain (but not define) the notion of a propositional function: "Every proposition" Russell writes, "may be divided...into a term (subject) and something which is said about the subject, which something I shall call the 'assertion'".⁶ Accordingly, if we represent what is said about a subject--the assertion--by \emptyset , and replace the term by a variable 'x', we obtain a propositional function: " $\emptyset x$ is a propositional function if, for every value of x, $\emptyset x$ is a proposition, determinate when x is given."⁷ Examples. then, of propositional functions are "x is a man"--where x has replaced a subject term like "Socrates" or "the king of the jungle"--"x is seven less

than y"--where two terms, say, "the square root of 10", "10.16" have been replaced by distinct variables. The values of a variable x for which a propositional function is true form a class, and, as Russell notes, "a class may be defined as all the terms satisfying some propositional function."⁸

It is this proposal--to introduce classes as made up of values of x for which an arbitrary propositional function $\emptyset x$ is true--which the paradox threatens first. For, if $\emptyset x$ is the propositional function "x is a member of itself (xex)" and $-\emptyset x$ is its negation " $x \notin x$ ", then the class of values of x for which $-\emptyset x$ is true is the class W such that, for any u, ueW iff $u \notin u$. In particular, some u may be the class W itself, whence WeW iff $W \notin W$.

The doctrine of types, put forward tentatively in Appendix B to Russell (1903), addresses itself to the values of propositional functions and restricts them to types in the following manner:

"Every propositional function $\emptyset x...has$, in addition to its range of truth, a range of significance, i.e. a range within which x must lie if $\emptyset x$ is to be a proposition at all, whether true or false. This is the first point in the theory of types; the second point is that ranges of significance form types, i.e. if x belongs to the range of significance of $\emptyset x$, then there is a class of objects, the type of x, all of which must also belong to the range of significance of $\emptyset x$, however \emptyset may be varied."⁹

The immediate corollary Russell adds to this restriction of the values of a function to types is to divide the domain of his logic into a heirarchy of such ranges. Terms and individuals comprise the lowest type--type-O--in the heirarchy. As Russell expresses it, somewhat circularly, the lowest type of object is "a term or individual...any object which is not a range."¹⁰ A less circular specification of the lowest type occurs a few sentences

later: "Individuals are the only objects of which numbers cannot be significantly asserted." (c.f. Frege's example of "Thales and Solon are one", above). Type-1 objects consist of ranges or classes of individuals, and the heirarchy develops from hereon upwards. Type-2 objects are classes of classes of individuals; type-3 comprises classes of classes of classes..., and so on. To this simple heirarchy, in the original version of the doctrine, Russell adds also heirarchies of couples, triples, etc., to encompass the ranges of significance of polyadic, relational propositional functions, and a heirarchy of types for propositions arising from the need to distinguish complex propositional functions whose variables range over propositions, from simple propositional functions not involving propositional variables.¹¹

This heirarchical subdivision of the domains of significance of propositional functions is the essential characteristic of a type theory. Since the values satisfying a propositional function $\emptyset x$ must always belong to a lower type than the class of those values, any assertion that a class occur among the values of a propositional function whose range of significance is that class (or is of the same type as that class) cannot be a true or false proposition. The assertion or denial, therefore, of a class' being a member of itself is nonsignificant.

Russell's exposition of type theory in the <u>Principles</u> (1903) is, avowedly, a "rough sketch".¹² It is neither clear nor comprehensive in its resolution of paradoxes other than his own. In particular, as Russell points out, since numbers are defined in terms of the totality of types and ranges (equinumerous classes may be from heterogeneous types), Cantor's paradox concluding that the greatest cardinal number is less than the cardinal of

its own set of subsets--and, hence, is not the greatest--is not removed by this version of type theory.

As a preliminary to examining how Russell developed this rough sketch into a full solution to the paradoxes, attention needs to be drawn to a historical oddity in the exegesis of Russell's views--one which threatens to trivialise the attempt to interpret type theory in the wider context of meaning and significance. I conclude this preamble with an outline of the problem to which this oddity gives rise.

Subsequent to the <u>Principles</u>, Russell spent five years seeking to resolve the paradoxes by other means¹³--being engaged throughout this period in a series of debates over the source and status of the paradoxes (with Jourdain, Couturat, Poincaré and Maxime Bôcher¹⁴). Russell returned to type theory in 1908--prompted by advances he had made in the theory of denoting between 1905 and 1908¹⁵--developing the full ramified theory of types as a solution to all the paradoxes under discussion at that time, and subsequently incorporating the full theory into <u>P.M</u>. Ramsey (<u>1925</u>) urged a thorough revision of type theory, following a proposal by Chwistek (<u>1921</u>), a revision which Russell acknowledged:

"renders possible a great simplification of the theory of types which, as it emerges from Ramsey's discussion, ceases wholly to appear implausible or artificial, or a mere ad hoc hypothesis designed to avoid the contradictions..."¹⁶

The simplification Russell refers to, here, consists in:

(i) Ramsey's modification of the P.M. notion of a predicative function to encompass functions of type-1, all of whose arguments are individuals, and which are 'extensional' in the sense of being truth-functions of arguments all of which (finitely or infinitely many) are either atomic functions of individuals (of the form ' \emptyset x'--where \emptyset is truth-functionally simple) or are propositions;

(ii) Ramsey's distinction between the <u>logical</u> and <u>semantic</u> paradoxes. Simple type theory (i.e., roughly, the <u>Principles</u> theory together with a modified version of predicative functions) proves sufficient to resolve those paradoxes which Ramsey argues belong properly to logic or set theory. The semantic paradoxes were to be resolved by linguistic considerations not pertinent to mathematical logic.

The further attraction of Ramsey's simplification--one which commended the view to Russell--was that simple type theory could dispense with the troublesome 'Axiom of Reducibility'. This controversial axiom--guaranteeing the existence of a predicative function (of type-1, of individuals) extensionally equivalent to any function of higher type--had been required to preserve the definability of real numbers in the ramified heirarchy of <u>P.M.</u> Ramsey's simplification removes the need for ramified type-orders (which subdivide propositional functions according to the type of their quantified constituent variables), thereby removing the need for the axiom, also.

The endorsement Russell gives to Ramsey's simplification (loc. cit.) seems to indicate that Russell feared that the theory of types--particularly that of the ramified type-orders--might appear 'artificial' and 'ad hoc'. Yet, in 1903, 1908 and 1910, Russell had argued that the justification for type theory lay in consideration of the ranges of significance of propositional functions, in the nonsignificance of predications over objects falling outside these ranges. Furthermore, Russell had claimed, in 1906, 1908 and 1910, that the additional justification for the full ramified theory--one which gave it "a certain consonance with common sense which makes it inherently credible"¹⁷--was the Vicious Circle Principle. VCP was the principle--advocated originally be Poincaré and formulated above--that impredicative specification of totalities were circular and, thus, nonsignificant.

Admittedly, Russell said in 1938 only that type theory might appear ad hoc, not that it was. The anomaly remains, however, for it seems that Russell changed his mind over the status of type theory sometime between 1927 (the last occasion when Russell appeals to the philosophical grounds for type theory¹⁸) and 1938 (when Russell espouses what has become the customary view: that type theory constitutes simply one set of formal restrictions necessary for a consistent set-theoretic foundations of mathematics 19). Indeed, the date of Russell's change of heart can be determined more exactly, from Russell's review of Ramsey's book The Foundations of Mathematics (1925), published in 1931, where Russell provisionally concedes the simplification 20 . The historical anomaly is further compounded by the fact that, apparently, Russell never gave up the view that more was required of a solution to the paradoxes than an ad hoc device proscribing paradoxical constructions in set theory or higher-order logic. Thus, commenting much later on his search for a solution to the paradoxes in the period 1900-1918, Russell lists the conditions he believed a solution should satisfy:²¹

"While I was looking for a solution, it seemed to me that there were three requisites if the solution was to be wholly satisfying. The first of these...was that the contradictions should disappear. The second...was that the solution should leave intact as much of mathematics as possible. The third, which is difficult to state precisely, was that the solution should, on reflection, appeal to what may be called 'logical common sense'."

It is tempting to respond immediately to this historical anomaly that Russell simply changed his mind, abandonning his former views in the light of Ramsey's new approach. Support for this response can be gleaned from Russell's admission elsewhere that, by 1938, he was no longer working seriously, nor contributing to research, on mathematical logic. His intellectual preoccupations had changed and Ramsey's proposal commended itself, since it retained the technical efficacy of the simple theory of types, yet dispensed with the notationally cumbersome ramified theory and the philosophically controversial axiom of reducibility. Indeed, one can acquiesce in this response's being a wholly reasonable explanation of Russell's change of mind. For, one can add to it the fact that Ramsey's modification of the notion of a predicative function -- with which Ramsey accomplishes the simplification--is based upon Wittgenstein's extensional analysis of propositions in the Tractatus, an analysis which Russell himself had adopted after his formulation of the ramified theory (i.e., publicly, in his 1918 "Lectures on the Philosophy of Logical Atomism"). The change in Russell's views on propositions, on the status of logical truths, and on mathematics resulting from his adoption of logical atomism, we can say, made his original philosophical grounds for type theory seem inappropriate in the light of his new views.

For all that one can acquiesce in this diagnosis of Russell's reasons for changing his mind, there remains a question to be confronted. That is: was Russell <u>right</u> to change his mind over the need for a philosophical justification for type theory? At least one commentator on Russell's logic-himself an advocate of a logicist philosophy of mathematics, W.V. Quine--has added to this diagnosis of Russell's change of mind the claim that Russell was right to abandon his former view:

"Russell's theory, with its discrimination of orders for propositional functions whose arguments are of a single order, came to be known as the 'ramified theory of types'; and Ramsey's position was that it should be reduced to the so-called simple theory'... He did not, indeed, make his case as strong as he might. Sharing Russell's failure to distinguish clearly between attribute and expression, he, in turn, missed the really decisive point: that the axiom of reducibility guarantees outright the dispensability of the ramified theory... Thus, what Ramsey was arguing, and I, a few pages back, was in effect just the disavowal of an ill-conceived foundation."²²

Quine's diagnosis of the source of Russell's misconceptions as a confusion of sign with object (a 'use/mention' confusion) introduces an additional complication to the argument. Quine's claim, if true, seems to threaten the enterprise of seeking a semantic basis for type theory. If the sole justification for type theory consists in its providing one among a number of technical devices necessary to free formal logic or set theory from paradoxical constructions, and if the grounding of type theory in the notion of impredicativity is simply misplaced, it becomes gratuitous to ask for an account of the status of type theory in relation to significant predication. One must distinguish, then, the question of what changed Russell's mind over the status of type theory, from the question of the correctness of Russell's change of mind. The former is a biographical question -- to be answered, in all likelihood, by citing the changes in Russell's other views through this The latter question, however, concerns the philosophical basis for period. type theory, and requires further discussion, below.

I shall argue that the additional claim from Quine is false. Not only does it constitute a substantial misinterpretation of Russell's views on logic and mathematics; but it also fails to represent a proper perspective from which to examine Russell's particular contribution to the problems raised by the paradoxes--namely, type theory. Quine's claim is one which,

once adopted, drives a wedge between the philosophical foundations of logic and mathematics, and the formal, set-theoretic approach to foundational research. Such a wedge encourages the philosopher of mathematics to regard questions as to the justification or explanatory adequacy of his solutions to foundational problems as settled solely by considering the simplicity and convenience of his formalisms. In this, one can detect the influence of just that view of the logician's enterprise which is derived from the 'ideal language view', discussed in the Introduction. If one follows, for example, Quine (1963) in simply identifying the logicist programme with that of the reduction of classical mathematics to one or another form of set theory; or if, with Pollock (1970), one regards the issue of the truth of logicism as identical to the issue of whether mathematics is reducible to set theory, one can attach little or no significance to questions which both Frege and Russell--the originators of logicism--regarded as fundamental to their enterprise. Such questions as concern the nature of logical concepts, the justification of definitions, the analysis of propositions and the proper semantics for significant predication were of central importance for both Frege and Russell. Yet, they lie outside the purview of many contemporary philosophies of mathematics, since such questions typically do not arise in the mathematical theory of sets. It is not that some practices of idealisation are not necessary for the feasibility of some formal investigations. Rather, the thrust of my argument is to be that such idealisations cannot thereafter be used as counter arguments to renewing the investigation of topics which the formal idealisations were, for different reasons, intended to avoid. The argument begins, therefore, with a re-examination of the

source of the impredicativity of Frege's semantics which, it shall be claimed, issued in Russell's paradox.

Section B: The impredicativity of Frege's semantic theory:

(I): Russell's reaction to his own paradox had changed in a decade from his belief (noted above) that "some trivial error in the reasoning" was responsible, to acknowledging, with Whitehead in <u>P.M.</u> that a primary motivation for that monumental work consisted in its being "specially framed to solve the paradoxes which, in recent years, have troubled students of symbolic logic and the theory of aggregates."¹ One can infer then that Russell came to see the paradox not simply as a threat to one or another branch of mathematics, but to the logicist conception of mathematics as a whole. As he reports: he had become convinced that "the trouble lay in logic rather than mathematics and that it was logic which would have to be reformed."²

Russell did not infer from the inconsistency of Frege's <u>Grundgesetze</u> <u>der Arithmetik</u> (1893--hereafter, <u>Gg</u>) that mathematics itself is inconsistent. This contrasts, for example, with the attitude of the neo-Kantian Poincaré-and, later, that of Poincaré's disciple, Hermann Weyl--both of whom attributed the paradoxes, in general, to violations of mathematical principle. In particular, they attributed them to the committment to actually infinite sets which the mathematician makes in embracing Cantorean transfinite number theory.³ This committment follows immediately from the Cantorean assumption of an unrestricted comprehension axiom for sets--guaranteeing that, for an arbitrary property of objects (propositional function), there exists a set comprising all and only those objects having that property--together with Cantor's theorem that the set of all subsets of a given set always contains more member sets than the given set. Jointly, these assumptions yield the result that the domain of sets (if non-empty) can have no finite bound. I shall return to this conception of sets, and Poincaré's objections to it, in discussing Russell's espousal of the VCP, below.

Russell's attitude to the paradoxes, and that of the "predicativist" Poincaré do not exhaust the responses that have been made. Reaction has varied from Frege's:

"What is in question is not just my particular way of establishing arithmetic, but whether arithmetic can possibly be given a logical foundation at all."⁴

to Wittgenstein's:

"If a contradiction were now actually found in arithmetic that would prove only that an arithmetic with <u>such</u> a contradiction in it could render very good service; and it will be better for us to modify our concept of the certainty required than to say it would really not yet have been the proper arithmetic."⁵

and, if these two represent extremes of reaction, there have been many intermediate responses. Cantor had supposed that the paradoxes were a result of a misconception in the notion of a set⁶; whilst Zermelo and Mirimanoff had construed them, similarly, as proceeding from an inexact conception of sethood--one which required the strictures of formalisation and axiomatisation to render it consistent.⁷ In the proposal to 'eliminate' the paradoxes by rigorous formalisation and axiomatisation of set theory, however, there has always been some question, firstly, of how secure each new formalised set theory is; and, secondly, of how to argue against its apparent arbitrariness. The difficulty has been well-characterised by Von Neumann:

"Naturally it can never be shown in this way that the antinomies are actually excluded; and much arbitrariness always attaches to these axioms (of the rigorously formalised set theory). There is, to be sure, a measure of justification of these axioms in that they turn into evident propositions of naive set theory, when the axiomatically meaningless word 'set' is taken in Cantor's sense. But, what is deleted from naive set theory--and to avoid the antinomies it is essential to make some deletion--is absolutely arbitrary."⁸

Neither Russell nor Frege could accept such an arbitrary approach to 'resolving' the problem of the paradoxes, in so far as it failed both to identify the source of the paradox in question, and, in proposing a solution, it failed to relate that solution in a suitable way to the source identified. Part of the reason for the contrast between the Zermelo 'formal, set-theoretic' approach and the logicist's approach consisted simply in the difference between a practising mathematician's view of his enterprise and that of a philosophical logician. Only part of the reason is thus accountable, though, since neither Russell nor Frege--mathematicians in their own right --- would limit existing classical mathematics to resolve the paradoxes. Both, indeed, regarded the preservation of existing mathematics as a criterion of adequacy for a 'solution' to the paradoxes.⁹ The additional reason for the difference in attitude stems from Frege and Russell's conception of logicism as a philosophical foundation of mathematics. In seeking the source of the Russell paradox in Frege's semantic doctrines, I shall be concerned to bring out this difference.

Frege's initial reaction (in his reply to $Russell^{10}$ and in a hurriedly prepared appendix to $Gg.^{11}$) had been that the paradox stemmed from the

falsity of one of the axioms of \underline{Gg} . and not, as Zermelo proposed, from the vagueness or inexactitude of the conception of a class. In particular, Frege incriminated Axiom V of \underline{Gg} .--an axiom of which he professes always to have been suspicious:

"It is a matter of my Basic Law V. I have never concealed from myself its lack of self-evidence which the others possess, and which properly must be demanded of a law of logic,... I should gladly have relinquished this foundation if I had known of any substitute for it."¹²

It could be argued, perhaps, that, of itself, there is no real difference between Frege's construing the paradox as falsifying an axiom and Zermelo's construing it as indicating an inexact conception of sethood. As is well-known, there is an intimate relationship between, on the one hand, Frege's semantic theory (of sense and reference, concept and object) and his formal notation (the modified <u>Begriffschrift</u> of <u>Gg</u>.), and, on the other hand, between his semantics and his ontology of functions, concepts, truthvalues, senses, courses-of-values and extensions. Thus, we could regard the axioms of <u>Gg</u>. as <u>implicit definitions</u> of their constituent expressions. In particular, then, we could suppose that Axiom V implicitly defines Frege's notion of a 'course-of-values' by giving the conditions under which any two expressions stand for the same course-of-values: namely, when and only when the functions corresponding to that course-of-values have the same value for the same argument in all cases--which is what Axiom V states. (i.e. in modern, symbolic notation)¹³

 $\forall: \ \hat{\mathbf{x}}(\boldsymbol{\phi}\mathbf{x}) = \hat{\mathbf{x}}(\boldsymbol{\psi}\mathbf{x}) \ . \equiv . \ (\mathbf{x}) \ (\boldsymbol{\phi}\mathbf{x} \equiv \boldsymbol{\psi}\mathbf{x}).$

Consequently, in incriminating Axiom V as the source of paradox, Frege could be construed as identifying an inadequacy (or 'inexactitude') in his notion of a course-of-values ' $\hat{x}(\phi x)$ ', which can be interpreted as his equivalent to the Cantorean notion of a set.

There are certainly remarks Frege has made which seem to add plausibility to this argument. In commenting upon his formulation of Russell's paradox in the symbolism of Gg., Frege writes:

"If, in general, for any first-level concept, we may speak of its extension (course-of-values), then the case arises of concepts having the same extension, although not all objects that fall under one fall under the other as well. This, however, really abolishes the extension of a concept in the sense we have given the word... We see from the result of our deductions that it is quite impossible to give the words 'the extension of the concept $\phi(\xi)$ ' such a sense that from concepts being equal in extension we could always infer that every object falling under one falls under the other, likewise."¹⁴

Similarly, in offering tentatively a solution to the paradox by construing extensions as the sole exceptions to the "transformation of an equality which holds generally into an equality of course-of-values" (which is licensed by Axiom V), Frege notes that the effect of this proposal is to remove univocity for the definition of the course-of-values, entailing that the second-level function ' $\hat{x}(\phi x)$ ' is no longer well-defined:

"Obviously, this cannot be taken as <u>defining</u> the extension of a concept, but only as specifying the distinctive property of this second-level function."¹⁵ (my emphasis).

The proposal to construe extensions as the sole exceptions to Axiom V does not free Frege's <u>Gg</u>. from the inconsistency. As has been reported both in Quine's "On Frege's Way out", <u>Mind</u>, LXIV, 1955, pp. 145-159; and in Geach's "On Frege's Way Out", <u>Mind</u>, LXV, 1956, pp. 408-409, there is a further contradiction derivable from the amended axiom together with the assumption that the cardinality of the domain of <u>Gg</u>. exceeds one.¹⁶ Disregarding this,

however, the argument does suggest that, if we could so interpret Frege as sharing the views of set theorists--that the paradoxes result not from false logical principles but from inexact concepts of sethood, then there would be no need to look further for the source of the paradox in Frege's semantic doctrines. We could concede, simply, that the paradox is to be removed by rigorously axiomatising the theory of sets, interpreting the axioms as implicitly defining the revised notion of sethood, and disregard the questions involving the 'logical adequacy' of a solution to the paradoxes that Frege and Russell had taken so seriously. By implication, there would then be little need to look for the correlative justification for <u>Russell's</u> solution to the paradoxes--type theory---in the context of his semantic theory.

Unfortunately, such an interpretation as this argument requires is ruled out explicitly by further examination of Frege's own logicist views. For, Frege goes to some lengths to argue against interpreting the axioms of Gg. as implicit definitions of their constituent expressions:

"This transformation (Axiom V) must not be regarded as a definition; neither the word 'same' nor the equals-sign, nor the word 'course-of-values' nor a complex symbol like ' $\hat{x}(\phi x)$ ', nor both together, are defined by means of it... So, if we tried to regard our stipulation in Section 3 as a definition, this would certainly be an offence against our second principle of definition*",

to which he adds the footnote:

"* In general, we must not regard the stipulations in Volume i, with regard to the primitive signs as definitions. Only what is logically complex can be defined; what is simple can only be pointed to."¹⁷

Frege's antipathy for implicit definitions, supplemented by his positive account of the criteria governing the introduction of logical

expressions through definition, are a corner-stone of his semantic views. As will be shown, below, his additional reservations over 'contextual definitions'--the introduction of a form of notation within a context, specifying rules for the elimination of the expression introduced from all similar contexts--marks a significant difference between his conception of logicism and Russell's "mature conception".¹⁸ If we formulate logicism, after the fashion of Russell, as the twin theses that:

"All pure mathematics deals with concepts definable in terms of a very small number of fundamental logical concepts, and all its propositions are deducible from a very small number of logical principles."¹⁹,

then significant differences in Frege and Russell's respective accounts of 'definition' will entail congruent differences in their understanding of the logicist programme. By marking these differences between Frege and Russell's conceptions of logicism at the semantic level (rather, say, than in their axiomatisations of the theory of classes), I propose to develop the argument that, whilst Frege's semantic doctrines make the contradiction unavoidable (without wholesale revision of those doctrines), Russell's modified account of definition (in particular his theory of 'incomplete symbols' and 'contextual' introduction of classes) provide him both with a positive justification for his own solution to the paradoxes -- type theory -and a negative explanation of the paradox' source--impredicativity and the Vicious Circle Principle. It is just this separation--primarily between Russell and Frege's accounts of 'definition'--which has been obliterated in more recent expositions of logicism, which embed the logicist view in the context of axiomatic set theory (c.f. Quine, 1963 and Pollock, 1970). A corollary to my argument, therefore, will be to discourage this tendency

to divorce the philosophical foundations of logic and mathematics from the formal set-theoretic approach--and to regard the latter as the sole arena for the appraisal of type theory.

(II): Frege's Theory of Types :

Frege gave content to the thesis of logicism that mathematics and logic are identical by ascribing to the propositions of both the special characteristic of being "analytic"--whereby, a proposition is analytic if it can be shown to follow merely from general laws of logic, together with definitions of logical concepts formulated in accordance with them.²⁰ The procedure for showing that a proposition follows consists not only in the listing of the fundamental laws from which it is to follow, but also in displaying the methods of inference it is legitimate to use, and in mediating each transition within the demonstration, from simple logical notions to complex non-logical notions, with explicit definitions.²¹ In the Grundlagen der Arithmetik (1884--hereafter: G1.)²², Frege had initially proposed a method for introducing numerical terms by 'contextual definition'-that is, the definition of number is such as to provide for such terms only as they occur on either side of an identity-sign. Frege rejects this proposal and his subsequent hostility to any kind of definition, especially the use of axioms to define terms 'implicitly' and the practice of what he called "piecemeal definition". (introducing a concept defined over one range of objects; e.g. defining "x = y" only over positive integers, then subsequently reintorducing it for a different range, say, rational numbers), other than his own use of explicit definitions, postdates the Gl. and is a consequence of his doctrine of complete definition, formulated in FUB (see

footnote: 22) and Gg. (volume ii, Sections 56-57). It is the doctrine of complete definition in Frege's semantics, when conjoined to his treatment of courses-of-values and extensions as objects, which makes it impossible for Frege to avoid the argument which generates Russell's paradox. In so far as Russell came to embrace a theory of contextual definition in his efforts to solve the paradoxes, it is his abandonment of the doctrine of complete definition, and of the rationale for that doctrine, that leaves him free to agree to Poincaré's diagnosis of the source of paradox as resulting from impredicative definitions which violate VCP. In the next section I shall argue that Russell's positive solution to the paradoxes-type theory--can be construed as a logical development from the denial of the doctrine of complete definition. That is, if one gives up the view that a concept (or propositional function) must be defined for every object in the domain of discourse, one introduces subdivisions within that domain corresponding to the ranges for which concepts are defined. This constitutes the initial semantic motivation for a theory of types.

My concern in the remainder of this section will be to expound and criticise the doctrine of complete definition, and Frege's reasons for espousing it, and to show how it is responsible for the vicious circularity which generates Russell's paradox. To do justice to the doctrine, however, it is necessary to examine the foundations of Frege's semantic theory in some detail--in the definitions and notation of <u>Gg</u>.--and to concentrate initially upon a version of the theory of types which appears in the syntax of Gg.

The syntax of Gg. embraces a heirarchy of levels which is a natural

reflection of Frege's ontological bifurcation between concept and object.²³ Expressions in general, on Frege's view, can be divided into complete and incomplete--indeed, for any sentence to have a sense (express a thought) or reference (have a truth-value), it must contain at least one subordinate expression which is incomplete in respect of requiring an appropriate complete expression with which to combine to form the sentence. Complete expressions are of two kinds: singular terms and sentences. Together these occupy the lowest level of the heirarchy--that of expressions whose referents are objects. For Frege, the only condition for something to be an object is that it be the reference of a complete expression. If expressions for objects occupy the lowest level, all the remaining levels are occupied by incomplete expressions -- those whose referents are functions or concepts, which are themselves "incomplete" or "unsaturated".²⁴ A basic principle of Frege's semantics is that a symbol for an incomplete expression can never occur meaningfully without its "argument-place" or "gap-sign", indicated by a bracketed, lower-case Greek letter--which is to be filled by the appropriate kind of complete expression to form a sentence or singular term. (Indeed, a concept or function is identified as the referent of what remains of a complete expression--term or sentence--after a complete expression has been omitted. They are therefore essentially 'incomplete' or 'unsaturated', and have no meaning on their own). A sole exception to this principle-one that indicates a difference in kind in the thought expressed (or judgement made) thereby--is the occurrence of an incomplete expression in a quantified sentence or in the scope of some other variable-binding operator. In this case, the gap-sign is replaced by a symbol (a Gothic letter)

indicating not incompleteness, but the dependency of the range of the function referred to by the incomplete expression upon the operator within whose scope it falls. That functions denoted by incomplete expressions of one level can serve as arguments to higher-level functions (quantifiers, the 'smooth-breathing' operator, the numerical operator) to yield complete expressions as values, is a point I shall return to shortly.

Incomplete expressions are of various kinds. In so far as Frege assimilated sentences to complete expressions standing for objects (truthvalues), there is the first-level category of n-place sentential operators (normally n=1 or 2) which form a complex sentence when their gap-signs are filled by complete sentences.²⁵ Examples of such are:

(1)
$$---\xi$$
 (negation) (2) $----\xi$ (conditionality)

Of the first-level also are one-place concept expressions which result from a sentence by omission of one or more occurrences of a single complete expression, e.g.:

(3) ξ is wise. (4) $\xi^3 = 9.,$

as are relational expressions which result from a complete sentence by omission of one or more occurrences of each of two singular terms, e.g.:

(5)
$$\xi$$
 loves γ . (6) $\xi^3 \leq \gamma$

Clearly, the formation of n-adic expressions of first-level could continue for any n --though, in practice, Frege requires only dyadic (rarely, triadic) concept expressions.

There remains at the first-level functional expressions of any adicity, formed exactly analogously to first-level concept and relational expressions, save that they result from complex singular terms by omission of singular terms, rather than from complete sentences, e.g.:

(7) the father of ξ . (8) 2, $(\xi)^3 + (\eta)$. In addition, in <u>Gg</u>. (Section 34), Frege introduces a further kind of firstlevel expression, namely that formed by the sign " $\xi \cap \eta$ " which is to indicate the reduction of a second-level functional expression to one of firstlevel—the rationale for which emerges later.

The second-level of the heirarchy consists of incomplete expressions that form complete expressions only upon completion by first-level function (concept) expressions. Since expressions for the various arguments to second-level functions can have any adicity, Frege introduces (Section 23) a classification of <u>types</u> of argument, and correspondingly of <u>types</u> of argument-place, to discriminate between incomplete expressions within each level. This use of the term "type" (M. Furth's translation of Frege's "Art") is unfortunately different from Russell's use of "type" for the range of significance of a propositional function. The correlations and dissimilarities between Frege and Russell's heirarchies of 'types' are between Frege's "Stufen"--which I follow Furth in translating as "level"--and Russell's "logical type". To avoid confusion, I shall underline the term "type" when it occurs in its Fregean sense. It resembles, as we shall see, Russell's notion of the 'order' of a propositional function.

Argument-places that are appropriate for admission of singular terms cannot admit expressions for functions, and <u>vice-versa</u>. Similarly, argumentplaces admitting expressions for first-level functions of one argument are unsuitable for first-level functions of two arguments. For the notion of

an argument-place being suitable for a particular kind of expression, Frege uses a phrase which Furth translates as "expressions (of a particular <u>type</u>) ... being <u>fitting</u> for an argument -place of that <u>type</u>"; so that we can characterise Frege's classification of types as follows: (c.f. Section 23)

arguments of type-1: Objects. arguments of type-2: first-level functions of one argument. arguments of type-3: first-level functions of two arguments.

Proper-names and object-marks are <u>fitting</u> for argument-places of <u>type-1</u>; expressions for first-level monadic functions are <u>fitting</u> for the argumentplaces of <u>type-2</u>. In general, then, the objects and functions whose expressions are <u>fitting</u> for the argument-places of the expression for a function are fitting for this function. So, functions of one argument for which arguments of <u>type-2</u> are fitting will be <u>second-level</u> functions of one argument of <u>type-2</u> (because only second-level functions can have first-level functions falling within their range as arguments).

Some examples--preserving Frege's strict notational distinctions for expressions of different <u>types</u> and levels--will clarify this characterisation of Frege's heirarchy of <u>types</u>. In general, Frege reserves lowercase Greek letters ' ξ ', ' ζ '--in brackets--for the gap-signs in first-level function expressions. Greek capitals ' ξ ', ' ζ ' go proxy for expressions standing for determinate first-level functions or concepts, though the particular function or concept is unspecified. That is, they are used "as if they were first-level incomplete expressions referring to something, without specifying what the reference is."²⁶ This contrasts with the use of lower-case ' φ ', ' ζ ' which are genuine variables ranging over all first-level

functions of the appropriate <u>type</u>. Similarly, the Greek capital ' Ω ' is reserved for determinate second-level function expressions; whilst lowercase ' μ ' is a gap-sign for a second-level function expression as that may occur as argument to a third-level function. Thus, if $\Phi(\xi, \zeta)$ is a firstlevel function whose arguments are of <u>type-1</u> (objects)--e.g. ' $\xi^2 \leq \zeta$ ' or ' ξ loves ζ '--then $\Omega_{\beta,\gamma}(\phi(\beta,\gamma))$ is a second-level function whose arguments are of <u>type-3</u> (first-level relations). An instance of such a function would be that expressing the many-oneness of a relation:²⁷



which is a second-level concept within which fall all and only relations R such that for every x there is at most one y such that R(x,y).

Several points of detail need to be mentioned. In the general form of expression for a second-level function with arguments of <u>type</u>-3, above, the occurrences of β , γ in brackets are such as to indicate the argumentplaces to the first-level expression. They are keyed to the subscripts following ' Ω_{-} ' to indicate the dependence of the value of the second-level function upon the kind of arguments to a first-level function which falls within it. They are <u>not</u> variables or gap-signs--being a proper syncategorematic part of the second-level expression. On the other hand, the occurrence of ' β ' in the same expression (and in the quoted instance) <u>is</u> a gap-sign--but one for which the kind of expression which can replace it is limited to those which contain the requisite number and <u>type</u> of argumentplace. It is precisely in this way that the form of Frege's notation,

itself, displays the type-restrictions--at the level of syntax--which debar substitutions of the wrong type of argument to a function or concept. (This feature of Fregean type-theory becomes important in the discussion of Wittgenstein's doctrine of showing, as an account of nonsignificance, in Section D). That is, the style of the notation, itself, blocks the formulation of type-violations: and, for this reason, I preserve the original Fregean symbolism, wherever possible, throughout the rest of this discussion. For example, suppose we took the wrong type of argument to the second-level function described above. This second-level function $\Omega_{\rho,\gamma}(\phi(\beta,\gamma))$ can only take first-level relations as arguments; but suppose we took (i) a monadic first-level function $\mathbf{\Phi}(\boldsymbol{\xi})$ as argument, or (ii) the second-level function, itself, as its own argument. Then, in each case, the value of the function would not be determined, and the resulting expression would be ill-formed. In case (i), the result of replacing the gap-sign by ' $\overline{\Phi}(a)$ '--a determinate value of $\overline{\Phi}(\xi)$ --leaves the expression incomplete:

$$\mathcal{L}_{a,()}(\Phi(a,()))$$

In the second case (ii), the result of replacing the gap-sign by the expression itself:

$$\Omega_{\beta,\gamma}(\Omega_{\beta,\gamma}(\phi(\beta,\gamma)))).$$

leaves an occurrence of ' \emptyset ' unspecified, and the value, again, undetermined:

"'Functions of two arguments' are just as fundamentally different from 'functions of one argument' as the latter are from objects. For, whereas objects are wholly <u>saturated</u>, functions of two arguments are saturated to a lesser degree than functions of one argument, which, too, are already unsaturated."²⁸ Frege's appeal here to degrees of saturation pertains to the level of reference (what expressions stand for), whereas my characterisation, above, of substitution of improper arguments to functions has been wholly syntactic--that the result of improper substitution yields no value of the function is a <u>consequence</u> of (not a reason for) the ill-formedness of the expression. The interplay, here, between the syntactic level (the forms of expression) and the semantic (the interpretation of expressions) is characteristically Fregean. For, though the heirarchy of levels (Stufen) is introduced only in syntactic terms, the subsequent classification of argument <u>types</u> (Arten) is expressed first in semantic terms and then, in consequence, the necessary notational restrictions are introduced (ibid. Section 23).

As already noted, the sole condition differentiating object from function or concept (it has always to be borne in mind that, for Frege, a concept is simply a function whose value, for the appropriate <u>type</u> of argument, is a truth-value) is embodied in the form of expression referring to them. Thus, provided the argument-place of a given function-expression is filled by an expression of the appropriate <u>type</u>, the function or concept concerned <u>must</u> have a value for that argument. In other words, with respect to any first-level function, say, whose arguments are of <u>type</u>-1, a value of that function must result from the substitution of <u>any</u> complete expression whatever for the gap-sign. This is Frege's <u>doctrine of complete</u> <u>definition</u>. It requires that a function be defined (yield a value) for <u>every</u> object in the domain of the logic as argument to the function. Indeed, for a function expression to have a reference, it must satisfy the

condition of 'complete definition':

"An expression for a first-level function of one argument has reference...if the complete expression which results from filling the argument-place of this function expression always has a reference, just as long as the expression substituted has a reference."²⁹

Under the canons for the formation of function expressions, as noted, the notion of an incomplete expression is explicated in terms of the operation of omitting occurrences of a complete expression (singular term or sentence). Thus, that a concept is true of an object (that an object 'falls under' that concept) is explained in terms of the completion of the corresponding concept expression by any name of that object, to form a complete sentence. Frege invokes the doctrine of complete definition on the ground that <u>every</u> complete expression must stand for something (object or truth-value), so, the result of completing any function expression by the name of <u>any</u> object, whatsoever, must have a reference. Frege expresses this as follows:

"A definition of a concept...must be complete; it must unambiguously determine, as regard any object, whether or not it falls under the concept...We may express this metaphorically as follows: the concept must have a sharp boundary. If we represent concepts in extension by areas on a plane...to a concept without a sharp boundary, there would correspond an area that had not a sharp line all round...This would not really be an area at all; and likewise a concept that is not sharply defined is wrongly termed a concept."³⁰

I shall first examine and criticise Frege's reasons for this doctrine; then complete this section with my criticisms of the doctrine itself.

(III) The doctrine of Complete Definition:

So far as I can find, Frege has four different arguments to support the doctrine of complete definition. For ease of reference I shall give these arguments the letter-names "A", "B", "C" and "D":

A: Were the definition of a concept or function not complete, he argues in Section 56 (p. 159), then the concept defined could not be recognised as a concept by logic because precise laws would not hold true of it. For example, one logical law which would fail for an incompletely defined concept, he argues, would be the law of excluded middle. Indeed, Frege maintains that this law is just another form of the requirement that concepts be completely defined. If the function 'F(x, y)' were not completely defined for every pair of objects x, y, then there would be some pairs for which it had not yet been determined whether they stand in the relation F or not--strictly, then, for which some values of F(x, y) had no reference. If 'F(ξ, ζ)' stands for a concept, then, whose values, for objects x, y as arguments are truth-values, there would be some pairs of objects x, y for which the law 'F(x, y) or not-F(x, y)' would have no truth-value; i.e. the law would not be necessarily true for all objects. This undermines the universality of logical truths.

B: Just to the extent that concepts are not completely defined, so to that extent there will be no corresponding complete thoughts, <u>qua</u> the senses of complete sentences which result from 'saturation' of incompletely defined concepts. Frege's example is the sentence "there is only one square root of 9"--where the function "square root of (ξ) " has been defined only for positive integers as arguments to the gap-sign. In this case, the reference of that sentence is the True (the thought expressed is true); but only so long as consideration is limited to positive integers. Because of that limitation the thought expressed is not objectively determinate.

At some time, later on, it may become false if we decide to expand the number system beyond the positive integers, thereby extending the concept "square root". Thus, unless the range of possible arguments to a concept is determined completely, once and for all, then, Frege suggests, no complete thought involving that concept will be determinate, either finally true, or finally false. That is, since truth-values are the references of sentences, the truth of a sentence can change as the range of objects over which a concept is defined changes. (see: <u>Gg</u>. Section 56, pp. 164-5).

C: To deny the doctrine of complete definition, Frege argues, is to allow the possibility that a concept could have two or more definitions, and thus be introduced on separate occasions as ranging over differentkinds of object. To allow this, however, is to "leave in doubt" whether one definitional introduction of the concept conflicts with the other. If we define a concept once, for all objects, including those for which we would not ordinarily assign a value for those objects as arguments, then the concept will have a "sharp boundary" affording us a clear criterion for whether or not any particular object falls under it. If definition is not complete, we could have one criterion subdividing objects into three groups--those falling under the concept, those not falling under it, and those objects for which it is not yet determinate whether they fall under the concept or Then, if we have another criterial definition of the same concept, not. for a new range of objects, we have another boundary, and another partition of the range into two or three groups (depending upon whether the new definition exhausts the domain). The question arises whether these two criteria are related, whether the 'boundaries' of the concept overlap, or leave some

objects still undetermined. And Frege's answer is: we do not know--by failing to define the concept completely in the first place, we "are left in doubt" (Gg. Section 56, p. 163).

D: Finally, Frege suggests that to fail to define concepts completely places an intolerable strain upon the requirement that, in a suitably regimented, scientific language, the referents of all terms be determinate --and no terms which fail to refer be admitted. The argument would seem to be that, if a given concept $\overline{\Phi}(\xi)$ is not defined for an object a then the sentence " $\Phi(\underline{a})$ " will not have a truth-value. Certainly this puts a strain on Frege's thesis that objects are the referents of complete expressions. For, though Frege admits that ordinary language may contain expressions which fail to refer ("Pegasus", "the present King of France"), he insists that the exigencies of scientific precision require that every complete expression of the Begriffschrift have a reference. An analogous argument is constructed around determination of the values of the second-level is not defined for some object <u>a</u>, the value of the function $--- \phi(\sim)$ for the argument $\overline{\phi}(\xi)$ will not be determinate--since the sentence "----- $\Phi(\Lambda)$ " asserts that every result of completing the first-level function $\overline{\Phi}(\xi)$ with an expression standing for an object is a complete expression standing for the True. This would seem to be false for the instance " $\overline{\phi}(\underline{a})$ " even though it does not follow that $-\overline{\phi}(\underline{b})$ for some complete expression "b".31

A consequence of the doctrine of complete definition is that the only limitation upon the range of arguments to a given function or concept are

those provided by type and level restrictions. Now, the impropriety of substituting expressions of the wrong type in an incomplete expression is quite different from the circumstance in which the argument to a given function, though of the correct type, is such that it is quite arbitrary what value the function takes for that argument. For example, the value of the first-level function " ξ + 1" must be determinate not only when expressions for integers (numerals) replace the gap-sign, but when any complete expression is substituted. So, Frege maintains, if 'O' is a complete expression standing for the sun, "it is necessary to lay down rules from which it follows...what ' \bigcirc + 1' stands for...What rules we lay down is a matter of comparative indifference."³² The essential difference between these two circumstances is that, in the latter, the expressions are properly formed, though their sense (hence, their reference: object or truth-value) is a matter of stipulation. In the former case, however, expressions which violate type and level restrictions are not well-formed -- so the question of their sense and reference does not arise.

There is a prima facie implausibility in holding that a concept like "(ξ) is a prime number", must be defined over all objects, including the sun, moon or stars, as arguments. And Frege's arguments A through D are far less compelling than they may at first appear. Consider, first, argument A: that laws of logic would fail for incompletely defined concepts. Frege cannot be seriously maintaining, here, that laws of logic hold only for "one-sorted" logical systems; i.e. logics having only one style of individual variable, and one sort of singular term, ranging over the totality of objects in the domain. There are perfectly respectable many-sorte-
logics which distinguish different kinds of individual variables and terms, specifying a distinct domain for each sort of variable to range over. In such logics, there is no question of, say, the law of excluded middle failing for some concepts (predicates). For every object in the domain of the appropriate sort, any given concept is either truly or falsely predicable of it. Indeed, in most such systems, substitution of a term of the wrong sort into a concept-expression yields, not a 'truth-value gap', but an improperly formed formula (just as is the result, in Frege's logic, of substituting an argument-expression of the wrong type). Two examples of such many-sorted systems are (i) Von Neumann's axiomatised set theory (in "Eine Axiomatisierung der Mengenlehre", Journal für reine und angewandte Mathematik, 154, 1925, pp. 219-240) with its separate style of variable for 'proper classes' (which have members) and sets (which are members); and (ii) Quine's system ML (of Mathematical Logic, New York, 1940; rev. ed. Cambridge, Mass: Harvard U.P., 1951) which restricts sets to the values of bound variables in stratified formulae, but adds in "ultimate classes" through special terms, like 'V' for the class of all sets, as a separate sort.

Perhaps, then, argument A is intended only to object to the possible admission of 'truth-value gaps' into logic--statements which fail to be either true or false through the referential failure of a term in the sentence yielding them, or the failure of a concept to be defined for objects of a certain sort. Yet, if we are prepared to make adjustments elsewhere in the logic, the admission of such truth-value gaps into the interpretation of a logic need not entail abandonning logical laws. There may be good

reason, for example (as Strawson has done³³) to argue that:

(9) "The present King of France is bald"

yields neither a true nor a false statement, because it presupposes what is false: that there is at present a King of France. Logical systems which admit truth-value gaps through failure of presupposition have been muchstudied (see, especially: B. Van Fraassen, "Presuppositions, Supervaluations and Free Logic", in K. Lambert, ed., <u>The Logical Way of Doing Things</u>, New Haven: Yale U.P., 1969). It remains tautologous in (some of) those systems that for every statement S, either S or not-S holds.

Frege's objection A, then, to denying the doctrine of complete definition appears to be little more than a prejudice in favour of onesorted logics, or a refusal to countenance terms which fail to refer or concepts defined over limited domains.

Similarly, argument B is only compelling to the extent that we concede Frege's other semantic doctrines. In particular, it certainly places a strain upon Frege's thesis that truth-values are the referents of sentences to allow concepts defined over limited domains. Thus, one and the same sentence "There is only one square root of 9" refers to the True in the domain of positive integers, and the False in the domain of all integers. This reflects poorly on the treatment of <u>sentences</u> as truth-bearers, in general, in a manner analogous to the argument that the sentence "Britain is a monarchy" is true, now, but has been false--because of its token-dependence upon time of assertion. To hold, however, that no <u>complete thought</u> is expressed by a sentence containing a partially-defined concept, on the ground that the reference of such a sentence can change if the domain of definition changes, is to be committed--as Frege clearly was³⁴--to regarding the sense of a sentence (the thought expressed) as a fixed, immutable object which is independent of the occasion of the sentence's utterance, and unaffected by context. At best, such a conception of sentence-sense may be appropriate for the timeless truths of science and mathematics; though it is certainly insensitive to the semantic complexity of everyday discourse. There are reasons of convenience and simplicity which may justify this conception of sentence-sense when one's focus is--as Frege's was--upon the language of mathematics. But this is just to reduce the argument to pragmatic considerations (notwithstanding that one might object on ontological grounds to the reification of thoughts, <u>qua</u> sentence-senses, as fixed, inmutable objects).

Argument C raises the possibility of an incompletely defined concept's having two or more definitions which may conflict. In such a case, Frege is sceptical whether we can determine how the separate domains of definition are related. This is connected to his insistance--in the quotation above from <u>Gg</u>. Section 56--that a concept must have a "sharp boundary" which affords us a clear criterion for whether or not any particular object falls under it. His concern would seem to have two sources: that incompletely defined concepts would be <u>vague</u> ("a concept that is not sharply defined is wrongly termed a concept" (ibid.)) and that concepts defined over different restricted domains might conflict in their application. In the first instance, his concern over the admission of <u>vague</u> concepts is simply misplaced. If the domain over which a concept is defined is so restricted that such anomalous substitutions as ' \odot + 1' are outlawed,

there need be no vagueness, nor loss of sharpness, entering into such a definition. It is apparent that Frege has here confused "vagueness" with the restricted application of a concept. Certainly, for a vague concept, such as "\$ is greenish in hue", there may be no determinate answer to the question whether a given object falls under it. This is quite different from the circumstance of a concept's being defined over a limited domain. If we can lay down, in advance, an effective delineation of the domain of definition for a given concept, then we can decide what value the concept has for each argument from this domain. For an object not in the domain as argument, the determination of the value the concept is to take is not vague or unclear; it simply does not arise. Frege does advance arguments against restricting the domain of definition in advance. I shall consider them below. We have, though, to assess the second principal source of Frege's concern over incompletely defined concepts: that conflicts of application might arise.

The kind of conflict Frege seems to have in mind is illustrated by him in <u>Gg</u>. Section 58 (p. 162):

"For example, we may define a conic section as the intersection of a plane with a conical surface of rotation. When once we have done this, we may not define it over again, e.g. as a curve whose equation in Cartesian coordinates is of the second degree; for now that has to be proved ... Here, then, the boundary of the concept is not drawn in the same way, and it would be a mistake to use the same term 'conic section'. If the second definition is not ruled out by the first one, that is possible only because the first one is incomplete ... i.e. in a condition in which it may not be employed at all..."

The nature of Frege's objection, here, is puzzling until one adds to the principle of complete definition a further principle governing definitions

which Frege invokes (Section 60 and passim):

"We must reject a way of defining that makes the correctness of a definition depend upon our having first to carry out a proof."

Thus, Frege is illustrating his concern over conflicts in definitions by

claiming that if we first define "conic section" in wholly Euclidean terms:

Def.I: A CONIC SECTION is a plane figure which can be obtained as the intersection of a plane with a right circular cone.

and then proceed to apply the concept 'conic section' in a co-ordinate

system according to the definition:

Def.II: A (non-degenerate) CONIC SECTION is any curve of second order (Frege's "degree") whose equation can be brought into the form $y^2 = 2px (1 - e^2)x^2$ -- where p is positive and e the numerical eccentricity.

then we "leave in doubt" whether the application of the term to different planar figures will conflict--at least, until we prove that every conic section defined by Def.I can be identified with a curve of second order whose equation is of the form given in Def.II.

The object of Frege's attack, here, is the mathematician's practice of "piecemeal definition" (as Frege calls it: Gg. Section 57):

"...logic must reject all piecemeal definition. For if the first definition is already complete and has drawn sharp boundaries then either the second definition draws the same boundaries ... and then it must be rejected, because its content ought to be proved as a theorem ... or it draws different ones--and then it contradicts the first one."

Certainly, in the absence of a <u>proof</u> that every planar figure obtained as the intersection of a plane with a cone is given by an equation of the form $y^2 = 2px - (1 - e^2)x^2$ in a suitable co-ordinate system, then it is spurious to re-introduce the term 'conic section' by Def.II, having first employed it in sense I. What is mysterious is why Frege should refuse to regard II as a proper definition <u>after</u> the relevant proof has been given. The procedure involved is quite analogous to the practice in set theory (and elsewhere) of defining an operation on sets (e.g. forming the intersection of two sets "a \cap b") only after a theorem has been proved (or axiom lain down) to the effect that, for every pair of sets a, b, there exists a <u>unique</u> set formed from their intersection ("(x)(E!z)(x ϵz . \equiv . x $\epsilon a \& x \epsilon b$)"--where "(E!z)" reads "there exists one and only one z s.th. ..."). Prior to discovery of the proof, conflicts between Defs.I and II are possible; but after the proof is found, Frege's only objection to the procedure is to reject definitions that demand antecedent proofs on the pragmatic ground that:

"...for this makes it extraordinarily difficult to check the rigour of the deduction, since it is necessary to inquire, as regards each definition, whether any propositions have to be proved before laying it down." (Section 60: following the rejection of definitions depending on proofs).

In the end, then, Frege's objection C is directed only against piecemeal definitions--and then is only sound to the extent that we allow "complete definition" of a concept to be pragmatically simpler than definitions which require antecedently proved theorems.

Frege also objects to the practice of using conditional definitions to limit the domain of definition of a concept in the definition itself, so as to rule out the need for separate stipulations of the values of cases like $'_{\bigcirc}+1'$ It is conditional definition that is the object of his criticism D. His argument is that practical delineation of the domain over which a concept is to be defined, in advance of determining the values the concept takes for arguments from that domain, would be more difficult than simply stipulating the values the concept is to take for 'inappropriate' arguments. Thus, for example, to the suggestion that the domain of the function $\xi + \xi = 1$ ' be restricted to natural numbers, Frege replies:

"...we may indeed specify that only numbers can stand in our relation ... But with that would have to go a complete definition of the word 'number', and that is just what is most lacking." (Section 62, p. 165).

Now, Frege's point would be well-taken if one had to invoke such a function as " $\mathbf{\xi} + \mathbf{\zeta} = 1$ " to <u>define</u> the domain of numbers--since the procedure restricting the domain would then be viciously circular. But in the absence of such circularity, the strength of the objection lies only in the practical difficulty (with respect to some domains) of delineating in advance such restrictions as block the formation of anomalous substitutions like "the moon + the moon = 1". Thus:

"If people would actually try to lay down laws that stopped the formation of such concept-expression, which though linguistically possible, are inadmissible, they would soon find the task exceedingly difficult and probably impracticable." (Section 64, p. 168).

We have no guarantee from Frege, however, that there will be any less difficulty in laying down uniform <u>stipulations</u>, which yield an arbitrary value when an 'inappropriate' argument completes the function. <u>Ad hoc</u> stipulation alone, of an arbitrary value, will not do--for we require prior conditions distinguishing cases when <u>ad hoc</u> stipulation is needed from cases where no such stipulations is permissible. That is, for the function $\xi + \zeta = 1'$, before it can be legislated that for non-numerical arguments the function takes the value the False, or the Moon, or whatever, there must already be a clear distinction between what is and what is not a term for a number. In the absence of such a distinction, one cannot separate cases when the stipulation applies from cases of 'appropriate' substitution, when the stipulation <u>cannot</u> apply on pain of falsifying arithmetic truths by fiat. But, if such a distinction between numerical and non-numerical terms is available before the function is defined for all objects as arguments, it can be used to confine the function to numerical arguments, in advance, thereby eliminating cases for which stipulation may have been required.

Frege's reply to such an argument is wholly inadequate:

"Let us suppose for once that the concept 'number' has been sharply defined; let it be laid down that italic letters are to indicate only numbers; and let the sign of addition be defined only for numbers, ... By a wellknown law of logic the proposition:

'if <u>a</u> is a number and <u>b</u> is a number, then <u>a</u> + b = b + a'

can be transformed into the proposition:

'if $\underline{a} + \underline{b} \neq \underline{b} + \underline{a}$, and \underline{a} is a number, then b is not a number.'

and here it is impossible to maintain the restriction to the domain of numbers. The force of the situation works irresistably towards the breaking down of such restrictions." (Section 65, pp. 169-170).

Frege's reply rests upon a deliberate misconstrual of a conditional definition as laying down the truth of a conditional assertion, rather than of its intended purpose in affirming, conditionally upon the truth of the antecedent, the truth of the consequent. In any case, by an argument I owe to D. Bell (see his Ph.D. dissertation, "Frege's Theory of Judgement", McMaster University, 1976), Frege's reply can be generalised to militate against Frege's own ontological bifurcation between concept and object. For assume that italic letters indicate unrestrictedly, ranging over all objects. Then it can be claimed that:

'if <u>a</u> is an object and <u>b</u> is an object, then $\underline{a} + \underline{b} = \underline{b} + \underline{a}$. (though the claim need not be true). Hence by contraposition:

'if $\underline{a} + \underline{b} \neq \underline{b} + \underline{a}$ and \underline{a} is an object, then \underline{b} is not an object.' It becomes similarly impossible to maintain the restriction to the domain of objects. The force of the situation works irresistably towards the breaking down of the distinction between concept and object.

We cannot remain satisfied with such an argument, however, since the syntactic canons of Frege's Begriffschrift are broken in its formulation. A concept-expression like " 5 is an object" is a syntactic oddity, since it will yield a true sentence for every argument that can be significantly substituted for the variable -- a feature which, subsequently, in connection with his doctrine of showing, Wittgenstein was to use a condition for 'formal concept's, (see Section D, below). If anything other than a symbol for an object (a complete expression) is substituted for the gap-sign, the result will not be false but syntactically ill-formed. An analogous expression, which is true of everything which is the referent of a concept-expression, therefore, ought to be one whose gap-sign demands a concept expression to fill it, and when thus filled, will never yield a false statement. The inadequacy of Frege's argument D stems directly from the fact that no such expression can be formulated in the ideography of Gg. In particular, such an expression should be an expression of second-level,

taking arguments of type-2 or -3. The expression " ξ is a concept" is entirely unsuited to this task, since the predicate-expression can only be treated as if it were formed from a complete expression by omission of a singular term (name or sentence), and singular terms stand only for objects. The values of such a formal concept, then, would be uniformly false or syntactically ill-formed. It becomes impossible to assert, in Frege's semantics, that an expression stands for a concept (or, in the material mode, that anything is a concept). This is the root of Frege's apparently paradoxical denial that "the concept 'horse'" stands for a concept, but for an object. Frege's only solution to this difficulty was to devise a notation for a primitive second-level function which has a value when its argument-place is filled by any first-level incomplete expression (i.e. a concept-expression). The values of this second-level function for concepts as arguments are not concepts, but objects. That is, Frege introduces the second-level function " $\dot{\epsilon} \phi(\epsilon)$ "--which I shall represent by " $\hat{x}(\phi_x)$ "--which assigns to every first-level function or concept as argument a course-of-values or extension. For non-sentential first-level functions, the course-of-values $\hat{\mathbf{x}}(\mathbf{0}\mathbf{x})$ is the set of pairs the first member of which is the argument, and the second its value for that argument. For concepts, the extension is the set of pairs whose first member is the object as argument (any object, by the doctrine of somplete definition) and whose second member is the truth-value of the sentence resulting from completion of the concept-expression by a name for that object as argument. Coursesof-values and extensions (for simplicity I shall refer only to 'extensions' in the following) are objects on a par with any referents of a name or

sentence. They are, therefore, of the same level (namely, zero) as any 'complete' referent. I shall proceed, shortly, to the argument that it is this treatment of extensions as objects which, together with the doctrine of complete definition, is responsible for the 'impredicativity' of Frege's semantic theory which issues in Russell's paradox.

The exegesis and criticism of Frege's doctrine of complete definition has required a long and sustained attack upon the semantic basis for Frege's theory of definition. This attack has been motivated by three distinct concerns. It would be well, therefore, to summarise these concerns before proceeding to the final analysis of the impredicativity of Frege's semantics. My preliminary concern in the exegesis of the doctrine has been to form a clear idea of Frege's concept of definition, in order to support the argument, in Section C, below, that a key difference between Frege and Russell's conceptions of logicism resides in their different accounts of how mathematical concepts are to be defined in terms of purely logical ones. Frege rejected contextual definitions, along with implicit, conditional and piecemeal definitions; whereas it was not until Russell came to accept a contextual elimination of classes in terms of propositional functions (by contextual definition) that his positive solution to the class-paradoxes--type theory--could be grounded in the notion of a 'range of significance' for a propositional function.

In sum, Frege's theory of definition can be given by the following principles (abstracted from <u>Gg</u>. vol. ii, Sections 56-67 and 146):

P.1: "A definition of a concept must be complete."(56).
P.2: "We ought to regard it as quite self-evident that a word may not be defined by means of itself." (59).

P.3: "We must reject a way of defining that makes the correctness of a definition depend on our having first to carry out a proof." (60). P.4: "If .. (an) .. expression is actually to stand for a concept with sharp boundaries, then it must be determinate ... i.e. there must be one and only one object designated by this." (63). P.5: "The laws of logic presuppose concepts with sharp boundaries and therefore also complete definition for names of functions ... Accordingly, all conditional definitions and any procedure of piecemeal definition must be rejected. Every symbol must be completely defined at a stroke, so that ... it acquires a reference." (65). P.6: "Still less will it do to define two things with one definition; any definition must, on the contrary, contain a single sign and fix the reference of this sign." (66). P.7: "We may not define a symbol ... by defining an expression in which it occurs, whose remaining parts are known." (66). P.8: "The word (symbol) that is defined must be simple. Otherwise it might come about that the parts were also defined separately and that these definitions contradicted the definition of the whole." (66). P.9: "Only what is logically complex can be defined; what is simple can only be pointed to." (146: footnote).

In respect of these principles, my second concern has been to criticise Frege's grounds for P.1, P.3, P.4, P.5 and P.7, as they are correlated with the doctrine of complete definition, and with the use to which this doctrine is put. A secondary objective in this attack upon the doctrine has been to discredit the wholly pragmatic appeal to the convenience of one-sorted logic, and to the practice of assigning "don't care" values by stipulation--in contrast to the obvious implausibility of regarding expressions like ' \odot + 1' as meaningful. Frege, at least, advanced systematic arguments for defining concepts completely--the modern predilection for one style of variable rarely receives such systematic support.

Thirdly, to regard the doctrine of complete definition as an

unobjectionable (and convenient) device of semantic theory engenders puzzlement over why logical theory should be concerned with type-violations and category mismatch in predication, at all. If it is sufficient to embrace stipulations, in the formation-rules for a formal language, which dispense with the need to distinguish the ordinary falsehood of "2 + 2 = 1" from the absurdity of "the moon + the moon = 1", then the investigation of type theory as an explanation of this difference need never get started. Nonsignificant sentences are proscribed at outset, by stipulative assignment of "don't care" values. To the extent, therefore, that Frege's doctrine <u>is</u> objectionable, the need for a proper explanation--within the context of formal semantic theory--of the nonsignificance of type-violations and categorymistakes becomes more urgent. <u>(IV) The source of the paradox:</u>

(IV): We have observed above that Frege does not countenance expressions which remain incomplete when their argument-places are filled. The kind of argument (complete or incomplete) a function takes determines its level in the heirarchy of levels; but the values of a function for those arguments must belong to one and only one level--that of objects. It is for this reason that Frege objected to Russell's first formulation of the paradox of non-self-membered classes in terms of "a predicate is predicated of itself."³⁵ A function or concept of any level--for example, the concept " $\emptyset(\xi)$ " could not have itself as argument. If both concept and argument to it contained a gap of the same <u>type</u>, the result of filling the one by the other would leave one gap unfilled and the corresponding expression incomplete. Thus, taking " $\emptyset(\xi)$ " as argument to itself yields:

 $"\phi(\phi(\xi))"$ --where ' ξ ' is a place-holder.

Moreover, in view of the classification of argument <u>types</u>, a function or concept would simply not be 'fitting' for its own argument. The ideography of <u>Gg</u>., hence, bars this formulation of the Russell paradox, but this is of little consequence. The inconsistency remains, not only in respect of the "predication of a notion of its own extension" (which is Frege's preferred formulation³⁶), but even when the ill-fated notion of the <u>extension</u> of a concept is removed. For, as several commentators on Frege fail to observe, it is <u>not</u> the mere presence of extensions which, with Axiom V, generates Russell's paradox. As Frege demonstrates, himself, in the Appendix written in response to the paradox for volume ii of <u>Gg</u>., the contradiction can be generated from an <u>arbitrary</u> second-level function whose arguments are of <u>type</u>-2.³⁷

In brief, it can be shown that the paradox is a consequence of the twin theses that the domain of definition of every concept encompasses the totality of objects (the doctrine of complete definition), and that to every first-level concept $\oint(\xi)$, there corresponds an <u>object</u> $\hat{x}(\widehat{\Phi}x)$, whose identity conditions are provided by Axiom V, and which is an admissible argument to any first-level concept. The question which concerns us first, then, is: in what sense are extensions <u>proper</u> objects?

The need for a theory of extensions is that mathematical practice requires that numbers and classes of numbers be investigated, properties of them defined, and statements about them proved, <u>as if</u> each number and class were an identifiable object. For Frege, not only are numbers and classes investigated <u>as if</u> they were objects, but, in view of what Frege meant by an 'object'--anything that is the referent of a complete

expression -- numbers and classes (extensions) are objects. That is, the only kind of argument Frege advances when he contends that things of a certain kind--numbers, extensions, truth-values--are objects is that the form of expression for things of that kind, in the language, are complete expressions. A definition of number, he maintains in The Foundations of Arithmetic, must account not only for the adjectival occurrence of numerical terms, in such contexts as "Socrates has two legs" (answers to "How many?" questions), but also for their occurrence as names -- in arithmetical contexts like "2 is the only even prime". In effect, the rationale Frege gives for saying that numbers are logical objects rests on the character of the transition from answers to "How many?" questions--statements of the form "there are $n \phi$'s", "there are just as many ϕ 's as ψ 's"--to numerical terms filling the argument-places of first-level concept-expressions of the form "the number of $\phi s = \xi$ ", " ξ is both the number of ϕ 's and the number of ψ 's". The Fregean analysis we have already given of concept-expressions requires that the gap in such expressions be filled by complete expressions standing for objects. Therefore, numbers are objects.

It is at this point that Frege's antipathy for contextual definitions-indeed for any kind of definition other than explicit definitions which both demarcate what objects satisfy the definiens and indicates that there are such objects--becomes important. A contextual definition of a term T gives us the truth-conditions for sentences "...T..." containing T in such a way that, provided we can recognise an occurrence of an expression within a sentence as an occurrence of T, then T's referential capacity (both <u>what</u> T's refer to, and what kind of thing is a T) is exhausted by the truth or

falsity of sentences in which T occurs. That is, a contextual definition of T gives the 'sense' of the term T by presenting the kind of context in which T terms occur and fixing the truth-conditions of such sentential contexts without reference to T's. Questions whether T's really exist, or what kind of things T's are, then, are answered solely in terms of the truth or falsity of sentences containing T. If we have determined the truthconditions for all sentential contexts which contain T terms, everything that is needed to secure reference for such terms, on this view, has been done--any further question about the existence of T's can at best be a question about the truth-conditions of further (existential) statements which are entailed by sentences containing T. So, just as the questions whether there really was a poet called "Homer", or whether protons exist, are questions for the antiquarian and physicist, respectively, concerning the truth of such sentences as "Someone wrote the Iliad and the Odyssey", or "Some constituents of matter possess an invariant electrical charge"; so, the question whether some number -- say, the cardinal "aleph-1"--exists is decided for the mathematician by whether some such sentential context as "there is a least non-denumerable cardinal" has determinate truthconditions. Of course, the truth or falsity of such existential statements is decided (if at all) only by methods intrinsic to the domain of investigation concerned (observation and evidence, for the historian and physicist; proof for the mathematician). On this view of how defined terms secure their reference, there is no further philosophical question, no sense of "existence" beyond this, which would permit us to say that numbers really do or do not exist as objects. Once procedures for determining the truth

or falsity of existentially quantified statements in the language of the domain of investigation have been established, the questions concerning the existence of the referents of terms of the language introduced by contextual definition have been settled.

Such an interpretation of how (contextual) definitions secure the reference of defined terms comes into immediate conflict with Frege's account of complete expressions. Any complete expression--whether it is a simple, primitive name (<u>Eigename</u>) or a logically complex term formed from a functional expression of any level--refers to an object, whether concrete or abstract, particular or universal. This we can call Frege's "Realism". To the extent that the name/bearer relation is taken to be the prototype of reference for complete expressions, then the possession of reference by terms of the language guarantees that there are objects answering to those terms.

On the other hand, if we were to impute to Frege any such view as the account of contextual definition gives, above, then, when the truthconditions for sentences containing terms can be given by a rule which transforms them into sentences containing no such referential terms, then the realist doctrine becomes an unnecessary ornament. The referential capacity of a language will be fixed only by the term-forming and sentenceforming operations in the language (including quantifiers, and variablebinding operators). Thus, the function of the name/bearer relation would be taken over, for defined terms, by an explanation of the semantic role of such terms which accounted for the contribution they make to the senses, and hence the truth or falsity, of sentences containing them.

Frege's objections to the practice of defining terms contextually can, thus, be seen as a partial recognition of this conflict with his account of the reference of complete expressions. Once Frege had adopted a fully-fledged doctrine of reference for both complete and incomplete expressions³⁸, he could not advocate a contextual introduction of numerical terms (though he had considered the possibility in the <u>Foundations of</u> <u>Arithmetic³⁹</u>). His particular objection to a contextual definition of number is that the suggested definition of terms of the form "the number belonging to the concept \emptyset " (the number of \emptyset 's) supplies a sense--and hence a truthvalue--only for identity-statements in which <u>both</u> sides of the equation are occupied by terms of this kind. No procedure exists for determining the truth or falsity of sentences of the form "the number of \emptyset 's = Δ ", where Δ is any complete expression, whatsoever. Thus:

"It would consequently be completely impossible to prove a numerical equality, because we could never isolate a definite number. It is only apparent that we have defined 0 and 1 (contextually); as a matter of fact, we have only determined the sense of the expressions 'the number 0 belongs to the concept \emptyset ' and 'the number 1 belongs to the concept Ψ '; but it is not permissible to isolate in these 0 and 1 as independent, recognisable objects." (<u>The Foundations of Arithmetic</u>, transl. J.L. Austin, Sect. 56).

The objection is, then, that if we allow contextual definition of numerical terms, then the introduction of the term is so tied to the particular kind of function--term-forming--expression, or concept--sentenceforming--expression, in which the term is introduced, that the reference of the term could not be fixed outside of those expressions. The force of the objection, one supposes, consists in the apparent implausibility--given Frege's theory of objects--of the consequence that what kinds of objects there are will depend upon what kind of term-forming operations a language contains. This undermines--so the argument might run--the objectivity of mathematical truth.

The issues raised by this objection lie beyond the scope of the present discussion; though they are certainly of perennial concern: to what extent is our apparent committment to abstract objects (numbers, classes, functions) in mathematics a feature of the forms of expression we employ in the language of mathematics? What relations obtain between the defined terms of abstract mathematical theories (geometry, arithmetic, set theory) and the significance such terms acquire outside of mathematics--in everyday reasoning and calculation?

Specifically, in terms of Frege's <u>Gg</u>., to propose that the references of defined terms <u>could</u> be fixed by contextual definitions--hence, that such terms need have no referential committment to objects beyond their contribution to the senses of the sentential contexts in which they occur--would amount to the suggestion to confine the domain of objects of <u>Gg</u>. to the referents of logically complex expressions, introduced by definition. That is, the suggestion might be made to disregard simple names and treat <u>all</u> objects as courses-of-values, extensions, or truth-values. Reference for a genuinely singular term <u>a</u> could then be identified with the reference of a logically complex expression; for example, with the extension of a concept under which one and only one object falls--such as " $\hat{\mathbf{x}}(\mathbf{x} = \underline{a})$ " (c.f. Quine's NF identification of individuals with their own unit-classes⁴⁰). In a significant footnote, Frege rejects this suggestion on the ground that:

"Such a stipulation is possible for every object that is given us independently of courses-of-values... But before it can be generalised, the question arises whether it may not contradict our notation for recognising courses-of-values, if we take...an object that is already given us as a course-of-values. In particular, it is intolerable to allow it to hold for such objects as are not given us as courses-of-values; the way in which an object is given must not be regarded as an immutable property of it, since the same object can be given in a different way." (Gg., Section 10, footnote 17, p. 48). (my emphasis)

Frege is supposing, then, that the totality of objects is given in advance of the values that functions and concepts take -- in particular, independently of the values of the second-level function $'\hat{x}(\phi x)'$ assigning extensions to concepts. Indeed, as we have seen, Frege has to maintain this if the heirarchy of incomplete expressions is to be constructed--for the reference of each complete expression must be determinate in order for incomplete expressions, formed therefrom, to have a value for every complete expression as argument (complete definition). In this respect, Frege has to hold that the manner in which an object is given--the kind of function or concept whose value for a given argument is that object -- cannot be proprietary to it. On the other hand, if the totality of objects is to be closed under the mapping of arbitrary concepts onto their extensions -- and extensions are objects -- no object is given except as the extension of some concept; whence the 'manner in which an object is given' will be proprietary to it. In this we detect the symptom of that circularity which results in Russell's paradox--rendering the specification of the totality of values of $''_{x}(\phi_{x})''$ 'impredicative'. My investigation proceeds towards this conclusion.

(V): Impredicativity and the Totality of Objects:

Frege's conception of the 'totality of objects' is such that there

is no criterion Frege can give that an object must satisfy to belong to this totality--for, every object automatically belongs to it. As we noted, the bogus predication "§ is an object" is true of everything to which it can be meaningfully applied. The notion of an object is simply that of the correlative of a complete expression -- an object is the kind of thing for which complete expressions stand. We cannot assume, however, that a language contains a simple name for every object, since there are non-denumerably many. (There are non-denumerably many real numbers, numbers are objects; therefore, there are non-denumerably many objects). So, in forming the conception of the Fregean totality of all objects, we have to include any object of which it is true that an expression for that object could be generated in the language--even if it is impossible that expressions for all such objects be simultaneously generated in the language. I have already shown that Frege's twin theses that a concept be defined for all objects, and his realism--that every referent of a complete expression is an object-require that there be no restriction of the domain of objects which makes being an object relative to the way in which the object is picked out, or to the kind of term-forming and sentence-forming operators which yield expressions for objects. In fact, in the language of Gg., Frege includes in the totality the referents of complete expressions formed in any way, including those whose formation requires the application of operators binding variables which range over this totality. Operators cf this latter kind include the following:41

(a) the description-operator:- the second-level function $\hat{x}(\phi x)$ whose value is Δ for the argument $\overline{\Phi}(\xi)$ if $\overline{\Phi}(\xi)$ is a first-level concept under which Δ alone falls; otherwise $\hat{x}(\phi x) = \hat{x}(\phi x)$.

(b) the abstraction-operator:- the second-level function $\hat{x}(\phi x)$ whose value for any first-level function is the course-of-values of the function; and for any first-level concept is the class (extension) comprising the ordered pairs in each of which the second element is the (truth-) value of the concept for the first as argument.

(c) the 'reduction' operator:- the first-level function $\xi \cap \xi$ whose value for any course-of-values $\hat{x}(\phi x)$ for the ξ -argument is the same as the value of the function $\Phi(\xi)$ --whose course-of-values is the ξ -argument--for the ξ -argument as argument; otherwise its value is $\hat{x}(x \neq x)$.

(d) the numerical operator:- the second-level function $\Re \, \phi(\xi)$ --"the number belonging to the concept $\phi(\xi)$ "--whose value for the argument $\Phi(\xi)$ is the extension of the concept 'equinumerous with $\Phi(\xi)$ '.

What I shall show in conclusion, under the separate headings (a) - (d), is that, for each such operator, with the exception of the first, the presumption that the totality of objects is closed under a mapping--effected by such an operator--of concepts defined completely over this totality, onto objects in the totality, contains a vicious circle which renders the presumption illicit. That is, the conception of such a totality is 'impredicative' in the sense in which Russell and Poincaré used the term (see above, Section A). It is precisely this presumption which generates the inconsistency to which the semantics of Frege's <u>Gg</u>. falls prey. Since the notion of an arbitrary function or concept, as we have seen, is explicated only in terms of the reference of any expression formed from an expression for an object <u>in</u> the totality, it is circular to suppose that objects picked out only as values of operators taking such arbitrary functions or concepts as arguments could belong to the original totality. That this circularity is vicious can be shown as follows:

(a): The description operator is harmless--it merely maps every

concept under which only one object falls onto that object. Every totality is closed under such an operation. 42

(b): The abstraction operator is not harmless. Certainly, if there is some determinate totality T over which the arguments to first-level concepts $\emptyset(\xi)$ range; and, if $\underline{\Phi}(\xi)$ is a concept well-defined over T, then there will be a definite subset of T (the values of $\underline{\Phi}(\xi)$), which comprises all the objects in T satisfying $\underline{\Phi}(\xi)$. It is by such reasoning, for example, that Zermelo (<u>1908</u>) arrived at a weakening of the Cantorean 'abstraction' axiom which avoided the set-theoretic paradoxes. In its strong form:

(C) $(\exists x)(y) (y \in x. \equiv . \quad \oint(y)),$

the axiom yields Russell's paradox immediately. Taking ' $\oint(y)$ ' as ' $\sim(y \in y)$ ' and instantiating to x, we infer, for some x:

(C') $(x \in x. \equiv . \sim (x \in x)).$

However, if the totality over which $\overline{\Phi}(y)$ is defined is restricted to objects belonging already to some determinate, but unspecified, set z, then $\overline{\Phi}(y)$ will simply pare off a determinate subset of z--yielding:

(Z) $(\exists x)(y)$ $(y \in x. \equiv . y \in z \& \overline{\Phi}(y))$ --Zermello's <u>Aussonderungsaxiom</u>. (Z) will yield a paradox only if the set z is taken to be some very large totality--such as the set V of all sets. By axiomatically restricing the closure conditions for the domain of objects for ZF set theory, Fraenkel and Skolem (<u>1922</u>)--improving upon Zermelo's originally informal discussion-eschewed the formation of such large sets as V.⁴³

Such a restriction of the domain of objects would violate a basic principle of Frege's semantics, since it would make the definition of the concept $\Phi(\xi)$ conditional upon determining which objects belong to the restricted domain T. As we have seen, that " $\Phi(\xi)$ " is completely defined (that it has a reference) consists in its yielding a value for <u>every</u> argument. If we now were to say that only objects specified in some other way are permissible arguments to $\Phi(\xi)$, we have to acquiesce in there being some further conditions an expression has to satisfy before it can stand for an object--thus, that only <u>some</u> incomplete expression " $\phi(\xi)$ " formed by omission of an appropriate kind of complete expression will have a reference. Whether $\overline{\Phi}(\xi)$ is a determinate concept will then depend on whether those complete expressions which are omitted to form an expression standing for that concept are of the requisite kind. Whether they are of the requisite kind will, in turn, depend on what extra conditions an expression has to satisfy to be a permissible argument to $\overline{\Phi}(\xi)$. The circularity has become vicious.

What there is no ground for, in Frege's semantics, is his supposition that term-forming operations, defined only by means of quantification over the totality of objects in the domain, will <u>always</u> generate a term standing for an object <u>in</u> this totality. That is, we can have no ground for supposing there is <u>any</u> totality closed under an abstraction operator which maps arbitrary concepts defined over the totality onto objects in the totality-for Russell's paradox shows that there can be no such totality.

As stated, all that is needed to avoid this circularity is that we have some <u>prior</u> means of specifying, for example, a totality of subclasses of a given class for which it is determinate, for any concept defined over the given class, that there is amongst the subclasses one consisting of all those elements which fall under that concept. If, as Frege does, on the other hand, we seek to <u>explain</u> the notion of a subclass (an extension), and of the totality of subclasses, by characterising them as the referents of terms formed by applying the abstraction operator to concepts defined over the <u>given</u> class, then the principle adumbrated above requires that such concepts be only those that are defined in advance of the characterization of the totality of subclasses. In requiring concepts to be completely defined--even for values of the abstraction operator (extensions) as arguments--Frege offends against this principle.

Formally, Frege's introduction of the primitive second-level function $\hat{X}(\phi x)$ requires that this function assign the same extension to two concepts if and only if precisely the same objects fall under each; i.e. if and only if each concept has the same value for the same argument as the other. Equivalently, if the first-level concept $\overline{\Phi}(\xi)$ is assigned the extension $\hat{X}(\Phi x)$, then every first-level concept assigned an extension Γ such that $\hat{X}(\Phi x) = \Gamma$ must be such that exactly the same objects fall under it as fall under $\Phi(\xi)$. Now, if we assume that the totality T is closed under the abstraction operator, then, if <u>a</u> is an element of T, and for any g, if $g(\underline{a}) = \underline{d}$, then $\underline{d} \in T$; then, if $\hat{X}(gx) = \Gamma$, then $\Gamma \in T$. Since this holds for arbitrary first-level concepts, it holds also for the concept:

$$" \sim (g) [\hat{x}(gx) = \xi . \supset . g(\xi)]"$$

This is the concept $\overline{\Phi}(\xi)$ such that \underline{d} falls under $\underline{\Phi}(\xi)$ if and only if \underline{d} is not the extension of $\underline{\Phi}(\xi)$. The abstraction operator $\hat{x}(\emptyset x)$ assigns to this concept the extension:

$$\widehat{\mathcal{Y}}\Big(\sim (g)\Big[\widehat{x}(gx) = y \cdot \supset g(y)\Big]\Big).$$

Call this extension " Γ ". By the principle which introduces $\hat{x}(\phi x)$ --correspondi

to Frege's axiom Vb--any first-level concept assigned Γ as extension must be such that every object falling under $\overline{\Phi}(\xi)$ falls under that concept also. Conversely, no object not falling under $\overline{\Phi}(\xi)$ can fall under any concept assigned the extension Γ . Since $\Gamma \in T$ and $\overline{\Phi}(\xi)$ is completely defined for all objects in T, either Γ falls under $\overline{\Phi}(\xi)$ or it fails to do so:

I: Suppose
$$\Gamma$$
 falls under $\underline{\Phi}(\underline{\xi})$, i.e.:
(1) $\hat{\mathcal{Y}}(\sim(\underline{g})[\hat{x}(\underline{g}x)=y, \supset, \underline{g}(y)]) = \Gamma, \supset, \underline{\Phi}(\Gamma).$

then, by Vb, Γ falls under every concept assigned Γ as extension:

(2)
$$(g) [\hat{x}(g \times) = M : \supset g(M)].$$

But $\Phi(\xi)$ was the concept such that any object <u>d</u> falls under $\overline{\Phi}(\xi)$ iff <u>d</u> is <u>not</u> the extension of $\Phi(\xi)$; whence, expanding (1):

(3)
$$\hat{y}(\sim (g)[\hat{x}(g \times) = y \cdot \supset \cdot g(y)]) = \square : \supset \cdot$$

 $\sim (g)[\hat{x}(g \times) = \square : \supset \cdot g(\square)].$

Now, by our stipulation of M:

(4)
$$\hat{y}(\sim(g)[\hat{x}(gx) = y \cdot \supset g(y)]) = \Gamma$$
.

so we infer, by modus ponens, lines (3), (4):

(5) $\sim (g) [\hat{x}(gx) = \square : \supset : g(\square)].$

which contradicts (2), and supposition I must be false.

II: Conversely, suppose Λ does not fall under $\overline{\Phi}(\xi)$, i.e.:

(1)
$$\sim \left[\widehat{\mathcal{Y}}(\sim(g) [\widehat{x}(gx) = y \cdot \neg g(y)] \right) = \square : \neg \overline{\Phi}(\square) \right].$$

then, by Vb, \bigwedge is not the extension assigned to any concept under which it does not fall. That is, \bigwedge falls under <u>every</u> concept to which it is assigned as extension, i.e.:

(2)
$$(g)[\hat{x}(gx) = \square \supset g(\square)]$$

In particular, since (2) holds for any concept, it holds for $\Phi(\xi)$:

$$(3) \quad \hat{x}(\underline{\varphi}x) = \underline{r} : \neg \cdot \underline{\varphi}(\underline{r}).$$

But $\hat{\mathbf{x}}(\boldsymbol{\Phi}\mathbf{x})$ is the extension of the concept:

$$" \sim (g) [\hat{x}(gx) = \xi : \supset g(\xi)] . "$$

so (3) becomes, by substitution:

(4)
$$\widehat{\mathcal{G}}(\sim(g)[\widehat{\mathcal{G}}(gx) = y : \supset g(y)]) = \Gamma : \supset \Phi(\Gamma).$$

which contradicts (1), so supposition II must be false.

By I and II, we infer that the assumption that the totality T is closed under the abstraction operator $\hat{x}(\phi x)$ leads to contradiction and must be false. (The contradiction is a special case of Russell's paradox).

A similar result obtains for the assumption of closure under Frege's other second-level operators of <u>Gg</u>.--the 'reduction' operator ' $\xi \cap \zeta$ ' and the numerical operator ' $\Re \phi(\xi)$ '--though in the latter case, since the assumption is one of closure under an equivalence-relation, the circularity generates, not Russell's paradox, but a paradox of a somewhat different kind.

(c) The essential purpose of the 'reduction' operator is similar to the translation of a monadic predicate F(x) in modern predicate logic into a dyadic relational statement about class-membership-- $x \in \{y: F(y)\}$. For Frege, this translation is effected by transforming a <u>second-level</u> assertion about objects falling under a first-level concept (e.g. the <u>existential</u> assertion "the concept 'horse' has some objects falling under it." (= "there are horses")), into an assertion which expresses a <u>first-</u> level relation between an object and the extension of a concept (e.g. "Dobbin belongs to the extension of the concept 'horse'" (= "Dobbin is a horse")). In particular, Frege so defines the relation ' $\xi \cap \zeta$ ' to enable him to represent that the same objects fall under two concepts, in terms of a relation between objects and the <u>extensions</u> of concepts (also objects, of course). That is ' $\xi \cap \zeta$ ' serves the same purpose as the classical ' ϵ ' of set theory. The analogue in <u>Gg</u>., then, for the set-theoretic assertion that <u>a</u> is a member of a set B comprising everything satisfying some condition $\phi(\xi)$, is that the truth-value of the <u>concept</u> $\overline{\phi}(\xi)$ for the argument <u>a</u> is the same as the value of the relation ' $\xi \cap \zeta$ ' when <u>a</u> stands in the relation of 'belonging' to the extension of $\overline{\phi}(\xi)$. This last means that, if " $\overline{\phi}(\underline{a})$ " refers to the True ('1'), then the class of ordered pairs $\langle \underline{b}, \Gamma \rangle$ each of which contains the truth value Γ of $\overline{\phi}(\xi)$ for the argument <u>b</u>, contains a pair $\langle \underline{a}, 1 \rangle$ such that, if <u>a</u> is the first element (argument to $\overline{\phi}(\xi)$) then the second is the truth-value '1'. Then and only then is the assertion " $\underline{a} \cap \hat{x}(\overline{\phi}x)$ " true.

Not surprisingly, Frege's assumption of closure under ' $\xi \cap \xi$ ' turns out to be equivalent to the unrestricted assumption of closure under ' ϵ ' which makes naive set theory inconsistent. In terms of <u>Gg</u>., Russell's paradox is an immediate consequence of a theorem which asserts that every argument to an arbitrary first-level concept yields a value of the firstlevel relation ' $\xi \cap \xi$ ', i.e.

(1) $\underbrace{ \overline{\Phi}(\underline{a}) = \underline{a} \cap \hat{x}(\underline{\Phi} \times) }_{\text{(theorem 91: Gg., vol. i, Sections 54-55)}}.$

If, in this theorem, we take the first-level concept " $\sim(\xi \cap \xi)$ "--with which we can compare Russell's " $\sim(x \in x)$ "--for $\Phi(\xi)$ and the <u>extension</u> of this concept " $\hat{y}(\sim(y \cap y))$ " for <u>a</u>, we obtain:

(2) $\vdash \sim [\hat{y}(\sim(y \cap y)) \cap \hat{y}(\sim(y \cap y))] = [\hat{y}(\sim(y \cap y)) \cap \hat{y}(\sim(y \cap y))]$ which, having the form " $\sim(x = x)$ " is explicitly contradictory.

(d): The numerical operator $\Re \phi(\xi)$ maps every first-level concept onto a class of equinumerous extensions. That is, the value of $\Re \phi(\xi)$ for an arbitrary first-level concept $\overline{\phi}(\xi)$ is a class of extensions equivalent in cardinality to the extension of $\check{\phi}(\xi)$. The supposition that the totality of objects is closed under this operation does not generate a contradiction immediately. The impredicativity of Frege's conception of the totality of numbers would be viciously circular only were he to suppose that every one-to-one correspondence (or 'bijective mapping') between exten-the assumption of closure requires only the condition that the same cardinal number be assigned to any concepts $\phi_1(\xi), \ldots, \phi_n(\xi)$, between which there is a bijective mapping $M(\xi, \zeta)$ correlating one-to-one the objects falling under any $\phi_i(\xi)$, $\phi_j(\xi)$ (1 \leq i, j \leq n)--whether we can form in the language an incomplete expression " $B_k(x, y)$ " which yields a class extensionally equivalent to $\hat{x} \hat{y}(M(x,y))$. The identity condition, then, for an object (number) to be an element in this totality is simply that, if, for any first-level concepts, as many objects fall under the first as the second, (third, ... and so on) and vice versa, then each is assigned the same number by the operator $\Re \phi(\xi)$. Equivalently, if the concept $\overline{\phi}(\xi)$ is assigned the cardinal number <u>n</u> (= $\Re_{a} \Phi(e)$), then every concept assigned <u>n</u> must be such that at least and at most as many objects fall under it as fall under $\Phi(\xi)$ --even if it is not possible for us to actually supply a relation which correlates the

objects one-to-one under the concepts assigned <u>n</u>. One cannot expect the class of bijective mappings between extensions to be exhausted by all instances of the identity-schema "the number of \emptyset 's = the number of ψ 's". For, the recursive characterisation of the heirarchy of incomplete expressions generates at most denumerably many first-level expressions, and, hence, denumerably many arguments to " \Re $\emptyset(\xi)$ " yielding numbers as values. On the other hand, of course, the class of bijective mappings (say, of real numbers in the interval (0,1) onto themselves) will certainly be of cardinality greater than \mathcal{N}_{0} (the cardinality of a denumerable set).

The point of this observation is to explain how Frege's assumption that the domain of numbers is well-defined by the numerical operator involves a circularity analogous to that which besets the other operators. In this case, though, the argument relies not upon generation of the Russell paradox, but upon what has been called the "Skolem paradox" of predicate logic and first-order set theory.

The Skolem paradox is a consequence of the Downward Skolem-Löwenheim Theorem which establishes that, if a theory (set of sentences) of predicate logic or set theory has a model at all (if they turn out 'true' under some interpretation), then it has a model in a domain of cardinality N_0 . This holds even though the theory itself may contain or imply a theorem (for example, Cantor's Theorem) which, in effect, asserts that there are nondenumerably many objects. It is therefore apparently paradoxical that sentences which, in the theory, are true of non-denumerably many objects can be interpreted as true in a domain of only denumerably many objects. The result is really but a seeming paradox: if we inspect the sentence which, in the theory, expresses Cantor's theorem, and which we have paraphrased as "there are non-denumerably many objects", then we can observe that, more precisely, the sentence asserts the absence of bijective mappings from the set of all subsets of \mathcal{N}_0 onto the set \mathcal{N}_0 of natural numbers. It is hardly surprising if this is true in some denumerable domain of objects. Such a domain, being countable, is bound to be short of bijections.

The implications of the Skolem paradox are of more consequence for the introduction of the domain of numbers in Gg. To assign a number to a concept, on Frege's view--as a value of the second-level operator " $\Re \phi(\xi)$ "-is to enumerate a set (course-of-values or extension); i.e. to give a oneto-one mapping of the set (of objects falling under the concept) onto a particular enumerable set (a finite set of integers from 1 to n, or the infinite set of numbers N). The Skolem paradox shows that it may be possible for the subsets of the domain of numbers (the numbers of concepts) to be enumerated from outside a theory, as it were,; and yet be non-enumerable from within the theory because no enumerating subset of correlated pairs is among the sets definable in the language of the theory. If we suppose, here, that the theory in question is constituted by the axioms, rules, theorems and operations of Gg., then the formation in Gg. of equinumerous extensions, with which to identify the values of $\mathcal{Q} \phi(\xi)$, has to be accomplished by considering the set of axioms, operators and derived notions of Gg. as a whole and so interpreting them that, if the set N (of natural numbers) is definable in the language of Gg., then the set of its subsets 'P*(N)' exceeds it in cardinality (Cantor's Theorem). But to construct the set P*(N) may not be possible using only those operations provided by the theory Gg.; so

that this theorem--that there is a non-denumerable set--becomes relative to the cardinality of the domain in which the axioms of the theory employed in proving the theorem are interpreted.⁴⁴ (Briefly stated: there may be no concept-expression formulable in the language of <u>Gg</u>. which can only be interpreted as having a non-denumerable extension--for, if there were, some set of sentences of <u>Gg</u>. would be satisfiable only in a non-denumerable domain; a possibility ruled out by the Downward Skolem-Löwenehim Theorem).

If we pursue this line of reasoning further, we can detect in Frege's use of the numerical operator a recurrence of that circularity which vitiates the conception of the totality of objects formed by means of the operators $\hat{x}(\phi x)$ and $\xi \cap \xi$. In this case the effect of the circularity is to militate against the conception of the domain of numbers of concepts as a welldefined infinite totality. The circularity becomes evident when we pose the question whether there is any guarantee that the mapping effected by $\Re \phi(\xi)$ of concepts onto equinumerous extensions will generate the infinite set N. There seems to be an obvious guarantee in the fact that the presumption of closure of the totality under $\Re \phi(\xi)$ and $\widehat{x}(\phi x)$ is so intended that all objects which are either the number 0 (the extension of the concept "equinumerous with $\sim (\xi = \xi)''$ or belong to the f-image of $\Re \phi(\xi)$ --where f is the successor function, will belong to the totality of objects; and only infinite totalities satisfy this condition (see: Gg. Sections 41-43). Unfortunately, the f-images of $\mathfrak{A}\phi(\xi)$ will form an infinite totality only if the set of permissible substitutions into " $\Re \phi(\xi)$ " is infinite (i.e. only if there are infinitely many first-level concepts to have numbers). This, in turn, will depend on whether the initial totality of all objects is

infinite. That is, whether there are infinitely many first-level conceptexpressions as substituends to " $\Re \phi(\xi)$ " depends upon their being infinitely many complete expressions from which expressions for first-level concepts are formed. But, since our initial question concerned whether the totality of objects contained an infinite set--to be the f-image of $\Re \phi(\xi)$, we have come full circle.

Frege's only means of showing the totality of objects to contain an infinite set of numbers is by reference to the values of the operator $\Re \phi(\xi)$ it contains. This involves an appeal to there being infinitely many extensions of equinumerous first-level concepts. This, in turn, is guaranteed only if the enumerations of subsets of the totality of objects generates an infinite set; i.e. only if the totality of objects is at least denumerably infinite. Hence, the circularity could be avoided if it could be shown that there were infinitely many objects in the totality even when the notion of 'object' was explained <u>without</u> reference to the formation of extensions and numbers by means of $\widehat{x}(\phi_X)$ and $\Re \phi(\xi)$. The question remains then: can this be shown independently of construing extensions and numbers as objects?

One might suppose that, in answer to this question, Frege can point immediately to the fact that any partition of the totality of objects into sets of values of first-level concepts must already generate an infinite set. The reasoning might proceed as follows: if the totality of objects contains material objects <u>a</u>, <u>b</u>, <u>c</u>,....-as referents of simple names--then it also contains all results of applying first-level functions which yield abstract properties of <u>a</u>, <u>b</u>, <u>c</u>, ...; e.g. "the colour of <u>a</u>", "the mass of <u>a</u>", "the shape of <u>b</u>", "the dimension of <u>c</u>", and so on. As Frege proposes

in <u>The Foundations of Arithmetic</u> (1884), it is a feature of the use of terms for abstract properties like these, that the criterion of identity associated with that use can be given by means of an equivalence-relation defined over objects of the kind for which the simple names "a", "b", "c",... stand. That is, to identify the values of such first-level functions is to determine an equivalence-class to which each argument belongs, relative to some equivalence-relation associated with the introduction of such complex terms. So, the range of values of the function "the mass of §" can be identified with the equivalence-class of the relation " ξ is equal in mass to ζ ". The range of values of the function "the dimension of §" is taken as the equivalence-class of the relation " ξ is isometric to ζ ". In general, the proposal continues, every such first-level function has a range of values identifiable with the equivalence-class of the equivalencerelation in terms of which the function is introduced.

The argument that the partition of the totality of objects into sets of values of such first-level functions guarantees that the totality is infinite can thus proceed. If we start with some totality $T = \{\underline{a}, \underline{b}, \underline{c}, ...\}$ which, let us say, is finite, but contains more than one member, and we form the set of equivalence-classes under any equivalence-relation defined over T, then we obtain P*T--the set of all non-empty subsets of T (also called "the restricted power-set of T"). But Cantor's Theorem, P*T has a greater cardinality than T, if T contains more than one member. If we form next the union of T with P*T--its restricted power set--we obtain a still larger set. Finally, forming the union of the <u>n</u>-fold iteration of this operation of adjoining the restricted power set of each resulting set--for all finite <u>n</u>--then we obtain a denumerably infinite set. No totality is closed under this operation since the cardinality of the resulting set is always increased; i.e. $\overrightarrow{P*T} > T$ if $\overrightarrow{T} > 1$. It appears, then, that here we have the guarantee required that we shall never, as it were, run out of first-level concepts to serve as arguments to $\Re \phi(\xi)$, which will yield the partition of T into denumerably many equinumerous extensions as values. The set N of natural numbers will thus constitute a well-defined infinite totality.

The answer which I have constructed around Frege's use of equivalence relations to determine the values of first-level functions is unfortunately not adequate. It fails precisely at that point where it supposes the equivalence-classes formed under some partition of T to be always distinguishable from elements of T; i.e. that if we start with a given finite totality and then introduce new abstract objects construed as equivalenceclasses of objects from the original totality, then these new objects can be differentiated from members of the totality in every case. There is nothing in Frege's conception of extensions as objects, however, which will enable us to do this. That is, though there is nothing wrong intrinsically with the introduction of equivalence-classes as the values of first-level functions like "the mass of ξ ", "the shape of ξ ", the possibility of doing so rests upon our indifference to whether the values of the functions concerned are to be identified with or differentiated from objects referred to by other means. For, the proposal to treat such abstract terms as "mass", "shape", "direction" and so on as referring to equivalence classes under some existing partition of the totality by means of equivalence-relations,

makes sense only provided that we have a sufficiently rich domain of classes, already, with which to make such identifications. But we have no such guarantee that the initial totality already contains sufficiently many classes; unless we assume that the totality of first-level functions yields denumerably many classes (courses-of-values) as values of $\hat{\mathbf{x}}(\mathbf{0}\mathbf{x})$. And this, finally, was just what was at issue in the original question.

It would seem, from later unpublished writings of Frege's (1919-1925) commented upon by Dummett, Frege, Philosophy of Language, Duckworth, London, 1973, p. 663, that Frege came to recognise the circularity of his presumption that the closure of the set of objects under the numerical operator would yield a well-defined infinite set to be the set N of natural numbers. He remarks, there, that to ground the theory of number, we have to be assured of the existence of infinitely many objects, something which is not guaranteed by iterated application of the function "the number of the concept \emptyset " to yield equinumerous extensions. Indeed, there are further remarks, there, which seem to indicate that Frege came to abandon the treatment of extensions as 'proper' objects entirely.

Perhaps, then, it is gratuitous to argue that Frege's \underline{Gg} . is subject to limitations which beset, equally, most approaches to the problem of finding a logically rigorous foundation of mathematics. As will be noted in the next section, Russell faced an analogous problem over providing some guarantee that infinitely many classes of classes could be generated in the type theory of <u>P.M.</u>, to comprise the set of numbers. His "solution" was to adopt an axiom to that effect. Nevertheless, to argue for the claim's gratuity is to miss the argument's point--which is not that Frege requires an assumption
or axiom to the effect that the totality of objects is infinite, to free the use of the numerical operator from circularity. The point is, rather, that Frege's presumption that any domain can be specified by forming its closure under arbitrary term-forming operators, and that a domain so specified will capture the set of numbers, is an <u>illicit</u> presumption, because the specification of the domain is circular--hence, simply stated, such a presumption fails to specify a determinate domain.

This concludes the systematic attempt to reveal the impredicativity at the core of Frege's semantics. My argument has not been concerned to offer criticisms of <u>Gg</u>. with a view to rejecting it in favour of some other set-theoretic foundation for mathematics. My concern has been, rather, to show precisely how the logic of the <u>Gg</u>. exhibits that vicious circularity-in its most fundamental semantic notions--which results in inconsistency; and upon which Russell and Poincaré were to fix, subsequently, as responsible for the paradoxes.

I have shown that this circularity results from two doctrines which are basic to Frege's semantics--complete definition of all concepts and the treatment of extensions as proper objects. In arguing, thus, for the general bankruptcy of the semantic views on which Frege's theory of extensions was based, the ground is prepared for the introduction of the more restrictive type-theoretic accounts which are the concern of the next section.

Section C: The Theory of Logical Types (I) Preliminary Questions:

Frege's specification of the totality of objects is impredicative and this impredicativity generates inconsistency in the semantics of Gg. But what makes a specification of a totality 'impredicative'? and how does the realisation of a ban on impredicative totalities by means of Russellian type theory remove the threat of inconsistency? What kind of theory is type theory (of what is it a theory)? and what justifies the formation of type-heirarchies and classification of things into types? These are the questions which arise naturally from the discussion up to this point. It is the task of this section to answer them. In particular, my exegesis will be focussed upon the following: to decide what is being termed 'impredicative' in Russell and Poincare's appeals to VCP and to examine Ramsey's and Gödel's criticisms of those appeals; to show how the theory of types develops from changes in Russell's theory of meaning (between 1903 and 1913); and to substantiate the claim--made in Section A--that Ramsey's simplification of type theory is successful only to the extent that (i) his revised notion of 'predicative, propositional functions' is coherent; and (ii) his separation of the logical from the semantic paradoxes is justified.

Following this review of the development of type theory within Russell's mathematical logic and leaving on one side the question of the adequacy of type theory within the logicist philosophy of mathematics, I

shall concentrate next upon the critique of Russell's conception of logic begun by Wittgenstein in 1912. In particular, I examine in Appendix A to this Section Wittgenstein's criticisms of the foundations of ramified type theory--Russell's 'multiple-relation' theory of judgement. The section concludes, therefore, with the first discussions of the use of type theory to circumscribe the limits of descriptive significance in Wittgenstein's logical atomist doctrines of 'correct symbolism' and of 'what can only be shown'.

To begin: let us return to the diagnosis of the source of the paradoxes at which Russell had arrived by 1908---which I mentioned first in Section A when stating the Vicious Circle Principle. In Russell (1908), Russell reviews the paradoxes and then concludes:

All our contradictions have in common the assumption of a totality such that, if it were legitimate, it would at once be enlarged by new members defined in terms of itself. This leads us to the rule: 'Whatever involves all of a collection must not be one of the collection'; or conversely: 'If, provided "a certain collection had a total, it would have members only definable in terms of that total, then the said collection has no total'. And when I say that a collection has no total, I mean that statements about all its members are nonsense. (Russell, 1908; repr. in Russell, 1956, p. 63).

Again, in introducing the theory of logical types in PM (Introduction to First Edition, Chapter II), Russell restates VCP:

An analysis of the paradoxes ... shows that they all result from a certain kind of vicious circle. The vicious circles in question arise from supposing that a collection of objects may contain members which can only be defined by means of the collection as a whole. Thus, for example, the collection of propositions will be supposed to contain a proposition stating that 'all propositions are either true or false'. It would seem, however, that such a statement could not be legitimate unless all propositions' referred to some already definite collection, which it cannot do if new propositions are created by statements about 'all propositions'. We shall, therefore, have to say that statements about 'all propositions' are meaningless. (loc. cit., p. 37).

Finally, in summarising his analysis of the paradoxes as resulting from vicious circle fallacies, he remarks:

In all of them, the appearance of contradiction is produced by the presence of some word which has systematic ambiguity of type, such as 'truth', 'falsehood', 'function', 'property', 'class', 'relation', 'cardinal', 'ordinal', 'name', 'definition'. Any such word, if its typical ambiguity is overlooked, will apparently generate a totality containing members defined in terms of itself and will thus give rise to vicious circle fallacies. (PM, Introduction to First Edition, C. II, Section VIII, p. 64).

Three questions which confront us immediately are:

(i) How did Russell argue from the fact that the paradoxes result from vicious circle fallacies to the conclusion that assertions about 'impredicative' totalities are "meaningless" (rather than, say, merely false)?

(ii) How was the diagnosis of the source of paradox as violation of VCP to support the <u>positive</u> solution to the paradoxes--type theory?

(iii) What support could be found for the theory of types over and above the fact that it blocked the vicious circle fallacies?

In terms of the above quotations, Russell's argument seems to be that, ordinarily, it seems possible to define new members of certain collections by speaking of <u>all</u> members of those collections. For example, from Russell, the proposition "all propositions are either true or false" seems to be a proposition. As such, it is included in the totality of all propositions. Similarly, the property of having all properties not specifiable in fewer than nineteen words seems to be a property specifiable in fewer than nineteen words. And the class of all non-self-membered classes seems to be a class which may or may not be among the non-self-membered classes. In such cases, though, to be a member of the totality (of propositions, properties or classes), so defined, leads to contradictions.¹ So, such 'new' members of the totalities cannot exist; it is never possible to define them in terms of the totality, and, hence, it doesn't make sense to speak of <u>all</u> members of such totalities.

A moment's reflection on this reconstruction of Russell's argument, however, makes it clear that the conclusion goes far beyond what follows immediately from the premises. Apparently, Russell argues from the fact that <u>some</u> attempts to define new members of a totality in terms of all members lead to contradictions, to conclude that <u>all</u> attempts to define new members in those terms are illicit. Secondly, from concluding that new members cannot be <u>defined</u> in terms of all members, he asserts, without further argument, that we cannot do anything by speaking of "all members"; i.e. that speaking of "all members" of the totality is meaningless. Why should the fact that some specifications of members of a totality which involve reference to all members of the totality lead to contradictions justify our concluding that universal quantifications over the members of that totality are meaningless?

The question turns on what we take a <u>definition</u> or specification of a totality or 'class' to be. Consider, for example, an assertion about all propositions--similar to Russell's own illustration in the second quotation above:

(1) All propositions are false.

If (1) is included among the totality of propositions it is about, then (1),

if true, is false. If (1) is false, however, it follows only that some propositions are true. (1), if true, is false; and, if false, is false. So, (1) is false. Why should we suppose it meaningless?

Similarly, consider again the defining characteristic of the class which issues in Russell's paradox. This is the class w such that:

(2) "(x) (x \in w. \equiv .x \notin x)"

and the paradox comes from instantiating the universally quantified 'x' to w. The thrust of VCP, it is suggested, is to make it illegitimate to include w, or any class whose specification might "involve" or "presuppose" w, in the range of the quantifier "(x)". Should we not say, following Quine, that it is simply false to suppose there is any class satisfying (2), because the supposition that w exists leads to contradiction? Why should we suppose (2) is meaningless?

Finally, is it always illicit to specify an object as a member of a class if, in specifying that object, we have to make reference to all members of the class? If so, then there has to be something illicit in singling out the typical Oxford student as one whose exam results are nearest the average of all exam marks at Oxford, including his own. Yet, there seems nothing visibly illicit in such a specification; it strikes us as very odd to suggest that such specifications are meaningless.

These criticisms of the use of VCP to reject impredicative definitions as meaningless are essentially those of Ramsey, Gödel and Quine.² Ramsey's criticisms are a part of his general attack upon Russellian ramified type theory. They are necessary to his proposed simplification of type theory--described in outline in Section A--only to the extent that VCP and the notion of impredicativity support a ramified type theory, but not the modified simple theory which Ramsey develops.³ Godel's arguments extrapolate from points which arise naturally from Ramsey's simplification⁴; and Quine provides a review of those arguments---adding to them a rejection of Russell's contextual elimination of classes as based upon use/mention confusions (also outlined in Section A). It is natural, therefore, to assess the force of these arguments in the context of more detailed exposition of the ramified type theory of PM and Russell's reasons for its construction.

In responding to these criticisms, it has to be admitted immediately that there are occasions where Russell writes as if the <u>mere</u> fact that a definition, or specification of a class, leads to contradiction is sufficient to declare the definition or specification meaningless. There is then every reason to object that what leads to contradiction is properly deemed 'false', not meaningless. (One example--quoted above from PM, p. 37--is Russell's remark that "we shall <u>therefore</u> have to say that statements about 'all propositions' are meaningless" (my emphasis), as if its being meaningless <u>followed</u> from the circularity of its specification. Another example occurs in Russell's <u>1908</u> discussion of the Liar Paradox which he concludes:

It is useless to enlarge the totality (of all propositions) for that equally enlarges the scope of statements about the totality. <u>Hence</u> there must be no totality of propositions, and 'all propositions' <u>must be a meaningless</u> <u>phrase</u>. (p. 62--my emphasis)

In these cases, and others, where Russell argues as if simply to specify a class which, ceteris paribus, leads to contradiction, is to violate VCP

and to assert something meaningless, then what I shall refer to as the "Ramsey arguments" are <u>valid</u>, and Russell has no justification for this inference.

I shall first <u>state</u> my replies to the Ramsey arguments (with Godel and Quine's additions) in order that they may be more clearly traced and substantiated through the long exposition of Russell's development of ramified type theory, and discussed in subsections (V) and (VIII):

(a) Excepting occasions like the above (which might, in fairness to Russell, be discounted as 'oversights'), Russell was frequently emphatic in his demand that the mere diagnosis that paradoxical constructions involved vicious circle fallacies was insufficient to 'solve' the paradoxes. This contrasts with Poincaré's use of VCP--Poincaré simply rejected 'impredicative' class specifications (together with their committment to actually infinite totalities) <u>because</u> such specifications generated paradoxes through their circularity. On the contrary, Russell insisted that VCP must <u>itself</u> be explained as a consequence of a <u>positive</u> solution to the paradoxes which showed how paradoxical cases could be rejected as meaningless.

(b) The source of the paradoxes, Russell argued, revealed in the vicious circle fallacies, went much deeper than the theory of classes, and involved the basic notions of propositional functions and their ranges of significance. To remove the paradoxes, then, it was not enough simply to modify the notion of a 'class', or restrict the domain of classes axiom-atically. For, this would not show that impredicative definitions were meaningless. Instead, the positive account must demonstrate that "the exclusion (of impredicative definitions) must result naturally and inevit-ably from our positive doctrines, which must make it plain that 'all propositions' and 'all properties' are meaningless phrases." (Russell, <u>1908</u>, p. 63).

(c) Godel's claim has been that, whether or not impredicative definitions are regarded as viciously circular and, hence, illicit, depends on whether we take a 'realist' or 'constructivist' attitude towards the existence of classes.⁵ Quine has endorsed this claim and argues further that VCP does not reveal a <u>source</u> of the paradoxes but amounts only to a proposal to "thin the universe of classes down to the point of consistency" (Quine, <u>1963</u>, p. 243). In contrast, though I leave aside the question whether ramified type theory (with the controversial Axiom of Reducibility) provides an adequate foundation for a constructivist set theory as beyond the scope of my present enquiry, I <u>shall</u> argue that to construe Russell's positive theory thus is to misconstrue it. That is, I shall show that the ramified theory is not simply an adjustment to a set-theoretic foundation of mathematics, but, primarily, a natural consequence of Russell's mature theory of meaning (i.e. for Russell, of 'denoting') and of his theory of judgement. Only by locating the positive account in the theory of meaning can the claim that impredicative definitions are 'meaningless' be given content. This leads naturally to the examination of Russell's use of type theory in the analysis of significant predication.

(II): Origins of Type Theory:

Early in his quest for a solution to the paradoxes, Russell felt the need to call into question the existence of classes. In the <u>Principles</u>, (1903), he had already said:

In the case of classes, I have failed to perceive any concept fulfilling the conditions requisite for the notion of a class. And the contradiction ... proves that something is amiss, but what this is I have hitherto failed to discover. (Preface to Russell, 1903).

Russell proceeded to attempt the logicist reconstruction of mathematics without reference to classes until, as he wrote later in a letter to Jourdain, dispensing with classes "went well until I came to consider the propositional function \overline{W} , where $\overline{W}(\phi) = \phi \cdot \phi(\phi)$ [here, " ϕ " ranges over propositional functions]. This brought back the contradiction, and showed that I had gained nothing by rejecting classes."⁶ Nonetheless, Russell persisted in the conviction that some part of the trouble lay in the notion of a 'class'--even though he now knew that the contradiction infected his theory of propositional functions, also. By 1905 he had completed his Theory of Descriptions which he considered a partial breakthrough--not only for his theory of meaning (denoting); but also for the problem of the paradoxes. This connection between the Theory of Descriptions--which I take to be too well-known to bear repetition, here--and his search for a solution to the paradoxes is related by Russell in several places. In Russell, <u>1959</u> (p. 79), and subsequently in his <u>Autobiography</u> (<u>1967</u>, vol I, pp. 152, 177), he remarks that the Theory "was the first step towards solving the difficulties which had baffled me for so long"; and quotes a letter to Lucy Martin Donnelly of June 13, 1905, in which Russell writes:

This little puzzle (of whether descriptions denote in the way names denote) was quite hard to solve: the solution, which I have now found, throws a flood of light on the foundations of mathematics and on the whole problem of the relation of thought to thing.

In sum, there are three respects in which the Theory of Descriptions proved relevant to the quest for a positive solution to the paradoxes: (i) The contextual definition of descriptive phrases of the form "f($\neg x$) $\emptyset x$ " (corresponding to English: "the one and only one \emptyset is f") in terms of "($\exists y$) [(x) ($\emptyset x \equiv .x = y$) & f(y)]" is explicitly used in PM to facilitate the actual development of mathematics from the axioms. This use of the Theory I do not discuss further below.

(ii) The success of the Theory in eliminating phrases which appear to denote some definite object, from propositional contexts in which they cannot be said to denote anything (e.g. "The present King of France is bald") convinced Russell that he need not sustain his <u>Principles</u> theory of meaning which required that each word or phrase in a meaningful sentence stand in a relation of meaning ('indication') to constituents (terms) of a proposition which must have, in some sense, existence (or, at least, 'subsistence') as non-linguistic items. As Russell puts it:

What was of importance in this Theory was the discovery that, in analysing a significant sentence, one must not assume that each separate word or phrase has significance on its own account ... It soon appeared that class-symbols could be treated like descriptions, i.e. as nonsignificant parts of significant sentences. This made it possible to see, in a general way, how a solution to the contradictions might be possible. (Russell, "My Mental Development", in Schillp, <u>1944</u>, p. 14).

(iii) As is apparent from the above quotation, the Theory of Descriptions also afforded Russell an analogy between descriptive phrases like "the one and only one x such that $\emptyset x''$ and class-expressions like "the class of all x's such that $\emptyset x''$. Indeed, the Theory afforded more than an analogy, since it provided the crucial stimulus Russell needed for developing both the doctrine of 'incomplete symbols', which is the foundation for the theory of types, and the characteristic thesis that the overt grammatical form of a sentence need not mirror the logical form of the proposition expressed. This latter thesis was to guide much of Russell's later work on the logical atomist conception of the analysis of propositional significance and the form of the propositions of logic. At this early stage, even, the relation of a word or phrase to its meaning is, for Russell, only one part of the problem of meaning. The significant relationships are the logical relations between terms and denoting complexes, qua constituents of the proposition. The success of the contextual elimination of apparently denoting phrases like descriptions led Russell to seek a method eliminating class referring expressions from the logic of PM by means of contextual definitions. This method is given in the key section *20 of PM defining class-expressions. In these definitions, the expressions occurring on the left of ' = df' are to be understood as abbreviating the expressions on the right. The exclamation points confine the propositional function variables to their left to "predicative functions". I shall discuss these shortly:

"*20.01 $f \{\hat{z}(\psi z)\} =_{df} (\exists \emptyset): \emptyset =_{x} \psi :: f(\emptyset =_{x}).$ *20.02 $x \in (\emptyset =_{x}) =_{df} \emptyset =_{df}.$ *20.03 C1s (Class) = $\hat{a} \{ (\exists \emptyset) : a = \hat{z}(\emptyset =_{x}) :$ *20.07 (a). fa. = $\hat{a} \{ (\emptyset) : f \} =_{df}.$ *20.08 $f \{\hat{a}(\psi =_{x})\} =_{df}.$ ($\exists \emptyset$): $\psi =_{a}. \emptyset = f(\emptyset =_{x}).$

To be sure, as Godel has pointed out (in Schillp, <u>1944</u>, p. 126), these contextual definitions require additional conventions governing the order in which defined expression are to be eliminated; especially where two or more definienda occur in the sentential context--but this complication can be ignored.

Of these three influences of the Theory of Descriptions the second ((ii), above) is the most intriguing when considered alongside Russell's demand that a positive solution to the paradoxes <u>show</u> why impredicative definitions are meaningless. For (ii) embodies a significant change in Russell's theory of meaning. Russell had already pointed out that the connection between vicious-circle fallacies and committment to classes turned upon the use of unrestricted variables in propositional functions which determine classes (which, as noted above, is <u>one</u> sense in which specifications of classes are 'impredicative'):

Thus we require, if the vicious-circle principle is to be verified, that classes should not be among the possible values of a wholly unrestricted variable, which is another way of saying that we require that there should be no classes. We cannot then give any meaning to the supposition of a class being a member of itself, and thus we escape the paradox. (Russell, <u>1906</u>, repr. & transl. in Lackey, <u>1973</u>, p. 210).

The question arises, then: how are restrictions upon class-variables in propositional functions to be a natural consequence of the account of propositional functions in terms of which class-referring expressions are contextually analysed? Such restrictions as are natural have to be derived from the nature of propositional functions (and, hence, of propositions), themselves. What is doubly unfortunate for the exegesis of type theory is, as Russell admits, that "the question as to the nature of a (propositional) function is by no means an easy one" (PM, p. 39); and that at least one critic of ramified type theory--Quine--locates his most telling criticism in Russell's confusion of sign with object in describing propositional functions (Quine, <u>1963</u>, pp. 245, 255 - 256; also "Russell's Ontological Development", <u>J.Phil.</u>, 63, 1966, 647-667; repr. in Klemke, <u>1973</u>, pp. 3-14: references are to pagination of Klemke, <u>1973</u>). I discuss this criticism in subsection (V), below.

(III): Incomplete Symbols and the Multiple Relation Theory: The basis for the ramified theory.

First, let us reconstruct the reasoning behind Russell's ramification of propositions and propositional functions into orders. The important step in this reasoning--one which Russell could not have made before his 1905 formulation of the Theory of Descriptions--is the inference from the description of propositions as (expressed by) <u>incomplete symbols</u> to the conclusion that propositional functions containing apparent (bound) variables must be of a radically different kind from elementary propositional functions; i.e. that the general judgements of logic and mathematics are different in kind from particular assertions of fact. This contrast between Russell's pre-1905 and post-1905 views on propositions is most strikingly brought out by considering his different accounts of propositions as the objects of an act of judgement. I shall summarise these accounts very briefly.¹¹

Prior to 1905, Russell had held the view that propositions--true or false--were the objects of judgement. Such objects were "transcendental", i.e. their being was not dependent upon the act of judging, believing or disbelieving, and they were <u>complex</u>, i.e. they contained constituent "terms" which, themselves, had being, in some sense; though they characteristically involved a "certain kind of unity, apparently not capable of definition, and not a constituent of the complexes in which it occurs" (Russell, <u>1904</u>, repr. in Lackey, <u>1973</u>, p. 62). (This "certain kind of unity" which is involved in the propositional complex appears, from Russell's discussion in Russell, <u>1904</u>, to be essentially that which I have characterised below as the propositional unity required to distinguish a proposition from a mere

<u>list</u>—see below p. 132). The act of judgement, then, involved a dyadic relation between an experiencing subject and an object—a true or false proposition:

The position we have now arrived at is that there are, apart from and independently of judgement, true and false propositions, and that either kind may be assumed, believed or disbelieved. (Russell, 1904, loc. cit., p. 74).

We may contrast this position immediately with the account given in 1910 in PM: (which parallels the "Multiple Relation" Theory of Russell, <u>1910b</u>, pp. 147-159)

When a judgement occurs, there is a certain complex entity composed of the mind and the various objects of the judgement. When the judgement is <u>true</u> ..., there is a corresponding complex of the <u>objects</u> of the judgement, alone. Falsehood, in regard to our present class of judgements, consists in the absence of a corresponding complex composed of the objects, alone. It follows from the above theory that a 'proposition', in the sense in which a proposition is supposed to be <u>the</u> object of a judgement, is a false abstraction, because a judgement has several objects, not one. (PM, p. 44).

The 'present class of judgements" to which Russell here refers are such as comprise those "of the same form as judgements of perception, i.e. their subjects are always particular and definite" (PM, p. 44)--an example of which is given as judging (say) "this is red". For such cases:

a judgement does not have a single object, namely the proposition, but has several interrelated objects. That is to say, the relation which constitutes judgement is not a relation of two terms, namely the mind and the proposition, but is a relation of several terms, namely the mind and what are called the constituents of the proposition. (PM, p. 43).

The truth or falsity of judgements from this class is then explained in terms of the presence or absence of a complex corresponding to the arrangement of objects in the judgement. An example will clarify this explanation: suppose a and b are particular, definite objects and I judge, say, that a is bigger than b. Now, Russell held that, provided a, b were simple objects which were present to me, in some sense, (which which I was "acquainted"), then I could simply perceive the complex consisting of a's being bigger than b i.e. "a-in-the-relation-bigger-than-b". When I judge that a is bigger than b, this judgement of perception--derived from the perception of the complex by attending to it -- is a relation of four terms: a, b, the relation 'being bigger than' and myself (as percipient). The perception itself, however, consists simply in a dyadic relation between the complex and myself. Since what I perceive when I see a to be bigger than b cannot be nothing, I cannot have perceived the complex 'a's being bigger than b' unless in fact a is bigger than b. Hence, my judgement is true, because there is a complex corresponding to the arrangement of objects in my judgement. Should there have been no complex corresponding to the arrangement of objects of my judgement -- even though there appears to me to be one--my judgement that a is bigger than b is false (c.f. PM, p. 43).

Judgements of this class, which involve only simple objects (of acquaintance) having qualities, standing in relations, and which are true when there is a complex corresponding to the judged arrangement of objects, false when there is no such complex, Russell called "elementary judgements". (p. 44). The objects of true elementary judgements can be called "elementary propositions" though it must be recognised that to speak thus is to use a "false abstraction", for the object of judgement is "not a single entity" (ibid.). This is what is meant by calling the phrase (sentence) which

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expresses a proposition an "incomplete symbol" (ibid., p. 44)--it has no meaning in itself (does not stand for a determinate entity), but requires some supplement (namely, being judged or asserted) to complete its meaning. I shall discuss 'incomplete symbols', shortly.

Not all judgements are elementary; so not all propositions are elementary---in particular, the general propositions of logic and mathematics are of a radically different kind from elementary propositions. It is in this classification of the separate kinds of proposition---based upon the kind of judgement involved---that the basis for the ramification of propositions (and propositional functions) into separate orders can be discerned. It is thus that we can follow how Russell had reasoned that the solution to the paradoxes---that statements involving "all propositions", "all properties", and so on, were meaningless--was to be a consequence of his <u>positive</u> doctrines of propositional meaning. What has to be shown, then, is how the doctrine that propositions are expressed by 'incomplete symbols'--a view which evolved from the contextual elimination of denoting phrases by means of the Theory of Descriptions---led to the ramification of orders of functions.

To deny that propositions are single, autonomous objects of judgement and to claim that they are expressed by 'incomplete symbols' which have no meaning on their own, amounts, for Russell, to saying the same thing: that, though we appear committed to the existence of such entities as are denoted by phrases like "the redness of this book", "the proposition that Socrates is human", "that a is bigger than b" which function as grammatical subjects of sentences, the analysis of these phrases in use shows that this

committment is illusory:

Whenever the grammatical subject of a proposition can be supposed not to exist without rendering the proposition meaningless, it is plain that the grammatical subject is not a proper name, i.e. not a name directly representing some object. Thus, in all such cases, the proposition must be capable of being so analysed that what was the grammatical subject shall have disappeared. (PM, p. 66).

The paradigm of an incomplete symbol is that of a descriptive phrase of the form "the so-and-so" occurring in a sentence in use. The occurrence of such a phrase, though apparently denoting an object, is analysed in the context in terms of expressions having no such denotational role. Thus, "The F is G" becomes "At most and at least one F is G". The analysis is such as to provide a meaning for all contexts in which such phrases occur. Such expressions, then, are <u>incomplete</u> in that they do not symbolise (have no meaning) on their own,; but only function in determining the truth or falsity of the whole sentential context in which they occur. Analogous reasoning lies behind Russell's treatment of expressions apparently denoting classes. Expressions for classes are also incomplete symbols, to be analysed out as making assertions about what satisfies propositional functions. The question thus becomes: can a proposition be said to be expressed by an incomplete symbol in this sense?

A sentence alone--considered as a string of symbols or phrases--is not an object of judgement (unless a constituent of a judgement <u>about</u> sentences). To have judged that Richard Nixon was dishonest is not necessarily to have uttered, nor even entertained, the sentence "Nixon is dishonest"-though it may have been to have asserted, or been prepared to assert of Nixon that he conspired to cover-up White House involvement in the Watergate break-in, etc.; and so judge him to have been dishonest. The important insight Russell achieved in embracing (by 1910) the theory of judgement as involving a 'multiple relation' between the subject and what enters into the act of judging (as opposed to a dyadic relation between subject and proposition) is to make 'propositions' -- conceived as the objects of intentional acts of judging or asserting--disappear on analysis. But 'propositions' fulfill a double purpose for Russell: they are what we "falsely abstract" as the objects of intentional acts; but they are also what sentences express (their 'meaning') and the bearers of truth-values. Even though Russell dispenses with propositions as the objects of intentional acts by means of the 'multiple relation' theory of judgement, this double role remains important. It connects the view of propositions as expressed by 'incomplete symbols' to the theory of judgement, through the claim that what 'completes' the sentential symbol is the contribution it makes to an act of judgement or assertion (i.e. the incomplete symbol becomes a proposition in being asserted).

The view of the proposition I shall develop, therefore, is to consider the proposition as the 'kind of judgement' made in the assertion of a sentence. This conception of the proposition supports the ramified heirarchy of propositions and functions through the classification of the kinds of judgement expressed in sentential assertions.

The basic distinction that has to be explicated is that which Russell draws between an "elementary" and a "non-elementary" judgement (PM, p. 44). This distinction is immediately problematic because, in PM, the separation of 'elementary' from 'non-elementary' judgements upon which the heirarchy

is based is made in terms of an epistemological theory about the judging subject's relation to the objects entering into his judgement. The order of Russell's argument is as follows: Russell maintains (ibid., p. 39) that the values of a propositional function $\oint x$ are presupposed by the function, and not vice versa. The values of $\oint \hat{x}$ are propositions $\oint a$, $\oint b$, $\oint c$,...; so in saying this Russell is committed to holding that the kind of proposition that is the value of a function is not determined by the function, but is presupposed by it. This is explained by noticing that differences in kind between propositions are reflected in differences in the judgements made through assertion of sentences which, in their assertion, express those propositions. Thus, differences in 'kind' (or "order") between propositional functions ultimately depend upon differences in the kind of judgements made. We have first to explain, therefore, differences in kind between judgements--and these differences are, for Russell, grounded in the epistemology of judgement.

In an elementary judgement--as noted above--the objects arranged in the judgement must all be objects of immediate acquaintance. In judging Sockates to be human (c.f. PM, p. 50), strictly speaking, I am not making an elementary judgement ('pointing to' a perceived complex, if true, and which 'contains' a perceived object and a perceived quality). The judgement is strictly non-elementary, if only because I am not 'acquainted with' Socrates (I do not directly perceive him). Indeed, Russell seems to suggest, in the above, that <u>only</u> Socrates is immediately acquainted with himself. I, like most of us, know Socrates only through descriptions that are true of him--"the Athenian who drank hemlock", "Plato's mentor",...-and an analysis of these descriptions shows that the judged proposition that Socrates is human is only apparently of the form 'Øa'. The occurrence of apparent (bound) variables in the fully analysed version of the proposition precludes its being elementary, in Russell's sense.

The problematic character of this distinction arises essentially from the epistemological difficulties over what can or cannot be an 'object of acquaintance'--particularly, from the difficulty of how communication in language is secured when what is an object of acquaintance for a speaker is not so for his audience. I do not believe that these difficulties in Russell's account can be resolved; but to argue this is beyond the scope of my present enquiry. I shall attempt, instead, to reconstruct the classification of kinds of judgement along lines with which Russell would certainly not have agreed, but which can provide an epistemologically less contentious ground for the ramified theory. This reconstruction is necessary if it is to be shown that ramified type theory is not vitiated by Ramsey and Quine's attacks upon it, and if the justification for Russell's positive solution to the paradoxes is to be carried forward into consideration of significance-failure and of category mistakes.

The reconstruction of the classification of judgements is based upon the following (lengthy) quotation from PM (Introduction to First Edition, Ch. II, Section (111), p. 44):

We will give the name of 'a <u>complex</u>' to any such object as 'a in the relation R to b' or 'a having the quality q' or 'a and b and c standing the relation S'. Broadly speaking, a <u>complex</u> is anything which occurs in the universe and is not simple. We will call a judgement 'elementary' when it merely asserts such things as 'a has the relation R to b', 'a has the quality q' or 'a

and b and c stand in the relation S'.... Take now such a proposition as 'all men are mortal'. Here, the judgement does not correspond to <u>one</u> complex but to many,... Our judgement that all men are mortal collects together a number of elementary judgements. It is not, however, composed of these, since (e.g.) the fact that Socrates is mortal is not part of what we assert.... We must admit, therefore, as a radically different kind of judgement, such general assertions as 'all men are mortal'. We assert that, given that x is human, x is always mortal That is, given any propositional functions $\emptyset x$ and Ψx , there is a judgement asserting Ψx with every x for which we have $\emptyset x$. Such judgements we will call general judgements.

The first distinction between kinds of judgement, then, is that between elementary and general judgements. It is connected to the ramified heirarchy of orders of functions in the following way: (for ease of exposition, at this point, I confine attention to assertions of 'atomic' sentences; i.e. sentences not containing connectives: negation, conjunction, disjunction or the conditional).

Consider a basic class of sentences which can be used to assert of particular items that they have qualities and stand in relations. Such sentences are commonly represented as of the forms 'F(a)', 'R(b,c)', 'S(d,e,f)',...; where these may assert, in use, that I am hot, that John is Mary's father, that Iago sees Cassio visit Desdemona. "a", "b", "c",.... occur in these sentences as names or singular terms referring to individuals. An <u>individual</u> by PM *9.131 (pp. 132-3 and c.f. p. 51) is anything which is neither a proposition nor a function. Since a proposition is expressed by an 'incomplete symbol' that has no meaning on its own, and a function is an "essentially ambiguous" expression (i.e. its meaning is given by its "ambiguously denoting" all the propositions expressed by

asserting some value of the function-see p. 40), we can regard the explanation of an 'individual' as equivalent to saying that a <u>name for an</u> <u>individual</u> is any expression which has meaning on its own, i.e. a <u>name</u> is a complete symbol. Within these basic sentences, "F, R, S,..." occur as schematic for n-adic predications true or false of individuals.

The judgements made in the assertion of such sentences are 'autonomous', in the sense that, ordinarily, to understand what is asserted by them, it is sufficient to know what would make them true or false. That is, nothing further has to be judged to verify or falsify what is expressed in asserting "F(a)" or "R(b,c)" save that the circumstances of a's being F, or b's bearing R to c, obtain or do not obtain. We can thus call what is thereby judged "elementary" (in a sense slightly different from Russell's) to indicate that, usually, to understand directly what is asserted is to know what circumstances count as making it true or false. Understanding "F(a)", that is, amounts simply to knowing that an assertion of "F(a)" is about a, asserts F of it and is true if a is F, false if a is not. This is a rather rudimentary description of a basic class of judgements expressed in the assertion of simple declarative sentences--but it will suffice for my purposes.

We may consider all such elementary judgements as subsumed under the forms 'F(a)', 'R(b,c)', and so on. Next, we may judge that some or all of a subclass of judgements of that form (all those, for example, whose expression asserts being F of some individual) are true. To do so is to make a different kind of judgement. Whereas the minimal support required for a judgement that F(a) is the particular circumstance of a's being F,

the support required for a judgement that every individual is F (or "everything is F") is that each of the elementary propositions expressed by "F(a)", "F(b)", "F(c)", and so on, is true; i.e. every such circumstance obtains. To express such a judgement, it is not enough to assert that F(a), generally--since "a" is now no longer occurring as a name, but as a placeholder for any of an arbitrary number of names. Properly, a variable "x" is demanded in the expression for that judgement.

What we intend in judging that everything is F is, thus, that any arbitrary <u>instance</u> of "F(x)" (where an <u>instance</u> is what results from replacing "x" by a name), when asserted, expresses a truth. We can no longer say that to understand what is asserted is to know what circumstance would make an arbitrary instance of "F(x)" true. For, there is no such circumstance. We understand by an assertion of "F(a)" only <u>this</u> a's being F--where a is some definite (non-arbitrary) individual. Judging that everything is F must differ in kind, therefore, from judging that a is F--being understood in different ways is indicative of their assertion being different in meaning. The next step in reconstructing Russell's argument is to apply this difference to propositions and functions.

A general judgement (that everything is F, that some F is G) involves a judgement about what satisfies propositional functions. This is Russell's analysis of quantifier-phrases:

Corresponding to any propositional function $\oint \mathbf{\hat{x}}$, there is a range, or collection, of values, consisting of all propositions (true or false) which can be obtained by giving every possible determination to x in $\oint \mathbf{\hat{x}}$... Now in respect to the truth or falsehood of propositions of this range, three important cases must be noted and symbolised ... Either (1) all propositions of the range are true, or (2) some propositions of the range are true, or (3) no proposition of the range is true. The statement (1) is symbolised by '(x). \emptyset x' and (2) is symbolised by '(\exists x). \emptyset x' ... '(x).~ \emptyset x' symbolises the proposition that every value of \emptyset X is untrue. This is number (3) as stated above. (PM, p. 15).

To assert that everything is F is, thus, to judge every value of the function $F\hat{X}$ to be true. Provided we understand what is involved in the assertion of an elementary proposition (namely, we are acquainted with the complex of which it is true or false), then, when the values of $F\hat{X}$ are confined to elementary propositions, we understand what is involved in the assertion of what Russell called a "first-order proposition". That is, since we know, for each proposition expressed in asserting $F\hat{X}$ of some definite object a, what makes it true or false, then we understand what is judged in asserting some or all such propositions to be true.

Similarly, if we consider all general judgements which are made in assering of each function Fx, Gy, H^2 ,... that some or all propositions in its value-range are true, then we can judge that some or all of these first-order propositions are true. That is, we can assert of a propositional function whose value-range includes first-order propositions that it is true in some or all cases. Such an assertion makes a still more general judgement than that some first-order function is sometimes or always satisfied; because this latter assertion is 'about' some or all first-order propositions. Provided the value-range of this <u>second-order</u> function is definite, then we understand what has to obtain for the judgement made thereby to be true. We understand what it is for each value of a first-order function to be true or false; whence, we understand what is involved in judging some or all such functions to be sometimes or always satisfied. Proceeding in an entirely analogous manner to functions of third and higher-orders (bearing in mind that the order of a function depends upon the order of propositions presupposed in its value-range-hence, upon the <u>type</u> of argument to the apparent (bound) variable in its assertion of a definite range of items), we generate the heirarchy of orders according to the kind of judgement expressed in asserting (i) an elementary proposition; (ii) a general judgement 'about' all or some elementary propositions; (iii) a general judgement 'about' all or some

For the purposes of this intuitive characterisation, though, I have described the "orders of generality" of functions, etc., as if at each order the only 'new' propositions asserted affirm all or some values of lower-order functions to be true. In fact, however, Russell conceived the ramifications of the heirarchy to be much more extensive. For I have discussed only <u>atomic</u> propositions, whereas the same distinctions apply to all kinds of <u>molecular</u> propositions (involving 'logical' functions: negation, disjunction,...). If "Fa" is an elementary proposition, then "(x).Fx" is one first-order assertion. There are also at first-order, assertions of <u>logical</u> functions which combine one or more elementary propositions into molecular judgements. Thus, if $p_0, q_0,...$ are elementary propositions, there is a first-order function 'Or(\hat{p}, \hat{q})' which presupposes the totality of elementary porpositions in being asserted <u>of</u> $p_0, q_0,...$ Thus, the judgement that- p_0 or q_0 , made in asserting 'Or(\hat{p}, \hat{q})' of p_0, q_0 , is true just in case there are perceived complexes that- p_0 or that- q_0 .

Similarly, if "Fa" is elementary, and "(x).Fx" is first-order (asserting all values of Fx), there is the "branching" function (of 'implication'; i.e. $'Imp_2(\hat{p},\hat{q})')$ which is of second-order, presupposing elementary and first-order propositions. Asserting $'Imp_{2}(p,q)'$ of an elementary and a first-order propositions yields the second-order proposition "(x).Fx \Box Fa". Since this last can be asserted generally (where " ϕ " is a variable taking elementary functions as arguments) we can assert it of any elementary function $\emptyset \hat{x}$, yielding: \vdash : (x). ϑx . $\supset_{\vartheta} \emptyset a$. Such "branching" of functions (over different orders of functions, or over functions, propositions and individuals) leads to a multiply complex heirarchy. By the doctrine of "systematic ambiguity", however, --which is discussed below, and in Appendix A to this section--it is rarely necessary to assign definite orders to 'logical' functions. Indeed, the import of the Axiom of Reducibility--also discussed below--is primarily to remove the need to consider the ramification of "branched" higher-order functions and their separate types of arguments.

This concludes the intuitive characterisation of how the ramified theory develops out of the twin theses central to Russell's theory of propositional meaning: the thesis that propositions are expressed by incomplete symbols through their being judged or asserted; and the thesis that differences in the value-ranges of propositional functions reflect differences in the kinds of judgement made in assertions involving those functions. Two points of discussion remain before I turn to the criticisms of this theory by Wittgenstein and Ramsey. The first point is that much more than the intuitive characterisation above is required to support the technical application of the theory to the logic of PM. Since I do not intend to examine in detail, however, the application of ramified type theory to the foundations of mathematics, I shall simply outline some of the technical aspects of the theory which prove relevant to later portions of this thesis. The second point arises out of the use of the ramified theory to declare paradoxical assertions meaningless. To explain paradoxes as violations of type requires the additional classification of types which serve as arguments to (not values of) propositional functions of a given order. It is necessary therefore to examine the relations between 'orders' and 'types'.

The intuitions behind the heirarchy of orders, I have suggested above, involve the kinds of judgement made (either elementary or general). Russell is appealing, thus, to the role of the proposition as the object of an intentional act of judging. By 1910, certainly, he did not believe that judgement had a <u>single</u> entity--a proposition--as its object. Nevertheless, the analysis of propositional orders as based upon kinds of judgement makes the assumption that, for the purposes of logic, he can speak of these "false abstractions"--propositions--<u>as if</u> they entered into judgements. If the heirarchy of orders concerns the kinds of propositions, viewed (falsely) as objects of judgement, the heirarchy of types is based upon consideration of the second role of propositions: namely, as what is expressed in the assertion of sentences, and as the bearers of truthvalues. That is, distinctions of order depend upon the kind of judgement made; distinctions of type concern the meaningfulness (significance) of what is expressed. The connection between the two kinds of distinctions

(really, one set of distinctions drawn in two ways) is simply as stated above: to deny that propositions are 'single entities' which are the objects of judgement, and to view propositions as (expressed by) 'incomplete symbols' which are 'completed' through being asserted, amounts, for Russell, to saying the same thing. What is expressed in the assertion of a meaningful sentence is what is judged in an act of judgement. Analysis of judgement and of propositional assertion reveal that their composite natures are the same whether described in the logical idiom of functions, argument and value, or in the epistemological idiom of perceived complexes, individuals, qualities and relations. (Indeed, in view of Russell's conception of logic, the modern separation of 'matters logical' from 'matters epistemological' is artificial when applied to the subject-matter of PM). This direct relationship between assertion and judgement, however, (essentially, then, between language and thought) is certainly obscured by Russell's construing assertion as a logical supplement to what is symbolised in a sentence (see: PM, pp. 8-9).

Russell had inherited the view that sentences require supplementation by a sign for their being <u>asserted</u> (as opposed to, say, being considered, or entertained) from Frege--the assertion sign ' \vdash ' of PM is Frege's own "content + judgement-stroke" of the <u>Begriffschrift</u>, Section 2. I shall have occasion below to consider briefly this conception of assertion--in examining Wittgenstein's criticisms of type theory in Appendix A. I believe Russell's conception to be mistaken: that an <u>act</u> of asserting is being carried out in the utterance of a sentence cannot be <u>symbolised</u> by a supplement to the array of symbols (words) which is the sentence uttered. In

supposing this could be symbolised, Russell confuses what is yielded by the verbal act which asserts something significant with the verbal expression for the performance of that act. (This criticism becomes important, in its effect upon Russell's theory of meaning, in the wider context of logical atomism where type theory is given a more general significance. I shall clarify the criticism in considering that wider context, and, subsequently, in Part II, Section B--where the speech act of 'assertion' (or 'statement-making') is discussed in detail). Despite this problematic conception of assertion, however, I shall seek to explicate the relation between assertion and judgement, as that is revealed in Russell's classification of types and orders. This will complete the examination of Russell's 1910 formulation of ramified type theory. The final task of this section will then be to consider the changes in the theory induced by Wittgenstein's criticisms of the theory of judgement and of propositional meaning, leading to Ramsey's simplification of the theory of types. (For ease of exposition I have confined detailed discussion of Wittgenstein's criticisms to a separate Appendix to this Section).

The theory of orders--of propositions and functions--reflecting as it did, differences in the kind of judgement made in assertions of sentences, realised Russell's intention of showing how the ban on 'impredicative' definitions (specifications of classes or functions which violated VCP) was to be a <u>consequence</u> of his positive doctrines. The theory realises this intention in the following way: (again, for ease of exposition, I confine attention to monadic propositional functions. The ramified heirarchy becomes notationally unwieldy when n-adic functions are included,

because of the need to assign (possibly different) orders to each of the n-variables in the expression). Each propositional function FX is welldefined provided that there is a definite collection of propositions which comprises its value-range. That the function <u>presupposes</u> the propositions in its value-range (PM, p. 39), amounts to saying--on the above interpretation of what it is for a proposition to be expressed by an incomplete symbol--that the judgements made in asserting any definite value of the function (where a "definite value" is what results from replacing the variable of the function by a name of the appropriate type) belong to one, determinate kind or order. We have already seen how <u>elementary</u> judgements are expressed in the assertion of definite values of functions all of whose arguments are individuals. The totality of individuals, therefore, comprises the lowest 'type'--though we should pause, momentarily, to reflect on the notion of 'individual' involved.

'Being an individual' was, for Russell, both an epistemological and a logical notion: an individual is an immediate object of acquaintance and is anything which is denoted by a 'complete' name (any name which, unlike propositional-, function-, description- or class-symbols, has meaning on its own). I have chosen to ignore the epistemological role of 'individuals' and have tried to reconstruct Russell's reasoning using the neutral description which requires that we paraphrase "whatever is denoted by a complete name is an individual" to meant "whatever can be symbolised, in that context, as being what an assertion is 'about' is, for the purposes of the symbolism, an 'individual item'". The notions of 'what an assertion is about' and of 'context of assertion', appealed to, here, are certainly vague--their

systematic explication is a chief concern of Part II--but the intuition behind the reconstruction is not entirely non-Russellian. For, to make the notion of an individual relative to the context of assertion embodies Russell's doctrine, discussed more fully, below, that assignments of types can always be regarded, in practice, as relative to a particular context in which a definition or proof is being symbolically given:

It is unnecessary, in practice, to know what objects belong to the lowest type, or even whether the lowest type of variable occurring in a given context is that of individuals or some other. For, in practice, only the <u>relative</u> types of variables are relevant; thus, the <u>lowest</u> type occurring in a given context may be called that of individuals, so far as that context is concerned. (PM, *12, p. 161).

That is, though the conception of individuals as the denotata of simple names is required for Russell's grounding of the heirarchy of orders in elementary judgements about objects of acquaintance (see: Appendix A, pp. $\forall i - \forall ii$), the only change involved in my reconstruction of Russell's theory is one of emphasis: to consider his suggestion, that only relative types need be considered, in practice, as a systematic requirement dictated by the context of assertion. There is, then, nothing intrinsically wrong with regarding, say, "The greatest common divisor of 169 and 338 in the interval $10 \leq x \leq 15$ is prime or divisible by three" as of the elementary form "Fa"--with everything before "is" being the singular term or name-save that the logical complexity of the assertion, and, hence, what could be derived from a more perspicuous symbolisation of it, would be disguised. The advantage of this different emphasis accrues primarily from its making us less inclined to conceive either the heirarchy of types or of orders as

absolute, in the sense in which this might lead us to believe that distinctions between individuals, properties of (functions defined over) individuals. and so on--embodied in the description of type theory--are somehow reflected in the nature of things. Not that the doctrine of 'relative types' settles the question whether type-distinctions hold of expressions or things-a question which is central to the next Section (D)--rather, the doctrine shows that the terminology of "individuals", "functions", "propositions", "classes", "relations", used in describing type-distinctions, need not commit us immediately to regarding differences of type as differences in 'things'. In sum, then, on this interpretation, there is not one heirarchy of orders and one classification as to type; there is rather, for each context in which an assertion or set of assertions is being considered, an heirarchical assignment of orders and types which guarantees that the context is "safe" from violations of VCP and from nonsignificance through violation of type. After these preliminaries, the next step is to show how "safe" heirarchies are generated.

Propositions containing bound (apparent) variables are expressed in the assertion of all or some values of a function of a given order. The values of a function of that order from a totality of propositions, presupposed by the function, none of which can contain bound variables in determining the range of which the function, itself, or any expression of that order, is employed. To speak, thus, of a proposition "containing bound variables" is to speak loosely. Properly, a proposition is not a single, determinate entity which can "contain" other items (expressions or things). Rather, as has been argued above, we are to construe the incomplete symbol--the

sentence--expressing what is judged truly or falsely in being asserted, as containing expressions to which we assign an order, in asserting a definite value, or some or all values, to be true. Propositions which "contain" no bound variables, in this sense, are called "elementary". They are regarded as subsumable under the elementary forms "F(a), R(b,c), S(d,e,f) ...". We can consider the expressions which result from replacement of one or more names from these forms by free (real) variables "F(x), R(y,x), S(u,v,w)..." as forms of elementary propositional functions. Then, the functions $F\hat{x}$, $R\hat{x}, \hat{y}, S\hat{u}, \hat{v}, \hat{w}$ --which presuppose different totalities of elementary propositions (monadic, dyadic, and so on) -- can be asserted to hold for some or all values. The resulting general propositions will be the first-order propositions, say, that everything is F ("Fx, always"), that every y is R to some z, that some u is the S of every v and w. Notice that, in a firstorder proposition, the "essential ambiguity" of the function---say, Fx--is a genuine constituent of the judgement expressed in asserting some values of Fx to be true. That is, in asserting "Fx, always", one judges the propositional function to be always satisfied (see: PM, p. 18). This fact, that functions 'enter into' higher-order judgements as amongst the objects of judgement, becomes crucial when the changes in Russell's theory of judgement, and of types and orders, is considered in the light of Wittgenstein's criticisms (see: Appendix A).

A form, as I have characterised the expressions above which contain only free variables, is called by Russell a "matrix" (PM, p. 162). Every function or proposition can be regarded as obtained from matrices of various kinds by means of 'generalisation'--asserting some or all values to be true (ibid.). This leads to the following recursion for generating "safe" heirarchies of order and type.

From the matrix "R(x,y)", for example, we can derive the four functions: (x).R(x, \hat{y}), (\exists x).R(x, \hat{y}), which are functions of y; and (y).R(\hat{x} ,y), (\exists y).R(\hat{x} ,y), which are functions of x. These functions can take only individuals as arguments (arguments of type-0) to yield first-order propositions as values. That is, first-order functions are obtained from firstorder matrices by generalising some, not all, individual variables. Firstorder propositions result from generalising all individual variables in a first-order matrix (PM, p. 163).

Where $f\hat{x}$ is a definite, elementary monadic function, " \emptyset !x" represents any value of <u>any</u> elementary, monadic function. Here, the \emptyset -symbol is a variable, and the exclamation-mark indicates that the function is <u>predicative</u> in that nothing other than elementary functions of individuals can serve as permissable substituends for the variable. Thus, " \emptyset !x" contains <u>two</u> variables " \emptyset ! $\hat{2}$ " and "x". Neither variable is bound; so the notation is a matrix--not, indeed, a first-order matrix, because " \emptyset ! $\hat{2}$ " is not an expression for individuals, but for first-order predicative functions. From this matrix, new matrices can be built up--all of which have first-order functions of one variable as substituends: " $\sim \emptyset$!a", " \emptyset !x. $\supset .\emptyset$!a",.... Such new matrices will be <u>second-order</u>, and, by generalising some or all of their variables, we obtain second-order functions and propositions. Second-order functions presuppose the totality presupposed by third-order functions (ibid.). The order of a function, therefore, is determined not by the type of its arguments (individuals, functions of individuals, and so on); but by the order of propositions presupposed in its value-range. Finally, thus, a function is said to be <u>predicative</u> if it is a matrix (PM, p. 164) or of order one greater than that of the permissable substituends for its argument variables. A first-order matrix--containing no bound variables--can take only names of individuals as substituends. A second-order matrix "f!(\emptyset !x)" has at least one first-order matrix amongst its argumentexpressions, but has no permissable sbustituends other than first-order matrices and names of individuals--and so on, for higher-order matrices.

Every function is derived from some matrix of a given order by considering the propositions which are assertions that the function in question is true for all or some values of one or more argument variables-other arguments being left undetermined. For example, the second-order matrix "f!(\emptyset !x)" contains no bound variables and is predicative. The collection of functions f!($\hat{\emptyset}$! \hat{z})--where "f" is a variable--comprise predicative functions of first-order functions. Thus, the proposition: $\vdash (\exists f). f(F!a)$, is second-order and is amongst the values of predicative third-order functions G![\hat{f} !($\hat{\emptyset}$! \hat{z})].

(IV): Impredicativity and the Ramified Theory:

It is important to show how this characterisation of predicative functions, within the heirarchy of orders, realises the ban on 'impredicative' specifications of classes, or, equivalently, of functions. VCP is stated in different ways through Russell's works on the paradoxes; but I shall concentrate upon only two such statements:

Given any set of objects such that, if we suppose the set to have a total, it will contain members which presuppose this total, then such a set cannot have a total. By saying that a set has 'no total', we mean, primarily, that no significant statement can be made about all its members. (PM, p. 37).
Whatever contains an apparent variable must not be a possible value of that variable. (Russell, <u>1908</u>; repr. in Russell, <u>1956</u>, p. 75).

Consider, now, two of the paradoxes which the ramified theory is supposed to solve: Russell's paradox and the paradox of the Liar (in the form in which Russell gives it). Russell's paradox may be stated either in terms of classes or in terms of propositional functions--and, in view of the contextual elimination of class-expressions (see above, p. 14,b), the latter must be regarded as more fundamental. The paradox begins with the supposition that there is a class $\hat{z}(\phi z)$ comprising all objects <u>a</u> satisfying $\hat{\phi}\hat{z}$, where $\hat{\phi}\hat{z}$ is true of an object <u>a</u> if and only if <u>a</u> does not satisfy itself. The first supposition is, then, that $\hat{z}(\phi z)$ satisfies the defining function ϕz ; i.e.:

(1) $\phi(\hat{z}(\phi z))$.

By *20.01, this is equivalent to:

(2) $(\exists \gamma t): \gamma t : z : \phi x : \phi(\gamma t : z).$

For (2) to be true or false, it must be true or false that some predicative function $\gamma'!\hat{z}$ of order n must be satisfied by every object satisfying $\vartheta \hat{z}$ and <u>it</u> must satisfy $\vartheta \hat{z}$, also ($\vartheta \hat{z}$ must be true or false of $\gamma'!\hat{z}$). The function $\gamma'!\hat{z}$, if predicative, presupposes a totality of values of order n-1, all of which are propositions which result from assertion of those sentences in which the variable "x" in " ϑ x" is replaced by an expression of the appropriate type. Amongst these expressions, " $\gamma'!z$ " must appear for the right-hand conjunct of (2) to be well-defined. But, for " $\vartheta(\gamma'!\hat{z})$ " to be well-defined, $\vartheta \hat{z}$ must be of order at least n+1--since $\gamma'!\hat{z}$ is of order

and

n. Otherwise " $\emptyset(\psi | \hat{z})$ " is impredicative. Since " $\emptyset(\psi | \hat{z})$ " is a propositional function of order n+1, it presupposes a totality of propositions of order n--namely, every proposition asserting a definite value of the predicative functions of order n. But, then, there can be no totality of propositions of order n-1 as is presupposed by ψ ! \hat{z} , since these were to be just those propositions which are asserted in substituting for the variable in "Øx", and to fix this totality requires that \emptyset_2^2 be of order less than n. Since the value-range of $\emptyset \hat{z}$ cannot be both of order less than and greater than n, it follows that $\phi \hat{z}$ is not well-defined and no proposition is expressed in asserting (1). When no true or false proposition is expressed in the assertion of (1), no definite judgement is made, and the incomplete symbol " $\phi(\hat{z}(\phi z))$ " is nonsignificant. Here, "to be nonsignificant" is equated, as Russell suggests, with "failing to express a true or false proposition" (see: PM, p. 48; and especially footnote *). The 'impredicativity', in this case, applies to the specification of the function \emptyset_2^2 and concerns; not simply the fact that the specification is circular (ϕ_2^2 presupposes the totality of propositions which result from substitutions into " $\emptyset x$ " of names including " $\hat{z}(\phi z)$ " which is defined, itself, in terms of $\phi \hat{z}$); but, primarily, that no significant proposition can be asserted about all or some values of $\emptyset \hat{z}$, since the totality of values of $\vartheta \hat{z}$ contains a proposition about $\vartheta \hat{z}$.

The Liar paradox can be given various formulations. The version Russell prefers is given in PM, (p. 60:

The simplest form of this contradiction is afforded by the man who says 'I am lying'; if he is lying, he is speaking the truth, and vice versa.

Though there may be some contraversy over the assumption that "lying" is

equivalent to "speaking falsely" (which is required to engender the contradictories: if he speaks falsely, he is speaking the truth, etc.), I shall consider only this formulation, here. The Liar paradox is also discussed in Part II, Section C.

Russell construes the assertion "I am lying" as interpretable as "There is a proposition which I am affirming and which is false". Since this is a general proposition, it asserts some value of the function "I assert $\hat{\rho}$ and $\hat{\rho}$ is false" to be true. This function presupposes a totality of propositions resulting from substitutions for "p". Let us say that this totality is of order m. Then the function is of order m+1, as is the proposition which asserts this function to be sometimes satisfied. This last proposition of order m+l is itself in the value range of the function "q is true", which must therefore be of order m+2. We are led, thus, to maintain that the right-hand conjunct in the first function of order m-namely, "p is false"--is of different order from the function "q is true" of order m+2. This entails, Russell argued, that truth and falsehood must have different meanings when asserted of propositions of different orders (PM, pp. 41-43). This, in turn, entails that the generic notions of truth and falsehood must be "systematically ambiguous" over propositions of varying orders. Properly speaking, Russell argued, since "I assert \hat{p} and \hat{p} is false" is of order m, the notion of falsehood involved should be restricted only to propositions of orders less than m. We can call this "m-falsehood". The function involved in the Liar paradox is thus: "I assert \hat{p} and \hat{p} has m-falsehood". This function is asserted to be sometimes true; i.e. not always false. But since this latter assertion expresses a proposition of

order m+1, then the function "not(\hat{q} is false)" involves not m-falsehood, but m+2-falsehood (of order one greater than that of the values of its bound variables). Since different orders of falsehood are involved in asserting "I assert \hat{p} and \hat{p} has m-falsehood" to be not always m+2-false, no contradiction ensues.

The impredicativity involved in the Liar paradox is thus shown to rest upon the supposition that the totality of propositions presupposed by " β is false' form a well-defined collection. Applying the second version of VCP, given above, to this analysis of the paradox, we can see that the circularity results from supposing that the general proposition asserting \hat{p} is false to be not always false occurs among the values of the apparent variable in " $(\exists p)$. p is false". The occurrence of the apparent variable in this proposition demands that it make a judgement of order at least one greater than any assertion in the value-range of " \hat{p} is false'. That is, asserting \hat{p} is false to be not always false makes a different kind of judgement from asserting a definite <u>value</u> of \hat{p} is false. No definite valuerange of a function is composed of judgements of different orders--thus, no significant proposition is expressed in asserting " \hat{p} is false' of itself.

In applying the ramified heirarchy of orders to these paradoxes, I have followed Russell in interpreting "nonsignificance" as "failure to yield a true or false proposition in being asserted". Since propositions are not single entitles which can be true or false, however, we have to reconstrue this latter as "not making a true or false judgement." For no judgement to be made in the assertion of " $\emptyset(\widehat{z}(\emptyset z))$ ", there must be arguments a to $\emptyset \widehat{z}$ for which " $\emptyset a$ " is meaningless--in the sense in which no definite

value of $\emptyset \hat{z}$ is asserted by asserting $\emptyset \hat{z}$ of a. Consequently, the arguments with which $\emptyset \hat{z}$ has definite values from a "range of significance (PM, p. 161), which is defined to be a "type". Whereas propositions, functions and matrices are divided into orders according to their presupposed value ranges, the arguments to functions are divided into types according as the function is significant over them (makes true or false judgements in being asserted of them). In this way, the ranges of significance of functions form a heirarchy, according as functions are significant over individuals, functions of individuals, functions of functions (and individuals), functions of functions and propositions, and so on--where the non-elementary judgements yielded by assertions of functions of the appropriate type of argument divide into orders according as their value ranges from well-defined, "predicative" totalities. (In characterising the heirarchy of types thus, however, as 'ranges of things' over which functions are significant, the problematic ambiguity in the notion of a function--between 'expressions' and non-linguistic items--reappears).

Confining attention, still, to PM, the heirarchy of types is given in three separate sections: in the Introduction (First Edition), Ch. II, Section iv; in *9 (pp. 127-137) and in *12 *13 (pp. 161-172). In the first of these, Russell offers the following argument for distinctions of type:

'(x). \emptyset x', ... is a function of \emptyset x; as soon as \emptyset x is assigned, we have a definite proposition, wholly free from ambiguity. But it is obvious that we cannot substitute for the function something which is not a function: '(x). \emptyset x' means ' \emptyset x in all cases', and depends for its significance upon the fact that there are <u>cases</u> of \emptyset x, i.e. upon the ambiguity which is characteristic of a function. This instance illustrates the fact that, when a function can occur significantly

as an argument, something which is not a function cannot occur significantly as argument. But conversely, when something which is not a function can occur significantly as argument, a function cannot occur significantly. (PM, pp. 47-48).

Two difficulties attach to this argument for distinctions of type. Firstly, the argument demonstrates only the need to separate functions from nonfunctions as differing in type (and not, say, functions of individuals from functions of functions). Secondly, the argument is infected with that confusion of expressions with what expressions stand for (denote), to which Quine has drawn so much attention.

Taking the second difficulty first: it is said to be obvious that "we cannot substitute for the function something which is not a function". What is substituted into any expression is clearly an expression (not what, if anything, the expression denotes); this accords with our interpretation of propositional functions, above, as essentially ambiguous expressions which mean (denote) nothing on their own, but mean something in being asserted of some definite object a, or in the assertion of their being sometimes or always true. Within three lines, however, Russell writes that when a function can occur significantly as argument, what is not a function cannot. The arguments to a function-as the above characterisation of the type-heirarchy makes clear-are not expressions, but what expressions denote. Individuals are arguments to predicative first-order functions. That is to say, first-order functions are asserted of individuals (not of names of individuals). Thus, for first-order functions to form the range of significant arguments to second-order functions, functions must be not expressions, but what such expressions denote. How damaging is this confusion?

(V): Criticisms of Russell's PM notion of a propositional function: Quine, Chihara:

[To specify Quine's criticism more precisely, and to appraise its effect, I shall consider only Russell's account of propositional functions in Ch.I and Ch. II (Section 2) of the Introduction to the first edition of PM (1910, pp. 14 - 19, 38 - 55). I cannot pretend that, through the period 1903-1910, Russell's theory of functions and, associated with it, his theory of propositional meaning, can be summarised briefly or easily. In addition, there are certainly accounts of functions in Russell's other works through this period which differ markedly from the account I consider. Thus, I shall not claim to have refuted Quine's criticism that, in different places, Russell makes use/mention confusions in describing propositional functions. I shall claim, however, to have indicated that the notion of a propositional function is not vitiated by such criticisms. (Indeed, I shall have occasion to argue in Part II that the standard account of the use versus the mention of expressions, upon which Quine's criticism is based, is itself a confused and confusing distinction-see: Part II, Section C).]

Though Russell had claimed in 1903 that the notion of a propositional function belonged to the indefinable notions of logic:

We may explain (but not define) this notion as follows:

 \emptyset x is a propositional function if, for every value of x, \emptyset x is a proposition, determinate when x is given. (Russell, <u>1903</u>, p. 19)

the relevant section of PM opens with what looks like a definition, albeit an informal one:

By a 'propositional function' we mean something which contains a variable x and expresses a proposition as soon as a value is assigned to x. (PM, p. 38)

Since a variable is a symbol of a certain sort (PM, p. 4), the definition suggests that propositional functions are <u>expressions</u>, linguistic items formed from sentences of various kinds by omission of names or denotingphrases (c.f. Frege's account of 'incomplete expressions', above pp. 87-9). This suggestion is confirmed by statements both prior to PM, and subsequent to it:

The undefinable of which I speak is the notion of an expression which contains one or more variables, such as 'x is a man' I represent by \emptyset !x every expression which contains x; ... Such expressions are propositional functions. (Russell, 1905(b), p. 261).

A propositional function of x is any expression \emptyset !x whose value for every value of x, is a proposition. (Russell, 1905(c), in Lackey 1973, p. 136).

and, finally:

Whitehead and I thought of a propositional function as an expression containing an undetermined variable and becoming an ordinary sentence as soon as a value is assigned to the variable: 'x is human', for example, becomes a sentence as soon as we substitute a proper name for 'x'. (Russell, <u>1959</u>, p. 124).

These statements make it very clear that propositional functions were to be expressions similar to Fregean incomplete expressions, or to what later came to be called "open-sentences". (What remains of a sentence like "John is the father of Mary" after the 'argument-expressions' "John", "Mary" are removed, is the open-sentence: "... is the father of ---").

Immediately following the PM definition, however, there is an instance apparently of just that confusion of sign with object which is the substance of Quine's criticism:

That is to say, a (propositional function) ... differs from a proposition solely by the fact that it is ambitious; it contains a variable of which the value is unassigned. (PM, p. 38).,

but there is every reason to suppose that, for Russell, a <u>proposition</u> is certainly not a linguistic item; it is not to be identified with the <u>sentence</u> which expresses it, but with what the sentence expresses or 'means', (c.f. Russell, <u>1903</u>, p. 47). How can the only difference between a propositional function and a proposition be that the former contains a variable when the one is a linguistic and the other a non-linguistic item? It seems, thus, that Russell has here either confused the function <u>qua</u> opensentence with what (if anything) the open-sentence expresses; or he has confused the proposition as the meaning of a sentence (what is expressed by a sentence in use) with the sentence, itself (what is mentioned by a quotation of the sentence).

There is some justice, then, to Quine's claim that Russell confuses sign with object; but what is the force of this criticism? Quine's argument proceeds: to just the extent that we identify propositional functions with open-sentences, so we may be misled into believing that the contextual elimination of class-expressions in terms of propositional functions effectively eliminates ontological committment to classes in favour of committment only to forms of expression---classes become a 'facon de parler' derived from our talk of what satisfies a propositional function. On the other hand, since it is more proper to correlate propositional functions with <u>propositions</u>--which are non-linguistic entities--the committment involved is to propositional functions as the 'meanings' of open-sentences. Since Quine finds the notion of the 'meaning' of, say, ' \hat{x} is a man', which he identifies with the attribute or property of being a man, more obscure than the notion of a class, then contextual elimination of classes succeeds only in replacing a relatively perspicuous notion by an obscure one.⁸

In responding to this criticism, it is first necessary to point out a further difficulty. Historically, though Russell certainly regarded propositions as non-linguistic items in 1903, the view is no longer clearly his after 1905. Certainly, at the conclusion of his 1905 article "On Denoting", in which the Theory of Descriptions is formulated, he insists that:

... in every proposition ... all the constituents are really entities with which we have an immediate acquain-tance. (Russell, 1905, p. 56)

which requires that we regard propositions as non-linguistic entities. Yet, in the same article, he speaks repeatedly of "... when a denoting phrase occurs in a proposition" (ibid., p. 50), and asserts that "the Tneory gives a reduction of all propositions in which denoting phrases occur to forms in which no such phrases occur." (ibid., p. 45). Since denoting phrases are <u>expressions</u> like "a man", "some man", "the King of England", propositions have to be construed as linguistic entities in order for such expressions to "occur in" them. By 1918, propositions, for Russell, are

definitely linguistic:

A proposition, one may say, is a sentence in the indicative, a sentence asserting something... (Russell, <u>1918</u>, p. 185).

and this seems to remain Russell's view through his 1919 article "On Propositions: what they are and how they mean", (repr. in Russell, <u>1956</u>, see espec. p. 308). The historical complication behind Quine's criticism, then, is whether propositions, on Russell's 1910 view, are linguistic items or not. For, if propositions are sentences, there is no conflict in supposing propositional functions (<u>qua</u> open-sentences) <u>become</u> propositions through provision of an argument-expression for the variable(s). Nevertheless, if propositional functions are to be open-sentences, simply, there will then arise severe problems for the contextual elimination of classes-for there are certainly less open-sentences than are needed to construct a proper domain of classes for mathematical purposes (particularly, real number theory).

In contrast, if we construe propositions as non-linguistic, and therefore require that propositional functions be analogously construed, there is little ground for following Quine in interpreting a Russellian propositional function-<u>qua</u> non-linguistic entity--as an 'attribute' or property. If a proposition is, in some sense, the 'meaning' of a sentence (what a sentence expresses), and a propositional function becomes a proposition by supplying a value for the undertermined variable, then there is good reason for supposing that Russell would not have meant by "propositional function" either attribute or property in the sense Quine intends us to take those terms. The case against construing propositional functions as attributes or properties can be made as follows: neither attributes nor properties can do the work Russell intended for propositional functions. Consider the dyadic function ' \hat{x} is father of \hat{y} '. Here I intend the function to be <u>whatever</u> is so related to the meaning of the sentence S = "John is father of Mary" that it becomes the proposition that S expresses when the names "John", resp. "Mary", are substituted into the <u>expression for</u> the propositional function. Since I need, in this case, to mention <u>both</u> the function and the expression for it (disregarding the question of what relation obtains between the two), I adopt the convention of using single quotes around an expression containing circumflexed variables to <u>mention</u> the function itself (as opposed to <u>asserting</u> it, for some or all values--c.f. PM, p. 40); whilst I retain double quotes for the mention of the expression for the function (or the sentence resulting from substitution of argumentexpressions).

The dyadic function ' \hat{x} is father of \hat{y} ' becomes the proposition that John is Mary's father (which is what S expresses) when "John", resp. "Mary", replace the variables in "x is father of y". Now Russell insists, as had Frege before him with respect to the senses of sentences, that we cannot analyse the meaning of S into constituent parts all of which can be picked out, or referred to by singular, referring expressions. Frege had maintained this in arguing that concepts must be essentially incomplete, 'unsaturated' entities in order that the sentence expressing that an object falls under a concept have a complete sense. Similarly, Russell, in analysing the proposition (qua sentence-sense) into 'subject of assertion' and 'what is asserted' insisted that:

... when a proposition is completely analysed into its simple constituents, these constituents taken together do not reconstitute it. (Russell, 1903, p. 83)

Russell repeats this claim in <u>1905</u> (pp. 49-50); the reasoning behind it can be reconstructed as follows: we would like to be able to describe the proposition expressed by S as composed of the meaning of "John", the meaning of "Mary" and the meaning of "is father of"--for the meaning of the whole sentence (the proposition) is properly regarded as dependent upon the meanings of its constituent parts. But, we cannot refer to (denote) the proposition expressed by S simply by saying it is composed of three items:

the meaning of "John", the meaning of "is father of", the meaning of Mary",

because to do so is to lose the essential 'propositional unity' which is expressed in saying that John is Mary's father, rather than, say, Mary is John's father, even though this latter is a proposition also composed of these three items.

On the other hand, Quine wants us to interpret non-linguistic Russellian propositional functions as attributes or properties <u>and</u> to maintain that Russell is ontologically committed to such 'deplorable' entities (see Quine's remarks in <u>1963</u>, p. 2). If Quine demands this interpretation and committment, then there is every reason to insist that we <u>must</u> be able to <u>refer</u> to attributes or properties, to pick such existents out, by the use of expressions like "the attribute of being a man", "the property of being human" and "the relational attribute of being someone's father". (This is an assumption for the sake of the argument: I do not understand what it would be like to be ontologically committed to something if I could not, in principle, pick out or refer to the things to whose existence I am committed, by means of some such expressions). Such expressions for attributes, we must suppose, <u>denote</u> attributes or properties. Therefore, to interpret propositional functions as attributes is to confront Russell with the very problem which his analysis of propositions in terms of functions, arguments and values was intended to avoid. The problem is that, if functions are attributes, then the proposition expressed by S is still the value of the function ' \hat{x} is father of \hat{y} ' for the arguments John, Mary; but now the proposition expressed must be said to be composed of:

(S) John, the attribute (property) of being a father, Mary. Similarly, the proposition expressed by S^1 (= "Mary is John's father") contains the same attribute and the same objects:

 (S^{1}) Mary, the attribute (property) of being a father, John. Neither (S) nor (S^{1}) , however, are propositions; they are merely <u>lists</u>. Not only has the essential propositional unity been lost; but there now seems nothing between the lists to differentiate the proposition that John is Mary's father from the proposition that Mary is John's father. As Russell expressed it:

... it is very difficult to regard xRy as analysable into the assertion R concerning x and y, for the very sufficient reason that this destroys the <u>sense</u> of the relation, i.e. its direction from x to y, leaving us with some assertion which is symmetrical with respect to x and y ... (Russell, 1903, p. 86)

I conclude that it is therefore mistaken to identify a Russellian propositional function with an attribute or property, on the grounds that we have either to abandon the analysis of propositions in terms of function, argument and value, or maintain that we can be ontologically committed to entities of the kind E without being capable in principle of referring to E's by means of phrases of the form "the E such that ..." or "this E which ...". The former alternative destroys Russell's theory of the proposition; the latter makes ontological committment wholly mysterious.⁹

If it is wrong to interpret propositional functions as non-linguistic attributes, can we regard them as 'open-sentences'? If so, does it then become proper to construe a proposition as somehow a linguistic item---a sentence (as Russell had come to believe by 1918)? I shall argue next that we cannot so construe Russellian functions---at least if we employ the term "open sentence" in the sense that Quine gives to it. My response to Quine's criticism, therefore, will be that the alternatives which he presents to Russell are unrealistic: Russell has not confused propositional functions <u>qua</u> open sentences with propositional functions <u>qua</u> attributes or properties because propositional functions cannot be either of these. It will remain, therefore, to reconstruct the notion of a Russellian propositional function so that it can function in the manner Russell intended.

The issues raised by the question whether a propositional function can be an 'open sentence', in the sense of Quine, prove to be fairly complex. They certainly extend far beyond the scope of the present section. Consider, firstly, Quine's explanation of the term "open sentence":

Expressions such as: (1) x is a book, x = x, x is a man \supset x is mortal; which are like statements except for containing 'x' without a quantifier, are called <u>open sentences</u>. They are fragmentary clauses neither true nor false as they stand ... Open sentences may, as notational forms,

be described as differing from various closed sentences only in lcaking a quantifier; or they may be described equally well as differing from various other closed sentences only in containing 'x' in place of a name of a specific object. (Quine, 1952, Section 17, p. 90).

Concentrating, for the moment, only upon the second characterisation: open sentences result from closed sentences by omission of names of specific objects. We may compare this with Russell's description of a propositional function:

Let $\emptyset x$ be a statement containing a variable x and such that it becomes a proposition when x is given any fixed determinate meaning. Then $\emptyset x$ is called a 'propositional function'; it is not a proposition since, owing to the ambiguity of x, it really makes no assertion at all. (PM, p. 14).

There certainly seems to be a close similarity between the two characterisation. We have only to construe "giving 'x' a fixed determinate meaning" as substituting for 'x' the "name of a specific object", and the Russellian propositional function is, to all appearances, a Quinean open-sentence. This appearance of similarity, though, is very misleading. Only a certain sub-class of Russellian functions take 'names of objects' (logically proper names) as argument expressions, and it is only to these "elementary propositional functions" that Quinean open sentences are similar.

An "elementary propositional function (PM, *1, p. 92) is a function containing variables such that when values are assigned to the variables the resulting value of the expression is an elementary proposition. And an "elementary proposition" (ibid., p. 91) is a proposition not involving "such words as 'all', 'some', 'the' or equivalents ..." In addition, any combination of given elementary propositions by means of negation, disjunction or conjunction will be elementary. Thus, no propositional functions containing variables bound by quantifiers or description operators (what Russell called "apparent variables") will be elementary. Now, Quine does not admit among the class of open sentences expressions containing quantified predicate-variables preferring a separate treatment of such expressions as "quantificational schemata" and "closed predicates" of varying kinds (where a <u>schema</u> is not to be confused with an open sentence, rather as an expression going proxy for both <u>closed</u> and open sentences and predicates--see Quine, <u>1952</u>, Section 23, pp. 127+; espec. p. 136). Since quantificational schemata are not open sentences, whereas non-elementary propositional functions are still genuine functions, then, <u>a fortiori</u>, some propositional functions are not open sentences.

The above argument concluded safely that some Russellian functions cannot be identified with open sentences in the sense of Quine--but no answer was given to the question why this identification cannot be made. It is at this juncture that syntactical matters of far greater complexity come to the fore. Though I shall trace what is problematic in this question, I cannot hope to give a complete account of these matters, here. Fortunately, I shall have occasion to take up these matters again in Part (I (Sections C-D) and will simply defer a fuller discussion until then.

Under what conditions can we turn a closed sentence into an open sentence? The answer from Quine is that either we omit a "name for a specific object" or we disregard the occurrences of quantifiers. In the latter case, Quine refers only, of course, to sentences which are already 'regimented' (to use his term), i.e. already translated into formal

symbolism, e.g.

(2) $(\exists x)$ (x is a book), (x) (x = x),

which read "Something is a book" and "Everything is self-identical". respectively. It would make no sense to attempt a direct conversion of ordinary language sentences to open sentences by omission of quantifierphrases, if only because "is a book", "is self-identical" are not sentences (open or closed) but predicates. The connection between the two ways in which open sentences are formed (omission of quantifiers or of names) is that the second occurrence of the bound variable 'x' in each of the examples (2) is to mark a position in the sentence which is either accessible to quantification or can be replaced by a singular term or name. The basic unit of Quine's logic--the atomic subject-predicate statement--is characterised by the union of the occurrence of a term 'a' in a position accessible to quantification with a general term 'F' in predicative position (which is not accessible to quantification), to yield the schematic closed atomic sentence 'Fa'. Omitting the singular terms 'a' (which Quine elsewhere argues to be eliminable from a fully regimented language¹⁰) gives the schematic open sentence "Fx". Whether this "Fx" is a mere notational variant of Russell's '\$ 'turns upon how Quine draws the distinction between positions in the sentence accessible or inaccessible to quantifiers, and how this compares with Russell's account of the "variable" element in the function of which "ambiguously denotes" the undetermined value ox of the function (PM, pp. 39-40). In fact, Quine's open sentence "Fx" and Russell's " $\oint x$ " are crucially different at precisely the point where Russell was led to invoke the ramification of types of propositional functions into different orders according to the bound variables they contained, Russell continues the argument of this section (p. 48) with the following illustration:

Take, e.g. 'x is a man', and consider ' $\oint \hat{x}$ is a man'. Here, there is nothing to eliminate the ambiguity which constitutes $\oint \hat{x}$; there is nothing definite which is said (i.e. asserted) to be a man. (ibid., my insert).

The ambiguity which constitutes $\emptyset \hat{\mathbf{x}}$, one must suppose, must consist in the fact that " \emptyset " is a variable which is confined, by the theory of orders, to a substitution-range comprising monadic functions of individuals. The circumflexed (bound) variable " $\hat{\mathbf{x}}$ " displays that the function is monadic and is significant over individuals. Hence, " $\emptyset \hat{\mathbf{x}}$ is a man" still contains a (real) variable and no definite judgement is made in asserting it. A function, then, cannot be substituted for the " \mathbf{x} " in " \mathbf{x} is a man" to yield a definite value.

C.S. Chihara (Chihara, <u>1973</u>, pp. 24-30) has argued that to make sense of this argument of Russell's, we have to interpret Russell's 'propositional function' as a Fregean concept, or attribute, which the opensentence " ϕ x" denotes. I have argued already that a propositional function cannot be an attribute (in the sense of "the denotation of ' ϕ x'"), and it is unlikely that a Russellian function can be a Fregean concept, <u>simpliciter</u>, in view of the impredicativity of Frege's demand that concepts be completely defined (see above: Section B). There are certainly analogies, however, that appear to hold between Frege's appeal to the 'unsaturated' nature of concepts and Russell's to the 'essential ambiguity' of the function. It is worthwhile, therefore, to clarify what is involved in this argument for dividing the ranges of functions into types.

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Chihara first reasons (loc. cit., p. 24) that, if " $\oint \hat{\mathbf{x}}$ is a man" asserts nothing definite, then the function ' $\hat{\mathbf{x}}$ is a man' cannot be an argument to ($\hat{\mathbf{x}}$ is a propositional function), since "($\hat{\mathbf{x}}$ is a man) is a propositional function" is as ambiguous as " $\oint \hat{\mathbf{x}}$ is a man". Hence, Chihara continues:

... one would think that, in all consistency, Russell and Whitehead should say that if $'\emptyset\hat{x}'$ denotes a propositional function, then $'\emptyset\hat{x}$ is a propositional function' is meaningless. But they don't. For them, $'\emptyset\hat{x}$ is a function' is not a statement containing an ambiguity; it is a true statement 'about an ambiguity' (PM, p. 40). (Chihara, 1973, pp. 24-25).

I believe the mistake in Chihara's interpretation can be located by closer attention to the passage following the illustration he cites-from which I quote at length:

When it is said that e.g. $\phi(\phi \hat{z})'$ is meaningless, and therefore neither true nor false, it is necessary to avoid a misunderstanding. If $\phi(\phi \hat{z})'$ were interpreted as meaning 'the value for $\emptyset \hat{z}$ with the argument $\emptyset \hat{z}$ is true', that would not be meaningless, but false. It is false for the same reason for which 'the King of France is bald' is false, namely because there is no such thing as 'the value for $\emptyset 2$ with the argument $\emptyset 2$ '. But when, with some argument a, we assert ϕ a, we are not intending to assert 'the value for $\emptyset \hat{\mathbf{x}}$ with the argument a is true'; we are meaning to assert the actual proposition which is the value for $\oint \hat{\mathbf{x}}$ with the argument a. Thus, in accordance with our principle that $\phi(\phi \hat{z})'$ is meaningless, we cannot legitimately deny 'the function (\hat{x} is a man) is a man' because this is nonsense, but we can legitimately deny 'the value for the function $(\hat{\mathbf{x}} \text{ is a man})$ with the argument $(\hat{\mathbf{x}} \text{ is a }$ man) is true', not on the ground that the value in question is false, but on the ground that there is no such value for the function. (PM, p. 41).

The values of the function ($\hat{\mathbf{x}}$ is a man) are propositions--so there is no proposition (nothing true or false) asserted in asserting " $\emptyset(\hat{\phi}\hat{z})$ ". The

mistake in Chihara's argument, then, results from his failure to take seriously Russell's denial that propositions are definite single entities (denoted by 'propositional descriptive phrases' in assertions like "A judges (the proposition that) Socrates is a man", "(The proposition) that Plato is a man is true"). Propositions are "falsely abstracted" as the objects expressed in asserting sentential incomplete symbols to yield the different kinds of true or false judgement. So an <u>incomplete symbol</u> is meaningless when, in being asserted, it yields no true or false proposition (fails to make a definite judgement). In the end, this analysis of propositions may not be coherent—it stands or falls on the analysis of judgement as involving multiple relations between the subject and the constituents of judgement, and on the analysis of 'incomplete symbols' which are completed in being asserted. But the coherence of the analysis as a theory of propositional significance is not challenged by Chihara's argument.

Russell's denial that propositions are single entities denoted by propositional descriptive phrases is made very clear in the quoted passage: "the value for $\emptyset \hat{z}$ with the argument $\emptyset \hat{z}$ is true" is false, <u>because</u> there is no proposition expressed in asserting $|\hat{\vartheta} \hat{z}|$ of $\hat{\vartheta} \hat{z}$, just as there is no present King of France (asserting " \hat{x} is bald" of what is uniquely a present King of France is false). In contrast, that " (\hat{x}) is a man) is a propositional function" is not meaningless, but a true assertion "about an ambiguity", does <u>not</u> result from taking the expression "x is a man" as <u>denoting</u> (as a name of) the propositional function <u>qua</u> attribute. For, the expression <u>is</u> the propositional function, and the means Russell adopts to mention the function (rather than <u>using</u> it to assert some definite value) is to represent its "essential ambiguity" by placing a circumflex over the variable:

When we wish to speak of the propositional function corresponding to 'x is hurt', we shall write ' \hat{x} is hurt'. (PM, p. 15)

Chihara (loc. cit., p. 38) interprets this convention as giving "a device for constructing 'names' of propositional functions." Such an interpretation is seriously misleading. If we conceive " \hat{x} is a man" as a <u>name</u> of some non-linguistic item (an attribute, property or Fregean concept)-recalling that Russell characterises an 'individual' as the denotation of a complete symbol (a name)--it is wholly mysterious how Russell could have claimed that a propositional judgement is yielded by <u>asserting</u>' \hat{x} is a man' of some definite object--say, Socrates--i.e. asserting some definite value of the function. For, it is a mystery how we, in uttering sentences, can <u>assert</u> non-linguistic individuals <u>of</u> individuals. If I assert F of objects a, b, c,..., F must be an expression (true or false of a, b, c,...)--I cannot <u>assert</u> attributes of a, b, c,... It follows that "(\hat{x} is a man) is a function" is not meaningless, but true in asserting' \hat{y} is a function'<u>of</u> what is essentially ambiguous, of a given order, and defined over a type; i.e. the expression "x is a man".

In rebutting Chihara's argument, further light has been shed upon the notion of a type and its relation to the ramification of orders in the 1910 version of type theory in PM. The range of significance of a propositional function is the set of items of which the function can be asserted to give true or false propositions as values. The 'values' yielded in asserting functions of arguments are not, of course, autonomous entities -- propositions -- which, as such, are somehow supposed to have the distinctive (but systematically ambiguous) "properties" of being true or false. The doctrine of the systematic ambiguity of truth and falsehood, like the analogous ambiguity of negation, disjunction, implication and the other 'logical functions' of PM (see: pp. 46-47), derives not from their "corresponding to" different properties of objects (propositions, attributes, etc.), as these 'objects' are somehow sorted into orders and types. That would be nonsensical. The systematic ambiguity of these notions derives from their application to the different kinds of judgement that are made---thus, ultimately, to the differences in the circumstances which make different kinds of judgement true or false. Differences in kind between judgements depend upon differences which obtain in the relations between the judging subject and the objects arranged in the judgement-though, on Russell's "Multiple Relation" theory of judgement, every judgement is subsumed under the general rubric of a complex relation between a mind and the constituents of judgement:¹²

Every judgement is a relation of a mind to several objects, one of which is a relation; the judgement is true when the relation ... relates the ... objects, otherwise it is false. (Russell, <u>1910b</u>, p. 156)

To discuss the "Multiple Relation" theory in detail, and the criticisms of it by Wittgenstein, at this point, would take me too far from the main purpose of this Section--the examination of the foundations of ramified type theory. Nevertheless, Wittgenstein's criticisms (in 1913) of Russell's theory of judgement had a profound effect upon Russell's logical and epistemological doctrines. In the literature on the question of

Wittgenstein's influence on Russell these criticisms have not been wellunderstood; in particular, it has not been shown that the <u>effects</u> of Wittgenstein's critical attack upon the 1913 theory of judgement were quite extensive in their implications for the theory of types, the theory of the proposition as an 'incomplete symbol' and the account of the nature of logical truths. I have found it expedient, therefore, to set forth in an Appendix to this section (Appendix A), my own, somewhat speculative, reconstruction of the history behind Wittgenstein's criticisms of Russell's doctrines; together with an appraisal of the effects of the criticisms and an assessment of their validity. Much of the discussion which follows--of the changes in type theory induced by Wittgenstein and Ramsey's criticisms-presupposes the argument of this Appendix. I include at this point, therefore, a summary of the case I argue in Appendix A:

(VI):Summary of Appendix A:

(1) There are two specific criticisms, apparently, that Wittgenstein made:

(a) a proper theory of judgement must show that it is impossible to judge nonsense (i.e. that understanding the proposition judged is presupposed in judging; hence, the proposition must be significant).

(b) from a correct analysis of "A judges aRb" the proposition "aRb .v.~aRb" must follow directly, without additional premises.

Russell's 1913 theory of judgement (formulated in an unpublished manuscript) satisfies neither (a) nor (b). In fact, it is shown in Appendix A that (a) and (b) are effectively the <u>same</u> criticism.

(2) That (a) and (b) are the same is shown by considering them as applying to the manner in which the account of judgement was to support the ramified theory of types. It can be argued that Wittgenstein's chief preoccupation through the period preceding his criticisms of Russell, and subsequent to them, was to make sense of ramifie type theory. The 1913 analysis of judgement, it can be shown, permits violations of type to be analysed exactly as Russell analysed judging aRb. There is nothing in Russell's account of judgement to prohibit judging nonsense of the kind resulting from type-mistakes. The <u>formal</u> definition of type-sameness (by PM, *9 and *13.3) requires that "aRb .v.~ aRb" follow from any proposition "aRb" in which a, b are of the requisite type of argument to $\hat{x}R\hat{y}$. Thus, the criticisms are effectively the same.

(3) Since the classification of argument-types to functions is derived from the heirarchy of orders of functions, which, in turn, is based upon the kinds of judgements made in asserting functions of arguments, the failure of Russell's analysis of judgement threatens the basis for ramified type theory. In consequence, the problem of showing how the solution to the paradoxes was to be a natural consequence of Russell's positive doctrines of propositional meaning returns. (Whether Russell recognised <u>all</u> the consequences of Wittgenstein's criticisms is discussed in the Appendix).

(4) Further effects of the criticisms are very extensive. Approaching them from the doctrine of incomplete symbols shows that Wittgenstein's general attack upon Russell's attempt to eliminate propositions as objects of judgements--though retaining 'perceived complexes' as what makes elementary judgements true--is an extension of criticisms (a)/(b). Objects of acquaintance, for Russell, are <u>nameable</u> items--so, in the analysis of judgement, propositional constituents appear as <u>named</u>. Wittgenstein insisted that it would follow that some judgement that this table penholders the book (Wittgenstein's example)--composed of named items--would fit Russell's analysis. But, since named items are all individuals, this analysis collapses type-differences, permitting the judgement of nonsense. This leads to Wittgenstein's general criticism of type theory (discussed in Section D) that it tries to <u>state</u> what, properly, can only be <u>shown</u> by a correct symbolism.

(5) A further effect of Wittgenstein's attack, it can be shown, may have been to convince Russell that it was necessary to bring back the <u>proposition</u> as an essential constituent of judgement or assertion. This threatened the primitive idea of PM of "assertion of a function" and undermined Russell's conception of logical truths as "completely general" truths which involve only "pure form". Wittgenstein argued that some "completely general" propositions would be contingent-including the Axioms of Reducibility and Infinity of PM--leading Russell to fear that ramified type theory with the Axiom of Reducibility might "appear <u>ad hoc</u>"--justified solely on pragmatic grounds. Several of the changes in the second edition of PM are directly consequential upon Wittgenstein's attack upon type theory and the analysis of judgement. (VII): Changes in ramified type theory from PM1 to PM2:

The conclusions of Appendix A cast severe doubt upon the utility of the multiple relation theory as a justification for the classification of judged propositions into orders--hence, for construing functions as significant only over arguments of definite types through their presupposing a predicative totality of propositions. From Wittgenstein's criticisms, we know that the source of these doubts must reside in the analysis of elementary judgements and in the notion of a 'perceived complex', with which we are acquainted, which possesses a 'logical form' and which makes an elementary judgement true (see Appendix A, pp. xxxvi-xxxix). In PM, Russell gave two conditions for a judgement to be elementary: that it is true or false only according as the corresponding complex exists or not (there are no complexes corresponding to non-elementary judgements); and that the judged 'proposition' (qua incomplete symbol) "does not involve any variables" (PM *1, p. 91). The first problem for ramified type theory arising from Wittgenstein's criticisms concerns the nature of complexes--whether type-differences are preserved in analysing them into constituents and form. The second problem for ramified type theory concerns the elementary combinations of elementary propositions by means of logical functions: negation, dusjunction, etc. (see: PM *1, pp. 91-2). That is, the second problem concerns the doctrine of systematic ambiguity as that applies to logical expressions. Further reflection upon these two problems reveals what Ramsey came to isolate as problematic in the PM account of predicative functions and in the technical application of the Axiom of Reducibility to the logicist foundations of mathematics.

It remains, then, to consider Ramsey's criticisms of ramified type

theory in the light of the changes in PM prompted (in the main) by Russell's espousal of Wittgenstein's views. In the first edition of PM (*1), "elementary propositions" are a primitive idea--they are explained, but not defined, as "falsely abstracted" objects of judgements which are true or false as they correspond or fail to correspond to complexes. Complexes are objects of acquaintance. Equivalently, thus, in 'completing' the incomplete propositional symbol by asserting or judging, the resulting assertion or judgement is 'elementary' provided that the incomplete symbol contains no variables (real or apparent). In the intuitive characterisation of the development of the heirarchy of orders from a basic class of elementary judgements, I employed the epistemically neutral description of "elementary" assertions being those for the understanding of which nothing further is needed than knowledge of what would make the assertion true or false in the context--knowledge I summed up as comprising what the assertion is 'about' and what it asserts of what it is about. In the light of Wittgenstein's criticisms, however, this description of "elementary" assertions is far less neutral than it may have appeared. The requirement that only knowledge of truth-conditions contribute to the understanding of elementary assertions, in this sense, amounts to the condition that what is expressed in asserting such a proposition (its sense) be exhausted by knowledge of the circumstances which have to obtain for the proposition to be true or false. "Circumstances", here, are simply Russellian "complexes" stripped of their dubiously simple natures as objects of acquaintance. What remains of Russell's use of the notion of a complex is essentially the condition that, for an assertion to be elementary, in this

sense, we should, in principle, be capable of understanding it without having to know whether any other assertion (elementary or otherwise) is true. For example, the condition is that, in normal contexts, we understand, say, "This is red" in perceiving this to be red. Similarly, we understand "Lightning precedes thunder", pre-scientifically, in experiencing the temporal order of seen-lightning-flash and heard-thunder. That the sense of some such elementary propositions is determinable without determining the truth of other propositions is a basic tenet of Wittgenstein's "atomistic" conception of propositional meaning (see;: Tractatus 3.23, 5.134-5--not that either of the above examples are "elementary propositions" in Wittgenstein's sense). The principle is voiced in the requirement that genuinely "elementary propositions" be mutually independent--whether one "elementary proposition" has sense cannot depend upon whether another is true (c.f. 6.3751). The doctrine is connected to the Tractarian thesis of extensionality whereby all propositions are truthfunctions of elementary propositions (5, 5.54); thus, to the account of the special nature of the propositions of logic as consisting, not in their "generality" or "self-evidence", but in their being truth-functional tautologies which "say nothing" (have no descriptive content) but show the logical form of language (6.11 and passim). This, in turn, supports Wittgenstein's earlier claim--in letters to Russell--that a theory of types is impossible because it tries to state what can only be shown. The final task of this section will be to relate these doctrines to Ramsey's critique of ramified type theory and the Axiom of Reducibility. More detailed discussion of Wittgenstein's alternative to type theory--the

doctrine of showing--is postponed to Section D, where the recurring question: are type-differences differences in <u>things</u> or in <u>expressions</u> which 'stand for' things? is considered.

Ramsey attributes much of the reasoning behind his critique of ramified type theory to doctrines of Wittgenstein's <u>Tractatus</u> (see: Ramsey, <u>1925</u>, p. 1 and pp. 9-13). His criticisms are directed at three defects in the ramified theory as that appears in the <u>second edition</u> of PM (<u>1925</u>), and concern the following:

(i) the impossibility of defining infinite classes by means of the non-extensional propositional functions of PM (Ramsey, <u>1925</u>, pp. 22-23)

(ii) the necessity for the Axiom of Reducibility--which Ramsey argues to be contingent--to preserve proofs by induction in arithmetic and definitions by Dedekindian section in real number theory (ibid., pp. 27-29);

(iii) the inadequacy of the treatment of identity (in *13 of PM) which necessitates introduction of the Axiom of Infinity--which Ramsey also argues to be an "empirical proposition" (ibid., pp. 29-32 and 59-61).

Of these three criticisms, only (i) and (ii) apply directly to the ramified theory and, then, only to the theory which results from the changes made for the <u>second edition</u> of PM (hereafter: PM2). I shall discuss only (i) and (ii), together with Ramsey's suggested modifications. It is necessary first, though, to examine the relevant changes embodied in the new introduction to PM2, many of which result directly from Wittgenstein's criticisms of PM (see: Appendix A, pp. li-lii), (I list the changes numerically):

(1) The most definite improvement ... is the substitution ... of the one indefinable "p and q are incompatible" ... for the two indefinables "not p" and "p or q". This is due to Dr. H.M. Sheffer. (PM2, p. xiii)

(2) There is no need of the distinction between real and

apparent variables, nor of the primitive "assertion of a propositional function". (PM2, p. xiii)

(3) One point in regard to which improvement is obviously desirable is the axiom of reducibility (*12.1.11),. This axiom has a purely pragmatic justification: it leads to the desired results and no others ... There is another course recommended by Wittgenstein for philosophical reasons (<u>Tractatus</u>, 5.54FF). This is to assume that functions of propositions are always truth-functions, and that a function can only occur in a proposition through its values ... It involves the consequence that all functions of functions are extensional ... We are not prepared to assert that this theory is certainly right, but it has seemed worthwhile to work out its consequences ... it seems that the theory of infinite Dedekindian and well-ordered series largely collapses, so that irrationals and real numbers, generally, can no longer be adequately dealt with. (PM2, p. xiv)

From Appendix A (pp. xiv-xx), we know that revision (2) is Russell's recognition of Wittgenstein's demand that "only apparent variables occur in logical propositions" (Letter R.2 to Russell). Discussed also in Appendix A is the effect of this demand upon Russell's conception of logical propositions as "completely general" and involving "pure form". It introduces a problem for ramified type theory in applying the doctrine of systematic ambiguity across orders to logical truths. By PM, pp. 128-9, " \vdash . p v~p" would not, in practice, be restricted to one order and could, thus, be asserted generally. Since it contains a real (free) variable "p", however, Wittgenstein insisted that it could not be a <u>proposition</u> at all. Turning "p" into a bound variable " \vdash .(p) p v p", however, had the undesirable consequence of confining the truth to a definite order of propositions (the values of the function ϑp , where $\vartheta p =_{df} p$.v.~p). Adopting, in PM2, Sheffer's stroke-function ("p/q"--revision (1)) and embracing the <u>tractarian</u> theses of extensionality (revision (3)), led Russell to change the basis

for the heirarchy of orders. No longer based upon the multiple relation theory of judgement and the "assertion of a propositional function" in some or all cases (for the account of "general judgements"), the original intuitive justification for the Axiom of Reducibility (PM, p. 166) is lost and the axiom remains "with a purely pragmatic justification". The main thrust of Ramsey's criticism is against the axiom of reducibility; so, to understand those criticisms, we should examine first the revised basis for ramified type theory in PM2.

Now the revised version of ramified type theory in PM2 has to be reconstructed from a number of separate additions to the text of the first edition and it is a long and taxing process to try to reconcile these additions with the text itself, or even with one another. In particular, the new introduction (PM2, pp. xiii-xlv) gives a sketch of the main changes whilst three separate Appendices give, respectively:

(A) a revised quantificational base-logic (*8) to replace *9 of the text--to accommodate leaving out the primitive idea "assertion of a function" and the abolition ot the real variable;

(B) a piecemeal series of modifications to proofs by mathematical induction which purport to show their validity in the absence of the Axiom of Reducibility; and

(C) an examination of the <u>Tractarian</u> thesis of extensionality that
 (i) all propositions are truth functions of "elementary propositions"; and
 (ii) functions of functions are derivable by generalisation from matrices
 constructed by successive applications of the stroke-function to "elementary functions".

Material from each of these three Appendices and the new introduction frequently overlaps, and, as often, conflicts with, material in the other additions. Appendix B, for example, is a thorny text, full of slipshod notations and errors of detail which are very perplexing to unravel. For

instance, as Gödel has observed (Gödel, 1944, pp. 126 and 145-6), the revised proof of the definable equivalence of the classes of inductive numbers of orders higher than order-5 to the class of order-5 (by *89.29)-based upon *89.16--is certainly defective. It asserts that every subset a (of arbitrary order) of an inductive class B of order-3 (c.f. *89.12) is itself of order-3, though the proof requires induction applied to orders greater than 3 if a is of some order greater than 3. This vitiates the proof--whether it can be remedied has since been answered in the negative, by Myhill (in Nakhinian, 1974, pp. 19-28). Similarly, *13.101--which asserts that "x=y" holds when y satisfies every function, predicative or not, which is satisfied by x, is said to depend upon the Axiom of Reducibility (*12.1). Yet, it is nowhere shown whether removal of the Axiom of Reducibility invalidates the proof of *13.101. On the contrary, it can be shown that, for typical functions of order n (on the revised account of functions), the proof remains valid without appeal to *12.1 (see: Ramsey, 1925, p. 30) -leading one to suspect that the original proposition (the PM analogue of "Leibniz' Law") did not require the Axiom for its proof.

In view of these problems and complexities arising from these separate additions to the text of PM, I have deemed it expedient to attempt to formulate a coherent systematisation of the ramified theory which, though it does not preserve all of the <u>detail</u> of the revisions in the second edition, it preserves the spirit of the major changes Russell describes in the new introduction. This reconstructed version of the ramified theory of PM2-called "RTT"--can be more directly compared with Ramsey's account; so that his criticisms can be evaluated, and his alternatives assessed:

A Formulation of the Ramified Type Theory of PM2 (The System RTT):

Preliminary notions:

(a) Atomic propositions: (PM2, xv), The set \underline{AT} of atomic propositions consists of all propositions of the forms:¹³ $R_1(x)$ ("x has (the intension) R_1 ") $R_2(x,y)$ ("x has R_2 to y") $R_3(x,y,z)$ ("x has R_3 to y and z")...

(b) Molecular propositions: let p, q,... be members of <u>AT</u>. The set <u>Mol</u> of molecular propositions is the least set containing <u>AT</u> such that, if it contains p, q, then it contains (PM2, xvi):

- (1) (i) p/q
 (ii) ~p = (p/p) Df.
 (iii) p⊃q = (p/(q/q)) Df.
 (iv) p v q = ((p/p)/(q/q)) Df.
 (v) p . q = ((p/q)/(p/q)) Df.
- (2) If $m_1 \ldots m_n$ are in <u>Mol</u>, then $(m_1/(\ldots)/m_n)$ is in <u>Mol</u>.

Members of Mol are also called "elementary propositions" of RTT.

(c) Rules of inference: (i) if p, q, r are in Mol, then from p and p/(q/r), infer r. (Degenerate case: from p and $p/(q/q) = p \supset q$, infer q.)

(ii) if m_0 , m_1 <u>Mol</u> and m_k is any member of <u>Mol</u> containing a well-formed part of the form (m_0/m_1) , then any result of replacing m_0 , resp. m_1 by any m_1 , $m_1 \in \underline{Mol}$ ($i \neq j$) is a member of <u>Mol</u>.

(d) Axioms for "/": A1: p/(p/p). A2: p⊃q:⊃. s/q⊃p/s.

Functions definable over Mol: (PM2, xx - xxviii),

(a) An <u>elementary function</u>, or <u>matrix</u>, is an expression " \emptyset !x" for which all <u>values</u> (results of replacing x by arguments of an appropriate type) are elementary propositions (atomic or molecular). For example: "F!(\emptyset !z, \emptyset !z, x,y)" is a matrix defined over elementary functions of individuals and individuals. All values will be elementary propositions; e.g. "R₁(a) . \supset .S₁(b)", "R₂(a,b)/p₀",...

(b) A <u>general proposition</u> is an assertion of some or all values of a function. When it occurs <u>unasserted</u> (in the scope of a stroke-function), it is to be regarded as derived from a matrix in the following way:¹⁴

statements of Russell's doctrine that propositions are not single entities, but are what we "falsely abstract" from incomplete symbols, which, properly, are only completed in being asserted, believed, or judged true. Analysis reveals that all apparent references to propositions by means of descriptive phrases are removed by the contextual elimination of incomplete symbols:

Now a proposition is, in my opinion, an 'incomplete symbol', i.e. some context (of assertion) is necessary before the phrase expressing the proposition acquires a complete meaning... We must ... say that, in the sense in which propositions are involved in believing and in propositional understanding, there is no difference, as regards reality, between true and false propositions. And this, in turn, since it is repugnant to admit the reality of false propositions, forces us to seek a theory which shall regard true and false propositions as alike unreal, i.e. as 'incomplete symbols'. (1913 m.s. pp. 200-201/t.s. pp. 43-44)

The 1913 manuscript ends in Part II with the analysis of judgement, belief, truth and falsehood, and self-evidence applied to atomic propositions, only. On Eames and Blackwell's reconstructed Table of Contents--(loc. cit. p. 10), this was to be followed by Part III in which the theory was applied to 'Molecular propositional thought', i.e. judgements involving inferences and logical constants (Archives document 210.06556-F1 is an outline, on one page, of the proposed contents of Part III, entitled "Molecular Thought"). It is interesting to note that this pattern of development--beginning with atomic 'elementary' judgements and proceeding through molecular judgements to logical inference--mirrors the introduction of the 'orders of generality' of propositions and functions which is characteristic of the ramified heirarchy of PM. One can at least surmise that in classifying the different kinds of judgement, thus, Russell had in Consider a matrix of n-variables " $\emptyset(x_1, \ldots, x_n)$ "--in which " \emptyset " goes proxy for a stroke function of elementary matrices (see: footnote 14). We shall call the <u>generalisations</u> of this matrix the result of prefixing n-many quantifiers ("(x), (\exists y),...") in as many different styles as possible (there are n.2ⁿ -many prefixes for a matrix of n-variables). Whenever the scope of a generalisation is less than the whole proposition, the quantifiers prefixed to the matrix can be brought to have the whole proposition as scope by means of definitions (1)-(8) of (e), below.

(c) A <u>first-order proposition</u> is a generalisation of a matrix all of whose variables are individual variables.

(d) Axioms for general propositions: Al, A2 with elementary matrices for the variables p, q, r, s.

A3: $(\exists x_1 \dots \exists x_n) \cdot \phi(a_1 \dots a_n) / (\phi(x_1 \dots x_{n-1}) / \phi(x_n))$.

Af: $(\exists x_1 \dots \exists x_n)$. $\emptyset(x_1 \dots x_n)/(\emptyset(a_1 \dots a_{n-1})/\emptyset(a_n))$. --where a_1, a_2, \dots are constant names of individuals.

A3, A4 reduce (by definitions) when n=1 to:

A3': $\emptyset(a) : \supset : (\exists x) \ \emptyset x.$ A4': $(\exists x) : \sim \emptyset x \ v \ \emptyset(a); i.e. (x) : \emptyset x \supset \emptyset(a).$

(e) Scopes of general propositions: When a quantifier occurs in a stroke-function except as a prefix to the whole matrix, the quantifier can be eliminated in favour of a prefixed quantifier as follows:

(1) $((x) \cdot \theta x)/q \cdot = \cdot (\exists x) \cdot \theta x/q.$ (2) $((\exists x) \cdot \theta x)/q \cdot = \cdot (x) \cdot \theta x/q.$ (3) $p/((y) \cdot \theta y) \cdot = \cdot (\exists y) \cdot p/\theta y.$ (4) $p/((\exists y) \cdot \theta y) \cdot = \cdot (y) \cdot p/\theta y.$

Where more than one "nested" quantifier occurs:

(5) $((x).\emptyset x)/((y).\emptyset y) = .(\exists x):(\exists y).\emptyset x/\emptyset y.$ (6) $((x).\emptyset x)/((\exists y).\emptyset y) = .(\exists x):(y).\emptyset x/\emptyset y.$ (7) $((\exists x).\emptyset x)/((y).\emptyset y) = .(x):(\exists y).\emptyset x/\emptyset y.$ (8) $((\exists x).\emptyset x)/((\exists y).\emptyset y) = .(x):(y).\emptyset x/\emptyset y.$

Thus, all propositions, of whatever order, can be derived from matrices (combinations of elementary matrices by the stroke-function). For, in any matrix m_i , if $p \in Mol$, and p occurs in m_i , replace p by any other elementary matrix ($\emptyset(x)$, $\emptyset(x,y)$, etc.), prefix quantifiers appropriate to the variables of the matrix, and move the quantifiers to have the whole of m_i as their scope-giving a general proposition resulting from m_i by prefixing n-quantifiers for n distinct variables.
The heirarchy of orders in RTT: (PM2, xxviii - xxxix),

(a) The totality of values for the matrix "Ø!x" comprises the union of the sets of elementary propositions of which the first set consists of all results of combining elementary propositions by the stroke-function, at least one of which contains a name "a" (thus, of all results of varying "Ø" whilst keeping "a" constant); and the second set consists of all results of varying "x" (for individuals a, b, c, ...) whilst keeping "Ø" constant.

(b) Successive generalisations of elementary matrices yield different totalities of propositions according to the following schema of orders:

ORDER-0: elementary propositions, matrices and stroke-functions.

ORDER-1: (i) generalised matrices all of whose variables are individual variables.

(ii) functions of individuals containing at least one bound individual variable.

(iii) first-order propositions (described above).

ORDER-2: (i) generalised matrices containing at least one functional variable of order-1 (second-order propositions, if all variables are bound).

(ii) functions of individuals containing bound variables of order-1,

(iii) functions of functions of order-1 (and, possibly, individuals).

ORDER-3: (i) generalised matrices containing at least one functional variable of order-2.

(ii) functions of individuals containing bound variables of order-2

(iii) functions of functions of order-2 and individuals

(iv) functions of functions of order-2 and functions of order-1

(v) functions of functions of order-2

(vi) generalised matrices containing at least one bound variable for values of functions of order-2, (third-order propositions).

••••

(c) Some idea of the branching heirarchy built up from, say, a single elementary proposition may be gleaned from the diagram overleaf:



(d) In Diagram I, the symbolic conventions are essentially those of Russell. They are not as perspicuous as they might be through Russell's use, for example of "Ø!x" for both a definite (constant) function of individuals all of whose values are elementary propositions, and a variable function whose values, though elementary, may differ widely in occurrences of the stroke and of individual names. In general, "x", "y", ... are indi-vidual variables, "a", "b", ... are individual constants (names, "Ø", "V" are retained for first-order functional variables or constants (or elementary functions = stroke functions), "g", "f" are second-order functional symbols, and "G", "H", ... are third-order. The shriek "!" succeeding a functional variable or constant indicates that all its values are elementary propositions. This replaces the first-edition notion of a "predicative function"--one whose order exceeds that of its values by one--since, in PM2, every function of whatever order can be derived from a matrix which is obtained from an elementary proposition by replacement of atomics by elementary matrices. Thus, the "predicative" functions are just those all of whose values are atomic or molecular propositions. This requires that, for example, the values of a first-order generalisation "(x). \emptyset x" are infinite conjunctions of elementary propositions "Øa . Øb .,", for the various stroke functions "Ø". Since an infinite enumeration is not possible, Russell argues (PM2, p. xxxiii) that, by proving various theorems (in Appendix (A), *8), we show that what holds of finite segments of infinite conjunctions (or disjunctions for existentials), holds generally. Since these can be extended to each order (for quantified "Ø", "f", "G",...), these particular demonstrations purport to take the place of the axiom of reducibility (see: discussion of Ramsey, below).

(e) The corresponding heirarchy of types remains the same as in the first-edition: arguments to functions are divided into types according to the significance range of the variable. Thus:

1) If "Ø!x" is a function of order-1, the range of significance comprises type-0 items--individuals.

2) If "f!(\emptyset !x)" is a function of order-2 of first-order functions, the range of significance comprises type-1--functions of individuals. Members of this type are <u>expressions</u> which have elementary propositions as values when individual names are supplied for their constituent variables.

The difference between a function of individuals and a function of functions is explained by the assumption that a function can occur in a proposition only through its values--functions cannot be <u>arguments</u> (items) to functions; only the propositional values of functions can be combined to form new functions ("of" the original function).

3) By parity of reasoning, the range of significance of a function of functions of functions comprises type-2--all significant arguments to third-order functions.

The Semantics of RTT:

(a) I do not intend to give, at this point, an interpretation of RTT which can "fit" intuitively, Russell's intentions as to the 'meanings' of the symbolism of PM2. I claim, in Part II of the thesis, that an intuitively adequate semantics for type theory demands consideration of, <u>inter alia</u>, what sentences are "about" in context; and, thus, that the logics CL and CS in Part II go some way towards codifying Russell's intentions. The following remarks are intended only to suggest the approach taken later in the thesis--they are based, in part, upon Russell's exposition in PM2, pp. xxviii-xxix.

(b) A semantics for RTT which, though diverging considerably from Russell's exposition, can preserve the spirit of the branching of levels in ramified type theory, has to give content to the claim that the ramified theory of PM2 is <u>extensional</u>. Now, in respect of propositional <u>functions</u> in RTT, this claim amounts to the <u>Tractarian</u> view that all functions of functions are truth-functions of their propositional values (atomic or otherwise). Russell's theory, however, is definitely <u>not</u> wholly extensional, because the basic class AT of atomic propositions is described in <u>intensional</u> terms. That is, the atomic propositions $R_1(a)$, $R_2(a,b)$, and so on, assert the "intensional attribute" R_1 of a, the <u>relation</u>-in-intension R_2 of a, b; etc. R_1 is not to be identified with the <u>class</u> of objects having R_1 (extension of the function); nor is R_2 to be construed as the class of couples $\langle x, y \rangle$ such that xR_2y .

(c) On the other hand, all functions of functions are extensional, in that, for any functions $\emptyset \hat{z}$, $\vartheta \hat{z}$ of individuals:

 $\phi_x \equiv x \ \psi_x \ . \supset . \ f(\phi_z^2) \equiv f(\psi_z^2) - by \ PM2, \ p. \ xxxix.$

Thus, materially equivalent stroke-functions of propositions are intersubstitutable salva veritate.

(d) The contrasing "intensionality" of atomic propositions--thus of elementary functions--is the subject of Ramsey's first criticisms, and is discussed below, and in Part II. An appropriate semantics for RTT, therefore, requires an "intensional" interpretation of atomic propositions. (Roughly, instead of an arbitrary assignment of truth-values to atomics, an intensional assignment must be a function from what a sentence is "about"-in a sense to be explained in Part II--together with other parameters, to a truth-value). In addition, it requires that all <u>other</u> propositions (nonatomic) and functions thereof be interpreted extensionally (assignments will be functions from items of the appropriate type-level of truth-values). The semantics of CS--given in Part II, and based, in part, on remarks in Church, <u>1956</u>, pp. 347-8, footnote 577--is an attempt to codify these intentions formally.

In particular, the basic idea behind the intensional interpretation

of atomics is to realise Russell's claims that:

(i) the function \emptyset !**a** collects together all those elementary propositions which are <u>about</u> a; (ii) the function \emptyset ! $\hat{\mathbf{x}}$ collects together all those elementary propositions which "say the same thing" of what they are <u>about</u>; (iii) every elementary function of individuals has as values atomic propositions or combinations thereof by means of the stroke-function; (iv) every non-elementary function is a truth-function of elementary functions or propositions (so: at each order m, an assignment to a functional variable of order-m is a truth-value assignment from items of the appropriate "aboutness-type" to sequences $\langle t_m \rangle, \langle f_m \rangle$ of truth-values (see: Part II, Section D).

This completes the description of RTT.

(VIII): The Criticisms of Ramsey:

Ramsey's first criticism ((i), above, p. 198) involves the "intensional" nature of the atomic propositions of PM2, and the impossibility of defining infinite classes by means of the contextual elimination of class-expressions in PM2, *20. His second criticism (ibid.) is of the Axiom of Reducibility. Russell had accepted in PM2 that, without the axiom, proofs by mathematical induction in arithmetic and Dedekindian Section in analysis would not be valid in PM2. He supposed proofs by induction could be restored in each particular case (PM2, Appendix (B)), but it is now known that the method of Appendix (B) is defective (see: Myhill, 1974, pp. 19-28). For the purposes of analysis, Russell re-introduced the axiom solely on the pragmatic ground that it preserved the method of Dedekindian section. Since Ramsey espoused Wittgenstein's "tautology theory" of logical propositions (Ramsey, 1925, part I) -- and the Axiom was not demonstrably a tautology--he could not accept its use in the ramified theory of PM2. The first criticism is, in effect, related to the second via the notion of a "predicative function" of PM2. This can be shown, as

I turn, now, to the detail of the criticisms.

By the contextual definitions of class-expressions of *20--given above, p.146 -- every class, it is supposed, is defined by a propositional function. In short, a class is composed of all objects which as arguments to a predicative function yield true propositions. As described in RTT, whether a function is predicative depends upon whether the totality of its values (true or false), for arguments of the appropriate type, are either atomic propositions or stroke-functions of atomics. Equivalently, every predicative function is derived from an elementary matrix by generalisation. Ramsey begins by noting (p. 22) that, clearly, every finite class can be given by a predicative function. For, any n-membered class An is completely determined as the totality of individuals for which " $\hat{x}=a_1$.v. $\hat{x}=a_2$.v. ... v. $\hat{x}=a_n$ " is true. Suppose, however, we wish to specify a function satisfied by a denumerably infinite totality of objects. As a first guess, it might seem that such a class B would comprise all individuals satisfying the function $'\hat{x}=\hat{x}'$ --for, every individual is self-identical and, if there are denumerably many individuals, there is a totality B such that, for every x, $x \in B$ if and only if x=x-where the range of "x" is confined to the lowest type--individuals.

Are there, then, denumerably many individuals (at least)? In respect of RTT, and, hence, Russell's ramified theory, this question is just as problematic as it had been for Frege, in <u>Gg</u>., in determining the closure of the domain of objects (referents of singular terms) by means of the abstraction and numerical operators of <u>Gg</u>. (see: above, section B, pp. 127-134). Moreover, in terms of PM2, Russell had no better solution than

to invoke an axiom to this effect---the Axiom of Infinity--an axiom Ramsey regarded (criticism (iii), above) as no more justified than the Axiom of Reducibility.

Whatever individuals satisfy the function $\hat{x}=\hat{x}$, they will comprise the lowest type of the heirarchy of types. For an item to be of this type, it must belong to the range of significance of an elementary function $\emptyset!\hat{\mathbf{x}}$. An item a belongs to the range of $\emptyset!\mathbf{x}$ if and only if the result of replacing the variable "x" in $\emptyset! \hat{x}$ by "a"-a name of <u>a</u>--is an elementary proposition. It follows that there are infinitely many individuals over which an arbitrary function \emptyset 'x is significant only if there are denumerably many names of distinct individuals in the vocabulary of RTT. In general, then, can there be a function $\oint x$ (elementary or not), or combination of functions, which is true of infinitely many arguments, but false of every finite subset of those arguments? Since every function of RTT is an expression built up by generalising matrices derived from elementary propositions by replacing names by variables, this question reduces to the question whether there is any combination of elementary propositions of RTT which is true only in an infinite domain of individuals. By an indirect argument, however, it can be shown that this question cannot be answered by specifying just such a combination of propositions in RTT. That is, the impossibility of showing that infinite classes are definable in RTT (Ramsey's first criticism) turns out to be related to highly significant properties of the system which the system shares with many other axiomatic foundations of mathematic (e.g. Zermelo-Fraenkel set theory, Hilbert and Ackermann's Grundzuge¹⁵).

Suppose RTT is supplemented by its complement of axioms, theorems and definitions of PM2 appropriate to the logicist reconstruction of arithmetic and real number theory. For simplicity, I refer to this system as "PM2"---though it lacks some of Russell's doctrinal basis. If, in PM2, there were a set of propositions true only in an infinite domain of individuals, since every proposition of PM2 is, in effect, the value of a function obtained by generalising a matrix obtained from elementary propositions, such a set would have to contain every set U_{ω} having the following properties.

Call an ω -set A_1 of propositions any set such that if $\emptyset \hat{x}$ is an elementary function of individuals, and $\sim \emptyset a, \sim \emptyset b, \sim \emptyset c, \ldots, \sim \emptyset n$ are negations of n-many atomic propositions (n is finite), then:

(i) $\sim \emptyset a, \sim \emptyset b, \ldots, \sim \emptyset n \in A_1,$ (ii) if $p_i, p_j \in A_1$, then $(p_i/p_j) \in A_1$ ($i \neq j \leq n$) (iii) $(\exists x) . \emptyset x \in A_1$.

For each elementary function $\oint \hat{x}$, there is an ω -set A_i , each of which is a finite set consisting of negations of atomics, stroke-functions of these and a negated universal (" $(\exists x) \dots = \sim(x) \sim \dots$ ").

Let U_{ω} be the union of the family $\{A_1, A_2, \ldots\}$ of ω -sets, so defined. U_{ω} is <u>consistent</u> provided that:

- (i) for no (negated) atomic p_k , $p_k/p_k \in U$.
- (ii) for no elementary functions $\emptyset! \hat{x}, \forall! \hat{x}$ of PM2 do we have: $\vartheta! x . \equiv . \, \, \, \, \, \, \forall! x$ -together with the appropriate equivalences for dyadic, triadic functions, etc.

We can suppose the consistency conditions for U_{ω} are satisfied. If they were not, RTT would be simply inconsistent¹⁶.

Now, as is evident, if U is <u>semantically</u> consistent (satisfied in some domain), it comprises a set of propositions which are:

(i) satisfied in no domain containing n-individuals, for arbitrary finite n;

(ii) satisfied in at least one infinite domain.

That (i) is true of U can be inferred from the fact that, if U_{ω} were satisfied in some n-membered domain D_n , and $"a_1"$, $"a_2"$,..., $"a_n"$ were names of individuals in D_n , then some subset u_n+1 of U_{ω} would comprise the set:

$$\begin{split} u_n+1 &= \big\{ \sim \emptyset a_1 \ . \ \sim \emptyset a_2 \ . \ , \ldots, \ . \ \sim \emptyset a_n, \ \ldots, \ (\exists x) \ . \ \emptyset x \big\}. \\ \text{(Notice that: } &\sim p \ . \ \sim q \ . \end{matrix} \\ &= \ . \ p/q). \\ \text{But, } u_n+1 \text{ is inconsistent, unless for some individual } a_i \in D_n, \ \emptyset a_i - \text{whence,} \\ \text{for some } \emptyset a_i \in U_\omega, \ u_n \cup \ \& \emptyset a_i \big\} \text{ is not semantically consistent (relative to } D_n); \\ \text{though } u_n \cup \ \& \emptyset a_i \big\} \subseteq U_\omega \ (= u_{n+1}). \\ \text{This contradicts the consistency} \\ \text{of } U_\omega. \end{split}$$

That (ii) is true of U_ω follows from the fact that all finite sets of atomic propositions of RTT are <u>infinitely extendible</u> (a notion due to R.K. Meyer and described in Leblanc, <u>1976</u>, pp. 7-12) in the sense that indefinitely many individual constants (not all of which need designate different individuals (!)) are foreign to any finite set of atomic propositions. Thus, every finite subset u_i of U_ω comprises some such set as u_{n+1}, above, for different functions $\emptyset_1 \hat{\mathbf{z}}, \, \emptyset_2 \hat{\mathbf{x}}, \ldots$; and every such set is rendered semantically consistent by addition of a proposition " \emptyset_{a_k} ", where the constant " a_k " is foreign to u₁ (see: Leblanc, <u>1976</u>, p. 12 and footnote 9). But addition of \emptyset_{a_k} always increases the cardinality of the domain--for arbitrarily large cardinals D_n, D_{n+1}, (n is finite). The semantic consistency of U_ω, therefore, requires an infinite domain.

It should be noted immediately that the definition of an ω -set of propositions of PM2 mirrors semantically the syntactic requirement of ω -consistency that Gödel employed (<u>1931</u>, pp. 173-198) in demonstrating that PM2 contained true but unprovable propositions. If PM2 is ω -consistent, then, for no $\emptyset x$ and constants a_1, a_2, \ldots is it the case that:

(i) ⊢Øa₁, ⊢Øa₂,; but
(ii) ⊢. ~(x). Øx.

and Rosser has shown (<u>1936</u>, pp. 87-91) that ω -consistency, in this sense, implies simple consistency (non-provability of a contradiction). Thus, either PM2 is ω -inconsistent or it is simply consistent; and, if ω -consistent, then the simple consistency of PM2 cannot be proved in PM2 (Gödel's Second Undecidability Theorem XI of Gödel, <u>1931</u>). So, if it could be <u>proved</u> that U_{ω} is effectively definable in RTT (from some initial stock of functions)-hence, that some domain of RTT is, necessarily, infinite--then, since PM2 is RTT supplemented by the axioms and definitions of arithmetic, the simple consistency of PM2 would be provable, effectively, in PM2. Hence, there can be no proof that U_{ω} is effectively definable in RTT.

It follows from this indirect argument that Ramsey's first criticism-that infinite classes are not definable in (are not demanded of the interpretation of) PM2--is valid. We cannot attach a great significance to the validity of this criticism, however, since PM2 shares this property with any axiomatic system (lacking an axiom of infinity) sufficiently strong to generate the concepts and theorems of arithmetic.

The point of reconstructing the argument in this way has been to suggest that Ramsey's criticisms of 1925 were anticipated, in a large part, by Wittgenstein in 1913 (see: Appendix A). This should not surprise us, in so far as Ramsey embraced an avowedly Wittgensteinian conception of logic and mathematics. Nevertheless, it is particularly true of Ramsey's second criticism--that of the Axiom of Reducibility--that it derives from shortcomings of PM of which both Russell and Wittgenstein were aware. Thus, from remarks of Wittgenstein's Tractatus:

5.535: ... What the axiom of infinity is intended to say would express itself in language through the existence of infinitely many names with different meanings.,

and,

6.1233: It is possible to imagine a world in which the axiom of reducibility is not valid. It is clear, however, that logic has nothing to do with the question whether our world really is like that or not.

We can infer that the nub of Wittgenstein's criticisms of these axioms is that they are propositions not of logic, but, properly, "of physics"--as Wittgenstein expresses it--i.e. they are "empirical propositions". This is essentially Ramsey's second criticism (Ramsey, <u>1925</u>, pp. 28-9). Yet, it can be argued in response that, if the axioms of reducibility and infinity are "empirical", they are so only in an extended sense--not in the simple sense in which one would infer from a proposition's being "empirical" that it was contingently true.

This response depends upon an argument concerning the peculiar status of identity-propositions in PM2. I have taken the argument, in the main, from N.L. Wilson (<u>1959</u>, Ch. VI, pp. 99-106) and acknowledge my debt to him at this point. (His argument, though, is not applied specifically to these axioms of PM2).

Recall that, in RTT, it was indicated that type-0 comprises <u>named</u> individuals a,b,c,...--where the dots "..." suggest that there are indefinitely many of these. It is quite conceivable that a symbolic language like RTT contain <u>infinitely</u> many <u>names</u> of individuals (constants) "a", "b",; and yet it not follow that the domain of RTT contain infinitely many individuals. This would fail to follow, for example, if denumerably many of those names could stand for the <u>same</u> object. The problem of guaranteeing that the domain of RTT is infinite is, thus, related to the interpretation of propositions of the form "x = y" for <u>distinct</u> names "x", "y" in RTT. This, in turn, is related to the status of the axiom of reducibility in PM2 in the following way:

In 5.535, Wittgenstein's point is that it is not enough that a language like RTT contain an infinity of names foreign to any particular set of arguments to a propositional function. We have also to know that denumerably many of these names stand for <u>different</u> individuals.¹⁷ As

Wittgenstein expresses it, to say that infinitely many names stand for different individuals requires that we mention the language <u>and</u> its interpretation (the "designations" of names, predicates and functions), and Wittgenstein claims this can only be <u>shown</u>, not stated. The question arises: in virtue of what property of the language RTT would it hold that denumberably many names of RTT would have to stand for <u>different</u> individuals?

There are really two parts to this question: (i) what is required in RTT as to the existence of <u>any</u> individuals? and (ii) if there are individuals, what is there about RTT which might demand that there are infinitely many? The first question, apparently, is easily answered. PM2 contains theorems like:

(2) \vdash : $(\exists x)$. $\emptyset!x/(\emptyset!x/\emptyset!x)$ --where $\emptyset!\hat{x}$ is elementary, which asserts the existence of at least one individual. This is the standard assumption for classical logic--that every domain of interpretation be non-empty. If there were no individuals, nothing would be a significant argument to an elementary function; no such function would be true or false (significant) of anything. From the discussion in Wilson (<u>1959</u>, loc. cit.), I shall draw the consequence that such existential assertions of RTT as the above "are necessarily true, if significant at all, <u>but are only contingently significant</u>" (Wilson, ibid., p. 99); and that this characteristic extends to the axioms of infinity and reducibility.

Taking the Reducibility Axiom, first (PM2, *12.1--I shall not discuss the Axiom of Infinity separately), we notice that it is existential, like (2), above. It asserts that to every non-elementary function, there is a formally equivalent elementary function. Two functions are formally equivalent when they have the same (propositional) values for the same arguments. A "non-elementary" function is one not all of whose values are elementary propositions (atomic or molecular). The assumptions that every function is the generalisation of an elementary matrix (and, <u>therefore</u>, equivalent, for all values, to an elementary function) and that a function occurs in a proposition only through its values, take the place of *12.1 for as much of PM2 as possible. What cannot be derived in PM2, without the Axiom of Reducibility, is the theorem of real number theory that every bounded class of real numbers has at least upper (greatest lower) bound.¹⁸ (I have described in footnote 18 why this theorem requires the Axiom).

Ramsey's criticism of the Axiom of Reducibility is that it is neither a tautology nor a contradiction; and, thus, it must be "empirical" (Ramsey, <u>1925</u>, p. 57). It is clearly not self-contradictory that there could be, in RTT, an elementary function defining every class. In such a case, since classes are eliminated in favour of functions, then, for every function, there would be an elementary function defining the class of arguments for which it was true. Ramsey argues that the Axiom is not tautologous on the ground that:

It is clearly possible that there should be an infinity of atomic functions and an individual a, such that whichever atomic function we take there is another individual agreeing with a in respect of all other functions but not in respect of the function taken. Then: (ϕ) . ϕ !x. \equiv . ϕ !a could not be equivalent to an elementary function of x. (Ramsey, 1925, p. 57).

The function Ramsey specifies "(\emptyset). \emptyset !x . \equiv . \emptyset !a" is, supposedly, a

function of individuals. Since atomic propositions in PM2 are intensional, atomic <u>functions</u> of individuals are also intensional and can be identified, for the moment, as ranging over <u>properties</u> of individuals. Thus, since " $(\emptyset).\emptyset!x . \equiv .\emptyset!a$ " concerns all <u>predicative</u> functions of individuals (all whose values are atomic or molecular propositions), we can describe this function as true of an individual b if and only if b has the property of having every property of <u>a</u>. This function, Ramsey suggests could not be equivalent to an elementary function of individuals in a domain in which there are infinitely many properties and a designated individual <u>a</u>, such that for each property \emptyset , some individual has all properties of <u>a</u>, except \emptyset . That is, I surmise, Ramsey holds the following propositions to be inconsistent:

- (1) (\emptyset) . $\emptyset!\hat{x}$. Ξ . $\emptyset!a$ is true of each individual b_1 , b_2 , ...
- (2) $(\emptyset)(\emptyset a : \supset : (\exists y)[(\emptyset)(\emptyset y \equiv \emptyset a) : \sim \emptyset y]).$

(3) $(\exists \forall)$ (x) $(\hat{\phi}_x \equiv \forall!x)$ of every function $\phi_1 x$, $\phi_2 x$, ... (*12.1). Unfortunately, by every effort of testing for semantic inconsistency, I cannot detect an inconsistency in (1) - (3); i.e. there is always at least one assignment of truth-values to the atomic instances of (1) - (3) on which each of (1) - (3) are true together.¹⁹ (This is shown by consistencytrees in footnote 19). So, Ramsey's counterexample is not contraexemplary. One can see, through, how Ramsey may have been led to believe it was.

The Ramsey function " (\emptyset) . $\emptyset!x . \equiv . \emptyset!a$ " asserts of any individual b that it has the property of having every property of <u>a</u>. This is claimed to be non-elementary and non-equivalent to any stroke-function of elementary functions of individuals in a domain in which <u>a</u> is such that, for each atomic property F, if <u>a</u> has F, there is some b which has every other property of <u>a</u> except F. In such a domain the Ramsey function is false of each individual b_1 , b_2 ,... having atomic properties such that, no matter which property we select, b_1 agrees with <u>a</u> on all except the property selected. Thus, apparently, there will be no atomic property, or combination of them, true of exactly the individuals of which the Ramsey function is true. So, it seems, "(\emptyset). \emptyset !x . \cong . \emptyset !a" could never agree for all values (be formally equivalent to) a combination of atomic properties-and the Axiom of Reducibility fails.

What is problematic in the above reasoning is the status of the individual <u>a</u> in the specified domain. Since, in RTT, a universal quantification can be regarded as an infinite conjunction (see: PM2, p. xxxiii), the values of the Ramsey function can be infinitely enumerated in the array given by: for each individual b_i (i = 1,2,3,...) and predicative functions $\phi_1 \hat{x}, \phi_2 \hat{x},...$

If we regard each $\phi_{i}b_{i}$ as atomic propositions, the specification of the Ramsey function can be given by an assignment along the <u>diagonal</u> of this array, such that the i-th individual b_{i} in the ordering b_{1}, b_{2}, \ldots lacks the k-th property ϕ_{k} only if b_{i} agrees with <u>a</u> in having every ϕ_{i} (i \neq k). Assigning 'False' to, say, " $\phi_{n}b_{i}$. $\equiv .\phi_{n}a$ " requires that either b_{i} lacks ϕ_{n} , or a lacks ϕ_n . If a lacks ϕ_n , the Ramsey function can still be true of b_1 --trivially, because $\phi_n x$ is false of <u>a</u>. If b_1 lacks ϕ_n , then take ϕ_n to be that property which is determined as the (infinite) conjunction of " $\phi_1 x. \equiv .\phi_1 a$ ", " $\phi_2 x. \equiv .\phi_2 a, \ldots$ with each of the b_1 along the diagonal replaced by a variable "x". Then, there is no n in the array at which " $\phi_n x. \equiv .\phi_n a$ " is false of some b_1 --i.e. (ϕ). ϕ ! $\hat{x}. \equiv .\phi$!a for each of b_1 , b_2, \ldots ; but b_1 disagrees with <u>a</u> in lacking ϕ_n . That is, there will be no vertical enumeration (infinitely conjoined) of functions $\phi_1 \hat{x}, \phi_2 \hat{x}, \ldots$ which is formally equivalent to "(ϕ). ϕ ! $x. \equiv .\phi$!a", because the value of any " $\phi_k b_1$. $\equiv .\phi_k a$ " disagrees with the value of the Ramsey function in at least the k-th place (vertically) in the array. So, the Axiom of Reducibility is false of the Ramsey function. (The reasoning behind the counterexample, thus, I surmise, reproduces a version of the Richard Paradox in PM2).

The flaw in the reasoning, I believe, can be detected in asking whether any such <u>individual</u> as <u>a</u> could be specified coherently by means of functions true or false of it. Individual-identity is <u>defined</u> in PM2 (by *13) and the specification of <u>a</u> requires that <u>a differ</u> from each b_i in having some property which b_i lacks, when the <u>only</u> property b_i can lack is that of having every property of <u>a</u>'s. This, I shall argue, is not coherent.

By *13.01 of PM2, for any individuals x, y:

 $x = y : \bigcirc$: (\$\phi\$). \$\phi!x \equiv . \$\phi!y\$. Df.,

from which we can infer (again, for any x, y)

*13.191 \vdash :. y = x . \supset . ϕ y : \equiv . ϕ x.,

i.e. stating that everything identical with x has a certain property is

equivalent to stating that x has the property. Instantiating "x" to "a" gives: that everything identical with <u>a</u> has a certain property is equivalent to stating that <u>a</u> has the property. Now, to differentiate <u>a</u> from b, for the purposes of Ramsey's counterexample, we have to assume that <u>a</u> has some property b lacks (and <u>vice versa</u>); even though this is <u>not</u> reflected in the <u>atomic</u> (intensional) properties of <u>a</u>, b (upon all of which <u>a</u> and b agree). In other words, we assume <u>a</u>, b are discriminable even though no elementary function of individuals can discriminate between them—in order that <u>a</u>, b differ <u>only</u> in respect of "(ϕ). ϕ !x. \equiv . ϕ !a". To be an individual in PM2, however, is to be an argument to an elementary function. That is, <u>a</u> is a significant argument to a function ϕ ! \hat{x} if and only if " ϕ !a" is an elementary proposition. Equivalently, "a" stands for a definite individual iff ϕ_1 !a, for some ϕ_1 !x (if ϕ_1 !x is false of <u>a</u>, then $\sim \phi_1$!a). But, adding the assumption that <u>a</u> has <u>some</u> property to Ramsey's counterexample is demonstrably inconsistent. That is:

(1) Ø!a

(2) $(\emptyset): \emptyset a . \supset . (\exists y) [(\emptyset)(\emptyset y \equiv \emptyset a) . \sim \emptyset y].$

together with the identity-condition *13.191, generates a contradiction in PM2.²⁰ (Again, this is shown by consistency-trees in footnote 20).

It follows that either (1) or (2) is false. (2) is the formal equivalent of Ramsey's specification of the domain in which the Axiom of Reducibility fails. If it is false (for arbitrary \emptyset), there can be no such domain (or no such function as " (\emptyset) . $\emptyset!\hat{\mathbf{x}}$. Ξ . $\emptyset!a$ "). If (1) is false (there is no $\emptyset!\hat{\mathbf{x}}$ such that " $\emptyset!a$ " is an elementary proposition), then <u>a</u> is not an individual of type-0. Not surprisingly, if <u>a</u> is not of type-0, then

all individuals disagree with <u>a</u> in respect of some elementary function-since this is the condition for <u>a</u> to differ in type from that of individuals.

What, then, of the "diagonal argument" which appears to generate the Ramsey counter-example? In effect, I claim, the argument shows that the conditions for the discriminability (non-identity) of individuals in PM2 are too weak. This reflects back upon the previous discussion of the definability of infinite classes in PM2. We can infer that there are infinite classes in the domain of PM2 (without the Axiom of Infinity) only if we can guarantee that PM2 contains infinitely many names of individuals with different meanings; i.e. if the designata of individual names in PM2 can be discriminated one from another in every case (or, at least, denumerably many cases). But the discriminability of individuals a,b in PM2 demands intensional functions (atomic or elementary combinations of atomics) true of a but not b. In this sense, the individual a of the Ramsey counterexample could be specified coherently if we could add to the atomic propositions of PM2 denumerably many identity-propositions from the list: $\underline{a} \neq b_1$, $\underline{a} \neq b_2$, $\underline{a} \neq b_3$, ... -- whence <u>a</u> would be discriminable from each b_1 though agreeing with bi in every atomic property and disagreeing in respect of "(ϕ). ϕ ! \hat{x} . \equiv . ϕ !a" (' $\hat{x} \neq b_i$ ' is non-elementary in PM2). But identity propositions of the form "x = y" have a peculiar status in PM2--they are never primitive atomic propositions, but defined in terms of *13.01 in virtue of agreement in respect of all elementary functions. Indeed, the theorem which guarantess that identity is well-defined in PM2 (that from agreement in respect of elementary functions, agreement for all functions

follows (irrespective of order)--*13.11) requires the Axiom of Reducibility for its derivation.

The peculiar status I have accorded to identity propositions in PM2, above, is essentially that discussed in Wilson (<u>1959</u>, pp. 99-107). It was noted that to be an individual (of type-0) in PM2 was to be an argument to some elementary function--which, for the purposes of discussing Ramsey's criticism, I identified with having some atomic property. Now, it might seem that for propositions like "a = b", " $a \neq b$ " to be significant at all (true or false), then such propositions, if true, are necessarily true; and, if false, necessarily so. This much is suggested by Wittgenstein in <u>Tractatus</u>, 4.423:

Can we understand two names without knowing whether they signify the same thing or two different things. Can we understand a proposition in which two names occur, without knowing if they mean the same or different things.

To borrow an argument from Wilson (<u>1959</u>, p. 100), the point seems to be that, if, to fail to understand the significance of "Cicero is a Roman", whilst knowing the significance of "Roman", is but to fail to know that "Cicero" designates, then knowing the significance of "Cicero" is just knowing what item it designates. That is, to know the significance of a name, one needs to know <u>that</u> it is a name (which is a syntactic matter) and <u>what</u> it names (which is semantic). So, where "a", "b" are two names, one who knows the significance of "a" and of "b" will know that "a = b" is true--he cannot, as it were, understand "a = b", and then have to cast about for evidence whether a is the same as b or not. So, "a = b", if significant, is necessarily true or necessarily false--but it is only contingently <u>significant</u>, since it is contingent that there are individuals to be the designata of "a", "b".

If it is only contingently significant that a = b or that $a \neq b$, it might be thought to follow that it is only contingent whether there are individuals to be the arguments to elementary functions; i.e. that there are elementary properties only if there are individuals to have them. In one sense, this is true. For, suppose we construe "Red" as an atomic property of individuals. If there were no individuals, <u>a fortiori</u> there would be no coloured individuals and the statement "Something is red" would not be significant. In this sense, properties are <u>in re</u> (c.f. Wilson, loc. cit., p. 103). On the other hand, properties are <u>ante rem</u>, if they exist at all, in that even if the property Red is never exemplified, "Red" would be significant in statements like "Red is complementary in colour to green", "Red is between orange and purple" (Wilson's example). That is, the significance of "Red" does not depend upon the existence of red individuals; but it does depend upon there being some individuals which have colours--i.e. upon the existence of coloured individuals.

To apply these remarks to the discussion of Ramsey's second criticism, notice that, for the function $\hat{x} \neq a$ to be satisfied at all, then, necessarily, " $(\exists x)$, $x \neq a$ " is true, and there are at least <u>two</u> individuals (or <u>a</u> is not an individual; whereupon $\hat{x} \neq a$ is not a well-defined function since identity holds only between items of the same type). It is contingent that $\hat{x} \neq a$ is satisfied at all, because it is contingent that there are individuals. But the existence of <u>two</u> individuals, at least, is made a <u>neces</u>sary consequence of there being individuals. Parity of reasoning assures us that " $(\exists x,y)$: $x \neq y$. $\neq a$ ", if significant, is necessarily true--so, that there are <u>three</u> individuals is a necessary consequence of there being individuals. Clearly, we cannot stop at any finite number without jeopardizing the generality of the inference. Thus, we are confronted with the puzzling consequence that, if $\hat{x} \neq a$ is satisfied at all, then it is satisfied by infinitely many individuals, and this follows necessarily. Contrary to Ramsey's <u>first</u> criticism, then, if we suppose that his counterexample to the Axiom of Reducibility is coherent, then it is tautologous that at least one infinite class exists. On the other hand, if it is not a necessary consequence of there being individuals that there is an infinite class (arguments to the functions ' $\hat{x} \neq a$ ', ' $\hat{x} \neq \hat{y}$. $\neq a$ ', and so on), then the specification of the individual <u>a</u> such that, for each property F and individuals b₁, b₂,..., b₁ has every property of <u>a</u> except F, is not coherent.

To recapitulate: I have argued that Ramsey's first and second criticisms of ramified type theory are related. The first criticism--that infinite classes are not definable in PM2 (without the Axiom of Infinity)-is valid. Its validity, however, does not vitiate the ramified theory as a base logic for the logicist foundation of mathematics. For, if the Axiom of Infinity were not required of PM2, as I argued, the simple consistency of PM2 would be provable in PM2 by effective means. This we know, independently, not to be possible for PM2; just as it is not possible for most higher-order logics or set-theoretic foundations of mathematics. Every such higher-order system (Zermelo-Fraenkel Set Theory, Hilbert's <u>Grundzuge</u>, Von Neumann-Bernays Set Theory) requires an axiom providing for infinite

classes. To put this indirect argument another way, if it were provable that, at each type-level, there were infinitely many items to <u>be</u> the arguments to functions satisfiable only in infinite domains (e.g. the Successor function of Peano arithmetic, as ordinarily defined), then it would be provable---at each type-level---with respect to any function $\oint x$, that no set of propositions:

$$\{ \emptyset a_1, \emptyset a_2, \emptyset a_3, \dots, \sim (x). \emptyset x \}.$$

is satisfiable at that level. Such a proof would demonstrate the ω -consistency of PM2 <u>in</u> PM2 (without the Axiom of Infinity). Since ω -consistency implies simple consistency, the simple consistency of PM2 would be provable in PM2, per impossibile.

Ramsey's first criticism was shown to be related to his criticism of the Axiom of Reducibility through observing how Ramsey's counter-example to the Axiom required the assumption of an individual whose identityconditions could not be given in terms of PM2 (i.e. the individual would have to be discriminable from every type-0 individual, yet share every property of an arbitrary number of individuals). This assumption, it was argued, is not coherent if we insist upon the intensionality of atomic propositional functions of PM2--for, every individual has to satisfy some intensional function, not involving identity, to be discriminable from others. The individual designated in Ramsey's counter-example did not satisfy this condition.

For all that the above amounts to <u>criticism</u> of Ramsey's attack upon ramified type theory, it must be noted that Ramsey's objections do reveal severe inadequacies in the use of the axioms of Reducibility and Infinity

in PM2. Some of these inadequacies had been anticipated by Wittgenstein, in criticising the first edition of PM (discussed above and in Appendix A). Nor surprisingly, therefore, the major revision to PM2 that Ramsey proposed (Ramsey, <u>1925</u>, pp. 49-56) was to deny the <u>intensionality</u> of propositional functions, and to replace the notion of an atomic function-in-intension by that of a function-in-extension. In effect, this revision is as follows:

As a result of his acceptance of Wittgenstein's tautology theory of logic, Ramsey had redefined the PM2 notion of a "predicative function of individuals" in the ramified theory as "any truth-function of atomic functions and atomic propositions" (loc. cit., p. 49). Not all non-elementary functions of PM2 were truth-functional. Thus, the first effect of this redefinition was to make non-atomic functions of individuals extensional in the sense that truth-functionally equivalent propositions were to be intersubstitutable salva veritate in every non-elementary function of propositions. Functions of individuals, hence, remained intensional only in respect of their constituent atomic propositions, which were intensional. The intensionality of an atomic proposition, in PM2, amounted to the supposition that, if $\emptyset : \hat{\mathbf{x}}$ is atomic, it collects together a definite set of propositions (its values) in all of which what is asserted of individuals is constant; i.e. that "Ø!a" predicates of a the same thing as "Ø!b" predicates of b. Supposing, for the purposes of illustration only, that dyadic functions defined over natural numbers are atomic functions of individuals in RTT, this supposition would require that, although all and only successors of numbers are greater-by-one than their arguments, if $S!\hat{x}, \hat{y}$ is the relation (in intension) ' \hat{x} succeeds \hat{y} ' and $G!\hat{x},\hat{y}$ is ' \hat{x} is greater-by-one than \hat{y} ', then

S! \hat{x}, \hat{y} , G! \hat{x}, \hat{y} collect together <u>different</u> ranges of atomic propositions. For, given any numbers a,b, what "S!a,b" asserts of a, b is not what "G!a,b" asserts of a,b. In particular, that a is greater than b ("a > b") follows analytically (in some sense) from a is greater-by-one than b (because the <u>intension</u> of G! \hat{x}, \hat{y} is included in that of $\hat{x} > \hat{y}$). On the other hand, though it is always <u>true</u> that, if a succeeds b, then a > b (and can be derived logically), the intensionality of S! \hat{x}, \hat{y} requires that "a > b" does not follow analytically from "S!a,b". In other words, there are, in principle, contexts $f(\hat{p}\hat{x}, \hat{y})$ of PM2 in which "f(S!x,y). $\equiv_{x,y}$. f(x > y)" fails (e.g. f(A) = "it is believed that-A"); even if such contexts are never required for mathematical purposes.

Ramsey proposed that the notion of a predicative function of individuals be modified so as to deny that " \emptyset !a" says about <u>a</u> what " \emptyset !b" says about <u>b</u> (Ramsey, loc. cit., p. 52). The effect of the modification is dramatic, but wholly removed from the original, philosophical grounds for ramified type theory. It concludes this section to describe how far from Russell's original basis for ramified type theory--the Vicious Circle Principle, the doctrine of incomplete symbols, and the theory of judgement--the "Ramsification" of type theory takes us.

Denying the intensionality of the predicative functions of PM2, Ramsey explained the modified notion as follows (it is not defined, but taken as primitive):

We ... explain ... the new concept of a propositional function in extension. Such a function of one individual results from any many-one relation in extension between propositions and individuals; that is to say, a correlation, practicable or impracticable, which to every individual associates a unique proposition, the

For the purposes of classifying these functions the heirarchy of types is taken as fundamental. Individuals are of type-0; functions of individuals are of type-1, functions of type-1 functions are of type-2; and so on. The <u>order</u> of a function or proposition is now made to depend upon the type of arguments to bound variables it contains. Propositions or order-0 (elementary) contain no variables (reall or apparent). They are atomic, or truthfunctions of atomic propositions. Propositions of order-1 contain at least one variable ranging over type-0 items. Propositions of order-2 contain at least one variable ranging over type-1 items. Similarly, functions are elementary, of order-1, or order-2 according as they are matrices, contain a bound variable over individuals or bound functionvariables, respectively (Ramsey, <u>1925</u>, pp. 46-7). Functions in extension are symbolised by " \emptyset_e " and the totality of such functions is the range of " \emptyset_a " as it occurs bound in such assertions as:

$$(\phi_{e}). \phi_{e^{x}} \equiv \sum_{x,y} \phi_{e^{y}}.$$

This asserts that, for every correlation of individuals with propositions, the proposition correlated with any x is equivalent to that correlated with y--where equivalence is here confined to truth-functional equivalence. This totality is clearly impredicative, in Russell's sense, in that the propositional equivalences, which are values of particular functions in extension

included in this totality, will include propositions which correlate individuals with propositions which are truth-functionally equivalent. Yet the contradictions resulting from impredicative totalities need not arise for such functions in extension--since whether or not there <u>is</u> such a correlation does not depend upon whether some expression defining the correlation can be written in the ideography of PM2, but only upon the actual (matter of fact) equivalence of correlated propositions.

Russell's response to Ramsey's modification was cautious:

The drawback to Ramsey's functions in extension ... is that no instance of them can be given. If the individuals in the universe were finite in number and all known, instances could be given by enumeration, but in the absence of these two conditions enumeration is impossible, and no other method of specifying such a function exists. (Russell, "Review of Ramsey", <u>Philosophy</u>, 7, January 1932, pp. 84-86).

Russell goes on to comment that there are analogies between this use of functions in extension and the use of infinite, non-lawlike decimal expansions in mathematics. But he concludes with some reservation:

If a valid objection exists--as to which I feel uncertain-it must be derived from inquiry into the meaning of 'correlation'. A correlation, interpreted in a purely extensional manner, means a collection of ordered pairs. Now such a collection exists if somebody collects it, or if something either empirical or logical brings it about. But, if not, in what sense is there such a collection? I am not sure whether this question means anything, but if it does, it seems as if the answer must be unfavourable to Ramsey. (Russell, loc. cit., p. 85).

Russell's point is well-taken and illustrates clearly Russell's "constructivist" attitude towards the existence of classes at the time of PM2. For, the effect of "extensionalising" the functions of PM2 is to cancel out the advantages of eliminating committment to classes by means of the contextual definitions of *20. On Ramsey's proposal, the existence of a correlation between individuals and propositions does not depend upon any practicable enumeration of the domain and range of the correlating function. It is simply <u>any</u> many-one mapping from the class of individuals to the totality of propositions. Thus, functions in extension are simply finite or infinite classes (of couples, triples, etc.) by another name. Ramsey considers whether " $\emptyset_e \hat{x}$ " should be identified with " $\hat{x}(\emptyset x)$ " and chooses not to do so on pragmatic grounds:

Predicative functions of functions are extensional, ... if the range of $f(\emptyset \hat{x})$ be that of predicative functions of functions:

 $\phi_{e^{x}} \equiv_{x} \phi_{e^{x}} \cong f(\phi_{e^{x}}) \equiv f(\phi_{e^{x}}).$

This is because $f(\emptyset_e \hat{x})$ is a truth-function of the values of $\emptyset_e \hat{x}$ which are equivalent to the corresponding values of $\emptyset_e \hat{x}$... If we assumed this, we should have a very simple theory of classes since there would be no need to distinguish $\hat{x}(\emptyset_e x)$ from $\emptyset_e \hat{x}$. But, though it is a tautology, there is clearly no way of proving it, so that we should have to take it as a primitive proposition. (Ramsey, 1925, p. 55)

The primitive proposition which Ramsey refers to here is, of course, merely a notational variant of the Axiom of Extensionality of Set Theory. Introducing it into PM2 would effectively obliterate the distinction between propositional functions and classes--type-distinctions becoming simply a heirarchy of conditions upon membership (members of classes of classes differing from members of classes of individuals). Instead, Ramsey prefers to avoid this additional primitive proposition, preserving the defined notion of classes of *20 as separate from functions in extension. A class of individuals can, thus, be defined as the argument-range of a function in extension all of whose values are true elementary propositions. And a class of classes will be defined by that predicative function which is the logical sum (infinite disjunction) of $\hat{\phi}_{e^{X}} \equiv_{X} \hat{\phi}_{e^{X}}$, for all functions whose argument-range is a member class of the class of classes. Such a logical sum cannot be written down, if the class of classes is infinite-but this Ramsey regards as an accident of human finitude:

The logical sum of a set of propositions is the proposition that these are not all false, and exists whether the set is finite or infinite. (ibid., p. 56, footnote 1).

Committment to classes--finite or infinite--thus returns in Ramsey's assumption that the finite or infinite disjunction of all values of functions which define classes of individuals comprises a definite value-range for a function defining a class of classes. This exists whether the disjunction can be written out in the ideography of PM2 or not.

As a consequence of these assumptions, the contextual elimination of class-expressions of *20--by means of Russell's theory of incomplete symbols--can no longer be said to exclude from Ramsey's theory those impredicative totalities which violate VCP. For the existence or non-existence of such totalities does not depend upon the pre-assigned value-ranges of predicative functions of each order. It should not be thought, however, that Ramsey was content to allow the re-appearance of the threat of viciouscircle fallacies. For, Ramsey had <u>rejected</u> Russell's diagnosis of the source of paradox as violation of VCP, (Ramsey, <u>1925</u>, p. 41). In its place, he proposed a regrouping of the known paradoxes into two kinds according as either "they involve only logical or mathematical terms such as class and number, and show that there must be something wrong with our logic or mathematics" (ibid., p. 20), or "they contain some reference to thought, language, or symbolism, which are not formal but empirical terms ... they may be due not to faulty logic or mathematics, but to faulty ideas concerning thought and language" (ibid., p. 20). If we call paradoxes of the first kind "logical paradoxes" and include therein Russell's paradox, Burali-Forti's paradox of the greatest ordinal and the Zermelo-König paradox of similar classes, then each of these is resolved by the type restrictions upon arguments to functions. The Russell paradox, for example, requires that the class of non-self-membered classes can or cannot be a member of itself--whereas the members of classes must differ in type from the classes themselves, so the supposition is nonsignificant. In the second group of paradoxes -- called the "semantic paradoxes" -- Ramsey includes the Liar paradox, Richard's paradox, Grelling's paradox of 'heterologicality' and a number of related contradictions. In all of them, he supposes, their solution requires some clarification of concepts involving "thought", "language", "meaning", "symbolism" or "reference". Such solutions, he claims, are not relevant to logic or mathematics.

In sum, then, we can acquiesce in the Ramsification of type theory if we accept two conditions: (i) the assumption of the existence of finite and infinite classes as extensional correlations--even if such classes are not definable by means of logical expressions; (ii) the separation of the paradoxes into "logical" and "semantic". Neither condition conforms to Russell's original motivation for introducing ramified type theory into PM. Assuming the existence of classes at outset, there remains little reason to prefer Ramsey's type theory to any other set-theoretic foundation of mathematics. Logicism, as a philosophy of mathematics, becomes the claim

that mathematics can be reconstructed in a suitably chosen axiomatic set theory which posits both the extensionality of functions, relations and predicates, and the infinity of the domain. If logicism, in brief, is the claim that logic and mathematics are identical in respect of content and deductive strength, logic has to be supplemented, on Ramsey's view, by the theory of sets. In addition, if the semantic paradoxes are to be dismissed as irrelevant to logic and mathematics, then logic has at the same time to be confined to the formulation of wholly extensional, symbolic calculi (axiomatic or otherwise) which make little or no claim to represent canons of judgement, inference and reasoning embodied in thought and language. For the purposes of Ramsey's logicism, therefore, logic has to be artificially supplemented and philosophically confined. It is a small step from this conception of logic to that pre-occupation with "ideal language" views which is characteristic of the successors to Russell and Wittgenstein's logical atomist views--the logical positivists. The discussion of this section, thus, illustrates that historical transition from traditional conceptions of logic--entrenched in theories of judgement, analyticity, and meaning--to the ideal language view discussed, above, in the Introduction. Just how far this conception of logic is removed from that of the original proponents of logicism--Frege, Russell and Whitehead--can be inferred from the extended discussion of these last two sections.

The enterprise of giving a systematic exposition of the ramified theory of types and its justification--from its inception in Russell <u>1908</u> to its modification by Ramsey in <u>1925</u>--has been long and complicated. It has required a discursive analysis of the changes in Russell's philosophy of logic and mathematics through the period 1905 - 1927. Through this period, Russell's views were rarely static and almost always holistic--changes in his theories of meaning and judgement and in his conception of logic and mathematics reciprocally influenced one another. In addition, through this period, his general philosophical outlook underwent radical change in the light, first, of Wittgenstein's critical attacks upon the foundations of FM; and, second, in the light of his own perception of Wittgenstein's doctrines. Of those doctrines, the one which Wittgenstein came to see as <u>replacing</u> the theory of types--the doctrine of showing--is arguably one doctrine that Russell never fully understood. In passing, now, from discussion of the role of type theory in the philosophy of logic and mathematics, to consideration of its role in accounting for significant predication and the nonsignificance of category-mistakes, it is to the doctrine of showing that I turn first in Section D.

In view of the length and complexity of this section, I offer, here, a summary and recapitulation of the main steps in the argument:

(IX): Summary and Recapitulation:

(1) My chief concern has been to examine how Russell realised the demand that the diagnosis of the paradoxes as violations of VCP was to be a consequence of his <u>positive</u> theories of meaning, truth and judgement. I argued that the solution--ramified type theory--developed out of three central Russellian doctrines:

(i) the account of propositional functions and of propositions in terms of function and argument;

(ii) the doctrine of incomplete symbols: that descriptive-, propositional- and class-denoting phrases are not autonomously meaningful, but are meaningful only in the manner they contribute meaning to a whole sentential context. Names are complete symbols standing for objects of immediate acquaintance. (iii) the multiple relation theory of judgement: dispensing with true or false propositions as objects of understanding, belief or judgement, Russell analysed judgement as a multiple relation between a subject and the constituents of judging-complexes.

(2) (1) was to support the ramified theory through the assumption that functions presuppose determinate totalities of propositions as their valueranges. In effect, propositions analysed as assertions of functions (of different orders) of arguments (of given types) are classified into a heirarchy of branching orders according to their generality (the ranges of their bound variables).

A recurring difficulty in this account was that functions do not have a clear status in the ontology of FM. As what is asserted of individuals, they are expressions; but, as arguments to other functions, they are non-linguistic items. The criticism from Quine--that Russell's doctrine of incomplete symbols is vitiated by use/mention confusions--was rejected, however, on the ground that Quine presents Russell with an unrealistic dichotomy: functions are either open-sentences or attributes--when functions cannot be explained as either of these. An attempt was made to offer a coherent account of propositional functions as <u>expressions</u> which are <u>asserted of</u> arguments.

(3) It was argued that (ii)--the doctrine of incomplete symbols--and (iii)-the multiple relation theory--support the grounding of the ramified heirarchy in differences in kind between the judgements made in asserting functions of arguments. The basic distinction was drawn between elementary judgements-whose understanding involved only truth-conditions--and general judgements asserting some or all values of functions. From this account of general judgements, Russell was led to characterise logical truths as "completely general".

(4) The multiple relation theory was discussed in detail in Appendix A. The theory as a whole, its use in supporting type theory and the basis for the ramified heirarchy in PM were all subject to sustained criticism from Wittgenstein in 1913-1914. In Appendix A, I offer a reconstruction of those criticisms which shows why Russell felt "paralysed" by Wittgenstein's attack; and how Russell gave up writing a major work because of it--fearing thereafter that the basis for ramified type theory was incorrect, especially where it led to the need for the Axiom of Reducibility. Several of Wittgenstein's criticisms led to major changes in the second edition of PM through Russell's espousal of logical atomism.

(5) In PM2, Russell gave up much of the doctrinal basis for ramified type theory--leading to his fear that, in particular, the Axiom of Reducibility was only pragmatically justified. The changes in PM2--abolition of the real/apparent variable distinction, and the attempt to dispense with the Axiom of Reducibility in favour of Wittgenstein's thesis of extensionality-effect radical changes in the character of the ramified theory. I reconstruct from these changes, a version of the ramified theory of PM2--the system RTT-to facilitate comparison with Ramsey's critique of PM2.

(6) Ramsey embraced a Wittgensteinian conception of logic. As a result, his criticisms of ramified theory are extensions of criticisms of Wittgenstein--more fully and technically articulated. Ramsey attempts to dispense with the Axioms of Reducibility and Infinity by revising the notion of a predicative propositional function as an <u>extensional</u> correlation of arguments (of appropriate types) and propositions. He argues that only typedistinctions are necessary to avoid paradoxes which are relevant to logic and mathematics--the remaining paradoxes being semantic in nature, hence not relevant to logic.

(7) In concluding the section, I offered several criticisms of Ramsey's argument. His first and second criticisms of PM2 are related. The first is valid--but reveals a shortcoming of PM2 which is shared by most higher-order systems of logic: the non-categoricity of their axiom-sets relative to the domain of mathematical objects. His second criticism I argued, was not coherent. He attempts to find a counter-example to the Axiom of Reducibility, but makes illicit assumptions concerning the identity-conditions for individuals in PM2. Finally, I examined Ramsey's alternative account and argued that it led, in effect, to a separation of matters logical from matters of "thought and language" which was alien to logicism as originally propounded. This separation of subject-matters has, however, become characteristic of modern mathematical logic.

SECTION D: Types and Categories in Ordinary Language.

Throughout the preceding section a recurring question concerned whether type-differences are differences in things or in the expressions which stand for things. In other words: is a theory of types a theory about language and the ways in which expressions are combined in language, or is it a theory about the world and the kinds of things there are? Phrased thus, the question offers us what may be an unreasonable dichotomy. To the extent that descriptive language is about the world and the items in it, a theory of the manner in which expressions may be significantly combined may tell us a great deal about the kinds of items that exist in the world. Conversely, since to describe the items in the world is to use some language or other, a theory of the manner in which items in the world differ in kind may tell us a great deal about what can be significantly stated in such a language. Confronted with this dichotomous question, then, a reasonable answer may turn out to be that a theory of types is about <u>both</u> language and the world.

Up to this point, the discussion of type theory has been confined-more or less--to the role of the theory in resolving paradoxes in logic and set theory. In this section, the scope of discussion is widened considerably to include significant discourse as a whole. The intent of the section is to examine whether a theory of types can explain the nonsignificance of category-mistakes. This question requires that we explain what a categorymistake is, why it is nonsignificant and how a theory of types might account

for this. The important question, then, is not: are all category-mistakes type-mistakes? (to which the answer will always be "It depends upon what you mean ..."); but rather: <u>how</u> (if at all) is the nonsignificance of category-mistakes related to a theory of types?

To answer this last question I have chosen to consider several competing accounts of category-mistakeness. The section begins with the briefest possible survey of relevant distinctions--particularly between "theories of categories" and "theories of category-mistakes"--and rapidly becomes particularised to several of the most recent accounts of the latter. Each of these accounts can be subsumed under one of Models I - III described in the Introduction (pp. 11-17)--these being generic frameworks (or 'approaches') into which rival accounts can be embedded. In each case, criticisms I offer of rival accounts may or may not apply to the framework as a whole; where they do apply, they constitute <u>prima facie</u> grounds for rejecting that whole approach to explaining category-mistakes. In the end, the conclusions of this section will be essentially negative: none of the rival theories I consider seem to me to provide an explanatory account which meets the criteria I listed in the Introduction (section B). It must await Part II of the thesis to expound upon the alternatives I have to offer.

As a preliminary to consideration of rival theories of categorymistakes, it is necessary to focus attention upon what a category-mistake is. In the Introduction I classified the examples ((1) - (10), p. 1) that I cited as belonging to that subclass of unintelligible utterances in context which fail to be significant through some property of the utterance itself (rather than some 'accident' of context). To classify <u>category-mistakes</u>

as non-significant utterances of this kind appears to entail that such sentences as fail in context contain some kind of semantic-mistake. In other words, the remarks of the Introduction may have suggested that a category-mistake is some kind of breach of semantic-rule. Amongo the accounts I consider, there is one such description of category-mistakes: that, in general, category-mistakes are a sub-species of semantic anomalies which result from violations of linguistic rules belonging to the semantic component of a language. (In general, this is a Model III-type account, attributable to Katz and Fodor (1963)). This account of category-mistakes is one upon which I shall exert the utmost critical pressure; for, not only do I believe that it misdescribes what is philosophically important about category-mistaken assertions, I believe also that it misrepresents the nature of the semantic component of a language--imposing upon it a quite arbitrary separation of 'matters of language' from 'matters of fact'. These criticisms will be substantiated below (p.318+).

At other points in the Introduction--where I was concerned, for example, to introduce the <u>contextual sensitivity</u> of utterance meaning--I ascribed the nonsignificance of category-mistakes to asserting, in context, of items of one 'type' or kind, what is predicable only of items of different types. Category-mistakeness was here made to depend upon categorydifference: where the <u>fact</u> that items <u>a</u>, <u>b</u> belong to different categories may appear to support inferences to its being <u>analytically</u> or <u>necessarily</u> <u>false</u> (false by definition) to assert of <u>a</u> what is predicable only of <u>b</u>-type items. Another account I consider below--subsumable under Model I and attributable, <u>inter alia</u>, to Prior, <u>1954</u>, Ewing, <u>1937</u>, and Baker, <u>1956</u>--proposes
thus, to treat category-mistakes as a sub-class of necessary falsehoods (a priori falsehoods, self-contradictions). With respect to such accounts, much depends upon the notion of "falsity" involved. If, by a priori falsehood, they intend to subsume category-mistakes under the genus of "a priori rejectable utterances"--where an utterance is rejectable a priori if it is unintelligible because nonsignificant in context--then, as I shall argue below, this locates one distinctive feature of category-mistaken utterances. The preferrence to classify category-mistakes as false, rather than meaningless, is, indeed, well-motivated. For it appears to be supported by what I shall call the "Argument from Significant Denial". If a predication like "Virtue is blue" is category-mistaken, it can plausibly be suggested that its negation "Virtue is not blue" (or "It is not the case that virtue is blue") is significant and true. If the negation of a category-mistake is true, bi-valency requires that category-mistakes be false. I shall have occasion to consider the Argument from Significant Denial in detail, below.

Just such a treatment of category-mistakes--as necessary falsehoods-is found, for example, in the following:

Statements like "Equality is easily annoyed", "Greenness is hexagonal" do involve category-mistakes, but this is because they can be shown to lead to contradictions rather than because they can be read off at once as absurd. (A.J. Baker, <u>1956</u>, p. 20).

Accounts of this kind, too, I shall be concerned to criticise--particularly where the account in question equates "necessary falsehood" with "false by definition". For, it does not seem to me that philosophers who have accused others of being category-mistaken believe them to have made mistakes in defining their terms. Similarly, I do not believe that category-mistakes can be identified with contradictions (or as "leading to contradictions"). Certainly, a contradiction is necessarily false (<u>not</u> meaningless); so, perhaps with a certain naivety, negations of contradictions have to be regarded as tautologies. And, continuing in this naive vein, it does not seem to me to be <u>tautologous</u> to say that greenness is not hexagonal, nor to deny that the number seven is indifferent to tomato soup. (Routley's example; see: Introduction, p. 1, footnote 2).

Further consideration of the differences between category-mistakenness and necessary falsehood, however, does lead to non-naive speculation upon the relations between our supposed <u>a priori</u> rejection of category-mistakes and our apprehensions of category-difference. If the diagnosis of a sentence as category-mistaken is made to depend upon an apprehension of category-difference, what do differences of category distinguish? (c.f. Are type-differences in expressions or things?). In short: <u>what are cate-</u> gories?

If we boggle at the task of answering this question, we can pause at least to enquire whether a theory of category-mistakes should, <u>ipso</u> <u>facto</u>, embody a theory of categories. I have chosen not to examine the two thousand year history of theories of categories in this thesis. This choice is not wholly governed by prudence; for, I do not believe that an account of the non-significance of category-mistakes need compel its proponent to espouse a theory of categories in the sense with which philosophers, from Aristotle to Kant, have endowed the term.

When we ask "what are categories?", it may seem as if we want a

description of some special items in our ontology-items which, like classes, group together individuals. Yet Aristotelian or Kantian theories of categories explain not so much what categories are, but rather what constitutes "belonging to a category". Or, better, they answer the questions: "what is categorising?", "what justifies our allocating items to categories?" A theory of categories, that is, demands criteria only for sameness and difference of category with respect to items. It need not demand that categories be items, themselves. Nonetheless, we get very different answers to the question of what categories are if we ask "do category-differences in items stand to categories as colour-differences in house-paints stand to indigo blue, sepia yellow or scarlet?" than if we ask "are categoryallocations related to category-differences as budget-allocations are related to differences in economic needs, policies and interests?" In the first case, we expect category-membership to depend upon quite specific properties of the items categorised. In the second, we expect only general criteria for items to be allocated to one, rather than another, category (just as money is allocated to agriculture, say, rather than education), without supposing that category-allocations are final or immutable, or that they depend upon intrinsic qualities of the items themselves. A colour-chart serves as a paradigm for colour-differences between housepaints; whereas a budget serves only the needs and interests of the economy-it can change with changing priorities. Such analogies, though, perhaps serve only to deepen the mystery over how best to ask the question "what are categories?". A better approach to an answer may be to survey what philosophers have, in the past, taken categories to be.

The term "category" has been used in philosophy to signify a very general class, a <u>summum genus</u> or type marking a division of items in our ontology; or, at least, a conceptual division that has to be made if we are to talk sensibly about the world. At the least, what seems required of an allocation to categories is that to items in different categories we not apply the same descriptive terms, unless we speak non-literally or equivocally. For example, a zeugma like:

(1) The cape was stormy and loose about her shoulders. equivocates upon the ambiguity of "cape" between 'article of clothing' and 'geographical feature'. Yet, a theory of categories which made articles of clothing and geographical features different in category on the strength of <u>one</u> equivocal term would certainly be profligate. A theory of categories, that is, cannot be expected to account for all varieties of equivocation. (This point has been made often enough: in recent times by Sommers (<u>1963</u>, <u>1965</u>) and Passmore (<u>1961</u>, Ch. 7); less recently by Aristotle (<u>Topics</u>, 107a3-17)). On the other hand, we do want to regard <u>some</u> equivocal statements as based upon category-differences. To borrow an example from Sommers (<u>1965</u>, p. 263), most of us would regard "hard" as ambiguous in:

- (2) Some questions are hard.
- (3) Some chairs are hard.

---enforcing the ambiguity through a category-difference between chairs and questions. For, if "hard" were not equivocally used in (2) and (3), then we could perfectly well maintain that:

(4) This question is harder than that chair.

makes unequivocal sense. The problem is, then: which equivocal predications enforce category-differences and which are mere equivocations (like the zeugma (l))? Here, I am concerned only to argue that, whatever an answer to this question may be, it cannot be expected of a theory of <u>categories</u> that it account for the difference between equivocal predications and category-mistakes. In other words, a theory of categories may answer the question: what are the most general kinds of item there are?; but, in answering that question, it need not supply criteria which separate predications which enforce category-differences (like "is hard" over chairs and questions) from those that are heterotypical across categories (like "interests Mary" over men and mathematics). For:

(5) Men interest Mary more than mathematics does. makes unequivocal sense, without committing us to allocate men and mathematics to the same category ('objects of Mary's interest').

In essential respects, this separation of questions of categoryallocation from questions of category-mistakenness is Aristotelian. Aristotle's answer to the question: which tests of category-difference confirm allocations to categories and which are mere equivocations across categories? remains the most reasonable. If we ask for tests of categorydifference, Aristotle claims (Topics, 107a3-17, Categories, 1a-1b), we must distinguish <u>equivocal</u> statements (which may or may not <u>mark</u> categorydifferences) from statements which violate category allocations and are, therefore, category-mistaken. Thus, the appearance of literal absurdity accompanying a zeugma can always be removed by distinguishing the ambiguous senses of an expression in the equivocation. When an assertion is genuinely

category-mistaken, however, literal sense can never thus be restored.

Aristotle's claim is supported by his account of essential <u>versus</u> accidental predication (<u>Metaphysics</u>, Bk. 5, 1025al3+ and <u>Posterior Analytics</u> 75al8-22), his explanation of 'pros hen' equivocals (<u>Metaphysics</u>, A 990b22-99 and Γ 1003a32-b20) and by his description of the ten categories (<u>Categories</u>, 1b25-2b10). I shall argue, however, that an account of the distinction between equivocal predications and category-mistaken predications need not and, indeed, should not derive its support from a metaphysical theory of categories. That is, I shall argue that a theory of category-mistakes need not be a theory of categories.

Certain predications, Aristotle claimed (loc. cit.), apply to items in some or all of the ten categories: substance, quality, quantity, relation, location, time, posture, state, action, affection. Predicating, say, "exists", "is good", "is the same" of items does not fix the category of those items because such predications are <u>equivocal</u>--they cut across category-differences. Not all equivocations, for Aristotle, cut across category allocations (c.f. the discussion of "sharp flavours" and "sharp edges" in <u>Topics</u> 106a7-107a35). Those which do are predicable <u>accidentally</u>. That is, whatever is univocally predicable of items in any category except substance is so only <u>essentially</u>--the accidents, in non-substance categories, of individual (primary) substances do not themselves have accidents. Thus, what is predicable of non-substance items (qualities, relations, states, etc.) is <u>paronymous</u>; it applies to those items only derivatively from its application to individual substances (<u>Categories</u>, 2b4-6). Frequently, Aristotle speaks of "paronymy" as a kind of equivocation (not 'pros hen'

equivocation) applying to how we speak of the sense of predicables of non-substances, like "healthful", "virtuous", as they are exhibited in the qualities, states, and actions, say, of individual substances. In the category of substance, though, what is predicated of individuals (primary substances) is <u>either</u> an accident (from a non-substance category) <u>or</u> belongs to the definition (secondary substance) of that individual (the genus and differentiae which are essentially true of the subject). What is predicable essentially of individuals is so univocally--and primary substance, itself, is never predicable of anything (ibid., 3a32-35).

What is problematic in the strategy of Aristotle's argument is that the separation of equivocal predications from category-mistakes requires the metaphysics of "essence" and "accident". Without it, the Aristotelian thesis has an air of circularity. "Healthful", for example, is predicable both of individual qualities and of people (individual substances), in so far as the latter are healthy, but it is so only in different senses (equivocally) because "healthy" is not essential to a quality. Temperance may be healthy, paronymously, because conducive to health, and thereby a "good"; but were the preservation of health not a "good", that would not change the essential nature (the definition) of the quality of temperance. That "healthy" is predicable of temperance, therefore, is equivocal because derivative from its being good for the health of individuals.

On the other hand, that a predication is equivocal should not be made to <u>depend</u> upon what is essential to items in each of the categories and what is accidental. Yet the manner in which Aristotle draws

category-distinctions--items (what is predicated) belong in separate categories according as they are possible answers to different interrogatives: What is?, How big?, What like?, Where? etc .-- makes the test for distinguishing equivocal from univocal categorial predications presuppose category-allocations. That is, predications univocally apply to an item provided that they satisfy the same interrogative and, if non-substantival, are essentially true of that item. Items like 'snub-nosedness' (Quality), 'five-foot height' (Quantity), 'drunkeness' (State) and 'boredom' (Affection) are allocated to whichever category their correlative predication appears in as a possible answer to questions such as "What is Socrates like?" - "He is snub-nosed"; "In what state is Socrates?" - "He is drunk". What we predicate of such items, thus, is true or false of the particular quality, quantity, etc., concerned only according as it is univocally predicable of items of that category and essential to their being of that category. Otherwise, for example, "being unattractive" is only equivocally predicable of, say, snub-nosedness, because being unattractive is inessential to that quality of noses--it is true of it only so long as there are individuals who, in being snub-nosed, are unattractive.

I conclude, thus, that determination of which equivocal predications signal category-differences and which do not, for Aristotle, requires prior determination of what is essential to items in non-substantival categories. This, in turn, depends upon the underlying Aristotelian metaphysics of the essential and accidental attributes of individual substances. To the extent, though, that we are unwilling to <u>assume</u> such an underlying metaphysics for an account of the absurdity of category-mistakes, we should not

require that an account of category-mistakes rest upon a prior determination of category-membership. For, we will want the absurdity of categorymistaken predications to provide a <u>test</u> for misapplication of categories. So, we should not suppose that categorial differences, drawn in terms of what is essential to items and what is accidental, can be delimited prior to, and independently of, our recognitions of category absurdity.

In sum, then, an account of the nonsignificance of category-mistakes should provide criteria distinguishing predications which are simply equivocal from those which are genuinely <u>hetero-typical</u>, in the sense of applying to items which, for independent reasons, we want to maintain are categorially distinct. Reverting to example (5), for illustration, we want to be able to declare men and mathematics different in kind (or 'type') even though it is significant to say of both that they interest Mary. Our account, thus, should <u>preclude</u> 'objects of Mary's interest' from being a determinate category of item, despite the significance of (5); because:

(6) Men are of opposite sex to Mary is paradigmatically significant; whereas:

(7) Mathematics is of opposite sex to Mary could well be included alongside (1) - (10) of the Introduction (p. 1) as a further illustracion of category-nonsense. After all, from (5) and (6) we want to be able to infer that Mary's interest in the opposite sex is categorially different from her interest in mathematics--if only because Mary attributes qualities to men which no sensible person would attribute to mathematics.

We can generalise this conclusion to the requirement that any account

of category-mistakes should provide some basis for those inferences we make, characteristically, on the strength of what can be asserted significantly of items in different categories. Indeed, to substantiate a significance-claim---an assertion that a sentence is or is not significant--it is often sufficient to draw inferences which conclude that a given sentence, if significant (and true), would support further non-equivocal predications of items in separate categories. For example, to one who would claim (with Quine, 1960, pp. 130-131), that both chairs and guestions are unambiguously hard, it should suffice to point out that this entails its being sensible to ask whether canvas chairs are harder than the question of God's existence. If this last is not nonsense, nothing Thus, if the account of category-mistakes, in terms of predicating is. of one type of item what is predicable univocally only of a different type, is to function at all, then such an account should support inferences based upon significance-claims. And it is hard to see how prior allocations of items to categories or types which did not appeal to the literal absurdity of some unambiguous cross-categorial predications could support such inferences.

There seem to be two general answers that might be made to the question: what kind of mistake is a category-mistake? At outset, we can separate answers which classify category-mistakes as making a particular kind of <u>false</u> claim; from those which construe category-mistakes as <u>meaningless</u>. Within these general answers, various alternatives can be distinguished--paths through the following diagrams that logicians and philosophers have taken in the past:







With respect to these diagrams, it should be noted that several of the alternatives overlap--even in responding to the initial question whether category-mistakes should be classified as false or meaningless. For example, if, like Quine (<u>1960</u>, p. 223), one regards category-mistakes as necessarily false because false by definition (false in virtue of the meanings of constituent terms), then it makes little difference whether one calls this a kind of meaning-failure or an extreme kind of falsity. Similarly, claiming category-mistakes as <u>a priori</u> rejectable may be justified by <u>direct</u> appeal to their conflict with <u>a priori</u> principles (e.g. "Justice cannot be blue, because, <u>a priori</u>, virtues are not coloured"), or by <u>indirect</u> appeal (e.g. "That Justice is blue is inconceivable (Drange, <u>1966</u>, 'unthinkable'), because, <u>a priori</u>, whatever is coloured is spatially extended and a virtue is not a spatially extended item"). Either way, we could call "Justice is blue" necessarily false or <u>a priori</u> rejectable, and, hence, meaningless (in the sense of conveying no information).

Secondly, for several of the accounts I consider below, it is not clear immediately to which node on the diagrams the account should be assigned. This may be because the account in question is insufficiently explicit, or because my interpretation is contentious. For example, as is clear from the extended discussion of Wittgenstein's Tractarian doctrine of showing, which follows, it is highly debatable whether his account can be made to fit any of the positions in the diagrams. From that discussion we might infer that Wittgenstein regarded sentences violating typerestrictions as "essentially ungrammatical"; i.e. such violations conflict with syntactic principles essential to a language's being capable of expressing thought. Such principles of syntax, however, are never statable, on Wittgenstein's view, because they can only be shown. Similarly, it seems that both Russell and Carnap would have denied that a theory of types can be applied directly to the sentences of ordinary language. That there are grammatical, but nonsignificant sentences in ordinary language, they argued, is one feature of language which necessitates a "rational reconstruction" (in Carnap's sense) of notions like "meaning", "logically true",

"analytic", and so on. Thus, their applications of type theory were restricted to explicitly formalised languages.¹

Of these various approaches, clearly the two threats to investigating the contribution of type theory to accounts of the nonsignificance of category-mistakes derive from:

(i) those who deny, on <u>a priori</u> grounds, the possibility of a nonvacuous, non-stultifying formulation of type theory; (Wittgenstein, <u>1922</u>; Black, <u>1944</u>)

(ii) those who deny any <u>need</u> for an independent account of the nonsignificance of category-mistakes, because they can be subsumed under other varieties of anomaly; (necessary falsehood: Prior, <u>1954</u>; Ewing, <u>1937</u>; semantic anomaly: Katz, <u>1963</u>).

A final note must be added to the diagrams, above. I make no attempt to consider in this thesis a theory of category-mistakes based upon a pre-determined, <u>a priori</u> partitioning of categories grounded in metaphysics or epistemology (grounded in the nature of judgement and understanding--a Kantian approach; or in the nature of things--an Aristotelian approach). I have reasoned above that it is at least <u>plausible</u> to separate questions of category-mistakeness from those of categoryallocations. This, along with considerations of expediency, must suffice as a rationale for these omissions.

Wittgenstein's doctrine of showing:

Wittgenstein's <u>Tractarian</u> critique of Russellian type theory not only denies the explanatory utility of appeals to type-differences, it denies the possibility of a <u>theory</u> of types at all. The roots of this denial lie in Wittgenstein's sustained criticisms of the ramified type theory of PM, of Russell's multiple relation theory of judgement, and of Russell's theory of meaning. These criticisms have been discussed in detail in Section C and Appendix A. Wittgenstein's denial that type theory can be meaningfully formulated, and his correlative doctrine of showing, appear very early in the genealogy of the <u>Tractarian</u> views. In June 1913 Wittgenstein wrote to Russell: (Letter R.9--quoted in Appendix A),

What I am most certain of is not however the correctness of my present way of analysis, but of the fact that all theory of types must be done away with by a theory of symbolism showing that what seem to be different kinds of things are symbolised by different kinds of symbols which cannot possibly be substituted in one another's places. (Wittgenstein's emphasis).

In terms of the discussion of Section C, Wittgenstein is applying pressure, here, to Russell's use of 'systematic ambiguity' in applying type theory to PM. In order to talk <u>about</u> the heirarchy of orders and types, Russell had had to use expressions and sentences referring to 'individuals', 'functions', 'propositions', and so on. But to refer, say, to an individual is to use some propositional function significant only over individuals-to specify "A is an individual" is to assert the propositional function ' \hat{x} is an individual'; which is true for all arguments for which it is significant. To <u>deny</u> that A is an individual, thus, is to use an expression <u>not</u> of type-0, as argument to ' \hat{x} is an individual'--which is meaningless, by the theory of types, itself. The alternative is to construe all talk of "types", "individuals", "functions" and so on, as typically ambiguous-meaning different things at different type-levels.

Wittgenstein criticised this consequence of type theory. His

criticism had two main thrusts:

(i) The typical ambiguity of type-theoretic assertions makes the theory self-stultifying;

(ii) The theory of types is, in any case, redundant. For it tries to state what, properly, can only be shown by the symbolic form of sentences.

The first criticism leads to the second through the claim that what Russell states as type-restrictions can only be <u>shown</u> by a correct symbolism; and that what is <u>shown</u> in language, cannot be represented by language.

The crux of Wittgenstein's first objection is that the statements of type theory involve formal predications--asserting formal concepts like 'individual', 'function', 'proposition', 'number', 'relation', 'type', of items. The peculiar character of formal concepts consists in their being true of everything to which they can be meaningfully applied. For example, when a name for an individual replaces the "x" in "x is an individual", the statement yielded is true no matter which name is substituted. If anything other than a name is substituted, however, -- say the sentence "This is red"--then the result is syntactically ill-formed ("This is red is an individual") and, thus, nonsignificant. Wittgenstein insisted that formal predications make no informative, factual statements. Rather than making an empirical claim, to assert that an item falls under a formal concept amounts to stipulating that only symbols with a certain syntactic role (e.g. 'naming objects') can be substituends in formal predicates. In sum, Wittgenstein argued, such symbols for what falls under a formal concept--combined in accordance with "logical syntax"--show the formal properties of items in differing types. His argument that this is all

we can do with respect to formal concepts is as follows.

If we try to <u>state</u> (make true or false predications about) what is shown by certain combinations of symbols being permissable, and others not---as type theory does---we have to use further symbols combined in prescribed ways, which, themselves, require syntactic conditions delimiting the kinds of formal concept under which what they symbolise falls. Trying to <u>describe</u> formal properties, that is, amounts to trying to get outside language in order to describe the relation between the symbols of language (names, predicates, sentences, numerals, and so on) and what they stand for. To do so, however, would require another language which itself stands in a symbolising relation to what <u>its</u> expressions stand for. This regress, Wittgenstein claims, is stultifying.

In contrast to Wittgenstein, Russell accepted this regress-argument, conceding only that such a heirarchy of languages--each describing the relations between words and things in the language below it--might be objectionable on different grounds. Commenting, thus, upon the doctrine of showing in his Introduction to the Tractatus, Russell writes:

Every language has, as Mr. Wittgenstein says, a structure concerning which, in the language, nothing can be said; but that there may be another language dealing with the first language, and itself having a new structure, and that to this heararchy of languages there may be no limit ... Such an hypothesis is very difficult and I can see objections to it ... Yet I do not see how any easier hypothesis can escape from Mr. Wittgenstein's conclusions. (Russell's Introduction, <u>1922</u>, p. xxii).

Russell's hypothesis records less a genuine alternative to the doctrine of showing than a measure of the extent to which Russell had misunderstood the doctrine. That we should consider such an hypothesis

feasible, Wittgenstein argues—in an earlier entry in <u>Notebooks</u>, <u>1914–16</u>, which considers this possibility—demonstrates only our misapprehensions about how unique the relation between symbol and what is symbolised is:

But is <u>language</u> the <u>only</u> language? Why should there not be a mode of expression through which I can talk <u>about</u> language in such a way that it can appear to me in coordination with something else.... I myself only write <u>sentences</u> down here. And why? <u>How</u> is language unique? (Notebooks, 29.5.15).

The comment is terse; but to explain what Wittgenstein found unique about language, which precluded Russell's hypothesis, reveals just how deeply the issue raised above, over whether type theory <u>can</u> describe differences in things or expressions, penetrates into philosophy.

Predications which express formal concepts are called <u>formal</u>, by Wittgenstein, not because they assert anything true of the <u>forms</u> of sentences, or any factual information that sentences ordinarily convey. Instead, <u>formal</u> predicates pertain to what is <u>essential</u> to a language-its form--in virtue of which its sentences <u>can</u> convey information. <u>Form</u> is what is <u>shown</u> by the propositions of a language (when analysed) and this cannot belong to what can be stated in the language, because it is presupposed by the language's being used to state anything:

Propositions can represent the whole of reality, but they cannot represent what they must have in common with reality in order to be able to represent it ... logical form Propositions cannot represent logical form; it is mirrored in them ... What expresses itself in language, we cannot express by means of language. Propositions show the logical form of reality. (Tractatus, 4.12-4.121)

These remarks are the kernel of the <u>Tractarian</u> alternative to type theory-the doctrine of showing--and it is important to observe that Wittgenstein took this doctrine to be the "main contention" of the book. Of it he wrote to Russell that:

You haven't really got hold of my main contention ... The main point is the theory of what can be expressed (gesagt) .. by language (and which comes to the same thing what can be thought) and what cannot be expressed .. but only shown (gezeigt); what I believe is the cardinal problem of philosophy. (Letter R.18; 19.8.19)

and endorsed this view subsequently in the preface to the Tractatus:

The whole sense of the book might be summed up:... what can be said at all can be said clearly, and what we cannot talk about we must pass over in silence. Thus the aim of the book is to set a limit to thought ... It will only be in language that the limit can be set; and what lies on the other side of the limit will simply be nonsense. (<u>Tractatus</u>, Preface, p. 3)

What a proposition <u>says</u>, in contrast to what it <u>shows</u>, is revealed in its truth-functional analysis into a combination of elementary propositions each of which is a configuration of names "depicting" a state of affairs--a possible fact (4.22-7). The notion that, fully analysed, the relation between symbol and what is symbolised, or between a proposition and what it expresses, is one of <u>picturing</u> is the dominant image of the <u>Tractatus</u> (in just the sense in which the generic image of the <u>game</u> dominates Wittgenstein's later theory of meaning in <u>Philosophical Investigations</u>, (1953)). The detail of the Picture Theory I cannot go into here; but what must be explained is how the requirement that the relation between language and the world (and thought and the world) is <u>pictorial</u> forces upon us the consequence of the doctrine of showing that formal type-differences cannot be described, but must be shown.

To describe the relation between the propositions of language and the

facts which constitute the world as pictorial, Wittgenstein appeals to a very general concept of 'picture', with respect to which he regards, for example, gramaphone records and musical scores as "pictures" of the music they reproduce. Similarly, phonetic spelling is a 'picture' of spoken language (4.011); thought pictures facts (5.542). The central feature of this generic relation of picturing is one of a structural isomorphism between what depicts and what is depicted. Not every isomorphism is a picture in this sense, though. Trivially, every fact is isomorphic with itself; but facts do not depict themselves. For A to depict B, A must be both like B and unlike B. A is like B in respect of what A and B have in common--which Wittgenstein calls "pictorial (iconic) form" (Form der Abbildung: 2.16). What distinguishes A from B is what makes A a picture of B, rather than a reduplication of B--which Wittgenstein calls "representational (symbolic) form" (Form der Darstellung: 2.173-4). The notion of "form" enters into both these concepts but means different things in each; and it is essential to the doctrine of showing to appreciate this difference.²

A picture represents what it depicts from a position outside it (2.173) and the position <u>from</u> which it depicts something--its standpoint-is its representational form. A cassette-tape and a musical score may be <u>of</u> the same piece of music--they both depict the music, but they differ in 'standpoint' adopted; hence, they differ in the form of representation involved. To 'understand' what the tape depicts we have to correlate the magnetically sensitized portions of the tape with musical sounds, with the help of a suitable playback-device. The complex electronic and auditory

relationships between tape and sound make up the standpoint <u>from</u> which we can view the tape as a picture representing that piece of music. A musical score, similarly, correlates with the same piece of music—perhaps, even numerically the same performance: imagine monitoring the taping of a concert whilst reading the score—by means of a complex notational and experiential net correlating marks on paper and sounds. The <u>possibility</u> that such a structural net can correlate score with music is the <u>representational</u> <u>form</u> which the score possesses (in our reading of it), in virtue of which it can depict the performance.

Representational forms (hereafter "R-forms") can, thus, differ widely. A representational painting employs a spatial form to depict; whereas a symbolist painting, say, often employs forms--mediated perhaps by archetypes--having a highly abstract relation to what is depicted (which need not be spatial). On the other hand, <u>logical form</u> (pictorial form is <u>logical</u> in so far as it pertains to propositional meaning, i.e. to states of affairs) is what pictures have in common with what they depict. This must be different from R-form; for, in so far as the tape and score differ widely and yet are "pictures" of the same music, their pictorial form is the same. Roughly stated, that a picture has pictorial form is required for it to have <u>significance</u>, for us to be able to interpret it as meaning something (representing a state of affairs). In contrast, that a picture has R-form is required for it to be a <u>correct</u> or <u>incorrect</u> representation (true or false) of what it depicts.

"Pictorial form" (hereafter: "P-form") is defined in 2.15 as the possibility of structural identity between elements related in the picture

and the fact depicted (<u>qua</u> configuration of simple objects). Thus to adopt the familiar example of Wittgensteinian exegesis, the fact that an object named "a" is to the left of an object named "b" <u>could</u> be depicted by a sign " $\langle a,b \rangle$ " (with "a" to the left of "b"). The possibility that this structural isomorphism holds is what makes " $\langle a,b \rangle$ " a possible picture of a's being to the left of b. P-form is the possibility of this structural isomorphism (2.151).

"Form", in general, is defined in the Tractatus as the "possibility of structure" (2.033), so R-form must also be a possible structural relationship. As described above, though, R-form cannot consist merely in the possibility of isomorphism between elements of the picture and constituents of fact depicted. For, R-form accounts for how A differs from B in order for A to be a picture of B. R-form, then, can vary according to the different ways pictures can represent states of affairs (by means of lines, colours, spatial arrangement, temporal arrangement, pitch of sounds, strength of electric signal,....). The standpoint from which a picture represents a possible arrangement of objects is arbitrary. That is, by a convention I stipulate that $\langle a, b \rangle$, read left-to-right, stands for the state of affairs: a-to-the-left-of-b. Another way of representing ("Lezeichnungsweise") might be "{A,b}"--where the order is now unimportant-but the upper-case "A" symbolises that a is to the left. Every such method of representation can depict the state of affairs concerned, because conventions can be established to represent a,b that way. Thus, the possibility that some such representation will depict a-to-the-left-of-b is what all pictures of that state of affairs have in common in virtue of

which they are <u>pictures</u> of a-to-the-left-of=b. The possibility of structural depiction is R-form.

Applied to language, the distinction between P-form and R-form results in a demarcation between what it is possible to depict, and what has to be the case for propositions (qua pictures) to depict anything. Logical form is the P-form required for sentences (in use) to depict facts in the world through their being structurally isomorphic with states of affairs. Not every sentence is literally isomorphic with a possible fact. For, ordinary language sentences disguise the forms of the facts they depict (4.002). It is only by analysis of what sentences express (by means of definitions and truth-functions) that what is common to sentence and fact is revealed. Supposing such an analysis completed, the logical form, Wittgenstein claims, is what is common to sentence and fact (2.18). What this claim amounts to is that every sentence can be so analysed that its elements, revealed by analysis, are capable of some combination, one with another, in a pattern which is shared by the constituents of a possible fact depicted. Which combinations result in a pattern shared by a possible fact depends upon the R-form--the sense of a sentence is the method by which an R-form is projected (possibly by conventions, stipulations or ostensions) onto a state of affairs, by correlating names with objects (3.5, 3.11-12). The analysed sentence is projected, as it were, on to a state of affairs via its sense; i.e. through the medium of this name standing for that object, and that name for another object, and of this name's standing to that name in a certain way representing these objects being so related:

Instead of "this proposition has such and such a sense" we can simply say "this proposition represents such and such a situation" ... One name stands for one thing another for another, and they are combined with one another. In this way the whole group--like a <u>tableau</u> <u>vivant--presents</u> a state of affairs. (<u>Tractatus</u>, <u>4.031-4.0311</u>).

I hope this suffices to explain the distinction between P-form and R-form, and to connect it to the notions of 'sense' and 'logical form'. My account certainly does not exhaust the Picture Theory--and a great deal more could be said about the manner in which only <u>analysed</u> sentences are to depict, directly; and what kind of analysis this is. I have now, though, to show why P-form cannot be described in language.

In brief, Wittgenstein argues: if our description of the world ("the totality of facts") could be given so that what is common to picture and fact (P-form) is not mediated by representations, then facts could be described without pre-assigning any particular representational meaning to the terms of our description. Such a picture would correlate <u>essentially</u> with facts; i.e. provided we could understand it--provided it was intelligible--it would automatically be a true description of the facts. (Recall that a <u>formal</u> concept is whatever is automatically true of everything to which it can significantly apply). Such a description, though, is impossible---there is nothing distinguishing description from descriptum, picture from depicted. In other words, a description which shares all essential features of the facts described, in all respects; but which is non-representational, is simply a reduplication of those facts. The description neither agrees with reality nor disagrees with it (it is neither true nor false), for it is the same as the reality. Every logical picture, thus, has to be

representational.

Some evidence from the <u>Notebooks</u>³ suggests that Wittgenstein's object, here, is to argue against the Russellian theory of propositional meaning (Russell, <u>1910</u>, <u>1912</u>). Russell construed analysis as revealing that sentences are meaningful only in relation to propositions with all of whose constituents we are acquainted. Russell had reasoned: in order for descriptive sentences to communicate true or false factual information, the meanings of expressions must ultimately reduce to <u>bona fide</u> constituents of the facts described (the "perceptual complexes", described in Appendix A, or "sense-data"). Wittgenstein objects that an ideal of analysis for which sentence-meanings are constituted by items which comprise the facts which obtain if the sentence is to be true cannot be an ideal for <u>linguistic</u> meaning at all. It is essential, he claims, to a sentence's being used to communicate factual information that its <u>sense</u> agrees or disagrees with the facts comprising the world:

A picture depicts reality by representing a possibility of existence and non-existence of states of affairs ... a picture agrees with reality or fails to agree; it is correct or incorrect; true or false ... What a picture represents, it represents independently of its truth or falsity, by means of its pictorial form. What a picture represents is its sense ... In order to tell whether a picture is true or false we must compare it with reality. It is impossible to tell from the picture alone whether it is true or false. There are no pictures that are true a priori ... (Tractatus: 2.201, 2.21-2.221, 2.223-5).

It is a short step, now, from this objection to the Russellian conception of meaning, to the denial that <u>formal</u> predications like "<u>a</u> is an individual", " \emptyset is a function", "R is a type", can be pictures of facts, i.e. can have sense. Such propositions cannot tell us anything about the world

because, if true, they would be true <u>a priori</u> (their significance guarantees their truth, so no experience is necessary to verify them). If there is no possibility of their being a <u>false</u> (but meaningful) depiction of facts, they are not depictions at all. They lack sense-they do not represent "a possible situation in logical space" (2.202), "a possibility of existence and non-existence of states of affairs" (2.201). For example, "<u>a</u> is an individual" lacks sense because its denial "<u>a</u> is not an individual", if true, is meaningless-either "<u>a</u>" functions as a nameor it doesn't; and if it doesn't, "<u>a</u> is not an individual" is ill-formed (argument of the wrong type). The reasoning which preceded this account of the requirements for sense now enforces the conclusion: that the statements of type-theory are selfstultifying--they try to state (depict) what can only be shown.

Wittgenstein also claims (criticism ii, p.254, above) that type theory is redundant. What type theory purports to set as restrictions upon significant predications are features which must already be shown in the logical (P-) form of sentences. Any language, that is, which fulfills the conditions for expressing thoughts through propositional signs with sense must already embody, however disguised, syntactic features prohibiting type violations. So, a theory of types is superfluous.

Wittgenstein had come to this conclusion early in the genesis of the doctrine of showing. In the Notes Dictated to Moore, (1914), he commented:

It is impossible to <u>say</u> what these (logical, formal) properties are, because in order to do so you would need a language which hadn't got the properties in question and it is impossible ... to construct (an) illogical language. An illogical language would be one in which e.g. you could put an <u>event</u> into a hole. (Notebooks, Appdx. B, p. 107)

A language in which it would make sense to say that an event could be put into a hole would breach category-distinctions. Typically, then, the "theory of symbolism" for a possible language should <u>symbolise</u> how expressions for items of different types cannot replace one another significantly, in propositions. Wittgenstein continues with an illustration of how a theory of symbolism is to do this:

This same distinction (showing/saying) ... explains the difficulty that is felt about types--e.g. as to the difference between things, facts, properties, relations. That M is a thing can't be said; it is nonsense; but something is shown by the symbol "M". In the same way, that a proposition is a subject-predicate proposition can't be said; but it is shown by the symbol. (Notebooks, Appdx. B, p. 108)

How, then, does a theory of symbolism (syntax) show what type theory tries to say? We have first to attend to a distinction Wittgenstein drew between signs and symbols (Tractatus: 3.32-3.3421).

A word or sentence considered in terms of its perceptible qualities-spoken or written--is a <u>sign</u>; a sentence, construed thus, is a "propositional sign". Considered in <u>use</u> as expressing something meaningful--together with the conditions for its being meaningful--an expression is a <u>symbol</u>: (3.326: "In order to recognise a symbol by its sign we must observe how it is used with a sense"). And a sign does not have a sense unless "taken together with its logico-syntactical employment" (3.327). Restrictions upon how a sign can be meaningfully used--upon how it <u>symbolises</u>--must appear, therefore, in syntax:

In logical syntax the meaning of a sign should never play a role. It must be possible to establish logical syntax without mentioning the 'meaning' (<u>Bedeutung</u> = significatum) of a sign. (<u>Tractatus</u>: 3.33)

Russell (and others; e.g. Carnap, 1934) construed this remark, together

with 3.325 as Wittgenstein's advocacy of an "Ideal Language view". 3.325 mentions avoiding philosophical errors by making use of a sign-language in which confusion is avoided by not using "the same sign for different symbols". Such a "sign-language", it may be supposed, could be an "ideal language" in which philosophical 'pseudo-problems' do not arise--because the vagaries of ordinary language have been eliminated. I criticised this interpretation of Wittgenstein in the Introduction. It may suffice to note, here, that, not only does Wittgenstein diagnose <u>why</u> we might feel inclined to such a view:

Now, too, we understand our feeling that once we have a sign-language in which everything is allright, we already have a correct logical point of view. (Tractatus: 4.1213),

but he also explicitly denies the need for such an 'ideal language':

(4.011): At first sight a proposition--e.g. one set out on the printed page--does not seem to be a picture of the reality with which it is concerned. But no more does musical notation seem to be a picture of music ... And yet these sign-languages prove to be pictures even in the ordinary sense..

(5.5563): In fact, all the propositions of our everyday language, just as they stand, are in perfect logical order.

So, a "theory of symbolism" is not an ideal language. How, then, do <u>symbols</u> show what type theory tries to say?

Consider the sentence (S): "Plato is different in type from Justice". This, according to Wittgenstein, is a piece of nonsense--if only because, if it asserts truly a difference in type between Plato and Justice, the relation "x differs in type from y" is <u>heterotypical</u> (whence, (S) is ambiguous). Alternatively, if (S) is univocal, then (S) is restricted to a type (whence what (S) asserts is self-refuting). What (S) tries to say may be brought

out in the Picture Theory as follows. In (T): "Plato is tall", "Plato" occurs as a proper-name, but "tall" does not. What symbolises the state of affairs that Plato is tall is not some relation between what "Plato" names and what "tall" names; but that the subject-predicate form " $\phi(x)$ " of (T) correlates with the form of the possible fact that Plato is tall-the form of an object's having a property. A propositional-sign is itself a fact (configuration of objects (names)) and the identity of P-form between sign and fact, which is effected by using some method of representation (some R-form) which correlates name with object, predicate with property, is how the sign acquires its sense (how it becomes a symbol in use). For it to be possible for (T), qua fact, to do this, however, there must already be the possibility of facts of this form being represented by this method (Bezeichnungsweise). This possibility--R-form--is what is essential to a propositional-sign, without which it would not have sense (would not depict: 3.34). If subject-predicate sentence and state of affairs depicted are to share P-form, it is essential to the sentence that it can represent an object's having a property (whether truly or falsely). Thus, it is essential to "Plato's" occurring as a name in that proposition (and "only in the nexus of a proposition does a name denote (have Bedeutung)" at all: 3.3), that it can function as a symbol for an individual object. "What is essential in a symbol is what all symbols that can serve the same purpose have in common" (3.341--Wittgenstein should probably have said "what all signs that serve the same purpose", here). Thus, it is R-form which guarantees that whatever arrangement of signs depicts a fact, something in that way of symbolising will pick out an

individual and predicate a property of it. The reason why the wrong <u>symbol</u> cannot be substituted for the argument-place in " $\emptyset(x)$ " is, thus, <u>not</u> that the (type-) <u>rules</u> of language prohibit it; but that any <u>wrong</u> arrangement of symbols would represent an impossible fact (thus, would not represent at all). Such an impossible fact might be, say, that some property has some individual--that running Plato's--or, as Wittgenstein put it, that an event can be put in a hole.

In this way the <u>Tractarian</u> critique of type theory can be seen as extending the accounts of "pictorial-" and "representational-form" to the claim that, for " $\phi(x)$ " to be a possible way of symbolising the state of affairs that an object has a property, then something must function as a name, and something as a predicate. In this way, Wittgenstein dismisses Russell's paradox:

No proposition can make a statement about itself, because a propositional-sign cannot be contained in itself (that is the whole of the 'theory of types') ... The reason why a function cannot be its own argument is that the sign for a function already contains the prototype of its argument, and it cannot contain itself. (Tractatus: 3.332-3).

The "prototype" of "proto-picture" (<u>Urbild</u>) which a function contains is the symbol which is appropriate to the <u>type</u> of its argument (in propositions of that form). If "x" ranges over individual objects in " $\emptyset(x)$ " then no <u>proposition</u>, say "not-F(a)", can be substituted, since no fact can be represented by the form " $\emptyset(not-\emptyset())$ " (c.f. 3.333+). No propositionalsign, <u>qua</u> fact, has this <u>incomplete</u> form (R-form), so no state of affairs is depicted by "F(not-F(a))". Thus, "F(not-F(a))" lacks sense. One premise of the self-referential paradox is rendered senseless, and the paradox disappears. Of course, I violate the doctrine of showing throughout the above by talking <u>about</u> the R-form and P-form of signs, as if they were matters of fact--whereas they are <u>shown</u> by a particular way of symbolising being a possible method of representation. In the end, my discursive statements are as senseless (<u>Sinnlöss</u>) as the assertions of the <u>Tractatus</u>, itself (6.54). They try to state what can only be shown in the essential vacuity of the symbol " $\emptyset(not-\emptyset())$ ".

This completes the exposition of the <u>Tractarian</u> critique of type theory as a theory of significant predication. I have sought to develop Wittgenstein's criticisms in a way which indicates clearly the relationship between the doctrine of showing and the dominant imagery of the Picture Theory, expressed in the account of "pictorial (iconic) form" and representational (symbolic) form". In turning, now to the evaluation of Wittgenstein's criticisms, I have first to record several important reservations.

A Wittgensteinian response to "showing" and "saying":

A major problem in criticising the <u>Tractatus</u> is that, in general, it constitutes a holistic, systematic body of philosophical theory. One does little justice to it to attack its doctrincs piecemeal. On the other hand, to consider the text as a whole and yet provide non-trivial criticisms-which are not just alternative philosophical theses--is a task of too wide a scope for this thesis. Fortunately, the ablest critic of Wittgenstein's <u>Tractatus</u>, in subsequent philosophy, has been Wittgenstein, himself--much of his later work on meaning, reference and truth can plausibly be interpreted

as systematic dismantling of his earlier views (and much, also, as a reasoned defence of them from a different standpoint). Thus, some criticisms can be gleaned from attention to Wittgenstein's post-Tractarian notes and lectures.⁴

The consequences of the doctrine of showing are certainly startling: that the <u>Tractarian</u> enterprise of setting the limits to discursive thought, through the requirements for significance, should result in severe constraints upon what can be meaningfully talked about, is one consequence which might suggest to some the fruitlessness of construing the meaningrelation as fundamentally <u>pictorial</u>. Abandoning the Picture Theory, however, must entail abandonning the <u>Tractatus</u>, itself--for, as I have shown, the "main contention" of the book (what can be <u>said versus</u> what can only be <u>shown</u>) depends upon the analysis of the propositional-sign, with sense, as a logical picture isomorphic with the form of facts.

What <u>can</u> be argued more briefly, perhaps, is how the doctrine of showing, and the critique of type theory, depend upon the <u>atomistic</u> metaphysics underlying the Picture Theory. We can suggest, then, that, just as an Aristotelian account of category-mistakes should not have to <u>depend</u> upon a metaphysics of essential and accidental attribution; so there should be no presumption that an account of type-mistakes demands an atomistic metaphysics. This is no criticism of the <u>Tractatus</u>; for, it may turn out that <u>any</u> theory of category-mismatch must rest upon some committment to an ontology of irreducibly different kinds of item. I shall argue through Part II that in this respect an atomistic basis for logic is no worse than another ontology. I shall also conclude the thesis with a more general discussion of just this question: must an account of how sentences go wrong in respect of category and type commit its proponent to substantive ontological theses? In concluding, I shall answer this question in the affirmative--but, for the moment, I do not wish the question to be begged in uncritical acceptance of the <u>Tractarian</u> doctrine of showing.

'Linguistic meaning as pictorial' is a compelling image of propositional significance; isomorphism between sentence and fact yields a theory of representation on the model of projective geometry. Propositionalsigns project onto states of affairs through our conventions, and our representational standpoint: objects are named; properties and relations depicted; and logical relationships shown in the truth-functional agreements and disagreements of non-elementary propositions with their elementary components. There are indefinitely many ways in which states of affairs may be represented, without any particular sets of objects, properties and relations being those without which our propositional signs could not be used with sense. The atomism of the Tractatus, that is, makes committment to neither physicalist nor phenomenalist atoms; it is a logical atomism.² This means that simple atoms, entering into elementary, mutually independent states of affairs are a prerequisite for sentences to be meaningful, for logical truths to be tautologous. The demand of logic, thus, is only that names standing for simple objects in atomic states of affairs be possible; so that tautologies can be shown by truth-functional combinations of sentences whose sense is not mediated by definitions of constituent expressions or analytic relations between one elementary configuration of

names and another.

Elementary propositions, therefore, must be logically independent (4.2, 4.21)—for suppose they were not: if one elementary sentence "p" contradicted another "q" (<u>contra</u>: 6.3751). Then, since q would be false whenever p was true and vice-versa, q would be truth-functionally equivalent to not-p. This would mean that p and not-p were (different) elementary sentences. A difference between elementary sentences demands a difference in the elementary states of affairs depicted. So, p and not-p depict different states of affairs—which can only be accountable to the presence of 'not'. Thus, the addition of 'not' changes the sense of p and not-notp has to differ in sense from not-p and, hence, from p, also. But notnot-p follows from p, as does not-not-not-not-p, and so on. So, from the existence of <u>one</u> state of affairs, the existence of infinitely many (different) states of affairs follows. This, Wittgenstein suggests (5.43, 5.44) is scarcely credible. So, elementary sentences must be logically independent.

Mutual independence of atomic states of affairs (also, elementary sentences) is one principle of Wittgenstein's atomism. Conjoined to the demand of the Picture Theory that analysis terminate in names of simple objects (3.23-4), these principles necessitate that we can never <u>say</u> that this or that state of affairs or sentence is elementary; nor can examples of simple names be given--except within sentences in use. This is one implausible consequence of the <u>Tractarian</u> theory of meaning which Wittgenstein came to criticise most severely as he dismantled the atomistic metaphysics of the Tractatus--and, with it, the Picture Theory--in his

later writings.⁶ We can summarise how this implausible consequence came to be re-examined as follows:

Non-elementary propositions are reduced, by truth-functional analysis and replacement of defined terms by their definitional equivalents, to combinations of elementary propositions: "The sense of a truth-function of p is a function of the sense of p" (5.2341). Wittgenstein does not give examples of this procedure of analysis in the Tractatus, nor does he give examples of the simple names at which analysis terminates. He seems to regard the determination of any particular set of objects as simple, or any specific propositions as elementary as an a posteriori matter, relative to the subject matter to which logic is applied (5.55, 5.5542, 5.5571). In the 5.55's, that is, it is claimed that what elementary propositions there are is determined empirically by the application of a logically correct symbolism to an empirical subject-matter. Thus, he speaks of the relation between thoughts, beliefs and judgements and their subject matter as analogous to the representational relation between sentences and states of affairs--but regards the question of which constituents of thoughts, etc., are directly representational (and what they are) as a matter for psychology as an empirical science. Logic establishes only the prerequisites for sense--it does not establish a priori the particular senses of elementary propositions; for these are relative to the application of logic.

It follows, if this is a correct interpretation of the 5.55's of the <u>Tractatus</u>, that, to the extent that any language possesses the essential logical features for communicating sense, then our ordinary descriptive discourse possesses these features (5.5563). Clearly, though, with respect

to the question of what the <u>forms</u> of elementary sentences are ordinary language disguises the answer considerably. For example, many propositions involving diverse subject matters share the subject-predicate form--even though, in reflecting upon their significance, or upon what has to obtain for them to be true or false, it becomes clear that this overt similarity disguises radical differences. Consider, say, the three subject-predicate propositions:

- (a) This (colour) is bright.
- (b) Justice is blind.
- (c) This (sound is loud.

When logic is applied to ordinary ascriptions, like these, it might seem that simple predications of colours, brightness, intensity of hues; or ascriptions of pitch, volume and duration to sounds, etc., are the most likely candidates for irreducibly elementary sentences. This possibility, however, is explicitly ruled out in the <u>Tractatus</u>. 6.3751 remarks that, since assigning one colour to an item rules out assigning a different colour to the same item at the same time, then colour ascriptions are not logically independent, thus, non-elementary. To this we can add that (a), (c) to be significant cannot be elementary—for the demonstratives ("this") have to be understood, in context, as referring to colours and sounds, respectively. For it is literally nonsignificant to assert loudness of, say, smells or shapes. So the sense of (a) and (c) would depend upon whether other propositions (that <u>this</u> is a colour, that <u>this</u> is a sound) are true—which precludes their being elementary.

Similarly, though (b) shares the overt form of (a), (c), it becomes clear that it differs in the manner in which it can have sense. Ordinarily,

to assert blindness of an item presupposes that the item is of a kind which possesses (or <u>could</u> possess) organs of sight. Thus, the significance of (b), in some context in which an abstract item like justice is under discussion, demands our interpreting "is blind" as applying only <u>non-literally</u> to justice (or, upon our identifying the abstract referent of "Justice" with its familiar personification). To assert non-literally of justice that it is blind—in order to convey the necessary impartiality and indifference to rank of judicial processes—may, thus, not be to make an assertion of the subject-predicate form, at all. It is at least conceivable, that is, that the analysis of (b)'s non-literal significance may reveal it to be expressing far more complex qualities and, perhaps, relational properties of justice. Should we <u>still</u> regard (b) as subject-predicate in form—in just the sense in which (a), (c) have this form—if its <u>literal</u> nonsignificance demands a metaphorical paraphrase?

If the analysis of (b) does not preserve its sense of ascribing a property to an object, it is equally unclear whether the analysis of (a) and (c) will preserve this characteristic. For, any assertion of a particular hue that it is bright, or of a particular sound that it is loud, is understood as expressing the <u>degree</u> to which an item has a quality, rather than a simple quality-ascription. Asserting brightness of, say, a red colour, amounts to asserting that, with respect to most red items, this one has a degree of brightness greater than those items. Similarly, asserting loudness of a sound amounts to stating that, of sounds similar in pitch and duration, this sound is louder than most. Should we say, then, that (a) and (c), themselves, are more properly understood as <u>relational</u> in form?
From these examples, it quickly becomes clear how complex the Tractarian analyses of propositions to reveal their truth-functional structure will be. Subsequent to the Tractatus (in Wittgenstein, 1929 and 1967), Wittgenstein came to recognise that such procedures of analysis could not, as he had supposed, reveal the elementary propositions and simple names. He reasoned, to begin with, that, since simple colour ascriptions (to points in the visual field) are non-elementary, they must be capable of further analysis. In particular, ascriptions of brightness, intensity, and so on--depending, as they did, upon comparatives of degree-would also have to be further analysable. The sense of "This is loud", for example, would have to depend upon a whole range of circumstances: that this is a sound, with a certain pitch, originating some distance away, having a degree of volume greater than most (or less), and so on. Suppose we were to analyse qualities which admit of gradation, like "loudness", as combinations of simple qualities, together with a description of what qualitative differences those qualities made to the sense of "This is loud". Such an analysis, we could say, would depict a more fundamental state of affairs than the original assertion "This is loud"--in just the sense as that in which "This bag contains three apples, six oranges and five bananas" depicts a state of affairs more perspicuously than the unanalysed "This bag contains fruit".

Wittgenstein soon recognised (<u>1929</u>, p. 35) that such an attempt to analyse graduated qualities was not feasible. For, suppose we say of this sound <u>s</u> that it is loud. This means that it is louder than most similar sounds. We might suggest, then, that it be analysed into a combination of

propositions each assigning so many units of 'loudness' (volume) to similar sounds. One such 'atomic' proposition might assert of some sound a, that it has two units of volume; of another b that it has one unit "lV(b)"; of s, that it has twice as many units as $a - - 2 \times 2V(s)$ --or that s is louder than both a and b--"nV's > mV'a + m*V'b". How should we analyse "2V(a)" which asserts two units of volume of a? If we suppose that 'degrees of volume' are commensurable, we should regard "2V(a)" as analysable into the assertion that <u>a</u> has 2 x 1 unit of volume; i.e. "2 x 1V(a) = df 1V(a) + 1V(a)". This appears to make "2V(a)" truth-functionally analysable into "(1V(a) & 1V(a))" But, this asserts no more than "IV(a)". So we have to regard each ascription of a unit of volume as distinguishable--analysing "2V(a)" into " $(1V(a) \& 1V^{*}(a))$ ". But then, which of the two simple qualities "1V(x)" or "1V*(x)" should be ascribed to b, in order to make it half as loud as a? This question, Wittgenstein recognised, is obviously absurd; and he gave up the attempt to analyse graduated ascriptions truth-functionally (1929, p. 35---my remarks reconstructed a much briefer argument from this article ("Some Remarks on Logical Form")).

As a consequence of the failure of these kinds of analyses, Wittgenstein gave up, in <u>1929</u>, the view that elementary sentences had to be logically independent. This creates grave difficulties for the atomism of the <u>Tractatus</u>; since it admits <u>logical</u> relationships between propositions which will not be revealed in their truth-functional analyses. For example, suppose p is the (hypothetically) elementary sentence "F(a)" and q is "G(a)" and that, contrary to the atomistic principle, being F is incompatible with being G. Then, "p & q" is a truth-functional combination of those elementary sentences which is <u>impossible</u>, because of the incompatibility of p with q. The truth-table for "p & q", thus--expressing the agreements and disagreements with atomic states of affairs which is the <u>sense</u> of "p & q"--would have "False" uniformly in its final column. That is, for these particular elementary sentences, "p & q" would be a contradiction. Now, however, "not-(p & q)" becomes a <u>tautology</u> which is not revealed in the truth-functional analyses of propositions. (Obviously, we <u>could</u> so analyse "not-(F(a) & G(a))" so that it became an explicit tautology; but, then, F and G would not be elementary properties--contrary to our supposition).

Having abandoned the independence of elementary propositions, Wittgenstein soon saw that he had to give up the atomistic demand that analysis reveal simple objects combined in atomic states of affairs--and the main tenets of the doctrine of showing slowly fall away. In conversations with F. Waismann (in 1929-1930: now published as Wittgenstein, 1967, edited B. McGuiness), Wittgenstein wrestles with the idea (pp. 63-79), for example, that the language of colour ascriptions is not so much to be analysed into discrete, independent predications (of hue, brightness, intensity, and so on); but, roughly speaking, as a relational system of propositions which mutually support one another. Each proposition of such a system, he supposes, gets its sense, not so much by depicting directly an atomic state of affairs, but from its role in the system as a whole, and from the manner in which we apply the logical relationships between colourascriptions, within the system, to objects, percepts, and so on. This, clearly, is am embryonic form of Wittgenstein's later account of the sense of sentences as embedded in their use in 'language-games'. As a particular

analysis of colour-ascriptions and their significance, the tentative remarks of Wittgenstein have been given a fuller articulation in Wilson, <u>1971</u> (especially, Sections 7 and 8--though there is no explicit correlation of Wilson's account with Wittgenstein's of <u>1929</u>). The similarities are evident enough if we compare the following remarks:

I once wrote 'A proposition is like a ruler laid against reality'... I would now rather say: a system of propositions is laid against reality like a ruler For instance, when I say: such and such a point in the visual field is blue, I know not only that, but also that the point is not green, not red, not yellow and so on. I have laid the whole colour-scale alongside simultaneously... That is also the reason why a point cannot have different colours at the same time ... if I lay a' proposition-system against reality, it is already affirmed ... that only one state of affairs can obtain, and not more than one. (Wittgenstein, 1929, pp. 63-4)

Whatever word we use for the quality we settle on as the significance of ... 'green', our use of that word requires that we have factual knowledge about the quality it signifies How does a colour manage to get itself hooked onto by a colour-word? The answer is that the colour green, for example, gets itself hooked onto by the word 'green' in virtue of the fact that its extension is such that it satisfies more of the sentences of the corpus (of sentences containing 'green') ... than does any other colour we might say that the identity of a colour-quality depends loosely on the facts of which it is a constituent... (Wilson, <u>1971</u>, pp. 151-2--my insert).

What is important, here, though, is not whether this is a correct analysis of how colour-ascriptions are significant (though I believe it is); but how much of the doctrine of showing, and the atomism of the <u>Tractatus</u>, has to be given up. I have argued that much of the demand for discrete, independent states of affairs as the senses of elementary sentences is lost, if logical relationships between elementary sentences are admitted. Thus, the forms of elementary sentences and states of affairs cannot simply be <u>shown</u> by the procedures of truth-functional analysis. The threat of the negative consequence of the doctrine of showing--that type-distinctions cannot be meaningfully talked about--is thereby removed by Wittgenstein's own dismantling of the atomism upon which it depended.

Category-mistakes as "ungrammatical" sentences:

The threat of the doctrine of showing--to make the investigation of type theory as contributing to significance self-stultifying and, in any case, redundant -- was only one of the two general approaches to categorymistakes which threaten the enterprise of this thesis, (above, p.252). The second such threat stems from arguments which suggest, also, that such an investigation is redundant. Redundancy in these cases, though, derives not from claims about the essential features a language must possess; but from the claim that explaining category-mistakes as nonsignificant or meaningless, through violation of type, is misconceived in principle. The claim in these cases, is that those sentences which I am representing, preanalytically, as nonsignificant are, in fact, non-distinctive instances of other generic kinds of anomaly. I have assumed, for most of this section, that "category mistakes" or "type-violations" are distinctive in being sentences which are grammatically impeccable (not violations of syntax) and yet are nonsignificant in most normal contexts of assertion. If this assumption is restricted to assertions of indicative sentences in context, the claim is being made that such sentences are grammatical, but fail to yield true or false statements. The second threat to this assumption can, thus, be made along one of two possible fronts:

1) that every so-called nonsignificant sentence is in fact ungrammatical (breach of linguistic rule);

(2) that every so-called nonsignificant sentence of the kind illustrated in the Introduction (p. 1) is not only grammatical, but significant. The apparent abnormality of the examples is to be explained only in terms of their vacuous (analytic) falsity or necessary falsity.

In recent years, there have been notable advocates in support of both claims 1) and 2). Claim 1) has found support both among analytic philosophers of language (A. Pap, <u>1957</u>; M. Shorter, <u>1956</u>), and among current writers on philosophical linguistics (Katz and Fodor, <u>1964</u>; Chomsky, <u>1965</u>). Advocacy of claim 2) has been taken up by Quine (<u>1960</u>, p. 229):

The forms concerned (Category-mistakes) would remain still quite under control if admitted, rather like selfcontradictions, as false (and false by meaning, if one likes)...

Other arguments in support of claim 2) have been adduced by Prior (1954), Ewing (1937) and Drange (1966).

Claim 1), we can say, appears to be more sympathetic to the assumption I have made than is claim 2). For, claim 1) recognises the <u>distinctive</u> anomalousness of the examples I have considered; but seeks to re-classify them as "ungrammatical" (and, in so doing, often appeals to a much revised notion of 'grammar') Claim 2), however, stands in direct conflict with my assumption that category-mistakes are grammatical, but nonsignificant. For, claim 2) declares such mistakes non-distinctive.

Consider claim 1): I shall discuss, first, in general terms, the approach to category-mistakes it embodies. Later, <u>after</u> the discussion of claim 2), I shall particularise claim 1) to a specific attempt (by Katz and Fodor, 1964; and Katz, alone, <u>1967</u>, <u>1972</u>) to explain category-mistakes as breaches of linguistic rule. I criticise this account in detail towards the end of Section D.

It is quite clear that the instances of category-mistakes I have been considering--several of which I repeat, below--are not 'ungrammatical' in any sense which traditional idiomatic or taxonomic grammars of languages (descriptions of the "parts of speech") have given to the term 'grammar'.⁷ Claim 1), then, cannot be that such absurdities are attributable to using the wrong parts of speech (Verb for Noum, or whatever)--for such examples differ in kind from word-salad strings (see: Intro., p. 10). The claim, then, must draw upon a more comprehensive notion of 'grammar'. Just such a revision to the notion of grammar is available from the work of contemporary transformational, generative linguists like Katz, Fodor, Postal and Chomsky. I attend to their separate accounts, below. For the moment, though, we can reflect upon the grounds for and against revising the notion of 'grammar' to incorporate category-mistakes as 'ungrammatical'.

Philosophers of language who have advocated claim 1) have usually derived some support from the observation that, since structural, syntactic features of sentences manifestly play a large part in determining meaning, it would be highly artificial to separate matters of <u>syntax</u> (structural properties of sentences) from matters of semantics (the interpretation of sentences), and to confine "grammatical" to the former. Indeed, an appeal to <u>syntax</u> to classify violations of type is not without precedent. As was noted throughout Section C, there is frequent interplay in Russell's descriptions of type theory between distinctions based on <u>syntax</u> (the order of a proposition is due to occurrences of bound variables)

and those grounded in semantics (the 'ranges of significance' of functions). Moreover, most post-Russellian treatments of type theory within mathematical logic (with extensional interpretations) have firmly embedded the theory in the formation-rules belonging to the syntax of the formalism.⁸ Yet, our concern, here, is not with wholly extensional formal systems; but with the philosophical appeal to 'types' in evaluating significance.

Paul Ziff has offered general grounds for revising the notion of "ungrammaticality" to incorporate category-mistakes:

Some seem to think that, since they weren't taught at school not to say "He had a green thought", there's nothing ungrammatical about the sentence. The sentence is grammatical according to the grammar they learned at school, so the sentence is grammatical... But is the grammar they learned at school a correct grammar of English? (Ziff, "About Ungrammaticalness", <u>Mind</u>, LXXIII, <u>1964</u>, pp. 204-205)

Ziff goes on to argue that traditional grammar is too coarse and has to be revised. (In this article of <u>1964</u>, Ziff has, to some extent, changed his opinion of the scope of 'grammar'. He writes in <u>Semantic Analysis</u>, four years previously (<u>1960</u>, p. 33):

... just as it would be odd to speak of any deviation whatsoever ... as 'ungrammatical'; so it would be odd to speak of any such deviation as 'odd'. (Ziff mentions non-specific examples such as Finnegan's Wake.)).

I share Ziff's vacillation over uncritical extensions to the notion of 'grammar'--should we regard "He had a green thought" as violating a <u>rule</u> of grammar? If Ziff's later revision comprises a proposal to include rules dividing, say, nouns like "thought", "coat", "idea", "finger" into subclasses only some of which can be qualified by "green", it is arguable that such a proposal is not so much an extension as an <u>abuse</u> of the term "grammatical". We can express our reservations about Ziff's proposal more precisely by considering an example of his own. It concerns the count-noun/mass-noun distinction--which one might suppose to be a "grammatical" distinction:

The word "chicken" in English falls under the category Count Noun and also under the category Mass Noun, and thus, under the disjunctive category Count/Mass Noun... The relevant difference ... is that .. (mass nouns).. require neither an article nor plural affix. Thus, the sentences "It's nice to have chicken" and the sentences "It's nice to have a chicken" are all grammatical. But, if "tree" is simply a count-noun ... the sentence "It's nice to have tree" is ungrammatical.. (Ziff, <u>1964</u>, loc. cit., pp. 211-212)

Certainly, the sentence "It's nice to have tree" is ungrammatical. And, it is important linguistically that a grammar mark the distinction Count-Mass-Noun--always allowing that some nouns, like "chicken", "coffee", "oil", "sea",..., are ambiguous between count- and mass-occurrences (though we might say that that such differing occurrences are of different nouns, because they function syntactically in differing ways). Yet, if the revisions to 'grammar' that Ziff proposes are only such as to draw such distinctions, it cannot follow that category-mistakes ought to be deemed 'ungrammatical'. For, "It's nice to have tree" is not a category-mistake-at least, whilst we are confining "category-mistake" to those absurdities which, on the most general description I have offered thus far, predicate of one type of item what is predicable only of items of different types. Ziff's example would only be category-mistaken if we were to regard the Count-/Mass-Noun distinction as reflecting a difference in type--his sentence would assert of items belonging to the type "countable" what should only be asserted of "non-countable, mass" items. This conclusion, though,

based only upon the heterotypical predication "is nice to have" (predicable unequivocally of "ideas" just as meaningfully as of "rabbits"), would demand far too profligate a range of types.

It might be thought, in any case, that any revision of the extension of "grammatical" should be confined to matters pertaining to <u>syntax</u> and not to the semantics of sentences. Then, since the examples of categorymistakes discussed thus far exhibit a variety of anomaly which, at the very least, must be deemed "semantic" in origin, then arguing for their 'ungrammaticality' is misconceived. We should not regard the syntax/ semantics distinction, however, as bearing such weight as to <u>preclude</u> calling semantic-mistakes "ungrammatical". For, reflecting on the above, there is little reason to suppose that a syntactic criterion, alone, would suffice to draw even the distinction Count-/Mass-Noun--though we have conceded this to belong to 'grammar'. There are better reasons of a more general nature than this <u>ad hoc</u> separation of syntax from semantics, to refuse the necessary extension of 'ungrammaticality' to include rules or conditions proscribing category-mistakes.

In general, I believe, to extend the sense of the term "grammatical", so as to exclude various kinds of anomaly amongst which category-mistaken sentences may be located, is to risk the institution of canonical models of language which are unwieldy and insensitive to linguistic creativity. I have hinted, already, at the consequences of such 'monistic' conceptions of language in the discussion of the influence of the "Ideal Language view" in subsection C of the Introduction. The practices of idealisation for formal purposes there described create a gap between our linguistic intuitions

and our formal investigations, which widens the more we parse out, by idealisation, descriptive features and distinctions which have wide currency in our pre-analytic usage. This gap is not simply the necessary methodological consequence of simplification. As R. Rorty, in his preface to the anthology The Linguistic Turn (1967), has cautioned: the tendency to see the schism between "Ideal" and "Ordinary Language" philosophy, as current commentators have characterised opposing schools, in terms of irreducibly divergent attitudes towards language, can, on the one hand, make the insights into logical structure and inference gained by the former appear contrived and sterile. Equally, however, the same tendency can make the latter's attention to the opacity of idiom, to contextual variance, and usage appear inconclusive and atheoretical. To call attention to this tendency is not, of course, to offer an objection--at best it records a personal disquiet at too uncritical an extension of the scope of 'grammar' to encompass, within its stringent, rule-constituted subjectmatter, a phenomenon so intricate and fluid as a language-speaker's ability to evaluate the significance of sentences in context.

A less general objection to extending the evaluation "ungrammatical" to apply to sentences nonsignificant because category-mistaken, can be expressed in terms of the effect such a revision would have on notions tightly interconnected with that of grammaticality within linguistic investigations. It seems paradoxical to classify sentences like (5) "Caesar is prime", (6) "John frightens sincerity", (7) "Saturday is in bed", (2) "Quadruplicity drinks procrastination; as ruled out on grounds of 'bad grammar'. It seems paradoxical for two reasons: first, such sentences,

uttered in context, are frequently capable of (non-normal) interpretations which make them perfectly meaningful. Secondly, it is often sufficient to render such sentences meaningful to re-interpret their subject-terms as referring, in a context where they do not have their customary reference, to items which <u>are</u> of the appropriate type. Re-interpreting (5) as significant, thus, requires only a context in which a whimsical mathematician records names for his favourite primes; (2) invokes, perhaps, a context in which a tortuous metaphor warns against putting off doubly duplicit enterprises (!); (6) and (7) require only that "Saturday" and "Sincerity" be taken as proper-names of persons, in a suitable context, to make perfect sense.

Enough is enough. Two points emerge from this argument from the possibility of successfully re-interpreting category-mistakes. The first is that <u>one</u> factor in enforcing a metaphorical construal of another's utterance is often that, <u>literally</u>, what he has said is meaningless. Consequently, an appeal to metaphor is often a result of applying some such pragmatic principle as that we should always 'maximise' the possibilities of what another says making sense; rather than rule it nonsignificant. In contrast, the force of the evaluation "ungrammatical" is one of placing sentences <u>beyond</u> meaningful interpretation, unless some disclaimer is attached that the sentence in question is elliptic or colloquial. Thus, if judgements that sentences are ungrammatical are not to lose this evaluative weight (as a precondition for significance), then it must be inappropriate to proscribe sentences like (2), (5), (6) and (7) as "ungrammatical", if there are readings which can be assigned to them, which preserve the

syntactic functions of their constituent expressions, and which render them autonomously significant.

The second point emerging from this argument has to be confined to only certain examples of category-mistaken predications. Nonetheless, it is, I believe, more decisive. I shall dignify it, thus, with the title "the Principle of Referentiality", viz: that the only feature of singular terms which is relevant to determining the category-correctness of sentences in which such terms occur is their <u>reference</u>.⁹ I believe that a modified form of this Principle--discussed in Part II--must apply to <u>any</u> account of category-mistakes. Certainly the arguments in favour of the <u>weaker</u> principle: that <u>one</u> feature of singular terms necessary to determining category-correctness is their reference, are overwhelming. For example:

(11) Virginia is left-handed.

is perfectly meaningful when "Virginia" refers to a person; but it is category-mistaken if, <u>ceteris paribus</u>, "Virginia" refers to the U.S. state. Similarly:

(12)a) Burton's most important discovery was the source of the Nile.

and b) Burton's most important discovery is in Africa.

are, given the reference of "the source of the Nile", categorially impeccable, in contrast to:

(13)a) Curie's most important discovery was the source of gamma-ray radiation.

and b) Curie's most important discovery is in France. which is category-mistaken--despite the ambiguity of "source" between

(12)a) and (13)a). Thus, from these examples of sentences where only the <u>reference</u> of singular terms distinguishes category-correct from categorymistaken assertions, it is difficult to see how the Principle of Referentiality--even in its weak form--can be reconciled with the proposal to treat category-mistakes as violations of grammar. For, there are few extensions of the notion of 'grammar' which would permit determination of the <u>reference</u> of a singular term to be a <u>grammatical</u> feature of sentences, subject to grammatical conditions, simpliciter.

The conclusion to be drawn from these two appeals: to the possibility of reconstruing category-mistakes in alternate contexts, and to the Principle of Referentiality, is to cast severe doubt upon the utility of extending the sense of "ungrammatical" to apply to categorymistakes. The point of these appeals is not to suggest that, in the end, category-mistakes will not be anomalous or deviant in some way or other. The point is, rather, that the line between what is significantly assertible in context and what is not simply does not divide the spectrum of indicative sentences at the same point as that between 'grammatical' and 'ungrammatical' sentences. For, it is not that there is any essential oddity in the reconstruals of examples of category-mistakes which I gave, which could be taken as indicacive of the effort required to correct a grammatical distortion, or breach of a rule. Indeed, the alternative readings, however superficial and ad hoc, are distinctively heterogeneous-indicating how sensitive our claims as to the significance of utterances must be to the context in which they are produced.

This more general conclusion can be put another way: any

proposal to treat category-mistakes as ungrammatical should be underpinned by provision that the context in which we interpret them be understood as 'normal' or 'standard' for the expressions occurring in them. To make this provision, however, is to concede that "grammaticality" (in its extended sense) is relative to context--where "context" has to be taken as including both "linguistic context (surrounding discourse)" and "physical environment" of the utterance, so as to accommodate the Principle of Referentiality. For, terms may have different references in different physical circumstances. The implausibility of making this provision, however, is that, in consequence, all the distinctions we make along the spectrum of linguistic evaluations will collapse into the generic distinction "grammatical/ ungrammatical". Even such transient features of sentence-use as "relevance", "pragmatic appropriateness" might become 'grammatical' features on such a comprehensive extension to the domain of grammar. As I shall suggest, below, in criticising Katz' proposal to treat category-mistakes as "semantic anomalies", such an overweaning expansion of the subjectmatter of 'grammar' often lies behind such implausible accounts. In any case, the proposal under consideration demands a prior determination of when the context in which an expression is used is 'normal' and when not. It is not clear that this distinction--conditional as it is upon extralinguistic considerations--can be drawn in a non-question-begging way. In sum, I believe these arguments suffice to reject the general proposal to treat category-mistakes as ungrammatical (a Model-III-type account: Introduction, p. 13). More specific applications of these points will come to light in the examination of the particular account of Katz, Fodor

and Postal, below.

Category-mistakes as "falsehoods" (The SNeg-argument):

I turn, now, to general consideration of claim 2): that categorymistakes are not only grammatical, but significant and false.

It would be a telling criticism of the appeal to type theory to diagnose the nonsignificance of category-mistakes, if such sentences should prove better candidates for making <u>false</u>, rather than meaningless assertions. For, the meaningfulness of an assertion is ordinarily taken to be a precondition of its making a true or false statement. By claim 2) though, category-mistakes would have to be regarded as meaningful, in order to be truth-valued.

To begin: I shall consider the more general claim that categorymistakes should be construed as making truth-valued (though uniformly false) statements, having standard, classical negations. At outset, two versions of this more general claim can be distinguished (c.f. Diagram I, p.250):

2a) that a category-mistake S and its negation not-S are both false (so far as I know, no-one is prepared to maintain that category-mistakes are true);

2b) that a category-mistake S is false and its negation not-S is true.

In all likelihood, claim 2a) cannot be seriously maintained. Any semantics for a language permitting violations of type to be well-formed, but which assigned the value 'false' to any well-formed formula and its classical negation would either be inconsistent (if S and not-S are both false, 'S or not-S' is false; whence 'not-(S or not-S)' is true, as is 'S and not-S'); or a special sense of negation would be involved. If the former, then the semantics is uninteresting. If the latter, then some sentences are being marked distinctive (namely; category-mistakes) as having a significant negation which cannot operate uniformly in a classical manner.¹⁰ It follows that category-mistakes are being distinguished from other sentences as those for which classical negation laws fail. Thus, no real alternative to treating category-mistakes as yielding neither true nor false statements is being proposed.

2) is a more likely proposal. The benefits of adopting it are quite attractive: it preserves bi-valency and the law of excluded middle for a sentence and its classical negation.¹¹ It denies, thereby, one of the assumptions maintained throughout the above--namely, that category-mistakes, amongst other varieties' of nonsignificant sentences, are properly classified as meaningless; that the ground for their <u>literal</u> absurdity, or unintelligibility, is that, in context, they are devoid of descriptive significance--they fail to make true or false statements. Rejecting this assumption in favour of preserving classical logic has found support, in particular, in Quine (<u>1960</u>, p. 229), Ewing (<u>1937</u>), Prior (<u>1954</u>) and Drange (<u>1966</u>). Often enough, this support has been grounded upon what I shall call "the Argument from Significant Negation" (or the "SNeg-argument", for short); thus, from Ewing and Prior:

It is usually held that a sentence which ascribes to something a relatively determinate value of a determinable which does not qualify it is meaningless, whether the determinate value is asserted or denied of it ... This .. I am prepared to dispute. For, after all, "Quadratic equations do not go to race-meetings"

is entailed by "Quadratic equations do not move in space" and entails "Quadratic equations do not watch the Newmarket horse-races"; but, if it is capable of entailing and being entailed, surely it must be a proposition and not a mere meaningless set of words. (Ewing, 1937, p. 360)

and, from Prior (1954, pp. 159-160):

My proof that virtue is not square is a simple syllogism: what is square has some shape, but virtue has no shape, therefore, virtue is not square. That my left eye is not square would, of course, have to be proved a little differently ... from a logical point of view, the difference is not very great ... In saying that my left eye is not square, I am not saying that it is of some other shape and, in saying that virtue is not square, I am not saying that it is not of any other shape, either; in both cases I am saying that the thing is not square, and that is <u>all</u> I am saying.

Stated in the briefest way possible, my replies to the particular appeals to the SNeg-argument from Ewing and Prior are as follows: in Ewing's case, it is <u>false</u> that what is "capable of entailing and being entailed" must be "a proposition and not a mere meaningless set of words". The dichotomy is unrealistic: for, what is meaningless when uttered in one context may be meaningful when uttered in another--and what entails and is entailed need not be significant. For, the literal nonsignificance of "Virtue is blue or red" can be said to be entailed by the nonsignificance of "Virtue is blue" and "Virtue is red". Close attention has to be paid, here, though, to <u>what</u> is being said to be "nonsignificant" and <u>what</u> is entailing and being entailed. We need not suppose that the relata of 'entailment' are the same items (sentences, statements, propositions, utterances) as are the objects of significance. In brief opposition to Prior, moreover, it can be pointed out that in <u>saying</u> that virtue is not square, one can be interpreted either as asserting of virtue that it does not have that shape (which, at best, is misleading; at worst, unintelligible) or as asserting of virtue that it is not the kind of thing which has that shape--which is true--but which is not the classical negation of "Virtue is square".

These brief replies, however, await more general considerations to give them substance. In general, the SNeg-argument, it seems to me, introduces a far weightier reason for adopting claim 2b) than the appeal to preservation of classical negation laws. For the SNeg-argument, 'a la Prior, proceeds: apparent category-mistakes have negations which can be proved true by appeal to premises whose philosophical significance is beyond question. If category-mistakes have significant, true negations, must not they, themselves, be significant and false? Consider the following:

- (3) a) Virtue is an abstract quality.
 - b) Abstract qualities do not have colours.
 - c) Whatever is blue has some colour.
 - d) Therefore, virtue is not blue.
- (9) a) Smells pertain to the olfactory sense.b) What pertains to the olfactory sense cannot have an auditory property.
 - c) Being loud is an auditory property.
 - d) Therefore, smells are not loud.
- (10) a) Fears and beliefs are mental (states).
 - b) No mental state occupies position in space.
 - c) If one thing is three feet above another, both occupy positions in space.
 - d) Therefore, his fear of flying is not three feet above his belief in ghosts.

The conclusions of these arguments, it is supposed, follow from true premises, and are therefore true. If the negation of a category-mistake is provably true, the category-mistaken sentence must be necessarily false, logically false (contradictory), <u>a priori</u> false or (inclusively) false by definition (see: Diagram I, above p. 250). In considering the SNegargument, then, my focus will be upon whether a category-mistake can be said to be <u>false</u> in any of these senses. That is, the general discussion will concern the question: if category-mistakes are false, what kind of false statement is made by a category-mistake?

A general point of caution can be brought against claiming that category-mistakes are false, by reflecting upon the following argument from Pap (Elements of Analytic Philosophy, 1949, pp. 332-3):

If a theory of meaning goes to such extremes of tolerance as to condemn no sentences whatever as meaningless, it does not deserve consideration. Such is, for example, the theory that any sentence is meaningful provided it conforms to the rules of syntax. For the rules of syntax presumably define what strings of words are to be called sentences, hence a pattern of words that fails to conform to those rules is not so much a meaningless sentence as no sentence at all. Those who adhere to this theory ... would then have to say that the sentence "Quadratic equations like coffee" is meaningful, though undoubtedly false. But if they thuse refuse to admit that some sentences are not so much false as meaningless, they will simply have to draw a distinction between two kinds of falsehood which turns out to differ very little from the explicitly repudiated distinction between meaningful and meaningless sentences.

The threat to treating category-mistakes as false, in any sense, thus, is that, if a <u>unique</u> kind of falsity is necessitated for the description of category-mistakes, no genuine alternative to treating such sentences as nonsignificant is being proposed. The contrary position has to show, therefore, that category-mistakes can properly be degmed 'false' in some previously well-understood sense of "falsity".

It would be difficult to sustain a view which makes category-mistakes

contingent falsehoods; i.e. to claiming that there is no difference in kind between an ordinary standing falsehood, such as:

(14) September 15th, 1978, is a Saturday, and an example of a category-mistake like:

(15) The number seven is breakable.

Such a view may be seen in its best light, perhaps, as construing sentences as enumerated in a list in accordance with judgements we make as to <u>how</u> false they strike us; but, with respect to which there is no obvious boundary which can be drawn between sentences which are factually incorrect and sentences which are categorially incorrect. (If we pall at the notion of "degrees of falsity", we can always regard the measure as applying to the 'probability' that an informed speaker would assign 'false' to the sentence). Seen in its best light, then, the view has the merit of pointing to the difficulty of drawing an <u>a priori</u> distinction between 'matters of fact' and 'matters of language'--that "false by virtue of fact" and "false by meaning" is a spurious dichotomy.¹²

To concede to category-mistakes being contingent falsehoods, however, requires substantial revision of what we commonly say about contingent statements. The view would have to accommodate the possibility that some state of affairs could count towards falsifying the general truth that numbers are unbreakable. Indeed, this example can be generalised to apply to any of the views which take category-mistakes as false. If (15) is false, assuming bi-valency, then:

(16) The number seven is not breakable. is true--as is its equivalent:

(17) The number seven is unbreakable.,

which attests to the improbability of some state of affairs being one in which the sum of three and four breaks. But such a state of affairs is not just improbable (to any degree), it is impossible; there is no such state of affairs--to claim that there is is to assert no truth or falsehood, it is not to assert anything meaningful at all.

To this argument it might be replied that a Model I-type account of <u>degrees</u> to which sentences are increasingly rejectable as false provides an account which can accommodate the possibility that there is a progressive diminution in the acceptability of sentences--without there being a non-arbitrary boundary between 'factually false' and 'analytically false' or 'meaningless', which can be drawn <u>a priori</u>. T. Drange (<u>1966</u>, p. 17) has argued that, if we consider the list:

| (18) | i) | Nixon was | the fifteenth president of the U.S.A. |
|------|------|-----------|---------------------------------------|
| | ii) | Nixon was | honest. |
| | iii) | Nixon was | female. |
| | iv) | Nixon was | coniferous. |
| | v) | Nixon was | sedimentary. |
| | vi) | Nixon was | prime. |
| | vii) | Nixon was | derivable., |

then we may wish to regard all of (i) - (vii) as graded in degree of acceptability in such a way that no boundary between 'false' and 'meaningless' can be drawn in a non-arbitrary way. Drange argues that this establishes that category-mistakes (what he calls "type-crossings") <u>can</u> plausibly be regarded as false. For, he claims that the difference between (18) i) and (18) vii) is merely a difference 'in degree'; not a difference 'in kind'. He goes on to explain this distinction as follows:

To put it very roughly, two things differ in kind when

one has a property which the other lacks, and they differ in degree when there is a property which one possesses to a greater degree than the other ... (Drange, <u>1966</u>, p. 16, fn. 3)

To put such a distinction "very roughly", in such a way, is to make no distinction at all--for, it is true of <u>any</u> two items that "one has a property the other lacks" (by the indiscernibility of identicals); so there are as many 'kinds', in Drange's sense, as 'things'. Similarly, things radically different in kind (in some more adequate sense of "kind")--say, hangovers and play-readings--may share a property which one possesses to a greater degree than the other. Both hangovers and readings from Raçine are interminable; but the former endures longer than the latter. In brief, the distinction "difference in kind/degree" cannot bear the weight Drange places upon it--unless the notion of a 'property' is more carefully explicated.

A Model I-type account of category-mistakes regards sentences as graded in respect of abnormality along a line from contextually inappropriate to meaningless. To accommodate claim 2b), then, such a Model still has to account for the difference between a standing, contingent falsehood and the falsity of a category-mistake. I shall argue now that there is no sense of "falsehood" attributable to category-mistakes which is nontrivially distinguishable from regarding such sentences as distinctively nonsignificant. My first response to the SNeg-argument, then, will be that the putative proof of the falsity of category-mistakes, based upon the significant truth of their denials, is not supported by any recognisable sense in which such sentences can be deemed "false". Following this, I will argue against the specific versions of the SNeg-argument considered, that they do not prove what they claim.

Ewing (<u>1937</u>, p. 360 - quoted above) and Prior (loc. cit.) have suggested that category-mistakes be subsumed under the class of <u>logical</u> (self-contradictory) falsehoods. One can see at least two arguments which might have lead them to suggest this (both arguments presuppose the SNegargument):

Argument 1) a) Certain predications of items referred to as instances of a kind are necessarily true of those items.

b) What is deductively incompatible with a necessary truth is logically false.

c) Category-mistaken predications assert of items of one kind what is deductively incompatible with predications necessarily true of items of that kind

d) Category-mistaken predications, hence, are deductively incompatible with necessary truths.

e) Therefore, category-mistakes are logically false. The argument is appealing only in so far as it enables us to disregard any <u>distinctive</u> problems attaching to category-mistakes and concentrate upon the generic problem of conditions for necessary truth or falsity. Thus, it can be construed as offering either a Model I- or Model III-type account of category-mistakes according as we impose <u>strict</u> criteria demarcating between necessary falsehoods and others (Model III) or not (Model I). Argument 1), however, clearly involves a tacit appeal to the SNeg-argument in claiming that, in order for the classical negation of a category-mistake to be deducible from necessary truths, the mistake itself must be significant and (necessarily) false. Where Argument 1) lacks cogency is not so much in its appeal to 'logical falsity', as in the absence of a relevant sense of "necessary truth" to apply to those category predications mentioned in Premises a) and c). Various possibilities suggest themselves, none seem adequate.

Consider, again, the illustrative arguments from Prior, that category-mistakes are provably false because their classical negations follow logically from true premises (above, p. 293). Prior offers the example: (1954, pp. 159-160):

for which I offer the following derivation:

Abbreviations:v = virtue.
Ax = x is abstract (an abstract quality)
Sx = x is a shape
Hxy = x has y
Qx = x is square.Premise (i)Av
(ii)
(x)(y) (Ax & Sy $\supset \sim$ Hxy).

(iii) (x) (Qx ⊃ (∃y)(Sy & Hxy)). To prove: ∴~Qv

Qv (Assumption for -Intro.) 1) 2) Αv (Premise) $Qv \supset (\exists y)(Sy \& Hvy)$ (U-elim, (iii) x/v). 3) 4) (∃y)(Sy & Hvy) (⊃ -elim, 1), 3)). (**∃** -elim, 4), y/w). 5) Sw & Hvw (U-elim (x2), (ii) xy/vw).Av & Sw ⊃~Hvw 6) (&-elim, 5)). 7) Sw (& -Intro, 2), 7)). Av & Sw 8) (**⊃**-elim, 6), 8)). 9) ~Hvw Hvw (&-elim, 5)). 10) (~-Intro, 1)-9), 10)). 11) ~Qv

The derivation is deductively impeccable; it shows that (iv) can be validly

inferred from (i) - (iii). It does <u>not</u> show that (iv) is a necessary truth. This follows only if each of (i) - (iii) are 'necessary truths'-but in what sense? Clearly, none of (i) - (iii) are <u>logical truths</u> if we mean by that explicit tautologies whose negations are contradictory. Perhaps then, Ewing and Prior's claim has to be interpreted in the form: <u>given</u> that (i) - (iii) are all true; then the conjunction of (i) - (iii) with the denial of (iv) ("~~Qv = Qv") is inconsistent. That is true, but uninteresting. An enquiry into category-mistakes should tell us, if category-mistakes are false, not only the grounds for the falsity of (iv); but also grounds for the truth--and, if <u>logically</u> false, then grounds for the <u>necessary</u> truth--of (i) - (iii). What grounds do we have for the necessary truth of (i) - (iii)?

We can rule out at once, as question-begging, any ground for (i) -(iii) being necessarily true because their denials are category-mistaken. Nevertheless, the classical negation of Premise (ii):

(ii)' At least one abstract quality has a shape. strikes us as at least as category-mistaken as "Virtue is square". Similarly, the denial of (iii):

(iii)' There are shapeless squares.is, at least arguably, analogous to the existential generalisation of (17):

(17)' There are unbreakable numbers. which I argued to be meaningless. Perhaps, only the denial of (i):

(i)' Virtue is not an abstract quality.
is a candidate for a <u>false</u>, rather than meaningless, assertion. It is so
because its equivalent:

(i)" Virtue is a concrete quality.

appears to be a substantive claim, which is false on philosophical grounds (given that "abstract/concrete" is a philosophical distinction). This is insufficient for Ewing and Prior's claim, though. They require that all three premises express significant, necessary truths.

In other words, the claim from Ewing and Prior that categorymistakes are logical falsehoods having provably true negations reduced to a claim that certain predications like (i) - (iii) are non-tautologous necessary truths. Argument 1), then, establishes only that Ewing and Prior's answer to the question: what kind of falsehood is a categorymistake? reduces to the question: from what kind of necessary truth is the negation of a category-mistake provable? This does not take us very far at all.

Reflecting further on this argument, however, we may note at this point that deductions like (i) - (iv), and category-predications like (i) -(iii), do draw attention to a distinctive feature of category-mistaken predications which I shall exploit in Part II. The idea derives from A. Pap's discussion of types and meaninglessness (Pap, <u>1960</u>). Although Pap never specifically addressed himself to the problem of defining the notion of a category-mistake, and certainly would <u>not</u> have acquiesced in their being treated as necessarily false, it is possible to suggest an approach to category-mistakes from remarks he makes in this article. Towards the end of Pap, 1960, p. 54, we find:

The locution "x is not the sort of thing to which predicate P can be ascribed", which is frequently used by ordinary language analysts who caution against category-mistakes, expresses nothing else than the feeling that "x is P" is meaningless,...because a presupposed type-predication is false.

This suggests that category-mistakes can be construed as sentences which <u>presuppose</u> "false type predications". But what does "presupposition" mean here; and what is a type-predication? The substantive exposition of this claim for category-mistakes must await Part II (Section D); but we can notice, here, Pap's own remarks on these questions--and the decisive objection to Pap's account which appears in Max Black's discussion of Pap's article.¹³

Earlier in Pap, <u>1960</u> (pp. 47-48), Pap explains that he intends "B presupposes A" to mean "the falsity of A entails the meaninglessness of B". In addition, he suggests "T is a type predication" is to mean that the sentence "T" can be transposed into an assertion of the form " $x \in \underline{a}$ " where <u>a</u> is a type. Finally, then, (p. 48), a type is explained as:

A type is a class such that there are families of predicates which can be significantly, i.e. correctly or falsely, ascribed to all and only members of it. A predicate family is a set of predicates such that one and only one member of it must be true of anything of which some member of the set is true or false.

Pap continues with the following examples of type predications:

| True type predications | False type predications |
|-----------------------------|--|
| Socrates is a person. | Socrates is a number. Modesty has shape |
| Thoughts are mental events. | Thoughts are events in space. |

(These examples are only suggested in the text of Pap's article, p. 54). In consequence of these definitions, sentences which presuppose false predications of type, and are therefore meaningless, because category-mistaken, would be assertions whose truth or falsity demands the truth of some type predication in the right-hand column: e.g.

> Socrates is a prime number. Modesty is triangular. My thoughts are in my brain.

These assertions are exactly those paradigms of category-mistakes to which I have been referring throughout the above.

I believe that Pap's approach contains the beginnings of an adequate account of the nonsignificance of category-mistakes, based upon distinctions of type. I try to develop such an account in Part II. We can speculate, at this point, that the enterprise of Part II would have been rendered redundant, had Pap extended his account in certain, relatively obvious ways: which is further reason to regret his early death, shortly after publication of the article discussed above (--in September, 1959--the article had originally been read before the American Philosophical Association, Eastern Division, in December 1957, as part of a symposium with Max Black).

I defer further discussion of Pap's appeal to "type predications" until Part II; but, from the account above, we can notice the decisive objections that Black raised to the article---for they indicate flaws that my own account will have to avoid.

The difficulties in Pap's account to which Black draws attention (Black, <u>1957</u>, pp. 56-59) all involve the notion of a "type predication". First, Black comments: (p. 56)

... it seems that in order to determine whether a given class is a type, in Mr. Pap's sense, we must have at our disposal a prior inventory of types. Clearly, Pap's definition of a <u>type</u> as a class determined by families of predicates (above), does not give us a workable criterion for whether or not a given class is a type. Nor should we expect such a criterion, in view of the arguments I have already adduced--at the opening of this section-against the demand that an account of category-mistakes rely on prior determinations of category-membership. For, we should not suppose that <u>types</u>, <u>qua</u> classes of items, are, somehow, antecedently given--in advance of evaluating the significance of predications over items in the class. The solution, thus, seems to involve denying that types are antecedently given classes, and to maintain instead that, whether a given predication is, in context, significant, will depend upon procedures of 'sorting' and assigning 'types' to whatever the predication is about, in a way in which the predication can yield a true or false statement. This solution-severely condensed, here---is fully articulated in Part II.

Black's second criticism is that type-predications, themselves, must be autonomously significant--since they cannot themselves presuppose type predications without threat of infinite regress. Thus, the "True type predications" listed on the left hand column, above, must be significant and true, and their negations significant and false. Yet this requires an appeal to a notion of significance (with respect to type predications) which is not subject to category restrictions. It follows that the sentences, below, are not category-mistaken:

Plato is a relational attribute. The theory of types is an event. An idea is a physical object. Pierre Trudeau is a fact.

Though all such type predications will be <u>false</u>, they have to be meaningful, in order that they not presuppose further type predications. To this implausible consequence Black adds the following counterexample (p. 57): on Pap's view, a predicate like "is a logician" would turn out to be a type predication, because its range of significance is the class of items which can be significant arguments to "is admired by every other logician". This follows from the fact that the sentence:

(19) <u>A</u> is admired by every <u>other</u> logician. presupposes that <u>A</u> is a logician--whence, it fulfills Pap's explanation for "x is P" (A is a logician) to be a type predication.

Pap attempted to avoid this difficulty in his article (p. 50); but without success. He tried to argue that the range of "x" in "x has relation R to every <u>other</u> member of class K" must extend beyond the class K--thence, that some more comprehensive predicate than "x is a logician" is presupposed by (19). His attempt is unfortunately mistaken, since it requires that the significance of (19), when <u>A</u> is <u>not</u> a logician, can be determined independently of (19)'s presuppositions--which defeats his own account. What is required is an independent account of 'types' which restricts "type predications" to very general features necessary for the significance of predications. A hint is provided by noticing that Pap intended "type predications" to be precisely those <u>formal predicates</u> which express syntactically simple features which were discussed, above, in connection with Wittgenstein's doctrine of showing. Amalgamating Wittgenstein's doctrine of <u>formal predication</u> (without the doctrine of showing) with Pap's account of type predications is essentially the approach I undertake in Part II-where this hint is given full articulation.

I have digressed, above, from the primary purpose of this subsection: to criticise the view that category-mistakes are false--primarily because, in the light of Pap's account, we can at least comprehend what led Ewing and Prior to consider category-mistakes a kind of logically false statement. For, the necessity of the category-predications to which Ewing and Prior appealed in arguing for the provable truth of negations of categorymistakes derives from such predications being presupposed in the significant denial of the application of a predicate. I mentioned above, though (p. 299), that there are at least two arguments that might support Ewing and Prior's claim. The second has to be more probative than a bare appeal to the non-tautologous, necessary truth of category-predications. Ewing and Prior can claim that, if derivations like the one I gave, are to be admissable in classical logic, then both premises and conclusion must be significant (true or false). For, they could be interpreted as claiming that a necessary condition of B's being derivable from A1,..., A_n is that all of A_1, \ldots, A_n, B be significant. This can be inferred, at least, from Ewing's claim (1937, p. 360) that "if it is capable of entailing and being entailed, surely it must be a proposition ... "

This second argument can be construed as making a <u>weak</u> or a <u>strong</u> claim. As a weak claim, it may state a methodological preferrence to treat apparently nonsignificant sentences as uniformly false, in order to preserve classical logic. As such, it is subject to the same reservations and counterarguments as I gave in the Introduction (subsection C, pp. 26-32) against practices of <u>idealisation</u> which confine the scope of

logic solely on grounds of simplification.

As a <u>strong</u> claim, the argument may be construed as stating that <u>only</u> syntactically well-formed, meaningful sentences are admissable substituends for the variables of sentential logic. Hence, categorymistakes, unless treated as 'false' (by stipulation) are not subject to logical principles.

The <u>strong</u> claim, itself, divides into two substantive parts: (i) that logical principles apply only to meaningful, well-formed sentences.

(ii) that category-mistakes, if well-formed, can be shown to be necessary falsehoods, by the SNeg-argument.

Neither claim can be sustained if we attend to arguments adduced already. Claim (i) is trivialised if "syntactic admissibility" is identified with "generated by application of recursive formation rules in some classical formalism". (i) then entails that category-mistakes are "ungrammatical" in a strong sense--and what is lacking is <u>any</u> description of the syntax of a classical formalism which is sufficiently rich to express the grammar of a natural language (a question currently in dispute: see, Montague, <u>1970</u> and Jardine, <u>1973</u>). On the other hand, if (i) makes a non-trivial claim--that, in the end, logical principles <u>cannot</u> apply to categorymistakes because they violate syntactic rules placing them, thus, beyond what can be meaningfully discussed--then the claim is subject to just those arguments which rejected unwarranted extensions of the domain of 'grammar', given above.

Claim (ii) offers various alternatives. To begin with: it cannot be claimed that category-mistakes are logically false in the syntactic sense (that some formula of the form "p & not-p" is derivable from any category-mistake). Such a claim would have the consequence that negations of category-mistakes were <u>derivable</u> theorems of some classical formalism. In the absence of any such axiomatic theory, the claim is empty.

Alternatively, claim (ii) could be construed as identifying categorymistakes with a sub-class of logical falsehoods in the semantic sense; i.e. that, with respect to some interpretation of, say, first-order logic, category-mistakes can be syntactically admitted, yet assigned the value 'false' for all valuations (or, if quantified modal logic is required, then "false in all possible worlds"). Some category-mistakes, however, would have to be symbolised as atomic predications, in such logics. Such atomic predications would require ad hoc assignments of undesignated values to all and only those sentences we deem category-mistaken--in order that their negations be universally assigned the designated value 'true'. This merely restates the problem; for the question of what kind of falsehood is a category-mistake reappears as: in terms of what conditions upon assignment-functions in the formal semantics for such languages will only category-mistakes (or only nonsignificant sentences) be interpreted to yield 'false' uniformly and 'true' for their negations? Claim (ii), if adopted, reaps no special benefits.

If category-mistakes are not logically false in either of the standard syntactic or semantic senses, perhaps the SNeg-argument can be re-instated as supporting Quine's claim that category-mistakes may be regarded as "false by definition" or "analytically false". It may seem strange to cite Quine as an advocate of any kind of "analytic" falsity--in

view of his notorious scepticism over 'analyticity', and his antipathy for appeals to non-tautologous necessities or 'truths (falsehoods) by meaning'. His antipathy for any special treatment of category-mistaken, or other grammatical but nonsignificant assertions, apparently outweighs his concern over 'analyticity--as the comment (Quine, <u>1960</u>, p. 229 - quoted above, p. **281**) clearly shows.

There are a number of reasons why one might reject this proposal out of hand---not the least of which are Quine's own reservations about appeals to "truths or falsehoods of meaning", (Quine, 1953). Thus, if we attach minimal sense to Quine's dictum-that "the forms concerned remain under control if admitted ... as .. false by meaning ... " (op. cit.), then we have to concede to the negations of category-mistakes being analytic truths--in just the sense in which "An oculist is an eye-doctor" is true by definition. Just such an attempt to make category-mistakes 'definitional falsehoods'--Katz' treatment of violations of selection restrictions in lexical entries for constituent words in categorymistaken sentences -- is criticised in detail, below. Alternatively, we might reconstrue Quine as proposing that category-mistakes are reducible to logical falsehoods by replacement of constituent expressions by defined equivalents. Thus, we could regard Ewing and Prior's purported proofs of the truth of negations of category-mistakes as leading by definitions from premises to conclusion. This would entail, thus, that, by definition:

- (3) a) Virtue is an abstract quality. Df.
- (9) b) Objects of the olfactory sense do not have auditory properties. Df.
- (10) b) Mental states do not occupy spatial positions. Df.

It is simple non-explanatory, however, to render sentences like (3)a), (9)b) and (10)b) true by definitional <u>fiat</u>; if only because, reinterpreting a category-mistake as making <u>non-literal</u> sense would be to commit some kind of linguistic mistake. It is surely implausible to suggest that a shift in the sense of a word in a context is comparable to a <u>change</u> in definition. We do not <u>define</u> "olfactory properties" in terms of a lack of auditory properties ((9)b)); nor, when Descartes reflects that mental substance is not spatially extended, is he reflecting upon a <u>definition</u> ((10)b). Nor, when naturalists identify virtue with a simple quality of things are they guilty of errors of definition ((3)a)). In all, what is lacking in an account of definitional falsehood as applying to category-mistakes, is primarily a non-trivial account of what such "definitions" are like. It is relatively perspicuous to suggest, for example, that "Some bachelors are married" is reducible to contradiction by means of:

a) Some bachelors are married.

- b) "bachelor" means "unmarried man". Df.
- c) Therefore, some unmarried men are married.

It is a quite different suggestion that the inference:

from: a) Mental events are physical states of the brain. Df. Df. Df. to: n) Some events are both physical and mental.

proceeds from one purported category-mistake to a logical falsehood in n-1 steps, each of which replaces a constituent expression by a defined equivalent.

In sum, we have found that what is generally lacking from accounts
which equate category-mistaken assertions with logical, necessary or analytic falsehoods is an independent account of <u>how</u> such assertions exhibit that distinctive kind of falsity. There remains only the SNegargument to justify treating category-mistakes as false--together with an appeal to a generic notion of 'falsity' derivative from categorymistakes being rejectable <u>a priori</u>. At this point, I shall alter the course of argument, somewhat, and suggest that, in this sense, there is nothing wrong with treating category-mistakes as 'false' because rejectable on <u>a priori</u> grounds. Such an appeal to <u>a priori</u> grounds reveals what is compelling about the SNeg-argument. In addition, it suggests the Model of nonsignificance that I shall advocate in Part II. That is, in emphasising the family of related concepts which bear upon 'significance', 'assertibility', 'intelligibility', and <u>a priori</u> 'acceptability', it endorses an account which fits the general schema of Model II. It is a Model II-type account which I advocate in Part II.

Significance Claims - the SNeg-argument revisited:

If someone proposes to evaluate a category-mistake like "Facts change colour" as false because rejectable <u>a priori</u>--meaning, thereby, that category-mistakes are incompatible with truths he claims to know <u>a priori</u>--then I have no arguments to offer against him. Such a proposal takes seriously the suggestion that logical relationships (inference, incompatibility) are not properly confined to the well-formed, autonomously meaningful sentences of a formal language, since he holds that such relationships extend to inferences which concern the significance (as well as

truth and falsity) of indicative assertions. In addition, the proposal preserves an important feature of this variety of anomaly--namely, that in a standard context an adult speaker, endowed with adequate information, will reject a category-mistake as literally unintelligible, without having to appeal to any special experience other than that required to <u>understand</u> the expressions involved. So, such a proposal offers one important ground for connecting the semantic notion of 'significance' to the pragmatic notion of 'contextually assertible' in a way which justifies the evaluation of category-mistakes as contextually-relative significance-failures. Indeed, it is the <u>a priori</u> unacceptability of such sentences which is the source of the temptation to describe their negations as true, by the Argument from Significant Negation.

The claim, here, is that the plausibility of the SNeg-argument derives from the ambiguity of denials. As I shall argue, below, in discussing Ryle's account of category-mistakes (Ryle, <u>1938</u>), when points concerning category-differences are made "in a properly brusque way" (Ryle, <u>1954</u>, p. 9), then it is the inappropriateness of some range of predications to some class of subjects that is normally stressed. Typically, category-points are made in a "properly brusque" manner, when asserted in any of the following styles:

"x's are not the kind of thing which can be \emptyset ." "Items of the sort x neither \emptyset nor fail to \emptyset ." "Being \emptyset cannot be significantly asserted or denied of x's." "It is nonsense to assert \emptyset of x's".

I shall call <u>all</u> such assertions--made in this fashion--"significance claims" which are to be distinguished from "truth claims", "knowledge

claims", "grammatical claims" and claims for the "relevance" or "propriety" of what is said. There is no presumption, in so doing, that significance claims can only be made by assertions of the above form--nor that, in all contexts, they always are. In particular, category-mistakes, typeabsurdities, and significance-failures may manifest themselves in ways other than that of the grammatical relation between subject and predicate; for example, as between verb and adverb:

(20) John tripped thoughtfully.or between noum and qualifying adjective:

(21) Life is a green thought.

In neither case is a denial that the <u>predicate</u> can be significantly applied to the subject appropriate. I <u>shall</u> assume--from hereon--that, if an account can be given of category-mistakes in <u>predication</u>, the explanation may be extended in a natural way to other species of significance-failure. This is not a wholly empty simplifying assumption--but I have insufficient space to discuss other than category-mistaken predications.

The notion of a <u>significance-claim</u> is integral to the remainder of the thesis--though its full exposition must await Part II. At this point, it affords some explanation of the initial plausibility of the SNeg-argument. A significance-claim is usually, but not always, a denial which is both significant and true; and its negation is significant and false. Thus, "Saturday is not the kind of thing which can be in bed" is true--its contradictory, that Saturday <u>is</u> that kind of thing, is significant and false. But this way of expressing significance claims lends itself easily to the ellipsis which is expressed as a simple denial; i.e. "Saturday is not in bed", "Caesar is not prime", "Virtue is not blue" which, when asserted as significance claims, are significant and true. If, however, the classical negation "not-p" of a category-mistake "p", is evaluated as taking the opposite truth-value to "p", then, when "p" fails to be significant, "not-p" is nonsignificant, also.¹⁴ A denial of significance, therefore, is not a 'kind' of negation-for, when one protests "it is nonsense to say that facts change colour", one is not committed to asserting the invariance of the hue of facts. In this way, the ambiguity of, say, "Caesar is not a prime number"--with the name being given its customary reference--is between its construal as a classical negation and its appraisal as a significance claim (denying that "being divisible by one and itself" can be asserted of Caesar). So, an appeal to the falsity of category-mistakes based upon the SNeg-argument is insensitive to this ambiguity. Indeed, such simplistic appeals foster those confusions which promote taking category-mistakes as necessary falsehoods or analytic falsities, by appeal to syllogisms like those given by Prior (1954). Consider, again, for example:

(3) a) Virtue is an abstract quality.
b) Abstract qualities are not blue.
c) Therefore, virtue is not blue.

It is argued that, since a), b) are both true, and the argument valid, c) must be true, and its negation false. The truth of b), however, depends upon its being taken as a significance claim, in the style:

(3) b)' Abstract qualities are not the kind of thing which can be said to be blue or not blue.

whence, the premises of (3) remain true; but the conclusion should be

"Virtue is not the kind of thing which can be said to be blue or not blue". Or else, the whole argument begs the question by assuming that (3) b), when taken as a classical negation, and not a significance claim, is significant. For, as stated, (3) b)'s significance is just as much in question as that of the conclusion. Similar remarks apply to the other examples of 'syllogisms' considered on p. 294.

Now, if we reconstrue syllogisms like the above as having significance claims in their premises, much of the rationale for treating category-mistakes as false evaporates. Appraisal of, say, (3) b) now requires explicit recognition of significance features not accountable to the simple truth or falsity of sentences and their negations. That is, appraising the truth of significance claims may require inferences from and to significance features of sentences, and generalisations of them, which demand logical investigation in their own right. Suppose we infer, for example, "Saturday is not in bed" (qua significance claim) from "Whatever is in bed is not a day of the week (qua significance claim) and "Saturday is a day of the week". There is then an instantiation step in the inference which, if the step were to be classically valid, either "Saturday is in bed" would be autonomously meaningful (and false) -- whence the inference begs the question (false premise) -- or it has to be accepted that some classically valid inference patterns apply to significance features. The investigation of logical relations based upon significance features of sentences (uttered in context) is the primary aim of the logics developed in Part II.

There remains little wrong, of course, with construing category

mistakes as 'a priori rejectable', or 'deductively incompatible with a priori truths'--indeed, the systematic examination of the deductive grounds for such a priori rejections of category-mistakes is part of the aim of significance logic in Part II. A satisfactory account of significance claims, in general, will have to do two things: it must fit our intuitions inrespect of the kind of justification we might give to a significance claim; it will also have to explain the distinctive character of category claims in philosophy. It was Ryle (1938) who focussed most attention upon the possibility that many philosophical arguments involve significance claims, based upon points of category-difference; and the discussion of Ryle's views, in outline, on category propositions, below, prepares the way for the examination of contextual significance and category- and type-claims in Part II. Without a systematic treatment of utterance-significance, context, and descriptively absurd sentences, it remained for Ryle an open-question whether a rejection of categorymistakes as absurd or as a priori false constituted genuinely different claims. His classic article on "Categories" (1938) ends with an admission of despair:

And as only collocations of symbols can be asserted to be absurd, or, consequently, denied to be absurd, it follows that category-propositions are semantic propositions. This does not imply that they are of the same type as the propositions of philologists, grammarians, or lexicographers .. Nor does it imply that they can say nothing about 'the nature of things'... If a child's perplexity why the Equator can be crossed but not seen .. is perplexity about the 'nature of things', then certain category-propositions will give the required information about the nature of things. And the same will hold good of less frivoloustype-perplexities. But what are the tests of absurdity? (Ryle, <u>1938</u>, reprinted in Ryle, <u>1971</u>, vol. II, p. 184-all references are to the pagination of Ryle, <u>1971</u>). Semantic Theory, Representations, anomaly and absurdity, (Katz and Ryle):

No matter how elusive the "tests of absurdity", investigating how utterances fail of significance in context can clarify, to some degree, those inferences to and from significance features of sentences to which philosophers (<u>et alia</u>) have appealed in rejecting category-mistaken assertions <u>a priori</u>. Ryle's contrast--in the above quotation--between 'category-propositions' and the propositions of "philologists, grammarians and lexicographers" introduces the grounds for criticising the last account of category-mistakes I shall discuss in this Part of the thesis-namely, Katz' identification of category-mistaken sentences with a species of semantic anomaly.

In legitimising the claim that category-mistakes <u>can</u> be characterised as <u>a priori</u> rejectable, what was a general problem for Ryle--in his appeals to category-propositions--is brought to the fore. For, within the class of <u>a priori</u> rejectable propositions, some distinction has to be drawn between the <u>a priori</u> unacceptability of type-violations and category-mistakes, and that of other propositions which few would want to call <u>category</u>-mistaken. Philosophers are wont to reject <u>a priori</u> such falsehoods, for example, as the following--without claiming them to be, thereby, category-confused:

(22) Temporal precedence is a symmetrical relation.
(23) Some events precede their causes.
(24) There exist two objects a, b such that every property of a is a property of b and vice versa.
(25) In a positively curved space, one and only one line parallel to a given line can be drawn through a point some distance from that line.

The problem of distinguishing between assertions <u>a priori</u> rejectable, because category-absurd, from assertions like (22)-(25)--which conflict with <u>a priori</u> consequences of deeply entrenched scientific, metaphysical or mathematical theories--or that of separating (22)-(25) from assertions logically false or pragmatically stultifying, is only a part of the larger problem of demarcating between the <u>a priori</u> acceptable and <u>a</u> <u>priori</u> rejectable as a whole. It is in terms of this latter task--central to the enterprise of philosophy--that Ryle was inclined to claim, with respect to "category-propositions":

The matter (of doctrines of categories and types) is of some importance, for it is not only the case that category-propositions (namely assertions that terms belong to certain categories or types) are always philosopher's propositions, but, I believe, the converse is also true. (Ryle, 1938, p. 170).

Such a bold claim---which I intend to neither refute nor endorse--stands in stark contrast to the claim that category-mistakes are nothing more than a species of semantic anomaly resulting from confusions over "dictionary-entires" for constituent expressions. This latter claim is articulated in the context of the semantic theory propounded by Katz and Fodor (1963), Katz and Postal (1964), and Katz, alone (1966, 1972). For brevity, I shall refer to these separate accounts of essentially the same theory (embellished and revised) as the K-P-F account. It concludes the critical enterprise of this Part of the thesis to examine the contrast between Ryle's account (and the qualified support it has received from Strawson, 1961) and the K-P-F account (with the independent, but similarly motivated account of Chomsky, 1965). I should point out, immediately, that in this (hypothetical) debate between Ryle and Katz, I shall side with the former. I do so, though, not because Ryle's account of categorymistakes as a species of type-violation is immune from objections; but because, in the argumentation forming the background to Ryle's account, he indicates where the general flaws in the approach espoused by the K-P-F account are to be found. In so doing, he indicates why assertions about categories and types are philosophically important, and why they are not to be "explained away" as disguised descriptions of semantic rules or dictionary definitions.

I shall begin with the technical articulation of the K-P-F view that category-mistakes are a variety of semantic anomaly which are assigned <u>no</u> interpretation by the semantic component of a language. This is clearly a Model III-type account (Introduction, pp. 11-12), in so far as it offers a clear demarcation between significant and nonsignificant <u>sentences</u> (not relativised to context) on the basis of necessary and sufficient conditions (semantic rules) for a sentence to be assigned an interpretation.

The conception of semantic theory upon which the K-P-F account of semantic anomaly is based is outlined, briefly, in the following:

The basic fact that a semantic theory must explain is that a fluent speaker can determine the meaning of a sentence in terms of the meanings of its constituent lexical items. To explain this fact a semantic theory must contain two components: a dictionary of the lexical items of the language, and a system of rules (which we shall call 'projection rules') which operates on full grammatical descriptions of sentences and on dictionary-entries to produce semantic interpretations for every sentence of the language ... The central problem for such a theory is that a dictionary usually supplies more senses for a lexical item than it bears in an occurrence in a given sentence,... Thus the effect of the projection rules must be to select the appropriate sense of each lexical item in a sentence in order to provide the correct readings for each distinct grammatical structure of that sentence. The semantic interpretations ... must account ... for the speaker's ability to understand sentences: they must mark each semantic ambiguity a speaker can detect; they must explain the source of the speaker's intuitions of anomaly when a sentence evokes them; they must suitably relate sentences speakers know to be paraphrases of each other. (Katz and Fodor, <u>1963</u>, pp. 493-4).

Our interest, for the moment, is in the 'dictionary-entry' as an explanatory component of a semantic theory, and its relation to the manner in which projection-rules produce zero readings for semantically anomalous sentences (amongst which category-mistakes, by Katz' own examples, will be located: see, Katz, <u>1966</u>, p. 239). A <u>dictionary-entry</u> for a specific lexical item (word) consists of "a finite set of sequences of symbols, each sequence consisting of an initial subsequence of <u>syntactic markers</u>, followed by a subsequence of <u>semantic markers</u>, then, optionally, a <u>distinguisher</u>, and, finally, a <u>selection restriction</u>" (Katz and Postal, <u>1964</u>, p. 13). What the K-P-F theory proposes can be determined from the following sample dictionary-entry (from Katz and Postal, <u>1964</u>, p. 14, fig. 2.7):



Elements underlined are lexical items; elements not underlined and not in brackets are syntactic markers. These serve to "differentiate senses of a lexical item which differ primarily in their 'parts of speech' role", (e.g. "kill" as a verb or noun) (ibid., p. 13). Elements within parentheses, on descending paths through the tree, are semantic markers which are "the formal elements that a semantic component uses to express general semantic properties" (ibid., p. 14). Elsewhere, Katz explains a semantic marker as a "theoretical construct" which "represents the conceptual elements into which a reading decomposes a sense", (Katz, 1966, p. 155). The jargon of contemporary linguistics is here a little dense -- so we have to add that Katz intends a semantic marker -- e.g. (Physical Object) -- to represent "the class of similar ideas that we as speakers of English have in mind when we distinguish the senses of 'stone', 'man', 'car', etc., from the senses of 'virtue', 'tickle', 'time', etc." (Katz, 1966, p. 177-8; my emphasis). The theory, then, is avowedly conceptualist: semantic markers are the theoretical constructs the semantic theory employs to 'represent' ideas, concepts, senses--which are bona fide constituents of cognition. To complete the picture: a distinguisher (shown in square brackets) serves to "differentiate a lexical item from those closest to it in meaning, so that each distinguisher will be found only once in the dictionary" (Katz and Postal, 1964, p. 14); whilst a selection restriction (angle brackets) is defined as:

A formally expressed (boolean combination) necessary and sufficient condition for a given reading to combine with others ... The selection restriction attached to a reading determines the combinations with the readings of other lexical items into which that reading can enter when a projection-rule is applied. (Katz and Postal, <u>1964</u>, p. 15)

To understand how projection-rules apply to dictionary-entries and the phrase-markers generated by the syntactic component of the language to produce <u>semantic interpretations</u> (readings) for sentences (several, if the sentence, out of context, is ambiguous; none if the sentence is 'anomalous'), we have to wade a little more deeply through the technical jargon of the theory.

Briefly stated, the transformational-generative syntactic component of a language supplies structural descriptions of sentences represented as tree-diagrams. Lexical items are inserted (subject to categorisation features governing the 'part of speech' required) at the terminal nodes of the tree. The lexicon supplies dictionary-entries for each lexical item and the <u>projection-rules</u> amalgamate all the possible elements--along each path of the tree, from the bottom upwards--until the full sentence has received a number of admissable readings. Should any pair of elements, when combined, conflict in respect of selection restrictions (as when \langle (Physical Object) \rangle is combined with a marker requiring \langle (Activity) or (State) \rangle (as in "thinking stone"), the combination is blocked and no derived readings result from that combination of paths through the tree. If no reading at all results from any combinations of paths through the tree, the sentence receives no semantic interpretation and is semantically anomalous (category-mistaken).

An example--paraphrasing that given in Katz and Postal (1964, p. 16) will explain the manner in which selection restrictions (hereafter: "SRs")

are supposed to block anomalous results of combining markers. Consider:

(26) The boy has a green thought. which is assigned the following phrase-marker by the syntactic component:



to which, for each lexical item, the lexicon assigns a dictionary-entry (only such as are permitted by the syntactic markers). For, the item "green" a partial reading may be:

<u>green</u>: \rightarrow Adjective \rightarrow (Descriptive)--> (Colour [Of the color between blue and yellow],.., [Of the colour of growing foliage]), \langle (Visual Percept) or (Physical object) \rangle .

The SR \langle (Visual Percept) or (Physical object) \rangle is construed as confining the adjectival occurrence of <u>green</u> to the paths, combined by projectionrules, which contain nouns modified by the markers (Physical Object) or (Visual Percept). Katz and Postal continue:

If the reading for this nominal head ... lacks both these markers, no combination occurs and there is no <u>derived</u> reading which represents the meaning of the modifer-head constituent in terms of its components ... In cases where syntactically compound expressions are assigned no derived reading, we shall say that the semantic component marks them as 'semantically anomalous'. (ibid.)

So, the nonsignificance of (26) is attributable to a breach of

semantic-rule. One expression occurring in (26) "a green thought" has no meaning, because the reading for the lexical item <u>thought</u> cannot be combined with green without violating SR-attachment.

So far, so good: the theory exhibits the manner in which our linguistic ability to detect the anomalousness of (26) is a function of our knowledge of the meanings of constituent expressions. A judgement that (26) is nonsignificant, thus, is explained as a judgement to the effect that the rules of our language fail to assign a sense to a constituent expression of (26), because that expression fails at some point to meet the conditions for being semantically interpretable--expressed as SRattachments to the readings for its constituent, formative words.

Is this account genuinely explanatory of the nonsignificance of category-mistakes? There are internal difficulties in details of the account--with respect ot its "fit" with the syntactic component. I cannot go into these internecine disputes, here (see, for example: Weinrich, <u>1966</u>; Lakoff, <u>1971</u>). Nonetheless, I shall argue that the account is <u>not</u> adequate--because it leaves crucial questions unanswered. Moreover, by means of a short peroration on the assumptions behind the K-P-F account, and its intended scope, I shall suggest that this approach to category-mistakes relies upon a misconceived picture of sentence-meaning.

First: one question that demands urgent answer is: on what grounds do we determine which SRs attach to which readings for lexical items? In brief: where do SRs come from? In particular, why does <u>green</u> have just $\langle (Visual Percept) \text{ or } (Physical Object) \rangle$ attaching to it; rather than, say, $\langle (Regular Surface) \rangle$ or $\langle (Sense-Datum) \rangle$ or $\langle (Congeries of atomic particles)$

capable of deflecting statistically sufficient radiant energy, of wavelengths from 3900A to 7700A, to stimulate the human retina? Secondly, how are semantic markers determined? In virtue of what analytic (or logical) relationship between semantic markers and SRs do projection rules fail to generate a reading for the combination '(Colour)' + '(Mental), \langle (Action) or (Event) \rangle '?

To the first question: from where do we get SRs? there seem to be two answers available to the K-P-F account:

1) K-P-F could be taken as claiming that, as a matter of linguistic rule, convention or regulatory principle, only lexical items having (Visual Percept) or (Physical Object) attached to their readings can be qualified by "green".

2) As a second possibility, from remarks in Katz (1966):

Category-mistakes ... are due to a conceptual incongruity between the meanings whose combinations are directed by the syntactic structure of these sentences. (pp. 238-9)

and A semantic marker ... <u>represents</u> the class of equivalent ideas (concepts) that we as speakers of English have in mind.. (p. 177--my emphasis).

> we might take K-P-F as claiming that, in virtue of conceptual or ideational conflicts represented by relations between semantic markers, whatever can be conceived to be green is confined to the category of physical objects or percepts.

Neither of these answers are satisfactory. The claim (1) that SRs attach to lexical items as a matter of "rule" or "principle" is empty without a more explanatory account of how such "rules" apply; of what these "principles" are. To say that category-mistakes are semantically anomalous because they violate SR-attachments and that violations of SR-attachments are breaches of semantic rule, is just to mirror the incoherency of

category-mistakes in the incoherencies of SR-attachments--the one needs explanation just as much, and for the same reason, as the others.

The brief response to claim (1), then, should point out that the K-P-F account of semantic interpretation, in terms of dictionary-entries and projection-rules is claimed to give "a full analysis" of the "cognitive meaning" of sentences (Katz and Postal, <u>1964</u>, p. 12). Thus, to know that the SR attached to (Colour) proscribes the markers (Mental State), (Mental Entity) is to know that, as a matter of fact, it is nonsense to ascribe colours to thoughts with literal descriptive force. Knowing this, though, just amounts to knowing that (26) is meaningless; at least so long as we insist upon a <u>literal</u> reading of "green thought". What is lacking is an independent account of those regulatory principles which proscribe certain combinations of SRs and semantic markers. Let us look elsewhere, then, in the K-P-F writings, for such an account.¹⁵

With respect to claim 1), I wish to argue that there is <u>no</u> sense of "linguistic rule" or "regulatory principle" in terms of which a categorymistake---viewed as a violation of SR-attachment---can be said to breach a linguistic rule or principle. At the same time, in respect of claim 2), I shall argue that Katz' appeal to "conceptual incongruity" and to "a semantic marker" representing a "class of equivalent ideas" (above) reveal his committment to a 'representationalist' theory of meaning which relies upon an incoherent notion of 'representation'.

To argue this last requires a brief and necessarily synoptic detour through what Katz claims a semantic theory is a theory <u>of</u>; and through what I believe is misconceived in this claim:

The semantic component interprets underlying phrase markers in terms of meaning. It assigns <u>semantic interpretations</u> to these phrase markers which <u>describe messages</u> that <u>can be</u> <u>communicated</u> in the language... the semantic component provides a <u>representation</u> of that <u>message</u> which actual utterances convey to speakers of the language in normal speech situations. (Katz, <u>1966</u>, p. 151--my emphasis).

This is Katz' general description of what the semantic component of a language does—it provides representations of "messages" that "can be communicated" in terms of "semantic interpretations" which "describe" them. We should add to it, then, Katz' description of what it is for communication to take place in a language, so described:

When successful linguistic communication takes place ... The speaker .. chooses some message he wants to convey to his listeners: some thought he wants them to receive or some command he wants to give ... This message is encoded in the form of a phonetic representation ... by means of the system of linguistic rules with which the speaker is equipped. This encoding then becomes a signal ... picked up by the hearer .. converted into a neural signal from which a phonetic representation equivalent to the one into which the speaker encoded his message is obtained. This representation is decoded into a representation of the same message that the speaker originally chose to convey, by the hearer's equivalent system of linguistic rules. (Katz, <u>1966</u>, pp. 103-4--note: Katz admits that this is no more than a "rough statement").

As a description of what a semantic theory is a theory <u>of</u> (i.e. 'meaning'), the first description is almost hopelessly <u>circular</u>. As a description of 'successful communication' in language, the second is equally hopelessly <u>incoherent</u>. Cutting away the jargon of "markers", "selection restrictions", "projection rules" and "lexicon", the model of linguistic meaning with which the K-P-F account presents us amounts to the following:

I have various unarticulated ideas, thoughts and concepts in mind

(ibid., p. 177). Amongst these, I locate the "message" that I am hungry. Presumably, this (unarticulated?) "message" involves an association of my concept of myself with that of hunger---and, perhaps, the concept of all this going on at present, to distinguish this message from the one about my having been hungry this morning. Having chosen my message, I employ the rules I have learned which enable me to <u>encode</u> this message into a representation of it in sound or writing (all this is subconscious, perhaps). Then, I write my encoded message on a slip of paper and pass this "signal" across the bar to a friend. He scans it, calls into play the rules of his <u>decoding</u> device ("language") and the "<u>same</u> message"--in <u>his</u> conceptual form, now; perhaps associating his idea of me with his idea of present hunger,--leaps forth. We agree to retire to the restaurant.

Admittedly, the reconstruction, above, is of an acknowledged "rough" description of communication in language. What is plainly misconceived, though, is how this can be communication in language at all. It is a misconception of 'representation' to suppose that it is a species of 'encoding' and 'decoding'. It is a misconception of 'meaning' and 'understanding' to compare it to 'sending' and 'receiving' signals. It is a misconception of 'thought' and 'concept' to equate them with preverbal 'messages'.; and it is a misconception of 'language' to suppose its speakers are 'equipped' with rules which are constitutive of their linguistic practices. I shall discuss these four misconceptions, in curn, drawing upon Ryle's more careful discussion (<u>1938</u>), and upon noted criticisms of Katz' semantic theory from Wilson (1967).

If the concept of 'representation' is to bear any explanatory

weight in the account of the relations between language and thought, and language and the world, we have at least to exercise the same care in its use as that exhibited by Wittgenstein in comparing "representational" to "pictorial" form, in the exegesis of the doctrine of showing (discussed above). As described there, a sentence may be said to 'represent' both the thought it expresses and the fact in virtue of which it yields a true or false statement. Great care must be taken, though, to separate at least two senses in which 'representing' in general takes place. A 'representation' can be either iconic or symbolic--this parallels the Wittgensteinian distinction "representational/pictorial". A representation is iconic if it resembles, or shares some features of, what it represents. Save for Wittgenstein's atomistic Picture Theory, iconic representations are usually confined to the plastic arts--landscapes, sculptures, portraits--though they need not be thought to be exclusively visual, since they may include, say, my humming a Bach fugue to identify it to a record-salesman.

The paradigm of <u>symbolic</u> <u>representation</u>, on the other hand, is linguistic--though it extends further to maps, heiroglyphs, graphs, and systems of notation (e.g. music scores). Several things seem essential to symbolic representational systems:

a) Systematic symbolic representation can express both how things are and how they are not (some sentences make false statements, some maps are of fictional countries, some itineraries are of where <u>not</u> to go).

b) Within symbolic representations, two different sorts of activity (at least) are characteristic: representation of <u>items</u> (things, places on a map, numerical magnitudes, sounds), and representation of <u>situations</u> (the fact that aRb, the disposition of troops on a battlefield, the recommendation for the violins to play pianissimo).

c) When a system of symbolic representation is linguistic, several additional features seem essential:

(i) with respect to b), that the system distinguish symbols which can be used to represent items (designators: names, descriptions, demonstratives) from symbols used to represent situations (assertive-, statement-making-, truth-claiming- symbols; i.e. indicative sentences);

(ii) that the system of representation be finitely learnable without being taught (see: Ziff, 1960, p. 35); and that it be effectively intersubjective--it can be used to communicate both about public and private states of affairs;

(iii) that the system allow the potential for indefiniely many new representations for new items and situations (linguistic creativity);

(iv) that, though the system is amenable to description as a <u>rule-governed</u> system, the possibility of free deviations from norms and conventions within the system precludes its being a <u>rule-constituted</u> system (like, say, chess) in which following rules would <u>constitute</u> successfully representing some item or situation symbolically.

(The distinction "rule-governed/rule-constituted" is due to J. Rosenberg (1972, pp. 101-106), from which some of the above features of representational systems have been drawn)

(v) that where two or more systems share the features a)-c), the possibility of 'translating'--correlating system-to-system representations in accordance with agreements in what is represented--is admitted.

(I shall have occasion to discuss these distinctions further in Part II, Section E).

The above seems to me to indicate, roughly, what is necessary for the description of a language to be a systematic symbolically representational corpus of items (inscriptions, sounds, gestures). It includes, in general, the uses of symbols capable of being combined in rule-governed ways, publicly accessible and learnable, to perform actions (promising, stating, commanding, and so on) and express meaning. It should be clear how <u>few</u> of these intrinsic features of a system of representation (usually symbolic, though language has iconic features) are captured by Katz' account of both semantic interpretation and communication. I have not argued for these features' being intrinsic--nonetheless they can serve some heuristic purposes in the following.

The circularity in Katz' account of semantic interpretation is evident from its failure to supply an independent reason for SR-attachment to confine combined lexical items to meaningful readings. An appeal, here, to rules (claim 1)) is of little help--since it has to be conceded ((c), (iv)) that frequently a sentence for which projection rules will provide no context-free reading is readily interpretable in differing contexts. This indicates the sensitivity of sentential significance to context (both 'linguistic' and 'physical'). An appeal to rules, here, as constituting what it is for a sentence to be meaningful in context, ((c)(iv)), would be so profligate as to be beyond formulation -- when so small a contextual change as an agreement to employ a name with non-normal reference can render a category-mistake significant. A special case of exception to such rules are involved in contexts in which significance-claims are made. As has been argued by McCawley (1968) the K-P-F account of semantic anomaly requires that no readings be assigned to the constituent anomalies of:

(27) Max insists that he has green thoughts.(28) Max insists that rocks eat diabetes.

which results in (27) and (28) being marked as <u>synonymous</u> by the K-P-F account (sameness of derived reading). Similarly, then, either:

(29) It is nonsense to speak of eating diabetes.is assigned no reading; or the inference from (29) to:

(30 Diabetes is not the kind of thing which can be eaten. is not one that can be included amongst the <u>distinguishers</u> in the lexicon (in general, 'negative' distinguishers are not admitted).

In contrast to Katz, Ryle has drawn attention to the considerable flexibility required of an account of the "absurdity" of categorymistakes. In Ryle, 1938 (p. 183), he writes:

The operation of extracting the <u>type</u> of a factor (roughly, a 'lexical item') cannot exclude the operation of revealing the liasons of propositions embodying it. In essence, they are one operation ... Now the operation of formulating the liasons of a proposition is just the activity of ratiocination or argumentation ... And this is why philosophising is arguing, and it is just this element of ratiocination which, as a rule, is left out of the latterday definitions of philosophy as 'analysis'.

(By "liasons" Ryle means all the logical relations in which a proposition (sentence-in-use) can stand to other sentences).

Ryle's target, in his last comment, is the positivist's proposal (especially Carnap, <u>1934</u>) to "rationally reconstruct" philosophical concepts in suitably regimented semantic systems. Yet his remarks can be applied equally to the K-P-F account--for they make an equally grandiose claim for their semantic theory:

If there are theoretical constructions in the theory of language that do meet the conditions for the solution to some philosophical problems and if their empirical support in terms of linguistic evidence is strong enough, then these constructions must be an acceptable solution to the philosophical problem. (Katz, 1966, Preface, pp. x-xi)

The arguments already advanced--from both philosophical grounds (the incoherency of the model proposed) and from linguistic grounds (McCawley's counterexamples)--suffice to undermine the appeal of Katz' theory to solving philosophical problems over nonsignificance and category-confusion. Further reflection upon the incoherence of claim 2) suggests reasons why

the appeal cannot be made, because of the misconceptions it fosters.

I claimed that, as explicated, it is misconceived to construe linguistic representation as a species of encoding and decoding; to suppose 'meaning' and 'understanding' are reducible to 'sending' and 'receiving signals'. In general, these are misconceptions which stem from "the archaic notion that dictionary entries ... give the 'meaning' of a word" (Wilson, 1967, p. 62). To conceive systematic representation in language as analogous to 'encoding' messages by assigning dictionary entries to symbols is to confuse 'understanding' with 'translating'. When I send three dots "...", followed by three dashes "---", followed by three dots "...", I communicate my distress and request help, because of a lexicon correlating this grouping of dots and dashes with letters, and a 'projectionrule' which restricts "S.O.S" to distress calls in Morse Code. Both sequences of dots and dashes "...--..." and groupings of letters "S.O.S" are symbolic representations intertranslatable by the lexicon and rules for Morse Code. Both systems, though, are underpinned by the possibility of linguistic acts (of asserting, requesting, reporting) in which symbols can represent requests for help, feelings of distress, beliefs that one is drowning, and so on. The 'meaning' of "S.O.S" is to signal distress only because we can request help by acts of linguistic representation.

The model of linguistic meaning Katz proposes can be compared with that conception of a child learning language as acquiring <u>rules</u> for <u>trans-</u> <u>lating</u> his pre-verbal thoughts, concepts and ideas ("messages") by encoding them in symbolic form and associating dictionary-entries with structural sequences of symbols. This conception ignores the fact that acquisition

of a language is simultaneous with, and concommittant upon, acquiring a whole range of abilities -- to act in varied ways, make gestures, to articulate and structure experiences and sensations--together with forming concepts, introspecting, reasoning, acquiring beliefs, manipulating ideas, and so on. It is manifestly absurd to suppose a child "acquires" the concept of, say, squaring a number, then applies a lexicon to assign appropriate readings to symbols which represent this concept (learning, thereby, not to apply the concept to, say, green apples). Katz may, of course, reply that the K-P-F account of the semantic component is not intended as a genetic account of language acquisition. The reply would miss the point--which is not to oppose the K-P-F view with empirical facts of language acquisition-but to reject the view as an incoherent conception of linguistic meaning. For, the appeal to articulated "messages" (thoughts, concepts) which the semantic component 'encodes' into language is either circular--the "conceptual incongruity" of applying 'square of' to 'green apples' being explained by the violation of SR-attachment, because of the meaninglessness of "the square of a green apple"--or the appeal is incoherent -- construing pre-verbally articulated messages as encoded into linguistic representations which are the articulations of thoughts, facts and so on.

Wilson (1967, pp. 62-3) has opposed the K-P-F account as follows:

(The account) .. rests on the archaic notion that dictionary entries in general give the 'meaning' of a word. .. The assumption here is false ... The point is that the entry (say) for 'gold' does not give us the 'meaning' of the word; it gives us factual information about ... gold .. no entry could give us the meaning: there is no meaning to give, no a priori determined necessary and

sufficient conditions for a given substance's being gold ... There is no sharp line between what properly belongs in a dictionary and what belongs in an encyclopaedia..

I believe this claim (for which Wilson proceeds to argue) is right. I add to it the observation that due attention to the contextual sensitivity of significance (to radical "meaning" changes in contexts) establishes that there may be no limit which can be set a priori to the amount or kind of information (factual or theoretical) which may be necessary to determine the significance of a sentence in context. As remarked in the Introduction (p. 61), this claim is of thematic concern through the thesis--my critique of the K-P-F account of linguistic meaning is underpinned by it. The claim entails, for example, that no hard line may be drawn between "matters of language" and "matters of fact"; that no test for the nonsignificance of assertions is final (though many may strike us as such); that the distinction between 'having a concept' ("knowing the meaning") and 'having true and false factual beliefs about' what I represent by, say, "gold", may not make a difference. Your concept of gold may differ from mine-what we 'mean' by "gold" may differ -- that does not mean your dictionary is any better or worse than mine. It means that you and I have different beliefs about gold. What makes your concept of gold and mine both concepts of gold--which enables us to communicate--is, inter alia, that more of the beliefs you and I are prepared to assert (in sentences containing "gold") are true of gold than of anything else. This corresponds to what Wilson has termed the "Principle of Charity" (1967, p. 64 and passim) as applied to descriptive terms. I would add to it only that I can frequently make assertions involving "gold" and its paronyms, e.g.

(31) Britain is about due for another golden age.

whose significance conflicts categorially with most of what I believe to be 'literally' golden. Here, if the context does not make clear that I am not ascribing lustrous, metallic qualities to periods of time, then what I express is category-mistaken. But charity allows the non-literal significance of clichés to secure successful communication in most contexts.

The Principle of Charity, together with the rejection of <u>a priori</u> "messages" as meanings "in the minds of English speakers" is fatal to the K-P-F view. Not surprisingly, then, Katz has responded to Wilson's challenge:

Wilson's theory faces a variety of objections .. Suppose every member of the English-speaking community .. has the same beliefs about creatures with hearts and creatures with kidneys. Then lexicographers must provide the same entry for 'creature with a heart' and 'creature with kidneys', thereby predicting falsely that these expressions are synonymous. (Katz, 1972, p. 74)

On the contrary, only those who <u>assume</u> that dictionaries give the 'meanings' of words, in a strict sense, would be inclined to mark these expressions synonymous because they are true of the same things. The test for my having the same concept of 'creature with a heart' as you have of 'creature with kidneys' is not solely what we believe true of <u>only</u> those creatures--but what we are prepared to assert significantly (truly or falsely) of them. Amongst such beliefs, one may be that believing every creature has a heart has a heart, requires no empirical confirmation; whereas believing that every creature with a heart has kidneys, does.

A counterexample Katz offers to Wilson's account is:

(32) There is something about which no-one has at present any factual beliefs.

of which Katz claims the extension of "something about which no-one has any factual beliefs" must be both non-null (since a concept is the totality of my beliefs about items falling under it (Wilson, <u>1967</u>, p. 64), one of which is (32)), and yet must be null (because of what falls under it no-one at present has any concept). The contradiction is only apparent. To <u>have</u> a belief that there is something such that \emptyset , is often (but not always) to be able to produce some instance of \emptyset . I believe there are true but unprovable propositions of arithmetic (Gödel proved it). That does not mean my concept of "true but unprovable proposition" is the same as my conception of "Bew_n(\bar{n})"—a schematic instance—since what I believe with respect to each is widely different.

Finally, Katz objects to Wilson's description of a "bare minimum" of factual beliefs necessary to "tune-in on conversations about gold" (Wilson, 1967, p. 64):

Some sense must be given to Wilson's notion of 'bare minimum', since otherwise the theory runs the danger of making all beliefs irrefutable. (Katz, <u>1972</u>, p. 75)

If we restrict the 'minimum' to beliefs that are "statistically frequent enough" amongst speakers, Katz argues, then:

... clear cut contingent statements are <u>a priori</u>. For, suppose B is the set of factual-beliefs in the tune-in entry for the word W ... Each member of B expresses a necessary condition for the application of W ... Each of these beliefs has the form "W's are P" and so one would expect that .. they could be refuted ... However, an object that is not P will not be tuned-in on in referential uses of ..W. (ibid., p. 74)

Notwithstanding that, elsewhere, Wilson has explicated the notion of a

'bare minimum' of 'weighted' beliefs, (Wilson, 1959, pp. 522-539), the counter-argument from Katz contains a false premise. For, if to have a concept of 'horse' is to have various beliefs about horses, it is not necessary that each of these beliefs about horses be necessarily true of horses. Some may be--if you assert horses to be 'cold-blooded, scaly creatures', I may question whether you are talking about horses--other beliefs are not. I believe the four horsemen of the Apocalypse rode horses, but I do not believe necessarily there are any such horses. What is necessary is only that sufficient of what I believe about horses should be true of them--to avoid ending-up referring to something else, or not referring at all, when I use "horse". None of my beliefs need be necessarily true of all and only horses. Katz, on the other hand, is committed on his own theory to maintaining that it is analytic, in a strong sense, that horses are physical objects (which, I presume, is a SR for the noun horse). Then, I am breaking a linguistic rule in denying that Pegasus is a physical object--whereas it seems clear to me that I am not. I simply am not.

In sum, the K-P-F view demands too much of a semantic theory, and, thus, ends up with an account of very little. The description of the semantic component--comprised of lexicon, projection-rules and readings-provides us with a system of representation--call it "Semantic Markerese"--which is translatable into a far richer system of representation which is the English language. I do not say "<u>inter</u>-translatable", because of the noted counterexamples. A request for a philosophical account of linguistic representation and significance, however, is not a request for a

translation-device between English and Semantic Markerese. It may be essential to representational systems (I am not claiming language is just a representational system) that expressions in differing systems be intertranslatable when applicable to similar subject-matters. I can give you a list of instructions to finding my house; equivalently, I can draw you a map. Translatability may be <u>one</u> test for something to be a representational system (and not, say, free associations of symbols and sounds). That is not all that can be expected, however, of an account of meaning and communication. What is required primarily are answers to the questions: how is it possible to express thought in language?, how is successful communication of facts possible?, In virtue of what features of assertions do some succeed and others fail to be significant?

These are questions Strawson has claimed (<u>1970</u>, pp. 188-9) are what lie behind Ryle's attempt to explain what makes category-mistaken sentences absurd. A non-sympathetic reading of Ryle's 1938-discussion of categorymistakes would point out, soon enough, that Ryle's purported test for category-mistakenness through type-violation is subject to immediate refutation by counter-example. Ryle gives the test (p. 181) in terms of "proposition-factors" (expressions in sentences) which "collect together whatever is signified by any expression ..., which can complement a gapsign in some sentence-frame (open-sentence) or other":

Two proposition-factors are of different categories or types, if there are sentence-frames such that when expressions for those factors are imported as alternative complements to the same gap-signs the resultant sentences are significant in the one case and absurd in the other. (Ryle, 1938, p. 181)

As J.J.C. Smart (<u>1953</u>, pp. 227-8) has pointed out, almost any two expressions differ in type or category by this test. For example, the sentenceframe:

(33) The seat of the x is hard

is significant if the factor "chair" is imported; not if the factor "bed" is imported; whereas, if beds, chairs and other items of furniture do not belong in the same type, there may well be as many categories as things.

Ryle accepted, and argued for, the conclusion that, in terms of his account, there would be a great many more 'types' than philosophers like Aristotle or Kant have led us to believe, in formulating doctrines of categories. His remarks, elsewhere, intimate that he is suggesting far more than a simple (and ultimately unsatisfactory) test for typesameness or difference. I quote at length from Dilemmas:

The truth is that there are not just two or just ten different logical <u>métiers</u> open to the terms or concepts we employ in ordinary or technical discourse, there are indefinitely many such different <u>métiers</u> and indefinitely many dimensions of these differences.

I adduced the six Bridge terms 'singleton', 'trump', 'vulnerable', 'slam', 'finesse', and 'revoke' as terms none of which will go into any one of Aristotle's ten pigeon-holes ... not one of them is, in an enlarged sense of "category", of the same category with any of the other five. We can ask whether a card is a diamond or a spade ...; but not whether a card is a singleton or trump; not whether a game ended in a slam or a revoke; not whether a pair of players is vulnerable or a finesse The same is true of most though naturally not all of the terms that one might pick at random (Ryle, 1954, pp. 10-11)

The point of Ryle's Bridge-analogy is not to give up the possibility of accounts of types; but to redirect the question: what are the basic types of things? to consideration of the logical and significance relations which hold in virtue of which type-violations are nonsignificant. (His word is "type-absurd"). This much is clear from a separate description in Philosophical Arguments:

In fact the distinction between the logical types of ideas is identical with the discrimination between the logical forms of propositions from which the ideas are abstractions. If one proposition has factors of different types from those of another proposition, those propositions are different logical forms and have different logical powers ... There are as many types of terms as there are forms of propositions, just as there are as many uphill as downhill slopes. (Ryle, <u>1945</u>, p. 9)

In other words, there is not, on Ryle's account, one set of types (one heirarchy) or one list of categories to which we can refer to eliminate category-mistakes. This much has been made clear already in my earlier arguments that a theory of category-mistakes should not (and need not) appeal to a specific metaphysical doctrine of categories. This much also clarifies why Smart's counter-example misses the point. For, we should not expect rigid criteria distinguishing significant assertions from category-mistaken, nonsignificant assertions (a Model-III type account); nor should we seek to abandon appeals to the nonsignificance of typeviolations in favour of treating category-mistakes as false (Model Itype account). The examination of what logical features and significance features of sentences in context pertain to the appraisal of assertions as category-mistaken, or conflicting in type, will be at least as complicated as an analysis of the significance of sentences in context. We need not agree with Ryle that all such features are "logical", or have to do with "logical form"--for his appeal to this notion is too vague to

bear explanatory weight. Moreover, we need not expect that an exact notion of "form"---understood as pertaining only to the symbolisms of classical first-order languages--will prove suitable for Ryle's purposes. For, classical systems have primarily been used to formalise extensional features of sentences (logical truth, consistency, tautologicality), and we have no reason to believe that significance is an extensional notion. (yet, there is little reason to believe it is not). On the other hand, formal logic does provide perspicuous notation and a rich diversity of semantic concepts whose use may clarify what Ryle meant by identifying "logical types" with the "logical powers" of propositions. At outset, one can presume that "logical powers" is intended to include kinds of inference, deductive relations, presuppositions and entailments which support significance-claims. This, at least, is the assumption of Part II of the thesis, where an approach is made towards just such an account of the logic underpinning significance claims. To the extent that Ryle's account draws attention to the complexity and variability of procedures which classify and sort items we talk about, according to what we significantly assert of them, then his concentration upon philosophical analyses which sought to reveal the category absurdities involved in some philosophical theses, reveals the need for systematic investigation of significance claims. This last provides a motivation for the significance logics of Part II.

PART II: TYPES AND SIGNIFICANCE

Section A: Preliminaries.

Natural languages may contain expressions which, though grammatically impeccable, are nonsignificant. This conclusion of Part I, together with the arguments against subsuming cases of significance failure under other varieties of linguistic anomaly--necessary falsehood, violation of semantic rule, ill-formedness or self-contradiction--impresses upon us, more than anything else, the need for careful examination of the following questions:

(i) When a significance claim is made (i.e. a judgement that an utterance is or is not significant), what is being said to be "significant" or otherwise? Recall from I, Sect. C, Quine's accusation of use/mention confusions in Russell's exposition of type theory; and from I, Sect. D, Black's crucial objection to the notion of "types" that, if type differences are differences between non-linguistic entities, it becomes impossible to assert significantly that two things differ in type. From these criticisms, it is evident that it can make a great deal of difference whether it is <u>sentences</u> (which are linguistic) which are said to be "significant", or <u>propositions</u> (which, one supposes, are extra-linguistic) or <u>statements</u> (which are not clearly either), or <u>utterances</u> (which are particular sentence-tokens). Different answers to the question: "To what do we attribute significance?" will yield different accounts of the nonsignificance of type-violations.

(ii) Upon what does significance depend? One accepts, intuitively, that, for a speaker to judge whether what another has said is significant, he must 'understand' what was said. Ordinarily, there is an easy transition from "A understands what S is saying" to "A knows what S means" to "what A knows when he understands S is the meaning of what S is saying". This transition leads immediately into the traditional problem of meaning: what is it for what S is saying to mean something?, or, in general, what is it for a speaker S to mean something in what he says? To answer the first question is to give a semantic theory for the language S and A speak. To answer the second will also involve giving a semantic theory, but it may also involve pragmatic features of the speech-act S is performing. So, a theory of significance must, at least, involve a semantic theory. Whether it has to involve features of language other than syntax and semantics is an issue taken up below.

Recall that our answer to (ii) will, in general, depend upon how a semantic theory is involved in a theory of significance. Recall, also, from the Introduction (p. 2), the methodological requirement that an account of the nonsignificance of type-violations meet the condition of 'explanatory relevance'. Thus, we will need to examine how the nonsignificance of type-violations is related to other varieties of linguistic anomaly. In this respect, whether a semantic theory can offer explanations for the diverse ways in which language can go wrong constitutes a criterion of adequacy for the theory. In particular, with respect to theories of significance, an account which simply isolated type-violations as of a unique kind of non-significance, distinct from other kinds of anomalous

utterance, would be inadequate, unless it could offer sound reasons for treating these anomalies as unique. I shall argue below, on the contrary, that there is no good reason for isolating type-violations in this way; that it is possible to extend the logics of contextual significance--developed below for type-violations and category mistakes--in natural ways, to encompass a number of linguistic anomalies traditionally regarded as semantic in nature. I shall regard it as sufficient, in so doing, to have indicated how the account of the nonsignificance of type-violations fits naturally into other accounts--available in the current literature on formal semantics--for these varieties of anomaly grouped under the classification "semantically unsuccessful" in the diagram II(i), on p.351. For the moment, I shall not discuss this diagram in any detail, satisfying myself that it may serve, with the examples which conclude this section, an illustrative purpose.

I shall assume--in relation to the diagram--that we have already a satisfactory account of the syntactic division (nodes (1) and (2)) between grammatical and ungrammatical (well- and ill-formed) sentences. It has been concluded already, in Part I, Sect. D, that, though Russellian typetheory is customarily formulated, for the formal languages of classical logic, in the syntactic part of the language--as restrictions on the formation-rules defining well-formed expressions--there are persuasive arguments against adopting this approach in treating the nonsignificance of type-violations and category mistakes in natural languages. This conclusion is reflected in the diagram in locating "type-violations" under node (3) which classifies non-syntactic (i.e. semantic and pragmatic)

anomalies.

A second assumption I make--perhaps more controversially--is that an adequate formal semantic theory is available for the description of those semantic properties falling under node (4)--whether it involves a Lewis- or Cresswell-type 'possible-world' semantics for categorial languages, a Montaguetype set-theoretic semantics, or an account on the lines of Wilson's general semantics. There remains, therefore, the question of describing the semantic properties falling under node (3), and of the adequacy of theories of contextual significance to this task. This question of adequacy will require a more careful formulation in the concluding section. For, in the light of arguments based upon the formal developments below, it becomes unclear whether the traditional distinction between semantics and pragmatics--as defining the subject matters of distinct investigations-remains tenable.² It becomes an issue, then, whether there is any aspect of meaning or interpretation which belongs wholly in the domain of semantics; for pragmatic features of (linguistic and non-linguistic) context and speech-act seem to be required for the description of all the linguistic phenomena involved in this investigation. Discussion of this issue is deferred, naturally, to the conclusion.

To conclude this preliminary section, a list of examples accompanying diagram II(i) will illustrate the kind of anomaly represented at each node. Intuitively, there is reason to suppose the lower the rank of the anomaly on the tree-diagram, the greater the likelihood its explanation will involve pragmatic
Examples:

- (5.1) This statement is false
- (5.2) Facts change colour
- (6.1) The brother of Henry VIII's only son was bald.
- (6.2) Pythagoras discovered the rational square root of two.
- (6.3) a) I promise hereby not to make this promise (performative antinomy)
 - b) I hereby state that you believe to be false what I hereby state (declarative antinomy).

Diagram II(i):



features of context and speech-act. On the basis of arguments in I, Section D, and below, however, I shall conclude that there is no <u>least</u> rank (between (1) and (6)) at which contextual features become irrelevant to determining what anomaly is involved. In this respect, the claim for the priority of contextual significance over context free 'semantic' meaning is substantiated.

Motivation of Part II

At this point, it is necessary to pull together in summary form many of the disparate themes which have been argued in the thesis thus far. For my claim is that utterance-significance is an irreducible dimension amenable to logical treatment, not adequately explainable in terms of traditional two-valued truth-functional logic, on the one hand, nor in terms of grammaticality and well-formedness on the other. My claim is also that utterance-significance is not reducible to context-independent semantic representation, nor is it adequately captured by descriptions of rule-constituted symbolic systems. Thirdly, my claim is that appraisal of significance is just as much a factual, non- a priori, non conventional activity--strongly tied to contextual features of the speech-acts of assertion and statement-making--as are most empirical enquiries (which, nonetheless, themselves have non-factual, a priori and conventional elements). Thus, my third claim is that it is philosophically misconceived to attempt either to set a priori limits to what we can significantly assert (as do a priori theories of categories); or to set limits a priori to the amount or kind of information necessary to support a significanceclaim. In sum, then, viewed <u>negatively</u>, my claims amount to a denial that <u>a priori</u> solutions to philosophical problems derived from facts about "language" or about "linguistic usage" can be non-trivially separated from ongoing philosophical investigation of those problems--at least, to the extent that appeals to the <u>nonsignificance</u> of certain philosophical assertions, on grounds of category, are intended to dispense with the need to consider those assertions further. It does <u>not</u> follow that <u>no</u> philosophical assertions are nonsignificant; only that appraising the significance or otherwise of philosophical claims (or any claim, for that matter) is likely to be no less complex than the enterprise of philosophy, itself, in so far as that enterprise is, at least in part, one of <u>under-</u> standing those claims.

In one sense, viewed negatively, these claims are trivial: is not any rejection of what another says as nonsignificant simply a declaration that one has failed to <u>understand</u> what was said? Viewed <u>positively</u>, however, the claims have non-trivial consequences. The aim is to provide a working, formal semantic framework in which the <u>logical</u> relationships between the significance-features of utterances, their contexts of assertion (linguistic and non-linguistic) and their success or failure to yield true or false statements are displayed. It is in the detailed <u>application</u> of that framework that the rejection of an assertion as category-mistaken, hence nonsignificant, is clarified as depending upon those logical relationships displayed and systematised within the framework.

To be sure, it is not claimed, here, and certainly not shown, that

detailed applications of the framework will succeed in every case; nor even that important features properly belonging to significance and statement-capability have not been omitted entirely from consideration. The full development of the eventual semantic framework for CS-2--built upon CS-1--is seriously incomplete in ways that have to be brought to attention in the concluding discussion. CS-2 is <u>not</u>, itself, fully articulated--deliberately so--in order that less technical discussions of the kinds of significance-failure and linguistic anomaly not discussed hitherto can suggest avenues along which the logic may be given fuller articulation.

I, Section D concluded that nonsignificance is a further logical dimension not reducible to falsehood, ill-formedness or ungrammaticality, nor context-independent semantic anomaly (violation of lexical-rules). The demand for a separate treatment--in significance logic--of contextually sensitive significance-claims follows from arguments for this irreducibility; together with the discursive consequences of II, Section B's separation of acts of uttering sentences expressing significant content in context from illocutionary acts of stating, commanding, promising, etc. By noticing how significance-failures differ from truthvalued and ungrammatical sentences, one notices at the same time the point of the dichotomy "significant/norsignificant", the need for it, and some of the philosophical and logical uses to which it may be put.

Crucial in this last respect, as evidenced by the extended discussion of the SNeg-argument in I, D, is the behaviour of classical negation in relation to nonsignificance. Unlike truth-valued assertions

(but <u>like</u> ungrammatical assertions), the classical negation of a nonsignificant assertion is nonsignificant. Equally crucial to the avoidance of inconsistency in the relational semantics described for CL is the recognition that classical identification of sentence-negation with Boolean complementation is not <u>essential</u> to the claim of a truth-functional operator to <u>represent</u> negation. Indefinitely many operators, it is argued in Π, C , would meet intuitive criteria upon a negation-operator (defined by strongly regular matrices), yet diverge in respect of bivalency and excluded middle. Together, these arguments support a concept of negation as a <u>logical determinable</u> under which several determinate senses may be represented in distinct systems.

The consequence to be drawn from these arguments--generalised to apply to <u>every</u> logical notion amenable to formal investigation: entailment, equivalence, disjunction, quantification, possibility, incompatibility, validity, inference,...-is that each can be regarded as a <u>determinable</u> under which distinct and mutually incompatible <u>determinants</u> can be investigated in separate, but not necessarily competing formal logics.

Such an approach immediately subverts appeals--based upon vestiges of the "ideal language" view, perhaps--which seek to reclassify distinctions not traditionally included in formal logic (e.g. significance/nonsignificance) in traditional terms; on the grounds that classical operators--negation, conjunction, quantification, and so on--require revision to accommodate such distinctions. For, the determinability of 'not' between various alternative determinates shows up strikingly in our ordinary, non-formal discourse when several logical determinables are

combined. Consider for example, "cannot"--which combines negation and modality, as it may appear in:

- (1) a) Proper inclusion, in set theory (a C b), <u>cannot</u> be a symmetrical relation.
 - b) A particle cannot travel faster than light.
 - c) A tiger cannot have a spotted coat.
 - d) Redness cannot taste sweet.
 - e) A thought cannot be green.

The logical impossibility ("necessarily <u>not</u>") involved in (1) a) contrasts with strong physical impossibility ("<u>not</u> by current physical theory") in b) and weak physical impossibility ("<u>not</u> by normal taxonomic principles") in c). All three contrasts with d)-e), where analysis of the nonsignificance of qu(Redness tastes sweet), qu(Thoughts of grass are green) may fix the sense of the determinable "<u>not</u> significantly said to be" relative to appropriate modifiers.

Often enough, though, speakers may assert (1) d) and e) as a determinable without intending, or being prepared, to go on to assert some particular determinate, e.g. "not the kind of thing which..." or "not of the <u>type</u> of which ... is predicable". Compare, for example, asserting qu(The assailant was <u>not</u> tall) where the speaker is not prepared to go on to specify a determinate height; with qu(The number two is <u>not</u> an object), where the speaker is not preapred to go on to specify a determinate sort of type. The examples support the conclusion, <u>not</u> that there are many different "kinds" of negation, <u>qua</u> logical determinable, nor that "not" is either ambiguous or vague amongst determinates (c.f. Russell's claim, discussed in I,C, for the systematic ambiguity of negation across types). Rather they seek to illustrate that what is ordinarily involved in <u>negating</u> a sentence is not automatically explicated in full by its representation in a two-valued matrix; but requires additional investigation in relation to the determinables with which it may appear combined. As much has been established already, on linguistic grounds, in Klima's excellent survey of the uses of negation in English by means of the particles "un-", "ill-", "a-", "dis-", "non-" and so on (see: E.S. Klima, "Negation in English", in Fodor and Katz, <u>1964</u>, pp. 246-323). The conclusion, in any case, effectively lays the ghost of the SNeg-argument for its failure to make fine enough discriminations between denials of significance, simple denials, denials of possibility,...amongst many species of denial.

Symmetry with respect to negation is one feature nonsignificance shares with ungrammaticality. This should not incline us, though, to look anew at <u>grammatical</u> criteria for significance-failure. As was argued in I, D, the appeal to grammaticality is simply different from an appeal to significance. For, we want to set aside ungrammatical sentences from <u>all</u> contexts of assertion, all uses of unquoted strings violating rules of syntax. That is, traditional grammar, at least as understood by linguists like Lyons (<u>1968</u>, p. 424), possesses four features in respect of which grammaticality and significance diverge:

(i) context independence.
(ii) language relativity.
(iii) homogeneity.
(iv) recursive application. (c.f. Introduction, p. 9)

By (i), the same English sentence-type cannot be grammatical in one

context and ungrammatical in another. Paradigmatically, though, significance can vary across contexts--compare qu(Virginia is left-handed) asserted of a person in one context, with its assertion of a U.S. state in another. In respect of (ii), that "grammatical" unlike "significant" is relative to permanent features of the syntax of a particular language is essential, for example, to the claims of linguists to compare recursive grammars of distinct languages. In contrast, in some contexts, qu(My thought is green), qu(Ma pensee est berte) and qu(Mein Gedanke ist grun) share the same failure of significance. Similarly, by the Principle of Referentiality of I, Section D (p.288), there is no grammatical counterpart to context dependent nonsignificance, when significancefailure depends upon referential features; e.g. qu(That is happy) is nonsignificant in a context in which qu(that) refers, apodeictically, to a house-brick, not so in a context where that is a person or dog. Few would be prepared to claim that an ungrammatical sentence is rectified by a change in the circumstantial reference of an expression.

More importantly, ungrammatical sentences lack the heterogeneity of significance-failures. By this I mean that unquoted ungrammatical strings are homogeneously ungrammatical even when compounded with grammatical clauses or sentences. Given, say, that qu(if have I were) is ungrammatical, we cannot form therefrom a grammatical sentence by composition, as in qu(John mistakenly believed that if have I were) or even qu(That if have I were cannot be said). An ungrammatical string, that is, does not even provide the basis for a mistaken belief or warranted denial. When S is ungrammatical, f(S) is ungrammatical <u>unless</u> f sets S within quotation-marks: f(qu(S)). In contrast, many significant utterances <u>can</u> be formed as compounds whose components are <u>unquoted</u> nonsignificant sentences.

For example, certain philosophers have believed (or have been <u>thought</u> to believe) that the Absolute is one, that the mind is a sort of theatre, that virtue is triangular, that the mind is located near the pineal gland. It is certainly not satisfactory to reconstrue reports of arguably nonsignificant beliefs as "tacitly" quoted. Pythagoras may have mistakenly believed it profound to assert triangularity of virtue. It is logically hopeless, nonetheless, to try to paraphrase a report of Pythagoras' expression of his belief as of the form:

(2) Pythagoras mistakenly thought qu(Virtue is triangular). The inadequacy of the paraphrase derives from standard objections to the analysis of intensional (and intentional) contexts as quotational (see: Church, <u>1950</u>). Whatever Pythagoras' thought in relation to virtue was, it was a thought <u>about</u> virtue and not about the English word qu(Virtue). Even if, like Wittgenstein (<u>Tractatus</u>, 5.542), we insist that whatever the constituents of a thought or belief are, they stand in relations to that of which they are thoughts or beliefs, which are analogous to (or even identical to) the relations between words (sentences) and things; thoughts and beliefs, nevertheless, are not <u>in</u> words, nor <u>in</u> sentences---and it is probably category-mistaken to attempt an analysis in terms of quotation which suggests that they are.

Finally, some of the most intriguing discursive consequences of investigating significance-features--as distinct from grammatical features--

concern primarily this problem of the significance of intensional compounds with nonsignificant components. For example, if it is plausible to maintain that one basis for X's lunacy is the truth of (3):

(3) X believes the prime numbers are persecuting him. then (3)'s truth demands its significance. Should we infer from (3)'s truth, however, that X has a belief that prime numbers persecute him? Construed literally, is it proper to ascribe such a belief to X? In general, can anyone be said to have formed such a judgement when, ceteris paribus, the articulation of it is nonsignificant? (c.f. I, Appendix A: where Wittgenstein criticizes Russell that his theory of judgement does not preclude judging type-nonsense). I would claim, on independent grounds, that a better description of the example would involve ascribing to X a false (second-order) belief about the subject-matter and content of his first-order belief. This follows from my interpreting "nonsignificance" as failure to express any propositional content which could yield a true or false statement in the context (it does not follow that significance-failures do not express any content--their being subject to non-literal construal requires that they do). On such grounds, the impossibility of believing or judging what is nonsignificant is a consequence, not of some psychological limit to what can be thought, but of the conditions for having made a correct introspective identification of the thoughts and beliefs one has.

Controversial as it may seem, thus, I am denying the view that each individual has privileged introspective access to the subject-matter and content of his own judgements and beliefs. If every attempt X makes to

state his belief fails through the nonsignificance of what he says, and if additional sentences he utters to support his belief (3) are nonsignificant--e.g. that the number 17 has homicidal intentions towards him or that 23 has threatened him with a knife--then it is precisely the ground of our denying X's capacity to identify at the second-order the source and content of his conscious first-order beliefs, fears, thoughts and judgements that supports our diagnosis of his lunacy.

Less controversially, perhaps, denying the strong form of the doctrine of privileged introspective access--either in the form that, in principle, only I can decide upon the content of my conscious mental processes; or in the form that all first-person reports of sensations, thoughts, or judgements are incorrigible (in principle not revisable by another party) -- amounts only to the recognition that, ordinarily, I can be mistaken in identifying what I believe and what my belief is about. For example, for many years, I believed the Hesperides were a group of islands (similar to the Hebrides). Thus, in thinking about the Hesperides, many of the claims I was prepared to assert of them were not even significantly predicable of those mymphs whose task was to guard the fruits of the Garden of Gaia. In such a case, it is less misleading to construe my beliefs (second-order) about what my thoughts of the Hesperides were about as false, than it is to maintain that I was thinking the literally meaningless thought that the rocky coasts of the Hesperides were a danger to shipping.

In sum, then, these properties of significance-features and of inferences based upon significance-claims reveal on their own the inadequacy of attempts which reduce nonsignificance to illformedness or ungrammaticality. In addition, they endorse the arguments of I, Section D, and II, Section B, to favour regarding the truth or falsity of a significance claim as highly sensitive to contextual features of the illocution being appraised. A significance-failure, in brief, is the failure of a speaker's utterance to convey content or information which can be publicly identified as having made a true or false statement in the context of utterance. Nevertheless, though, it does not follow that significance-failures convey <u>no</u> information and, thereby, support <u>no</u> inferences to what a speaker is judging or believing. Fairy-stories, myths, metaphors and lunatic ravings may all be occasions when, by suitable modification to the transferences of literal sense--from appropriate to inappropriate expression of it--or to the contextual entailments and presuppositions of <u>literal</u> sense--genuine content is conveyed by overtly nonsignificant sentences.

It should not be thought, however, that a significance logic can provide an <u>a priori</u> recipe for detecting the non-literal significance of assertions in fairy-stories or myth, where it may <u>not</u> be categorymistaken, for example, for characters to converse with broomsticks ("Sorceror's Apprentice") or for sufficiently well-motivated thinking to cause unassisted human flight ("Peter Pan"). Nor need significance logic provide anything but a schematism for the differences between a 'fictional' statement (which need not depend, for its significance, upon associated literally meaningful assertions) and a metaphor (which characteristically, but not always, <u>does</u> convey significant content by

transference of sense from recognised literally meaningful assertions). In metaphor, for example, the literal nonsignificance of what is expressed is often sufficient to force a contextual reinterpretation which transfers sense to the metaphor from different, though contextually related expressions. The frame on my car suffers from metal fatigue, and the cruel sea is a harsh mistress--though to prescribe a sleeping draught for my car's frame, or to advise sailors to offer soothing endearments is to reveal a literal-minded idiocy which has ignored the surrounding linguistic context of anthropomorphism.

An intuitively adequate account of logical relationships between what is expressed by even an obvious metaphor and the literal sense from which it is transferred, expressed wholly in formal terms, will be too complex and probably far more unwieldy in paraphrse than what a critic of language infers without the aid of formal semantic structures. For example, a dead metaphor like qu(This length of metal is fatigued) presumably now has quantitatively measurable properties associated with it, by the metallurgist, which preclude the significance of its originally anthropomorphic connotations. The point of allowing the metaphorical reconstrual of literally nonsignificant sentences in context, however, is not to advocate a semantics for significance logic which can capture the highly fluid processes involved in the transference of sense from, say, a dead metaphor like "metal fatigue" to its currently observational significance in metallurgy. The point is only to emphasise that to deny that nonsignificant utterances to convey some information in diverse contexts is to risk treating significance at a level of abstraction

remote from that at which we appraise the significance of one another's assertions in terms of the statements they appear to make. It is therefore to recommend that a logical theory for significance claims be devised in such a way that it leaves room for a <u>measure</u> of the information an overtly nonsignificant utterance can convey in some contexts; rather than fore-closing upon the problematic concept of 'metaphor' by assigning nullcontent to literally nonsignificant utterances in context. A logical theory which goes some way towards meeting this last requirement can be outlined as follows.

Synopsis of the aims and arguments of Part II (B-E):

1) The first step in developing a logical framework within which to embed an account of category-mistakes is to address the questions posed at the start of this section: what are the proper bearers of significance and upon what does their significance or nonsignificance depend? To this end, I argue in the next section (Section B) for a primary distinction between sentence-tokens (utterances) functioning within a context to perform a speech act, and sentence-types--an abstraction from classes of tokens similar in respect of grammatical form, and uniquely identifiable in terms of syntactic and morphophonemic features. To make this distinction I have found it necessary to reappraise the customary account of type and token in conjunction with a critique of the traditional distinction between using an expression and mentioning it for the purpose of ascribing properties to it. The most familiar device for the mention of an expression-placing the expression within quote-marks or in a distinctive type-face-fails to differentiate between particular token (uttered or written in a context) and expression-type, qua class of tokens. In addition, there are logical problems which result from the

unrestricted use of quotation within a formal language. In particular, it has been argued by Tarski (1936, transl. 1956, pp. 161-2) that, unless we regard expressions which themselves mention (refer to) expressions of a formal language, by means of quotation, as belonging properly to a distinct meta-language--in terms of which the formal "object" language is described--then inconsistencies analogous to the antinomy of the Liar (see I, Section C, p. 173) result.

The logical frameworks--CL, CS-1, and CS-2--which I develop for the explanation of category-mistaken assertions require use of a quotationoperator within the formal languages themselves. For this reason--not wishing to interrupt the exegesis of the significance logics with detailed discussion of the quotation-operator--I have appended a brief account of the type/token and use/mention distinctions together with an analysis of quotation and defence of quotation-operators against Tarski's general criticisms. This comprises Appendix (B), below (pp. i-xliii).

For several arguments of Appendix (B) and for much of the discussion of <u>kinds</u> of quotation-operator (pp. xi-xvii), I have relied upon a similarly motivated article by R. Routley and L. Goddard (<u>1966</u>, pp. 1-49). I record here my debt to their work. In addition, I argue in (B) - subsection IV, p. xxix - that an existing semantic consistency theorem for a formal language containing a quotation-operator can be adapted to fit the account of quotation I give. This theorem repudiates the general claim (sometimes wrongly attributed to Tarski) that languages admitting quotation of their own syntax are necessarily inconsistent. The theorem in question is due to D.L. Grover (<u>1973</u>, pp. 108-110); though I have not <u>demonstrated</u> that Grover's theorem is reproducible for the semantic part of CL, CS-1 and CS-2. I have therefore to report this proof as an open problem for my approach. Nonetheless, I record my debt to Grover's article also at this point.

2) Drawing upon the distinctions drawn in Appendix (B), I seek to show in Section B that significance is best construed as a property of a speaker's utterance of a sentence-token, in a context, to perform an illocutionary act (stating, promising, warning, commanding, and so on). I do not argue in general that utterance-significance is to be equated with sentence-meaning; partly because the concept of 'meaning' is subject to such ambiguities as have arisen from the diverse approaches made to the problem of meaning ('meaning' as 'intending', 'meaning' as 'representing', 'meaning' as 'denoting') that it is no longer clear whether the concept can be identified with a well-defined notion. Nevertheless, it is of thematic concern for my thesis that an articulation of how utterances succeed or fail to be significant in context should contribute to the general analysis of meaning and communication in language. My preferrence for the term "significance" at this point indicates no more than a desire to avoid the unhelpful connotations of "meaning".

The dichotomy 'significant/nonsignificant' applies, thus, to sentences uttered in context to perform illocutionary acts. In short, as I argue in Section B, significance is a property of a sentence (token) which is determined by the interaction of what a speaker says with the context (linguistic and physical) in which he says it. Accordingly, I provide in Section B an analysis of speech acts (of assertion, or statementmaking, in particular)--derived from Austin (<u>1962</u>) and Searle (<u>1969</u>)-which identifies three characteristic aspects of the speech-act:

- (i) utterance of a token (in a context),
- (ii) expression of content (of a proposition)
- (iii) performance of an illocutionary act (e.g. making a statement).

It would seem that by this analysis I am committed to the existence of sentence-tokens, statements and propositions. Few would cavil at countenancing sentences--for they are the basic units of linguistic investigation. Tokens are spatio-temporally locatable items with phonological or ideographic features. Types can best be regarded as abstractions from these--classes of similar tokens. A <u>prima facie</u> reason, thus, for ascribing significance to sentence-tokens rather than types is simply that, for a sentence to assert a truth or falsity, significance is a precondition; yet different utterances of the same sentencetype may, at some times, yield a truth, at others a falsehood, and may sometimes fail to yield either owing to their being category-mistaken or referentially unsuccessful (e.g. the sentence-type "It is raining" uttered when it is, when it is not, or when context indicates that "It" refers to the state of my health).

Though committment to types and tokens may be harmless, few philosophers of language have acquiesced in a committment to <u>both</u> statements and propositions. Both species of entity have a peculiarly parasitic status--dependent upon their being expressed or yielded by sentences-and much effort has been expended upon reductive analyses of sentential meaning which invoke only one such hypostasized item, or preferrably, neither.

Much of Section B is devoted to explaining the distinction between sentences, statements and propositions. Though I offer rebuttals of the best known objections to appeals to statements and propositions—Quine's arguments from the indeterminacy of translation and inscrutability of reference—I accept, nonetheless, the general charge that invoking both "multiplies entities beyond necessity" and risks replacing a complex concept—'meaning'--by two mysterious ones. To resolve this tension, the thrust of my argument in Section B is to claim that there <u>are</u> statements, which alone are distinctively bearers of truth and falsity, to just the extent that there are ways of featuring (talking <u>about</u>) the outcome of successful illocutionary acts of stating. Just as a promise is the outcome of a successful illocutionary act of promising, and a Ph.D. degree is the outcome of, <u>inter alia</u>, a successful defence of a thesis, so a statement is the outcome of successfully <u>stating</u> something true or false, in uttering a sentence in context.

Not every attempt to promise, state, threaten, command or cajole succeeds. The simplest reason I advance in Section B for adding a further dimension to the speech act--the expression of content--is that illocutionary success is not guaranteed by either the grammaticality of one's utterance or the honesty of one's intent. In brief, the significance of one's utterance is a precondition for its illocutionary success. No matter how hard a speaker tries, he fails to promise, state or threaten,

if nothing is expressed to his audience in what he says. And, provided his audience is competent in the language he speaks, illocutionary failure may be the result of the failure of his utterance to be determinately <u>about</u> something, or from its failure to assert anything significant of what it is about. Thus, as my argument continues through Section B, a category-mistaken assertion is an utterance which, in context, fails to yield any definite statement through its failure to express propositional content in that context. It is not until Section E, however, that this notion of 'significant content' is fully explicated.

It would be convenient if the failure of an utterance to express a proposition (to be significant) could be identified simply with its failure to yield a true or false statement. Nonsignificant utterances would, thus, yield statements neither true nor false--and there would be no need for propositions. Unfortunately, questions, promises, wishes, commands, threats and so on are alike in being neither true nor false; and yet significance cannot be witheld from them. Indeed, it is just as easy for what purports to issue in a question or promise to fail through being category-mistaken: e.g. "When is three a prime number?" or "I fully intend to eat her virtue".

In sum, for the purposes of the logic of significance-failures, there are statements, only if speakers perform successful constative acts. Moreover, there are propositions in just that sense in which, when speaker and audience agree upon what is expressed, in context, and can report, in different contexts, with the same or different utterances, their intersubjective agreement in understanding, successful communication of content has taken place.

3) Section B provides the essential pre-formal analysis and motivation for the exegesis of the formal languages of contextual significance in Sections C-E. The first such formalism--CL (for "Context Logic")-does not itself provide an explanation for significance-in-context; but forms the basic syntactical and semantic framework for discussion of that interaction of utterance with context which contributes to significance

and illocutionary success. CL itself, however, is not without intrinsic interest. It provides a schematism representing the circumstances of declarative utterances yielding statements in and across contexts. I do not in general investigate illocutionary acts other than statement-making-though I believe the framework of CL may be adapted to accommodate other illocutions (questioning, commanding, promising...). Since CL offers explicitly only identity-conditions for statements, as the outcome of successful acts of stating, I restrict my investigation to the significance and statement-success of apparently declarative assertions. That is, I concentrate upon utterances which, in context, fail to be significant and to yield statements, though they purport to make genuine truthclaims. Amongst these I intend to locate category-mistaken assertions-though, as noted, declaratives are not <u>distinctive</u> in their vulnerability to category-mismatch.

In making this heuristic simplification, I do not intend to suggest that stating, <u>qua</u> illocution, is primary or more basic than other illocutions. It is likely that, owing to its historical association with mathematical logic and positivist philosophies of science, formal semantics has acquired a prejudice, in its development, in favour of factstating, descriptive language--whereas, on different grounds, it is certainly arguable that a linguistic ability to make truth-claims is neither primary nor basic to a child's acquisition of linguistic competence. This is not an issue I can take up, and, thus, I must regard my simplifying assumption as of methodological import, only--motivated in part by the history of the techniques I employ.

It must be emphasised at outset that much of the formalism of CL is not original to this thesis. I have deemed it expedient to model my approach to contextual significance upon the excellent detailed exegesis of context and significance logics in R. Routley and L. Goddard (<u>1973</u>), <u>The Logic of Significance and Context</u>, Volume I, (Edinburgh: Scottish Academic Press). Their work in this first volume of a projected two volume investigation of various formulations of significance logic has provided the stimulus for my own investigations, and the basis upon which my account is built. It is essential, therefore, that my debt to Routley and Goddard be recorded here, together with a summary of the similarities and differences in our approaches.

The formal language CL of Section C is a straightforward adaptation of Routley and Goddard's system CL of <u>1973</u>, Chs. 2 and 3. In particular, I owe to them the basic representation of the statement-making illocution as a relation between a speaker's utterance-in-context and the statement it yields. Thus, I borrow from their work (with footnoted acknowledgements through Section C) the formulation of the "Yields" relation whose arguments comprise a term composed of a quotation-expression and context- determiner, and a statement-clause composed of a <u>that</u>-operator '§' and restricted sentential variable or constant 'r', 's₀',..., respectively. That is, the form of atomic well-formed formulae of CL (hereafter 'wff'), in terms of which compound truth-functional and quantificational wffs are recursively defined is given, as in Routley and Goddard, by the schema:

"qu(Ø)(ŋ) Y \$s".

Secondly, I adopt and explain from Routley and Goddard (Ch. 2.3, p. 47) their rationale for the "worlds assumption", whose effect is that iteration of contexts is redundant and that, when component subformulae have the <u>same</u> context-symbol affixed (as in "qu($\emptyset(\eta) \supset \psi(\eta)$)"), then the context-determiner may be exported to have the whole quoted formula as scope ("qu($\emptyset \supset \psi$)(η)"). The worlds assumption is justified in observing that, if an utterance is evaluated as significant and statement yielding in a context, this evaluation is not itself subject to re-evaluation relative to a further context--upon pain of regress. In addition, it is assumed that explicitly defined (or primitive) connectives do not vary in their contribution to significance across contexts (i.e. connectives and operators within quotation expressions are extensional).

Thirdly, as recorded above, my explication of the quotation operator "qu(-)" is derived from a synthesis of Routley and Goddard's account of quotation in <u>1966</u>, pp. 1-49, and Grover's formal language L_{qu} for quotation in Grover, <u>1973</u>, pp. 107-112.

Fourthly, the strategy of taking the Yields relation as primitive

(many-one), and of treating nonsignificant and statement-failing sentences as semi-well-formed formulae occurring only within mentioned contexts is Routley and Goddard's (see 1973, Ch. 2.11, pp. 81-90). This avoids the unwelcome consequences of including as well-formed formulae of the logic substituends for the variables (unquoted) which are themselves nonsignificant and statement-failing (thus, not generating welldefined compounds). Such anomalous substituends are confined to semiwell-formed formulae (hereafter: "swffs") appearing as the base-level of the substitution range for the quotation operator-as described in Appendix (B), p. xxxii. In this sense, CL is itself a two-valued, firstorder metalogic with special postulates every wff of which is significant and statement-yielding--even though mentioned instances of swffs belong to an unspecified domain of sentence-tokens some of which may be nonsignificant, or anomalous in a variety of ways. This strategy relies upon the observation that, though, say, the gobbledy-gook string "Blins plinder grue" may be meaningless when asserted in context--thus, not interpretable within the semantics of CL--it is always significant and true or false statement-yielding to affirm or deny significance or statement-success of such a meaningless utterance--when that utterance is mentioned suitably.

It is in the development of a formal semantics for CL that my approach diverges markedly from that of Routley and Goddard. Notice first that, since I have not explicitly axiomatised CL (nor CS-1),--though I discuss axiomatisation at various points--nor have I given a wholly formal proof theory for the formal language (though I appeal to inference rules derived from Routley and Goddard, Ch. 2.11), the systems I consider remain interpreted formal <u>languages</u>, not calculi. Their intended domain of interpretation includes an appropriate fragment of a natural language (say, English). This approach--considering partly or wholly formalised languages without explicitly forming their deductive sub-calculi--is a familiar device of formal semantics when one's emphasis is not upon using model theory in its traditional role of defining the syntactic notions of theoremhood, derivability or proof; but upon developing the <u>semantic</u> concepts of significance, reference and 'aboutness' from rigorous specification of satisfiability in a model and validity in a set of models.

My approach is facilitated, here, by the fact that, since Routley and Goddard have given detailed exegesis of different varieties of the deductive structure their context and significance logics possess--both in axiomatic terms, and in terms of matrices (see Chs. 5 and 6)--then in modelling CL upon their account, I can appeal to a number of diverse axiomatisations and matrix assignments for connectives and operators to justify the inference steps I make.

Routley and Goddard have deferred full exposition of the semantics for significance range theory and quantified significance logic until the as yet (1978) unpublished volume II, (see comments in Ch. 7.1, pp. 431-435). They have, however, anticipated several features of their proposed semantics - in ways that enable one to judge the difference in aim from their account to mine. My own aims in providing an articulation of the notion of a category-mistake and its relationship to type-theories in general are much more severely limited than Routley and Goddard's programmatic amalgamation of extensional and intensional systems within generalised frameworks of significance logic. In consequence, my approach to the interpretation of CL, CS-1 and CS-2 is simpler and correspondingly less comprehensive. The primary divergence between Routley and Goddard's proposed "possible-world"-like or "situational" semantics and my own use of partial interpretations consists in the following:

(i) By supposing that each model of swffs of CL is <u>partial</u>, in that not every subject term is assigned a denotation and not every swff receives from the valuation functions a "truth-value" from the <u>pair</u>-set of values $\{0(False), 1(True)\}$, I seek to embody in the semantics two features of claims I have defended in I, D and II, B. This contrasts with Routley and Goddard's use of <u>four</u> values $\{t, f, n, i\}$ (Ch. 2.15, pp. 110-111).³ The claims concerned are that nonsignificance and statement-failure are <u>not</u> properly construed as comparable semantic values on the same level as truth and falsity; and, secondly, that significance is not a permanent or enduring feature of sentences, but a function of the interplay between speaker's utterance in a context and audience's

identification of the statement, if any, made. That is, 'truth' and 'falsity' apply properly to <u>statements</u> (derivatively to judgements and beliefs); whereas significance and illocutionary success are features of a speaker's utterances in context; i.e. truth or falsity accompanies the successful outcome of an act--significance is a precondition for that success, in so far as a speaker's utterance yields a true or false, publicly identifiable statement to an audience. A nonsignificant utterance <u>fails</u> to yield anything true or false--it is misleading to suggest that it yields some further non-determinate value--through the failure of the utterance to express significant content in that context. Of course, Routley and Goddard's four-member set of values could receive an interpretation along these lines--my preference, however, is for twovalued assignments which leave 'gaps' in the domain of statements corresponding to significance-failures.

(ii) In virtue of (i), the partial models of CL and CS-1, I believe, reflect more adequately the denial of the doctrine of complete definition for predicates for which I argued systematically through Part I (especially in I, B, pp. 94 - 111). That the domain of statements contains truth-value gaps, however, raises problems over whether the domain forms a well-defined structure for a logical theory. The problems are: (a) Is the domain of statements non-empty? (Are there statements?) and (b) If there are statements, can they stand in well-defined logical relationships in terms of which validity and consistency can be defined?

My answer to (a), in Section C, subsection II, is somewhat rudimentary. There are statements if some acts of stating are successful. More fully, though it is contingent that any token-sentence is significant in some context (indeed, it is contingent that there are tokens at all), given that there are significant sentences, then necessarily there are statements standing in determinable logical relations.

In answer to (b), much of Section C (subsections III and IV) is devoted to finding and demonstrating that the domain of statements forms a well-defined logical structure, within which classical tautologies and logical truths may be located. I argue indirectly for this claim: first, through <u>direct</u> examination of the set-theoretic structures which partial models assign to predicates and relations in swffs of CL, I define basic relations amongst statements - "relevant compatibility" and "statemententailment". Next I utilise existing techniques (due primarily to J.M. Dunn, <u>1966</u>) to characterise the abstract structure of the domain of statements as a particular kind of non-Boolean algebra (a distributive lattice), whose elements are the assigned set-theoretic complexes corresponding to statements in the partial models of CL. Various theorems from the representation theory for lattice algebras can then be used to demonstrate the logico-semantic properties desired for CL's domain of statements.

This strategy-forming partial models as (incomplete) inter-(111) pretations of swffs of the formal language and then discovering an algebraic structure in terms of which semantic properties can be defined--is reduplicated in II, Section D's formal characterisation of utterance-aboutness and of the sortal and feature-placing resources of language upon which aboutness depends. The structure of aboutness and sortal assignments is similarly characterised as a lattice--connecting together success in yielding a statement with successful identification of utterance-aboutness in both an algebraic and conceptual fashion. The technique can be viewed as analogous to the strategy of Lindenbaum and Tarski in proving consistency (absence of contradiction) and completeness (derivability of all valid formulae) for classical propositional calculi by demonstrating that they could be regarded as properties of the representation theory for Boolean algebras whose elements are classes of equivalent formulae (see Tarski, 1935; transl. by J.H. Woodger in Tarski, 1956, Essay XII). Apart from the intrinsic interest in linking semantical results of formal logic to representation theory for non-Boolean algebras, the strategy is motivated by the need to provide a general guarantee that "gappy" partial models can be employed to investigate intuitively grounded properties of nonsignificant assertions, without the risk that valid formulae and tautologies are lost in such models.

4) Having introduced statement-compatibility and entailment in II, C; and having provided some resolution of the distinctive problems which attach to the interpretation of negation in non-Boolean models (for which "not-true" # "false" and "not-false # "true"), Section D continues the exegesis of contextual significance by introducing a fuller articulation of kinds of statement-failure (referential-failure, pragmatic anomaly, a priori incoherence and nonsignificance). In addition, it offers the first characterisation of what is to become the basis for the account of categorymistakes. Recall that in I, D and II, B (pp. 313-314) I argued that, relative to context, to deny the significance of an assertion on grounds of category-mismatch is, in the simplest cases, to claim that what the assertion is about is not of a sort or type of which what is asserted can be predicated. In an intuitive sense, thus, a category-mistaken predication exhibits a tension between the sortal and type-features necessary to identifying in context what item the speaker's utterance is about (the reference of its subject-term) and features necessary to determining the scope of application of what the speaker predicates of the item. In short, a simple category-mistake like "This stone thinks" contains a conflict (in literal interpretation) between conditions under which we identify what the speaker mentions as a stone, and conditions under which the predicate "X thinks" applies to items. Such conditions can be regarded as clusters of statements which, in the context, stand in 'relevant compatibility' and 'incompatibility' relations with the aboutness and content of the utterance. Indeed, to the extent that both speaker and audience contribute beliefs to the interpretation of the speaker's utterance which fix the aboutness and content of his utterance, such beliefs and conditions can be regarded as "statable" features of the context, and jointly constitute it.

Section D, thus, has to broach a number of separate issues:

(i) Can the distinction between subject term and predicate-term in a predication be drawn for all varieties of predications - including nonsignificant assertions? (For example, the syntactic form of a gobbledy-gook string like "Blinding greens" may not be determinate).

(ii) Can a notion of utterance-aboutness be defined in a sufficiently

general way, for different predications to have the same aboutness (from context to context)? and how is aboutness to be related to the sortal, feature-placing aspects of referring, and to the inferences we make from and to what an utterance is about, for the truth- and significanceconditions of an utterance to be determinate?

(iii) How best is the notion of context to be articulated to provide for the interplay between utterance and context which contributes to the content expressed?

(iv) What general account of how significance varies across contexts will accommodate some measure of the content a sentence can express from context to context?

5) In concluding the thesis in Section E, I have to devote the preliminary subsection to describing the shortcomings of the framework for CS-1 of Section D. By working through examples I shall show how CS-1 should be extended to a far more complex semantic apparatus to accommodate polyadic relational assertions and to provide a working analysis of quantified assertions (I defer from II, D, a major problem concerning quantification over fictional, mythical and impossible items) I do not propose to give a rigorous exegesis of CS-2—the extension of CS-1. I will be content to illustrate how further investigation into the semantics could suggest avenues along which research could proceed. CS-2 is, thus, not a fully formalised logic of contextual significance; but its advantage over CS-1 lies in its potentially wider applications and explanatory resources.

Extending the simple explication of category-mistakes of Section D, the aim of the concluding discussion is to examine how sortal, classifying, and type features of a language embody generalisations of the methods of identification and individuation employed in focussing upon the subject-matters of our assertions through a context. In this way, the thesis concludes with an informal, and necessarily tentative, extrapolation from the role of significance in semantic theories to the appeal of category-assertions in philosophy. It is hoped that this demonstration of relations between essentially formal distinctions of <u>type</u>-generated within the discussion of Russell's philosophy of mathematics and logic in Part I--and the generic notion of <u>category</u>-as derived from appraisals of nonsignificant acts of assertion--will illustrate the fruitfulness of

a synthesis between formal techniques and historical exegesis which has constituted an underlying aim of this thesis.

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II Section B: Sentences, statements, utterances and propositions:

Question (i) of Sect. A posed the problem of specifying what is said to be significant or otherwise. This is but a part of the general question: to what are semantic properties, like "is true", "is meaningful", "refers to", "designates", appropriately ascribed? Suppose we wish to affirm or deny of a given item that it is significant. To do so is to make a remark about that item and, hence, to mention it. Mentioning a <u>sentence</u>, however, is different from mentioning a <u>statement</u> and such a difference has to be captured syntactically, (i.e. the syntactic type or category of the item said to be significant is not part of the 'meaning' of a significanceclaim). Normally, a device--quotation marks or a distinctive type-face-is employed to indicate that a sentence is being mentioned and not used. To mention a <u>statement</u>, however, is ordinarily, to <u>use</u> a sentence (explicitly, in an indirect clause)--a supplementary device is unnecessary. Compare, for example, the mention of a sentence with that of a statement in:

(1) Tarski used the sentence "Der Schnee ist weiss" to state that snow is white.

If (1) were translated into French, the same statement would be mentioned by the last four words:

(2) Tarski a employé la phrase "Der Schnee ist weiss" pour dire que la neige est blanche.

But, it would be incorrect to translate (2) as:

(3) Tarski used the sentence "Snow is white" to state that snow is white.

For, (3) mentions a different sentence from (1) and (2).

Determining whether the item mentioned in a significance-claim is a

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sentence, statement or something else is of consequence for the significance logics developed below. Suppose we suggest, as Russell did (ibid. I Sect. (), that, so far as indicative assertions are concerned, nonsignificance is the same as lack of truth-value. Thus, "S is nonsignificant" means "S is neither true nor false" and "S is significant" means "S is true or false". Consequently, every indicative assertion is either true, false or neither. Now, if we take "S", here, to range over sentences, we are committed to maintaining both that it is sentences which are properly called true or false, and that significance is a permanent feature of sentences (for, a sentence is identified by its syntactic description; its identification is not relative to context of utterance). The implausibility of this latter committment has been noted already (Intro. Sect. 3, $p.1^8$), and in the rejection of Ryle's theory in Part I. In addition, the former committment--to sentences' being the primary truth-bearers--has been thought objectionable by many on different grounds¹. For, without further qualification, the view entails that a standing sentence-type like "It is raining" can be, at one time, true and, at another, false.

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On the other hand, if values for "S", above, are construed as <u>state</u>-<u>ments</u>, we lose the commonly accepted view that to make a statement is to assert something true or false. Making a nonsignificant statement would, then, constitute doing neither--which renders the view open to the **ripost**e that to make a nonsignificant statement is to make no statement at all-since nothing has been stated.

In either case, disregarding these objections, if we contend that every indicative assertion is either true, false or neither (nonsignificant), we suggest that the relevant basis for significance logic is a three-valued system, interpreted by means of three comparable values on the same semantic level. Such an approach has already been found objectionable--in the appraisal of alternative theories of category-mistakes in Part I. For the significance of an utterance seems to be a <u>precondition</u> for that utterance's having yielded a statement, at least for what is said to be capable of truth or falsity. But, if the significance of what is said is presupposed by a statement's having a truth-value, in what could a statement's being neither true nor false consist?

This last point suggests that, if truth and falsity are properly predicated of statements, nonsignificance should be distinguished as a feature of something other than statements. Thus, the procedure in formulating a significance logic should be to divide non-statements into those which can be used to make true or false statements and those which cannot, and, upon this basis, to introduce the classical division of statements into those which are true and those which are false. In brief, this is the procedure which will be followed below. That is, in accordance with diagram II(i), the focus of significance logic will be upon items falling under node (3), i.e. upon semantically unsuccessful utterances. The semantic capability of an utterance, under this classification consists in its success or failure to yield a statement in a cortext. Unsuccessful utterances fail to make statements and are, thus, neither true nor false--though, in general, there are two ways in which an utterance can fail to state anything true or false in a context:

a) if the utterance is nonsignificant;

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b) if the utterance is significant, but unsuccessful, for a number of reasons:

(i) that the referential term(s) of the utterance fails to refer to the item it purports to be 'about' (see Sect. D) in the context in which it is uttered;

(ii) that the statement-capability of the utterance (yielding a truth or falsity) presupposes some condition or feature which is not ful-filled in that context;

(iii) that the content of the utterance is such as to predicate, or ascribe to what the utterance is 'about' features which, though significantly predicable of such items, fail to be satisfiable by such items in that context, or, relative to the context, fail to express a determinate proposition with respect to those items.

(i) - (iii) constitute only a rough description of the conditions for statement-failure for significant utterances. More precise formulations of these varieties of statement-failure are discussed below.

Semantic success, then, becomes a precondition for an utterance's making a statement, for its saying something true or false. It might remain open to us, still, even granting the above distinctions, to take 'nonsignificance' to mean 'neither true nor false'--since this latter phrase could be reconstrued in the sense of 'truth and falsity necessarily fail to apply' to nonsignificant utterances. However, this option must also be closed in virtue of two arguments introduced below: to the effect that a) some unsuccessful significant utterances can fail necessarily to make statements--in the strong sense that there is no logically possible

context in which such utterances could make true or false statements (e.g. Sect. A, (5.1)) and b) a statement-making act (and, thereby, the circumstances of its failure) differs in kind from the act of uttering a sentence to express a proposition. In virtue of a), b), identification of nonsignificance merely with lack of truth-value is precluded.

Various answers to question (i) have now been eliminated, but we are no nearer stating precisely <u>what</u> it is to which 'significance' is ascribed. In brief, the answer I shall adopt is that <u>utterances</u> are properly called 'significant' or 'nonsignificant'; more precisely, that an utterance's significance consists in its expressing a proposition in its relevant context. Consequently, the nonsignificance of an utterance consists in its <u>failure</u> to express a proposition. Thus, what is evaluated by a significance-claim I shall call the 'propositional content' expressed by the utterance of a sentence in a context.

Before this answer can be fleshed out in any detail, however, it is necessary to examine further the relations and differences between sentences, their utterance in context, statements and propositions. In particular, as I shall conclude this section, there is a need for a means of formally representing the circumstances in which an utterance is statement-yielding in a context, and of identicy conditions for statements.

My exposition of these relations and differences, from hereon, relies, in part, upon E.J. Lemmon's "Sentences, Statements and Propositions"²-to the gratifying extent at least that some of the arguments below can be referred directly to his article. Nevertheless, some additional discussion is required to deflect W.V. Quine's three-pronged attack on 'propositions'

in Quine 1953, 1960 and 1970. Quine charges that

(i) invoking propositions as "meaning-bearers" runs the risk of "hypostasizing mysterious entities"³

(ii) formulation of identity-conditions either for 'propositions' or for 'statements' is impossible owing to arguments from the indeterminacy of translation and inscrutibility of reference (1960, Ch. II) and that (iii) in any case, statements or propositions as truth-bearers, in contrast to sentences, are "theoretically dispensable" in favour of 'eternal sentences' (1960, Ch. VI §§40, 42, 43). I shall reply to charge (ii) in this section in clarifying the distinctions between statements and propositions. Discussion of the charges (1) and (iii)--that propositions are "mysterious entities", and in any case dispensible, I defer until Section D where substance is given to the claim that an utterance's significance or otherwise consists in its expressing or failing to express a proposition in its appropriately determinate context.

As Lemmon points out (ibid., p. 98), the general notion of 'what is said' by an utterance of any given sentence--often used to pin down the notion of a statement--suffers from a deep-seated ambiguity. This ambiguity is revealed in cases where the same sentence is uttered in different contexts and where different sentences are uttered (perhaps by different people) in one context. Let us take it for granted that identity conditions for separate utterances being utterances of the same sentence are given (usually by identifying the <u>sentence-type</u> with the equivalence-class of its orthographically or acoustically similar <u>tokens</u>--where a sentencetoken consists of a single, spatio-temporally locateable event which is the utterance of a syntactically complete linguistic unit).

In the first case, then, if I utter the sentence, today

(1) "I am cold"

and repeat this utterance, tomorrow, there will be a <u>prima facie</u> sense in which what I have said is the same. Yet, there is also a sense in which what I have said is different--since my utterance of (1) today may state something true, and, tomorrow, something false. Similarly, in the second case, if I utter (1) today, and simultaneously, you address me in uttering:

(2) "You are cold"

what we have both said is, in one sense, the same (we have both stated the same truth) and, in another sense, different (we have uttered different sentences). All this is simple enough. We appeal to the sentence/statement distinction to observe that my repetition of (1) on the following day is a token-utterance of the same sentence type to make a different statement, whereas, our simultaneous utterances of (1) and (2) are different tokens of different sentence types used to make the same statement. But, what is it for you and I to make the <u>same statement</u>? Clearly, that you and I utter simultaneously different tokens of different sentence-types to say something true (or false), in that context, is an insufficient answer. For, indefinitely many of our simultaneous utterances in that context may agree in truth-value, yet fail to make the same statement (you might have said "You are not wearing a hat"--which also happened to be true). It is clear, also that one cannot simply appeal to the fact that what we have said in uttering (1) and (2) is the same, for (though it is true in <u>one</u> sense) the ambiguity in the notion of "what is said" is precisely the problem to which the sentence/statement distinction was to provide an answer. What is needed, therefore, is an examination of statement making utterances which will yield an account of their identity-conditions. Such an examination, moreover, must deal with a number of questions:

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a) What is it for an utterance of a sentence to make a statement? An answer to this provides for the identification of statement-making utterances and, in so doing, focusses upon the first step in the procedure mentioned above (p. 3) for formulating a significance logic--i.e. the division of utterances into statement-making and non-statement-making.

b) When do different utterances of tokens of the same or different sentence-types make the same statement? The example above requires only that different utterances of tokens of different sentence types can make the same statement. It is also the case that utterances of the <u>same</u> sentence-type can make or mention the same statement. In quoting what another has said, I may employ the same words (incorporating a suitable device) to make the same statement--thereby mentioning a token of the same sentence type.

c) A moment's reflection upon b) introduces a third complication. Quoting another's words is not the only means of mentioning what another has said. When I report a conversation, I may mention, not the sentences uttered, but the <u>statements</u> each party made, and, in so doing, the sentencetokens I utter may not be of the same type as the originals. The question arises: when I report another's utterances in indirect speech, under what conditions do I mention the same statement as was made by the other's

utterance?

Clearly, there are instances where this last question is easily subsumed under question b). If I utter the sentence (1) and you, perhaps in response to a query, report what I said by uttering:

(3) "He said that he is cold.",

then roughly the same conditions under which utterances of (1) and (2) make the same statement will have to be satisfied for an utterance of (3) to mention the same statement as (1). Not all such cases of indirect speech will be so easily accommodated. For example, as the context of an utterance of (1) is replaced by a different context, the conditions under which a report may mention the same statement become more stringent. As time goes by, a request for a report of what I said will have to be answered by:

(4) "He said that he was cold",

owing to the context-sensitivity of the tense of the original utterance.⁴ Yet such stringency is mitigated by the fact that indirect speech appears to admit degrees of leeway in the conditions under which a report can be said to have mentioned the statement originally made. In an appropriately remote context from that which occasioned the original (1), an utterance of:

(5) "S. Sommerville complained about the temperature" may adequately report, if not the statement made, then the utterance's illocutionary force (in Austin's sense⁵). Perhaps it is wrong to include (5) as an example to be accounted for in a description of identity conditions for statements, since identity is, here, being relativised to 'sameness of illocutionary force'; but it is true enough that we often allow
ourselves a certain licence in reporting indirectly the statements of another. Our reports are accurate only relative to obvious implications and background truths available to all parties (those to whom the report is addressed and parties to the original utterance). If we add to this that variation in contexts between the original and our report may <u>require</u> suitable alterations in the indirect clause-formation which mentions the statement, then, perhaps, only preservation of truth-value fixes a limit to the degree of deviation which can be accommodated within the same statement's having been mentioned or made.

On the possibility of obtaining adequate identity-conditions in such cases, Quine has expressed considerable pessimism:

"There is nothing approaching a fixed standard of how far indirect quotation may deviate from the direct. Commonly, the degree of allowable deviation depends on why we are quoting. It is a question of what traits of the quoted speaker's remarks we want to make something of; those are the traits that must be kept straight if our indirect quotation is to count as true." (<u>1960</u>, p. 218)

Quine's point is well-taken, but it lacks penetration. The extensional canon of preservation of truth-conditions between the original statement and the indirect report is necessary but obviously insufficient. One wants to add that "preservation of 'meaning-content'" provides the additional canon, except that Quine's known antipathy for appeals to synonymy⁶ gives one pause. But, even if we grant that <u>exact</u> synonymy is unobtainable between different sentence-types (or between a sentence-token and the statement-clause of an indirect report)--and I cannot argue that issue, here--preservation of content may serve, at least, as an ideal against

which to measure the accuracy of our indirect reports; an ideal, no doubt, which contextual considerations and the exigencies of the report may override, but in virtue of which it cannot be inferred that no standards obtain, at all. The relation between the content expressed by one utterance and that expressed by a different utterance (perhaps an indirect report of the original) may always fall short of identity--but, analogously, it is probably the case that the relation between any two tokens of a given sentencetype, from the perspective of morphophonemic description, always falls short of identity, but that does not preclude our judging that the same sentence has been uttered. In other words, that a qualitative equivalencerelation holds between what different utterances express may be sufficient to determine that the same statement has been made, and, in view of contextual variations between utterances, it is likely that numerical identity of propositional content is unobtainable, in principle. Indeed, as the notion of 'propositional content' is explained in the next sections, it becomes unclear what the phrase "numerically identical content" could mean in respect of the significance of distinct utterances.

Quine's point is supplemented, of course, by his noted arguments from the indeterminacy of translation and inscrutability of reference. The first argues from the fact that any number of radical translations (i.e. translations of a previously unknown native language) may effect a mapping of sets of non-observation sentences of the native language onto sets of English sentences, disjoint in respect of content and truth-value, but consistent with the behavioural data available to the translator, to the conclusion that, even within <u>one</u> language, no objective, content-preserving

descriptive expressions) as a precondition for identifying statements. And, this relativity of statement-identification (even in the provision of conditions under which an utterance makes a statement at all) has already been conceded in requiring that the procedure for distinguishing between statements and non-statements, thereby, between significant and nonsignificant utterances, be embedded in a generic account of semantic interpretation for the language (see above Sect. A and Intro., p. 1). Granting this relativity removes the impact of arguments from the indeterminacy of sentence-synonymy or of co-referentiality of expressions; but, to do so, is to make no major concession to those arguments. For, it is a platitude that our determination of the content, force and truthvalue of one anothers' utterances must take place against a background of assumptions to the effect that we speak a common language in roughly similar ways, that the senses we attach to expressions in our common vocabulary do not deviate wildly from person to person, and that the subject-matters about which we speak, items to which we refer, are derived from a common stock of intersubjectively shared beliefs, perceptions and species-similar experiences.

Where the indeterminacy arguments acquire more bite, if valid, is in circumstances where our identification of the statement made by a speaker's utterance is to serve as a ground for ascribing to him a propositional belief (that the proposition expressed by the utterance is believed true by the speaker). For, suppose we take an utterance by X of:

to be our ground for attributing to X the false belief that Iran has a

(6) "Persia is larger than Peru".

correlation of sentences with sentences is possible, except relative to a framework of "analytic hypotheses" i.e. to a more or less arbitrary set of meaning-postulates from which relations of synonymy can be derived. Since any number of frameworks of analytic hypotheses may all fit the behavioural data, and fall within margins provided by the non-absurdity and non-contradictoriness of the proferred translations⁸, Quine infers that no appeal to 'sameness of content' between different sentence-types is intelligible (save for observation sentences, truth-functional tautologies and Quine's idiosyncratic class of "stimulus-analytic" sentences-all of which are immune to translational indeterminacy). The second thesis-for the inscrutability of reference--offers a parity of reasoning from translational indeterminacy to the conclusion that no appeal to a preservation of reference between the constituents of different sentence types is available to pin down identity conditions for statements (again, except in relation to a framework of 'ontic hypotheses' which postulate a background ontology over which the referential expressions of the language are defined).

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This is not the place where these theses can be examined. Fortunately they need not be. For, the thrust of the indeterminacy arguments, if accepted, is not to show the <u>impossibility</u> of formulating conditions when distinct utterances make the same statement; but only that any judgment to this effect must be taken as relative to such a framework of analytic and ontic hypotheses as we impose upon the contexts of utterance. But, this is simply to introduce the background semantic theory for a language (specification of meaning-relations and assignment of referents to

greater surface area than Peru. Our inference requires the additional assumption that X employs the expressions of (6) with their normal sense and reference; in particular, that, say, X believes that "Persia" designates a geographical area whose borders coincide, roughly, with modern Iran (and, of course, that he attaches normal significance to the relation "is larger than", etc.). If, instead, X believes that "Persia" is another name for Brazil, his utterance of (6) is not grounds for our ascribing to him a false factual belief about the size of Iran, but an incorrect semantic belief about the designation of "Persia". In such circumstances, the indeterminacy arguments amount to the claim that there need be no decisive evidence which would resolve our doubt over whether his mistake is factual or semantic. Given everything else that X believes, and assuming X is sincere, we can determine what X means from what he says, i.e. his other beliefs about Persia and Brazil will determine the reference he attaches to the occurrence of "Persia" in (6). Conversely, given a description of the semantics of X's idiolect, we can determine from what X says the set of his true or false propositional beliefs. Unfortunately, we are given neither in any absolute sense. We derive both what X believes and what X means from what he says (and, of course, from his other behaviour). The theses of indeterminacy of translation and inscrutability of reference, if we accept them, entail that there need be no unique way of slicing up the evidence of X's utterances and behaviour which would, simultaneously, fix the significance X attaches to referential and descriptive expressions in his vocabulary, and determine, up to maximal consistency, the set of factual beliefs X espouses. Even though we employ what N.L. Wilson has called "the

Principle of Charity"⁹--whereby we fix the significance of X's descriptive vocabulary so that as many true beliefs are ascribed to X as tally with our own factual knowledge--it may be that, however much evidence we acquire, mutually inconsistent sets of beliefs fit that evidence equally well.

Tacitly in Quine, and explicitly in Davidson¹⁰, there is an extension of this argument from the indeterminacy theses to a rejection of 'propositions' as the objects of propositional attitudes (circumstances comprising e.g.: 'believing that p', 'wondering that p', 'accepting that p', 'hoping, expecting or fearing that p' are customarily described in terms of a subject standing in an attitudinal relation to a proposition 'p'). The argument takes the form: if there were such a thing as 'the propositional content' expressed by an utterance, there would be a unique objective criterion for the correctness of translation and the accuracy of referential commitment. But there is no such unique objective criterion, hence, there can be no such thing as 'propositional content'. The argument is cogent only in relation to a very strong form of the indeterminacy theses--one which Quine does indeed advance¹¹. Even then, however, it does not lead to the conclusion that the notion of propositional content is unintelligible; but only that the analysis of attitudinal contexts in terms of propositional content must be taken relative to the semantic frame of reference we introduce. A fuller evaluation of Davidson's argument and its effect on the notion of 'contenc', as it is used in the account of utterancesignificance, must await the exposition of this notion in section D.

There remain the twin problems of formulating identity conditions for statements, and their relation to the significance of utterances in

context. Thus far I have observed that:

i) truth and falsity apply properly to statements (derivatively to sentences uttered in context);

ii) different tokens of the same or different sentence types maymake different statements in different contexts or in the same context,resp.;

iii) since the success or failure of an utterance to make a statement in a context depends upon its significance, it is incorrect to characterise monsignificance as a <u>value</u> of statements comparable to truth or falsity; and that

iv) significance or nonsignificance is a feature of the utterance of a sentence in a context, pertaining to the proposition it expresses in that context.

The remainder of this section is devoted to connecting (i)-(iv) to form a coherent resolution of the above two problems. Such a resolution is indispensable, if the formal developments of sections C and D are to be grounded in the relevant preformal distinctions and analyses. The first step, then, in such a 'resolution' is to explain and defend the distinction between statements and propositions.

As a preliminary, consider the following:

- (7) The only Parti-Quebecois premier of Quebec has advocated independence.
- (8) The previous Parti-Quebecois premier of Quebec has advocated independence.

It is natural to say that the statements made by (7) and (8) would be incompatible in any one context, since they could not agree in truth-value. That is, in a context in which what (8) states is true (at some future time when the P-Q have had more than one leader elected premier), (7) would fail to state anything true, owing to the referential failure of its subject term. Yet, it would also be natural to say that what (7) stated in <u>one</u> context would be the same as what (8) stated in any context in which the definite description in (8) referred to René Levesque, say, in the circumstances of his successor coming to power. This much is granted already by (ii)--utterances of (7) and (8) in different contexts may make the same statement. In addition, it has also been granted that, if, say, (8) occurs in an indirect context:

(9) He claimed that the previous P-Q premier of Quebec ha advocated independence.,

the same statement may be <u>mentioned</u> as is <u>made</u> by an utterance of (7), in a different context. (Indeed, (9) might report another's utterance of (7)-since the token-reflexivity of "only" in (7) is transformed to "previous" in a post-election context). In both cases, our ground for maintaining that the same <u>something</u> is involved is that both utterances are about Levesque, and ascribe to him an advocacy of independence. Yet, it is not necessary that <u>statements</u> be involved at all, in judging that what utterances of (7) and (8) say about Levesque is the same. For example, (8) may be a constituent of a conditional assertion.

(10) If the previous P-Q premier of Quebec has advocated independence, so will his successor.

It would be wrong to claim that what the occurrence of (8) in (10) 'states' in one context is the same statement as is made by (7) in a different context. For, the utterance of (8) as a constituent of (10) does not make a

statement, at all. To suppose that it does would be to fall into the same confusion as beset Russell (Principles \$38) through a desire to say both that "Peter is a Jew" makes the same statement when it occurs alone as when it occurs in "If Peter is a Jew, then Andrew is a Jew"; and, on the other hand, that it does not make the same statement. It must make the same statement, Russell suggests, else modus ponens would not be valid (i.e. in an inference from 'p' and 'p \supset q' to 'q', 'p' must stand for the same thing in both its occurrences). On the other hand, it cannot be the same statement that is involved, because, then, "Peter is a Jew; if Peter is a Jew, then Andrew is a Jew; therefore, Andrew is a Jew" would be the same as "If both Peter is a Jew and, if Peter is a Jew, then Andrew is a Jew, then Andrew is a Jew". And it was precisely Lewis Carroll's discovery that these last two cannot be the same $\frac{12}{12}$. Hence, we cannot explain what (7) and (8), as it occurs in (10), have in common by appealing to their making the same statement. This shared something, one must suppose, is what the several utterances of (7) and (8) express, in their respective context,

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i.e. a proposition.

This conclusion is an instance of a far more general observation that requires the statement/proposition distinction for its explanation. Only a relatively small class of utterances make statements--though it is no small task to enumerate even these. For, different utterances of tokens of one and the same sentence type may perform any number of linguistic acts, in different contexts, only one of which may constitute making a statement. Consider:

(11) You will learn to drive in two weeks.

I can utter (11) as a prediction, as a command, as an expression of my intention to teach you, as a promise (as in "(11) or your money back"), or with inflection and intonation of incredulity, as a rhetorical question, etc. Accepting that only the first of these constitutes making a statement, it remains appropriate to say that, in all five cases, I have referred to someone (in the context) and predicated of him/her 'learning to drive in two weeks'. In only the first case is it apposite to evaluate what is said as true or false (i.e. the statement made). So, to describe how, in the separate utterance of all five, the reference and predication are the same, whilst the linguistic act is different, we distinguish what is expressed (or signified) by an utterance of (11), from the statement made (if any) by an utterance of (11). We can reject immediately any proposal to identify what is expressed by each utterance of (11) with the sentence (type or token) used, since, in a different context, a different token (of a different type), say, uttered the following week, would state, promise, or command, etc. the same thing. In sum, then, we need to introduce the distinction between the content expressed by an utterance in its context, and what Austin has termed the "illocutionary act" performed in uttering a sentence in that context¹³.

The upshot of this discussion, then, is that an utterance of any given sentence characteristically involves at least three distinct kinds of act:

- a) uttering a token of a sentence-type,
- b) expressing a propositional content,
- c) performing an illocutionary act.¹⁴

Arguing from distinct illocutionary acts having a common propositional

content is cogent, however, only upon provision of clear criteria distinguishing a) from b), and a) and b) from c). The condition that utterances of distinct sentences may express the same content in different contexts serves to separate a) from b), in kind. How are b) and c) distinguished?

Provisionally, one can follow Austin in making a condition for the performance of an illocutionary act that there be a first-person indicative verbal phrase which, when affixed to the sentence uttered, is constitutive of the act performed in uttering the sentence¹⁵. Austin has claimed that there are more than a thousand such verbal phrases in English--examples of such being: "I state that..", "I promise that..", "I demand that..", "I warn that..", "I question whether..", "I approve that..",.... It may hold that there are more than a thousand illocutionary acts that any given utterance can, in principle, perform.

Austin's condition, though necessary, is not sufficient. For, at first glance, there seems no reason to prohibit 'expressing a proposition' or 'uttering a sentence' from being construed as illocutions on a par with 'commanding', 'promising', and so on. That is, under Austin's criterion, is there any ground for prohibiting "I express the proposition that.." or "I utter the sentence S to say that.." from fulfilling the illocutionary criterion?

To counter this objection, notice, first, that a)-c) are not three separable actions that speakers perform--as may be the case if I utter "The Nothing negates", raise my finger for emphasis, and fall from the lectern at the same moment. Neither should a) and b) be construed as acts functioning as means to ends--as packing a suitcase and buying a ticket are to

going on a vacation. Rather, uttering a sentence stands to expressing a proposition as filling in a ballot slip stands to voting, and to performing an illocutionary act as moving the pieces in a rule-governed way stands to playing chess. Nevertheless, each act <u>is</u> separable in respect of what can be predicated of any given instance and in respect of each act's identity conditions. Thus, of a sentence-token, one can predicate location, length and shape, etc.; of a sentence-type, one can predicate grammaticality, logical form, etc.; of the content expressed, one can predicate significance, ambiguity, relevance (to the context); whilst of an illocutionary act, one can predicate truth (of a statement), sincerity (of a promise), justifiability (of a demand), validity (of a question), fairness (of a command) and so on.

On its own, however, this will not do. For, what we say of each kind of illocution is sufficiently different to remain consistent with construing 'uttering a sentence' or 'expressing a proposition' merely as (different) kinds of illocutionary act, in their own right.

Austin, in the work cited above, concluded that there are no clear grounds for distinguishing performative illocutions from what he called "constative" utterances (what I have called "statement making utterances")--a conclusion with which the tenor of the above argument is in agreement. For to make a statement <u>is</u> simply to perform an illocutionary act for which truth and falsity are the relevant evaluations (notwithstanding that <u>what</u> makes a statement true or false raises philosophical questions). In this respect, to make a truth-claim--one which, if sincerely made, in a nonfictional, unpretending context, serves as grounds for an ascription of

belief--is to perform an act not distinguishable generically from the many instances of illocutionary acts already noted. Yet neither 'expressing a proposition' nor 'uttering a token of a sentence-type' are acts of this kind, primarily because, unlike illocutionary acts they are not actions <u>I</u> can fail to perform, even though it is occasionally the case that when I utter a sequence of expressions of a language, no proposition may be expressed, or no sentence uttered. The relevant difference, between acts of kind (c) and of kinds (a) and (b) is to be located, then, in the conditions under which such performances fail.

To clarify this point, we can draw upon observations made by Austin:

"The performative (illocutionary act) is not exempt from all criticism ... (it) must be issued in a situation appropriate in all respects for the act in question: if the speaker is not in the conditions required for its performance (and there are many such conditions), then his utterance will be, as we call it in general, 'unhappy'." (Loc. cit., p. 22-3)

Austin distinguishes three **varieties** of "unhappiness" to which the performance of an illocutionary act is prone:

a) the performative, like any other ritual or ceremony, may be null and void if the speaker is not in a position to perform an act of that kind. For example, a bigamist cannot get married a second time (Austin's example) his repetition of "I do" is void. Similarly, I cannot command you to do x either if I have no authority over you, or if x is impossible. Neither can I state what is neither true nor false (If I say "All John's children are asleep" when John has no children, I have failed to make a statement).

b) Secondly, the 'performative formula' may be 'abused' (Austin's term) if issued insincerely. If I say "I promise that x" with no intention

of bringing x about, perhaps even believing it beyond my power to bring x about, I have certainly <u>made</u> the promise, but it is an empty act--a 'hol-low' promise.

c) The third kind of "unhappiness" involves what Austin calls "breach of commitment". That is, with varying degrees of commitment, I may successfully perform an illocutionary act, then renege upon the commitment consequent upon the act. Obvious "breaches of commitment" are involved in promising x but failing to do x, in threatening to do y and not doing it, in demanding z from one who is, in principle, unable to supply z.

In all three cases a)-c), it is appropriate to attribute responsibility for the "unhappiness" of the illocutionary act to the utterer (speaker). In void illocution, \underline{I} fail to marry you if I am married already (in monogamous societies), though the form of my utterance is entirely adequate. \underline{I} lack the authority to command you, although my utterance is in the form of a command, \underline{I} fail to make a truth-claim if a presupposition of my utterance is unfulfilled (in that context). Similarly, in the case of 'hollow performances', \underline{I} lack the intention (or ability) to bring about what I have promised, and it is <u>my</u> failure to do x when threatening or promising x that constitutes a breach of illocutionary commitment. The point I wish to stress in these observations is that the circumstances in which an illocutionary act can be said to fail are extrinsic to the performative utterance itself. They pertain properly to the failure of the utterer to fulfill the pragmatic or conventional conditions with which the act is associated.

This feature of illocutionary acts is readily substantiated by the noted asymmetry¹⁶ between the first person singular present indicative and other persons and tenses of the verbal phrases denoting illocutionary acts. I can make a promise to bring it about that another person perform some action---as when I say "I promise that he will come"---it is still <u>my</u> promise which will be void if I am unable (or have no intention) to bring it about that he comes. In other words, if I seek to carry out an illocutionary act on another's behalf, then, at best, I succeed only in <u>reporting</u> another's (possible) act, or, at worst, I commit <u>myself</u> to bringing about the relevant consequence. When the bullied child threatens the retribution his elder brother will exact, it is not his brother but the child himself who has issued the threat, and it is <u>his</u> threat that is empty, should he be an only child.

Similarly, the performance of an illocutionary act usually requires the present indicative of the relevant performative verb. To say "I will promise x" is to predict a <u>future</u> performance (not to make a promise), just as to report one's past utterance is not to make but to <u>mention</u> the statement one has made. Notice, however, that even though illocutions are sensitive to mood, the performative formula can be satisfied in the passive voice: e.g. "You are requested to refrain from smoking", which carries an implicit "hereby" (or equivalent phrase: in French "par ces mots-ci") which enjoins the relevant behaviour without the need of the corresponding indicative. The enthetic "hereby" serves, also, to effect the illocutionary act even in the relatively rare instances of third person indicative illocutions. For example, in the ubiquitous "Her Britannic Majesty's Principal

Secretary of State for Foreign Affairs requests and requires (hereby)... all those whom it may concern to allow the bearer to pass freely without let or hinderance..." (British Passport, p. 1), the third-person verbs carry out the illocutionary act even in the absence of a specific object to whom the request and requirement is addressed.

Nevertheless, the fact that in its normal form an illocutionary act is performed by a first-person indicative utterance bears out the conclusion that responsibility for the <u>failure</u> of an illocutionary act accrues, normally, to the speaker. That is, the failure is characteristic not of the utterance but of the agent.

Such a conclusion stands in marked contrast to the circumstances in which no proposition is expressed, or no sentence enunciated, by a given utterance. The reasons for failure in this latter case <u>are</u> attributable directly to the utterance itself--in the sense in which one would claim that the given utterance would fail to express a proposition, or fails to be a sentence, no matter who uttered it. For example, an utterance of:

(12) * If have I were

fails as a token, since it is not of a grammatical English sentence-type. The failure, we can say, is <u>intrinsic</u> to the utterance, and not consequent upon the speaker's intentions, committments or presupposed beliefs, all of which are <u>extrinsic</u> to that particular utterance. Similarly, the nonsignificance of:

(3) The number seven is indifferent to tomato soup, In any literal context, is not a feature of the speaker's "breach of committment" or whatever, but of the utterance's failing to express a proposition in

that context. However we explain what it is for an utterance of (13) to fail to express a proposition, it remains the case that the failure resides in features of the utterance itself, not pertaining to the residual characteristics of the utterer.

Here then, are the relevant differences that we sought between the notion of an illocutionary act and the acts of expressing a proposition and uttering a sentence. One can summarise them in concluding that illocutionary acts (statement-making, promising and so on) are acts done in uttering a token of a sentence-type, which expresses a proposition in a context. Thus, neither uttering a sentence nor expressing a proposition qualify as illocutions, for the sound reason that, in contrast to illocutions, their success or failure in any given instance, qua acts, does not depend upon features of the speaker who utters the sentence to express a proposition in that context. Illocutionary acts, on the other hand, typically succeed or fail in virtue of circumstantial features of the speaker's performance of the act. In other words, one can succeed or fail to perform an illocutionary act even though all relevant features of one's utterance are impeccable; but if one utters a non-sentence, or one's utterance fails to express a proposition, one cannot but fail to perform the act intended, since one's utterance does not meet the conditions for that act.

Having drawn the distinction between statements and propositions in this manner, it would be well to address directly the question of what statements are. Thus far, statements have been classed as the outcome of successful performative acts--just as promises are the outcome of successful acts of promising. It is entirely apposite, however, to demand of an

answer to a "What is X?" question that it clarify what is essential to something's being an X. And, in this respect, it is insufficient to have shown merely that, whatever else Xs (statements) are, they are different from Ys (propositions). In this case, however, an answer to the question "What is a statement?", in the form of a definition, is not immediately forthcoming-for it depends upon what we intend the notion of a 'statement' to explain, on the role this concept has to play in a general semantic theory. I will not attempt here such a general semantic description as would be necessary to locate statement-making uniquely amongst all other linguistic acts--defining "statement" thereby. I will take, however, two steps towards such a general description in order that the notion of a statement -- as the product of a successful illocutionary act performed in uttering a sentence--bear the explanatory weight required of it later when the contrast between an utterance's significance and its success in yielding a statement (in a context) is used in diagnosing the source of the nonsignificance of type-violations and category-mistakes. These two steps are as follows:

 (i) To expand upon the analysis, in the remainder of this section, of the conditions for an illocutionary act's being statement-making (i.e. successful or unsuccessful); and,

(ii) To examine, in Section C, formal conditions for statementidentity. For it is an answer, in part at least, to a "What is X?" question to have explained what has to be the case for this to be the same X as that--thus, to have represented formally the circumstances in which statements yielded by speakers' utterances in the same or different contexts are the same.¹⁷

Though it has been concluded that statement-making is only one of a large number of illocutionary acts, I shall disregard from here on utterances produced in the performance of any act other than (purported) statement-making acts. Thus, when I divide speaker's utterances into statement-making and non-statement-making, the division is to be construed not as between one illocution and the rest, but between purported statementmaking acts with respect to whether they succeed or fail. Confining the discussion in this way is not to commit myself to claiming that significance or nonsignificance is ascribed only to sentences with which a speaker purports to make a truth-claim, i.e. to state something in uttering them. One can (attempt to) give absurd orders, or make a category-mistake in posing a question ("When is three a prime number?"). Statement-making is one illocutionary act, and, like all such acts, it can succeed or fail for a variety of reasons -- some of which have been discussed above. In general, the "happiness" or otherwise of a statement-making act can be attributable to:

a) features pertaining to the physical environment of the act (to the utterance-token: that, say, it occupies a finite time span);

 b) features pertaining to the speaker (that he articulates clearly, that he is not speaking involuntarily, that his intention is revealed in his utterance);

c) features pertaining to the type of which his utterance is a token (that it is grammatical, belongs to an interpretable language);

d) features pertaining to the illocutionary act, itself (that it is non-void, that it involves no breach of convention or commitment);

e) features pertaining to the content expressed by the utterance in

its context--where "context" is intended to cover both linguistic and nonlinguistic factors relevant to determining the utterance's significance (that it is unambiguous, determinately <u>about</u> some item(s), and states something determinable of what it is about).

This classification is merely approximate -- one requires, in addition, some indication of the order of priority in which each of these features bears upon the success of the statement-making act. One essential ingredient in the success of such an act is derived from the distinction between propositions and statements, as drawn. The rationale for this distinction has been to separate, pre-theoretically, the role of the declarative utterance in communicating information to an audience (for which the relevant evaluations are the significance, relevance, and intelligibility of the speechact in context) from the role of the utterance in making truth-claims (for which the relevant evaluations are the truth or falsity of the statement yielded). The former role is prior to the latter in the sense that, ignoring issues concerning the contribution an audience makes to interpreting a speaker's utterances, that an utterance is significant in its context (that it expresses a proposition) is a necessary condition both for the speaker to have made a statement in uttering it, and for our successfully identifying and re-identifying that statement. Epistemologically, this claim for priority is trivial--one has to have understood (grasped the significance) of a speaker's utterance to have ascertained what the speaker thereby states. With respect to the explanatory role of the concept of statementhood, however, this claim for the priority of significance over statementhood proves contentious. First, I shall illustrate the claim; then, examine objections to it.

Suppose a speaker S utters the sentence "Logic is dull" in the middle of a class on sentential calculus. It would seem sensible, <u>ceteris paribus</u>, to infer from what S said, in the context, that S did not enjoy doing logic---at least, not at that time. That is, there would be natural reasons for finding what S said odd, or for doubting his sincerity, if he went on to add "of course, I do not wish you to believe I find logic uninteresting or tiresome". This suggests that, whatever conditions we impose upon the concept of 'statement', inferences from <u>our</u> identifying the statement made by S, in the context, to a belief S has about logic, or to some attitude to which S is giving vent during the class, should be supported by our acknowledgement that S's uttering "Logic is dull" is sufficient, in that context, for his having made the <u>statement</u> that logic is dull.

This last introduces what I shall call the <u>publicity requirement</u> (PR) upon statement-making acts. (PR) consists in the claim that:

> (PR): Whatever else making the statement that-a amounts to, a speaker, in uttering a sentence in a suitable context, makes a committment to what his utterance expresses to others which serves as prima facie grounds for attributing to the speaker the belief that-a, or some attitudinal relation to what we identify as the truth-claim that-a.

(PR) is essentially a minimal condition upon an illocutionary act of statement-making being successful. Nonetheless, it is surprising how contentious the requirement turns out to be when viewed in the light of current

discussions of illocutionary acts. For my purposes, I shall justify (PR) only by appeal to the explanatory consequences one expects of our account of statement-making. In addition, however, it will be necessary to discuss briefly one account of illocutions (derived from H. Grice's theory of nonnatural meaning--see below) which purportedly conflicts with (PR).

It is an immediate consequence of (PR) that if the statement that logic is dull is yielded by a speaker's utterance of "Logic is dull" in a given context, <u>our</u> understanding his having significantly expressed of logic that it is tiresome, uninteresting or boring <u>publicly</u> identifies a truthclaim to which S is committed in uttering that sentence. To say, here, "S is committed to the truth-claim that-a" is to hold that any disavowal of the statement that-a S may make, <u>in that context</u>, is rendered pragmatically stultifying by (PR). Thus, if S says "Logic is dull but I don't believe it", what he has said is not formally contradictory. Rather, what he has said is <u>stultifying</u> (fulfills no illocutionary function), because (PR), as a condition upon statement-making, commits S, through what his utterance signifies to others, to the statement he thereby makes.

Further support for (PR) might be derived from a consideration of the general condition upon linguistic acts that they be, in principle, repeatable and, in some sense, communicative (even if mumbled to oneself); also, from examination of the grounds for blame or responsibility for what one says (for example, in libel suits, in cases of 'deception' and 'lying'). I shall forego investigating (PR) further, though, to examine the following objection.

Consider what it would be like to deny the publicity requirement:

in the above example, it might appear open to S to claim that, in uttering the sentence "Logic is dull", he did not <u>intend</u> us to interpret 'being dull' as having the sense of 'being uninteresting, or boring', but, say, as having the alternate sense of 'not being sharp, unsuitable for cutting (as of knives...)'. So, it might be claimed, S was not committed to the truthclaim that we identified--to the effect that logic is boring--when he uttered the sentence, because his <u>intention</u> was not to be taken as stating this.

It is in refuting this claim--that identification of an illocution is relative to speakers' intentions--that the priority of significance over statement-capability is given point. The example given is, of course, contrived. For, saying of an abstract discipline of philosophy--logic--that it is unsuitable for cutting is literally nonsignificant. So, unless contextual considerations favour S's idiosyncratic construal (as they do not in my example), the nonsignificance of S's alternative disqualifies his utterance from yielding a statement we can identify, in the context, as a truth-claim about logic. As such, if we believe S sincere in his explanation, we should deny that S has made a statement in saying "Logic is dull", for no other reason than that S's utterance expresses no proposition for which readily discernible significance conditions are available. Uttering a nonsignificant sentence precludes making a statement.¹⁸

Not all such examples are so immediately clear. For, in this case, it is only because S's alternative is literally meaningless that the implausibility of ascribing to S an unintelligible <u>intention</u> (to state that logic does not cut well) can be inferred. In less contrived examples, the claim

for the priority of significance over statement-capability (or, as it is sometimes called "the priority of assertibility-conditions over truthconditions") may not be so readily substantiated. On the contrary, it has been suggested, in rebuttal, that an account of statement-making--and of the logical relations between statements and beliefs--is incomplete, unless it can accommodate inferences <u>from</u> the utterances of a speaker <u>to</u> what he intends in what he says. I shall consider this rebuttal in some detail-primarily because it not only threatens the notion of 'statement ' I employ in formulating identity-conditions for statements; but it introduces, also, several important issues in the philosophy of language, pertinent to the notion of significance. My argument will be that the rebuttal does not genuinely conflict with the publicity requirement; indeed, that the position from which it derives most support <u>presupposes</u> some such requirement as (PR) as a condition upon illocutionary success.

The objector to (PR) denies the claim that inferences to ascriptions of belief (from <u>our</u> identifying the statement S has made, to attributing a belief, say, that-a, to S) are supported only if S's statement that-a is publicly revealed in what S says (that is, identified independently of S's "inner states", intentions, wishes, and so on). The objector rejoins: if what S utters is to serve as <u>grounds</u> (inductive support) for what S believes, then the inference must proceed in the opposite direction. That is, we can infer S's belief that-a only from ascertaining the sincerity of S's <u>intent</u> that-a and of his <u>intent</u> to be understood as stating that-a in saying "a". Consequently, we are correct in identifying S's utterance "a" as constitutive of his making the truth-claim that-a only if we recognise S's <u>intention</u>

to elicit in us, through his utterance, an acknowledgement of the truth of (and his belief in) his statement. So, the objector continues, unless we include in an account of what it is for S to state that-a some reference to S's intention, in uttering "a", to elicit in us an appropriate acknowledgement of that-a's truth through our recognition of this intention, then <u>our</u> identifying what statement S has made cannot justify ascribing a belief that-a, to S, on the basis of what S says.¹⁹

The proponent of this objection is claiming that the inference: if S states that-a then S believes that-a, is justified only if S's intention in uttering "a" was both to bring it about that we (as S's audience) respond as if his statement that-a were true, and to elicit in us a recognition of this intention that could function as a reason for responding in this manner. In the absence of S's so intending, it is claimed, we would be incorrect to identify S's saying "a" as constitutive of his making the statement that-a in a way which could support our judging that this is what S believes. For, the objector argues, S may say many things -- as illustrations, epithets, lies, jokes, and so on--which do not represent his beliefs, because he does not intend--when he says these things--to be taken as stating them (i.e. in a way in which he might be held culpable for them). Thus, it is concluded, what is relevant to determining that S's illocution in uttering "a" is statement-making is not that we identify that-a as the statement yielded, in context, by S's utterance (PR); but that S intend his utterance to yield the statement that-a because of his belief, desire, thought, supposition or whatever that-a. Identifying S's statement, then, depends not simply upon what S says, but, typically, upon what S intends

in what he says.

The objection vividly illustrates a contemporary conflict within the philosophy of language between theories of meaning which divorce linguistic meaning from psychological considerations (connotation, association, thoughts, desires, intentions...) and theories which seek to explain linguistic meaning in terms of certain irreducible psychological notions²⁰. The conflict can be illustrated in terms of the claim of explanatory priority, introduced above: proponents of theories of meaning which divorce meaning from 'mental states' argue that, if meaning is to constitute what is learnt and publicly communicated in speech, it must be describeable without reference to the particular psychological states of language-speakers (which are taken to be, in some sense, 'private'). On the other hand, proponents of a psychologistic view argue that, since understanding and expressing thought is paradigmatically a matter of relating public signs to private states, no explanation of utterance-meaning is complete without reference to the intentions, thoughts and desires the speaker articulates and the listener comprehends in uttering and interpreting sentences. So, for example, H. Grice's analysis of non-natural meaning is often taken as providing a general account covering linguistic and non-linguistic meaning, into which a descriptive account of the overt semantic meaning of sentences must fit (see, for instance, Searle's discussion²¹). So, the claim continues, a psychologistic account is explanatorily prior to the descriptive semantic approach.

I cannot attempt to resolve this conflict within the philosophy of language, here; though my argument is intended to suggest an approach towards its resolution. It is unfortunate, perhaps, that the issue is often

clouded by an ambiguity in the verb "to mean", whereby in one sense, it is roughly synonymous with "to intend" whilst, in another sense, it has the force of "to signify", "connote". I have tried to avoid this ambiguity through using "to signify" and the noun "significance" for "to mean", "meaning", whenever my focus has been upon the second sense. (It would beg the question, of course, to contend at outset that the two senses are synechdochally related).

The core of the objector's rebuttal of (PR) lies in his claim that the force of an illocutionary act (making a promise, issuing a threat, stating, warning, entreating), if it is successful, originates in the speaker's act-oriented intentions to secure certain responses in us, his audience. When those intentions are fulfilled, the illocutionary act is successful, not in the sense of 'cause to effect', rather in the sense that fulfillment of the speaker's complex of intentions is <u>constitutive</u> of illocutionary success--just as we might say hitting the ball over the boundary in a cricket match successfully scores six runs; not because so hitting the ball has that effect, but because scoring a 'six'is constituted by so hitting the ball. M.P. Grice has characterised this sense of what it is for a speaker S to (non-naturally) <u>mean something</u> by an utterance U_o in the following way²²:

- S non-naturally means something by an utterance U if a) S intends (i₁) to elicit, by uttering U, a response R in an audience A.
- b) S intends (i_2) that A recognize S's intention (i_1) in uttering U_0 and,
- c) S intends (i_3) that this recognition, on the part of A, of S's intention (i_1) shall serve as A's reason, or part of A's reason, for responding R.

To the conditions a)-c), P.F. Strawson has added a further clause to accommodate a number of counter examples, the substance of which need not concern us, here.²³ Let it suffice that for S's utterance to be a genuine case of <u>communicating</u> to A what S non-naturally means by U₀ Strawson requires also that

S intends (i_4) that A recognize S's intention (i_2) . [that is, that S should not only intend A to recognise S's <u>primary</u> intent to elicit the response R, but that he should also intend A to recognise his <u>secondary</u> intent to get A to recognise his intention to elicit R] (I owe this distinction between "primary and secondary intentions" in this case to N.L. Wilson: see his "Grice on Meaning: the Ultimate Counter-example", <u>Noûs</u>, 1970).

So, for example, what S non-naturally means in saying to A "You cannot leave" is exhaustively characterised as S's <u>entreating</u> A not to **leave (rather, say, than** S's <u>stating</u> that A cannot leave) according as S fulfills the conditions that:

- a') S intends to get A to stay (= R) by saying "You cannot leave";
- b') S intends that A recognise that S wants him to stay;
- c') S have the intention that A's recognition of S's intention to get him to stay is to be a reason (perhaps in part) for A's remaining; and
- St.d') S intends that A should also recognise S's intention to get A
 to acknowledge what S wants as among S's intentions in saying
 "You cannot leave".

Such an account of non-natural meaning is to contribute to an explanation of illocutionary success by establishing as a necessary condition of ascertaining what illocutionary act is performed that a speaker succeed in bringing it about that his audience took him, in saying what he did, to have just the complex of intentions as are given by a)-St.d). The psychologistic objection proceeds: unless the conditions for statement-making--<u>qua</u> illocutionary act--involve the speaker's intentions with respect to his audience in this manner, then they are inadequate to that determination of speaker's beliefs, thoughts, desires and inner states, which is required of an account of utterance-meaning. Making public-identifiability a condition upon successful statement-making is, therefore, too restrictive through its eschewal of speaker's intentions.

The reply should be immediate: it is no part of the publicity requirement to deny that identifying speakers' intentions, desires and so on is concommitant upon understanding what is said. Any account of utterance-meaning which precluded inferences from what a speaker says to what he believes, thinks, desires or intends would be too restrictive. But this is precisely the reason for the publicity requirement. It is only to the extent that S's uttering "a" can be taken by others, in the context, as constitutive of his stating that-a, that his saying "a" to state that-a is distinguished, for example, from his saying "a" to entreat, warn or command that-a. The fallacy in the psychologistic objection lies in its assumption that identifying the complex of intentions a speaker has is additional to (and, therefore, explanatory of) the recognition of the statement he makes in uttering a sentence. Yet, it is only to the extent that a speaker's intentions are revealed in his utterance (and in his utterance's fulfilling the public conventions for stating, say, as opposed to entreating) that his illocutionary act is communicated to his audience and, thereby, given effect.

It is in overlooking this fallacy that the objector can separate 'what S non-naturally means by U_0 ' from 'what an audience understands S to be doing (stating, entreating) in uttering U_0 ', in order to take the Gricean analysis of the former as explanatory of the contribution of non-natural meaning to the latter.

The fallacy is most apparent in the following illustrative counterexample (due to Wilson, loc. cit., p. 296):

> Suppose...I am conversing with Grice. I say 'snow is white'. By uttering 'snow is white', I mean that snow is white. It follows that by uttering 'snow is white', I mean something. According to Grice, it follows that I intend... $[(i_1), (i_2), (i_3) \text{ and } (i_4) =$ my variables]...Now I do intend to say [make the statement]...that snow is white, but the only secondary intention I have is to avoid having any of the intentions Grice attributes to me...

It follows that Wilson has <u>meant</u> something by uttering "snow is white", but his meaning this does not consist in his having the complex of intentions his audience (Grice) attributes to him because of his contraexemplary secondary intention. As Wilson points out (p. 297), unless the Gricean objector postulates <u>co-variance</u> between the response a speaker intends (i_1) to elicit and what his audience understands him to be doing in uttering a sentence, there need be no relation at all between how a speaker's utterance is taken and the intentions a speaker has (however devious) in uttering it.

To postulate co-variance, however, between a speaker's illocutionary intent and his audience's "uptake" (to use Austin's term) is implausible unless a speaker's having certain of those intentions is revealed to his audience in his utterance--either because of some explicit performative formula a speaker uses, or because of circumstantial or conventional features of the context of his utterance. Thus, S's saying "a", in a given context, is constitutive of his making the statement that-a if it holds that S's intending (i_2) --that his audience recognise that S intends (i_1) to claim that-a (indicate his belief in a's truth) -- is exemplified in his utterance's expressing to his audience that-a. That is, co-variance is plausible when S's illocutionary act is successful. But, S's saying "a" to warn, entreat or state that-a is successful only if S's utterance communicates to his audience the warning, entreaty or statement that-a-either through his use of an explicit performative ["I warn you that ... ", "I entreat you that...", "I claim that..."] or through his utterance's fulfilling some (public) convention (gesture, intonation...) or feature of the speech-situation. And this, finally, is simply a re-statement of the publicity requirement (PR).

Unless 'uptake' of S's illocutionary intent is publicly secured, S may intend many things, in all sincerity, in uttering "a". His failure to carry these out derives <u>not</u> from his failure to <u>have</u> the relevant Gricean intentions (his sincerity guarantees this), but from some feature which, in that context, his utterance lacked--in the absence of which his audience could not recognise his act. Examples of such detrimental circumstances-with respect to void illocutions, breaches of committment or 'unhappy' utterances--have already been given (above p. 399). Conversely, even where S <u>lacks</u> the requisite Gricean intentions (S has given us every reason to believe he is being deliberately perverse, pretending or deceitful), it can still be the case that 'uptake' is secured merely by the fact of S's utterance fulfilling conventional conditions, as it were, accidentally: [P. Ziff²⁴ has an extended example of an intelligent man trying to avoid the draft by babbling incoherently before the draft board, so as to be classed incompetent for service. Uptake is secured, however, not because the army sergeant recognises his intentional deceit (which indeed, the sergeant does recognise) but because in recognising the deceit, the sergeant -- his audience--uses his intentions as a reason for classing the man incompetent on the grounds of moral perversity.] In other cases, for example, it is essential to S's lying successfully--rather than merely speaking falsely--that S's saying "a" be constitutive of his stating that-a, even in the absence of his intention (i_{1}) to get his audience to recognise his single-minded intent (i_{2}) (in this context) to deceive. If so, it is essential only when it is granted that our identifying S's saying "a" as making the statement that-a is sufficient for S to have made that statement (and be culpable for it, if it is a lie). Again, the publicity requirement is presupposed in S's (dishonest) illocutionary success.

These examples conclude the discussion of the supposed objection to (PR). By removing the psychologistic objection, the difficulty that an account of statement-making involve, in an essential way, the intentions, thoughts, wishes and so on, of speakers--thus, complicating beyond feasibility the description of contextual features relevant to statement success-is avoided. I proceed now to step (ii), listed above, in elucidating the role of the concept of 'statement' within the general semantic and pragmatic framework being developed. That is, I proceed to examine, in a preliminary

manner, the formulation of identity conditions for statements--on the basis of which the formal semantic apparatus to be introduced in sections C and D will be introduced.

It is not hard to enumerate the relevant intuitions involved in formulating identity-conditions for statements--at least for a class of simple, indicative, subject-predicate sentences uttered in relatively standard contexts. Informally, they would appear to be the following:

a) that, in general, the conditions for statement-identity should be adequate to explain both how different utterances, in the same or different contexts, yield the same statement, and how the same statement as is <u>yielded</u> in one context may be <u>mentioned</u> (say, in an indirect report) in a different context;

b) that utterances U_1 , U_2 of tokens of different sentence types in one context C_0 make the same statement if U_1 , U_2 are both <u>about</u> some item I, and what each <u>states of</u> I in C_0 is the same;

c) that utterances U_1 , U_2 in distinct contexts C_0 , C_1 , make the same statement if what U_1 is <u>about</u> in C_0 is what U_2 is <u>about</u> in C_1 (say, I), and if what U_1 <u>states of</u> I in C_0 is what U_2 <u>states of</u> I in C_1 ;

d) that a report r_1 of an utterance U_1 in C_0 , mentions the same statement in its context C_r as is yielded by U_1 in C_0 if the item I r_1 mentions in C_r is what U_1 is about in C_0 , and if what r_1 mentions in C_r being stated of I is what U_1 states of I in C_0 .

What is more difficult, given a)-d), is to formulate--in a formally precise manner--an identity relation between statements which can accommodate these intuitions--yet which allows both that there is some admissible leeway in identifying utterances as yielding the same statements across contexts, and that some utterances fail to yield statements in context owing to referential failure (not being <u>about</u> any item), failure of presupposition,

nonsignificance or a variety of other reasons. To represent a)-d) completely requires analyses of:

(i) what it is for an utterance to <u>yield</u> a statement <u>in</u> a context,
(ii) what it is for an utterance to be <u>about</u> an item <u>in</u> a context,
(iii) what it is for something to be <u>stated</u> of an item <u>in</u> a context,
and (iv) what it is for a report to <u>mention</u> what an utterance is about
and <u>report</u> what is stated of an item in a context.

These are the basic notions that are explicated in formulating the logics CL, CS-1 and CS-2, below (beginning at section C)

Given this motivation for CL, CS-1 and CS-2, it is an immediate priority to examine in detail the distinctions between the use and mention of utterances and sentences, the quotation of tokens and types and the syntactical properties of quotation operators. What a speaker utters in any given context, on anyone occasion, is a sentence <u>token</u>. So, in so far as each report using an indirect clause (i.e. of the form:

(1') X uttered the sentence "..." in the context c_0 to state that ____)

is but another utterance, in a context, the constituent quoted sentence is simply a <u>further</u> token occurrence of a sentence within a quotation expression '"...."'--though an occurence of a sentence in a quotation context is not a <u>use but a mention</u> of that sentence. In the ideal case, of course, a report of the form (1') is correct when the quotation expression reproduces (within it) the words of the original utterance. So, it might be supposed, the function of quotation is just to single out (as a <u>name</u> singles out) the individual item which is the actual token uttered by the speaker with the relevant illocutionary (statement-making) force.

Token utterances, however, are single unrepeatable events -- uniquely identified in terms of the time, location of utterance and identity of speaker. Furthermore, that a given utterance successfully yields a statement, as has been argued above, depends upon a number of features of context, together with significance, grammar and illocutionary force which may be unique to the context of utterance. Some of these features are properly ascribed only to the sentence-type; whilst others pertain only to tokens. Thus, a quotation expression, on its own, may be ambiguous between typeand token-mention. For example, consider the grammaticality of an utterance: this property would seem to hold quite independently of both the idiosyncracies of context, and, say, the pitch, dialect or intonation of the speaker's verbal performance. As such, the property pertains best to a sentence qua sentence-type. For, it would seem proper to claim that, irrespective of contextual features, any token occurrence of a grammatical sentence remains grammatical in virtue of its being an instance of a grammatical type. Contrast this with the referential success of a descriptive phrase like "the tallest man in the room": different utterance-tokens containing this phrase may, in different contexts, be about wholly distinct individuals (and, sometimes, about no-one at all). So referential features of utterances would appear to pertain properly to tokens.

In view of such cases, how can a report of another's statement-making act successfully reveal the connections between the statement, the utterance a speaker produced in making it, and the context of his utterance, if what a report mentions--even by means of quotation--is always different from what

a speaker originally says? To answer this question, it is necessary to clarify further the varities of use, mention and quotation of sentence types and tokens--and to examine some of the shortcomings of the standard account of these distinctions. To accomplish this task, I have given a very brief commentary on use, mention and quotation in Appendix B, pp. 723-766

In this section, I have accomplished a systematic examination of some of the vital pre-formal concepts and distinctions which are employed in formulating the logic of statement-making acts in context--CL--and the logics of significance--CS-1, CS-2--developed later in the thesis. In so doing, I have identified a statement as the outcome of a successful illocutionary act of making a truth-claim and I have suggested various ways in which such acts may be unsuccessful and no statement yielded by the speaker's utterance. In addition, I have argued that there is a need for a notion, separate from the statement-capability of an utterance--in terms of which the propositional significance of an utterance--as opposed to its illocutionary success--may be evaluated. The upshot of this extended argument was to conclude that an utterance of a sentence in a context characteristically involves at least three distinct kinds of act:

a) uttering a token of a (grammatical) sentence type,

b) expressing a significant proposition,

c) performing an illocutionary act.

Finally, it proved necessary to examine in some detail the conditions for the successful performance of an illocutionary act of statement-making, in order to counter the objection that an account of illocutionary success requires an essential appeal to the psychological determinants of verbal behaviour--intentions, thoughts, wishes and the like. This objection was not refuted, rather it was shown that confining the identification of
statements, <u>qua</u> illocutions, to publicly identifiable objects of speech acts was explanatorily prior to investigating how the determinants of speech-acts are revealed in their performance. Such a supplementary investigation of inferences <u>to</u> psychological states and processes from a theory of illocutions is beyond the scope of this thesis.

II Section C: Statement-identity in the logic of statement-making utterances (CL)

(I) Preliminaries:

A <u>statement</u> is the product of a successful illocutionary act of statement-making, publicly identifiable in context in terms of what the utterance used in making it is <u>about</u>, what is <u>stated of</u> what it is about, and contextual features pertaining to circumstances of the utterance. This was a conclusion of Section B where, once problems over an adequate account of use/mention, type/token and quotation were settled, it was proposed to continue examining the concept of 'statement-making', through provision of identity conditions for statements. This is the task of this section: i.e. treating statements as logical objects, it examines some of the relations between utterances, contexts and statements necessary for the distinction between an utterance's contextual significance and its illocutionary success. On this basis, the section introduces a formal framework--the logic CL¹--for inter- and intra-contextual identities between statements.

As observed in Sect. B (p. 420), the paradigm of a statement-making act is exemplified in a report of the form:

(1) x uttered qu (p_0) in c_0 to state that, where the constant 'qu (p_0) ' mentions a specific utterance (token), ' c_0 ' abbreviates a contextual description and the clause 'that...' mentiors the statement (if any) yielded by x's utterance.

(1) mentions a person x, a sentence uttered by x to make a statement, and a context in which x's utterance yields the statement mentioned by the that-clause. In one respect, it is a solecism to describe an utterance as "yielding a statement, in a context". As was argued in Sect. B, a <u>person</u> makes a statement (performs an illocutionary act) <u>in</u> uttering a sentence (if his act is successful). The symbolism I shall adopt below, however, absorbs two arguments of the tetradic relation: "x <u>utters S in c to state</u> that-a", to form the dyadic relation: "An utterance (by x) of S-in-c <u>yields</u> that-a". Such a symbolism, neglecting the person-variable, is therefore less explicit than it might be; but it has the advantage of simplicity (and a reference to the speaker can always be regarded as part of the contextual description).

I have observed above that an utterance of a well-formed sentence can fail to be statement-yielding in a context if it is nonsignificant or illocutionarily unsuccessful. Formal investigation of the conditions for significance-failure is the concern of the next sections (D, E). It would be arbitrary at this point, however, to exclude nonsignificant utterances from the domain of the formal theory CL. Consequently, the initial divergence from classical logic in the development of CL is the admission of nonsignificant and unsuccessful utterances into the argument-range of 'qu(--)'. This raises the problem of how to evaluate compound sentences which may contain nonsignificant or unsuccessful components. Yet, we need not deal with this problem immediately since, in general, in CL we do not use formulae which take nonsignificant or unsuccessful substituends, but only mention them (by means of the qu-operator). This distinction will become clearer in the definitions, below.

I have said that statements are mentioned by that-clauses in reports of the form (1). Statements are not only mentioned by factive clauses,

however. One may pick out a statement as one of a class, as in:

(2) What you have just said (stated) is a cliche.,

or individuate a statement in terms of its subject-matter, as in:

(3) Nixon's denial of involvement in the Watergate cover-up proved false.

Similarly, statements can be classified generically, for diverse purposes, as in:

(4) Most lies damage the interests they seek to protect.,
 or referred to uniquely in terms of location, date and identity of speaker:
 (c.f. the distinction between type- and token-features in (3);

(5) Wittgenstein's claim in 3.25 of the Tractatus (1921) is the logical foundation of his atomistic metaphysics.

Clearly, then, there are many ways in which statements can be referred to, classified or individuated--they form a coherent domain of objects, in some sense.

There may be some question, however, whether it is appropriate to treat statements as <u>logical objects</u>—in the sense in which, say, the sentential calculus treats syntactic relations between, and operations upon, <u>sentences</u> as logical objects, (substituends of sentential variables). For, though statements mentioned by factive clauses do have properties and stand in relations, it may not be clear that such properties and relations form a <u>logical</u> structure. Under the customary notion of logical form or structure, it is appropriate to say of <u>sentences</u> that they are logically structured, because they are syntactically complex entities. Indeed, modern sentential logic is precisely the investigation of purely syntactic relationships between syntactic objects. We say a sentence qu ($p \supset q$) is interderivable with qu (~q \supset ~p), relative to a suitable formal language, axiom-set and rules of inference, because such sentences possess a compound syntactic structure. Equally clearly, statements do <u>not</u> possess a syntactic structure in this sense, because they are not syntactic objects. This follows immediately from the fact that we cannot say of two <u>statements</u> that one has the form \emptyset , and the other the form of its negation $\sim \emptyset$. For different sentences, one containing the sign for 'not', the other lacking it, may yield the same statement when uttered in a suitable context; c.f. qu (he is alive) (c₀) and qu (he is not dead) (c₀).

Nevertheless, one <u>can</u> assert of statements that logical relations hold between them, by mentioning them appropriately. For, statements <u>imply</u> one another, they may be <u>incompatible</u> with one another, one may be true only when another is true, and so on. Such relations between statements, indeed, are exactly those truth-functional relationships upon which the classical two valued interpretation of sentential logic depends. Since we can affirm such relations explicitly--as when we say: "(The statement) that Socrates is a man implies (the statement) that Socrates is human", or affirm them less directly, as when we say: "To say qu (Socrates is a man) is (to state something) incompatible with denying humanity of Socrates", and so on, then a logic of statement-making must reflect this fact.

It might be objected: truth-functional relations between statements are intended simply as the mirror-image of those syntactic relations between <u>sentences</u> with which classical logic deals. So, no special logic is needed for statements, as opposed to assigning sentences truth-values in classical bi-valent models of sentential logics.

To counter such an objection, I shall draw attention to the important differences between sentences and statements discussed already in B, and, to begin with, to note the following disclaimer: the statement-logic CL is not intended as an <u>alternative</u> to sentential logic (indeed, it presupposes it). To the extent, however, that classical logic treats significance (i.e. well-formedness), truth and falsity as permanent features of sentences, CL <u>supplements</u> this formalism by introducing context of utterance, contextually relative significance and semantic success as further properties of sentences uttered in context capable of formal representation. For, the differences between sentences and statements, <u>when such additional</u> features are introduced, are sufficient to require a separate treatment.

Firstly, where p, q are sentences, in classical logic, so are $\sim p$, (p v q), (p & q), (p \supset q), (p \equiv q)--they are syntactically compound sentences. But, where a, b are statements (values of factive-clauses), $\sim a$, (a v b), (a & b) and so on, are <u>not</u> compound statements. For, the substituends of statement-variables are that-clauses mentioning statements, and compounds formed from that-clauses do not mention statements. For example, from the sentences "Pluto is a dog", "Mickey is a mouse", the compound sentence "Pluto is a dog and Mickey is a mouse" is formed. Given the clauses "that Pluto is a dog ", "that Mickey is a mouse", however, the compound "that Pluto is a dog and that Mickey is a mouse" is not a statementclause. This reflects the salient grammatical fact that sentences are autonomous linguistic units capable of syntactic transformation; whereas factive clauses are terms (noun-phrases) which are not autonomous.

Though compounds formed from clauses and connectives are not

admissable, that-clauses of compounds <u>are</u> admissable. For example, from the sentence "Pluto is happy whenever Mickey is happy", we may form the clause "that (Pluto is happy whenever Mickey is happy)", but not the illformed "that Pluto is happy whenever that Mickey is happy." In general, then, anticipating the notation, below, if \emptyset is a well-formed (grammatical) sentence, $\${\emptyset}$ is a well-formed clause (wfc)², whatever the complexity of \emptyset . So ' $\${\circ} a$ ', ' $\${a v b}$ ' and so on, are wfe's; whilst ' \sim $\${a}$ ', '($\${a v \${b}}$)' are not. Thus, CL captures the difference between the grammatical:

(6) X stated that if it rains, then he will stay at home, and the ungrammatical:

(7) * X stated if that it rains, then that he will stay at home. The important logical difference, here, is that, though X may utter a conditional sentence to make a statement, it does not follow that X has made a conditional statement (for, in an important sense, there are <u>no</u> conditional statements--just as there are no disjunctive, negative or conjunctive ones). That is, a speaker's conditional sentence does not assert a conditional statement; rather it asserts the consequent of the sentence, conditional upon the assertion of the antecedent ('that b, if a').³

(II) CL: Formal developments.

It is proposed, then, that the domain of statements comprise the universe of discourse for a formal theory of the logical relations between statements, differing from the purely syntactic relations of sentential logic. With the domain of statements construed as individual values of statement-variables, the proposed logic CL--employing separate styles of

variable for utterances, contexts and statements--represents the semantic properties of statement-yielding utterances in context through its interpretation. Formally speaking, then, CL is an applied predicate logic of first-order with special postulates, serving as a two-valued, many-sorted meta-theory for the two-valued object language of statements. This definition is explained further below, in discussing the admission of nonsignificant and statement-failing utterances into CL.

I introduce, first, utterance variables and constants:

u-variables: p, q, p', q', p", u-constants: p₀, q₀, p₀,

At this point, also--for the purpose of later discussion of 'aboutness'--I introduce also two sorts of variables corresponding to the classical individual and predicate variables i.e.

subject-arguments: x, y, z, x', y',
subject-constants: x₀, y₀,

Subject arguments take as substituends any expression which functions as a <u>singular</u> grammatical subject in a sentence. Subject arguments (but <u>not</u> constants--which abbreviate token expressions) are quantifiable variables, whose admissible ranges of variation will be clarified in a later section. I use ' ξ ', ' ξ_0 ', ' ζ ', with or without subscripts, as schematic for any subject constant or argument. Much as in classical predicate logic, I introduce, also:

n-place predicate parameters: f^1 , g^1 , h^1 , f^2 , ..., f^n , g^n , which take as substituends expressions functioning grammatically as predicates (i.e. what remains of a sentence when one or more subject-terms are omitted). Indices for the number of places of a predicate parameter (the number of replacements by singular terms necessary to result in a wellformed sentence) will often be omitted as understood. I use 'F', ' G_0 ' as schematic for any predicate parameter.

Substituends of u-variables are utterance-tokens of grammatically well-formed types. No further restriction upon the substitution class of u-variables is imposed; in particular, some substituends-utterances in a given context-may be nonsignificant, or fail to yield statements.

As I have argued, that an utterance yields a statement (that a speaker successfully states something in uttering it) depends upon its expressing a proposition, in a <u>context</u>. In D, E, an analysis of the notion of 'context' is undertaken, but I take due note, here, in introducing context-variables and context-constants:

c-variables: c, d, c', d', c", c-constants: c_0 , d_0 , c_0^{\dagger} ,

Context-variables qualify sentential formulae and their subformulae in accordance with the following rules for the formation of 'semi-well-formed formulae' (swffs). That the formulae defined below are called "semi-wffs" rather than, simply "well-formed formulae" reflects the fact that swffs are not admissible formulae of CL on their own, but are admissable only as welldefined parts of admissible formulae:

(DF I): Swff: Let '~, &, v,⊃, Ξ' be classical connectives,
Ø, Ψ schematic for swffs and sub-swffs defined as follows
(where θ,η are schematic for c-variables or constants):
a) If Ø is a u-variable or u-constant, Ø is a swff. Ø is a sub-swff of itself,

- b) If \emptyset is a swff, so is $(\sim \emptyset)$, with sub-swffs \emptyset and any sub-swffs of \emptyset ,
- c) If \emptyset, ψ are swffs, so are $(\emptyset \& \psi)$, $(\emptyset \lor \psi)$, $(\emptyset \supset \psi)$ and $(\emptyset \equiv \psi)$. Their subswffs are, respectively \emptyset, ψ and any sub-swff of \emptyset, ψ .

- d) Where ξ_1, \ldots, ξ_n are subject-arguments or constants and F^n a predicate parameter, $F(\xi_1, \ldots, \xi_n)$ is a swff; its only sub-swff is itself.
- e) Where ξ_1 , ξ_2 are subject-arguments or constants, $(\xi_1 = \xi_2)$ is a swff; its only sub-swff is itself.
- f) Where Ø'is a swff (perhaps containing occurrences of ξ), (Uξ)Ø' (read 'for every ξ, Ø') (Pξ)Ø' (read 'for some out of all ξ, Ø') (]ξ)Ø' (read 'there is an ξ such that Ø') are swffs; their sub-swffs are Ø'and any sub-swffs of Ø'.
- g) If θ is a c-variable or constant, \emptyset a swff containing no subswff of the form $\psi(\eta)$ -for c-variable or constant η -then $\emptyset(\theta)$ is a swff, whose sub-swffs are \emptyset and any sub-swffs of \emptyset not qualified by a c-variable or constant.

[If \emptyset is a swff of the form $(U\xi)\psi$, $(P\xi)\psi$ or $(\exists\xi)\psi$ and sub-swffs of ψ contain occurrences of ξ , then ξ 's occurrence(s) is said to be a <u>bound</u> occurrence (otherwise free). If all occurrences of ξ in sub-swffs of ψ are <u>bound</u> occurrences, ξ is said to be <u>bound</u> and \emptyset is said to be <u>closed</u>]

Reasons for the choice of three quantifiers '**U**', 'P' and '∃' in CL will be discussed in the next section. Meanwhile, some clarification of Df I g) is required. Complexes of the form $\phi(\theta)$, $(\phi \lor \psi)(\eta)$ are to be read " ϕ <u>in</u> the context θ ", " $(\phi \lor \psi)$ with respect to circumstances η ." It is not fortuitous, of course, that $\phi(\theta)$ resembles the functional notation 'f(x)'--for the resemblance emphasises the functional dependence of the significance or statement-success of utterances upon their context of utterance. Since ' p_0 ' abbreviates an arbitrary token of some sentence-type, the notations ' $qu(p_0)(c_0)$ ', ' $qu(p_0)(d_0)$ ' reflect the circumstance that different tokens of one sentence-type may occur in different contexts and vary in respect of significance and success in those contexts.

As shall be explained more fully in D, a context is given by a set of descriptive features yielding time, place of utterance, utterance 'aboutness', interpretation of content expressed, and factual and semantic information relevant to determining the statement made. Context-variables, therefore, range over.sets of such features; i.e. they take as substituends sets of statements whose truth-conditions fix the referential and significance conditions determined by such information. How much information may be needed to describe a context uniquely is unimportant. What matters is that enough coherent information fixes what is expressed by the utterance, and identifies (publicly) the truth-claim (if any), yielded, thereby. Contextual information is not communicated by a speaker's utterance; but, in grasping the significance of his utterance and agreeing upon what has to be the case for what is said to be true or false, an audience acquires (or possesses, already) as much information as is presupposed in his utterance's yielding a true or false statement, if significant. Such information will consist partly in a determination of the 'aboutness' of referential expressions and agreement upon what is 'stated of' what such expressions are about in the utterance, relative to context. From this point of view, contexts can be more or less highly structured, though, minimally, their description involves at least two components (α , G)--the 'aboutness' and 'content-G'⁴ expressed. So, " $\phi(\alpha, G)$ " could be written in place of " $\phi(\theta)$ ", subject to definition of these features in E. However, in this section, the structural complexity of contexts is not under discussion. It simplifies the notation a little to retain primitive contextual variables.

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Additional comment upon Df I g) must draw attention to two assumptions made with respect to the dependence of compound utterances upon context (see Routley and Goddard, <u>1973</u> pp. 47-8). Each sentential connective, interpreted truth-functionally for all statement-making utterances (undefined for seman-tically unsuccessful utterances), operates within <u>one</u> context. That is, if

a compound swff has the same c-variable or constant qualifying each subswff, the context-symbol is exported to qualify the entire swff: e.g. $qu(\emptyset(\Theta) \lor \psi(\Theta))$ is always $qu(\emptyset \lor \psi)(\Theta)$. Assuming that contexts are 'exportable' in this sense is plausible for the standard sentential operators--though, one might argue, it may fail in some natural extensions of the notion of 'context'. For example, in the context of royal etiquette, it could conceivably be that, in the presence of the monarch, men bow and women curtsey--' $(p_0 \& q_0)(d_0)$ '--though it is neither the case that men bow when no women are present, nor that women curtsey in the absence of (other) men--' $(p(d_0) \& q_0(d_0))$ '. However, it is always possible to refine the contextual specification to include such contra-exemplary features, so that the absence of men, or women, respectively, is an additional determinant of $qu(p_0)$, $qu(q_0)$'s truth-conditions. In general, the limitation to 'exportable contextual features' is no major limitation unless highly 'intensional' contextual features are involved (e.g., ' \S_{P_0} believed by X in Θ ').

The second assumption qualifying Ig) is that iteration of context symbols is redundant. Ig) excludes swffs like $(qu(p_0)(c_0))(d_0)$ or $qu(p_0(c_0) \supset q_0(c_0))(d_0)$ --on the plausible ground that once an utterance is evaluated in a given context, such evaluation is not relative to some 'wider' context, though tokens of the same utterance-type may receive different values in different contexts. This second assumption requires the first for its explanation. For, it could be suggested that, in $qu(p_0(c_0) \supset q_0(c_0))$ (d_0) , though the contexts of 'p_0', 'q_0' are fixed by 'c_0', 'd_0' is required to fix the context of 'D'. The first assumption guarantees, however, that $qu(p_0(c_0) \supset q_0(c_0))(d_0)$ is equivalent to $qu(p_0 \supset q_0)(c_0)(d_0)$ --where the occurrence of 'do' is redundant.

A significant, statement-successful utterance of an indicative sentence in a context yields a statement. Statement-yielding utterances are a sub-class, therefore, of the class of swffs, defined above. Though membership of this sub-class is not fixed until the extension of the predicates 'Sig $qu(\emptyset)(\Theta)$ ' (for ' $qu(\emptyset)$ is significant in Θ ') and 'St (\emptyset, Θ) ' (for' what \emptyset states in Θ ' are determined (see: $\Pi; D(\mathfrak{S}), (\mathfrak{C})$, we introduce, in anticipation, a restricted class of u-variables and constants taking as substituends (in CL, always within a qu-context⁵) statement-yielding utterance-tokens:

restricted u-variables: r, s, r^1 , s^1 , restricted u-constants: r_0 , s_0 , r_0^1 ,

Restricted u-variables and u-constants are a subclass of those already introduced, so they satisfy the formation-rules Df Ia)-g). Though a value of qu(r) is statement yielding in a context, it would be wrong to identify r with the statement made in that context, for tokens s, s¹ of different types may make the same statement in that context, though distinct from r. Thus, the relation between statement-yielding utterances and statements is at least many-many, in a context. In fact, it is many-one. For, though different utterances may yield the same statement in a given context, there corresponds one and only one statement to each statement-yielding utterance in its context. I shall not argue further for this presumption, here-though, if different tokens of the same type are uttered in a context in which their 'aboutness' and significant content are the same, the requirement that what they state should be unambiguously true of false guarantees the many-oneness of the statement-yielding function. On the basis of this argument, one <u>could</u> introduce a primitive function, in CL, from utterance tokens to statements, represented by:

(8) St(Ø, Θ) (read: 'what qu(Ø) states in Θ' or 'the statement (x's utterance of)qu(Ø) makes in Θ'),

where substituends of \emptyset are restricted to significant, successful utterances (relative to θ). The individual values of such a function would, thus, be <u>statements</u>--mentioned by factive-clauses, which are abbreviated as <u>statement</u>-constants:

St-constants: a_0 , b_0 , a_0^1 , b_0^1 , ..., or which fall in the range of values resulting from substitution of specific

clauses for statement-variables:

St-variables: a, b, a¹, b¹,

A suitably defined equivalence-relation '= s ' <u>could</u> then be introduced to enable us to formulate statement-identities in CL as equations of the form:

(9) $a_0 = St(r_0, c_0)$ --which reads: 'that-a <u>is</u> what a significant, statement-making utterance of r_0 states in c_0 '.

Statements, so defined, would be abstract items: the value-range of statementvariables whose substitution-instances are that-clauses. So, if $'St(\emptyset, \theta)'$ were taken as primitive, we could define, formally, a two-place relation 'Y' from utterances to statements, by means of:

(10) $qu(\phi)(\theta) \bigvee \propto df (St(\phi, \theta) = s \propto) - -$

where the left-side reads 'an utterance token of a value of \emptyset in the context θ <u>yields</u> the statement that- α ', which holds when and only when what \emptyset states in θ is that- α .⁶

In CL, however, it is advantageous to reverse this procedure, to introduce 'Y' as a primitive dyadic relation, with 'St(\emptyset , θ)' becoming a defined function for the restriction of the left-field of Y to statementmaking utterances. The advantage accrues from the manner in which CL is carried over into the broader universe of discourse comprising nonsignificant, statement-failing utterances--with respect to which, of course, the function 'St' could be only partially defined. In particular, suppose we wish to affirm as a general thesis of CL the claim, adumbrated informally above, that an utterance in a context yields a statement if and only if it is both significant and statement-successful. This would have to be formulated as:

(11) (α=_S St(Ø, θ)) for some α iff qu(Ø) is significant and successful in θ.,

which is wholly inadequate. For we want to affirm that <u>any</u> utterance satisfies (11), but we have restricted utterances to substituents of ϕ , i.e. to statement-yielding utterances--thereby begging the question. The reverse procedure avoids this by taking 'Y' and St-constants as primitive and allowing that <u>any</u> swff, within the qu-operator, may appear in the lefthand argument-place of 'Y'. Then we can affirm, generally: for any swff ϕ ,

(12)
$$qu(\emptyset)(\theta) \forall \alpha$$
, for some α iff Sig $qu(\emptyset)(\theta)$ and for some α ,
 $\alpha =_{s} St(\emptyset, \theta)$.

Now, in case \emptyset takes a nonsignificant or statement-failing substituend 'qu(\emptyset)(Θ) $\forall \propto$ ' is simply false, for all \propto . Since 'Sig qu(\emptyset)(Θ) and for some \propto , $\propto =_{s}$ St(\emptyset , Θ)' is also false for such a case, both sides of the equivalence are false, and (12) trivially true. [This reflects the important

fact that, in CL, though some <u>values</u> of $qu(\emptyset)$ are nonsignificant or semantically unsuccessful--hence, they induce truth-value gaps in the domain of statements--it is always significant (and always a two-valued assertion) to affirm <u>of</u> a value of $qu(\emptyset)$ that it is significant or statement-successful. Thus, CL is essentially a two-valued meta-theory for the 'many-valued' or 'gappy' domain of statements and the utterances which yield them].

So far, the yields-relation 'Y' has been introduced informally as a many-one relation from utterances to statements (having 'singularity points' where it is undefined for some values of $qu(\phi)$). The substitution-class of the left argument-place of 'Y' takes swffs, in qu-contexts. Thus far, though, only primitive st-variables and constants appear in the right argument-place. It would be highly implausible, however, to suggest that to each atomic or compound swff of CL, which was significant and statementyielding in context, there corresponds a unique primitive value of a stvariable. We would then be committed to holding not only that, to each significant atomic utterance $'qu(p_0)'$, there corresponds the atomic statement '\$po' it yields; but also that, to each compound of atomic utterances- $qu(\sim p_0)$, $qu(p_0 \vee q_0)$, and so on--there corresponds the negative, disjunctive, conditional, statement each yields. But there are no negative, disjunctive, statements. There are no formulae of CL of the form '~ α ', '(α v β)' and so on, if only because st-constants and st-variables are terms, not sentences, and it makes no sense to negate or disjoin terms. In any case, should we suggest, per absurdum, that there is a primitive disjunctive statement $9a_0$ corresponding to $qu(p_0 v q_0)$ and a negated conjunction $19b_0$ ' to $qu(\sim(\sim p_0 \& \sim q_0))$, we would still want to explain the

truth-functional relationship between a_0 and b_0 in virtue of which qu(p₀ v q₀) and qu(~(~p₀ & ~q₀)) are interderivable (in standard, extensional contexts). It is essential, therefore, to exhibit what structural relationships hold amongst statements in the right field of the Y-relation. This is the task of the following sub-section.

(III) The semantic structure of the domain of statements:

If every sentence, uttered in a context, were to yield a true or false statement, and all syntactic operations on sentences were bi-valently defined, then, by well-known properties of truth-functional matrices for a sentential language S, the converse domain of the Y-relation (the domain of statements) would comprise a Boolean complemented, distributive lattice.7 Such a lattice structure has the property that any homomorphism h, from the Lindenbaum algebra of the set of sentences of S defines a valuation v over the lattice, such that, for any sentence \emptyset in S, $v(\emptyset) = h([\emptyset])$, where $[\emptyset]$ is the class $\{\emptyset : \emptyset \equiv \emptyset\}$ (the equivalence class of \emptyset). This property yields the familiar consistency and completeness results for a calculus S of S (say, the PM axiom-set, modus ponens and substitution), expressed in terms of the validity of a sentence \emptyset with respect to a class of Boolean algebras (with \mathcal{N}_{o} free generators). Thus, ϕ is valid in a class B of Boolean algebras iff, for every homomorphism h, into an algebra b' \in B containing greatest element 'l' (and least '0'), h ([\emptyset]) = v_h ' (ϕ) = 1. So is, then, consistent with respect to B iff every derivable sentence of S_0 is valid in each b' \in B. And S_0 is complete with respect to B iff every sentence valid in each b' \in B is derivable in S₀. Finally,

if there is a homomorphism from the Lindenbaum algebra of S_0 into the particularly simple two-element Boolean algebra b^2 (= $\langle \{0,1\}, \cap, U \rangle$, -, $\{1\}\rangle$ - with designated unit element) then the validity of each \emptyset in S_0 with respect to b_2 corresponds to the <u>decidability</u> of S_0 by standard truth-tables (\emptyset is derivable in S_0 when \emptyset is a truth-functional tautology); i.e. for every $\emptyset' \in S$, \emptyset' is either derivable, or truth-functionally refutable by assignment of 0, resp. 1, to finitely many of \emptyset' 's component variables. (This description is distilled from Curry, <u>1963</u> (2nd edition, 1977), Ch. 4).

I have allowed, however, that in some contexts an utterance may fail to yield a statement. Since we have noted already that the Yrelation is many-one between utterances (in context) and statements, it follows that the Y-relation is a function with singularity points for some arguments--points corresponding to 'gaps' in the truth-value assignments to statements in the range of Y. Allowing such 'gaps', it is natural to ask whether the semantic structure of a logic (CL) of statements--one which is <u>not</u> the truth-functional image of sentential calculus--can be characterized algebraically, in a manner analogous to the familiar Lindenbaum algebra for S_0 .

First, it is necessary to specify the morphology and syntax of CL more precisely--and to comment upon some of the intuitions one wants this structure to capture. As noted, the system I set out--CL--is not really new⁸, but my approach to its formalisation and interpretation is quite different. The basic idea is to set the domain of statements as <u>values</u> of individual constants and variables of a first-order language, and

introduce defined predicates for the semantic properties (truth, statementidentity, incompatibility) in which we are interested.

Formally, CL is a three-sorted predicate logic with primitive non-logical operators (qu(-))', $(g(-))'^9$ and Y':

(i) the argument-set of qu(-) comprising the class of swffs, defined above, indexed to;

(ii) the class of context-constants and context-variables ranging over sets of contextual features. (So, substituends of c-variables are contextual <u>descriptions</u>--sets of statements whose truth-conditions are constitutive of those features which contribute to the significance and success of utterances);

(iii) the class of restricted u-variables and constants--a sub-class of (i)--which are argument-expressions to a unary operator g(-), which, from a significant, statement-making utterance in a context, forms a wellformed clause. Such clauses--referentially determinate factive-clauses-form the substitution-class of (iv);

(iv) individual st-variables and constants.

From (i) - (iv), the well-formed formulae (wffs) of CL are defined

as follows:

(Df.III): Given the disjoint classes:

- a) <u>Swff</u> comprising the set S of swffs satisfying Df.I, above,
- b) Cx comprising c-variables and constants,
- c) St comprising st-variables and constants, then:

(i) the set \underline{Qu} is the expression-range of the qu-operator defined over \underline{Swff} s.th. for every \emptyset in \underline{Swff} , $qu(\emptyset) \in \underline{Qu}$. (\underline{Qu} is the set of 'quotation-expressions').

(ii) Y is the dyadic relation defined over $((\underbrace{Qu} \ U \ Cx) \ U \ \underline{Wfc})$ s.th. if $\underbrace{Qu} \ x \ \underline{Cx}^n$ represents the set of expressions in \underbrace{Qu} , indexed to each member c_i (if a_n) of \underbrace{Cx} , then the field of Y (domain and converse-domain) is the least set ($(\underbrace{Qu} \ x \ \underline{Cx}^n) \ x \ \underline{Wfc}$) of ordered pairs of expressions whose first member is in $\underbrace{Qu} \ x \ \underline{Cx}^n$ (for example: ' $qu(p_0)(c_0)$ '), and whose second member is in \underline{Wfc} ,

(iii) $\S(-)$ is a unary operation on $S_r \subseteq \underline{Swff}$ which comprises the subset of \underline{Swff} formed by restricting Df.I to substituends of restricted u-variables and constants. $\S(-)$ takes argument-expressions in the set S_r to value-expressions in the least set \underline{Wfc} (well-formed clauses)

containing St and s.th., if $a \in S_r$, then $Sa \in Mfc$.

(iv) Let θ, η be schematic for members of Cx, \emptyset , \emptyset for members of Swff, then the set Term of atomic terms of CL is the least set containing all pairs $\langle qu(\emptyset), (\overline{\theta}) : \emptyset \in Swff \& \theta \in Cx \rangle$. Notice that atomic terms are not well-formed clauses, and that I shall frequently omit the pair-brackets $\langle \cdot, , -- \rangle'$ from such terms.

(v) Let $\boldsymbol{\alpha}$, $\boldsymbol{\beta}$ be schematic for members of <u>Wfc</u>, then, for \emptyset in <u>Swff</u>, θ in <u>Cx</u>, the class of <u>Wffs</u> of CL comprises:

- a) for γ an atomic term, Sig(γ) is a wff of CL (reads: " γ is significant"),
- b) for $\boldsymbol{x}, \boldsymbol{\beta}$ in <u>Wfc</u>, $\boldsymbol{\theta}$ in <u>Cx</u>, $(\boldsymbol{\alpha} =_{\mathbf{S}} \boldsymbol{\beta})(\boldsymbol{\theta})$ (reads: "that- $\boldsymbol{\alpha}$ is the same statement as that- $\boldsymbol{\beta}$ "), Tru($\boldsymbol{\alpha}, \boldsymbol{\theta}$) (reads: "that- $\boldsymbol{\alpha}$ is true in $\boldsymbol{\theta}$ ") Fal($\boldsymbol{\alpha}, \boldsymbol{\theta}$) (reads: "that- $\boldsymbol{\alpha}$ is false in $\boldsymbol{\theta}$ "), $(\boldsymbol{\alpha}/\boldsymbol{\beta})(\boldsymbol{\theta})$ (reads: "that- $\boldsymbol{\alpha}$ is incompatible with that- $\boldsymbol{\beta}$ ") are wffs of CL.
- c) where γ is a term, \prec a wfc, $(\gamma Y \alpha)$, $(\forall \alpha) (\gamma Y \alpha)$ and $(E \alpha) (\gamma Y \alpha)$ are wffs of CL--provided no subswff of any swff \emptyset in γ contains a c-variable or constant distinct from a c-variable or constant indexed to $qu(\emptyset)$ in γ (prevents "iterated contexts").
- d) where A, B are wffs of CL and α a wfc, (\neg A), (A V B), (A \land B), (A \rightarrow B), (A \rightarrow B), (A \rightarrow B) and ($\forall \alpha$) A are wffs of CL.
- e) free and bound occurrences of st-variables in wffs of CL, and the closure of wffs are defined analogously to Df.I(g).

With respect to this definition, and Df.I, notice that though both swffs and wffs may be syntactically composed (by connectives), a different style of notation is required for compound wffs from that of swffs. This requirement reflects the fact that some swffs contain nonsignificant or unsuccessful components--truth-functional connectives are non-standard ('undefined') for such compounds. On the other hand, CL is, itself, twovalued (every wff is either true or false), which follows from the fact that it is always significant (though false') to assert significance of a nonsignificant swff (and <u>true</u> to deny it). Similarly, to assert of a statement-failing utterance that it yields a statement '(ξ_{α}) ($\gamma_{Y\alpha}$)' is false (because no statement is yielded). It is assumed, also, at this point, though discussed further, below, that identity-statements <u>about</u> statements are always two-valued. That is, when ' \neg (E_A) (γ YA)' is true, then '(E_B) ($\alpha =_{s} \beta$)' is false, not undefined.

Secondly, the only primitive relation holding between statements (other than $'=_{s}'$) is <u>incompatibility</u>--represented by '/', an analogy of the Sheffer stroke-function¹⁰. It has been argued already that, because statements are not syntactic objects, syntactic connectives are not defined over them. I shall offer reasons, now, for the choice of 'incompatibility' as a primitive statement-relation.

Statements are the values of quantifiable individual variables in CL. Under a referential reading of the quantifier, this represents a bare committment to statements as 'logical' objects--not, certainly, as 'syntactically compound' entities, but, through their properties and relations, as the bearers of truth-values. It is definitive of statements, that is, to be true or false; the relations which hold between them hold in virtue of the circumstances of their being true or false. I would argue that this committment is eliminable--it is, in any case, disguised by the metatheoretic nature of CL, which mentions utterances and statements through a substitutional reading of quantification. Contrary to some claims made on behalf of the substitutional interpretation, however, this does not dispense with an existential committment to statements, largely because the substitution-class of st-variables in CL (Wfc) comprises referentially determinate, irreducible singular terms (factive clauses). Nevertheless, though I will not pursue the point, here, it might be argued that this committment is minimised

through the analysis of statements as the outcome of successful illocutions (in Section B).

The essential connection of statements with the circumstances of their being true or false explains the choice of 'incompatibility' as a primitive relation between statements. To take a trivial example:

Suppose I state that a single spin of the coin in my pocket will come down heads. Since I am (supposedly) making this truth-claim in a relatively standard context c_s , one can say the statement §a I make in uttering qu(A single spin of the coin in my pocket will come down heads) (cs) is made true or 'confirmed', if there is some time t', after now to, at which I spin the coin in my pocket and it comes down heads. My statement is made false or 'disconfirmed', obviously, if there is no such t'. For the purposes of CL, it makes no difference whether we say it is the fact at t' of the coin's having come down heads, after spinning, or the event occurring at t' of the coin's being spun and coming down heads, or even some more complicated relation between coin, angle and velocity of spin, and so on. All that concerns my truth claim §a is that, whatever makes my statement true at t', there is nothing prior to t_0 or between t_0 and t' which precludes §a's being made true at t'. This is simply to say, though, that, if there is no statement whose truth at t_0 or between t_0 and t' is incompatible with whatever makes atrue at t', and my utterance yields the statement §a, then §a is true at t'.

All this is straightforward enough: what is incompatible with Ga's being made true at t', in the simplest sense, is either my spinning the coin at t' and its not coming down heads, <u>or</u> my not spinning the coin

at any t' after t_0 , or even that I have <u>no</u> coin in my pocket at t_0 , or lose the coin between t_0 and t'. Then, with respect to whatever makes my statement §a true, relative to the context, there are disjoint sets of statements whose truth is either compatible or incompatible with the circumstances of §a's being true. (I shall call these sets of statements "compatibility-", respectively "incompatibility-sets" with respect to §a.)

It would trivialise the notion of 'context' to allow that <u>every</u> statement is either in §a's compatibility- or incompatibility-set--since each context would then be represented by the union of the whole domain of statements (i.e. there would be only one context). To counter this, I shall introduce below a refinement of the basic incompatibility relation, called "relevant incompatibility" (relative to context). For the moment, however, I want to concentrate upon the unexplicated notion of a statement's being made true (false).

The example I gave is of a contingent statement, yielded by a <u>tensed</u> utterance--a statement which can (logically) be true or false, and whose truth or falsity is, <u>inter alia</u>, dependent upon some relation holding between the time of assertion and the time of the statement's being made true. A contingent §a, in this sense, has the property that its disjoint incompatibility- and compatibility-sets are asymmetrical with respect to the circumstance of §a's being made true. That is, if $\S b_i$ is any statement in §a's incompatibility-set $\{b_j: (b_j/a), \text{ for each } j = 0,1,2,...,n\}$, then $\$ b_i$'s truth falsifies §a. On the other hand, where $\$ c_0,...\$ c_n$ are <u>all</u> the statements compatible with \$ a--save for \$ a,

itself--every §c_i may be true, yet §a need not be true, unless what makes it true obtains. This asymmetry does not hold for tautological or contradictory statements: where §t is a tautology, the set of statements incompatible with §t is an equivalence class of contradictions, none of which can be true. On the other hand, §t's compatibility-set consists of every statement, bar these. (It turns out, below, however, that the "relevant compatibility" set of a tautology comprises only its tautological equivalents).

How is this basic notion of a statement's <u>being made true</u> to be explained? I propose to characterise it in terms of the manner in which truth is defined in the semantics of first-order languages.¹¹ Recall that CL is an applied first-order logic and that in the standard interpretation of a first-order language, truth is defined for sentences, relative to a model-structure and an assignment from the domain of the model to variables and constants in the language. A <u>model-structure</u> m is a triple $\langle D, R, d \rangle$, where D is a non-empty set (intuitively, items which referential expression in the language are 'about'), R assigns to each n-place predicate F^n an n-ary relation on D^n --the n-th Cartesian power of D--(intuitively, R assigns to each F^n a subset of ordered n-tuples-the <u>extension</u> of F^n in D), and d assigns to each term of the language an element of D, s.th.:

 $R(F^n) \subseteq D^n = \{ \langle d(\xi_1), \ldots, d(\xi_n) \rangle : d(\xi_i) \in D \}$ and $d(\xi_i) \in D$ if ξ_i is a constant or variable of the language.

A <u>valuation</u>, relative to m, is a pair $\langle v, w \rangle$ of functions such that w assigns to each variable or term in the language a member of D (for CL, $\langle v, w \rangle$ would be a level preserving basis assignment, see: Appendix B, and v assigns to each closed wff of the language either 1 ('true') or 0 ('false'). Thus, each valuation $\langle v, w: v: \underline{Wff} \rightarrow \{0,1\}$ and $w: \underline{Term} \rightarrow D \rangle$ is the unique extension of the basis satisfying:

> w(x₀) = d(x₀), if x₀ is a constant,
> v(Fⁿt₁,..,t_n) = 1 iff ⟨w(t₁),..,w(t_n)⟩ ⊆ R(Fⁿ), if t₁,..,t_n ∈ Term,
> v(t_i = t_j) = 1 iff w(t_i) = w(t_j),
> v(~Ø) = 1 iff v(Ø) = 0,
> v(Ø v Ø) = 1 iff v(Ø) = 1 or v(Ø) = 1,
> v((∀x)Ø) = 1 iff v' (Ø) = 1, for all ⟨v', w'⟩≈x ⟨v,w⟩,-i.e. iff v'(Ø) = 1, for all valuations differing from ⟨v,w⟩ at most at w'(x).

<u>Truth in</u> m, relative to valuations, is, thus, defined for a formula of the language if, for some $\langle v, w \rangle$, $v(\phi) = 1$. This recursion ensures that truth in a standard model of a first-order language is <u>completely defined</u>, i.e. every sentence yields a true or a false statement. In CL, however, some sentences may fail to yield true or false statements. This leads us to consider, instead, <u>partial models</u> of the swffs of CL, i.e. v: <u>Swff</u> \rightarrow $\{0,1\}$ may be undefined for some swffs, and w:<u>Term</u> \rightarrow D may be undefined, relative to some $d(\xi_i)$.

I will first set the semantics for CL formally, and then comment upon the resulting structure:

Relational semantics for CL:

Again, let a model-structure $m = \langle D, R, d \rangle$ consist of a (non-empty¹²) domain D; but, now, R assigns to each n-place atomic predicate F^n (in a swff of CL) an <u>ordered pair</u> $\langle R_i(F^n), \overline{R_i(F^n)} \rangle$ of disjoint n-ary relations on D, and d assigns to <u>some</u> terms (subject-constants) of CL, elements of D (d is now undefined for some closed expressions). Obviously, we cannot have v: $\underline{Swff} \rightarrow \{0,1\}$ for all (closed) swffs of CL, as before, so we must permit v to be undefined for some swffs. Similarly, since d(ξ) is undefined for some constants $\xi_1, \xi_2, \dots, w: \underline{Term} \rightarrow D$ is only partially defined.

As before, a <u>valuation</u> $\langle v, w \rangle$ in m, for each swff \emptyset , \emptyset in CL, on a level-preserving assignment $\langle R, d \rangle$, is defined by the clauses:

(Notice that $v(G_x)\phi$) can never be undefined; i.e. " ϕ 's exist" or "at least one thing ϕ 's" is always either true or false statement-yielding).

(11) If $w(t_1), \ldots, w(t_n)$ are all defined, then $v(F^n t_1 \ldots t_n) = 1$ iff $\langle w(t_1), \ldots, w(t_n) \rangle \in \mathbb{R}(F^n)$, $v(F^n t_1 \ldots t_n) = 0$ iff $\langle w(t_1), \ldots, w(t_n) \rangle \in \mathbb{R}(F^n)$.

(complementation in the semantics of CL is not, in general, Boolean; i.e. $'x \in y \neq x \notin y'$).

(12) If w(t_i), w(t_j) are defined, v(t_i=t_j) = 1 iff w(t_i) = w(t_i), v(t_i=t_j) = 0 iff w(t_i) ≠ w(t_j).
(13) If v(Ø) is defined, then v(Ø)(Θ) = v 1Θ_m(Ø), otherwise v(Ø)(Θ) is undefined.

((13) is not discussed in this Section. See Section D, pp. 627-30 for explanation of the notation 'v $1\theta_m$ '--"the function v restricted to θ , relative to m").

I shall comment upon some features of clauses (1) - (12); (1)-(2)Subject-arguments and constants:- despite their surface syntactic similarity, there are important semantic differences between the classical treatment of individual variables and constants in predicate logic, and their treatment in swffs of CL. In allowing the d-assignment (for "designation") to be undefined for some constants, by clause (2)--though defined for all argument-variables---the class of subject-terms of CL is considerably wider than that of first-order predicate logic. Substitutions for the free individual variables of predicate logic are usually restricted to names which denote actually existing items; all constants are assumed to denote independently of contextual determination (or, they are assigned "don't-care" values, e.g. the null set, in cases of referential failure: c.f. Introduction, p. 29). In CL, however, except for the exclusion of quantified subject-expressions ("all men", "some men", "any men", "no men"), any expression functioning as a singular, or particular-specifying term is admitted as a substituend of an argument-variable. Thus, amongst subjectarguments will appear: pronouns 'he', 'she',...; demonstratives 'this', 'that',...; proper names 'Pegasus', 'Rene Levesque',...' indefinite descriptions 'a table', 'a particular book,...; definite descriptions 'the King of France', 'the train at platform 3', ...; plural descriptions 'the

Martian chronicles', 'citizens of the U.S.'..; class descriptions 'the class of non-self-membered classes', 'the class of subject-terms'...; common nouns 'man', 'copper sulphate',...; abstract nouns 'number', 'temperance',...; adjectival nouns 'red (as in "Red is a colour"), 'square'...

Including such terms does not exclude the possibility that complex terms may be analysable in various ways (for example, singular definite descriptions as indefinite descriptions with identity--in the Russellian manner). Such analyses, however, explicate only the truth-conditions of sentences in which such expression occur; whereas my focus in CL is upon the function or rôle of such expressions in purported statement-making illocutionary acts. Thus, for example, though it may be logically perspicuous and, in some sense, correct to claim that qu(The King of France is bald) is true when and only when some bald thing is a king of France and every king of France is identical to it; nevertheless, qu(The King of France is bald) may be illocutionarily successful (statement-yielding) in a context only when qu(the King of France) is 'about' some item (is being used to refer to some item) and whatever it is about, in that context, lacks hair. The existential--that some unique thing is a king of France--may be presupposed, of course, by qu(the King of France)'s being 'about' an item in a context; hence, by the utterance's being successful, but that an utterance's presuppositions are fulfilled does not, alone, entail its illocutionary success. A use of such an expression is, thus, a referential, particular specifying use and the conditions under which it is successful, in a context, will be different from

(though intimately related to) the conditions under which the statement yielded is true or false. This is just to allow, however, that, though analysis may reveal the truth-conditions for a particular expression or sentence, there remains the question of the assertibility- or significanceconditions upon a <u>use</u> of that expression (utterance of the sentence) in a context. In CL, of course, my concentration is upon this latter question.

Admitting <u>any</u> expression that functions as a grammatical subject into the class of subject-terms, however, entails abandoning the Frege/ Quine criterion for demarcating between subject- and predicate-expressions, based upon the autonomous referentiality of 'names', their replacability by quantifiable variables in formal languages, and contrasted with the incomplete, 'unsaturated' nature of predicate-expressions. (Frege's theory of the subject-predicate distinction is discussed, above, in Part I, Sect. B). Questions concerning the adequacy of their criterion, and of alternatives to it, have generated a wealth of discussion in the literature of logical theory; so much that I cannot hope to do justice to the complexity of the issue, here.¹⁴ I shall be content, instead, to describe some of the problems involved in drawing the distinction.¹⁵

Current logic holds that schematic representation of a class of simple, declarative sentences by the forms "Fx", "Gxy", and so on, depicts a 'basic combination' in which a union of two different sorts of expression, or, at least, of expressions having two different kinds of rôle, forms a complete sentence--known as "subject-predicate", or as the form of "predication". One problem is to explain the general nature of this duality of role; that is, the complementary nature of the expressions

combined in predication. Another problem is to provide effective, or, at least, workable criteria by means of which to separate subjectexpression(s) from predicate-expression in a sentence.

In some writings, these two problems are fused in the claim that this or that criterion (or set of criteria) is, of itself, explanatory of the duality--since the criterial characteristics, themselves, <u>define</u> what it is to be a subject in combination with a predicate. Some such fusion is involved in the claim that demarcation between the two kinds of expression is grounded in some extra-linguistic difference between the kinds of entity for which the expressions stand (refer to, signify). For such a claim, it seems to me, it remains an open-question whether our recognition of a characteristic difference between kinds of entity <u>explains</u> the manner in which the subject-predicate distinction is drawn, when no independent account of the separate natures of the extra-linguistic items, concerned, (substance/attribute, object/concept, individual/property, particular/universal) is given.

A similar fusion is involved, it seems, in the quite contrary claim that the manner in which the distinction is drawn in syntax is, itself, <u>definitive</u> of the difference between subject and predicate (analogous to the claim, for example, that the manner in which we <u>count</u> is definitive of the nature of the natural number series), concluding that no <u>further</u> difference (between extra-linguistic items) should be inferred. For, with respect to this second claim, it remains an open question, again, why such and such a grammatical <u>practice</u> (separating noun-phrase from verb-phrase) should reflect the logical status of kinds

of expressions, or expression-roles, in virtue of which the predicate is appropriately correlated with the grammatical form of the verb-phrase, and the subject with the noun-phrase.

For some (one thinks of Carnap, <u>1948</u>, Quine, <u>1960</u>), the response to this "Open Question" argument involves only a methodological precept--that our grammatical or syntactic descriptions should reveal as much logical structure in the language as our formal explications of syntactic or semantic concepts require. Quine, in particular, has sought to reinforce this response (in Quine, <u>1970</u>, pp. 95ff; and "Reply to Strawson", Davidson and Hintikka, <u>1969</u>, pp. 320-5), through an appeal to his doctrines of the indeterminacy of translation and inscrutability of reference--first discussed above, in Sect. B. Taking up this issue again, as promised, I shall comment briefly upon the force of this appeal--it is notoriously difficult to determine what force the indeterminacy arguments have, in any particular case--concluding that, with respect to the problem of demarcating between subject and predicate, Quine's claim is either trivial or false.

Quine argues: (Davidson and Hintikka, 1969, pp. 320),

"The crucial thing about the position of 'x' in 'Fx' is that it is accessible to quantification...Pronominal cross-reference is the prototype of quantification... However, I argued in <u>Word and Object</u> that objective reference is subject to the indeterminacy of translation. This indeterminacy invests the whole peculiarly referential apparatus of quantification, pronouns, identity, predication and the distinction between singular and general. This whole apparatus, and with it the ontological question, itself, is in this sense parochial; it is identifiable in other languages only relative to analytical hypotheses of translation which could as well have taken other lines." In some respects, Quine's argument, here, represents an abandonment of the problem. The distinction, he claims, between subject and predicate is clear within our own language, or its suitably formalised fragments--relative to more or less arbitrary analytic hypothesis (concerning what basic syntactic categories there are, what forms of sentences). What the distinction lacks is applicability to all languages--ruled out by the empirically underdetermined nature of our translations.

Can it make sense to say that, though we distinguish well enough (by whatever means) between subject and predicate in our own language, we typically fail to determine these categories in other languages? The enterprise of radical translation, Quine holds, requires the linguist to formulate hypotheses about the syntactic forms of native speakers' sentences in the absence of sufficient information as to the sense or reference of natives' words or phrases. One can grant, quite easily, a degree of inductive uncertainty attaching to the linguist's syntactic description-but Quine argues for more than this: he claims that such descriptions will be irredeemably 'parochial' conditioned by the linguist's need to correlate native sentences with those of his own tongue. It is thus that the linguist invests the native language with a syntax analogous to his own. Yet, what further could be demanded of a translation? Quine suggests that simultaneously determining the references of pronouns, singular and general terms, the range of quantifiers, extensions of predicates in the language together with the basic syntactic forms of the language is forever beyond the translator. This would follow, however, only if the sole means of fixing upon the syntactic categories of subject and predicate

available to the linguist were confined to determining the references of terms and the referential ranges of quantifications. This, however, is to beg the question of formulating some non-arbitrary means of separating subject from predicate, <u>without</u> appeal to the accessibility to quantification of variables standing in for subject terms (Quine's <u>own</u> mark of the distinction). There may not be any such non-arbitrary means of drawing the distinction (see below), but to claim that there <u>cannot</u> be such a means, because of its dependence upon referential features subject to translational indeterminacy, is to give up the quest before it is begun.

Others, who have not tied the distinction to the methodological and epistemological difficulties of translational accuracy, have responded to the "Open Question" argument in a quite different way. For example, Russell (particularly in his <u>1918</u> exposition of Logical Atomism) responds by claiming that the distinction between subject and predicate reflects a deep-seated feature which the logical analysis of the propositions which sentences express reveals--that is, that analysis terminates in irreducibly atomic propositions composed of simple objects.

To contrast these claims, however, is only to sketch in outline the complex problems to which the issue gives rise--I shall take the matter no further, here. Independently of these claims, however, various <u>marks</u> of the formal difference between subject and predicate have been described-some of which are set out, below. None of the following seem to me to be adequate--for the reasons given--though I do not rule out the possibility that some combination of them may suffice.

One such mark of the difference between subject and predicate

(expressions) purports to be a wholly <u>syntactic</u> criterion of demarcation:¹⁶ that it is logically coherent to form the negation of predicate-expressions but not of subject-expressions. It should be observed at once that standard expositions of predicate logic adopt only <u>sentence-negation</u> as a syntactic connective (correlated with a truth-functional operation); but this does not preclude our enriching the logic with a negation operator defined over predicates, of the form:

(N) $\overline{F}(x_0) =_{df} \sim F(x_0)$, i.e. " x_0 is non-F" is true when and only when it is not the case that x_0 is F.

Considerations of syntactic uniformity seem to require that (N) should be extended to include non-atomic predications, in accordance with:

- (N') $(\overline{F \& G})(x_0) =_{df} \sim (F(x_0) \& G(x_0)),$
- (N") $(\overline{F \vee G})(x_0) =_{df} \sim (F(x_0) \vee G(x_0))$, and so on.

A simple argument (see Strawson, <u>1974</u>, p.7 and Wilson, <u>1959</u>) then suffices, apparently, to show that we cannot coherently enrich predicate logic with a device for forming the negation of subject-terms. For, suppose we have the conjunction:

(a) (F(x) & G(x)).

By double negation, this is equivalent to:

(b) $\sim (\sim (F(x) \& G(x))),$

which, by forming the negated conjunctive, by (N'), is:

(c) $\sim ((\overline{F \& G})(x)).$

Supposing 'x' to represent the negation of the subject-term,

(d) $(\overline{F \& G})(\tilde{x})$,

which seems to require expansion (by (N')) into:

(e) \sim (F & G)(\tilde{x}).

Uniformity of negation for subject terms makes (e):

(f) \sim (F(\tilde{x}) & G(\tilde{x})),

which, one supposes, is equivalent to:

(g) $\sim (\sim F(x) \& \sim G(x))$,

whence, by De Morgan's law:

(h) (F(x) v G(x));

whereas (h) is obviously not equivalent to (a).

Three observations on the argument in support of this criterion (that negating a predicate is logically coherent, whereas negating a subject is not) demonstrate its inadequacy:

(i) At best, the argument shows only that we cannot coherently frame a logic admitting <u>both</u> subject and predicate negation <u>and</u> retaining classical theorems for <u>sentence</u> negation (De Morgan's laws and double negation). No reason follows, however, for admitting <u>one</u> kind of negation rather than another.

(ii) The argument accepts implicitly the cogency of forming compound predications ('(F & G)(x)', '(F v G)(x)') from compound sentences--ignoring that a similar argument (given by Strawson, loc. cit., p. 6), <u>not involv-ing negation</u>, demonstrates an analogous incoherence in their acceptance.

[From: (a) (F(x) & G(x)) v (F(y) & G(y)), infer (b) (F & G)(x) v (F & G)(y), and (c) (F & G)(x v y). But (c) seems to be expandible into: (d) F(x v y) & G(x v y), whence, (e) (F(x) v F(y)) & (G(x) v G(y)), whereas: (f) ((p & q) v (r & s)) ≢ ((p v r) & (q v s)).]

(iii) The feassibility of constructing coherent logics permitting subject

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or predicate negation is, of itself, no clear indicator of the grounds, or lack of them, for demarcating between subject and predicate in this way. That some classical theorems may not be derivable in such a logic enriched with non-classical connectives is hardly surprising in view of the close relationship between the expressive power of a formal language (the number and kind of primitive and defined operators within it) and the deductive strength of its axiomatised theories.

A second mark of the subject-predicate distinction is sometimes suggested in the contrast between holding that a subject-predicate sentence is true if its predicate is <u>true of</u> (or 'applies to') what its subject expression refers to; but not conversely--a subject is not <u>true</u> of whatever predicate is asserted of it. As much is suggested by Quine's:

"The basic combination in which general and singular terms find their contrasting roles is that of predication... Predication joins a general term and a singular to form a sentence that is true or false according as the general term is true or false of the object, if any, to which the singular term refers." (Quine, <u>1960</u>, p. 96).

It is clear that, for all that this paraphrase may 'signal' a difference between subject and predicate, it does little to explain it, since it brings us back to the notions of 'reference', of what an utterance is 'about' in a context. As Strawson points out (Strawson, 1961, p. 74):

"... the difference in force between the expressions 'is true of' and 'refers to' calls as loudly for explanation as the expressions 'general term' (term in predicate position) and 'singular term', themselves.. What is the characteristic difference between the relations of the two terms to the object?"

At this point, Strawson answers his own question by introducing three further marks of the distinction (which I shall discuss shortly): a) "The characteristic difference is, I suggest, that the singular term is used for the purpose of <u>identify-</u> <u>ing</u> the object ... while the general term is not." (ibid. p. 74),

to which Strawson subsequently adds the following--which I paraphrase:

b) that failure of a predicate to apply to the relevant item (referred to by the subject-term) simply results in a false statement; whereas failure of the subject-term to refer generates a 'truth-value gap' (ibid., p. 74);

c) that two terms coupled in an utterance yielding a true statement stand in referential (subject) and predicative positions, respectively, if what the first designates (is 'about', in a context) is a case or instance of what the second signifies ('states of' what it is about). Items thus combined, or the expressions designating them may be said to be of lower and higher <u>type</u>, respectively; i.e. the term in predicate position typically supplies a 'way of grouping' items picked out by a singular term (ibid. p. 83).

I postpone consideration of a)-c) to discuss, briefly, Quine's rejoinder to Strawson's objection (in Quine, <u>1970</u>, pp. 62, 95):

"... the mark of a name (subject-term) is its admissability in positions of variables."

and,

"When we schematise a sentence in the predicative way 'Fa' or 'a is F', our recognition of an 'a' part and an 'F' part turns strictly on our use of variables of quantification; the 'a' represents a part of the sentence that stands where a quantifiable variable could stand and the 'F' represents the rest."

Quine's reply is, then, that there is <u>no</u> way of distinguishing the parts which enter into the 'basic combination' of predication, <u>except</u> as the parts one of which occupies a position where a universally or existentially quantified variable could stand, the other of which does not.

What is mysterious in Quine's reply is not so much its truth or

falsity, but why the reply should be supposed to be <u>relevant at all</u> to the issue of demarcating within language between subject and predicate. It is clearly unassailable that, if we stipulate, as in the first quotation, that subject-terms are <u>distinctive</u> in being in positions accessible to quantification, then our recognition of a distinction between subject and predicate "turns strictly upon our use of variables of quantification". Equally clearly, it contributes nothing to the issue to make such a claim. For, it is only in the formally austere languages of predicate logic (of first-, or higher-order) that content can be given to the claim that variables stand in positions accessible to quantification (there are <u>no</u> variables in natural language); and for such artificial languages, the syntactic division of primitive symbols into <u>individual</u>-, as opposed to <u>predicate</u>-, variables and constants is legislated already in the formationrules--no <u>mark</u> of the difference is necessary. There is no point in questioning such a stipulated feature, and no need to defend it.

More charitably, suppose we construe Quine as claiming of <u>natural</u> languages (say, English) that a mark of the distinction between subject and predicate expression is that the former appear in positions which <u>instantiate</u> occurrences of English quantifier phrases ("all", "some", "any", "whatever", ..). Then, the claim is simply false. A natural language like English does not share with formal quantificational languages the characteristic heirarchy of "orders"---whereby quantifying only over individual variables is permissable in first-order languages, only over predicates of individuals in second-order, and so on. So, it is equally the case that:

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(1) Tom runs if William runs.,

is as good an instance of the quantified:

(2) Tom does whatever William does. (Strawson's example),as:

(3) John runs.,

is an instance of the quantified:

(4) Everyone runs.

Though the semantics of such sentences differ, quantification into the predicate position seems as much a feature of the grammar of (2), as quantification into the subject position is of (4). No distinction is marked, therefore, by Quine's reply.

To return to Strawson's suggested criteria a)-c): Suggestion a): that subject-terms are distinguished by their characteristic use in identifying objects, requires that we abandon the hope of marking the distinction in a purely syntactic manner. For, the appeal, here, is to the role of the expression in <u>use</u>, and to the kind of entity with which it is semantically correlated. Given some independent manner of characterising 'objects', the suggested mark would suffice. In Sect. D, it will turn out, however, that the notion of an expression's referential use (of what an utterance is 'about') can be explained without appeal to some autonomous category of 'objects'.

Suggestion b) is of little avail. As has been maintained throughout this thesis, utterances whose subject-terms fail to refer, in a context, do indeed fail to yield true or false statements--but, it has been claimed, above, so do predications which are nonsignificant with respect to the

items they are about. Of <u>significant</u> utterances it is certainly a mark of the difference between their subject- and predicate-expressions, that when a predicate fails to apply to an item of which it is significantly predicable, the statement yielded is false; whereas referentially failing subject-terms induce truth-value gaps. Of <u>nonsignificant</u> utterances, however, failure to yield a true or false statement may be attributed either to referential failure (through 'nonsignificance'), as in:

(5) my father moved through depths of height. (e.e. cummings), or to a sortal mismatch of predication:

(6) Literature slices boredom.

Suggestion c) provides by far the most interesting candidate for a criterion distinguishing subject 'particular-specifying' expression and predicate 'grouping or classifying' expressions. It is of interest primarily because it provides the first hint as to how the general discussion of contextual significance, 'aboutness' and statementsuccess is to be tied eventually to the discussion of the theory of lgoical types with which this thesis began. On its own, however, it is simply insufficiently precise. (What is it, for example, for something to be a 'case' of 'instance' of something else?). It is the task of the first part of Section D, below, to remove this imprecision.

In sum, the remarks above lead to a negative conclusion: that, individually, the supposed marks of the distinction between subject and predicate do not provide an adequate means of demarcating, within a language, between the categories of referential and predicative expressions. How problematic this negative conclusion is for the distinction in CL between subject and predicate parameters is difficult to judge. When the d-assignment function for CL is undefined for nonsignificant or referentially failing subjectterms, it appears to follow that no purely referential criterion for the distinction could suffice. On the other hand, none of the proferred syntactic criteria are adequate. I suspect that a solution to the problem may eventually be found to lie in an investigation of distinctions variously formulated in empirical linguistics as between the topic and focus of a sentence functioning in a context (Chomsky, 1968, repr. in Studies on Semantics in Generative Grammar, The Hague, 1972, pp. 62-119), contextually bound and unbound sentence-segments, (Sgall, 1972, Philologica Pragensia, 15, pp. 1-14), and topic and comment in functional sentence perspective (Hajicova, 1973, Philologica Pragensia, 16, pp. 81-93). Such an investigation, however, lies beyond my scope at present.

I return now to the discussion of the more problematic clauses in the description of partial models for the swffs of CL:

By (3), the negation of a swff yields a truth (falsity) if and only if the swff yields a falsity (truth), otherwise no true or false statement is yielded. This gives the matrix for $qu(\sim \emptyset)$, (where 'u' represents <u>absence</u> of a value):

| ø | ~ |
|---|---|
| 1 | 0 |
| u | u |
| 0 | 1 |

For a nonsignificant swff, then, neither it nor its negation is statementyielding, e.g. both:

(1) Rational numbers like soup.

and

(2) Rational numbers dislike soup.

fail to yield statements. It might be suggested, however, that, in another sense of negation:

(3) It is not the case that rational numbers like soup, is a true denial that numbers cannot be said to have gustatory preferences. This thesis--called " the Sneg argument " in the discussion of Prior and Ewing's accounts of 'Category-mistakes' in Part I, Sect. D --was eventually rejected through consideration of the differences between 'negation' and 'denial of significance'. In CL, however, accepting that a negated swff is non-statement yielding if the swff is, has formal consequences which may appear damaging to its claim to <u>supplement</u> classical logic (whilst preserving logical truth and validity). In particular, CL may appear susceptible to two arguments which threaten the semantics adopted. In this subsection, I shall rebut these arguments. (The first is due to Rescher, <u>1969</u>; the second derives essentially from Aristotle <u>Metaphysics</u> Γ , Ch. 7, 1011b, 25-28);

Argument A: The classical semantics for sentential calculus, as noted at the start of this section, is the two-element Boolean algebra $\langle \{0, 1\}, v,$ $\wedge, \overline{}, \{1\}\rangle$, where f_v, f_{\wedge}, f_{-} are the operations defined by the familiar two-valued truth-tables for these connectives. It would seem natural to require of a semantics for swffs of CL, based upon matrix-assignments of cardinality ≥ 2 , that it have the features:

- (a) there are particular elements of the matrix M--call them '0', '1', S.th.
- (b) for some subset t of M (the "designated" subset)
 - (i) $1 \in t;$
 - (ii) $0 \notin t;$
 - (iii) for each connective c, the restriction of f(c) to $\{0, 1\}$ is the usual two-valued truth-table for c.

Most of the characteristic matrix-semantics for "many-valued" logics do indeed satisfy (a), (b). Construing the values assigned (including, perhaps, different 'kinds' of truth-value gap) as elements of an algebra, M, many of these logics assume a linear-ordering of the elements of M, with '0' least element, and '1' greatest. The question arises: which functions $\{f: Swff \longrightarrow M\}$ give an intuitively adequate representation of the 'truthfunctional' connectives 'not', 'and', 'or', with respect to (a), (b)? Suppose, for example, we let M be represented by an ordering M = $\{0, \frac{1}{n-1}, \frac{2}{n-1}, \dots, \frac{n-2}{n-1}, 1\}$ --where these rational fractions represent "degrees to which the formula approaches 'truth' or 'falsity'" according as different values are assigned to component sub-formulae.

Now, the question becomes: what functions adequately reflect our intuitions about the behaviour of connectives in this case? Rescher suggests there are <u>no</u> such functions--because truth-functions are <u>not</u> functions of degrees of truth. The argument proceeds as follows:

The truth-function of negation would be represented in M--one supposes-by $f \underbrace{\overset{M}{-}}_{[n-1]} = 1 - \frac{k}{n-1}$; a negation is true to the degree that a formula, itself, fails to be true. Then, assume that n is odd--so one of the rational values in the matrix is $\frac{1}{2}$. What value does f_{\bigwedge}^{M} assign to any pair of arguments $\langle \frac{1}{2}, \frac{1}{2} \rangle$? Presumably, the matrix should yield a value $\leq \frac{1}{2}$. But which value $\leq \frac{1}{2}$? Suppose, we have $v(\emptyset)_{f}^{M} = \frac{1}{2}$, then, by the definition of $f \underbrace{\overset{M}{-}}_{-}, v(-\emptyset)_{f}^{M} = \frac{1}{2}$ and $v(\emptyset_{\bigwedge} \emptyset)_{f}^{M} = \frac{1}{2}$ --but, how can a contradiction have any degree of truth? Suppose, then, we stipulate that $f_{-}\langle \frac{1}{2}, \frac{1}{2} \rangle = 0$, then we have to concede that, when $v(\emptyset)_{f}^{M} = \frac{1}{2}$, $v(\emptyset \land \emptyset)_{f}^{M} = 0$ --which is also absurd. Similarly, choosing values $\frac{1}{j}$ $(0 \leq \frac{1}{j} \leq \frac{1}{2})$ always gives inappropriate values for both ' $(\emptyset_{\bigwedge} \emptyset)$ ' and ' $(\emptyset \land \emptyset)$ '.

I believe that this argument reflects a mistaken conception of 'gappy' truth-value assignments based upon matrices--that the semantics for non-bivalent logics is <u>not</u> best represented by an algebra consisting of linearly-ordered value-sets. What the argument <u>can</u> be said to demonstrate is that <u>complementation</u>--which in the algebraic semantics for the logic is the operation corresponding to negation--is not generally Boolean, so that the algebra, itself, is non-Boolean. So, what algebraic structure <u>does</u> characterise the semantic structure of the right-field of the Y-relation in CL?

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Before answering this question, I shall consider the second argument (derived from Aristotle) which poses a similar threat: Argument B: It is sometimes held that to deny that every sentence yields

a true or false statement--to give up the principle of bi-valency--is, in effect, to give the terms 'not', 'true', 'false' a different meaning. That is, it may be claimed that the principle of bi-valency in the form given it by Aristotle:

"This is clear in the first place, if we define what the true and the false are: to say of what is that it is not, or of what is not that it is, is false; while to say of what is that it is, and of what is not that it is not, is true." (Metaphysics, M, Ch. 7, 1011b, 25-28)

has the status of a <u>definition</u>. As such, a rejection of bi-valency is explicitly a rejection of this definition, and a change in the meaning of the terms 'not', 'true', 'false'. It follows that, if a logician contends that the semantics of a logic admitting truth-value gaps best represents the truth conditions of nonsignificant or statement-failing utterances in context, the retort can be made that, unless he <u>means</u> by 'false', 'nottrue', and 'not-false' by 'true', then he is simply changing the meaning of syntactic operators 'not', and so on, and assigning values to statements <u>other than</u> 'true', 'false'--say, 'true or neither' and 'false or neither'. Then, whatever he calls these new connectives, or these different values, <u>not</u> having the first will entail having the secord, and bi-valency has to be re-instated to validate this entailment.

Notice that this argument depends upon the interpretation of 'negation'-as, indeed, Aristotle's dictum requires the assumption of the invariance of sentence-negation with respect to 'truth' and 'falsity' ("not-true" entails "false" and conversely). This is, of course, a commonplace: the two-valued interpretation of classical logic requires the validity of three, mutually interdependent, principles:

(i) Bi-valency: every statement is true or false
(ii) Excluded Middle: for every sentence p, 9(pv~p) is true.
(iii) Double-Negation: for every sentence p, ~~p ▶ p.
[A fourth principle--Non-contradiction--is derivable from these].

The argument in support of Aristotle proceeds: to the extent that, by (ii) and (iii), we require that negation always yield a value <u>opposite</u> to that of the negated sentence, then a negated sentence is true when and only when the sentence is false (by (ii)) and <u>vice versa</u>; whilst, by (i), these exhaust the possible values.

This last step in a <u>non-sequitur</u>--for there are indefinitely many operations upon sentences which meet these obvious conditions for the intuitive adequacy of a negation-operator; yet, with respect to which, bivalency need not hold. I shall show this, first, and, then, proceed to discuss the algebra of the semantic structure of CL (The argument cannot be purely <u>formal</u>, since we are here discussing the <u>intuitive adequacy</u> of operators).

Of the various criteria which might be demanded of a monadic connective c, in order that it be interpretable as negation, clause (b)(iii) above, restricts consideration to matrices which are <u>strongly regular</u> in the sense of Kleene (<u>1952</u>, pp. 333-336). There may be arguments for and against the adoption of regular matrices--I shall not consider them, here. In part, whether a non-classical logic is interpreted by regular or irregular matrix-assignments (with respect to its non-quantificational sub-logic) depends upon the particular structural properties demanded of its models, and upon the consistency property for the logic. For the time being, my restriction to strongly-regular matrix logics is an assumption which is supported, only in part, by the algebraic semantics for CL developed below.

The conditions which single out regular matrices (from Kleene, op. cit.), for monadic and dyadic connectives, are as follows:

I: A conjunction-connective with one false component is assigned 'false', no matter what values are assigned to its other components. It is assigned 'true' when and only when all components are true. Otherwise it is not defined.

II: A disjunction with one true component is assigned 'true', no matter what the values assigned to the other components. It is assigned 'false' when both components are false. Otherwise, it is not defined.

III: A negation connective is assigned 'false' if its component is assigned 'true'; 'true' if its component is assigned 'false'; otherwise, it is not defined.

Given I-III, an extension of an argument of Kleene's clarifies what it means to say of an assignment that it is "not defined" at points corresponding to statement-failing utterances in context. We can construe a matrixassignment as a partial recursive function $f^{M}(c)$ from value-assignments to values. Then, when $Pr_{f}M(c)$ is a partial recursive predicate defined over such a function, there is an algorithm on its range enabling us to say, for **such** a function, there is '-1' ('false') or '+1' ('true') (Different intervals (-1, +1), (-1, +2), ..., (-n, +m) will then correspond to sets of values, always with the last element designated--we ignore, for the moment, the questionable assumption that value-sets are linearly-ordered, for the sake of Kleene's argument). When Pr(c) is defined, the existence of $f^{M}(c)$ with respect to each connective, yields a decision procedure for formulae built up from components by means of these connectives. There may be no algorithm for deciding, however, whether Pr(c) is defined or not. To say "Pr(c) is undefined" is not to say that, with respect to a formula \emptyset , containing c, $f_{a}^{M}(\phi)$ cannot be made true or false, eventually (the same utterance may fail in one context, yet succeed in another). It is to say, rather, that the truth-value of ϕ is not determined, in that context, by means of the algorithm. This captures, in one sense, the dictum that the "meaning" (significance) of a compound sentence is a function of the meanings of its parts--at least to the extent that we are prepared to identify, temporarily, the conditions for the significance and illocutionary success of an utterance, in a context, with the circumstances of its being made true or false. Very roughly, then, a syntactically compound utterance is unsuccessful or nonsignificant in a context, when the circumstances which would make the statement it would yield true or false are not determinable in that context. (In the next section, this will come to mean: that the 'circumstances', appropriately defined, are not in that context).

Kleene's argument, reinforced by the metatheorems of CS-2 (section E) supports the general claim that connectives in swffs of CL 'contain' their classical counterparts. That is, it is claimed that the two-valued truth-tables for sentential connectives may be embedded in the matrix assignments for CL (its non-quantificational part). So, relative to an appropriately chosen value-set (-n, +m), the matrices have the form:

| ~ | | \vee | +m | - 7 |
|----|----|--------|--|------------|
| +m | -7 | +m | +m · · · +m · · · +m +m +m - · · · · · - | • n |
| | | • • • | * • • • • • • • • • • • • • • • • • • • | , , |
| -n | +m | -n | +m · · · · · $-n$ $-n$ $-n$ $-n$ | n |

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Various heuristic considerations can be used to extend Kleene's interpretation of "undefined" as "algorithmic undecidability" to its interpretation as "semantically unsuccessful" (through nonsignificance, or statement-failure). Suppose, as Kleene does (op. cit. p. 335), that assignments of values to compound sentences are made on the basis of assignments to components. Suppose, also, as Kleene observes, that the information that a sentence \emptyset is not defined is not utilisable by the algorithm, nor, indeed, by any procedure in terms of which truth and falsity (statementcapability) are determined. So, any procedure assigning 'true' or 'false' to a sentence containing \emptyset --say ' $(\emptyset v \psi)$ ' cannot depend on information about \emptyset , because there is none. From this it follows that ' $\neg \emptyset$ ', ' $(\emptyset v \psi)$ ', ' $(\emptyset v \psi)$ ' must be undefined when <u>both</u> \emptyset and ψ are undefined.

In terms of the intended interpretation of CL, however, any given swff, relative to contextual determination, is capable of yielding a true or false statement; but we deem it <u>undefined</u> when we lack information in the context enabling us to decide which circumstances would make it true or false statement-yielding. For example, if I have no coin in my pocket, though we understand my saying qu(A single spin of the coin in my pocket will come down heads), we lack enough information to decide whether my utterance yields a true or false statement because an existential presupposition of my utterance--that something is uniquely a coin and in my pocket--is not satisfied. Consider then, say, qu(A spin of the coin will come down heads and I will collect the jackpot) [= $(\phi \wedge \psi)$]. Clearly, we can imagine contextual circumstances in which ϕ would be assigned 'true' or 'false'; but it is not defined in this context (I have no coin in my pocket) so we cannot use this information in determining the value of the statement yielded by the conjunction. Suppose, however, that we have determined, independently of \emptyset , that I will not collect the jackpot (because it has been stolen). Since we can infer from information independent of \emptyset that the value of ψ will be 'false', then we can determine that ' $(\emptyset \wedge \psi)$ ' yields a false statement even though \emptyset is not defined. (Similarly, if we can determine independently that the value of ψ will be 'true' (I will collect the jackpot, because I stole it), then we can determine that '(\emptyset v Ψ)' yields a true statement even though \emptyset is undefined).

Viewed in this way, the "undefined" value(s) of strongly regular matrices is not properly interpreted as "non-truth-valued", or "neither true nor false"; but rather as "not truth-value determinable, in the context" (because non-statement yielding). This entails that the designated assignment 'true' (and undesignated 'false') should properly be read as "determined as true (false), in the context". This brings us back to the notion of what <u>makes</u> a statement true (false) in a context, and to Arguments A and B (from Rescher and derived from Aristotle).

It is quite easy to see, now, where Rescher's objection turns on a

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mistaken conception of the purported "third-value". ('undefined'). For, Rescher is quite correct in claiming that truth-functions are <u>not</u> functions of 'degrees of truth'; but rather functions of truth-values which, in some cases, may not be **algorithmically decidable**. It is wholly inappropriate, on this view, to construe value-sets as linearly ordered, with "undefined" being interpreted as some value half-way between '0' and '1'. Nevertheless, something of Rescher's objection remains when we notice that, by the regular matrices, ' $(\emptyset \ v \sim \emptyset)$ ' and ' $(\emptyset \land \sim \emptyset)$ ' are both undefined, when \emptyset is undefined. A solution to this problem will be sketched after the algebraic semantics of CL have been described. (It follows, in all essential respects, the well-known technique developed by B. Van Fraassen (<u>1968</u>, pp. 136-152 and <u>1971</u>, pp. 153-168) using 'supervaluations' to define validity and logical truth for presuppositional logics).

In response to Argument B, I shall illustrate how the intuitive properties of negation are preserved by regular matrices. The intuitive idea behind the illustration concluding this subsection is to investigate the properties of negation operators through the representation of their modeltheoretic counterparts--the <u>complements</u> of those subsets of Dⁿ (the n-th product of the domain) which are assigned by R to unnegated atomic predicates. Recall from clause (11) of the definition of valuations $\langle \mathbf{v}, \mathbf{w} \rangle$ that, for an atomic swff \emptyset [=(Fⁿt,...t_n)], $\mathbf{v}(\emptyset)$ = 1, when $\mathbf{w}(t_1)$, ..., $\mathbf{w}(t_n)$ are all defined and $\langle \mathbf{w}(t_1), \ldots, \mathbf{w}(t_n) \rangle \in \mathbb{R}(\mathbb{F}^n)$; $\mathbf{v}(\emptyset)$ = 0 when $\mathbf{w}(t_1)$, ..., $\mathbf{w}(t_n)$ are defined and $\langle \mathbf{w}(t_1), \ldots, \mathbf{w}(t_n) \rangle \in \overline{\mathbb{R}(\mathbb{F}^n)}$. Ordinarily, the Boolean complement $\overline{\mathbf{a}}$ of a set a is simply the set $\{x: x \notin \mathbf{a}\}$ (of all items in D not in a). I have allowed, however, that, when $\mathbf{w}(t_1)$ is not defined, for some $t_i \in \underline{\text{Term}}$, $qu(F^{1}t_i)$ yields neither a true nor false statement. Similarly, when $w(t_i) \notin R(F^{1})$ and $w(t_i) \notin \overline{R(F^{1})}$, $qu(F^{1}t_i)$ fails to yield a true or false statement. It follows that complementation in the partial models of CL is non-Boolean. The construction I describe below represents, in a perspicuous way, the properties of such <u>non-Boolean</u> complements.

I shall show in the next sub-section that the domain <u>St</u> (the rightfield of the Y-relation) forms a lattice-structure of a special kind-called an "intensional lattice". A lattice (indeed, any partially ordered set) can be represented by a HASSE DIAGRAM--in which distinct elements of the lattice are represented by distinct points on the diagram, and, if 'R' is the ordering-relation defined over elements in the lattice (reflexive, antisymmetric and transitive), if a bears R to b, this fact is represented in the diagram by there being an <u>ascending</u> line (or sequence of lines) from a to b, on the diagram.

Suppose, now, we stipulate that each diagram is being drawn for a subset of <u>St</u>--a degenerate sub-lattice of the lattice) comprised by the unit-set $\{a\}$, with respect to which matrix assignments from value-sets (-n, +m) are being made, relative to a context--and partial model m. That is, we associate <u>signed integers</u> (... +1, +0, ...), in a given interval, with the circumstances of a's being true in θ_m , being false in θ_m or being <u>undefined</u> in one of various ways (referential failure, paradoxicality, nonsignificance).

The ordering-relation defined over <u>St</u>, as will be described in the next sub-section, is to be "a Π_{Θ} b"; i.e. 'whatever makes (the statement) § a true in Θ , makes §b true'--a relation in terms of which the <u>relevant</u>

<u>compatibility</u> of §b with §a will be defined. So, an ascending line in the diagram represents the 'relevant compatibility' of §b with §a--expressed in terms of <u>complements</u> of elements associated with the signed-integer. Except for the two-valued case, there will be, in general, more than one complemented set compatible with the given statement §a. In view of the properties of these diagrams, I claim that, not only are the conditions for an adequate, regular negation-operator definable in terms of complementation in the lattice, but that the appropriate versions of excluded middle and double negation remain valid, even in the absence of bi-valency.

Consider, first, the limiting case of two-values: in a bi-valent context θ , with respect to the statement \S a, associate '+1' with \$a's being made true, and '-1' with that of \$a's being made false. So the interval represented is (-1, +1) in the series of signed integers without zero. Then the Hasse diagram with respect to assignments of values to \$atakes the simple form:



Clearly when \mathfrak{G}_a is assigned '+1', it is 'relevantly compatible' with (-1) and conversely; these being all the cases there are, bi-valently.

How consider the Hasse diagram should we allow that the statement a may be neither true nor false in Θ , i.e. simply undefined. Then we associate

with the <u>three</u> circumstances (of 3a's being true, false or neither) integers in the interval (-1, 0, +1), which yields the diagram:



(This is the '3-valued case'). Again, that \$a = +1 is relevantly compatible with <u>both</u> -1 and $\overline{0}$ is obvious. The absence of an ascending line, then, in the diagram corresponds to the <u>simple incompatibility</u> between the associated values.

Matters become only slightly more complicated should we associate value sets with intervals with 3 members. The four-valued case (which may represent the circumstances of a's being true, false, neither because paradoxical, in that context, or neither because of contingent referential failure) associates value sets with integers in the inverval (-1, 0, +1, +2), to give the Hasse diagram:



Since the mechanics of Hasse diagrams are familiar, I shall pass immediately to the five-valued case--where the interval is (-2, -1, 0 + 1, +2),

and the diagram is:



I will not give any more examples--but, I hope it is noticeable that the diagrams exhibit a certain symmetry, which is the geometrical analogue of the algebraic result that there is a structure-preserving homomorphism (logically speaking: a transformation upon value sets which preserves truth-value and truth-functional operations) from one sublattice into any other.

What has all this to do with negation and bi-valency? The following are directly consequential to the construction of Hasse diagrams:

(i) Complementation of value sets--as depicted in the diagrams--is a unary operation which can be considered the set-theoretic analogue, in the lattice, of sentence negation (where the sentence yields the statement, or the 'gap', whose value set has the associated integral value).

(ii) More importantly: if one reflects upon the geometric properties of the diagrams, it takes little intuition to observe that each represents the rotation of a planar figure through 180° around a mid-point on the axis of the plane. Let me explain this: Diagram (I)--the bi-valent case--corresponds, in an intuitive fashion, to Wittgenstein's frequent allusion in the <u>Notebooks 1914-16</u>, and in the <u>Tractatus</u> to the fact that a statement (his "proposition") should be conceived as having two 'poles', i.e. 'the true' and 'the false' which represent the circumstance which it depicts (if it is "elementary") or the combination of circumstances which its analysis reveals (if it is truth-functional) obtaining or not obtaining. Diagram (I) depicts this two-valued nature of a Wittgensteinian proposition as being represented by a <u>line</u> bounded by +1, -1. Figuratively speaking, if we "pick up" the line $\pm 1, \pm 1$ and turn it through 180° (parallel to the horizontal axis) we represent the <u>opposite</u> situation to that represented by the original line $\pm 1, \pm 1$. (It is in this sense, I claim, that the intuitive condition upon a negation operator is captured--since complementation of a sign \pm matches opposite <u>signs</u>).

Diagram (II) reflects further upon Wittgenstein's dictum to represent the possibility that a statement has <u>three</u> "poles" (true, false or neither)--whereupon the relevant intuitive sense in which complementation of opposed signs represents negation is geometrically represented in the Hasse diagram by the rotation of a planar <u>triangle</u> (three-pole figure) through 180°, (the rotation is of the triangle Δ +1, -1, $0 \Rightarrow \pm 1$, -1, 0). Clearly, an entirely analogous situation holds for the four-valued case, or the five-valued case, or however many finitely-valued cases--where the associated planar figures are, respectively, a square, a pentagon,...and so on. In each finite case, then, the existence of a structure-preserving homomorphism from that case to each other case guarantees that complementation preserves the condition upon negated sentences that they yield <u>opposite</u> values to the original statement.

(iii) Thirdly, if we continue the ascending lines in a Hasse diagram

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to represent the extension of the diagram to <u>doubly</u> complemented value assignments, it is immediately apparent that both excluded middle and double negation remain valid (in their appropriate version) in each finitevalued case. Thus, to extend the diagrams to evaluate the relevantly compatible doubly-complemented sets $\overrightarrow{+1}$, $\overrightarrow{-1}$, $\overrightarrow{0}$, and so on, the result represents, as it were, simply rotating the figure through the remaining 180° -bringing it back where it began; i.e. in general, for each value set $\overrightarrow{V_{t}(a)} = V_{t}(a)$. Similarly, in each finite-valued case, to the principle of excluded middle there corresponds the fact that the union of every disjoint value set exhausts the possible value sets for that statement in that context; i.e., where n = 3, (+1) \cup (0) \cup (-1) = $\{\overrightarrow{0, -1}\} \cup \{\overrightarrow{+1, -1}\} \cup \{\overrightarrow{+1, 0}\} =$ $\{-1, 0, +1\}$, which asserts, simply, that every statement is true, false or not determinately either.

I dispense with further comment upon the clauses of the definition of partial valuation for swffs in CL (discussion of clauses (8)-(10), (12)-(13) is deferred until Section D), to proceed immediately to the characterization of the algebraic structure of the domain <u>St</u> (the conversedomain of the Y-relation)

Algebraic semantics for CL:

Algebraic Semantics for CL:

If we suppose that, in certain contexts, some utterances fail to yield true or false statements, we confront the problem of whether traditionally valid logical principles remain valid in systems of logic which formalise the structure of these non-bivalent contexts. In the foregoing sections, I have defined what it is for an assignment in a partial model of the swffs of CL to assign 'true', resp. 'false', to atomic swffs, and to evaluate non-atomic swffs by recursion upon the basis assignments. I have identified the singularity points in the right-field of the γ -relation with points in the valuation where swffs are undefined. I have <u>not</u> given, however, a general description of the model theory of CL in terms of which the notions of 'validity', 'relevant-compatibility', and 'statement-identity' in CL can be explained. This is my aim in these two concluding subsections of Section C.

In the preceding subsection, an appeal was made to a relation holding statements $\hat{J}a$, $\hat{J}b$ when whatever makes $\hat{J}a$ true in a context makes $\hat{J}b$ true in the same context. I symbolised this relation $a \|F_{\Theta}b' - where \Theta$ is a schematic c-variable or constant--by analogy with the standard modeltheoretic relation $|\emptyset||_{I} \psi$ (read as " ψ is satisfied in all models I satisfying \emptyset ").

The relation $\mathbf{k} \parallel_{\Theta} \mathbf{b}'$ was first introduced by Van Fraassen (<u>1969</u>, pp. 477-487) in an exposition of the semantic relation of tautological entailment, for the purposes of classical first-order logic. So far as I know, its use in the semantic structures of partial interpretations--in logics admitting truth-value gaps--has not been investigated. It turns out, below, that, with very little modification, the relation can be redefined for these logics where, with the particular formalism of CL in mind, it takes on a somewhat different significance.

Consider, for the moment, the partial models of swffs of CL as if they were wholly abstract set-theoretic structures. For an atomic monadic predication $qu(Ft_0)$ in <u>Swff</u>, relative to a context θ , and assignment d from D for which $d(t_0)$ is defined, a valuation $\langle v, w \rangle$, based on $\langle D, R, d \rangle$ assigns to $qu(Ft_0)(\theta)$ an ordered couple $\langle\!\!\langle R(F), \overline{R(F)} \rangle\!\rangle$, $w(t_0) \rangle\!\rangle$, whose first member is a pair of disjoint subsets of D, and whose second is an element of D. In general, where $qu(\emptyset)$ is any atomic swff of CL, $\langle v, w \rangle$ assigns to it an n + 1-tuple, whose first-element is a pair of disjoint n-ary relations on $D^{n'}$, and whose other elements are members of d. Let us call any such n + 1-tuple a <u>complex</u> (in a model m, relative to θ , d).

DfV(a) A <u>complex</u> is an n + l-tuple whose first member is a disjoint pair of n-ary relations.

Thus:

By the definition, above, of a valuation $\langle v, w \rangle_m$ in a model m, it is easy to see that an atomic (say, monadic) predication qu(Ft₀) will be assigned 'true' if there is some complex c in m s.th. $\langle v, w \rangle$ assigns $c = \langle\!\!\langle R(F), \overline{R(F)} \rangle\!\rangle$, $w(t_0 \rangle\!\rangle$ to qu(Ft₀) and $w(t_0) \in R(F)$. Similarly, $\langle v, w \rangle$ assigns 'false' to qu(Ft₀) when $w(t_0)$ is defined and $w(t_0) \in \overline{R(F)}$. Relative to the model, m we can regard the set-theoretic union of all complexes c,c',c".. in m as the "valuation-space" of the predicate qu(Fx)--in the sense that it comprehends all the elements $w(t_i)$ in D, for which $v(F^X/t_i)$ is defined (for some $\langle v, w \rangle$). With this intuitive motivation, let us call a <u>circumstance</u> in m, any non-empty set of complexes in m. (Clearly, the singleton $\{c\}$ in m, plays the same role as the complex c, though $\{c\}$ is a circumstance). Finally, define a combination of circumstances in m as the set-theoretic union of circumstances and designate it by $C_1 + C_2 + \ldots + C_n$. Since circumstances are unordered sets of complexes, their union will comprise an unordered set of ordered n + 1-tuples. Intuitively, combinations of circumstances, subject to certain special limitations--which are my concern in Section D--are intended to be <u>contexts</u> (relative to m) to which the circumstances of a statement's being made true or false belong (if they belong at all), and in which any complex c_{ϕ} , which makes true a statement yielded by $qu(\phi)$ (if any), in a context, will appear.

Various relations between circumstances and complexes can now be defined:

DFV (b) Complexes c, c' are simply incompatible in a circumstance C, relative to m, if c, c'⊆ m and c is like c' (with respect to d) save that for each d(x_i) ∈ c, there is a d(x_i) ∈ c' s.th. if d(x_i) ∈ R(Fⁿ)_m, then d(x_k) ∈ R(Fⁿ)_m

I shall designate the <u>simple</u> incompatibility of complexes c, c' (in C = $\{c, c', ..., \}$)by "($c \sim c'$)". Notice that it holds between complexes only when $d(x_i)$, $d(x_j)$ are defined. I borrow, now, another notion used by Van Fraassen (loc. cit., pp. 479-80) to define a relation between circumstances:

V (c) By analogy with Cohen's use of forcing in metamathematics¹⁷, we shall say a union C + C¹ of circumstances forces complexes c, c' whenever C_k⊆C + C¹, and c, c'∈ C_k. So for any circumstances C_i, C_j:

(i) C_i forces C_j iff C_j⊆ C_i. Then, clearly,
(ii) C_i forces C_i; and
(iii) if C_i forces C_j and C_j forces C_k then C_i forces C_j + C_k.

In terms of V (a)-(c), we can define what it is for a statement to be made

true in a context (relative to m):

Since circumstances are sets of complexes and the forcing-relation holds between sets of complexes and complexes, we define, in general:

V (e) A statement $\hat{S}a$ is <u>made true</u> [<u>false</u>] in a context θ by any circumstance $C_j \in \overline{\theta}$ which forces some complex which makes $\hat{S}a$ true [false] in θ (relative to m).

Now, we can define, for any statement \S and context θ , the following sets (which I call "valuation sets (v-sets)" of \S a):

V (f) (i) the set T(a)--which is the set C₁ of <u>complexes</u> which make $\hat{\mathbf{j}}$ a true in $\boldsymbol{\theta}$.

(ii) the set F(a)--which is the set C_j of complexes which make **§** a false in Θ .

(iii) the set T#(a)--which is the set of all <u>circumstances</u> $\bigcup_{i \in A} C_i : C_i \in A$ such that C_i forces some complex c_a which makes fa true in θ .

(iv) the set F#(a)--which is the set of all circumstances $\bigcup_{i \in G} \{C_j: C_j \in \Theta\}$ such that C_j forces some complex c_a which makes § a false in Θ .

Intuitively, with respect to any partial model in, for swffs of CL, I

intend the disjoint v-sets T(a), F(a) to comprise the extensions in θ of <u>predicates</u> 'Tru(a, θ)', 'Fal(a, θ)' of CL (c.f. Df III), defined over statements a, a' \in <u>St</u>, yielded by those swffs. Since m is a partial model, however, it will not in general be true that there is always some complex ca in θ which belongs to T(a) \cup (Fa). That is, for some swffs qu(\emptyset), it will neither be that (qu(\emptyset)(θ) \vee §a \cdot \wedge Tru(a, θ)) nor (qu (\emptyset)(θ) \vee §a \cdot \wedge Fal(a, θ)). In such cases, we will infer that (\forall a) \neg qu(\emptyset)(θ) \vee §a, i.e. §a is <u>undefined</u> in θ (relative to m) and qu(\emptyset) is not statement-yielding in θ .

So far, I have only defined v-sets for statements yielded by <u>atomic</u> swffs. For compound and quantified swffs, I rely upon some basic settheoretic properties of disjoint n + 1-tuples and the primitive notion of statement-incompatibility. Notice first, where C_i, C_j are circumstances in m, their <u>product</u> '(C_i • C_j)' is the set of complexes c, c' such that c \in C_i and c' \in C_j; and that every product of circumstances is still a circumstance. In addition, where $\{C_1, C_2, \ldots, C_n\}$ is a combination of circumstances I designate by $\bigcap_{i=n}^{\infty} C_i$, the product of sets $C_1 • C_2 • \ldots • C_n$ such that each complex c' \in C_i for some i \leq n. The v-sets for statements yielded by non-atomic swffs in a context θ , relative to m, can be defined as follows:

$$\frac{\text{Negation } V}{(\text{ii})} (g) (i) \text{ if } qu(\sim \emptyset)(\Theta) \forall Sa, \text{ then, by (a), (b), (d) (i) and} \\ (ii) (for some m, \langle v, w \rangle) \\ T(a) = \{C_i \in \Theta: \text{ if } c \sim v_m(\emptyset)_w, \text{ then } c \in C_i \}$$

That is, the circumstance in which the statement yielded by $qu(\sim \emptyset)$ is made true in θ is the set of complexes simply incompatible with every complex assigned to $qu(\emptyset)$ by $\langle v, w \rangle$ in m. V (g) (ii) $F(a) = \{C_j \in \Theta: \text{ if for any } c' \in T(a), c \sim c' \text{ then } c \in C_j \}$. Notice that (i) if there is no complex c in Θ incompatible with $v(\emptyset)_W$, T(a) is empty; and, for every valuation $\langle v, w \rangle$, $v(\emptyset)_W$ is compatible with every complex in Θ . It does not follow, of course, that $v(\emptyset)_W$ makes some statement <u>true</u> in Θ , for there may be no such statement (the sentence may not have been uttered!). Rather, it follows that $v(\emptyset)_W$ would make any statement yielded by $qu(\emptyset)(\Theta)$ true in every consistent completion of \mathfrak{m} (every extension \mathfrak{m}' of \mathfrak{m} which results from arbitrarily assigning 'O', resp. '1' to truth-value gaps in \mathfrak{m} , provided \mathfrak{m}' is consistent).

(ii) Recalling, from above, the asymmetry between compatibility and incompatibility sets, the v-set F(a) is so defined that, when T(a) is nonempty, any <u>one</u> complex in F(a) is incompatible with any complex in θ which makes §a true; i.e. if θ contains any complex c' such that, for any complex c in T(a), c~c', then §a is <u>falsified</u> by that complex c'.

V(h) Statement-incompatibility: for statements \$a, \$b, context θ , relative to model m and valuation $\langle v, w \rangle$, if $qu(\emptyset)(\theta) \forall \a and $qu(\psi)(\theta) \forall \b , then: (i) $T(a/b) = \ C_i \in \Theta$: for all $c \in C_i$, $c \in F(a)$ or $c \in F(b)$ (ii) $F(a/b) = T(a) \cdot T(b)$.

Thus, an incompatibility is <u>undefined</u> iff (i) $T(a) = T(b) = \mathcal{N}(null)$ or (ii) $T(a) = F(b) = \mathcal{N}$ or (iii) $F(a) = T(b) = \mathcal{N}$

V(i) Conjunction, disjunction, conditional, bi-conditional

 $\begin{array}{ll} \wedge -(i) & T(a \wedge b) = T[(a/b)/(a/b)] = F(a/b). \\ \wedge -(ii) & F(a \wedge b) = F(a) \cup F(b) = T(a/b). \\ \vee -(i) & T(a \vee b) = T(a) \cup T(b) = T[(a/a)/(b/b)]. \\ \vee -(ii) & F(a \vee b) = F(a) \bullet F(b). \\ \rightarrow -(i) & T(a \rightarrow b) = T[a/(b/b)]. \\ \rightarrow -(ii) & F(a \rightarrow b) = T(a) \bullet F(b). \\ \leftrightarrow -(i) & T(a \rightarrow b) = T[a/(b/b)] \bullet T[b/(a/a)]. \\ \leftrightarrow -(ii) & F(a \rightarrow b) = T(a/b) \cup T(b/a). \end{array}$

To define the v-sets for statements yielded by quantified swffs in CL-qu((Ux) ϕ), qu((Px) ϕ)--we define the product $\bigcap_{\underline{v}} C_i$ of an infinite family of circumstances analogously to the finite case. (The existential quantifier, qu(($\underline{\exists}x$) ϕ), requires separate consideration in Section \underline{P}). Thus:

 $V(j) \text{ for any statement } \hat{\mathbf{y}}_{a}, \text{ context } \theta, \text{ relative to } m, \langle v, w \rangle, \text{ if } qu((Ux_{i})\emptyset)(\theta) \neq \hat{\mathbf{y}}_{a}, \text{ then:}$ $U - (i) T(a) = \bigcap_{i \leq w} \{C_{i}: v'_{m}(\emptyset)_{w'} \in C_{i}\} \text{ for every } \langle v', w \rangle \not\approx_{i} \langle v, w \rangle.$ $U - (ii) F(a) = \bigcup_{v \in C_{i}:} \text{ for some } c' \in C_{j}, c' \sim v'_{m}(\emptyset)_{w'}\} \text{ for every } \langle v', w \rangle \approx x_{i} \langle v, w \rangle.$

$$V(k) \quad \text{if } qu((Px_{i})\emptyset)(\theta) \forall ga, \text{ then:} \\ P - (i) \quad T(a) = \bigcup_{i \in \mathcal{U}} \{C_{i}: v'_{m}(\emptyset)_{w'} : C_{i}\} \text{ for some } \langle v', w' \rangle \approx x_{i} \langle v, w \rangle. \\ P - (ii) \quad F(a) = \bigcup_{i \in \mathcal{U}} \{C_{i}: \text{ for every } c' \in C_{j}, c' \sim v'_{m}(\emptyset)_{w'}\} \text{ for some} \\ \quad \langle v', w' \rangle \approx x_{i} \langle v, w \rangle. \end{cases}$$

What makes a statement yielded by a universally quantified swff <u>true</u> in a context is that, for all appropriate (matching) valuations $\langle v', w' \rangle$ on m, the product of every circumstance induced by $\langle v', w' \rangle$ contains a complex confirming an 'instance' of the statement (i.e. each statement yielded by replacing free variables in qu(\emptyset) by constants--for which $\langle v', w' \rangle$ is defined--to yield a 'truth-functional matrix). When some complex in θ is simply incompatible with an 'instance' of the statement (for some $\langle v', w' \rangle$) then some circumstance in θ contains a complex falsifying the statement. The 'particular' quantifier--qu((Px₁) \emptyset), read as " \emptyset , for some out of all x₁"--is, by V(k), equivalent to qu(\sim (Ux₁) $\sim \emptyset$). Should there be no complex in θ falsifying the statement yielded by a universally quantified swff, yet not every statement yielded by qu($\emptyset[_{xi}/_{to}]$) is made true in θ , then, again, qu((Ux₁) \emptyset) yields no statement in θ and T(a) = F(a) = Λ .

From Df. V (a)-(k)--determining v-sets for statements yielded by atomic and compound swffs in CL--we can relate statements to v-sets and contexts, in general, through the predicates 'Tru(a, θ)', 'Fal(a, θ)' as follows:

Df VI: (i) $\underline{Tru}(a, \theta)$, relative to m ["the statement § a is true in θ , relative to m"] iff for some C_i in θ , Ci = T(a) for all $\langle v, w \rangle$ on m.

(ii) $\underline{Fal}(a, \theta)$, relative to m ["the statement § a is false in θ , relative to m"] iff for some Ci in θ , Ci = F(a) for all $\langle v, w \rangle$ on m.

There would be little of interest in this complicated modelling apparatus for the domain of statements if it served only to redefine 'truth in a model'. A far more interesting semantic property definable in terms of v-sets, however, concerns the circumstances in which statements, in a context, agree on what makes them true.

Let 'a $\| \mathbf{f}_{\Theta} \mathbf{b}'$ abbreviate the relation holding between $\mathbf{5}a$ and $\mathbf{5}b$ whenever, in any model m, and context $\mathbf{\theta}$, every combination of circumstances in $\mathbf{\theta}$ which forces some complex in T(a) forces some complex in T(b). [We say a complex c' is forced by circumstances C_i whenever C_i forces C_j and c' $\mathbf{\epsilon}$ C_j]. That is, 'a $\| \mathbf{f}_{\Theta} \mathbf{b}'$ can be said to formalise that whatever makes \mathbf{s}_a true makes \mathbf{s}_b true in $\mathbf{\theta}$. This gives us, by V(b) and (e):

Df VII (i) $a \parallel F_{\Theta} b$ iff $T^{\#}(a) \subseteq T^{\#}(b)(in \Theta)$. We can observe at once that (relative to m, Θ):

(ii) $T\#(a) \subseteq T\#(b)$ in θ iff for each circumstance C_i in θ , if $C_i \subseteq T\#(a)$ then C_i forces some complex c' $\in T\#(b)$ [i.e. {c'} $\subseteq T\#(b)$]

')), f_{Θ} ' is an entailment relation of a particularly strong kind. Its

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definition in terms of v-sets (relative to partial models m) is fundamental to the semantics of the significance logic--CS-l--developed in Section D. To conclude this section, though, I shall examine ' $H^{+}\Theta'$ in relation to two questions posed earlier and left open until now. The first concerns the problem of whether CL possesses well-defined models in the sense in which, say, the modelling of sentential logic in the two-element Boolean algebra suffices to define the <u>validity</u> of two-valued tautologies, the consistency and completeness of its axiomatised calculus, and the decidability of theorems by means of truth-tables (see above p. 440). I have not attempted to axiomatise CL--for the moment, the status of CL's deductive theories is irrelevant to the consideration of its semantic framework. The question arises, though, whether CL is <u>sound</u> in preserving the validity of classical tautologies in the models of swffs of CL.

Since some substituends of swffs may be nonsignificant and semantically unsuccessful, in context; i.e. since $\S(-)$ is undefined for some arguments, it has to be shown that all and only truth-functional tautologies are assigned a designated value by valuations $\langle v, w \rangle$ on each model m. If, indeed, this cannot be shown, there is no reason to believe that the syntactic connectives $\{\sim, v, \&, \supset, =\}$, in terms of which the non-atomic swffs of CL were defined, possess a determinate meaning. For, if $qu(v_0)(\theta)$ is nonsignificant or unsuccessful, then $v(v_0)_m$ is undefined, and, by clause (6) of the valuation-rules, $v(v_0 \supset v_0)(\theta)$ is undefined. Then, it might be claimed, since $qu(v_0 \supset v_0)(\theta)$ is an instance of the logical truth $qu(\emptyset \supset \emptyset)$, either the valuation-rules are inconsistent, or ' \supset ' no longer has the meaning it has for sentential logic. [I confine discussion, for the remainder

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of this subsection, to the non-quantificational part of CL].

Similarly, if we recall the concluding illustration of the preceding subsection, the question arises whether negation constitutes a welldefined operation on swffs of CL. Notice that, by Df V, for each term ξ_0 , for which $w(\xi_0)$ is defined, $qu(F'\xi_0)$ is made true in a context if $w(\xi_0) \in$ R(F'), relative to m; and it is made false if $w(\xi_0) \in \overline{R(F')}$. For some values of $w(\xi_1)$, however, it may be that neither $w(\xi_1) \in R(F')$ nor $w(\xi_1) \in$ $\overline{R(F')}$ [i.e. qu(F') is not predicable of $w(\xi_1)$]; so $w(\xi_1) \notin R(F') \cup \overline{R(F')}$. It follows that complementation in the partial models of swffs of CL is non-Boolean. Investigation of complementation of v-sets in CL through its algebraic counterpart, however, resolves the question of the adequacy of a negation-operator for the validity of truth-functional truths in CL.

The theorem, above (Df VII (ii)) ensures that, like the subsetrelation ' \subseteq ', and the semantic entailment relation ' $|\Gamma_{I}$ ', ' $||\Gamma_{\theta}$ ' induces a partial ordering of the domain of statements. For:

- (a) a $\| f_{a} trivially$, since $T\#(a) \subseteq T\#(a) = [Reflexivity]$.
- (b) if all $t_{\Theta}b$ and bll $t_{\Theta}a$, then T#(a) = T#(b) [Antisymmetry].
- (c) if all $r_{\Theta}b$ and bll $r_{\Theta}c$, then all $r_{\Theta}c$ [Transitivity] --by transitivity of ' \leq ' and Df VII (ii).

Now, relative to any set of contexts Cx and model m, consider any non-empty subset s \subseteq St (the set of statements). Is there always some statement \S b \in St, such that for everv \S a \in s, all h_{Θ} b? There are four possibilities:

 T#(a), F#(a) are non-empty: Then T#(a) ∪ F#(a) forces C_i, C_j in θ ∈ Cx s.th if c ∈ C_i, there is some c' ∈ C_j, by V(g)(ii) s.th. c~c'. So F(a/a)⊆C_i • C_j, by V(h)(ii) and the definition of product. Then T#(a) U F#(a) forces F(a/a) by transitivity of forcing. But, by V(h),(ii), $F(a/a) = T(a) \bullet T(a)$ --which is T(a). So $T(a) \subseteq T#(a) \cup F#(a)$ and $a ||_{\Theta}(a/a)/(a/a)$ by V(i) (v-(i)) and VII. [Notice: '(a/a)/(a/a)' is '(a v a)'.].

- 2) $T_{\#}(a) = F_{\#}(a) = A$: Then s is the empty set so, trivially, for every $a \in s$, there is a $b \in s$. th. $a \parallel \Gamma_{0}b$.
- 3) $T\#(a) = \Lambda$ and F#(a) is non-empty: Then $T(a) = \Lambda$ and since $\Lambda \subseteq T\#(a) \cup F\#(a)$, $a \parallel I_{\Omega}(a/a)/(a/a)$ by Df. VII.
- 4) F#(a) = A and T#(a) is non-empty: (parity of reasoning with 3), F(a) = A and F(a) ⊆ F#(a)).

Thus, every non-empty subset of St has an <u>upper-bound</u> with respect to the ordering ' 117_0 '. Indeed, by extrapolation from cases 1)-4), we can define:

Df. VIII (a) A statement \$b is <u>tautologous</u> iff, for all models m and contexts $\theta \in Cx$, $T(b)_m$ in θ is non-empty and $F\#(b) = \Lambda$.

So, where \S b is tautologous, all \S b, for all \S a \in St. Since, any non-empty subset of St. has a tautologous upper bound, then the <u>least upper bound</u> (1.u.b.) of a subset s is any tautologous \S b' s.th., if \S b is an upper bound of s, bll P_{Θ} b' (always relative to m, Θ). [In general, if $\$a \in s$, the 1.u.b. of s will be any \$b' s.th. $T\#(a) \cup F\#(a) \subseteq T\#(b')$].

Quite analogously, we can define:

Df. VIII (b): A statement b is <u>contradictory</u> iff for all m, $\theta \in Cx$, $T#(b) = \lambda \neq F(b)_m$ in θ ,

--whence a <u>lower bound</u> of a non-empty subset $s \in St$ as any $\Sb \in St$. the for any $\Sa \in s$, $T(b) \subseteq T#(a) \cap F#(a)$ and $b \parallel f_{\Theta a}$, for an arbitrary $\Sa \in s$. The <u>greatest</u> <u>lower bound</u> of a bounded subset s is, thus any \Sb' s.th. if if \Sb is a lower bound of s, $b' \parallel f_{\Theta}b$.

The proofs of the following properties of the v-sets in the domain <u>St</u> are available from Van Fraassen (1973, p.101). I avoid burgeoning the text with his lengthy, but straightforward, set-theoretic definitions and derivations: IX: Let $\{a, b\}$ be any two-element subset of <u>St</u>. Then: (i) $T\#(a \land b) = T\#(a) \cap T\#(b)$ (ii) $F\#(a \land b) = F\#(a) \cup F\#(b)$ (iii) Suppose $C_i \in T\#(a) \cap T\#(b)$ and there is some C_j which forces C_i . Then $C_j \in T\#(a) \cap T\#(b)$, hence in $T\#(a) \cap T\#(b)$ [Valuation sets are closed under ' \cap ']. (iv) Suppose $C_j \in T\#(a) \cup T\#(b)$ and there is some C_i s.th. $C_i \subseteq C_j$. Then $C_i \in T\#(a)$ or $C_i \in T\#(b)$, whence $C_i \in T\#(a) \cup T\#(b)$ [Closure under ' \cup '].

- (v) $T\#(a \land (b \lor c)) = T\#(a \land b) \cup T\#(a \land c).$
- (vi) $T#(a v(b \land c)) = T#(a v b) \cap T#(a v c)$. [Distribution of '\\', 'U'].

Definitions VIII (a), (b) and IX yield the l.u.b., g.l.b., meet, join and distributive properties of a <u>complete distributive lattice</u>.¹⁸

J.M. Dunn (loc. cit. and private communication) has suggested that the domain of statements can be characterised as a distributive lattice of a special kind, called, by him, an "Intensional Lattice". In view of theorems proved by him--which I shall simply cite below--we can infer that the models of CL <u>are</u> well-defined, and that an intuitively adequate notion of <u>validity</u> is definable for CL, which respects classical tautologies. Indeed, Dunn's theorems suffice, also, to define non-Boolean complementation of vsets in the models of CL, and to introduce the notion of 'relevant' (as opposed to 'simple') 'incompatibility' discussed informally, above (p.413). The following is condensed from Dunn <u>1966</u> (loc. cit.--with appropriate notational adjustments):

An Intensional Lattice IL is a quadruple $\langle L, IIT, -, T \rangle$ where:

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    L is a distributive lattice under 'llt',
    for all a ∈ L ā = a
    for all a, b in L, if alltb then bilta
    (i) T is a filter on L (i.e. a sublattice F s.th. if a, b ∈ F, then (a ∧ b) ∈ F; and, if a ∈ F, (a ∨ b) ∈ F,
    (ii) T is consistent (there is no a in L s.th. a, T ∈ T),
    (iii) T is exhaustive (for all a in L, a ∈ T or ā ∈ T).
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To show that the domain <u>St</u> comprises an intensional lattice, we have only to show it has properties 2), 3) and that there exists a truth-filter T in St which satisfies 4) (i)-(iii).

Property 2) for <u>St</u> follows from the illustration concluding the preceding subsection together with Dunn's theorem I, below. Property 3) follows, with little supplementary work, from the embedding theorem (III), below, and the assumptions that matrices for CL are strongly regular (above p.445).

Complementation, as noted, in an intensional lattice is not Boolean. It follows from 2) and 3), however, that in IL is a one-to-one operation on L satisfying De Morgan's laws:

$$\begin{array}{c} --(1) & (\overline{a \wedge b}) = \overline{a} \vee \overline{b} \text{ and} \\ \hline --(2) & (\overline{a} \vee \overline{b}) = \overline{a} \wedge \overline{b}. \end{array}$$

From investigations by Belnap and Spencer (<u>1966</u>), cited by Dunn, it is necessary and sufficient for an intensional lattice to have a truth-filter T that it have no <u>fixed point</u>; i.e. that there be no a in IL s.th. $a = \overline{a}$. It is proved by Dunn (loc. cit.), by induction, on the set of all consistent filters in IL containing the least filter T₀ (comprising all $a \in L$ s.th. $a = (b \ v \ \overline{b})$), using Zorn's Lemma on the union of maximal chains C in T, that:

THEOREM I: If IL is an intensional lattice, then ' has no fixed point, and some filter of IL is consistent and exhaustive.

Analogous to the theorem of Stone (1936: The Representation Theorem), whereby every Boolean algebra (hence, the Lindenbaum algebra of sentential calculus) is isomorphically embeddable in a field of sets closed under meet, join and complementation, Dunn has proved the following theorems for IL's (recalling that every lattice can be represented by a 'Hasse diagram' as in the preceding subsection): Let a T-preserving homomorphism of an IL with truth-filter T into a lattice L' with filter T' be a homomorphism h from IL into L' s.th. if $a \in T$, then $h(a) \in T'$ and:

(i) h(a ∧ b) = h(a) ∧' h(b) ['∧'' is the meet-operation in L' corresponding to '∧' in IL]
(ii) h(a ∨ b) = h(a) ∨' h(b)
(iii) h(a) = h(a)'.

Now, the system of <u>diagrammed</u> IL's is the sequence of lattices determined by intervals in the series of integers $[\dots -1, 0, +1 \dots]$ so that:

 $M_{0}, o = \{-1, 0, +1\} \text{ and},$ $M_{n}, m+1 = \bigcup_{j,j \leq n} \langle in, i_{m}: i = 0, -1, -2...and j = 2_{i}, 2_{i} +1, 2_{i} +2...m \rangle.$

Then, let ILd be a diagrammed Intensional Lattice, whence: (Dunn, loc. cit.):

<u>THEOREM II</u>: A diagrammed IL has a truth-filter T iff it has a homomorphism into M_n , m for some finite n, m.

In addition--where /d/ is the cardinality of the interval [n, m] and M^{C} is the c-th Cartesian product of $M_{0,0}$, then we have the following embedding theorem: (Dunn, 1966, loc. cit.),

THEOREM III: Every IL of cardinality /d/ has a T-preserving isomorphism (1:1 homomorphism) into a product M^{c} , for some $c \frac{4}{d}/2$.

The manner in which these theorems fix the notion of validity in St (together with the assumption that matrices for swffs in CL are regular) is as follows:

Intensional lattices are intended as models of the domain St of statements. Construing an intensional lattice as an <u>algebraic structure</u> of statements; i.e. a set of statements closed under intensional complementation, meet and join, and ordered by ' \mathbb{N}_{Θ} ', we can define a model S_{St} as a pair $\langle L, \alpha \rangle$ --where L is an intensional lattice and where α is an assignment
function assigning to each St-variable an element of L. Given a model $S_{St} = \langle L, \neg \rangle$, we define a valuation f: St \longrightarrow L, determined by S_{St} , as a function f defined over elements of St and having values in L as follows:

Now we can say statements \$a, \$b are <u>relevantly compatible</u> when a \$b and b\$f a in any context θ . That is, in terms of the valuation sets introduced above, relevantly compatible statements are assigned identical circumstances T#(a) = T#(b) in a context θ , by a model m. Notice, of course, that relevant compatibility (symbolised by 'a¹/f'b' could <u>not</u> be defined in terms of identity of assigned <u>complexes</u>--T(a) = T(b) (see Df. V(f))--since this relation is far too strong (it is not even borne by \$(a/a)/(b/b), i.e. 'that (a <u>and</u> b)' to \$a). In addition, notice that, from a \$lt'b, it does not in general follow that F#(a) = F#(b)--two counterexamples being the so-called "paradoxes of implication": $F\#(a \land \overline{a}) \neq$ F#(b), though $T\#(a \land \overline{a}) = \checkmark \subseteq T\#(b)$; and $F\#(a) \neq F\#(b \lor \overline{b})$, though T#(a) $\subseteq T\#(b \lor \overline{b})$, since $T\#(b \lor \overline{b})$ is an upper bound on any subset $\{a: \$a \in \underline{\$st}\}$.

It is a short step, now, to identify the sound logic of the domain of statements with the standard logic of tautological entailment (as formulated, independently, by Van Fraassen <u>1969</u> and Dunn, <u>1966</u>). On this basis, statement-validity can be defined and completeness of <u>St</u> with respect to the axiomatisation " E_{fde} " (Anderson and Belnap, <u>1975</u> p. 158-9) for tautological entailments can be proved. Indeed, with the theorems of Dunn, listed above, an embedding of St/E_{fde} in the characteristic, strongly regular matrices (represented in Hasse diagrams for the intensional lattice of St/E_{fde}) for the canonical model C_{st} of St, establishes, by algebraic means, that the domain of statements has well-defined models in which truthfunctional validity is preserved. [The detailed proofs of these claims are available in Dunn, <u>1966</u>, Van Fraassen, <u>1969</u> pp. 480-487, and, also, in <u>56</u>15, 18.2-13.8 of Anderson and Belnap (ed.), <u>1975</u>, pp. 138-162, 193-206--the latter sections having been contributed by J.M. Dunn] I sketch these results, in outline, in TL, D. (B), below]. To the extent, therefore, that the strongly regular matrices of E_{fde} --the logic of tautological entailment-suffice to characterise validity in the lattice-structure of E_{fde} , so the claim for the soundness of CL, with respect to partial models m = $\langle D, R, d \rangle$, is substantiated.

I proceed, finally, to consideration of the remaining question of this section: can an appropriate equivalence relation between statements-representing statement identity--be defined in the semantics given for CL?

Statement Identity

Statement-Identity and Relevant compatibility:

Different utterances may state the same thing in one context. Utterances of tokens of one sentence-type may, in different contexts, state wholly different things. And what is stated in one context may be reported, correctly or incorrectly, in a different context, when the statement mentioned by the indirect clause of the report is the same or different from that originally made. What has to be the case for the same or different utterances to yield the same statement?

On intuitive grounds, one would expect that, minimally, utterances in a context will yield the same statement whenever what would make any statement yielded, in context, by one true or false, would make any statement yielded by another true or false in context, and vice versa. In addition, one would suppose that if distinct utterances yield the same statement, what is entailed or presupposed by one will be entailed or presupposed by the other. Finally, recalling the preliminary discussion of statementidentity in Section B, one expects that different utterances will yield the same statement, in the same or different contexts, provided that what each utterance is about in context is the same, and what each significantly states of what it is about is the same. In sum, an intuitively appealing defined relation of identity for statements should ensure that different utterances (tokens) in context yield the same statement provided that the statement yielded has identical truth-conditions, entailments and compatibility sets, whenever the utterances, themselves, have identical 'aboutness', 'significance conditions' and 'presuppositions'. Utterance 'aboutness' and 'significance

conditions' upon utterances will be discussed in the next section. For the moment I shall confine myself to identity of truth-conditions, entailments and compatibilities.

Formally, the strongest sense in which swffs of CL yield the same statement, in a context, would be given by the thesis that, if $qu(\emptyset)(\theta) \forall \mathbf{S}a$ and $qu(\mathbf{\psi})(\theta) \forall \mathbf{S}b$, then $(a = \mathbf{s} b)(\theta)$ (they yield the same statement in θ) if and only if in every partial model model m, whatever complex $\langle v, w \rangle$ assigns to $qu(\emptyset)$, relative to θ , $\langle v, w \rangle$ assigns to $qu(\mathbf{\psi})$. That is, identity of statements is simply identity of v-sets, T(a) = T(b) or F(a) = F(b), for every valuation $\langle v, w \rangle$, relative to a context θ , on every model m. Identity of v-sets, here, of course, is extensional, set-theoretic identity i.e. coincidence of membership in each valuation.

Unfortunately, such a wholly formal (semantic) identity relation reveals very little about those properties of utterances and statements in virtue of which they differ, or agree, across contexts (or within one context). For, the basis assignments R, d in a model m, in terms of which complexes are assigned to atomic swffs of CL, and from which the recursive specification of valuations $\langle v, w \rangle$ assigns truth-values to compound swffs (if at all) according as truth-values are assigned to component atomics, are essentially <u>arbitrary</u> assignments. That is, they are determined only by syntactic features of swffs, and motivated by the need to define the abstract notions of statement-validity, entailment and relevant compatibility. At best, taking a basis assignment to be an <u>arbitrary</u> assignment of elements of D to constants, subsets of Dⁿ to n-ary predicates, and mappings of variables over D, reflects the perhaps questionable assumption that the truth-conditions of atomic swffs are logically independent of one another-the truth or falsity of one atomic statement (yielded by an atomic swff) does not affect the truth or falsity of any other atomic statement.

Accepting the assumption, at this level of analysis, it would seem that the minimal condition upon statement-identity--that utterances yield the same statement when, in context, they agree on what makes them true--might be expressed in CL by the thesis:

(A) $(a = {}_{s}b)(\theta)$ if T(a) = T(b) or F(a) = F(b) in θ , relative to m and all $\langle v, w \rangle$ on m.

A moment's reflection on the definition of what makes a statement true, in the semantics of CL (Df. $V(A),(\omega)$, suffices, however, to establish that (A) is too strong for statement-identity in any interesting sense. Identity of v-sets $T(\alpha)$, $F(\alpha)$, is simply extensional coincidence of membership--as described above; and each v-set $T(\alpha)$, $F(\alpha)$ comprises a unique complex (n + 1-tuple) containing disjoint n-ary relations on D and elements of D, according as R, d makes arbitrary assignments to predicateparameters and subject arguments and constants of a given swff. Thus, if distinct utterances in a context yielded the same statement only if the context comprised the singleton-circumstance containing one complex making a statement true or false, then every circumstance in that context would collapse into a discrete unit-set. That is, each context, qua combination of circumstances, would be an unordered set of discrete autonomous complexes -- $\bigcup C_i$, such that $C_i = \{c': c' = \{c_0\}, v. c' = \{c_1\}, v. ..., v. c' = \{c_2\}, v. ..., v. c' = \{c_1\}, v. ..., v. c' = \{c_2\}, v. ..., v. c' = \{c_1\}, v. ..., v. c' = \{c_1\}, v. ..., v. c' = \{c_1\}, v. ..., v. c' = \{c_2\}, v. ..., v. c' = \{c_1\}, v. ..., v. c' = \{c_2\}, v. ..., v. c' = \{c_1\}, v. ..., v. c' = \{c_2\}, v. ..., v. c' = \{c_1\}, v. ..., v. c' = \{c_2\}, v. ..., v. c' = \{c_1\}, v. ..., v. c' = \{c_2\}, v. ..., v. c' = \{c_1\}, v. ..., v. c' = \{c_2\}, v. ..., v. c' = \{c_1\}, v. ..., v. c' = \{c_2\}, v. ..., v. c' = \{c_1\}, v. ..., v. c' = \{c_2\}, v. ..., v. c' = \{c_1\}, v. ..., v. c' = \{c_2\}, v. ..., v. c' = \{c_1\}, v. ..., v. c' = \{c_2\}, v. ..., v. c' = \{c_1\}, v. ..., v. c' = \{c_2\}, v. ..., v. c' = \{c_1\}, v. ..., v. c' = \{c_2\}, v. ..., v. c' = \{c_1\}, v. ..., v. c' = \{c_2\}, v. ..., v. c' = \{c_1\}, v. ..., v. c' = \{c_2\}, v. ..., v. c' = \{c_1\}, v. ..., v. c' = \{c_2\}, v. ..., v. c' = \{c_1\}, v. ..., v. c' = \{c_2\}, v. ..., v. c' = \{c_1\}, v. ..., v. c' = \{c_2\}, v. ..., v. c' = \{c_1\}, v. ..., v. c' = \{c_2\}, v. ..., v$ since c_n , for complexes $c_0, c_1, ..., c_n$, is simply $\{c_0, c_1, ..., c_n\}$. It is hard to see how any significant logical relationships either within contexts or between them could be revealed by investigating such simple structures.

A more promising candidate for expressing, in the semantics of CL, the condition that identical statements have identical truth-conditions might appear to be:

The inadequacy of (B) derives from different considerations. (B) asserts as a necessary condition upon statement-identity that, for any single context θ , \$a is the same statement as \$b when any circumstance forcing a complex making §a true or false in θ forces a complex making §b true or false in θ . Thus, (B) will not enable us to express inter-contextual identities of the form $'(\alpha(\theta) = {}_{S} \beta(\eta))'$, which are required for utterances in distinct contexts to yield the same statement. In the next section (D), this will prove to be a minor difficulty, however, given that we can introduce operations upon contexts, qua combinations of circumstances, which permit cross-contextual comparison. That is, taking $'0(9, \gamma)'$ to be an as yet undefined operation on contexts which forms, from any two, distinct contexts, a 'combined' context [their set-theoretic union, subject to restrictions discussed in $\mathcal{D}]$, then the assumption for CL that context-variables or constants are 'exportable' (above, p.408) guarantees that $(\alpha(\theta) = \beta(\eta))'$ is equivalent to $(\alpha = {}_{s}\beta)(\theta,\eta)'$, whence this latter can be evaluated for the 'combined' context '($\alpha = {}_{s}\beta$)(0(θ, η))'. For example, my saying qu(I am tired)(co), today, and commenting, the following day, qu(I was tired last night)(c'), make the same statement--'(a = sb) 0 (c₀, c')' when any circumstance forcing the truth of \$a (yielded by my first utterance) in c_0 , forces

a circumstance in which a complex making \S a true is one and the same complex as makes \S b true. This requires, amongst other things, that (i) w(I)_m in $c_0 = w(I)_m$ in c'; and that (ii) with respect to the past-tense operator in my second utterance--'P(I am tired) at t' (read: "it was the case that (----) at t", for variable t) is interpreted in a suitable extension of m by a function from t_0 --the time of my second utterance in c'--to t' $\leq t_0$, in the ordering $\langle \gamma, 4 \rangle$ (γ is the set of 'times'), such that, where assignments f_p of (possibly distinct) intervals (t_n, t_m) , (t_i, t_j) from $\langle \gamma, 4 \rangle$ are made to qu(I am tired), qu (last night), respectively, then, for some $t_k \in (t_n, t_m)$, $f_p(t_i, t_j) = t_k$. Then, §b in c', if true, is made true by $f_p[v(P(Iam tired) at t_k)(t_i, t_j)]_c'w$ --which is, simply, v (I am tired)_{co}, w if $t_k \in (t_n, t_m)$. [The complex expression, above, is a composition of tense-, indexing- and valuation functions, in the sense of Kamp 1969 and Lewis 1970]. Though inordinately complicated, the illustration gives an analysis showing, in short, that my second utterance is made true by the same complex as the first (i.e. the statement yielded is), when $O(c_0, c')$ is assigned intervals of time in which the tensed utterance of the second in c' is satisfied by some moment, during the interval assigned to qu(last night), at which my first utterance yielded the true statement \$a in c_0 .

Civen that inter-contextual identities for statements can be so interpreted in suitably refined extensions of the models of CL (such as that extension which interprets the addition of tense operators and the indexing of token-reflexive pronouns, like qu(I), to CL--for which: see Section D, and Lewis, <u>1970</u>), the question remains: does condition (3) upon statement-identity adequately reflect the intuition that identical statements have identical truth conditions?

Two arguments suggest that (D) remains inadequate. One problem is that not only do identical statements satisfy (B), all tautologically equivalent statements also satisfy (E). We should not be surprised, of course, that (B) is insufficient, in this sense, for statement-identity, since it is intended as only one of a number of conditions. Nevertheless, when $T^{\#}(a)$ is non-empty in a context Θ , $F^{\#}(a) = F^{\#}(b) = \lambda$ in Θ , and '=s' has the same force as 'a $\Pi_{a}b$ '. There is nothing untoward in this consequence -- indeed, it realises, in CL, the positive consequences of Wittgenstein's doctrine (Tractatus 5.124)" a proposition asserts every proposition which follows from it."; except that it is given the slightly weaker form that an utterance yields every statement that is tautologically equivalent to any statement it yields. This is as it should be -- for, providing that the "meaning" of truth-functional connectives does not vary from context to context (which they cannot in CL, at least, since $\langle v, w \rangle$ are defined over all contexts), then the tautological equivalents of a statement depend only upon the truth-functional form of the statement, and not upon variations in the utterance's 'significance conditions' or 'aboutness'. The validity of a tautology is unaffected by substitutions of different statements for its atomic components. This realises, in one sense, at least, the positivist doctrine that statements with identical truth conditions have identical entailments.

A technical difficulty, however, threatens the attractive consequence, described above. Since $\Lambda \subseteq T\#(a/(a/a))$ (i.e. $T\#(a \vee \overline{a})$) and $T\#(b) = \Lambda$ for an arbitrary (contingently) false statement §b, in a context θ , we have b $\||\mathbf{b}_{0}(a/(a/a))$. Similarly, $\mathbb{F}^{\#}(a \wedge \overline{a})$ is non-empty in every context θ , for an arbitrary statement §a. [§(a $\wedge \overline{a}$) is the g.l.b. on any subset §a§ \in St]. So, we have $\mathbb{F}^{\#}(a \wedge \overline{a}) \subseteq \mathbb{T}^{\#}(b)$, for an arbitrary statement §b which is true in θ --whence (a $\wedge \overline{a}$) $\||\mathbf{b}_{0}b$, for any §b. But, by De Morgan's laws for intensional lattices: (a $\wedge \overline{a}$) = (a v a) and $\mathbb{T}^{\#}(\overline{a}) = \overline{\mathbb{T}^{\#}(a)}$ whenever §a is an element of the truth-filter defined over the lattice, (which is to say §a is tautologically entailed or entails some tautology in St). Thus, (a $\wedge \overline{a}$) $\||\mathbf{b}_{0}b$, for an arbitrary §b (true in θ), and b $\||\mathbf{b}_{0}(a \vee \overline{a})$ whenever any §b is false in η (CL allows, of course, that §b can be true in one context and false in another, once we have specified how clause (13) of the definitions of valuations (Df III, above) restricts a valuation to a context). Yet how can the truth of a statement be forced by a contradiction? And how can an arbitrary falsehood force every tautology?

As is apparent, the argument reproduces the so-called 'paradoxes of implication' in the semantics of CL. Some might reply to the argument that there is nothing 'paradoxical' in such consequences, since natural languages contain familiar constructions which appear to rely on them: for example, when we utter rhetorical conditionals, like

(c) If you have squared the circle, I'm Lucretia Borgia.
--where the necessary falsehood of the antecedent forces the truth of an arbitrary statement.

In CL, however, the 'paradoxes' appear to be more damaging than they are for the conditional 'D' defined truth-functionally in sentential logic. For, let \$ be an arbitrary tautology and \$ c an arbitrary contradiction. Since clifton, for any true \$ b in θ , and blight, for any false \$ b in η , then, by the 'paradox' argument and definition of 'III', $c \cdot II_{O(\theta, \eta)}^{\dagger}$ t, for any context $O(\theta, \eta)$ formed by combining θ, η . Apparently, we have 'proved' that every contradiction is relevantly compatible with every tautology--which is absurd.

The response is immediate: if $O(\theta, \eta)$ were a genuine context, then So would be made both true and false in $O(\theta, \eta)$; i.e. $O(\theta, \eta)$ would be inconsistent and, one supposes, all statements would be 'relevantly compatible'. The response, however, proceeds too quickly. Is there any reason to suppose that no context in which the truth or falsity of statements is evaluated can be inconsistent? I have not, after all, restricted the notion of combinations of circumstances to exclude inconsistent contexts--since there is no logical reason why contradictions cannot be yielded by utterances in context (they are, simply, always false). Indeed, there is one positive advantage of allowing that ctilt for any contradictory Sc and tautologous St. For, suppose that, so far as tautologies and contradictions are concerned, (B) is sufficient for statement-identity. Then, the 'paradox' argument, above, establishes with respect to the context $O(\theta, \eta)$ that $F_{\pi}^{\#}(t) = T_{\pi}^{\#}(c)$ and T#(t) = F#(c); so, $F\#(t) \cup T\#(t) = T\#(c) \cup F\#(c)$, whence, for any C in $O(\Theta, \eta)$, $C \in F \#(t) \cup T \#(t)$ if and only if $C \in T \#(c) \cup F \#(c)$ and, by (B), $(t = c)(0(\theta, \eta))$. Thus, contradictions and tautologies make the same statement.

This conclusion is not unconnected with the <u>Tractarian</u> doctrine that tautologous and contradictory propositions say (i.e. "state") the same thing, to whit, nothing. For, a tautology, like a contradiction, <u>shows</u> the truthfunctional structure of states of affairs (circumstances). Nevertheless, the conclusion remains unacceptable--for the 'paradoxes' appear to demonstrate what is surely false: that an arbitrary statement relevantly entails <u>any</u> tautology, whilst a contradiction relevantly entails any statement. Surely, in general, what makes a particular statement true is irrelevant to what makes a tautology true (namely, everything), and nothing is 'relevant to' the truth of a contradiction--if we intend the notion of "relevance" to bear any of its normal senses? In reply to this question, first, I shall show how the 'paradox' arguments generate such unwelcome consequences; second, what sense of "relevance" is involved in 'relevant compatibility'; and, thirdly, how to avoid these unvelcome consequences. [There may be different ways in which the consequences can be avoided, but, since the notion of 'relevance' is only marginally related to the thesis topic, I will not pursue the matter any further].

The 'paradoxes' that $b \| \|_{\Theta}^{r}(a \vee a)$ and $(a \wedge a) \| \|_{\Theta}^{r}b$, for any statement §b, are a consequence of the definition of 'forcing' in terms of the subsetrelation; because, in set-theory, the null-set Λ is a subset of every set and $T_{\pi}^{\mu}(c) = F^{\mu}(t) = \Lambda$ for every contradictory §c and tautologous §t. One might suppose that some modification could be made to the definition--to declare, say, Λ an improper subset of an arbitrary set, and restrict v-sets only to proper subsets. [Ordinarily, Λ is a proper subset of every set Λ , since, for all x, if $x \in \Lambda$, then $x \in \Lambda$, vacuously, owing to the falsity of the antecedent--another instance of a 'paradox' of truth-functionalconditionals!] We cannot so modify the definition of 'forcing', however, without losing the definition of the g.l.b. and l.u.b. of non-empty subsets of St. As a result, we would lose the characterisation of <u>St</u> as an intensional lattice. An alternative, though, is to focus upon the notion of 'relevance' between statements which agree on what makes them true in context, and deny both that contradictions relevantly entail anything and that anything relevantly entails a tautology.

In characterising 'relevant compatibility', semantically, as holding between statements a, b, whenever a $\|l_{\theta}^{*}$ and b $\|l_{\theta}^{*}$, the intention was to offer agreement in entailing and entailed statements as one ground for the <u>relevance</u> of b to b. (Recall that, since <u>St</u> comprises a closed intensional lattice, the relation ' $\|l'$ ' became a contextually relativised analogue of tautological entailment by existing results on the logic of tautological entailment (Dunn, Van Fraassen, loc. cit.)). "Relevance" is, here, being given a narrow sense-though the term has heterogeneous uses in everyday discourse. I will comment briefly upon which sense it is intended to capture.

The senses in which it may be required of an explication of the notion of relevance that it appeal to a meaning-relation between relata might include those in which we demand, say, of an answer to a question that it be relevant to what is asked, or, say, of an inference from certain premises to a conclusion that the premises be relevant to what is inferred. Undoubtedly, we do demand, sometimes, that the relevance of (utterances) $qu(\emptyset)$ to $qu(\psi)$ consist in some relation of meaning between them (with respect to their subjectmatter and significant content). Some part of this demand is satisfied by the introduction, in Section D, of sameness of "utterance aboutness" in context. Nevertheless, this sense of "relevance" cannot be imputed to a relation between <u>statements</u>, because statements are not meaning-beagers--their illocutionary role is exhausted by the 'extensional' circumstances of their

being true or false. Relevant compatibility between statements, therefore, is not intended to capture any strongly 'intensional', generic relation between meaning-bearers, but a proximate species of relevance which pertains only to the contextual circumstances in which statements can be true or false together. What kind of proximate species of relevance is captured by this relation?

In an everyday sense, it might be said, some relevance between the circumstances, or states of affairs, whose obtaining, or failure to obtain, makes a set of statements true or false, is required, for example, of the summation of symptoms a doctor makes in supporting a diagnosis. Similarly, relevance of the data a scientist collects to support a pre-selected hypothesis is (to a degree which does not inhibit the formulation of innovative theories) required of an appeal to evidence. Confronted by choices as to morally right or wrong actions, relevance of the facts of the matter to the decision made is one condition upon the worthiness of the action performed. There are many such examples.

In these cases, relevance between circumstantial facts differs from that kind of relevance--involving relations of meaning--for which we would say that the presence or absence of one, or a number of symptoms is <u>criterial</u> <u>for</u> a doctor's diagnosis; e.g. for which we say having inflamed tonsils is <u>definitive</u> of having tonsillitis. Similarly, relevance between circumstances, here, differs from that in which the botanist, say, takes having a certain leaf-shape as definitive of being a certain kind of plant. [I do not want to suggest by these remarks, however, that the distinction between 'circumstantial facts' and 'criterial features' can always be drawn precisely].

With these examples in mind, then, relevant compatibility between statements can be taken to be a weak condition of relevance to be demanded of a relation between the circumstances which, in a context, agree or differ as to the statements made true, false or undefined in that context. It is unacceptable, therefore, if the definition of "relevant compatibility" given has the consequence that contradictions and tautologies are relevantly compatible in some contexts. The problem strikes at the <u>logic</u> of the relation--which is what the semantics of CL seeks to explicate. A solution to the problem, hence, should make a change in those logical principles from which the unacceptable consequence is inferred. Fortunately, just such a solution is to be found in existing work on the logic of relevance.²⁰

As noted, the argument leading to the unacceptable compatibility of contradictions and tautologies infers from the fact that, since no circumstance makes $\S(a \land \overline{a})$ true, then if any circumstance does, it also makes an arbitrary statement true. Clearly, that nothing makes a contradiction true is wholly irrelevant to what makes any particular statement true. Similarly, the inference that, since a tautology is always made true by any set of complexes, then whatever makes an arbitrary statement true or false, makes a tautology true, embodies an analogous fallacy of relevance. In general, these inferences are instances of the theses that a contradiction implies anything, and that anything implies a tautology. These theses, however, involve only the truth-functional relation of implication according to which '§a implies §b' is true if and only if §a is false or §b is true. When §a is truth-functionally false or §b truth-functionally true, the implications hold vacuously.

Truth-functional implication corresponds in classical logic to the syntactic connective ' \supset ' defined by the matrix yielding a true statement $qu(\emptyset \supset \psi)$ if $qu(\psi)$ yields a truth or $qu(\emptyset)$ a falsity. In virtue of this relation between $qu(\emptyset \supset \psi)$ and the implication 'a \mathbb{N}_{tf} b', the completeness and consistency of sentential calculus in the two-element Boolean algebra b^2 (see above) can be proved--along Lindenbaum/Tarski lines--by identifying all logically equivalent formulae. For, where $qu(\psi)$ is a theorem, so is $qu(\emptyset \supset \psi)$, for any formula $qu(\emptyset)$, and where $qu(\psi)$ is refuteable, $qu(\sim \psi \lor \emptyset)$, thus $qu(\psi \supset \emptyset)$, is derivable. Thus all theorems are logically equivalent and fall within one equivalence-class mapped, in b^2 , onto the 'designated' element '1'.

The solution to the problem confronting CL, therefore, consists in denying that all theorems are logically equivalent in this sense. That is, by axiomatising CL to generate all and only valid tautological entailments, we can employ a modified notion of implication (called "relevant implication") mirrored, in the syntax of wffs of CL, by a syntactic connective '-->' for which it does not in general hold that $(A \land \neg A) \rightarrow B$ is a theorem. Just such an axiomatisation is given by the system R_{\rightarrow} of relevant logic, formulated by R.K. Meyer, <u>1968</u>, and discussed in detail in Anderson and Belnap (<u>1975</u>, **§s** 3, 14, 15, 28 and <u>passim</u>). I discuss R_{\rightarrow} informally below--in brief--and refer the reader to R.K.Meyer, <u>1968</u>, for the detail of the formalism.

First: to rehearse the familiar argument justifying the 'paradoxes' of implication, recall that the following are derived inference-rules of classical logic: from (A A B), infer A, B,
 from A, infer (A v B),
 from (A v B) and ¬A, infer B.

Then, we can derive B from $(A \land \neg A)$:

| a) | ΑΛΠΑ | Premise. |
|----|------|-------------|
| b) | А | 1), a). |
| c) | ٦A | 1), a). |
| d) | ΑvΒ | 2), b). |
| e) | В | 3), c), d). |

Allowing, in addition, the classical principles of <u>reductio</u> <u>ad absurdum</u> (by a)-e)), De Morgan's laws and Double Negation, we can infer (A v \neg A) from any statement B:

| f) | В | Premise. |
|----|-------------|------------------|
| g) | ר (A v א) | Hypothesis |
| h) | ΑΓΓΑΑΓ | De Morgan, g) |
| i) | TA A A | Double Neg., h) |
| j) | רר (A v קA) | Reductio, g)-i) |
| k) | ΑνηΑ | Double Neg., j). |
| | | |

Without entering into Meyer's detailed discussion of the fallacies of relevance involved in a)-e), f)-k) (for which, see Meyer, Anderson and Belnap, loc. cit.), let it suffice that, by appropriate axiomatisation, the system R____avoids the 'paradox' arguments as follows:

'A \longrightarrow B' is a thesis of R only when there is a derivation of B from A in which the line on which premise A occurs is <u>used</u> (appealed to, in applying an inference-rule) and in which each inference from A is eventually used in obtaining B (where an "inference from A" results either from the introduction of an R axiom-schema instantiated to A, or results from applying an Introduction- or Elimination-rule to lines inferred from A). A proof of B from A in R, thus, excludes cases where B is derived independently of A, hence, irrelevantly. In particular, therefore, R differs from classical logic in rejecting the validity of the rule of disjunctive syllogism ('from (A v B) and "A, infer B'). R, therefore, also rejects $((A \land \neg A) \longrightarrow B)$ and $(B \longrightarrow (A \lor \neg A))$. If we define the classical conditional, however, as $(A \supset B) \longleftrightarrow (\neg A \lor B)$, then we can admit as theses of R_{\rightarrow} the principles [((A v B) \land \neg A) \supset B], [B \supset (A \lor \neg A)], [(A \land \neg A) \supset B] and [(A \land \neg A) \supset (B \lor \neg B)] Thus, though the classical conditionals are tautologically satisfied in models of R_{\rightarrow} , the appropriate derived rules of inference based on these are not valid in R_{\rightarrow} .

The rationale behind these restrictions can be illustrated in terms of the reasons for rejecting disjunctive syllogism. Clearly, no derivation (in any logic) of B from (A v B) and \neg A could make use of A as a premise without being inconsistent. On the other hand, any derivation of (A v B) antecedent to a derivation of B must either **e**nd with A (to introduce (A v B) by V-Introduction), or must end with B (for the same reason). If the preceding derivation ends with A, deriving \neg A to apply disjunctive syllogism to (A v B) makes the logic inconsistent. On the other hand, a preceding derivation of B to infer (A v B) is already a derivation of B which is independent of (A v B) and \neg A--so disjunctive syllogism is not needed.

Without disjunctive syllogism, the derivation of B from $(A \land \neg A)$ (a)-e)) is not forthcoming; neither is the derivation of $(A \lor \neg A)$ from B (since B is not used in obtaining $(A \lor \neg A)$). Nevertheless, R admits the classical A-Elimination rule in the form $(A \land B) \longrightarrow A$, B; so that both $(A \land \neg A) \longrightarrow A$ and $(A \land \neg A) \longrightarrow \neg A$ are theses. This permits the restatement of the principle of reductio in the form $((A \longrightarrow \neg A) \longrightarrow \neg A)$.

Adopting the axiomatisation of R as the base logic for CL does not

<u>remove</u> the cases of c_{1} , for inconsistent contexts θ --nor, indeed, should any effort be made to remove them, for the reasons given. What is achieved by the axiomatisation, however, is to ensure that, in contexts where entailments and compatibilities between yielded statements are being evaluated, the appropriate senses in which statements are <u>relevantly compatible</u> or <u>relevantly entail</u> one another will be those in which the theses of R, are being applied (i.e. the 'logically standard' contexts for statement-yielding utterances will be those in which all and only theorems of R, are valid). This does not exclude from consideration "logically non-standard" or even inconsistent contexts--but requires that, for such contexts, explicit postulates upon derivability and validity be introduced.

An additional consequence of the adoption of R, as the base logic for CL is to provide, for the first time, the minimal determination of the membership of $S_R \subseteq$ St--the subset of the domain of statements comprising all statements yielded by ('restricted') successful utterances in context. For, we can adopt the assumption that all <u>theses</u> of CL are asserted with respect to a logically standard context (CL, it will be recalled, is itself a twovalued 'meta language' for the 'object-language' of statement-yielding utterances in context). That is, since it is always a successful, true or false statement-yielding assertion, in CL, to affirm or deny semantic success or significance of an utterance in context, then derivable theses of CL will yield true or false statements, relative to the logically standard context determined by R, and any additional postulates of CL (see below). Since it is no part of this thesis to describe <u>which</u> utterances yield statements-rather, to describe in what their success and significance consist--the membership of the restricted domain S_R will not be further determined. [This minimal determination at least ensures the non-emptiness of the domain St, hence that $\hat{S}(--)$ is defined for some arguments].

To conclude this section, I return to the question of conditions for statement-identity. If (B) reflects a <u>semantic</u> condition, that identical statements have identical truth-conditions, the question arises whether the <u>formal</u> properties of an identity-relation between statements are derivable in the syntactic part of CL. The question of what formal properties should be demanded of an identity-relation is, of course, disputed in current logical theory. In an 'absolute' sense, any equivalence-relation satisfying

I 1)
$$(\forall x) (x = x)$$

I 2) $(\forall x) (x = y \longrightarrow y = x)$
I 3) $(\forall x) (\forall y) (\forall z) (x = y \land y = z \longrightarrow .x = z)$
I 4) $(\forall x) (\forall y) [x = y \longleftrightarrow (\forall F) (F(x) \equiv F(y))]$
(Leibniz' Law)

suffices as an identity relation. The first three may be granted, easily, and, indeed, can be adopted as axioms for '=s' in CL. Dispute arises over the adoption of I 4) in view of the fact that it licences an inference-rule (the intersubstitutivity of identicals) which fails in non-extensional contexts. In particular, adopting I 4) as an axiom for statement-identity seems to be precluded by the following argument.

Suppose we demand of any true statement-identity '(a =_s b)(θ)', for any θ , that \hat{g} b be everywhere substitutable for \hat{g} a, <u>salva veritate</u> [The stronger version of Leibniz' Law--that true identities licence intersubstitutivity, <u>salva significatione</u>, is clearly inappropriate, since statements are not significance-bearers]. Then, it might be demanded of '(a =_s b)(θ)' that it licence the inference from:

- 1) It is my true belief that cows chew the cud.
- 2) $\Im(\text{cows chew the cud}) =_{S} \Im(\text{cows are ruminants}).$
- It is my true belief that cows are ruminants.

Yet, if the context is one in which I am wholly ignorant of animal taxa, the conclusion may be false when the premises are true. On the other hand, if true statement-identities '(a =_s b)(θ)' do not licence inferences based on Leibniz' Law, in what sense can '=_s' be said to be an identity relation?

The argument reflects a mistaken conception both of the notions of <u>substitution</u> and of <u>statements</u>. (The counter-argument, given below, is adapted from Routley and Goddard, <u>1973</u> pp. 161-167):

1') $B(p_0)$ 2') $(\mathbf{\hat{g}}_{p_0} =_{\mathbf{S}} \mathbf{\hat{g}}_{q_0})$ (context can be disregarded, here). 3') $B(q_0)$

and 1'), 2') may be true when 3') is false. Yet, it can be seen immediately that 1') and 3') are not well-formed in CL. For 1') and 3') absorb a \S -operator whilst 2') includes a \S -operator applied to unrestricted u-constants. It is required in CL, however, that swffs (in this case, atomic u-constants) are only <u>mentioned</u> in wffs of CL (within quotation-contexts) and not <u>used</u>. Suppose, then we retranslate the argument by employing 'B(\S p_o)' for "I believe truly $(that-p_0)''-where the predicative role is carried by 'B' (c.f. "That cows chew the cud is my true belief") and '<math>3p_0$ ' is an individual wfc. Then, the argument has the form:

Clearly, if Leibniz' Law is to hold of all mentionable items, it should also hold for statements mentioned by factive clauses (we can suppose $qu(p_0)$, $qu(q_0)$ are both significant and statement-yielding, hence $\$p_0, \exists q_0 \in S_R$).

It is no longer clear, however, that 1')-3' constitutes a counterexample to I 4). We may cavil, certainly, at the reading of "I believe thatpo" as a predication of statements--qua products of assertive illocutionary acts--for, how can the outcome of a speech-act be an object of belief? Nonetheless, if we can attach a sense to 1') and 3'), and 2') is true, then my true belief that cows chew the cud is my true belief that cows are ruminants, whether I am aware of it or not; so the argument is valid. That is, it is a consequence of the publicity-requirement (PR) for statements--introduced in Section B--that if distinct utterances, in a context, are statementyielding, then they yield the same statement if what each is about, in the context, is publicly identifiable as the same item, and what each states of that item is the same. It was precisely this requirement that rendered disavowals of the statements I make, of the form $qu(p_0, but I do not believe$ that-po), pragmatically stultifying, in that context. It is not an illicit substitution, therefore, that generates a false conclusion from true premises, but an extraneous, and, in my view highly questionable, assumption that I cannot have a true belief without being aware of it. That is, when $\Im(\operatorname{cows}$

chew the cud) and §(cows are ruminants) are the same statement, then my assent to qu(cows chew the cu**d**) in a context--supposing <u>assent</u> to a statement to be a necessary condition for ascription of belief--is an assent to the <u>statement</u> that cows are ruminants. My denial that I believe that cows are ruminants indicates only my ignorance of the truth of 2'), it does not invalidate the argument.

This point can be clarified further by examining other familiar examples in which I 4) is supposed to fail. Failure to satisfy Leibniz' Law is taken to be <u>definitive</u> of 'intensional' predicates, i.e. predications whose criteria of identity seem to require stronger conditions than the 'extensional' condition that they are true of the same objects. Thus, the familiar account proceeds, though qu(x is a man) and qu(x is a featherless biped) may happen to be true of all and only human beings, their interchange may not be licensed by this wholly extensional conditon in contexts in which agreement, or identity of <u>intension</u> is required. To borrow the now dogeared example of Quine's:

4) The number of planets = 9.

is true and so is:

5) Necessarily, 9 is greater than 7.

from which we infer, by Leibniz' Law, the false conclusion:

6) Necessarily, the number of planets is greater than 7.

It is no part of my claim to deny that the merely circumstantial identity in 4) cannot licence the inference, within a modal operator, from 5) to 6)--perhaps, because identity of <u>intension</u> between qu(9) and qu(the number of the planets) would be required (which leaves open the problem of demystifying the notion of 'intension). It is my claim, however, that, if due attention is paid to the distinctions between sentences, statements and what utterances of sentences, in contexts, express (their propositional significance), then this problem, and related problems for doxastic, deontic, epistemic, <u>oratio obliqua</u> and temporal operators, do not arise for statementidentities. Statements are not 'intensional' objects, nor are substitutions of factive clauses, based on true statement-identities, within indirect reports, vitiated by referential opacity. Nevertheless, these problems appear at the level of utterances, and what utterances express in context. It concludes this section, therefore, to demonstrate that these problems can be shelved until the notions of 'significance' and 'utterance-aboutness' are introduced in Sections D, E.

Suppose we seek to formulate Quine's example in CL. The identity concerned, of course, does not hold of statements, but of the reference of qu(9) and qu(the number of the planets). Suppose, however, we construe the modal operator 'necessarily' as a <u>predicate</u> of statements 'Nec'. Then the argument might be formulated:

4') The number of planets = 9. 5') Nec (9 > 7). 6') \therefore Nec (9 + 7).

That 4') does not licence the inference from 5') to 6') is now evident. If 'Nec (\ll) ' is a predicate of statements then inferring 6') from 5') would require the premise:

4") $(\S(9 > 7) =_{S} \S(\text{the number of planets} > 7))(\Theta)$ for all contexts Θ --and this strikes me as frequently false. That is, 4"), if it were a true identity in general, would require that every utterance

which was 'about' the number 9, in a context, would be 'about' the number of planets--which is plainly false. What may be true is that, so far as we know, qu(9) and qu(the number which numbers the planets) have the same reference (but not on <u>every</u> occasion of their use: c.f. "Hegel argued for the number which numbers the planets being divisible by two"),²² but, as will become clear in the next section, identifying what an expression refers to in a context is not always sufficient to determine what an utterance containing it is about--particularly when what an utterance is about does not exist.

Satisfaction at avoiding the problem of referential opacity in this manner, however, must be short-lived--for I have not clarified at all the sense in which Nec can be said to be a predicate of statements. Similarly, with respect to the preceding argument over $'B(\mathbf{g}_{po})'$, I did not make clear how "is my true belief" could be construed as a predicate of statements. In short, I suspect that, at most, these senses may attach to what is true of statements, qua illocutionary actions, only in a derivative manner from what, primarily, is true of what an utterance expresses. That is, in the case of belief, there are far more plausible grounds for taking the proposition expressed by an utterance in a context to be the object of belief, rather than what is made true or false in a context by circumstantial facts. Only by so construing 'belief', for example, can having an inconsistent belief (that, say, the class of all non-self membered classes is a member of itself) be distinguished from merelv having a false belief (that, say, Persia is larger than Peru). For, in the former instance, if the object of belief were a statement, the fact that qu(the Russell class is a member of itself)

does not yield a statement (because paradoxical) would equate having an inconsistent belief with having no belief at all; whereas, in the latter case, since qu(Persia is larger than Peru) is statement-yielding in contexts in which qu(Persia) qu(Peru) have their customary reference, having this false belief amounts only to their being no circumstance making the statement true (as a matter of fact). Nevertheless, though I will not pursue the point any further, there may be plausible grounds for making $qu(I believe \alpha)$ predicable of statements in a derivative sense--to allow room for those occasions when we speak of the acquisition of a belief as resulting from another's persuasive oratory, or of being convinced by an argument. For, it is the other's illocutionary act which is persuasive, and it is what the arguer states that is convincing. To the extent, then, that it is the yielded that I identify in context and add to what I believe true, statement so it can be said that I acquire a belief in what another states derivatively from what his utterance expresses to me in the context. Furthermore, 'distinguishing statements from propositions as objects of belief, in this manner, will explain those rare occasions when it is valid to infer from its being true that I believe that-po and that-qo is, circumstantially, the same statement as that-po (even though I am justified in dissenting from that-qo, because I am ignorant of the truth of the identity) to the true conclusion that, as a matter of fact, I believe that-qo. For, in my ignorance of the fact that $qu(p_0)$ expresses the same proposition as $qu(q_0)$, and, hence, yields the same statement, my dissent from that-q (qua statement) does not contradict my assent to (or understanding of) the proposition expressed by $qu(p_0)$.

In an analogous manner, one could argue for 'necessary truth' being

predicable of statements (especially if it is defined, semantically, as "truth in all possible worlds"); whilst retaining the view that the fact that an utterance yields a necessary truth is derivative from the analyticity of the proposition expressed. Should one object to the 'analyticity' of necessary propositions--on Quinean grounds--one could restrict such universally satisfiable truths to those yielded by derivable theses of some suitably axiomatised modal calculus.

I will not go further than this in trying to make a plausible case for treating statement-identities as outside the purview of objections to substitutions in non-extensional contexts. Notice that, with respect to the semantics of CL, at least, the tenor of the argument is in agreement with the claim that, since statements are not syntactic objects, nor are they bearers of significance, one cannot make substitutions <u>into</u> them (for what is substituted, and what results from substitution, is always an expression of a language) nor can one demand of Leibnizian identity for statements that it preserve significance across (or within) contexts, when substitutions are carried out.

In sum, adopting (B) as the expression in CL of the demand that identical statements have identical truth-conditions and entailments gives one condition upon inter- and intra-contextual identities for statements. Other conditions--involving the 'aboutness' and 'significance' of statement-yielding utterances in context--will be made clear in the next section. It should not be thought, of course, that it is always possible to discern that the statements distinct utterances make, in the same or different contexts, are the same--for, identity of truth-conditions, even though insufficient, may itself be

indeterminable. Strictly speaking, for example, in a particularly meagre domain D, the partial modelling m may assign the same set of complexes in a context θ to the statement yielded by the universal closure of $qu((Py)R(x, y))(\theta)$ as it assigns to a finite conjunction of its instances $3(b \land b' \land ... \land b^n)$ --where each statement $3b_i$ is yielded by some instance $qu((Py)R(a_i, y))$ in θ . Then, should the relation R be irreflexive and asymmetric, this fact cannot be reflected in any completion of the model m up to maximal consistency. For, what makes the statement yielded by the universal closure true in m will also be what makes the finite conjunction true in θ , and <u>vice versa</u>-so $g((U_x)(Py)R(x, y)) =_s g(b \land b' \dots \land b^n)$ in θ , by (5). If so, however, the **inteflexi**tivity and asymmetry of R is inconsistent with every completion of m in which this identity holds--for, qu((Ux)(Py)R(x, y)) is satisfiable, then, only in non-finite domains.

Summary

The systematic development of a formal apparatus for the treatment of the relations between utterances in context, and the statements they yield (which we granted, at outset, was a simplification of the more proper relation "A speaker X utters $qu(\emptyset)$ in a context θ to state that $\neg x$ ") has proved a long and complicated enterprise. The general principles behind the formulation of CL are, however, quite simple. From the supposition that statements are mentioned by factive clauses of the form $\mathfrak{G}(a)$, CL was developed as a first-order language applied to mentioned utterances in context standing (or failing to stand) in the "yields" relation to mentioned statements. Arguing that statements do not have syntactic properties, it was shown that, nevertheless, the structure of the domain of statements would be systematically characterised as a lattice-structure, for which the axiomatisation of the logic of entailment in terms of Meyer's system R provides a deductively sound and complete theory. By connecting the domain of statements, through the partial modelling m of mentioned swffs, to the valuations of utterances in context, various semantic relations between statements could be defined in terms of the set-theoretic structure of these relative entailment', '11+' for 'relevant compatibility' and '= ' for statement-identity could be introduced in terms of the valuation-sets $T^{\#}(\boldsymbol{\varkappa})$, $F_{\#}^{\#}(\mathbf{z})$ m assigns to statements yielded (if at all) by swffs in context. These relations provide the initial base from which the logic-CS-1-of contextual significance is developed, in Section D.

Unlike the classical models of first-order logic, the partial models of swffs in CL only partially define the notion of truth-in-the model, in the sense that it allows that some subject-constants ξ_{\bullet} are undefined for the designation-assignment $d(\xi_{\bullet})$, and some swffs fail to yield true or false statements. At this level of analysis, therefore, the semantic failure of an utterance to yield a statement consists only in its being undefined as a result of (i) its containing a referentially-failing expression, or (ii) predicating of what it is about what is not defined for such items, or (iii) owing to its being a compound with undefined components. In the next section, the analysis proceeds to explicate some of the reasons for such semantic failure--through nonsignificance, failure of presupposition, referential failure or paradoxicality. In addition, a more detailed examination of the rôle of context in evaluating the significance and semantic success of utterances is undertaken.

At this point, however, I should comment upon some open problems for the logic CL. As was recalled in the discussion of subject- and predicateexpressions, there is some doubt whether workable criteria by means of which to separate subject-expression from predicate-expression in a sentence could be given. None of the criteria considered were individually sufficient, though it was allowed that some number of them may be adequate, taken together. The difficulty remains, however, of ensuring that, collectively, the criteria considered can be applied consistently. Further discussion of this problem, and of Quine's claim that any such criteria must be subject to the indeterminacy which besets the notions of 'reference' and 'translation', could be developed from the analysis of utterance-aboutness in context, undertaken below. I shall not continue this discussion, however, trusting only that the notion of utterance-aboutness will, itself, shed some light upon this complex problem.

Secondly, it cannot be pretended that an intuitively adequate account of statement-identity should require, formally, precise agreement in truth conditions as a necessary condition. For, was it not argued in Section B that considerable leeway is admitted, in natural language, in reporting the statements of another? Is it not that such reports only have to be as accurate as the needs and interests of the audience, and the purpose of the report, require? The questions reflect a misunderstanding of the rationale behind the provision of identity-conditions for statements. It can be granted that, epistemologically and pragmatically, the correctness of a report of another's statements may only be judged within considerable margins of lassitude. This does not preclude our insisting that, logically, only the identities which fulfill the stringent conditions I 1)-I 4) can be assigned the value 'True' in an interpretation of CL. For, only then will inferences from $qu(\phi)(\theta) \stackrel{\forall}{\mathfrak{f}}{\mathfrak{a}}$ and $(a = b)(O(\theta, \eta))$ to $qu(\phi)(\eta) \stackrel{\forall}{\mathfrak{f}}{\mathfrak{b}}$ be valid--and it is upon the validity of such inferences, involving statement-identity, that the logician focusses.

It remains, nevertheless, an open-question as to what relationship holds, in general, between the formal semantic apparatus developed by the logician, and the epistemological notions from which his analyses so often spring. This question is currently the topic of much debate in the philosophy of language--in connection with Davidson's claim²¹ that a formal truth-theory for a language (a recursive specification of truth-conditions based on model-theoretic assignments) is one form a theory of meaning for a language should take. Considerations of brevity demand that I not broach that issue, here.

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II Section D: Significance and Context: (CS-1):

Formulation of the logic CL for statement-making utterances in context provides quite a rich resource of semantic results and devices. What it has lacked, until now, is application to the question posed in the preamble to this part: to what do we attribute the nonsignificance of category-mistakes?; what inferences to and from significance claims are supported by the semantic apparatus developed for significance logic?; and to what other kinds of anomaly or meaning-failure can the account of category-mistakes be related? In this section, I apply the further development of CL into a significance logic CS-1 to answer these questions. In so doing, it has to be admitted that there are several competing logical systems which have a claim to represent the inference-structure of significance claims and significance-features. I have adopted features of some of these formalisms, below; though I do not, in general, argue against competing formalisms. At best, CS-1 is recommended only by its fidelity to the more discursive arguments of I, Section D and II, Section B. Moreover, as this section concludes, CS-1 has major shortcomings as a codification of inferences based upon significance-in-context-mainly in respect of the semantics for quantified swffs (semi-well-formed formulae) of CL. I intend to suggest ways in which those shortcomings may be removed in outlining the extension of CS-1 to CS-2 in the concluding section (E) of the thesis. CS-2 itself, however, is not fully

articulated--several open problems, of a formal nature, are left to the concluding discussion of the thesis. In the same discussion, I offer heuristic reasons for preserving CS-2 as preparatory to a full account of significance- and statement-failure in context, together with final arguments for the account of category-mistakes I develop.

(I) <u>Eackground</u> to <u>CS-1</u>:

Thus far, a context has been identified, for the purposes of CL, with a certain highly structured combination of "circumstances"--described set-theoretically--in which appear sets of complexes making some statements true, others false, and "forcing" complexes which verify or falsify the statement at issue. These combinations of circumstances relevant to evaluating whether a given utterance is statement-yielding were divided into sets compatible or incompatible with the statement (if any) yielded. Where a given utterance, in a context c_0 , fails to yield a statement (thus, fails to assert anything true or false):

(a) \neg (Pa) (qu(p₀) (c₀) Y \S a),

then the truth conditions of (a) reflect in the semantics the fact that <u>no</u> circumstance <u>in</u> c_0 (which is a union of a family of compatible and incompatible circumstances) forces a complex, or "truth-functional" combination of complexes, which makes 9a true or false. There are at least four primary reasons why a context, in this sense, fails to force the truth or falsity of what $qu(p_0)$ asserts in c_0 (c.f. II, Section A, Diagram II(i)):

(I) $qu(p_0)$ may fail referentially in c_0 :

In this case, $qu(p_0)$ may contain an expression which, in c_0 , is not determinately about an item. The expression, say, though apparently uniquely referential, may be about many items (qu(po) is referentially ambiguous in c_0). For example, qu(The man in the brown hat is a doctor) is "referentially multiple"--in different contexts the subject term may be used to introduce a reference to different individuals. In order for it to be referentially successful in any one context of assertion, though, it has to be about at least and at most one man wearing a brown hat, in that context. Should there be more than one brown-hatted man, the utterance is referentially ambiguous and statement-failing. Should there be no brown-hatted man, the utterance is referentially failing and, again, fails to yield a statement. In either case, in the semantics for CL given in II, Section C, the absence of any complex to verify or falsify what the utterance yields is a consequence of there being some member circumstance of the context which forces the falsity of a presupposition of the utterance's being statement-yielding--namely the scatement that at least and at most one man is wearing a brown hat. In other words, referential ambiguity or failure is represented in the semantics for CL by the context's containing a circumstance relevantly incompatible with a presupposition of the utterance's yielding a true or false statement. This primary sense of statement-failure is formally explicated in the

description of 'aboutness' and 'presupposition', below. (II) $qu(p_0)$ may fail to yield a statement if pragmatically stultifying in the context (c.f. Lakoff, 1973, and Diagram II(i), above): Traditional examples of pragmatically stultifying assertions (also called "ungrounded" assertions) have been: (i) qu(What is hereby asserted is false) --- where "hereby" indicates that the assertion is about itself. (ii) qu(Do not obey this order)--a self-stultifying command. (iii) qu(I know that po, but I do not believe it)--Moore's example of a pragmatic contradiction. (iv) qu(Most of what I have said in the last five minutes is false) -- when, of what I have said in the last five minutes, 50% of the statements I have made are true and 50%, except the assertion above, are false. Statement-failure through ungrounded or stultifying assertions in context

is not discussed further in the thesis; see, e.g. Herzberger, <u>197</u>. "On Paradoxes of Ungrounded Assertions" for an explication of a semantics which could be modified to fit CS-1.

(III) $qu(p_0)$ may be semantically unsuccessful in context because in conflict with a priori principles, i.e. a priori rejectable.

The notion of 'a priori rejectability' is itself rather vague. Whether one should call it a species of significance-failure or one of statement-failure, or even, of falsehood is subject to debate--at least so far as many significance-claims concern the incompatibility of what an assertion appears to express with deeply entrenched metaphysical, scientific or mathematical principles. One could call such principles "a priori" in one of at least three senses: it may mean no more than that we <u>do</u> not appeal to verifying or falsifying empirical circumstances for their support. More strongly, it may mean that such principles <u>can-</u> <u>not</u> be subject to empirical verification or falsification. Or, more strongly still, it may mean that the source of our knowledge of such principles is <u>independent</u> of empirical experience--in the sense in which, since we bring such principles <u>to</u> experience, we do not induce or abstract them <u>from</u> experience--hence, we do not subject them to empirical revision. In each of these senses, a particular assertion may be rejected as necessarily false, or even <u>nonsignificant</u>, in so far as it commits its speaker to the falsity of some principle which "makes sense of" his empirical experience. Thus, such principles have descriptive significance in the attenuated sense in which, should they be falsified, an extensive series of more directly descriptive assertions would turn out not to make sense (not to yield true or false statements in any context).

Two examples of this kind of <u>a priori</u> absurdity will have to suffice, since I do not have the space to discuss the notion in detail. It is at least arguable that, in view of the dominant philosophical image of "perception" as passive reception of data, then it is not simply incongruous or false, but may be nonsignificant to assert of someone, say, his "deliberately" or "with malice aforethought" <u>choosing</u> to perceive some state of affairs. Such an assertion is not an obvious candidate for a category-mistake---if only because the understanding of "perceiving", as "reception of sense-impressions", though entrenched, does not exclude consideration of its categorial opposite: that perceiving is a nonpassive, goal-directed activity more akin to "attending" than "receiving".
An extensive revision, that is, to what is to count, in a context, as <u>perceiving</u> may eventually concede the significance of "deliberately choosing to perceive". Such contexts are ones in which, for example, qu(He chooses not to perceive this) does not appear category-incongruous, or a priori anomalous.

As a second example of a more general nature, consider the radical shift in <u>a priori</u> conceptions necessary to spatial measurement and geometrical concepts required by recognition of non-Euclidean and n-dimensional abstract geometries. Axioms which conflict, say, with the Axiom of Parallels--which assign a positive or negative value to the curvature of space--may be incompatible with factually descriptive assertions made outside of axiomatic geometry. To coin an example of Wilson's, qu(There are four <u>straight</u> rods AB, CD, EF, GH which intersect at a point 0, such that all angles at 0, AOC, AOD, AOE, AOF, AOG, AOH.... are <u>right angles</u>) is not obviously a category-absurdity; and may best be regarded as necessarily false, or of indeterminate truth-value, owing to the indeterminacy of the notions of <u>straight line</u>, <u>right angle</u>, except in relation to some postulated non-Euclidean or multi-dimensional primitives.

The difference between such examples and those of category-mistaken assertions will depend upon the status we give to the <u>a priori</u> application of this or that metric to spatial relationships. If, like Poincaré, on the one hand, we regard the adoption of geometric axioms as a matter of convention, we should regard conflicting beliefs as logically inconsistent with our conventions of measuring. On the other hand, if, like Wittgenstein, we regard the significance of geometrical terms as tied ultimately to operations of measuring and calculating outside of formal axiomatic theories, then conflicting beliefs may indicate a conflict of "meaning" between theorems of non-Euclidean geometry and our "ordinary" conceptions (whatever they may be) of space, distance and measurement--albeit a conflict that issues in no observable inconsistencies in our calculations except when astronomically large distances are involved.

Despite the intrinsic interest of such examples, to discuss them any further would take me too far from the subject at hand.

(IV) Finally, the failure of $qu(p_0)$ to yield a statement may be the result of its nonsignificance through being category-mistaken in context.

The formal explication of the nonsignificance of category-mistakes must <u>supplement</u> the merely circumstantial failure of an utterance to yield a statement through unfulfilled presupposition or indeterminacy of aboutness. It must show how, in some sense, a category-mistake <u>cannot</u> yield a statement, not just in one context, but in a range of contexts in which its literal interpretation preserves its aboutness and significance conditions. It must show, that is, how category-mistaken assertions fail to be significant owing to their failure to express propositional content; and how this failure is related to the manner in which what is asserted is coupled to what the assertion is about, in a categorymistaken predication. It cannot go so far, though, as to deny <u>any</u> content to a category-mistake. In this respect, an account of the contribution of contextual features to significance has to <u>grade</u> contexts in respect of a measure of the content a given predication may express in a range of contexts.

What I mean by a "grading" of contexts, here, is that, instead of evaluating the statement-capability of a given utterance in relation to each, discrete context of assertion (construed as a partial modelling m of the sets of statements relevantly compatible or incompatible with the given assertion); we should consider a certain set S of extensions to the partial modelling m which are not just completions of m, but comprise different models which conflict only in certains ways with m. Such models will represent semantically the (hypothetical) situations in which the standards, say, for a predicate to apply to what a given utterance is about are either set so much higher as to exclude items that already satisfy the predicate in m; or else in which standards are set so much lower that items not fulfilling the conditions for a predicate to be (made) true or false of them in m, now fall within the range of significant application of the predicate. This (hypothetical) raising or lowering of the standards for an atomic predicate to apply to what a given utterance is about--together with consequential adjustments to the evaluation of its truth-functional and quantificational counterparts--thus induces an ordering over families of contexts. This ordering is similar to, but not identical with, the ordering that the comparative of an adjective, e.g. "is heavier than", induces over the subsets of a domain assigned to the corresponding simple predicate "is heavy": see, for example, Kamp's account of models of comparatives in Kamp, 1973, pp. 138-9--not that Kamp's approach can be made to fit the account I give immediately.

Such an ordering, we can say, provides an ordinal measure of

how much the interpretation of a given predication would have to diverge from a designated "minimal" interpretation (the "literal" meaning of the predicate) in order for a nonsignificant utterance of that predication to become successful in yielding a true or false statement. In other words, a measure of the significant content an utterance could express, in a given context in which its aboutness is fixed, is provided by the structure and number of contexts, qua partial modellings, which differ from the given context only in assignments to relevant statements, such that completing the alternate models, in each case, would either leave the "statement-value" of the utterance still undetermined, or would determine it as statement-yielding. This last description appears complicated-expressed informally--yet, it is much shorter when imported to the semantics for CS-1. I will be concerned to point out first, though, how this approach does reflect a number of the intuitive theses an account of significance-failure should capture, many of which I have argued for in Part I. At this point, it is important to observe that it does not follow from this description of the successive "sharpenings" or "blurrings" of a predicate's range of application, that a predication will have differing "degrees of significance" in each context. In any one context, an utterance will either be significant and statement-yielding or it will not. To understand why this does not follow is crucial to the application of CS-1--so I discuss this matter first:

(II) The Significance Logic CS-1:

CS-1 realises, in part, the aims of I, Section D and II, Section B

to amalgamate three separate features of accounts I have discussed in those sections: Russell's theory of types, Ryle's conception of categoryabsurdity, and Wittgenstein's doctrine of formal predication. Features of each of these accounts are incorporated in CS-l in the following ways ((a)-(c), below):

(a) From the discussion of Pap's account of type-mistakes in I, Section D (pp. 303-4), it was proposed to treat a category-mistaken predication--confining the discussion to predications only for the moment--as having a false presupposition which was to be a "typepredication" whose truth functioned as a condition for a given predicate to apply significantly to what an assertion is about, in a context. I reasoned, thus, that one intuitively obvious feature of denials of significance was captured by a claim disqualifying the predicate's application to a subject-item, because the subject-item--what a given utterance is "about"--is not of the appropriate 'sort' or 'type'; i.e. a significanceclaim. That is, the nonsignificance of a category-mistaken predication pertains to the manner in which the coupling of the predicate with an item mentioned by the subject term presupposes an incorrect allocation of items to sorts or types. For example, qu(Caesar is prime) in a context co in which qu(Caesar) is about the famous Roman, fails to state anything significant of Caesar because $qu(x \text{ is prime})(c_0)$ presupposes the truth of the statement \mathbf{s}_{x} is a number; whereas, for qu(Caesar) to be about the Roman conqueror of Gaul in co requires the truth of \$Caesar is a person. In brief, we can call the predications qu(x is a person), qu(x is a number) the super-predicates for the applicability of qu(x is

prime) to Caesar in the context.² (It does not follow that what is a super-predicate for the application of a predicate is always a superpredicate for it; nor that super-predicates do not themselves have presupposed super-predicates and other entailments). In consequence, the relevant incompatibility--in the sense defined in CL--between super-predicates entails that the literal assertion of primeness of Caesar, in context, has at least one false presupposition. When qu(Caesar is prime) has an unfulfilled presupposition in co, it is not statement-yielding; and in any context in which literal interpretation of the expressions demands simultaneous satisfaction of those super-predicates, or of predications which fix the aboutness of qu(Caesar) in context, the utterance will fail to be statement-yielding. A change in the modelling of the predicate, of course, or in the aboutness of the subject-term, will induce a corresponding change in one or another of the associated super-predications. In other words, by the Principle of Referentiality, when qu(Caesar) is about the number 23 in a suitably idiosyncratic context, the utterance ceases to have conflicting presupposed super-predicates, ceases to be category-mistaken, and yields not only a statement, in that context, but a truth.

The shortcomings of Pap's account, discussed in I, Section D, involved primarily its lack of an <u>independent</u> account of "type-predications". What makes a predicate a <u>super-predicate</u>? It is at this point that Pap's account is amalmagamated with features of three further accounts—the relationship between aboutness and identity (derived from Griffin's exposition of "Relative Identity" in Griffin, <u>1977</u>); Ryle's account of category-absurdity and Wittgenstein's doctrine of formal predications.

(b) To answer the question: what makes a predicate a superpredicate, in terms of some given heirarchy of type-predications (x is an individual, x is a property, x is a relation, x is a class, x is a number,....) would be to go immediately against my insistance in I, Section C (discussing Russell) and I, Section D, (discussing Aristotle) that a theory of category-mistakes should not, and need not, depend upon prior determinations of category- or type-membership (a theory of categories). Pap's account fails precisely through his construing typepredications as "absolute" and fixed; rather than, as I shall proceed to argue, relative to how we identify the aboutness of expressions in context, to how we introduce topics into discourse, and to how we sort and classify on the basis of our descriptions. A doctrine of "relative types" in this sense, is not new--it is written into Russell's PM account of the ramified theory of types (see: I, Section C, pp. 166-7). The following necessarily brief discussion seems to me to redescribe the Russellian doctrine of relative types as a semantic, rather than syntactic, doctrine. Equivalently, it describes, in some sense, what is left of Wittgenstein's account of "formal (type-) predicates" and the doctrine of showing, when the atomistic underpinnings of the Picture Theory of Meaning are removed.

(c) A frequent misinterpretation of Russellian type-theory (e.g. in Black, <u>1944</u>) has been to construe the heirarchy of types as a <u>fixed</u> classification of kinds (individuals, properties, relations, classes, numbers,..., and so on.) An equally frequent misinterpretation of Wittgenstein's doctrine of showing (evident, for example, in Sellars,

1962, in Copi and Gould, 1965, pp. 251-4) is to construe the <u>formal</u> type-differences shown in the syntactic difference between name and predicate variables, say, as mirroring an irreducible type difference in the objects configured in the state of affairs depicted. That is, it supposes Wittgenstein's analysis of the fact that-"aRb" can depict a's standing to b in the relation R, as requiring a type-difference between the individuals a, b and the relation R to be uniquely displayed in the symbolic difference between "a", "b" and "R".

Both are misinterpretations: the first, of Russell, because he insists both upon the relativity, in practice, of type-assignments (PM, *12, pp. 161-2) and upon the systematic ambiguity of notions like 'type', 'relation', 'individual', 'class', and 'function' across types (PM, p. 64). That the second is a misinterpretation of Wittgenstein requires a more careful exegesis of the Tractatus than I can give, here (c.f. I, Section D, pp. 252-263). It may suffice to indicate the consequences, though, of T.4.126 - 4.1274--the account of formal concepts as a "feature of certain symbols"; of T. 5.2 that "the structures of propositions stand in internal relations to one another"; and of 4.123 that "a property is internal if it is unthinkable that its object should not possess it ... hence the shifting uses of the word 'object' correspond to the shifting use of the words 'property' and 'relation'." Briefly stated, the consequences are: that a type-difference is shown in the signs occurring in, say, qu(Øx) is a feature of what makes these signs, combined in that way, symbols for a named object's having a property. This cannot be a property of the fact depicted, for, by 3.326, a sign becomes

a <u>symbol for</u> an individual, property, relation, etc., through its being <u>used</u> with a sense. Thus, the type-difference between "x" and " \emptyset " in $qu(\emptyset x)$ is a feature of the way we represent objects having properties-it is, thus, relative to the representational forms of the symbolism. It follows that predications of <u>formal</u> concepts are not <u>true of</u> items in the world. They apply, instead, to the manner in which we can (and do) represent the world through the representational modes of assertion, picturing, mapping, thinking, and so on.

We should infer from this that the description of the structure of superpredicates has to be understood as giving, <u>not</u> a list of typeproperties possessed by irreducibly different kinds of item, but a description of the conditions any representations of a particular subjectmatter have to satisfy to preclude type- and category-nonsense in its classifying, sorting and categorising of that subject-matter.

The problem which arises, then, is one of 'fitting' these three features of previously discussed accounts to the formal basis for the significance logic CS-1. The manner in which these features 'fit' CS-1 can be summarised as follows:

(i) In so far as type- and category-differences are to pertain properly only to our ways of sorting, classifying and describing (by using expressions <u>about</u> items and ascribing properties and relations to them), it need not hold that what is presupposed in the aboutness of a subject-term and the applicability of a predicate stays the same from context to context.

(ii) Ultimately, we do not want to deny (as Wittgenstein did) that type- and category-differences <u>can</u> be meaningfully stated and discussed; i.e. that to predicate a superpredicate which is presupposed in an expression's being about some item in a context is itself to make a significant predication, true or false statement-yielding in context. If I hold, for example, to some strong form of the thesis that individuative reference to particular items is incoherent unless some sortal concepts are available to provide individuating principles and criteria for identifying and re-identifying items of those sorts, then it seems that I am committed to holding that some superpredicates--the so-called "ultimate sorts" or "<u>summa genera</u>"--must be true of whatever they can be significantly predicated.

In other words, the thesis being considered is that making public reference to items, by the use of expressions functioning as singular terms, requires certain true beliefs <u>about</u> such items which identify them as of a sort or type associated with which are principles individuating items of that sort and criteria of identity and distinctness for those items. Having such beliefs commits one to <u>denying</u> the significance of some assertions in context, whose aboutness or statement-capability are incompatible with conditions one regards as necessary to having made a statement which, in context, is publicly <u>about</u> items of that sort or type. Such denials, I maintain, are precisely the force of those assertions I have called "significance claims".

(iii) Such a thesis, though, has to be formulated very carefully in order not to commit its proponent to some form of "significance essentialism": literally, the doctrine that some predicates true of items designate properties necessarily possessed by those items; i.e. that some properties are such that, without them, certain items would not <u>be</u> the individuals, qualities, relations, etc., that they are. I do not believe that this latter doctrine is true, or even coherent. So, to avoid the semantics of CS-1 having this kind of essentialist committment, it is necessary to stress that the truth-conditions of significance claims reflect not so much the <u>essential</u> properties necessary to items for us to identify and individuate what is being mentioned in context, as the necessary features of our identifications of what is talked <u>about</u>, and the conditions for our performing a successful act of statement-making, in a context, in relation to the aboutness and significance of what we say.

The following formalism--based upon CL of II, Section C, and borrowing some features of the significance logics of Routley and Goddard (<u>1973</u>, especially pp. 590ff)--is intended co capture the three salient features abstracted from previous accounts of type- and category-mistakes that I have described above ((a)-(c)).

(III) Formal development of CS-1:

The primary purpose of CS-1 is to represent in a direct and somewhat

rudimentary manner the semantic relations between what an utterance is <u>about</u>, what it <u>states of</u> what it is about, the contribution of context, and the structure of superpredicates presupposed in identifying and individuating what an utterance is about and what it states in context. The first part of this task, upon which I concentrate in this subsection, is to elucidate the notion of <u>aboutness</u>. So as not to encumber the text, here, with <u>all</u> of the complexities required to interpret diverse kinds of subject-terms and complex predications, I begin by listing the simplifying assumptions I make for CS-1:

1) CS-1 subdivides the complex multiple relation:

S(speaker) utters $qu(\emptyset)$ about a in context c_0 to state of a that- \emptyset

into the separate relations:

(i) qu(Fx₀)(c₀) Y §Fa. (dyadic)
(ii) qu(Fx₀) is about a in c₀. (triadic)
(iii) qu(Fx₀) states of a that-F. (triadic)
(Reference to a speaker S can, if necessary, be regarded as contextually determined).

2) CS-1 explicitly excludes interpretation of the aboutness of complex singular terms containing nested quantifiers and/or qu-expressions.

3) CS-1, like CL, is an applied, many sorted first-order language with primitive non-logical operators: 'qu(-)', ' \mathbf{S} (-)', and '-- Y ...'. CS-1 supplements CL with restricted <u>second-order</u> quantification over predicates Ø, \mathbf{V} ,.... in the field of a constant relation $\Delta Ø(\mathbf{v})$ over properties whose left-field comprises values of general term predicates A, B, C,...; and whose right-field comprises values of n-adic predicates. In addition, CS-1 adopts a defined relative-identity relation 'x = $_{\mathbf{0}}$ y' (from Griffin, 1977, pp. 140-141) whose interpretation partitions the extension/anti-extension pairs assigned by R, in the partial modelling m of CL, to predicates, into equivalence-calsses-the "sortal" and "mass" term ranges of general terms. Thus, CS-1 requires a <u>second-order</u> base logic (based upon Church, 1956, pp. 346-354); but lacks strong abstraction axioms for hetero-typical relations like membership, instantiation, attribution, between individuals and non-individuals (properties, classes, etc.). Restriction to order-2, though, is necessary for standard Henkin semantic consistency and completeness results to extend to CS-1.

4) CS-1 proscribes <u>all</u> intensional operators (modal, epistemic, doxastic, deontic) save for the qu-operator composed with the arguably "intensional" Y-relation and predicate 'Sig(qu(-))'. Modal operators, and others, <u>could</u> be introduced into CS-1, though the variations in partial models across <u>both</u> contexts and 'possible worlds' complicate the semantics beyond perspicuity.

5) A final heuristic simplification for CS-1 is to confine interpreted swffs, for the moment, to <u>atomic</u> monadic predications, truthfunctions and quantificational closures, thereof. This simplification is made solely in order that the detail of aboutness-assignments, superpredicates and context-conditions can be described without complicating the modelling with the detail of swffs containing complex subject-terms and relational predications. In the next section (E), the supplementary features of CS-2, necessary to incorporate more complex predications, are discussed.

Having listed five restrictions upon the application of CS-1, it would be well to point out immediately that CS-1 is not without certain desirable features. Unlike extensional, generalised second-order logics, CS-1 has wide-ranging quantifiers (first- and second-order). From the preliminary discussion of 'aboutness' below, it should be noted that the aboutness of subject-terms and quantification over items mentioned in context does not carry existential committment--for, the items speakers can successfully talk about (i.e. mention) may be fictional, merely possible, or even impossible. Nonetheless, a <u>defined</u> relation of <u>reference</u>-a species of aboutness--can be introduced into CS-1, to restrict mentionable items to those satisfying existential and uniqueness conditions which qualify referential success for some uniquely referring terms. For example, successful use of a demonstrative qu(this), qu(here) carries existential committment, unless it is used anaphorically, in context, as in qu(This (previously mentioned item) is-F). Secondly, all connectives in mentioned swffs of CS-1 have strongly regular (though not two-valued) matrices--as defined in II, Section C, pp. 469-71. The defined analogues of sentential connectives are precisely the (truth-functional) <u>statement-relations</u> of CL, given in Df. V(i), pp. 483-5, including a strong, multi-valued relevance relation of implication "->" whose use was explained informally at the end of II, C.

Thirdly, CS-1 preserves the central feature of CL that semi-wffs (which may be statement-failing or nonsignificant in context) are never used in CS-1, but appear only within the scope of a qu-operator, and are thereby mentioned. This last feature follows from the explicit exclusion of terms containing nested qu-operators, and from the requirement that, to evaluate whether a mentioned utterance, in context, is true or false statement-yielding, nonsignificant or significant, is to use a wff of CS-1 which is, itself, significant and true or significant and false statement-yielding, relative to the minimal context cs. In other words, the minimal, background context "cs"--discussed in II, C, pp. 512-13 -- is simply identified with the general context in which theses of CL and CS-1 are asserted, and which is logically standard (i.e. classical) with respect to valuations. Thus, though any mentioned swff may be nonsignificant or statement-failing when interpreted in its context, asserting the significance, nonsignificance, statement-success or statement-failure of the mentioned swff is always to make a true or false assertion relative to c_s . So, suppose $qu(p_0)(c_0)$ fails to yield a statement, because category-mistaken; then:

(i) Sig(qu(p_0)(c_0)--abbreviated: Sp₀ = qu(p_0) is significant in c_0 --is significant and yields a false statement relative to c_s .

(ii) \neg Sp_o is significant and true (statement-yielding in c_s).

(iii) Tru $(qu(p_0)(c_0)$ --abbreviated: $Tp_0 = qu(p_0)$ yields a truth in c_0 --is significant and false in c_s .

(iv) \neg Tp is significant and true in c_s.

(v) $Fal(qu(p_0)(c_0)$ --abbreviated: $Fp_0 = qu(p_0)$ yields a falsehood in c_0 --is significant and false in c_s .

(vi) \neg Fp_o is significant and true in c_s.

Finally, without giving an explicit list of inference-rules and defined proof-procedures for CS-1, I have assumed, for the sake of brevity, that some fairly standard natural deducive structure of inference-rules-applied only to wffs of CS-1 (not swffs)--can be incorporated into the formal language and its theories formed by explicit adoption of postulates for 'aboutness' and 'significance'. In general, what this inferencestructure is will be obvious from the derivations I cite.

For ease of exposition, I have divided the supplementary apparatus which extends CL to CS-1, restricting it, at the same time, in the way mentioned above, into three subsections (A - C, below). In each subsection I describe informally the rationale for the manner in which the notion is introduced and then proceed to its formal explication. Summarising, in advance, what each introduces, we supplement CL with:

(A) a formal explication of what it is for a restricted class of swffs--monadic predications, truth-functions and quantificational closures thereof--to be about items in a context. This is achieved by <u>defining</u> "aboutness" in terms of an explicitly adopted formulation of a relativeidentity relation and a modelling in terms of 'aboutness-assignments'-analogues to the algebraic semantics of value-sets, for the domain <u>St</u> of CL; (B) a formal semantics for the same restricted class of swffs which represents the entailments and presuppositions of an interpreted swff in terms of the sortal and identity-features necessary to determine the aboutness of the subject-term of a predication. The aim is to identify the "significance conditions" for a predication qu(Fa) in terms of the class of presupposed and entailed statements, which, in the context, are represented by value-sets. Thus, the semantics developed in B is, in part, the formal realisation of Ryle's claim--discussed in I, Section D, p.342 --that type- and category-claims involve the totality of logical relations into which a given assertion can enter with other ("relevant") statements.

(C) a formal description of the significance-failure of predications through category-mismatch, as contrasted with statement-failure through referential-failure, ambiguity or incompleteness. Categorymistaken utterances, at least for this restricted class of predications, are finally identified with statement-failing utterances which, in a range of contexts in which they are 'about' the same items, have incompatible presupposed superpredicates. The relations between 'types' and 'superpredicates' are discussed in Section E.

(III) (A) Aboutness:

It has been claimed (e.g. by Goodman, <u>1961</u>, pp. 1-24) that the relation " $qu(p_0)$ is about <u>a</u> in a context c_0 " is insufficiently precise to admit formal explication. Certainly, there are many reasons--several discussed below--to accept Goodman's reservations and to concede that, for some assertions, formal explication of what they are about will have to embody some fairly arbitrary semantic decisions. At least three problematic features of the notion of 'what an assertion is about' deserve primary attention.

Aboutness, as I intend to use the term, is a surrogate for 'reference'; but is construed as wider in application. "Reference", though, has been used, customarily, for a relation having at least three separable senses, not all of which can be subsumed under aboutness:

(a) "A refers to a" has sometimes been confined only to uses of

names, singular definite descriptions and demonstratives which pick out a unique individual as referent. In this sense, predications containing other types of subject-term may be <u>about</u> items, without being used to <u>refer</u> uniquely to them. Qu(Gold is a metal) is about gold; but does not refer to an individual 'gold', since 'Gold" is a matter-kind, not a single existent.

(b) "A refers to a" has sometimes been used to characterise the relation between any expression and what it stands for. In Frege's sense (1894), every expression has reference (Bedeutung)--singular terms to objects, predicate-expressions to functions and concepts, sentences to truth-values. Save in a Fregean semantic theory, this sense is too wide for most purposes.

(c) "A refers to <u>a</u>" is occasionally used to describe the illocutionary <u>success</u> of a speaker's referential use of a term. In this sense (evident in Strawson, <u>1964</u>) no necessity attaches to the actual existence or uniqueness of an <u>a</u>, provided that illocutionary 'uptake' is secured in the context (when the audience identifies what the speaker intends in using the referential term).

Sense (c) is closest to what I intend for 'aboutness'; whilst sense (a) is that to which I confine the term 'reference'. Thus, a speaker's utterance is <u>about</u> whatever is publicly identified in the context as being that of which his assertion makes a true or false claim (if anything). The speaker has failed to make a truth-claim if his utterance is not about anything. Further, in sense (a), some of a speaker's utterances will contain expressions used of at least and at most one item, in context. Thus, the speaker <u>refers</u> to that item when there is at least and at most one item which his utterance is about in that context. Failure of reference, in this sense, is a species of statement-failure <u>through</u> the failure of his utterance to be about some one item, in the context. The problem in explicating reference in terms of aboutness is, thus, to separate the conditions for the <u>success</u> of a speaker's illocutionary act, from the semantic conditions for his use of a singular term in context to be about at least and at most one item. I will be concerned exclusively with the second of these senses.

Two further problems arise for this generic relation of aboutness: $\underline{Ab}(i)$ the aboutness of non-referring terms as subjects of predication; and $\underline{Ab}(ii)$ the aboutness of non-atomic (truth-functional and quantificational) assertions.

<u>Ab(i)</u> It is clearly insufficient to identify the aboutness of an utterance with <u>any</u> admissible answer to the question "What is being talked about?". Such a polygamous relationship is borne to everything there is; everything there might be; most of what there cannot be; and anything else besides. In this sense, could there ever be a <u>failure</u> to be talking about something?

There is, of course, a trivial sense in which <u>no</u> utterance fails to be about something--in which, say, qu(snurds eat lans) is about snurds and lans. For formal purposes, this trivial sense of aboutness--the limiting case--can be simply regarded as composing mentioning by quotation with (absolute) identity. Since any question as to what is being talked about can be answered (vacuously) be repeating the subject terms, then:

(*) $qu(Fa)(c_0)$ is about <u>b</u> iff <u>b</u> = a (c_0) . represents a condition on aboutness entirely analogous to Tarski's Tconvention on definitions of truth:

(T) $qu(p_0)$ is true iff p_0 --of which the notorious example is qu(Snow is white) is true iff snow is white.

(T), as a condition upon truth-definitions, has to be regarded as a

<u>consequence</u> demanded of any adequate <u>explanation</u> of what it is for a statement to be true. (T) does not itself offer any account of truth. Similarly, I suggest, (*) provides a condition upon aboutness which should be a <u>consequence</u> of an adequate formal explication. Of itself, though, it tells us nothing as to what it is for an utterance to be about something.

A more interesting approach to 'aboutness' is provided by the question: since to talk about something publicly is to agree upon some information true of what is being talked about (share some true beliefs about an item), what are the necessary and sufficient conditions for speaker and audience to be correct in identifying what is being talked about? Owing to the frequent cases when a community of speakers can, in context, be mistaken as to what they are talking about, or can be talking at cross-purposes, it is more appropriate to frame this question in terms of the correctness of identifications (as I have), than in terms, merely, of agreement as to what is talked about. On the other hand, illocutionary success in perceiving what is talked about is often achieved without speaker and audience being correct in their identifications in a context. If you and I both have a false belief that the element with atomic number 65 is plutonium, my recommendation that you avoid handling the element bearing that number (say, on marked containers) may be illocutionarily successful "malgré nous", as it were. This problem, though, I shall shelve in the following by concentrating upon the question of correct identification.

For the moment, I concentrate upon the important relationship

between "aboutness" and "identification". The clue that aboutness composes quotation with identity, provided by (*), may be carried further. We can examine how the use of expressions <u>about</u> some determinate item (or items), in a context, requires or presupposes the means of <u>identify-</u> <u>ing</u> the item(s) as of determinate sorts or types. Identifying an item as of a sort or type, characteristically involves applying further predicates and relations which individuate and provide identity-conditions for items of that type. For example, when qu(she is old) is about a woman, in a context, determining the aboutness involves the application of further predicates <u>true of</u> women and individuating <u>this</u> woman amongst others.

Very roughly, then, I shall argue that, for a predication to be about an item, certain other predications, true of that item stand in logical relations to the predication. That is, specifying what a predication is about ordinarily involves further predications sufficient to identify the item as of a sort or type; to provide for its being a 'unique' instance of the type (when the predication contains a referring term); and to differentiate between the item and other instances of the sort or type.

Expressed thus, the relationship between aboutness and significance comes to the fore. On the most general description I have offered, a category-mistaken predication is nonsignificant when, in the context of assertion, what is stated of the item(s) it is about is not significantly predicable of item(s) of that sort or type. Anticipating somewhat the later description of significance conditions in CS-1, we can say that a predication is category-mistaken in context when the conditions for it to be <u>about</u> an item are statement-incompatible with presupposed conditions for the predicate to apply to an item of that sort or type.

The thesis that reference to items, and more generally talking about items, requires provision, in context, of principles individuating such items and of criteria for the identification and re-identification of items of a sort, can be borrowed, almost wholesale, from <u>two</u> sources. I shall not <u>argue</u> for this thesis, here, except to the extent that explaining it requires that I argue against various possible misinterpretations.

Firstly, P.F. Strawson (in <u>1959</u>, Chs. V, VI; <u>1961</u>, pp. 74-85; and <u>1974</u>, Ch. 1) has advocated consistently two general criteria distinguishing the role of subject-terms in identifying what is talked about from the role of predicate terms in declarative assertions. (Recall that problems over <u>making</u> this distinction in CL were deferred from the conclusion of II, Section C.) The two general criteria—the "identificatory-" and the "type- or category-criteria", respectively-are expressed in terms of singular, referring terms <u>versus</u> predicative terms, though they can be extended to subject- and predicate-terms in general. Strawson summarises them as follows:

Referential position (in a predication) is the position primarily .. occupied by a term <u>definitely identifying</u> a spatio-temporal particular in a sentence coupling that term to another signifying a property-like or kind-like principle of grouping particulars ... Second, referential position <u>may</u> be occupied by a term signifying a propertylike or kind-like principle of grouping particulars

provided that this term is, itself, coupled to another term which signifies a higher principle of grouping such principles of grouping. (Strawson, 1961, p. 85).

Ignoring the mention of the primacy of spatio-temporal particulars, what Strawson intends, and what I abstract from his account, is that the characteristic function of subject-terms in a predication is to make identificatory reference to items; whereas the characteristic function of predicate-terms is to signify property-like or kind-like principles of grouping items. When general terms appear in subject-position, they do so, qua principles of grouping, only in falling within the signification of higher-type principles of grouping. It follows that, for a subject-term to carry out this role successfully; i.e. for a term in subject-position to be successfully about items, use of the term commits the speaker to various predications adequate for the identification and individuation of the items the predication purports to be about. Secondly, to the extent that the role of a predicate-term is, in general, to supply property-like or kind-like principles of grouping for either the items identified by use of a singular term or the lower-type properties or kinds signified by a general term, then a significant coupling of a subjectterm to a predicate-term should--either from the context, or from the predication itself-supply principles of sorting and individuating which reconcile the application of the predicate-term to item(s) of the type which is required for successful identification of the item(s) the subject-term is about. So, a nonsignificant coupling--a categorymistake-will be one in which, in the context, these principles of sorting and individuating cannot be reconciled.

Extrapolating from Strawson's account has provided the most general description of the relation between aboutness and the significant and non-significant coupling of expressions (terms) in predications. A second, more specifically useful, thesis can be abstracted from N. Griffin's account of the role of general terms in the truth-conditions of relative-identity assertions (Griffin, <u>1977</u>). In replying to some general objections to the notion of relative identity, Griffin advocates the following:

It is hard to see how any sense can be made of the notion of an individual item without individuating, and it is hard to see how sense can be made of individuation without sortals which supply the principles which make individuation possible. In view of this, it seems to me that, while all types of identity statements are admissible, sortal-relative identity statements have the most fundamental role to play, for, without them, we cannot make sense of the notion of an individual item. Once we have individuated some items and found, say, that the item named by "a" and that named by "b" are the same F, we can go on to ask if they share all their properties and are thus the same absolutely Classical semantics suggests things proceed in exactly the opposite manner ... The difficulty with this approach .. is in trying to make sense of the notion of individual items on which it is based. What are these items? and how much of the world does each take up? (Griffin, <u>1977</u>, pp. 158-9).

I am in complete agreement with the tenor of these remarks. In the following, I seek to exploit the structure of sortals, mass terms and relative-identities which Griffin has described in defence of his claim for the primacy of relative-identity over absolute (classical) identity. Of course, there are many theses advocated under the generic exposition of a relative identity thesis--few are uncontested and several are indefensible. I cannot at this point enter the theatre of debate over relative <u>versus</u> absolute identity. Wherever I make use of the apparatus of relative identity, therefore, I shall be content to refer to Griffin's defence of the thesis I require.

The claim is, then, that for a speaker to use a term successfully about some item in a context requires that, in principles, the context (including speaker, audience, and utterance) provide principles for the individuation and identity of items of a kind or sort of which the mentioned item is an instance. An immediate objection might point out that speakers frequently do refer successfully to items -- by demonstratives: qu(This is square), qu(That is bright); sometimes accompanied by ostensive gestures -- without providing individuating or identity criteria and without allocating the item ostended to a sort or type. The short answer is to draw attention to the fact that mere ostension, in a context, is rarely sufficient to identify what is talked about (or to reidentify an item, say, in a subsequent report, in the absence of the item). Further, the conventions for interpreting an ostensive gesture, in the presence of the item, as uniquely indicating the item (rather, say, than the surface of the item, or the space from fingertip to the item), are themselves sufficiently complex to include criteria identifying and individuating the item under some sortal or classifying term-even if recognising that the criteria apply is often (but not always) tacitly understood.

How is this connection between 'aboutness' and identity-criteria to be expressed? A first attempt at necessary conditions for aboutness-bearing in mind (*) (p. 546, above), as a limiting case--should at least provide for the appropriate formal properties of a relative-identity relation. From (*), the following gives the first general condition upon aboutness:

Df. <u>Ab</u> I: $qu(Fa)(c_0)$ is <u>about b</u> if $(E\emptyset)(b =_{\emptyset} a)(c_0)$. which reads: "A predication qu(Fa) in a context c_0 is about an item <u>b</u> if, for some property \emptyset , <u>b</u> is the same \emptyset as a, relative to c_0 ". (Addition of an "only-if" clause to <u>Ab</u> I is discussed below, in B). Following Wiggins (<u>1967</u>, p. 2), \emptyset will be called the "covering concept" for the identity statement. Thus, the aboutness of a predication is made to depend upon identification of the item under some suitable covering concept (relative to context-specification). Griffin and Routley (unpublished; see: Griffin, <u>1977</u>, pp. 133-136) have shown that the relation "x =_{\emptyset} y" satisfies the formal requirements of an identity relation: reflexivity, symmetry, transitivity and restricted substitutivity. In CS-1, these features can be explained as follows:

Subject to later expansion and semantic explication, let $\Delta \phi(\psi)$ be a constant relation on properties (a function from predicates to predicates) which we add to CS-1 with the formation-rule:

where \emptyset, ψ are <u>monadic</u> predicates, $\Delta_{ij}(\psi)$ is a wff of CS-1. Intuitively, each relative identity statement $x_0 = \theta_1 y_0'$ is covered by a general term or phrase with which is associated a range of truthconditions for predications determined by that general term or phrase. Thus, for any items x_0 , y_0 and covering concept θ_1 such that $\oint \theta_1 x_0$ holds in the context, then when $x_0 = \theta_1 y_0$, there is a set of properties $\{\psi_1, \psi_2, \psi_3, \ldots\} = \Delta_{ij}(\psi)$ such that identity under θ_1 entails that for none of $\Delta_{ij}, \ (\psi_1 x_0 \cdot h \cdot \neg \psi_1 y_0)$ holds in the context. In other words, aboutness is fixed by the truth of a relative identity which entails the indiscernibility of items identified with respect to a range of properties associated with the application of a covering concept by Δ_{g} ; i.e. Δ_{g} indiscernible items. Absolute identity--the limiting case--makes <u>no</u> restriction upon membership of Δ_{g} . That is, relative to c_{s} , sentence and statement values of 'qu(-)' and '§(-)', respectively, satisfy Leibniz' Law:

(LL) a = b iff $(\forall \emptyset)(\emptyset a \longleftrightarrow \emptyset b)$ —provided that only extensionally determined predicates \emptyset are admitted in the range of the quantifier.

For many relative identities, however, identity under \emptyset , thus Δ_{\emptyset} -indiscernibility, does <u>not</u> entail absolute identity. For example, $\S_a =_{colour}$ b entails that a, b are indistinguishable with respect to a range of predications which may be colour-ascriptions and qualities ('a is bright', 'b is intense',...) and, for a colour constant c_0 , relations of colour-complementarily and difference ('a is darker than c_0 ', 'b is opposite-in-colour to c_0 ',...). It does not follow, of course, that a=b, absolutely, since there will be predicates ψ^{-1} outside of $\Delta_{colour}(\psi)$ such that $\psi^{-1}a$ but not- $\psi^{-1}b$. Thus, my car may be the same colour as your car, but our vehicles differ in respect of the truth conditions of several predications significant over cars.

Formally, $\Delta_{\phi}(\psi)$ restricts the range of predicates which are substituends for the quantifier in the appropriate version of (LL) for the right-hand side of Df. <u>Ab</u> I. Thus, we can introduce a <u>theory</u> into CS-1 by adoption of the following postulates for "x = $_{\phi}$ y"; and derive the formal properties of a relative identity relation: (see: Routley and and Griffin, loc. cit., pp. 14-18).

Consistency demands the standard assumption of the non-emptiness of the domain of the theory RI in CS-1, so we adopt the postulates:

(P1) $(\forall x) (E\emptyset) \neg S\emptyset x$ --which guarantees that not every predicate is significant over some item (i.e. no item has every predicate significant over it).

(P2) $(\forall x) (E\emptyset) \ \emptyset x$ --which guarantees that every item has some predicate significant over it (statement-yielding)

Next, we define the restriction of a second-order quantifier by $\Delta_{\phi}(\Psi)$, by means of:

(R1) $(\forall \psi \in \Delta_{g}) \triangleq =_{df} (\forall \psi) (\Delta_{g}(\psi) \longrightarrow \underline{A})$ --where \underline{A} is schematic for wffs of CS-1.

(R2) now provides a definition of relative identity (schematic):

(R2) $x = y = df (\forall \gamma \in \Delta_0) (\phi x \cdot \Lambda \cdot \phi y : \Lambda \cdot \gamma x \leftrightarrow \gamma y).$

Using theorems of Meyer's system R_{\rightarrow} of "relevant entailment", which was adopted for CL in II, Section C, we can now derive the appropriate formulations of the reflexive, symmetric, and transitive properties of the identity-relation:

(T1)
$$(\forall x)(\phi x \longrightarrow x = \phi x)$$
 (Reflexivity)

Proof:

1)
$$\phi x$$

2) ϕx
3) $\phi x \wedge \phi x$ (Repetition, \wedge -Intro, 1)).
4) $\psi x \leftrightarrow \psi x$ (Theorem, $R \rightarrow$)
5) ψx
5) $\psi x \leftrightarrow \psi x$ (Repetition, 3)).
6) $\Delta_{\phi}(\psi) \rightarrow , \psi x \leftrightarrow \psi x$ (\rightarrow Intro: x occurs in 4)-5).).
7) ($\forall \psi \in \Delta_{\phi}$) ($\psi x \leftrightarrow \psi x$) (R1, 6), ψ not free in ϕ).
8) $\phi x \wedge \phi x \cdot \wedge : (\forall \psi \in \Delta_{\phi}) (\psi x \leftrightarrow \psi x)$. (\wedge -Intro, 2, 7).
9) ($\forall \psi \in \Delta_{\phi}$) ($\phi x \wedge \phi x \cdot \wedge : \psi x \leftrightarrow \psi x$) (see *, below).
10) $x = \phi x$ (R2, 9).
11) ($\forall x$) ($\phi x \rightarrow x = \phi x$) (\rightarrow Intro, \forall -Intro, 1ines 1)-10)).

(*: the step from 8) to 9) is justified by the classical theorem-schema: $(p \land q \land .(\forall x) \underline{A}) . \longrightarrow : (\forall x) (p \land q \land . \underline{A})$ --where x does not occur free in p,q].)

(T2)
$$x = \phi y \longrightarrow y = \phi x$$

Proof:

1)
1)
2)
(
$$\forall \gamma \in \Delta_{g}$$
) ($\emptyset x \land \emptyset y \land (\forall x \leftrightarrow \forall y)$) (R2, 1).
3)
 $\Delta_{g} \qquad \underbrace{\emptyset x \land \emptyset y \land (\forall x \leftrightarrow \forall y)}_{0}$. (relative to Δ_{g}).
4)
4)
4)
5)
($\forall x \leftrightarrow \forall y$
5)
($\forall y \leftrightarrow \forall x$
($\land -\text{Elim}, 3$).
($\land -\text{Elim}, 3$), Theorem)
($\land -\text{Intro}, 5$), 6)).
8)
($\forall \psi \in \Delta_{g}$) ($\emptyset y \land \emptyset x \land (\forall y \leftrightarrow \forall x)$) (same as T1, *).
 $\forall -\text{Intro}, 7$).
9)
y = $_{g} x$ (R2, 8).
(T3) x = $_{g} y \land \land y = _{g} z \longrightarrow x = _{g} z$. (Transitivity)

Proof:

1)
1)
2)

$$x = g y \cdot \Lambda \cdot y = g z$$

 $\overline{\phi_X} \wedge \overline{\phi_Y} \cdot \Lambda (\Delta_{\phi}(\psi) \rightarrow : \psi_X \leftrightarrow \psi_Y) \quad (R2, R1, \Lambda - Elim, 1)$
3)
 $\phi_Y \wedge \phi_Z \cdot \Lambda (\Delta_{\phi}(\psi) \rightarrow : \psi_Y \leftrightarrow \psi_Z) \quad (R2, R1, \Lambda - Elim, 1)$
4)
 $\Delta_{\phi}(\psi) \rightarrow (\psi_X \leftrightarrow \psi_Y) \quad (\Lambda - Elim, 2).$
5)
 $\Delta_{\phi}(\psi) \rightarrow (\psi_Y \leftrightarrow \psi_Z) \quad (\Lambda - Elim, 3).$
6)
 $\Delta_{\phi}(\psi) \rightarrow (\psi_X \leftrightarrow \psi_Z) \quad (\Lambda - Elim, 3).$
6)
 $\phi_X \quad (\Lambda - Elim, 2)$
 $\phi_Z \quad (\Lambda - Elim, 3).$
9)
 $\phi_X \wedge \phi_Z \cdot \Lambda (\Delta_{\phi}(\psi) \rightarrow : \cdot \psi_X \leftrightarrow \psi_Z) \quad (\Lambda - Intro, 5, 7, 8)$
10)
 $(\forall_{\psi} \in \Delta_{\phi})(\phi_X \wedge \phi_Z \cdot \Lambda (\psi_X \leftrightarrow \psi_Z)) \quad (\text{same as T1, *}).$
11)
 $x = g z.$

Finally, an identity relation justifies intersubstitutivity on the basis of true identities in appropriately restricted predications. This is licenced by the following <u>substitutivity</u> theorem-schema:

(T4)
$$x = y \cdot \Lambda \cdot \Delta_{g}(y) \longrightarrow : \forall x \longleftrightarrow \forall y.$$

Proof:

(T1) - (T4) establish the classical properties of an identity relation (<u>qua</u> logical determinable) with respect to the defined relation ' $x = \phi y'$. Interpretation of the relation is, of course, subject to determination of the structure of general terms ϕ which serve as covering concepts for relative identity fixing aboutness in context, and to further explication of the primitive relation upon properties $\Delta_{\phi}(\phi)$, below.

Membership of the set of properties associated with a covering concept by means of $\Delta_{\phi}(\psi)$ is difficult to determine precisely. In formal terms, the closure conditions on $\Delta_{\phi}(\psi)$ under negation, conjunction and entailment can be taken from Griffin, (<u>1977</u>, pp. 140-141); together with sufficient conditions for Δ_{ϕ} -indiscernibility to ensure ϕ -identity--by adding the identity-criteria conveyed by ϕ to the set. These conditions are described in terms of the "aboutness-models", given below, in the concluding subsection C, of this Section.

<u>Ab</u>(ii): The second major problem confronting a formal explication of the aboutness-relation, as mentioned above (p.5+6), concerned semantic determination of the aboutness of non-atomic (truth-functional and quantificational) swffs of CS-1.

I proceed now to describe this problem for CS-1, and revise the algebraic semantics given for CL to overcome it. It has to be pointed out immediately that, in answering this problem, several further difficulties, of a formal semantic nature, arise in connection with interpreting the 'aboutness' of quantified swffs in CS-1. I describe these difficulties, below, and in Section E, I will offer some indication of how they may be overcome. Nonetheless, at present, the difficulties must await the development of more sophisticated model-structures for second-order formal languages.

There is a clear intuitive sense in which qu(Mick Jagger has

long hair) is about Mick Jagger. But what is qu(Mick and Bianca Jagger have long hair) about? Presumably, both Jaggers. Thus, if $qu(p_0)$ is about Mick Jagger and $qu(q_0)$ is about Bianca, then $qu(p_0 \& q_0)$ is about Mick and Bianca Jagger.

Suppose, now, one of the Jaggers has a haircut. Then, it is no longer true that- $(p_0 \& q_0)$, so we assert $qu(\sim(p_0 \& q_0))$. Since $qu(\sim(p_0 \& q_0))$ is ordinarily equivalent to $qu(\sim p_0 v \sim q_0)$, this last still appears to be about Mick <u>and</u> Bianca Jagger. It is sensible to affirm that it is. So, it seems that aboutness is unaffected by truth-functional composition.

This can lead to puzzling consequences, however. Suppose $qu(r_0)$ is an atomic swff independent from $qu(p_0)$, relative to the topic x (= Mick Jagger, which $qu(p_0)$ is about). Thus, $qu(r_0)$ gives us no definite information about x and cannot be said to be <u>about</u> x--qu(r_0) might be qu(Mars is a star) which presumably tells us nothing about Mike Jagger. If <u>A</u> is $qu(p_0 \& r_0)$, then not-<u>A</u> is $qu(\sim p_0 \lor \sim r_0)$ which appears not to give us the definite information about Jagger and Mars which $qu(p_0)$, $qu(r_0)$ gave us separately. Should we still say that not-<u>A</u> is about <u>both</u> Jagger and Mars, like <u>A</u>, or about either Jagger <u>or</u> Mars, or about neither?

The puzzle concerns composition of aboutness with negation and disjunction. Suppose we say that the statement yielded in a context by $qu(p_0)$ entails that $-p_0 \vee q_0$ --where $qu(q_0)$ is also statement-yielding. In some sense, we want to claim that for an entailment $A \longrightarrow B$ to hold, whatever B ends up being about, A must be about. For, on information-

theoretic grounds, it is universally assumed that the consequent of a logical entailment can convey no more information than its antecedent; and, thus, cannot be about more than its antecedent, (c.f. Carnap and Bar-Hillel, 1952, p. 16). On some accounts of entailment, though, that-Nixon is a Republican entails that-Nixon or Carter is a Republican--and the consequent seems to require an assertion about Nixon and Carter, whilst the antecedent does not. Alternatively, we might say, the consequent is not about Carter in the sense we intend; unless, heroically, we intend the antecedent to be about Carter as well. I think we should reason that, in a more exact sense, qu(Nixon or Carter is a Republican) is not really about Carter, because it fails to give us the definite information about Carter that the first assertion gives about Nixon (and fails to give us the definite sort of information about Nixon that the first gives, also). Thus we should say a disjunction is about only what it gives us definite information about; and that it gives us definite information about a topic x, only if each disjunct give us definite information about x. So, if some disjunct gives us no information about x (is not, thus, about x), we cannot expect the whole disjunction to be determinately about x, because the assertion of at least one disjunct does not introduce the topic x into the context. Dually, we might reason that a conjunction gives us definite information about only what each conjunct gives us definite information about, and is thus about the topic or topics thereby introduced.

Finally, though, as we observed above, negation causes a problem. Fairly obviously, if $qu(p_0)$ is about a topic x, then $qu(\sim p_0)$ is about x, if 'p_0' is an atomic predication. But, if <u>A</u> is $qu((p \lor q) \& q)$, then

not- \underline{A} is qu((~p & ~q) v~q); whereupon if qu(p) gives us definite information about x and qu(q) about y, it seems that neither \underline{A} nor not- \underline{A} gives us definite information about x and y. For, \underline{A} may be qu(either Nixon or Carter is Republican, but Carter is Republican), whence, not- \underline{A} is qu(One of Nixon and Carter is not Republican, or Carter is not); and we may be loathe to say this latter is <u>definitely</u> about both Nixon <u>and</u> Carter, or about either one. In one sense, intuition tells us that not- \underline{A} is clearly <u>about</u> Nixon <u>and</u> Carter—for it appears to mention both. In another sense, though, since not- \underline{A} is entailed by the denial of the statement yielded by qu(q)— $\frac{1}{2}$ -q entails $\frac{1}{2}(1p \land \neg q)$ v $\neg q$ --to claim that the latter is about <u>both</u> Nixon and Carter is to allow the consequent of an entailment to give us definite information about <u>more</u> than the antecedent, or to allow that the antecedent--qu(Carter is not Republican)--is <u>already</u> about Nixon; hence, to allow that it is about everything that it entails or by which it is entailed.

The solution I propose--counter-intuitive though it may appear-is to deny that knowing what <u>A</u> gives definite information about, is to know, <u>thereby</u>, what not-<u>A</u> gives information about, and conversely. In some cases, this seems to follow from ambiguities between "negating" and "denying": for example, qu(It is not that the least rapidly converging series is discontinuous) appears to be <u>about</u> the least rapidly converging series, except that it can also be read as <u>denying</u> that there is such a series--as in qu(It is not that the least rapidly converging series is discontinuous; rather there is no such series). Russell's distinction between large and small scope for description

operators is best understood as an explication of this ambiguity; i.e.

$$\begin{array}{ll} [\Im x(\emptyset x)] \sim \psi(\Im x(\emptyset x)) & \text{is} & (\exists y)(x) & (\emptyset X \leftrightarrow x=y. \land \sim \psi y); \text{ but,} \\ \sim [\Im x(\emptyset x)] \psi(\Im x(\emptyset x)) & \text{is} & \sim (\exists y)(x) & (\emptyset x \leftrightarrow x=y. \land \psi y). \\ & & (\text{Russell, PM, pp. 69-70).} \end{array}$$

Thus, it is not immediate that a denial is automatically about whatever the corresponding assertion is about, in context; since when a definite description is used referringly in an assertion, it may end up <u>not</u> being about anything (whence, the assertion fails to be statementyielding). Then, it makes no sense to claim that the corresponding denial is about the <u>same</u> thing (i.e. nothing). Rather, one should note that, owing to scope-ambiguity, if an asserted sentence is statementfailing, so is its <u>negation</u>, but the <u>denial</u> of an utterance is not necessarily the assertion of its negation. For it may be the affirmation of its statement-failure through its not being about a publicly identifiable item in context.

The upshot of this informal discussion is to motivate the interpretation of swffs of CS-1 in terms of valuations containing "aboutnessassignments" which map subject-terms in each swff onto a <u>pair</u> of topics from a domain--the first topic to be thought of as the items the swff gives definite information about, the second to be thought of as what the negation of the swff gives definite information about--always allowing that, for many simple predications, both may be the same topic, and that, for some, the swff may not be about a topic at all.

Formally, then, I identify the item a swff is <u>about</u>--the topic x introduced in the context--with the unit-class $\{x\}$; similar to the manner in which Quine holds to 'x = $\{x\}$ ' for the purposes of his system

NF of set theory, (Quine, <u>1961</u>, pp. 70-80). The formal advantage of identifying topics with the unit-sets of items is primarily to facilitate aboutness assignments for predications of anyadicity (relations, complex subject terms) in CS-2; though CS-1, itself, interprets only <u>monadic predications</u>. The simplification derives from the manner in which the <u>order</u> of subject-terms in an n-adic predication ($n \ge 2$) is preserved by the Wiener-Kuratowski definition of ordered n-tuples ($\langle a, b \rangle = \{a\}, \{a, b\}\}$) in the semantics of CS-2 (discussed in Section E, below).

I proceed directly to give the inductive conditions for an aboutnessassignment, relative to partial models m of swffs of CS-1, to fix the aboutness of truth-functional combinations of swffs in terms of assignments to their atomic components. This provides the first supplement to the semantic apparatus of CL to extend it to CS-1. Note, that <u>ulti-</u> <u>mately</u>, an aboutness assignment, relative to a model m, and context θ , does <u>not</u> tell us what actual item any given utterance is <u>about</u> in θ . That is an empirical matter decided, pragmatically, by applying the conditions for aboutness the structure of which is described below. Ultimately, of course, an utterance of, say, qu(Nixon is a Republican) may in some suitably esoteric context (or, say, in the context of a code) turn out to be about the surface of the sun. All that CS-1 provides are the formal conditions for an utterance to be about something or other, in relation to the representation of contextual features, the significance of utterances and their statement-capability.

The following definitions are derived in part from semantics

for first-degree entailments suggested by J.M. Dunn (<u>1966</u>, and private communication, March 1976):

A model-structure $m = \langle D, R, d \rangle$ for swffs of CL was defined in Section C, such that d assigns to <u>some</u> terms of CL elements of D, but is undefined for others. I now replace d by an aboutness-modelling $\langle D', s \rangle$, based on m, where D' = Dⁿ (the n-th Cartesian product of D ξ m, for each finite n) and s is a function assigning to each atomic swff a pair (x_1, x_2) of <u>subsets</u> of D'. Properly, x_1 , x_2 are unit sets $\{a\}, \{b\}$ where a, b are items an atomic predication and its negation are <u>about</u> (as defined by Df. <u>AbI</u>). Frequently, of course, a = b. When an atomic swff and its negation fail to be <u>about</u> anything, in a context, instead of d being undefined, s gives (λ, λ) as assigned subsets of D'. Thus, for any a, b ξ D, $a,b \notin \Lambda$ (the null-set)--null-set products are subsets of every product Dⁿ of D--and neither the predication nor its negation is about any item.

Various operations upon assigned aboutness-pairs can be defined. With the aid of these, we can express the logical properties and relations obtaining in the domain <u>St</u> of statements, when truth-functionally compound swffs are assigned aboutness in terms of assignments to atomic swffs. Indeed, it can be shown (by appeal to results of Dunn, <u>1966</u>) that the algebraic properties obtaining in the domain of statements, and described in II, Section C--in terms of which validity, statemententailment and statement-identity were defined--are preserved when aboutness modellings $\langle D', s \rangle$ are added to the partial model-structure $\langle D, R \rangle$. Thus, define: Df. Ab II: for any x_1 , x_2 in D', $\begin{array}{l} {\rm N}({\rm x}_1,{\rm x}_2) \ = \ ({\rm x}_2,{\rm x}_1) \, . \\ ({\rm x}_1,{\rm x}_2) \ \underline{{\rm v}} \ ({\rm y}_1,{\rm y}_2) \ = \ ({\rm x}_1 \ \cap \ {\rm y}_1, \ {\rm x}_2 \ \cup \ {\rm y}_2) \, . \\ ({\rm x}_1,{\rm x}_2) \ \underline{\wedge} \ ({\rm y}_1,{\rm y}_2) \ = \ ({\rm x}_1 \ \cup \ {\rm y}_1, \ {\rm x}_2 \ \cap \ {\rm y}_2) \, . \end{array}$ (c.f. II, Section C, pp. 485,491.)

from which we can observe that the modelling $\langle D', s \rangle$ is intended to determine a distributive ordered lattice of aboutness-pairs for which a partial ordering is defined by 'P, \leq P_i':

Df. <u>Ab</u> III: $(x_1, x_2) \leq (y_1, y_2)$ iff $y_1 \subseteq x_1$ and $x_2 \subseteq y_2$. (Usually, for atomic, monadic predications: $y_1 = x_1$ and $x_2 = y_2$, and $x_1 = x_2$).

Where Ab = $\langle D', s \rangle$ is an aboutness modelling, relative to m = $\langle D, s \rangle$

R,Ab, we define an <u>aboutness-valuation</u> determined by <u>Ab--</u> $\langle v, w_{ab} \rangle$ --as in Section C, pp. 448-449, as a valuation $\langle v, w_{ab} : v: \underline{Swff} \longrightarrow \{0, 1\}$, w_{ab} :

Term $\rightarrow \langle D', s \rangle$, agreeing with m on the clauses:

- (1) v: Swff \rightarrow {0,1} remains the same for clauses (3)-(7) of the definition (p. 448) of a valuation for CL.
- (2) (i) if $\emptyset \in \underline{Swff}$ and \emptyset is of the form 'Fx₀' for x₀ $\in \underline{Term}$ ('x₀' is a constant), then $w_{ab}(\emptyset) = s(\emptyset)_m$ --which replaces clause (2) of p. (ii) if $\emptyset \in Swff$ and of the form 'Fx' for $x \in Term$ ('x'
- is a variable), then $w_{ab}(\emptyset) \in D'$ (replacing clause (1)). (3) if \emptyset has the form $\sim \psi$, then if $w_{ab}(\psi) = (x_1, x_2)$, $w_{ab}(\emptyset) = (x_1, x_2)$ (x_2, x_1) .
- (4) if \emptyset has the form $(\Psi \vee \chi)$, then, if $w_{ab}(\Psi) = (x_1, x_2)$ and $w_{ab}(\chi) = (y_1, y_2)$, then $w_{ab}(\emptyset) = (x_1 \cap y_1, x_2 \cup y_2)$. (5) if \emptyset has the form $(\Psi, \&, \chi)$, then, if $w_{ab}(\Psi) = (x_1, x_2)$
- and wab (X) = (y_1, y_2) , then $w_{ab}(\emptyset) = (x_1 \cup y_1, x_2 \cap y_2)$. if \emptyset has the form $(\forall . \supset . X)$, then, if $w_{ab}(\forall) = (x_1, x_2)$ and $w_{ab}(X) = (y_1, y_2)$, then $w_{ab}(\emptyset) = (x_2 \cap y_1, x_1 \cup y_2)$. if \emptyset has the form $(\forall = X)$, then, $w_{ab}(\emptyset) = w_{ab}(\forall) \cup w_{ab}(X)$. (6)
- (7)

(Truth-functional equivalents, if true, are about the same items). At this point, inductive assignments should continue with determination of the aboutness of quantified swffs. However, these prove to involve

Df. Ab IV;

semantical difficulties with respect to several of which I have to report that I have, as yet, no solution. I intend to discuss these difficulties, along with an approach towards their solution, in the concluding section (E). The nature of the problems, though, can be indicated briefly, here.

CL contains three styles of quantifier in swffs: universal, and particular (both interpreted substitutionally) and a 'special' existential (bi-valently interpreted so that $qu((\exists x) \phi) = '\phi$'s exist' is always true or false statement-yielding). One problem concerns valuations for particular affirmatives and universal negatives. In general, I have argued that it need not be true that the negation of an utterance is about whatever the utterance is about. Clearly qu(Neither water nor mercury is solid (at room temperature)) is about water and mercury-stating of them that they are not solids at room temperature. But what is qu(Some metals are not solid) about? If we construe the statement this yields as asserting of all metals that it is not that everyone is solid, we should take its aboutness to be whatever we assign to the negation of qu(all metals are solid); i.e. the second element of each pair of the range of predications which are its substitution-instances. Equally, though, we could construe qu (Some metals are not solid) as about only some amongst all metals, asserting of at least one that it is not solid. Then, aboutnessassignments should map the first element of a range of pairs, which differ at most in the aboutness of the bound variable in the canonical representation of this sentence, onto that variable. This is unproblematic provided that $qu((Px)(Mx \& \sim Sx))$ is equivalent to $qu(\sim (Ux)(Mx \supset Sx))$ --as it is classically--since negation has the effect of inverting the
aboutness assignment: $N(x_1, x_2) = (x_2, x_1)$ for each instance of the quantified swff: $qu(Mx_1 \& \sim Sx_1)$, $qu(Mx_j \& \sim Sx_j)$; so that the appropriate element appears first. The truth-functional equivalence of the two formulae would ensure sameness of the aboutness of each instance. It remains a problem, though, which of the two plausible interpretations of the assertion is semantically more fundamental; and which is true or false statement-yielding in <u>non</u>-classical contexts, e.g. where, say, $qu((\exists x) Mx)$ yields a false statement.

What proves more problematic is the evaluation of the existential. Qu(men exist) is, in most contexts, about men; and should be evaluated as true in a model in which $s(t_0 \in \underline{\text{Term}})_m \in D'$ and $w_{ab}([{}^{x_1}/t_o])$ is a man) = (x_1, x_2) if $x_1 = t_0$ (by Df. <u>Ab</u> I). But in general, aboutness does not discriminate between existents and non-existents. Since $qu((\exists x)(x \text{ is a flying horse}))(c_m)$ is about some item, if statementyielding in c_m , and $s(Pegasus)_m \in D'$, then $w_{ab}([x_1/Pegasus])$ is a flying horse) = (x_1, x_2) if $x_1 = flying$ horse Pegasus (c_m) , and $v \upharpoonright c_m$ (Pegasus exists) $w_{ab} = 1$. Of course, qu(Pegasus exists)(c_m) may be said to yield a true statement in the context of a myth--but then the status of x2 in (x_1, x_2) is puzzling. For, $qu(\sim Pegasus exists)$ is seemingly still <u>about</u> Pegasus (though not referring to him), whence $x_1 = x_2$ and $v \land c_s$ (~Pegasus exists) $w_{ab} = 1$ iff $w_{ab}([x_2/pegasus])$ is a flying horse) = (x_2, x_1) and x_2 [#]flying horse Pegasus (c_s). It would follow from this that the truthconditions of statements yielded by at least one sentence and its negation would be the same, if we were to combine the contexts c_m, c_s. And such a combination of a mythical context with any standard context has to be

allowed for, if it is to be possible to state, relative to CS-1, that Pegasus, the mythical winged horse, never <u>really</u> existed. Alternatively, if we <u>deny</u> that qu(~Pegasus exists) is about Pegasus in c_m , yet allow that $qu((\exists x)(x \text{ is a flying horse})$ is true or false <u>of something</u> in c_m , then, <u>either</u> $x_2 \neq x_1$ and $x_2 = \emptyset$ Pegasus is false for every \emptyset --contrary to the assumption that $qu((\exists x)\emptyset)$ is always true or false statement-yielding--<u>or</u> $w_{ab}(\emptyset[x_2/pegasus])$ is undefined--contrary to the definition of <u>Ab</u>.

Fictional statements, non-existents, possible objects and noninstantiated predicates are problematic, of course for most accounts of reference; so it is not surprising if the problems recur in an account of aboutness. Nonetheless, the absence of inductive assignments for the aboutness of quantified swffs in CS-1 is seriously deleterious. The problems will require further attention in the concluding Section (E).

To complete the remaining clauses of the definition of aboutness valuations, we need to draw the obvious correlation between the aboutness assigned to a predication and its negation and the extension and antiextension assigned by the function R of m to each atomic predicate qu(Fx)of CS-1. This requires replacing clause (11) of II, Section C (p.449) by:

Df. Ab IV: (11) if \emptyset is of the form Fx_0 and $s(\emptyset)_m = (x_1, x_2)$ for some $(x_1, x_2) \subseteq D'$, then $v(\emptyset)w_{ab} = 1$ iff $x_1 \in \frac{R(F)}{R(F)}$ $v(\emptyset)w_{ab} = 0$ iff $x_2 \in \overline{R(F)}$ (Note that this permits a natural revision to accommodate n-adic predications in CS-2 where aboutness assigns ordered n-tuples to the extension and anti-extensions of many-place relations).

Clause (12) of II, Section C, (p. 449) for identity-swffs of CL goes through unchanged except for relativising $w(t_1, t_2)$ to w_{ab} . This means

that, within mentioned swffs, identities yield true statements only when closed terms on each side of the identity-sign have the same aboutness. Since topics x_1 , x_2 are unit-sets, this is interpreted as <u>extensional</u> <u>identity</u>— $x_1 = x_2$ iff for all z, z & $x_1 \equiv z \in x_2$. For example, qu(this = that) yields a true statement in a context c_0 if the demonstratives qu(this), qu(that) are indexed, in c_0 , to an item <u>a</u> by w_{ab} —something in the manner of Lewis (<u>1972</u>, pp. 174-5)—and $v([x_1/this] = [x_2/that])w_{ab} =$ l iff <u>a</u> = <u>a</u> (c_0). In a context, thus, where the aboutness of more than one demonstrative, pronoun, or token-reflexive expression is used to make an identity assertion, aboutness contracts again to the limiting case of absolute identity. (It does <u>not</u> follow that swff-identities yield <u>neces-</u> <u>sary</u> truths in context, since ostension, or token-reflexive reference, in context, may fail to pick out a unique or existent item).

Finally, clause (13) — which defines the restriction of a valuation to a context—is omitted, for the moment; until the exposition of contextrelative partial models is given in <u>C</u> below.

It is easy to see that, as given, these clauses fix the aboutness of truth-functionally compound swffs in accordance with assignments to their atomic components, as my informal motivating remarks intended. For example, in the condition determining the aboutness of a disjunction, we <u>intersect</u> x_2 and y_2 —the aboutness assigned to the <u>conjoined</u> negates of the disjuncts. For, $qu(\sim(\emptyset \ v \ \psi))$ is $qu(\sim \emptyset \ \sim \sim \ \psi)$, and this latter is definitely about whatever $qu(\sim \emptyset)$ is definitely about (members of x_2) <u>as</u> <u>well as</u> whatever $qu(\sim \ \psi)$ gives definite information about (members of y_2). Having introduced aboutness valuations into the partial models of

CS-1, and the definition of "aboutness" in terms of relative identity, I can conclude this subsection with conditions for the validity of statement-entailments $\alpha \rightarrow \beta$, yielded by mentioned swffs, relative to partial aboutness models $m = \langle D, R, Ab \rangle$. In the following sections, I am not primarily concerned with the deductive structure of St in CS-1-except where appropriate theories of CS-1 (for relative-identity, presupposition and statement-incompatibility) are introduced. Nonetheless, $' \not \rightarrow \beta'$ is a strong entailment relation, of interest in its own right. It is stronger in some ways than the defined relevant compatibility relation ' $\propto \parallel t_0 \beta$ ' of CL. For, it is defined in such a way that, not only is ' $\propto \lim_{\beta} \beta$ ' valid in CL, relative to m (m-valid, for short) when whatever makes \propto true makes β true (T#(\approx) \subseteq T#(β), relative to θ); but ' $\sim \rightarrow \beta$ ' is m-valid whenever, agreeing on what makes them true in θ , β is m-about whatever ∝ is m-about, as well. As J. Dunn has shown (Dunn, 1966, pp. 133-150), this fulfills one of the conditions upon an intuitively adequate relation of relevant entailment; namely, the semantic relevance of antecedent to consequent. The natural definition of mvalidity for $\prec \longrightarrow \beta$ -entailments in CS-1 (yielded by non-quantified atomic predications and truth-functions of these) gives standard consistency and completeness theorems for the valid statements yielded by theses of CS-1, relative to the minimal context c_s' . These results, I conjecture, can be established by appeal to Dunn's Representation Theorems for the embeddability of Meyer's system R___ in an intensional, De-Morgan lattice of aboutness-sets (Dunn, loc. cit., and described in II, Section C, pp. 491-495). I mention these results here, though, only to note that, at present, proving that an axiomatic theory of CS-1 behaves like $R \rightarrow in$ respect of valid statements in <u>St</u>, must await further investigations beyond the scope of this thesis.

Since $\alpha \Vdash_{\beta} \beta$ holds in CL, relative to m, whenever $T\#(\alpha) \subseteq T\#(\beta)$, and a relation ' \leq ' is defined on aboutness sub-models $\langle D', s \rangle$ in m so that $(x_1, x_2) \leq (y_1, y_2)$ whenever $y_1 \subseteq x_1$ and $x_2 \subseteq y_2$, a natural definition of m-validity for entailments $\alpha \longrightarrow \beta$ in <u>St</u>, relative to aboutness-models of CS-1, is as follows:

Df. <u>Ab</u> V: For any $p,q \in \underline{Swff}$, context θ , relative to m, such that $qu(p)(\theta)$ Y is and $qu(q)(\theta)$ Y is, let the entailment is $\xrightarrow{}$ b be <u>true-in-m</u>, relative to aboutnessassignments $\langle D', s \rangle$ of m, iff

(i) Sall'to Sb
(ii) when w_{ab}(p)_m = (x₁,x₂) and w_{ab}(q)_m = (y₁,y₂) then: y₁ ⊆ x₁ and x₂ ⊆ y₂.
Otherwise §a → §b is <u>false-in-m</u>. (Note: a statemententailment can, of course, <u>only</u> be true or false in m.
Swffs which fail to yield statements, <u>a fortiori</u> do not yield statements standing in entailment-relations).

Many statement-entailments, though true in a given aboutness model, need not be true in every aboutness-model—simply because, in accordance with the Principle of Referentiality of I, Section D, if utterances change their aboutness from context to context, then some entailments are falsified in contexts where swffs remain statement-yielding but change their aboutness. A trivial example would be the changes in statement-entailments of predications true of what qu(the number of planets of the Solar System) has been used to mention over the course of many years of astronomical discovery. Of course, throughout those years there has been one and only one number which numbers the planets of the Solar System; but speakers have not been given information <u>about</u> that number in years in which a community of speakers have had false beliefs about the number of planets. Accordingly, we define a statement-entailment as <u>valid</u> in an aboutness-model only when the aboutness of utterances yielding the antecedent and consequent has remained fixed from context to context; then, we define <u>general validity</u> for statement-entailments (confined, of course, to statements yielded by non-quantified swffs of CS-1):

Df. <u>Ab</u> VI: (i) A statement-entailment $\S a \longrightarrow \S b$ is <u>valid</u> <u>in</u> an abcutness domain <u>D'</u> if $\S a \longrightarrow \S b$ is <u>m</u>-true in every aboutness-model $\langle D', s \rangle$, relative to m, otherwise <u>invalid</u> <u>in</u> D'. (ii) $\$ a \longrightarrow \$ b$ is <u>valid</u> iff $\$ a \longrightarrow \$ b$ is valid in every domain D', relative to m; otherwise invalid.

Given the theorems of Dunn (Theorem VII: 5.1, VIII: 1 and IX: 1 and 2, pp. 129-30, loc. cit.), I conjecture that the general $\alpha \longrightarrow \beta$ -valid entailments of <u>St</u>--yielded by non-quantified swffs--are just the valid first-degree relevant implications of Meyer's system R, in the algebraic domain of Dunn's intensional lattices (see: Anderson and Belnap, 1975, pp. 352-353).

(III) B: Aboutness and Significance in CS-1:

Partial models $m = \langle D, R, \underline{Ab} \rangle$ of swffs in CS-1 represent how a statement-yielding utterance, in a context, is associated with the circumstances which make the statement true or false and with circumstances presupposed and relevantly entailed by the utterance's yielding a true or false statement. "Circumstances" were represented algebraically in CL by value-sets in a domain whose algebraic structure provided defined logical relations between statements--relevant entailment, statement-identity, incompatibility. Partial models were supplemented, above, by an analogously lattice-structured aboutness-modelling. At the semantic level, therefore, assignments defined over m, from swffs onto the algebra of aboutness and value-sets display the semantic properties of the primitive 'qu(-)', '- Y ...' and '§ (-)' operators of CS-1 in terms of the semantic relations between 'T#(-)', 'F#(-)', 'Tru(α, θ)', 'Fal(α, θ)' and 'qu(-) is <u>about</u> .. <u>in</u> θ '--schematic instances of which are asserted in CS-1 (as wffs), relative to the background context 'c_s'.

As yet, though, CS-1 lacks application until the correlations between m-valuations $\langle v_m, w_{ab} \rangle$ of swffs, 'aboutness' (relative to the structure of general term predications covering identities), and the value-sets in <u>St x Ab</u> (the product of the domains of statements and aboutness assignments) are drawn together in such a way as provides formal representation of the conditions for statement- and significance-failure.

This subsection takes the next step towards this general objective by examining the structure of Δ_A -predications which determine the application of general terms in identities defining utterance-aboutness, and in restricting the assigned extension and anti-extension of predicates. The final subsection <u>C</u> describes how contextual features (identified with families of sets of 'circumstances') contribute to utterance-significance. In this way, the significance-failure of a simple class of categorymistaken subject-predicate assertions is identified with a species of statement-failure through conflict of aboutness- with statement-conditions.

Df. <u>Ab</u> I, above, requires that, for an atomic, monadic predication-like qu(Britain is a monarchy)--to be about an item <u>b</u> in a context, some

covering concept identifies b, in the context, as the A such that <u>b</u> = Britain, and <u>b</u> satisfies Δ_A -predications associated with being (an) A (e.g. "State", "Country", "Territory"). One should not interpret this requirement so strongly that what obtains ordinarily when speakers successfully talk about or mention items fails to satisfy the specified conditions. It seems clear, for example, that Δ_A -predications are <u>not</u> necessary and sufficient conditions for the <u>truth</u> of b_{Δ} Britain'; nor even that, for each mentionable item, there is one and only one general term covering it. To be talking about, or "referring" to Britain is not to be committed, necessarily, to identifying Britain as a state or country; it is rather to presuppose the application of some sortal A whose associated Δ_{A} -conditions function as criteria for individuating and re-identifying an item, in the context, and in subsequent reports of what was said in different contexts. In other words, $\Delta_{\!\!\!A}$ -conditions are more like criteria for successful identification and individuation than necessary and sufficient conditions for the truth or falsity of statements.

In general, then, I am <u>not</u> claiming that for each mentionable item there is some individuating concept the application of which is necessary and sufficient to fix aboutness from context to context. Rather, fixing the aboutness of an utterance <u>in</u> context depends heavily upon how the topic of predication is introduced into the context of assertion. Reidentifying the topic, in subsequent contexts, is, thus, relative to which Δ_A -conditions it is necessary to transfer from one context to the next--which, one suspects, is not a matter subject to <u>a priori</u> systematisation. In any given discourse, what seems necessary for a predication qu(Fa) to be about <u>b</u>, in that context, is that speaker and audience share some beliefs about <u>b</u> (some of which may be evident from the context), some beliefs about <u>a</u> and sufficient of the <u>a</u>-beliefs and <u>b</u>-beliefs agree in truth-conditions (not all <u>need</u> be true) for <u>a</u> to be the same somethingor-other as <u>b</u>. Often, what those beliefs are is obvious from the context (including the "sentential context", as when the utterance includes a description). Equally often, however, the requisite agreement in truthconditions of shared beliefs is hard to discern; and utterance-aboutness is not obvious, if contextual features are insufficient.

Nevertheless, if qu(Fa) is to be statement-yielding in a context, qu(Fa) must be about some publicly identifiable item of which it is significant to assert that it is F. I shorten this condition to: for $qu(Fa)(\theta)$ to yield a statement, that qu(Fa) is about <u>a presupposes</u>-in- θ <u>a</u>'s falling under some general term which identifies <u>a</u> with some item satisfying the conditions associated with an application of that term. This leaves it open whether it is the preceding discourse (linguistic context), speaker, accompanying gesture, physical environment of his utterance or further circumstantial information which supplies an appropriate general term.

What does "presupposes in context" mean, here? There are two familiar notions of presupposition in the literature. Strawson (<u>1950</u>) makes presupposition a relation between statements S, S' such that S presupposes S' if and only if the truth of S' is a condition upon the truth or falsity of S; i.e. that S is true or false entails that S' is true.

In this sense, that all John's children are asleep presupposes that John has children (Strawson's example). A second notion, more general than the first and subsumed under "pragmatic presupposition" has been characterised as a relation between a speaker and a statement. This notion appears in recent works on theoretical linguistics (inter alia in Lakoff (1973), Keenan (1971) and Sgall (1973)). This latter notion suffers from much unclarity--not the least of which results from the failure to identify the appropriate relata of presupposition. For example, Sgall (1973, p. 300) defines a "presupposition" as "a semantic prerequisite of the meaningfulness of the semantic representation of a sentence". But the "semantic representation" is supposed to be (or at least to "represent") the meaning of a sentence. It is not itself meaningful or meaningless; i.e. the meaninglessness of a sentence is identified with the lack of a semantic representation. Sgall continues by identifying "x is a semantic prerequisite for the meaningfulness of y" with "the fulfillment of x by the given world and the given points of reference is a necessary condition for a given use of y to be meaningful". (ibid). Having defined presupposition, thus, between sentences and "semantic prerequisites", Sgall then explains it in terms of a relation between a speaker's use of a sentence (utterance-token) and the "fulfillment" of 'x' "by the given world". The unhappy example Sgall cites does nothing to clear up this confusion; thus, it is supposed:

(1) The speaker is a woman.

is a semantic prerequisite for the meaningfulness of:

(2) My husband caused a misunderstanding between two lawyers.

I concede that there is something conventionally inappropriate about (2) uttered by a man; even that (2)'s utterance by a man may fail to yield a statement. Simply stated, though, I do not believe (2) is <u>meaningless</u> when uttered by a man--unless Sgall intends to identify meaning-failure with statement-failure. I have argued against this identification already in II, Section B.

I propose, thus, to use Strawson's account of presupposition, modified to fit the formalism of CS-1. Formally, a natural definition appears to be:

Df. Ab VII: for all $\alpha, \beta \in St$; $\emptyset \in Swff$, $\theta \in Cx$, $qu(\emptyset)(\theta)$ Y α presupposes β iff (E α)($qu(\emptyset)(\theta)$ Y α). \rightarrow . Tru(β, θ)--where 'Tru(β, θ)' is defined as in II, C (pp. 457-67).

I shall abbreviate 'qu(\emptyset)(Θ) Y \propto presupposes β ' to 'qu(\emptyset) $_{\Theta}$ $\exists \beta$ '--by analogy with Strawson's explanation of presupposition in terms of the <u>converse</u> of statement-entailment. Clearly, 'Fal(β , Θ)' entails ' \neg (E \propto)(qu(\emptyset)(Θ) Y \propto)' where $\beta \in St$, and 'Tru', 'Fal' are defined over value-sets in Θ making β true, resp. false.

There are two steps necessary to connect "presupposition" to "aboutness". By Df. <u>Ab</u> I, qu(Fa) is about <u>b</u> in θ if (EA). $a = {}_A b (\theta)$. Permissible substitutions for 'A' in 'a $= {}_A b$ ' are general terms of varying kinds-mass terms, sortal nouns, sortal phrases, characterising terms--the general predicative function of which is, as Strawson's "type-criterion" (discussed in (<u>A</u>)) suggests, to group particulars, collect them together under classificatory schemata. For each such general term, I introduced in <u>A</u> an unspecified set of associated predications Δ_A --expressed formally in terms of a constant relation upon monadic predicates $\Delta_{\phi}(\checkmark)$. The characteristics of Δ_A^{-sets} will depend heavily upon syntactic and semantic features of the general term 'A' and upon the functioning of the subject term in an utter-ance whose aboutness is being evaluated.

Roughly described, I follow Griffin (<u>1977</u>, p. 141) in making the purpose of Δ_A -predications associated with a general term that of <u>individuating</u> items as A; identifying and differentiating A's; together with closure-conditions for combinations of Δ_A -predications under composition (by truth-functional and set-theoretic relations). I supplement Griffin's account in identifying Δ_A -sets determined in the modelling of $\Delta_{\emptyset}(\psi)$ predications with sets of value-sets representing (in the domain <u>St x Ab</u>) the presuppositions, entailments and incompatibilities of a given assertion in context. This reflects in the semantics of CS-1 Ryle's doctrine that the logical type of category of subject terms in assertions involves the totality of logical relationships into which the statement (if any) yielded enters into with other statements.

But <u>which</u> statements in this supposed totality of presuppositions, entailments and so on, are those which, in a context, fix the aboutness and significance of the assertion? Astonishingly, this question provokes immediately a tension in the approach to CS-1--one which broaches a seminal issue in logical theory, having profound implications for the views I advance. Though it represents a digression from the matter at hand, I pause to give a summary of how rapidly this question becomes complicated by the issue concerned--the issue is taken up again in Section E, where I extrapolate to considerations of greater generality.

Not every statement standing in some logical relation to a given

utterance in context can be included in the context. This would entail that each context comprise a potentially infinite set of statements--whereupon determining aboutness and significance takes on the appearance of infinitely complex activities. For, ' $\ll \rightarrow \ll \nu \beta$ ' is an axiom of the system of relevant entailment R__ upon which CS-1 is based (Anderson and Belnap, <u>1975</u>, p. 341). Thus, if <u>St</u> x <u>Ab</u> were to contain an infinite set of value sets, <u>any</u> statement would relevantly entail any of an infinite set of disjunctions, all of which would have to appear in the context.

On the other hand, as noted already (above, p. 437), it is not the task of significance logic to set an <u>a priori</u> recipe or list of the kinds of information—in the form of statements—necessary to fix aboutness or significance in context. Thus, I am confronted with the appearance of a dilemma. Significance in context is determined by contextual features represented by a certain set of statements in the domain <u>St</u> x <u>Ab</u>; but CS-1, itself cannot fix <u>a priori</u> limits to this domain without violating philosophical principles upon which it is based.

The solution is to show the appearance of dilemma to be illusory. In so doing, the claim first adumbrated in the Introduction (p. 61) and reappearing in I, D(p. 537) is brought to the fore: to argue that significance is not an enduring feature of abstract linguistic units (sentencetypes), but a contextually-relative feature of the interaction of tokens with surrounding discourse and conditions. This entails denying that <u>a</u> <u>priori</u> limits can be set to "meaning"—to the amount or kind of information (judgements, beliefs, perceptions, statements) needed to determine the significance of one another's assertions.

The risk of denying that significance is an enduring feature subject to <u>a priori</u> demarcation appears to be one which undermines the assumptions which make logic possible: the suppositions that logical truth, validity, inference and necessity are not concepts whose extensions are subject to variation from context to context or moment to moment; and that these notions have a universality beyond temporal and circumstantial features of particular assertions. To obviate this risk, logical theory has itself provided a distinction in respect of its subject

matter which separates logical truth, and so on, from the vagaries of convention and usage. The distinction separates the <u>logical form</u> of sentences from the <u>content</u> they express. It parallels, thus, the difference between what belongs to syntax and what to semantics. It appears first explicitly in Frege's philosophical writings (<u>1879</u>, Preface pp. 5-6; and <u>1894</u>, VI, pp. 316-324), receiving its best defence in the rigid separation of logico-syntactic form from pictorial content in Wittgenstein's <u>Tractatus</u> (5.552, 6.111 and 6.124). I cannot improve upon Wittgenstein's description of the distinction:

The propositions of logic describe the scaffolding of the world, or rather they represent it. They have no subject matter ... We have said that some things are arbitrary in the symbols that we use and that some things are not. In logic it is only the latter that express: but that means that logic is not a field in which we express what we wish with the help of signs, but rather one in which the nature of the natural and inevitable signs speaks for itself. If we know already the logical syntax of any sign-language, then we have already been given all the propositions of logic. (Tractatus, 6.124)

The tension between the demands of logic and the aims of CS-1 are here most apparent. Simply stated: if category-features belong to the <u>content</u> an utterance expresses, then some logical relationships represent aspects of content. Yet the supposition of modern logic is that logical relations pertain only to the <u>form</u> of sentences and not to their particular content. Thus, how can the aim of CS-1 to be a <u>logic</u> of <u>significance</u> be realised when what belongs to logic pertains only to form, and what belongs to <u>significance</u> pertains to <u>content</u>?

The tension is dissipated by the observation that the basis of the form/content distinction is not given by a difference in subject-matter, but in aims and methods of investigation. I will consider this difference further in Section E. Let it suffice for the moment that, for those who, like Wittgenstein, have emphasised a hard line between what belongs to form and what to content, a recurring problem is to provide some principle of classification or criterion of demarcation. It is true that standard textbooks on logic frequently <u>identify</u> what belongs to form with a <u>list</u>

of those connectives and operators appearing in sentences which have been, historically, most often investigated by formal means: the truth-functions, quantifiers, and, arguably, identity and set-theoretic membership. What is lacking is a clear account of why just <u>these</u> notions are 'formal', and the remainder relegated to 'content'.

In any case-as I shall argue in Section E--to draw the form/content distinction in terms of subject-matter does violence to Wittgenstein's insight that what makes logic 'formal' is what is common to any possible system of notation which can represent truth and falsity in terms of agreement and disagreement with states of affairs. A list of the 'formal' concepts of a language may reflect one system of notation; but tells us little as to what is common to all. It is more fruitful to regard the formal nature of logic as reflected in its rigorous exposure of structures within which practices of inference and deduction are carried out. There is then no conflict between CS-1 being a logical framework for inferences made on the basis of significance-features, and the fact that significance pertains to content. For, the judgements and significance-claims we make as to the particular content an utterance expresses in context require support which can only be derived from experiences in interpreting sentences in a wide range of similar and divergent contexts -- and what assembles these experiences relevant to the context at hand are structural features contexts share, irrespective of their individual differences.

The particular response to the question which provoked the digression, above, has to be more carefully phrased. If there is no <u>a priori</u> limit to be set to what we might need to know in order to circumscribe an utterance's aboutness and significance, then it is quite possible that the significance of some utterances eludes us, when a context is insufficiently specific, or when information is lacking. Certain kinds of pragmatic ambiguity can perhaps best be explained in terms of the unavailability of disambiguating information. A prosaic illustration would be any context in which the subject term is referentially ambiguous owing to the lack

of identity-conditions for the aboutness-item: e.g. qu(He is in the grip of a vice) (Grice's example)—where neither context nor surrounding discourse supplies any information as to whether the individual concerned is undergoing a crushing experience or merely has bad habits. My concern is not polysemy, though; but the particular features of context--expressed in CS-1 by sets of value-sets corresponding to $\Delta_{\phi}(\psi)$ -predications--which contribute to aboutness (and, in the next subsection, to significance).

The first task, thus, is to examine the general properties of those terms whose application is, in context, presupposed in identifying utterance-aboutness, and in determining the range of application of a predicate; then to seek to discover how those general properties might be represented in the domain St x Ab. First, I shall undertake an informal characterisation of properties of general term-predications--borrowing much of my description from Griffin (1977, Chs. 2-5)--and then utilising what I have already demonstrated of the (algebraic) structural properties of St x Ab, to characterise the properties in formal terms. It should be noted, however, that a full semantic treatment of the taxonomy of mass-terms, sortal nouns, sortalising phrases and characterising predicates which I invoke, is simply not available in the current literature--whilst a full explication of all the relevant distinctions and relationships would demand a thesis in its own right. Many of the semantic distinctions between kinds of general term covering relative-identity statements--in terms of which aboutness in context has been defined--are imprecisely drawn, overlap in part with other distinctions, or are of a highly controversial nature. Nevertheless, from Griffin's discussion,

I extract the following schematic taxonomy, accompanied by the general explanatory notes:



The diagram illustrates only some of the major distinctions drawn within the class of predicative general terms. It fails, in particular, to differentiate amongst distinctions drawn on the basis of syntactic criteria, those drawn on the basis of semantic criteria, and those for which general criteria are lacking. Something of the arbitrariness of the above taxonomy, then, is evident in the following notes--numbered according to the nodes on the diagram:

 (1) The distinction "singular/general" has been drawn above
 (p. 549) in respect of the syntactic role of expressions occurring within 'referring' (identificatory) or 'predicative' (classificatory) position in a predication.

(2) A general term 'A' is <u>+Count</u> if A is a noun-phrase for which qu(There are n A's such that \emptyset) is grammatical, where "n" takes numerical substituends. Otherwise, A is <u>-Count</u>.

(3) -Count terms divide into three kinds for which there are at best intuitive justifications: <u>Matter kinds</u> (e.g. 'gold' 'steel', 'powder', 'ice'; borderline cases are 'housing', 'footwear', 'rubbish'); <u>Abstract kinds</u> (e.g. 'music', 'information', 'greenness', 'entertainment'; + borderline cases: 'logic', 'art', 'behaviour', 'energy'--sometimes +Count). From Strawson (<u>1959</u>, p. 168), there are also <u>characterising</u> <u>terms</u> (or "ascriptives"), which are noun-phrase general terms derived (in the main) by nominalising adjectival or adverbial phrases (e.g. 'intelligence', 'virtue'--sometimes +Count--, 'congruence', 'efficiency', 'insolence', 'effort').

All three groups (but mainly the first two) may be classed as <u>mass</u> <u>terms</u>. There are many problems concerning whether the distinctions apply properly to how the terms are used, their grammatical function, or their "meaning". From Griffin (<u>1977</u>, p. 30) a plausible criterion for <u>mass</u> <u>terms</u> may be:

"A is a mass term iff A is -Count and the fusion of any two parts which are A is A" (where "fusion" is defined in the sense of Goodman's Calculus of Individuals, Goddman, 1966, pp. 50-1).

(4) The generic notion <u>sortal</u> (from Locke, <u>Essay</u>, III, 3) carries the most frequent connotation of "individual kind of item", though exact criteria admitting all and only genuine sortals are hard to find. I class <u>sortal terms</u> roughly as "individuating terms"—along with proper names, demonstratives and definite descriptions. Necessary and sufficient conditions for a term to be sortal have been investigated in detail. A rough-and-ready basis for an informal condition upon sortals can be condensed from Griffin (<u>1977</u>, Chs. 3 & 5):

A term <u>A</u> is <u>sortal</u> if (i) there are circumstances in which use of <u>A</u> provides principles of individuation adequate for counting <u>A</u>'s; (ii) provision of individuating principles for <u>A</u>'s is not conditional upon further conventions, circumstances or beliefs, except such as are conveyed by the use of <u>A</u>; and (iii) individuating an <u>A</u> does not require principles of individuation borrowed from other general terms. To explain these conditions, notice that a sortal is a particular kind of general term--a wholly linguistic item or expression--which can appear in sentences within a subject term or as part of a predicate. Any given literal use of a sortal--say Horse (as in e.g. qu(Dobbin is a horse) or qu(A horse neighed)--individuates an item through principles conveyed by the term which enable us to fix upon how much of an otherwise undifferentiated stuff constitutes one horse, what marks off one horse from another, and what counts for one horse to be the same horse as a previously identified nag. Thus, condition (i) requires a sortal to be a term whose use, on any given occasion, conveys the means of individuating amongs the items falling under the term sufficiently for us to be able, in principle, to count the items thus individuated. In short, to predicate horse significantly of items requires accompanying beliefs or judgements which justify claims as to what features belong to one horse as distinct from other horses and other items. This is not true, say, of a mass term like Water, whose use does not convey the means of marking off what is one 'water' from another (unless that use "borrows" the sortal principles of, say, Expanse of water).

Conditions (ii) and (iii) rule out "dummy" sortals whose use borrows sortal features from an antecedent root or from a sortalising phrase, when the term itself does not ordinarily convey those features. For example, qu(Imaginary thing) is <u>non-sortal</u> (does not convey principles determining how much is <u>one</u> imaginary thing) unless it is conventionally restricted to items individuated already under its root "<u>Image</u>". <u>Dummy</u> <u>sortals</u> (e.g. 'kind of <u>A</u>', 'species of <u>A</u>', 'sort of <u>A</u>') borrow principles of individuation from "sortalising phrases" of the form "S of <u>A</u>", where S is a <u>nominal sortal</u>, in the sense that their use <u>alone</u> of items (as in "being a sort", "being a thing", "being a kind") conveys no specific principles of counting or identity (c.f. "How many sorts (kinds, things) are there in this space?" is not answerable without antecedent sorting into non-dummy sortals (desk, chair, pencil, ruler, typewriter, and so on).

The question which arises is: how is this informal taxonomy

of general terms to be incorporated into CS-1 to function in determining aboutness of subject terms and 'super-predicates' of predications which fix the significant range of application of a predicate, in a context. For, the manner in which we sort and classify the items about which we speak by applying general terms indicates how, when what is presupposed in determining the aboutness of an utterance conflicts with what is required for the predicate to apply, no publicly identifiable statement is yielded by category-mistaken and referentially failing assertions. Thus, the place to look for the application of general terms, and the principles of individuation and identity they convey, is in the clusters of statements which are compatible or incompatible with a given utterance's aboutness and significance, and which together constitute the context of assertion. Applications of general terms--whether sortal, mass or characterising--will, thus, appear in the semantics of CS-1 as valuesets assigned in St x Ab to those statements presupposed by an utterance in context and associated with a general term predication by the constant relation $\Delta_{g}(\gamma')$.

In other words, general term predications form a sub-class of admissible monadic swffs in the range of the qu(-) operator. Thus in the semantic models of swffs, relations between general term predications and other statement-yielding predications will be represented by settheoretic relationships amongst the R_m - assigned extensions and antiextensions of predicate-variables--the field of the primitive $\Delta_{\phi}(\gamma)$ relation.

The domain of value sets St x Ab has already been determined in II,

C and II, D (A), above, as a special kind of lattice-algebra. It is appropriate, therefore, to seek to represent structural and semantic relations between, and properties of, general term predications in terms of a sub-algebra of the lattice-structure St x Ab, as described. For, it is no part of CS-1 to describe which particular general term predications fix an utterance's aboutness and significance, in any range of contexts -- that will always depend upon the practices of sorting and classifying that are relevant to the particular topics of our speech acts. That is, it is not a question of logic, and, thus, not a question for CS-1, whether there is some essential, or underlying register of 'sorts' or 'kinds of thing', which has been abstracted from, or imposed upon our conceptions of items. All that can be asked of the logic of general terms is whether, given the practices and conventions of sorting and classifying necessary to identify and individuate items, there are structural relations amongst the taxonomies of general terms, thus yielded, which support logical inferences to and from the aboutness and significance of our assertions. In the most abstract sense, this structure should stand in some close relation to other practices and conventions of predicating qualities, quantities and aspects of items which we feature in making statements about items. I propose, therefore, that certain of the most obvious properties and relations holding of the taxonomies of sorts and kinds we employ can be represented within St x Ab formally, in terms of the algebraic semantics already given.

Sub-Theory G: an algebraic semantics for general terms.

Within the class of predicate-expressions of CS-1, I employ 'A',

'B', 'C',... as schematic for general term predicates. They are, more perspicuously, monadic predicate variables: qu(Ax), qu(Bx),... appearing in mentioned swffs. Equivocally, some unspecified set of term variables 'A', 'B', 'C',... may appear in theses of CS-1 as subscripts on variables "x_A", as flanking identity assertions "x =_A y", and as bound variables "(EA) ϕ ", etc. In these latter cases, the formulae are to be understood as schematic for a range of wffs of CS-1 or well-formed clauses which are autonomously statement-yielding and significant.

An intuitively obvious semantic relation which holds between the extensions of general terms is that of <u>species</u> to <u>genus</u> (when "being (a) B" is <u>subordinate</u> to "being (an) A"; as in the relation of 'Man' to 'Animal', 'Water' to 'Liquid', 'Redness' to 'Colour'). A natural characterisation of <u>subordination</u> should draw upon those relations which have already been defined in general for the algebra of <u>St</u> x <u>Ab</u>--thus, expressing for any general term predication the relation which holds between its assigned value-sets when "being B" entails "being A" but not every A is B (whence B is subordinate to A).

For this purpose, it is instructive to compare the manner in which a genus <u>subdivides</u> into species, in a taxonomy of sorts or kinds, with an algebraic operation which is akin to numerical <u>division</u>. Such an cperation is available from studies of lattice-algebras by Certaine (<u>1943</u>) and Birkhoff (<u>1948</u>), and is called by them <u>residuation</u>. It is characterised informally, then defined explicitly for the sub-algebra G, below--together with a further defined relation between value-sets: <u>co-tenability</u>.

The strategy behind the following algebraic modelling of general

terms in CS-1 to yield a sub-lattice of St x Ab can be summarised as

follows:

1) I construe the m-assigned value sets of atomic, general term predications as <u>elements</u> of <u>St x Ab</u>, whose structure has been characterised already as a product of intensional, distributive lattices St, Ab.

2) As described in II, C and in Dunn (1966), a truth-filter on a classical, <u>two-valued</u> sentential logic $C = \langle P, \supset, \sim, T \rangle$ determines a homomorphism of the algebra of C into the simple, two-element Boolean algeabra whose Hasse diagram is:

For a homomorphism h, determined by T, such that $h(a_p) = 1$ if $a_p \in T$ and $h(a_p) = 0$ if $a \notin T$, the <u>valid</u> formulae of C coincide with some direct product of b_2 (i.e. at most denumerably many replicas of the Boolean, two-element algebra).

3) Analogously, I define a <u>product</u> of intensionally complemented, distributive (De Morgan) lattices (hereafter: 'icdl') <u>St</u> = $\langle S_x, H_x, -x, T_x \rangle$ and <u>Ab</u> = $\langle A_x, \epsilon_x, N_x, T_x \rangle$ as an icdlwith truth-filter such that if elements and operations are indexed to elements X = D_m^n , then:

 $\prod_{x} \{ \underline{St}_{x}, \underline{Ab} \} \quad (\text{the direct product } \underline{St} \times \underline{Ab})$

is an icdl $\langle S_x, A_x; \rightarrow x; \neg_x; T'_x \rangle$ such that $T'_x = T_{x \in X} \times T_{x \in X}; \neg_x T = \overline{T}_{x \in X};$ $S_x, A_x = T_{x \in X} \cup \overline{T}_{x \in X}; \{a_x\} \rightarrow \{b_x\}_{\in X} = a_x \parallel T_x b_x \text{ and } a_x \leq x b_x, \text{ for all}$ $x \in X; \text{ and } \neg_x \{a_x\} = \{\neg_x a_x\}_{x \in X}.$

Belnap (1965) has shown that every icdl with truth-filter is T-isomorphic to a sub-lattice of some product of an at most denumerable collection of replicas of M_0 -- where M_0 is the icdl with diagram:



Belnap's result establishes that the lattice-structure of G is well defined as a sub-lattice of $St \times Ab$, just as the homomorphism h determined the

soundness of C as a sub-algebra of b₂, above.

4) Relative to the manner in which closure-conditions upon the sets of Δ_A -value sets, associated by $\Delta_{\emptyset}(\gamma)$ with each general term A, are given, the sub-lattice of G of St x Ab will incorporate defined relations between value sets, corresponding to relations between general terms. Roughly stated, value-sets in St identify the presuppositions and entailments which "cluster" in the context around what an utterance states in a set of circumstances. Conjoined to St are the value-sets identifying the aboutness of the utterance, relative to the same set of circumstances. Relations between these sets of value-sets thus express semantically the conditions for the compatibility of what an utterance is about, in context, with what it states (if anything); i.e. the conditions for the significance of the utterance and its statement-success.

Two key notions I use to define relations between general terms in G are those of "co-tenability" and "residuation". They are intended to represent, respectively: (i) compatibility of statement- with aboutness-conditions, with respect to presupposed general terms and associated Δ_A -predications; and (ii) sub-division of higher general terms into subspecies, relative to taxonomic heorarchies. It is worthwhile to note, therefore, that (i) "co-tenability" is a multiplicative, binary operation corresponding to non-Boolean compatibility in the extension of CL's semantics to CS-1; and (ii) "residuation" in a lattice is an abstraction from the arithmetical operation of division (into factors).

With these remarks in mind I set the postulates for G as an intensionally complemented, distributive (De Morgan) lattice of value-sets $G = \langle A^* (\subseteq St \times Ab), \longrightarrow, \frown, T \rangle$, for which:

P1) A* is a set of value-sets A,B,C, \in <u>St</u> x <u>Ab</u> s.th. ' \rightarrow ' partially orders A*:

a) $A \rightarrow A$ (Reflexivity) b) if $A \rightarrow B$ and $B \rightarrow A$, then $A = {}_{S}B$ (Antisymmetry). c) if $A \rightarrow B$ and $B \rightarrow C$, then $A \rightarrow C$. (Transitivity). (Recall that ' \rightarrow ' is <u>defined</u> in CS-1 in terms of ' \\\\F' of <u>St</u> and ' \leq ' of <u>Ab</u>). P2) G has unique meet (A ∧ B) and join (A v B)--i.e. T(Ax) ∩ T(Bx), T(Ax) ∪ T(Bx)--which have distributive and De Morgan properties:
a) A ∧ (B v C) = (A ∧ B) v (A ∧ C).
b) A v(B ∧ C) = (A v B) ∧ (A v C).
c) A ∧ B = A v B
d) A ∨ B = A ∧ B
e) A = A
f) if A → B then B → A
g) A → (B → C) iff B → (A → C). ('→' permutes).
P3) T is a truth filter on G (T ⊆ G) such that, for all A, B ∈ G:
a) A ∧ B ∈ T iff A ∈ T and B ∈ T.
b) A v B ∈ T iff A ∉ T.

Co-tenability of general term predications (usually in a context, though I omit contextual considerations for the moment) can be <u>defined</u> in G in terms of strong statement-entailment. Intuitively, say, $\frac{9}{3}a$ is a man is co-tenable with $\frac{9}{3}a$ is a biped (or 'Man' is co-tenable with 'Bi-ped'--M o B) holds in a set of circumstances when every value set in the complement of value sets which coincide in making $\frac{9}{9}$ Mx true when they make $\frac{9}{9}$ Bx false, is forced by that set of circumstances. This is simply:

<u>Df. G1</u>: A o B = $\overline{A \rightarrow B}$. (Thus: $\overline{A \circ B} = A \rightarrow B$, and $\overline{\overline{A} \circ B} = B \rightarrow A$).

I proceed directly to the definition of residuation in G to complete its formal characterisation. These definitions are clarified, below.

Ordinarily, residuation in a lattice is the algebraic analogue of division. For example, (from Certaine, <u>1943</u>), in the multiplicative group R+ of positive rational numbers ordered under "less than or equal to" ($n \leq m$), such that $a \leq_n b$ iff a integrally divides b (i.e. b/a is a positive integer in R+), then residuation is defined as follows: R+ is

<u>right-residuated</u> if, for any a, b \in R+, there is an element a: b \in R+ (the right-residual of a by b) such $x \leq_n a:b$ iff $xa \leq_n b$ --where juxtaposition is a multiplicative operation on R+. R+ is <u>left-residuated</u>, similarly, if, for a, b \in R+, there is an element a:: b \in R+ such that $x \leq_n a::b$ iff $bx \leq_n a$ (a:: b is the left residual of a by b under \leq_n). In R+, x integrally divides a: b iff $xa \leq_n b$ --so <u>right</u> residuation is (b/a) = ba^{-1} ; and x integrally divides a:: b iff $bx \leq_n a$ --whence <u>left</u> residuation is (a/b) = $b^{-1}a$ (c.f. Birkhoff, <u>1948</u>, p. 202). Finally, if R' is a <u>commutative</u> lattice-structured group (a 0 b = b 0 a, for all a, b in R', where '0' is the multiplicative operation), then it is right residuated if and only if left-residuated; i.e. simply <u>residuated</u> (in which case, a:b exists whenever a::b exists).

Since it is proposed to define relations between value sets for general term predications in G in terms of co-tenability and residuation, it is instructive to note an example due to Certaine (<u>1943</u>, p. 61). A Boolean algebra for sentential logic is a lattice-structured group with join (p v q) taken as a multiplicative operation such that $p:q = \overline{q} v p$. Since, in classical terms, $q:p = \sim p v q = p \supset q$ (material conditional), residuation corresponds to detachment of a material consequent <u>if</u> the material antecedent is satisfied. This suggests defining residuation in terms of the strong statement-entailment relation already introduced for St x Ab.

Thus, where 'v' is a join operation, $A:B = \overline{B} \vee A$ represents the residual of A by B. For, suppose 'Ax' is 'x is an animal' and 'Bx' is 'x is a man', then A:B is the division of the general extension of

'Animal' into 'Man' which is a relation between value sets holding whenever ${}^{6}x$ is a man <u>entails</u> ${}^{6}x$ is an animal (B \rightarrow A), but not <u>vice versa</u> (i.e. subordination).

On the other hand, the material conditional $qu(p \supset q)$ is not genuinely analogous to the relevant entailment relation 'a \rightarrow b'. Both differ in respect of <u>syntax</u> (' \supset ' is a sentential connective, ' \rightarrow ' is a statement-relation); and in respect of strength ($qu(p \supset q)$ yields a truth if qu(p) yields a falsity or qu(q) a truth; and thus, unlike ' \rightarrow ', is susceptible to the paradoxes of the material conditional). Thus, it is necessary to <u>show</u> that co-tenability 'A o B' in the algebra G satisfies the requisite multiplicative properties, in order to show that G does indeed remain a lattice-structured group under ' \rightarrow '. These properties consist in showing the commutativity, associativity, and distributativity (over 'v' and ' \wedge ') of 'o'.

| | <u>G</u> : | <u>Theorem 1</u>) $A \circ B = B \circ A$ | (Commutativity) |
|--------|------------|--|--|
| Proof: | | 1) A o B is $\overline{A \longrightarrow B}$ | (Df. G.I). |
| | | 2) $\overline{A \longrightarrow B} = \overline{B \longrightarrow A}$ | (P2, f). |
| | | 3) $\overline{B \to \overline{A}}$ is B o A | (Df. G.I). |
| | <u>G</u> : | $\underline{\text{Th}} \cdot \underline{2} \text{A o (B o C)} = (\text{A o B) o C}$ | (Associativity) |
| Proof: | | 1) A o (B o C) \rightarrow . (A o B) o C = A $\rightarrow \overline{B} \rightarrow \overline{C}$ | \rightarrow . $\overrightarrow{A} \rightarrow \overrightarrow{B} \rightarrow \overrightarrow{C}$ |
| | | | (Df. G.I) |
| | | 2) $= \overline{A \longrightarrow B} \longrightarrow \overline{C} \longrightarrow A \longrightarrow (B \longrightarrow \overline{C})$ | (P2, f). |
| | | 3) = $C \longrightarrow (A \longrightarrow \overline{B}) \longrightarrow A \longrightarrow (B \longrightarrow \overline{C})$ | (P2, f). |
| | | 4) = C \longrightarrow (A \longrightarrow \overline{B}) \longrightarrow A \longrightarrow (C \longrightarrow \overline{B}) | (P2, e, f). |
| | | 5) = $A \longrightarrow (C \longrightarrow \overline{B}) \longrightarrow A \longrightarrow (C \longrightarrow \overline{B})$ | (P2, f). |

This last is an instance of $A \rightarrow A$, which holds in G. This suffices for associativity, since commutativity gives (B o C) o $A \rightarrow C$ o (A o B), and the proof can be repeated.

| <u>G:</u> <u>Th.</u> <u>3</u>) A o (B v C) = (A o B) v (A o C) | (Distribution 'c' over 'v'). |
|--|---|
| <u>Proof</u> : 1) A o (B v C) \leftrightarrow (A o B) v (A o C) = | |
| 2) $\overline{A \longrightarrow (\overline{B \ v \ C})} \iff (\overline{A \longrightarrow B}) \ v \ (\overline{A \longrightarrow C}) =$ | (Df. G.I) |
| 3) $\overline{A \longrightarrow (\overline{B} \land \overline{C})} \longleftrightarrow (\overline{A \longrightarrow B}) \lor (\overline{A \longrightarrow C}) =$ | (P2, d) |
| 4) $(A \longrightarrow \overline{B}) \vee (A \longrightarrow \overline{C}) \longleftrightarrow A \longrightarrow \overline{B} \wedge \overline{C} =$ | (P2, f) |
| 5) $(B \longrightarrow \overline{A}) \vee (C \longrightarrow \overline{A}) \longleftrightarrow (\overline{B \land C}) \longrightarrow \overline{A} =$ | (P2, f) |
| 6) $(B \longrightarrow \overline{A}) \vee (C \longrightarrow \overline{A}) \longleftrightarrow (B \vee C) \longrightarrow \overline{A} =$ | (P2, d) |
| 7) (B v C) $\longrightarrow \overline{A} \longleftrightarrow$ (B v C) $\longrightarrow \overline{A}$. | (Df. 'v'). |
| <u>G</u> : <u>Th</u> . <u>4</u>) A o (B \wedge C) = (A o B) \wedge (A o C) | (Distribution ' \circ' over ' \wedge '). |
| <u>Proof</u> : 1) A o (B \land C) \leftrightarrow (A o B) \land (A o C) = | |
| 2) $\overline{A \longrightarrow B \land C} \longleftrightarrow (\overline{A \longrightarrow B}) \land (\overline{A \longrightarrow C}) =$ | (Df. G.I) |
| 3) $\overline{A \longrightarrow \overline{B} \land \overline{C}} \longleftrightarrow \overline{A \longrightarrow \overline{B}} .v. A \longrightarrow \overline{C} =$ | (P2, e) |
| 4) $(A \longrightarrow \overline{B}) \vee (A \longrightarrow \overline{C}) \longleftrightarrow A \longrightarrow \overline{B} \wedge \overline{C} =$ | (P2, f) |
| 5) $(A \longrightarrow \overline{B}) \vee (A \longrightarrow \overline{C}) \longleftrightarrow A \longrightarrow \overline{B} \vee \overline{C}.$ | (P2, d) |
| and repeat 4) to 6) of G: Th. 3). | |

Next, it is necessary to prove a meta-theorem to the effect that G has residuals, if G is a lattice-structured group ordered under ' \rightarrow ', with 'o' as a binary, multiplicative operation. To prove this metatheorem, we need the following Lemma, for the interplay between 'o', ' \rightarrow ' and ' $\overline{}$ ':

Lemma (A): For all A, B, C,
$$\varepsilon$$
 G: (i) (A o B) \rightarrow C iff
(B o C) \rightarrow A iff
(ii) (C o A) \rightarrow B.

Proof: (in four stages):

(a) 1)
$$A \circ B \to C , \to B \circ C \to A =$$

2) $\overline{A \to B} \to C , \to \overline{B \to C} \to \overline{A} =$ (Df, G.I)
3) $\overline{C} \to (A \to \overline{B}) , \to A \to (B \to C) =$ (P2, f)
4) $\overline{C} \to (A \to \overline{B}) , \to A \to (\overline{C} \to \overline{B}) =$ (P2, f)

5)
$$C \longrightarrow (A \longrightarrow B) \longrightarrow C \longrightarrow (A \longrightarrow B).$$
 (P2, g)

(b) 1)
$$B \circ \overline{C} \rightarrow \overline{A} \rightarrow A \circ B \rightarrow C =$$

2) $(\overline{C} \circ B) \rightarrow \overline{A} \rightarrow (B \circ A) \rightarrow C =$ (G: Th.1)
3) $\overline{C} \rightarrow \overline{B} \rightarrow \overline{A} \rightarrow (\overline{C} \rightarrow \overline{A}) \rightarrow C =$ (P2, f)
4) $A \rightarrow (\overline{C} \rightarrow \overline{B}) \rightarrow \overline{C} \rightarrow (B \rightarrow \overline{A}) =$ (P2, f)
5) $\overline{C} \rightarrow (A \rightarrow \overline{B}) \rightarrow \overline{C} \rightarrow (B \rightarrow \overline{A}) =$ (P2, g)
6) $\overline{C} \rightarrow (A \rightarrow \overline{B}) \rightarrow \overline{C} \rightarrow (A \rightarrow \overline{B})$. (P2, f)
(c) 1) $(B \circ \overline{C}) \rightarrow \overline{A} \rightarrow (\overline{C} \circ A) \rightarrow \overline{B} =$
2) $(\overline{B} \rightarrow \overline{C}) \rightarrow \overline{A} \rightarrow (A \circ \overline{C}) \rightarrow \overline{E} =$ (Df, G.I; Th.1)
3) $A \rightarrow (B \rightarrow C) \rightarrow (\overline{A} \rightarrow \overline{C}) \rightarrow \overline{B} =$ (P2, f; Df. G.I)
4) $A \rightarrow (B \rightarrow C) \rightarrow A \rightarrow (B \rightarrow C) =$ (P2, f)
5) $A \rightarrow (B \rightarrow C) \rightarrow A \rightarrow (B \rightarrow C)$. (P2, g)
(d) 1) $(\overline{C} \circ A) \rightarrow \overline{B} \rightarrow (A \rightarrow C) \rightarrow \overline{A} =$ (Df, G.I; Th.1)
3) $(\overline{A \rightarrow C}) \rightarrow \overline{B} \rightarrow (\overline{B} \rightarrow \overline{C}) \rightarrow \overline{A} =$ (Df, G.I; Th.1)
4) $B \rightarrow (A \rightarrow C) \rightarrow \overline{B} \rightarrow (\overline{B} \rightarrow C) =$ (P2, f)

5)
$$A \rightarrow (B \rightarrow C) \rightarrow A \rightarrow (B \rightarrow C)$$
. (P2, g)

(a) - (d) give the Lemma by definition of ' \leftrightarrow ' and transitivity. From the Lemma, we obtain the meta-theorem for <u>residuation</u> in G:

 $\underline{MG} (\underline{A}): \text{ If } G \text{ is an icdl with 'o' as multiplicative operation,} \\ \text{ then } G \text{ is residuated and} \\ 1) \quad A:B = \overline{B \circ A} \\ 2) \quad A::B = \overline{\overline{A} \circ B} \\ \underline{Proof:} \quad By \text{ Lemma } (A): \quad (i) \text{ for any } A, B, x \text{ in } G: \\ x \circ B \longrightarrow A \text{ iff } B \circ \overline{A} \longrightarrow \overline{x}; \text{ and by } P2, \text{ f:} \\ \end{array}$

iff $x \rightarrow \overline{B \circ A}$.

1) Then, where ' \rightarrow ' is entailment and 'o' is co-tenability: $x \rightarrow A:B \text{ iff } x \rightarrow \overline{B \text{ o } A}$ -- which gives: $A:B = \overline{B \text{ o } A}$ (Definition)

2) Lemma (A) (ii) gives: x o B \rightarrow A iff \overline{A} o B $\rightarrow \overline{x}$; and by P2, f: iff $x \rightarrow \overline{\overline{A}}$ o B; whence, under the same assumptions:

$$x \longrightarrow A::B$$
 iff $x \longrightarrow \overline{A} \circ B$ and
A::B = $\overline{\overline{A} \circ B}$ (Definition)

MG (A) guarantees that G' = $\langle G, o, -, - \rangle$ is, indeed, a latticestructured group with residuals. Taking 'A v A' as an arbitrary join of elements A, A in G', then an identity element t E G' satisfies:

<u>Df. G3</u>) G' has an identity element t (= A \sqrt{A}) such that B o t = t o B = B, for all B \in G'.

Relative to a truth filter T on G', such that $T \subseteq G'$ and there is no A \in G' such that A \in T and $\overline{A} \in T$, and for every B \in G', either B \in T or $\overline{B} \in T$; Df. G3) ensures that the algebraic version of '-->' in G' (entailment between value-sets of statements) will be truth-preserving. That is, G' has T-preserving isomorphisms with products of M₀; hence with <u>St x Ab</u> (i.e. for all B \in G', t-->B iff B \in T and B \rightarrow t iff B \notin T). This gives us the Belnap result described above (p. 538), establishing the soundness of G' as a sub-lattice of <u>St x Ab</u>.

Taxonomic Relations in G:

The point of providing this abstract representation of m-assigned value sets corresponding to sortal, mass and characterising predications is to connect the presupposed general predications--which fix the aboutness and significance of an utterance in context--with the associated ordinary predications (value sets in <u>St x Ab</u>) determined by $\Delta_{\phi}(\cdot \varphi)$. To take an example: if an utterance like qu(John Smith is intelligent) (Θ) is to be about some <u>person</u> in the context Θ , then qu(John Smith)(Θ) is about <u>a</u> if <u>a</u> = person John Smith, and by the postulates for relative identity in subsection III(A), both <u>a</u> and John Smith are satisfiers (in D^n) of Δ_{person} -predications.

One supposes that any number of predications may be associated contingently with application of the general term 'Person' to an item. It is a point of some philosophical substance, of course, to determine which (if any) of these associated predications are definitive of items satisfying 'Person'. That- $\underline{a} =_{person}$ John Smith is true or false has been said to depend upon, at least, \underline{a} and John Smith being spatiotemporally contiguous items agreeing in memories (see e.g. Wiggins, <u>1967</u>). This is hardly an issue to be resolved by logical means, alone. Notice, however, that what <u>has</u> to obtain for the truth or falsity of $\underline{9}(\underline{a} =_{person}$ John Smith)(Θ) need not be a circumstance to which we appeal, in context Θ , in determining qu(John Smith is intelligent) to be about a person in Θ and state something significant of that person. All that may be needed to appraise the <u>significance</u> of that utterance in Θ is that circumstances (judgements, beliefs, statements, states of affairs) certify item <u>a</u> to be of a <u>sort</u> of which qu(x is intelligent) can be truly or falsely predicated.

Thus, whether the example being considered is significant and statement-yielding in θ does <u>not</u> depend upon a's being an item which necessarily <u>is</u> intelligent; but upon a's being of a sort of item to which intelligence or unintelligence <u>can</u> be ascribed. That is, the utterance's ignificance in θ requires the compatibility of predications associated ith the sort under which we identify its aboutness, and predications sociated with application of the predicate, (the "super-predicates" of x is intelligent)). Of any given group of items, there may be some

which we can say are definitely intelligent, through their satisfying sufficient of a weighted set of predications criterial for intelligence. Other items in the group may be those of which it can be definitely decided that they are unintelligent--through their satisfying the negations of criterial predications. And there may be a third subset from which we withhold judgement as to either their intelligence or unintelligence-either because it is not determinate whether they fall under a general sort compatible with being intelligent or unintelligent, or because they are identifiable only as falling under a general term (like 'Water', 'Consistency') whose associated $\Delta_{\phi}(\gamma)$ -predications are definitely incompatible ($\S Aa_0 / \S Intelligent(a_0)$) with predicating intelligence of them. (In this discussion, I make an unwarranted simplification: it is not true of every predication that it partitions the domain into exclusive subsets -- items satisfying the predicate, items satisfying its negation, and items outside both--since not every adjectival ascription is, in this sense, "polar". The complications arising from allowing nonpolar grading predicates, family-related predicates, nesting predicates, and type or categorial predicates are discussed briefly in Section E-most of the classification and discussion there attempted being taken from Goddard, 1966, pp. 155-162).

On the account I am offering, therefore, it is essential that the semantics for CS-1 provide for those taxonomic relations between general terms upon which determination of aboutness and significance--compatibili and incompatibility of associated sets of $\Delta_{\emptyset}(\psi)$ -value sets in context-depends. Accordingly, I seek to use the defined operations on value se

in G to represent these relations:

Df. G.4) B is subordinate to A/ A covers B iff (i) $Bw_{ab} \stackrel{<}{\leftarrow} Aw_{ab}$ for each $w_{ab}(Ax, Bx)$ on m rela-(ii) $Aw_{ab} \stackrel{<}{\leftarrow} Bw_{ab}$ tive to $\theta \stackrel{<}{\leftarrow} St$; and (iii) if A:B $\in \Theta$, then B:A $\notin \Theta$.

Intuitively, A ('Animal') <u>covers</u> B ('Man') if (1) an aboutness valuation of the swff qu(Bx), in a context, assigns a subset of the extension of qu(Ax) and $T\#(\P \neg Bx)$ contains $Aw_{ab} - Bw_{ab}$ (the difference: $(x_1, x_2) \subseteq (A - B)$ iff $(x_1, x_2) \subseteq A \cap \overline{B}$). In addition: (ii) for A to cover B, whatever makes \$Bx true of any x, in a context, is not co-tenable with the complement of any set of circumstances forcing T(Ax), in that context. In other words, the right residual of 'Animal' by 'Man' fixes a set of circumstances in <u>St</u> x <u>Ab</u> in which the statement that- x_1 is a man entails that- x_1 is an animal; but not <u>vice-versa</u>. If $T(\P \neg x_2$ is an animal) is forced in context, then, for no $(y_1, y_2) \in \underline{Ab}$ does ' $x_2 =_{man} y_2$ ' hold for any assigned complex in $T\#(\P y_2$ is a man). I abbreviate "A covers B (B is subordinate to A)" by " \mathbb{P}_A ".

Further defined relations between general term predications are given by:

Df. G5): A is co-ordinate with B (A
$$\approx$$
 B) iff B_A and A_B .
Df. G6): A, B intersect (A + B) iff, when Aw_{ab} \cap Bw_{ab} \neq
(Λ,Λ), A o B \in St whenever A, B \in St.
Df. G7): $\{B_1, B_2, \ldots\}$ restrict A iff for each $B_i \in \{B_1, B_2, \ldots\}$
if $B_i \in St$, then B_{iA} only if $B_i \not\approx A$.

(Thus, several possibly intersecting value sets of terms restrict a common general term when each is covered by a term not co-ordinate with it--e.g. 'water', 'beer', 'mercury', ... restrict 'liquid'. Since 'restriction' is analogous to subordination, I employ the abbreviation " $\{B_1, B_2, \ldots, 3_A\}$ ").

The following illustrates the relations defined thus far:

(i) <u>Subordination</u>: B_A is a basic relation between extensions of general terms. It should not be confused with set-theoretic inclusion ('being red and not red' has an extension $-\lambda$ -- included in every extension, but not subordinate to it). Nor is subordination equivalent to <u>proper</u> inclusion. Imagine three items a,b,c such that Red(a), White(b), but c is not coloured (c might be a prime number). Then, $\overline{red} \subset \overline{red} \cup \overline{white}$, but is not subordinate to $\overline{red} \cup \overline{white}$. For, $\overline{red} = \{b, c\}$, $\overline{white} = \{a, c\}$ and $\overline{red} \cup \overline{white} = \{a, b, c\}$. But the anti-extensions (not complements) of both Red and White are subordinate to Coloured; whence only $\{a\}$, $\{b\}$ and $\{a, b\}$ are subordinate to $\overline{red} \cup \overline{white}$. That is, $red:Coloured = \{b\}$ in G, not $\{b, c\}$.

Subordination holds characteristically between species and genus: e.g. 'water' is subordinate to 'liquid', 'oxygen' to 'gas', and both 'liquid' and 'gas', one supposes, to 'states of matter'. In general, many terms can be subordinate to some genus; and when there is no term to which a given genus is subordinate, it is said to be <u>ultimate</u>. I have identified only 'Item' thus far, as an ultimate sortal genus.

(ii) <u>Intersection</u>: (A + B) can be a many-termed relation--the most obvious taxonomic examples of which are <u>phase-sortals</u>, i.e. terms which apply in succession through the history of an item: 'Infant', 'Child', 'Youth', 'Adult'--all of which <u>restrict</u> 'Person'. Similarly, if an item is both red and square, then 'Colour' and 'Shape' intersect; or if an item serves two purposes, the terms it falls under intersect: decorative vases fall under the intersection (Ornament 4 Container), and so on.

(iii) <u>Co-ordination</u>: (A \approx B) represents coincidence of extensions, but is not merely set-theoretic extensional identity—since A \approx B requires both \widehat{A}_B and \widehat{B}_A and, as above, neither is equivalent to inclusion of all subsets of B in A and A in B. It seems, for example, that though 'Creature with Heart' and 'Creature with Kidneys' are co-extensive, they are not co-ordinate. For not every aboutness assignment will assign the same items to each (and subordination is defined, in part in terms of aboutness assignments). This is just to admit, of course, that not every occasion on which we talk about a creature with a heart will be one in which the item identified will satisfy Δ_{\emptyset} -predicates associated with 'Creature with Kidneys'; if only because we can certainly make statements <u>about</u> creatures having a circulatory system, but lacking that particular method of waste-disposal (namely: the statement that, as it happens, there appear to be no such creatures occurring naturally on our planet).

(iv) <u>Restriction</u>: $\{B_1, B_2, ...\}_A$ is analogous to "covering" in so far as each of a group of terms restrict a common term when each is covered by it. Frequently, such terms intersect, though not necessarily. Thus, gold, mercury, steel restrict 'metal' but not 'solid' (at room temperature). Some philosophers have argued (e.g. Wiggins, 1967, pp. 30-34) that if an item a falls under two non-intersecting sortals B, B' during its history, then there is at least one sortal which both B and B' restrict. I cannot say a priori whether this Restriction Principle is true (though it has been criticised, in Griffin, 1977, p. 76). Be that as it may, it is clear that there are occasions when the significance of an assertion can be called into question by the lack of a common term restricted by both the general term presupposed by the assertion's aboutness and the "super-predicates" of the predication. For example: what is expressed literally in asserting qu(Anger floats) in a context demands some compatibility of what 'Anger' restricts (presumably: 'Emotion' or 'Passion') with super-predicates of qu(x floats) -- that x is of a sort or stuff buoyed up by most liquids, that x displaces a volume whose ratio to mass exceeds that of x, and so on. The prima facie literal nonsignificance of qu(Anger floats) is attributable, thus, to our failing to determine, in context, any definite statement about emotions asserted thereby. Of course, in remote or non-literal contexts, varying the presupposed general term or the super-predicates of qu(Anger floats) may eventually result in some sort of content expressed--but only at the risk of losing the condition that the assertion is about some emotion, and not, say, some

personification of it.

At this point, the definitions of properties of general terms, expressed in terms of operations in G, should proceed to formal characterisation of the notion of a <u>sortal</u>. Unfortunately, I have to record as an open problem for CS-1 the lack of rigorous criteria for a term to be sortal. This is hardly surprising in view of the difficulties noted in the informal discussion of sortals (above, p.553). Problems arise in separating terms which function sometimes as +Count, sometimes as -Count (0il, energy, behaviour); genuine sortals from dummy sortals (sort, type, kind, thing); and sortal nouns from sortals formed from sortalising phrases (lump of x, piece of x, expanse of x,...).

What seems clear, nonetheless, is that sortals, through their associated predications <u>divide</u> their extensions into individuable items in a way in which non-sortal, mass terms—in the absence of sortalising phrases—do not. This suggests that one avenue towards a definition of sortalhood along which we might proceed would involve defining value-set extensions in <u>St x Ab</u> corresponding to sortal predications in such a way that the extension/anti-extension of a sortal is correlated with predications giving principles of persistence, uniqueness and identity for items falling under sorts. Something of this suggestion might be achieved by adding uniqueness-clauses to the Δ_A -predications for a sortal A--somewhat in the manner in which a uniqueness-clause appears in the Russellian definition of definite descriptions. For example, we might require of a sortal B that, for every term which covers it, the associated predications
are satisfied uniquely by each satisfier of B--just as, even though, everything falling under either 'man' or 'snow' also falls under 'existent', only Δ_{man} -predications convey principles satisfied by "the (unique) existent which is either this man, or that man, or....". There seems no easy method, however, for incorporating such a condition into the semantics of CS-1--at which point I leave the question open.

The remaining task of this subsection is to provide some formal characterisation of the $\Delta_{g}(\gamma)$ relation upon value-sets. That is: what are the closure-conditions for the field of this primitive relation? I begin with <u>logical</u> conditions for closure: (c.f. Routley and Griffin, <u>1979</u>).

Clearly, if $qu(F_{\underline{a}}) = \frac{1}{\theta} \Re_{\underline{b}}$, when $qu(F_{\underline{a}})(\theta)$ is about \underline{b} iff $\underline{a} = A_{\underline{b}}$, then <u>one</u> of the associated Δ_A -predicates in θ will be $\Re Ax$, itself (as much is guaranteed, already, by (R2) of Df. <u>Ab</u>.I (p. 553)). In addition, membership of Δ_A will be unaffected by truth-functional composition of Δ_A -predications--since the <u>significance</u> of predications of aboutness items is a pre-requisite for their entering into truth-functional relations (<u>qua</u> statements). That is, for any covering term A, Δ_A is closed under truth-functional operations upon statement value-sets in <u>St</u> x <u>Ab</u>, determined by the partial modelling m:

<u>Df</u>. 1): Let (x_1, x_2) , $(y_1, y_2) \in D^n$ (for some n and $D \in m$) and $\langle v, w_{ab} \rangle$ be defined on m such that, if $w_{ab}(Ax) = (x_1, x_2)$ and $x_1 \in R(A)$, $x_2 \in \overline{R(A)}$, then $v(A\underline{a})w_{ab} = 1$ when $x_1 =_A \underline{a}$: then $\Delta_A(\gamma)$ is a relation whose field is a set of swffs of CS-1, closed under the following conditions (where T#(-), F#(-) are defined as in II, C (p.4.83) (i) $T # (Ax_1) \in \Delta_A$.

(ii) If $T#(\emptyset x_1) \in \Delta_A$, then $F#(\emptyset x_2) \in \Delta_A$ iff $x_1 \in R(A)$, $x_2 \in \overline{R(A)}$, relative to w_{ab} , and $T#(\emptyset x_1) \cup F#(\emptyset x_2)$ $\subseteq (R(A) \cup \overline{R(A)}) \cap \Theta$, for some context Θ .

(iii) if $T#(\emptyset x_1)$, $T#(\gamma x_1) \in \Delta_A$ and $F(\emptyset x_1/\gamma x_1) \in \Theta$, then $T#(\emptyset x_1 \vee \gamma x_1)$, $T#(\emptyset x_1 \wedge \gamma x_1) \in \Delta_A$, relative to Θ . (iv) if $T(\emptyset x_1) \in \Delta_A$ and $qu(\emptyset x_1) = H S \gamma x_1$, for some $S \gamma x_1 \in \Theta$, then $T#(\gamma x_1) \in \Delta_A$.

(v) For all \prec , β such that $\alpha \in \Delta_A$ and $\alpha \rightarrow \beta$, relative to m, $\beta \in \Delta_A$.

 Δ_A -value sets also include those of predications whose significance over items falling under A is a consequence of A's covering, intersecting or restricting terms all of which the items satisfy or fail to satisfy in the context. For example, 'Colour' covers a range of general terms 'Hue', 'Brightness' 'Intensity', such that any item (say, qu(Titian's red)) falling under 'Colour' either satisfies or fails to satisfy some of the terms immediately restricting 'Colour', some of the terms restricting what 'Colour' restricts, and some restricting terms restricting significantly with being coloured will be entailed or presupposed by other statements contingent upon the items satisfying them being coloured. That is, in brief, with each general term, Δ_A associates a family of value sets which "cluster" about the items falling under the term much in the way depicted in the following

diagram:



We can call the family of value-sets clustering about application of qu(Ax) to items the "neighbourhood" of A. The task of the next section is to concentrate upon which value sets in a <u>context</u> are to be located in this "neighbourhood", so that significance and aboutness is restricted to features available from context.

This completes the general conditions for closure of Δ_{A} -predications and concludes this investigation into the structure of general terms functioning as presuppositions of the aboutness of utterances in context, and "super-predicates" of what is predicated, in context, of what an utterances is about.

(III)(C): Context in CS-1:

The notion of <u>context</u> has, thus, far, been left fairly non-specific with respect to the semantics for CS-1. Certainly, the term "context of assertion" is rather vague. "Context" itself, is often used equivocally for 'sentential context' (the least linguistically autonomous unit within which an expression appears), 'linguistic context' (surrounding discourse), 'physical context' (environment of the speech act) or, even more generally, to connote the total social, historical or cultural <u>milieu</u> for a text, event, performance or artifact.

For the moment, I leave the notion of context deliberately vague, in order to draw together aspects of each of the senses in which it might be used. As in preceding subsections, I discuss first the philosophical grounds for the formal measures undertaken subsequently.

I have not discussed, and will not discuss in detail, many of

the obvious features of assertions for the interpretation of which reference to a linguistic or physical context is necessary. For example, tokenreflexive expressions "now", "this", "here"; pronouns "he", "she" and egocentric words; tensing of verbs; and adverbial modifiers of time, location and action; each have complex interpretational structures requiring some form of indexing to context to fix their significance. Further, there arise, for such expressions, problems of their logic, in relation to other expressions in the <u>sentential</u> context, which would require cumbersome semantic restrictions for their resolution. To consider just one example:

(1) qu(The boy who kicked me at school is now an M.P.). The aboutness of (1) subsumes the mentioned item under the sortal 'Boy', yet (contingently) no boy is now an M.P.; i.e. "super-predicates" associated with "being an M.P." conflict, on legal grounds, with individuation of qu(the boy who kicked me at school) under 'Boy'. Nevertheless, (1) is certainly not statement-failing in context--even though its full semantic representation will be inordinately complex, in ways not immediately apparent.

For example, the aboutness of (1) is token-reflexive upon the speaker. Yet, though it is true that (1) is about whichever unique individual, who was a boy, was at school with the speaker, and there kicked him; to avoid the contextual entailment that some 30 year old adult male speaker was kicked by a boy, the identity of qu(me), in the context, has to be fixed, instead, under some phase sortal which intersects over time with a 30 year old speaker. Similarly, the identity of (1)'s aboutness has to be fixed, again, under intersecting phase-sortals restricting

'Person' to identify him as the <u>same</u> boy who kicked the speaker (as a boy) and became the man who was elected to parliament.

Such complexities I have to ignore in the following. On the other hand, what cannot be ignored is the claim made by some logicians (e.g. Quine, <u>1960</u>, Ch. vi) that, for the purposes of semantics, token-reflexives, egocentrics and other context-relative expressions can (and should be) disregarded--since every utterance containing them can be expanded to an analytically equivalent <u>eternal sentence</u> whose significance is not contextdependent. Such a claim cannot be ignored, if only because it threatens the aim of investigating significance through the interaction of utterance with context. I offer two arguments against this claim.

Note that, if the proponent of this claim intends only that, for most purposes of logic, eternal sentences only need be considered, then I have no arguments to offer against what amounts merely to an heuristic simplification.

On the other hand, if it is proposed that, as a matter of language, significance is properly construed as a property of the semantic representation of eternal sentences--for which context-relative utterance are abbreviated, or elliptical variants--then I believe the claim to be seriously mistaken.

Firstly, on grounds of inference or validity, there is no adequate sense of <u>equivalence</u> in respect of which context-dependent utterances can be said to be "equivalent" to expanded eternal sentences. To put this another way: none of a number of plausible candidates for the relation of 'equivalence' suffice for <u>all</u> cases of context-dependence. This can

be argued by cases.

One can hardly dispute that, say, $qu(\log_a n = x \text{ iff } a^x = n)$ does not vary in mathematical significance from context to context, provided that some uniform system of notation in which it appears (and not, say, as a series of cracks on a wall) contains appropriately defined operations of exponientation. Nonetheless, it might be suggested that the <u>truth</u>conditions for some instance of that schematic equation--say, that $\log_2 16 =$ 4 is interderivable by definitions and equations with that $-2^4 = 16$ -- are properly a matter of proof. Yet, <u>understanding</u> such an arithmetical assertion is hardly to be equated with its truth-conditions. For it may be no help to the pupil being taught <u>how</u> logarithms afford a method of rapid computation, to be shown a proof that $\log_a n = x$ is interderivable with $a^x = n$. In this way, it may be suggested, a wedge may be driven between truth-conditions and significance (which is understood in grasping the arithmetical assertion) which the reduction of propositions to eternal sentences overrides.

For descriptive assertions that are not already eternal, Quine proposes (op. cit.) an expansion which consists, first, in replacing tensed verbs by tenseless ones combined with time- and date-indicators. Secondly, egocentric and token-reflexive expression are replaced by exact, objective spati-temporal references; and, thirdly, incomplete descriptions and proper names are filled out so as to secure unique reference, for all time.

It is doubtful whether this expansion can be carried out in every case--but that is not the point. I am more concerned to argue, however, that the expansion remains inadequate no matter how weak a notion of 'equivalence' is invoked to justify passing from non-eternal to eternal sentence.

Consider, first, the proposal to treat tensed assertions in context as 'equivalent' to untensed sentences combined with dates, or eventmarkers. Quine has in mind, one supposes, expansion of (2) into (2'):

(2) qu(It is now snowing in Hamilton)(θ)

(2') qu(It snows in Hamilton on December 4, 1977).

The apparent methodological 'parsimony' achieved by paraphrasing the token reflexive "now" in (2) as in (2') is offset by the fact that what is a one-place predicate in (2) becomes a two-place relation in (2') (between "snow" and a date). Methodologically, the domain of sets required to interpret dyadic predications is more complex than that for monadic predications.

The expansion of (2) into (2') raises two further difficulties. (2') is well-formed, hence meaningful, only if times exist in the universe of discourse of the language. The expansion, that is, commits us to the existence of times in a way which is <u>presupposed</u> by the syntax, rather than a consequence of the semantics. Certainly, (2), if true, is necessarily true or false at some time; but (2) is only contingently significant. On the other hand, the denotation of the date constant in (2') has to be taken as fixed independently of the assertion of (2'). In this sense, the commitment to the existence of 'times' is prior to the assertion of (2'), in a way in which the commitment in (2) is not.

In addition, it is difficult to make sense of the supposition that

(2) and (2') are equivalent. Clearly, intersubstitutivity in all contexts <u>salva significatione</u> is too strong a relation: qu(John hopes that-(2)) does not yield the same statement as qu(John hopes that (2')). Similarly, necessary equivalence, or truth in all and only the same possible worlds, also fails, in virtue of:

(3) Not necessarily (if snow falls now in Hamilton, then snow falls in Hamilton on Dec. 45h, 1977)

whereas:

(3') Necessarily (if snow falls in Hamilton on Dec. 4th, 1977, then snow falls in Hamilton on Dec. 4th, 1977).

At best, then, (2) may be true (contingently) whenever (2') is true, and false whenever (2') is false; provided that (2) is asserted on Dec. 4th, 1977. The added proviso, however, threatens the expansion with a regress. In general, qu(Fa, now) is truth-functionally equivalent to qu(Fa at t_0) if t_0 = time of assertion of qu(Fa, now). It seems, then, that the former is only properly an "eternalised" equivalent of the latter if combined with the condition that- t_0 = now. Thus:

(4) $t_0 = \text{now} \cdot \supset \cdot$ Fa at t_0' is truth functionally equivalent to 'Fa, now'.

The left-hand side of (4), however, is no longer an eternal sentence, and a regress is threatened in its further expansion.

Further objections can be raised, I believe, against Quine's proposal to replace incomplete descriptions and proper names by definite descriptions true of items for all time. These objections stem from general arguments in Kripke (1972, pp. 253-356) and Donnellan (1972, pp. 356-379) against any set of definite descriptions being sufficient to identify the referent of a proper name, especially in contexts involving counterfactual assertions. The general tenor of this attack upon "Cluster Theories" of reference is sufficiently well-known for me not to have to reduplicate it here.

My general conclusion at this point is that the simplification of treating all assertions as paraphrasable into equivalent eternal sentences loses more than it gains. It seems a misconception of acts of making statements, referring, characterising, and classifying to suppose that sentences of a language can fulfill these purposes autonomously, without recourse to the circumstantial features--the environment--in which we, as speakers of the language, carry out these acts. The gain of this idealisation is that the subject-matter of linguistic and logical investigations can be isolated from the complexities of speech-acts and their contexts. The loss is the threat that idealised eternal sentences simply do not have the systematic logical and semantic properties which are exhibited in the interaction of utterance with context, act with occasion.

As mentioned at the start of this Section (D), and in II, Section B, <u>nonsignificance</u> through category-mismatch in predication is a generalisation of one variety of context-relative statement-failure: namely, when there is a conflict between aboutness-conditions and statement-conditions which recurs in a range of contexts relevantly like the given context in respect of the logical presuppositions and entailments of the utterance, but differing, perhaps, in respect of contingent contextual features. This accords with the conviction--expressed frequently, above--that, though speakers of a language <u>have</u> a strong preferrence to "maximise sense"

in interpreting one another's utterances (even to the extent of shifting automatically to nonliteral interpretation when literal significance fails), nevertheless, a significance-failure is not simply the failure of an utterance to yield a statement. Rather utterances fail to be significant when, despite our efforts, as it were, to revise the circumstantial commitments a speaker's utterance makes, the incompatibility of determining the aboutness of the utterance with what it states remains for a range of similar contexts. Characteristically, a category-mistaken predication is one which, in each contextual re-interpretation which weakens the $\Delta_{\!_{A}}$ conditions for aboutness, or sharpens (decreases) or blurs (increases) the domain of application of the predicate (extension and anti-extension), the conflict between aboutness and statement-conditions remains. Unlike a merely circumstantial failure of reference, therefore, the nonsignificance of a category-mistaken predication amounts to their being no complex in that context--for any of a certain subset of partial models m, m',..., which agree upon the relevant entailments and presuppositions of the utterance. Expressed somewhat figuratively then, a significance failure is a "value-set gap" in the domain St x Ab which remains unfilled by each attempt to fill gaps in all contexts relevantly like the context of assertion. In this respect, a measure of the significant content an utterance expresses is provided by the number and kind of contexts complementary to those in which it fails to yield a statement.

In order to implement this rough-and-ready description of significancefailure into the formalism of CS-1, I have to articulate this notion of a "measure of content" in relation to the partial modellings m of swffs onto value sets in <u>St</u> x <u>Ab</u> described in the foregoing sections. This task requires two separate steps. I define a certain set of models which result from completions of any given partial model m, relative to an ordering relation across value sets. I then define a <u>measure</u> over sets of value sets (contexts) preserving a weighted sample of aboutness- and statement-conditions (expressed as value-sets in each context). Intuitively, such a measure is an abstract representation of <u>how far</u> the content expressed in a context must deviate from an utterance's assigned interpretation before it becomes successfully statement yielding. Thirdly, I demonstrate how incompatibility in the designated range of graded models represents category-mismatch in the sortal and feature-placing conditions fixing aboutness and statement. Illustrations applying the resulting semantic structure to category-mistaken predications are deferred until the opening of the concluding Section E. There, I take up again the generic relationships between 'categories', sorts and types.

Formal development of Context Models for CS-1:

A <u>graded</u> model for CS-1 is a certain set of partial models m', based upon a ground model m, which are completions of m under a relation AN(M,S) (signifying, for want of a better phrase: "models S are <u>analogues</u> of M"). Much of the remainder of this section is devoted to characterising AN in terms of ' \leq_{ab} ', ' \rightarrow ', ' $_{\theta} \dashv$ ', ' $\Delta_{\phi}(\gamma)$ ' and m-assigned value-sets in <u>St</u> x <u>Ab</u>. Intuitively, AN functions in some ways like an "accessibilityrelation" for the semantics of modal logic, in so far as it delimits subsets of the set of all classical completions of m (supervaluations) which

agree with m in preserving contextual features of a given utterance.

With respect to this subset of completions of m, I consider a probability-measure defined over a field of subsets of this set of completions which contains, in particular, for each predicative swff $qu(\phi)$ and m-assigned value-sets <u>a</u>, the set of completions S' in which $\langle v_m, w_{ab} \rangle$ determines $qu(\emptyset)$ as satisfied in either $T(\underline{a})$ or $F(\underline{a})$ --i.e. $v_{S}'(\emptyset)w_{ab} = 1$ or 0, in some S' which completes m, relative to AN, according as T(a), resp. F(a) appears in S' (and $qu(\phi)$ is statement-yielding). This last description warrants that each swff has a measure in respect of how far contexts (qua families of circumstances) diverge in respect of 'entailed' and 'presupposed' value sets, before the gap corresponding to a statementfailure is filled (equivalently: before some subset of completions forces some complex in $T(\underline{a})$ or $F(\underline{a})$; c.f. II, Section C, p. 492). The possibility exists, of course, that, for some swff, some subset of completions of m forces both T(a) and $F(\underline{a})$ --whereupon the completion is inconsistent. It does not follow that the swff in question, say $qu(p \supset \sim p)$, fails to yield a statement. It merely yields a statement that is false, necessarily, for all classical completions of m. The following characterisation of contextually graded models for CS-1 is based in part upon investigations by J. Kamp (1973, pp. 123-155).

It is first necessary to specify a relation AN(M,S) over a field of subsets of <u>St x Ab</u> (sets of circumstances). A partial ordering particularly suited to the intuitions described above is the relation between partial models and their completions ' \leq_p ' which holds between sets of circumstances in S according as at least as many circumstances forcing $T(\underline{a})$, relative to M, force $F(\underline{a})$ --where '<u>a</u>' becomes in S the value set 'filling' the gap for a statement-failing swff, relative to M. Thus according as the ground model m=M varies, so a probability function defined over the field of subsets of <u>St</u> generated by AN(M,S) assigns to each swff a measure of the likelihood of its yielding a true or false statement as contextual circumstances alter only in relevant ways.

Subject to specification of S', !S'!, F and C*, below, set AN(M,S) as a relation over a field F of subsets of <u>St</u> x <u>Ab</u> which holds of M,S whenever, for any a \in F:

 $T(\underline{a}) \leq p, S^{4}F(\underline{a})$ iff at least as many circumstances in !S'!relative to M (the ground model) force $T(\underline{a})$ as force $F(\underline{a})$ in !S'!.

Thus, a <u>contextually graded model</u> (cgm) for CS-1 becomes a quintuple $M^* = \langle M, S, C^*, F, f_{\leq p} \rangle$ with the following properties:

1) M is a partial model of swff of CS-1 ($M = \langle D, R, Ab \rangle$, for some M as defined in II, C and II,D). M is called the "ground model" with respect to utterances under evaluation.

2) S is a set of partial models with the same domain as M.

3) S is a <u>chain</u> defined under $\leq_{p,S}$ such that the union of maximal chains in S is complete (see below: Note (\mathbf{d})).

4) C* is a function from partial models to partial models whose range is the set of pairs $\langle M, S \rangle$ where (i) M \in S; (ii) AN(M,S); (iii) the union of maximal chains of S is complete.

5) F is a field over the set !S! of unions of maximal chains in S under ' \leq_p '. (A field F over !S! is a set of subsets of !S! such that (i) !S! \in F; (ii) $\land \in$ F; (iii) if S,S' \in F, then S \cap S', S—S' \in F); and for each $\langle M'', S'' \rangle$ in C*, if !S''! is the set of unions of maximal chains in S'', then $\{M''' \in !S''!: A(M'',M''')\} \in F$.

6) f_{\perp} is a probability measure with domain F and range in the

real interval [0,1] such that (i) if $M \in F$, $f_{\underline{A}}(M) = 1$; (ii) if $M' \in F$, then $f_{\underline{A}}[(M-M')] = 1 - f_{\underline{A}}[(M')]$; and (iii) when G is a countable subset of F such that (a) for each $M'', M' \in G$ for which, if $\underline{a} \in M'$ and $\underline{b} \in M''$, then $T(\underline{a}/\underline{b}) \subseteq G$, $M' \cap M'' = \Lambda$, and (b) $\bigcup G \in F$, then:

$$f_{\pm} (\bigcup G) = \underset{M_{i} \in G}{\overset{f_{\pm}}{\times}} f_{\pm} (M_{i}).$$

To explain each of these features in turn:

1) $M = \langle D, R, \underline{Ab} \rangle$ is called the ground model of M*, and for each atomic swff, the M-assigned value-set in St x <u>Ab</u> is either in T(<u>a</u>), for some <u>a</u>, or in F(<u>a</u>), or in neither. Similarly, if C*(Θ) = $\langle M_{\Theta}', S_{\Theta}' \rangle$ then M_{$\Theta}'$ is the <u>ground model</u> in M* with respect to Θ ; where Θ is simply, for each atomic swff qu(\emptyset), the union of the family of circumstances in T#(<u>a</u>) \cup F#(<u>a</u>), whenever qu(\emptyset)(Θ) Y S<u>a</u>. When \neg (E<u>a</u>) qu(\emptyset)(Θ) Y S<u>a</u>, then T#(<u>a</u>) \cup F#(<u>a</u>) forces no complex in T(<u>a</u>) nor in F(<u>a</u>)--and there is no M'_{Θ} - assigned value-set <u>a</u> in Θ . At some point in the chain of completions, however, relative to AN(M',S'), some value-set may be assigned to qu(\emptyset) eventually. This represents the context at which qu(\emptyset) becomes statement-yielding.</sub>

2) Each partial model in S preserves the same domain as M. This assumption is, no doubt, arbitrary; since, as we acquire new information in more determinate contexts, it may well be that--making distinctions or reclassifying items--the domain of items may increase or decrease in cardinality. The assumption is required, however, to ensure that logical truths remain invariant with respect to aboutness-assignments and valuations.

3) (a) To assume that S is a chain of sets under the partial ordering $\underline{\zeta}_p$ reflects the fact that, as the significance of an utterance is appraised in a context, there is an order to the progressively more detailed information

to which we might appeal. That is, as I expressed it in my informal remarks above, aboutness and statement success is determined by a weighted sample of the information (in the form of beliefs, judgements, statements) brought to bear upon the utterance. Ordinarily, however, one would not suppose that, for every utterance which fails to yield a statement in context, there is only one way in which acquisition of new information resolves the statement-failure (if at all) in just this one The progression, that is, is not linear, but can involve all forms way. of observation, deductions, abductions and inductions. Suppose, for example qu(This (item) is large) fails to yield a statement in a given context--perhaps through the vagueness of the predicate qu(x is large). There may be no definite feature which decides, from context to context, what it is that determines a mentioned item to be 'large' or not. A large mouse is miniscule in relation to a small elephant -- so the truthconditions of assertions about large mice are not comparable, directly with those for small elephants.

(b) Though I cannot go into detail, here, one can hypothesise that, for utterances which fail through vagueness, all particular cases of vagueness can eventually be resolved, by addition of more definite information; though not all at once, and not all in the same way. For a 'large mouse' <u>versus</u> 'small elephant' example, one supposes that some transformation from the covering concept for aboutness to the adjectival occurrence of the predicate qu(x is large) restricts the extension/antiextension in a way suggested by the relation between:

This (mouse) is large. and This (mouse) is large (amongst mice),

where this latter is explicated in terms of the comparative: "Not (at least as many circumstances force F(\S this mouse is larger than that) as force T(\S this mouse is larger than that)." (see: Kamp, <u>1973</u>, p.141).

Nevertheless, we cannot assume that every case of significance failure is resolved in some context -- for, though even a word-salad sentence or random string of letters may be significant in some context (e.g. as a code or shorthand), it is doubtful whether such distinctive instances of the dependency of significance upon contextual features can be used to support the claim that every utterance is interpretable in some context -- at least, not in any philosophically interesting sense. That is, if what is characteristic of category-mistaken assertions is that they yield no publicly identifiable statement which can function as a truth-claim in context, then the fact that such assertions may yet be interpretable relative to some suitably esoteric contextual considerations is of no help in finding some measure of how much contextual information would have to alter to "make sense of" an overtly nonsignificant assertion. In this sense, what is needed is some restriction upon contexts diverging from the "ground" context of assertion which ensures that those contexts are "accessible"--in that a weighted number of the aboutnessand statement-conditions of the original context are retained in an "accessible" context.

In formal terms, this is precisely the function of the relation 'AN(M,S)': each chain of completions from a ground model M*, when a completion S" is such that AN(M*,S"), differs from M* only in, perhaps, containing some value sets not assigned in M*, or (inclusively) in

containing Δ_{B} -value sets, for some general term B, identifying aboutness, where M* contains Δ_{A} -value sets—and then only if B stands in an appropriate taxonomic relation to A. In particular, if a ground model M* <u>lacks</u> value sets in T(<u>a</u>) \cup F(<u>a</u>), for some <u>a</u> \in St x Ab (i.e. T(<u>a</u>) \cup F(<u>a</u>) = \wedge), it is quite possible that T(<u>a</u>) \cup F(<u>a</u>) = \wedge , for <u>all</u> S' such that AN(M*,S'). This is the case whenever aboutness- and statement-conditions for an utterance are simply incompatible in M* and there is no statement \$a the utterance yields. (Formally, this is granted in the requirement that, though $\wedge \notin M^*$, $\wedge \in$ F, for every field of subsets of (<u>St</u> x Ab)ⁿ, which are unions of maximal chains of $\{M^*\}$).

(d) In order that completions S of a ground model M form a <u>chain</u> under AN(M,S), and that <u>classical</u> completions of M comprise unions !S! of maximal chains in S, AN(M,S) has to be a partial ordering (reflexive, antisymmetric and transitive) relation with field S, subject to some postulated special constraints. In addition, AN(M,S) has to be linked to the ordering ' \leq_{p} ' in such a way that, for any given statement-failing swff qu(\emptyset) in a context θ , relative to ground model M, the likelihood of qu(\emptyset)'s yielding a true or false statement is measured in terms of the completions S₀ of (M1 θ)--the context θ restricted to value sets determined in M--in which some value set in either T(<u>a</u>) or F(<u>a</u>) is eventually forced, when qu(\emptyset) (S1 θ) $\stackrel{\cdot}{\cdot}$ <u>a</u>. In this way, provided there is no intrinsic incompatibility in the aboutness- and statement-conditions for qu(\emptyset), relative to (S1 θ) and subject to conditions on ' \leq_{p} ' ensuring consistency, then, when qu(\emptyset) is just as likely to yield a truth as a falsity in some context "accessible" from M(1 θ), the statement-failure of qu(\emptyset) in M(1 θ) is simply circumstantial (contingent). That is, our being unable to discern the truth-conditions of the assertion results from insufficient or inadequate information in Θ to determine the aboutness and statement made.

Typically, such statement-failing predications are anomalous only because, in context, they are <u>vague</u>, pragmatically <u>ambiguous</u> or contain a <u>referentially-failing</u> subject term. Consider an example of a contextually vague assertion (due to Kamp, 1973, pp. 146-8):

(1) qu(John is rather clever)(θ)

and suppose the reference to the person John is quite determinate in θ . There is no question that (1) is significant (there is no incompatibility of person-predicates with the applicability of qu(x is clever)); thus, the question is whether (1) yields a definite statement about John in θ . "Rather(Fa)", in English, is an adjectival modifier (syntactically: forms adjectives from adjectives) which, on some occasions, appears to entail that a is definitely F, if a is rather F. On the other occasions, however, it appears to follow from a denial of a's being F--analogous to the manner in which qu(Most a's are F) sometimes entails that not all a's are F; and yet in other assertions, it follows from qu(all <u>a</u>'s are F). The vagueness of (1), however, is not so much a feature of the equivocation upon what it entails and is entailed by, as a feature of the demarcation between the extension and anti-extension of qu(x is clever). The predicate qu(x is)clever), of course, does not have an invariant extension across aboutnessitems of differing sorts: cleverness in persons seems related to their quick wit and problem-solving ability; whereas cleverness in dogs is a far humbler property--and, certainly, cleverness in people may be judged

by different criteria in different circumstances. Be that as it may, confined to persons, qu(a is rather clever) seems to require a certain weakening in the criteria for applying the predicate, relative to the proportion of the extension of 'Person' which comprises definitely clever people. That is, "rather" can be interpreted as an operator on adjectival predicates which forms different adjectival predicates in such a way that, as further value-sets in S for predications: $\Im x_1$ solves problems, $\Im x_1$ is quick-witted,..., for some $x_1 \in \langle D, R \rangle$ such that $x_1 =$ John, then there is an increasing likelihood that $T(S x_1 \text{ is clever})$ is forced in some completion (S10) of (M10) such that AN(M,S). Thus, qu(John is rather clever) yields a truth if $\neg (T(\S_{x_1} \text{ is clever}) \leq_p F(\S_{x_2} \text{ is clever})),$ relative to some $(x_1, x_2) \in \underline{Ab}$ such that $x_1 = \frac{1}{person}$ John. Ou(John is rather clever) yields a falsehood if $\neg (F(\Sx_2 \text{ is clever}) \leq T(\Sx_1 \text{ is})$ clever), as new value sets are forced in (S10). So, for a probabilityassignment f_{\leq} over a field of subsets of !S! (unions of maximal chains in S such that $\langle M, S \rangle \in C^*$ and AN(M, S), ' \leq_p ' determines a measure of the likelihood that acquisition of further information will eventually resolve the vagueness of (1). This new information can be regarded as beliefs, judgements, observations, and so on, which constitute each new context θ' , determined by the function C*, accessible from a context in which an. initially statement-failing utterance is evaluated.

Determining the values in the interval [0,1] that the probabilityassignment $f_{\underline{c}}$ gives for any given set of value-sets, relative to a contextually graded model (cgm) M*, is not, of course, an <u>a priori</u> matter. However, it can be postulated, at least, that: (a) the probability that $T(\underline{a} \vee \neg \underline{a})$ will be forced eventually in a classical completion of M(a supervaluation on M--see Van Fraassen, <u>1971</u>) will always be 1; and that $f \leq [F(\underline{a} \land \neg \underline{a})] = 1$.

(b) for any atomic swff \emptyset , such that $qu(\emptyset)(\Theta_S) Y \underbrace{\Sb}_S$, $f_{\leq} [T(\underline{b})] = 1 - f_{\leq} [F(\underline{b})].$

(c) the field of subsets F over which f_{\perp} is defined is \neg -consistent, closed under truth-functions and isomorphic with some product of the algebras (<u>St x Ab</u>) x G.

(see further conditions on ' \leq_p ' and ' f_{\leq} ', below).

The example considered above is one of vagueness. In a sense, the vagueness of a predication is measured by the probability that some contextual determination will eventually resolve it; i.e. increasing or decreasing the extension assigned by R in each successive (S10) will eventually determine the aboutness item of the subject to appear either definitely in the extension or definitely in the anti-extension of the predicate. The strategy is somewhat analogous to a question and answer procedure which increases the information relevant to grasping the aboutness and truth-conditions of an utterance, as it proceeds. The <u>relevance</u> of the answers (value sets) depends upon the initial ground context, and upon whatever of the aboutness and truth-conditions is available intitially from both the utterance and the ground context. Suppose we were confronted with a line of a partial text which read: (2) qu(This sings). Successively we seek to determine:



Eventually, the statement-value of (2) will be identified in some θ_{M}^{n} .

Such examples contrast markedly with significance-failures. If there is an initial incompatibility between aboutness and statementconditions for a predication $qu(F_a)(\Theta_M)$, then the \neg -consistency of the field over which f_{\leq} is defined demands that for no assignment f_{\leq} does $T(\S F_a) \stackrel{f}{=}_p F(\S F_a)$ hold for any set S of completions, such that AN(M,S). That is, for contexts <u>accessible</u> from the ground model—as fixed by the function C*--no value set in $T(\S F_a) \cup F(\S F_a)$ is forced. Thus, $f_{\leq}[T(\S F_a)]$, $f_{\leq}[F(\S F_a)]$ gives no value in [0,1]. Nevertheless, we can specify $' \stackrel{f}{=}_p$ ' so that it does provide a measure of how far the interpretation of $qu(F_a)$ would have to diverge from M to yield a statement eventually. We can do so by considering the complement \widetilde{S} comprising the difference $!S! \longrightarrow$ $\{\langle M, S \rangle$: AN(M,S) $\}$. Within \widetilde{S} , we can suppose $qu(F_a)$ is eventually assigned some value-set according as either the aboutness or the superpredicates (or perhaps <u>both</u>) determined for $qu(F_a)$ in Θ_M are varied.

Does this mean that it is <u>arbitrary</u> which value-set in \tilde{S} is selected eventually as being forced in some \tilde{M} ? That is, does \tilde{M} preserve nothing of M? If so, <u>every</u> significance-failure can be assigned the same value-set in \tilde{S} — and the complicated modelling of CS-1 yields no special advantages over simply assigning an <u>ad hoc</u> value to significance-failures in <u>classical</u> models of CS-1.

Clearly, however, it is <u>not</u> arbitrary which value-set in \tilde{S} is eventually forced for a given category-mistaken predication. Even though $qu(F_a)$ may remain nonsignificant through the range of contexts accessible from its context of assertion, it does have aboutness and statement-conditions. They happen to be incompatible in that range of contexts. For example, suppose qu(Fa) is the category mistaken predication qu(This idea sleeps)(Θ). We can suppose, naturally, that the significance failure of this predication results from the conflict of qu(This idea) being about an idea, in Θ , with the presumed super-predicate of qu(x sleeps) in Θ ; namely, that-x is a living entity.

Now, since $\widetilde{S} \subseteq !S!$ (the unions of maximal chains in S), there will be some sets of value-sets in \widetilde{S} (i.e. contexts) sharing value-sets in Θ . Suppose $\gamma \in \widetilde{S}$ is just such a context. Then:

qu(Fa) is about <u>b</u> in $\gamma \in \widetilde{S}$ if, relative to valuations $\langle v_{\widetilde{S}}, w_{ab} \rangle$ on \widetilde{S} , (a) <u>a</u> = idea <u>b</u> and $(\forall \emptyset \in \Delta_{idea})(\emptyset_{\underline{a}} \leftrightarrow \emptyset_{\underline{b}})$; and qu(Fa) $(\eta : \widetilde{S}) \vee \S_{\underline{a}}$ sleeps, if (b) qu(x sleeps) $\eta \uparrow i \S$ Living entity (x), and $(\forall \psi \in \Delta_{idea})(\forall x)(\forall x) \Rightarrow x = \underline{a} = \underline{b})$.

The incompatibility between <u>a</u> being both an idea and a living entity remains for the context $\eta \in S$. One can hypothesize, however, that $qu(F\underline{a})$ <u>would</u> be significantly interpretable if conditions (a) and (b) were reconciled in some way, in a context $\eta' \in S$ which is accessible from η , but inaccessible from the original context θ . Suppose, then, one considers a rather esoteric context n' in which (b) is replaced by:

(b') qu(x sleeps)
$$\eta \overline{n}$$
 SItem (x), and
($\forall \psi \in \Delta_{item}$) ($\forall x$) ($\psi x \leftrightarrow x = a = b$)
item item b)

As I argued in II, D(A) (p. 554), 'Item' is a general term covering everything which is a member of the domain of CS-1. That is, '<u>a</u> = <u>b</u>' contracts to Leibnizian identity, relative to the predicates of CS-1. Clearly 'Idea item' and 'Living entity item' both hold in G (the sub-algebra of <u>St</u> x <u>Ab</u>); i.e. 'Item' covers both terms. Thus, in $\gamma' \in \widetilde{S}$, qu(This idea sleeps) yields a statement, only if the extension/ anti-extension of qu(x sleeps) is increased to include every item in D $\in \widetilde{S}$.

The example is no doubt artificial and contrived--deliberately, in order to clarify the procedure involved. Notice first how the transference to the esoteric context η' represents a change in the significance of qu(This idea sleeps) from its content in θ -since qu(x sleeps) now has a much wider range of application. (Equally, one could have argued for a change in aboutness). Roughly stated, for this category-mistaken predication to yield some statement in a context, we have to suppose the criteria for applying qu(x sleeps) to an item are weakened to the extent that every item now appears in the R-assigned extension or anti-extension of the predicate. Since it is significant and statement-yielding, in such a context, to assert qu(x sleeps) of any aboutness-item, we have to suppose that asserting that this or that item sleeps is never statement-failing, in that context. That is, in this sense, context η' is accessible from context γ to just the extent that value sets for every such predication appear in η' ; in which case η' represents the classical completion relative to $\langle D, R_{\overline{S}} \rangle$ (x sleeps) of the modelling \widetilde{S} which determines η . In particular, η' contains value-sets for every predication: $qu(\underline{b} \text{ sleeps})$, $qu(\underline{c} \text{ sleeps}), qu(\underline{d} \text{ sleeps}) \text{ for all } \underline{b}, \underline{c}, \underline{d} \in D_{M*}$. Thus, when qu(This idea)sleeps) is significant and statement-yielding in γ ', then it becomes significant to assert qu(x sleeps) of any item in that context.

Someone might wish to reply that there simply could not be a context in which significance was conferred upon qu(This idea sleeps). The

reply misses the point--which is not to suggest that such esoteric contexts are often appealed to are at all important. The example is intended to convey in terms of CS-1 what is a familiar procedure in appraising significance, in making and supporting a significance-claim.

As was pointed out in I, D (p. 341), Ryle believed that determination of the category-mistakeness or category-correctness of an assertion involved the totality of logical relationships into which that assertion entered with other assertions. I have modified Ryle's claim, here, to represent in CS-1 a familiar kind of argument to which Ryle's dictum avers, and which Ryle, himself, used frequently (in <u>The Concept of Mind</u>, <u>1949</u>); namely, a form of the <u>reductio ad absurdum</u> applied to significancefailures.

Briefly: if a category-mistaken predication like qu(This idea sleeps) were to be significant in some context, then <u>that</u> context would be one in which every predication sharing the presuppositions and entailments of asserting qu(x sleeps) of some item would be significant and statement-yielding. In terms of the semantics of CS-1, then, a measure of the likelihood that the statement value-set for a category-mistaken assertion will be forced in some set of circumstances, by re-assigning aboutness for the subject term or the extension of the predicate in the <u>complement</u> of the set of circumstances in which the predicate in the <u>complement</u> of the set of circumstances. That is, if qu(This idea sleeps) yields a truth or falsity in a context, so does qu(This virtue sleeps) qu(This mental event sleeps), qu(This purple number sleeps), and so on.

In such cases, it is characteristic of significance-claims that support for them is often drawn from use of the <u>reductio</u> argument (<u>RAA</u>) in the form:

(<u>RAA</u>): "if it is significant to assert qu(Fx) of <u>a</u>, then it is significant to assert qu(Fx) of <u>b</u>, <u>c</u>, <u>d</u>,... where these are of the same sort as <u>a</u>. But, it is absurd that qu(Fb & Fc & ..) should be significant. So, it is not significant to assert qu(Fa)."

What has to be recognised in RAA-arguments of this form which support denials of significance, is that, <u>unless</u> we regard the contextual determinants of the significance or nonsignificance of qu(Fa) as being transferred from one context to a second, through the course of the argument, then the form of the argument appears paradoxical. For, the form of RAA seems to be:

$$\begin{array}{ccc} \underline{\text{To show:}} & \neg \operatorname{Sig}(\operatorname{qu}(\operatorname{Fa})(\operatorname{c})) \\ 1) & \operatorname{Sig}(\operatorname{qu}(\operatorname{Fa})(\operatorname{c})) \longrightarrow \operatorname{Sig}(\operatorname{qu}(\operatorname{Fb} \& \operatorname{Fc})(\operatorname{c})) \\ 2) & \neg \operatorname{Sig}(\operatorname{qu}(\operatorname{Fb} \& \operatorname{Fc})(\operatorname{c}) \\ 3) & \operatorname{Therefore,} \neg \operatorname{Sig}(\operatorname{qu}(\operatorname{Fa})(\operatorname{c})) \\ \end{array}$$

To give RAA this form is paradoxical in the sense that, in order for premise 2) to be true, the consequent of the entailment in 1) 'Sig(qu(F<u>b</u> & F<u>c</u>)(c))' must be false. But then, if 1) has a false consequent, the entailment can only be <u>vacuously</u> true--since 'Sig(qu(F<u>a</u>)(c))' has to be false on grounds irrelevant to the actual significance of qu(F<u>a</u>); namely, the ground that, as a matter of logic, no true entailment with a false consequent has a true antecedent. Thus 3), also, has to be true irrespective of the significance of qu(F<u>a</u>)(c)--a fact which is hardly surprising in view of the validity of <u>modus tollens</u> in the classical logic of conditionals: $p \supset q, \sim q \vdash \sim p$.

But RAA is intended to support an inference to the <u>nonsignificance</u> of $qu(F_{\underline{a}})$ from the (non-vacuous) truth of a significance-entailment, together with the (independently assessed) nonsignificance of mentioned predications. A proper form for RAA argument, therefore, is:

3) Therefore, $\neg Sig(qu(F_{\underline{a}})(c_1))$.

Now this last form of the inference is no longer valid (because of change of variable 'c₁' to 'c₂'), <u>unless</u> contexts c_1, c_2 stand in some relation which preserves <u>some</u> significance-features in switching from $qu(F_a)$ and $qu(F_b \& F_c)$ in c_1 to c_2 , and the significance claim is appraised <u>on the basis</u> of those preserved features. This, however, was precisely the motivation for setting the semantics of CS-1 to accommodate the transference of significance features (aboutness and statementconditions) from one range of contextually graded models to another.

Having explained through these examples the functioning of the chains of completions in cgm's in relation to f_{\leq} , \leq_p , AN(M,S), C* and <u>St x Ab</u>, it remains to set the formal conditions upon cgm's M* which determine models of CS-1 having these features. Before doing so, however, I have to digress to report upon the open problem of quantification in CS-1.

Quantifiers in CS-1:

As observed in II, D(A) (pp. 564-567), the aboutness modelling for

swffs of CS-1 lacks inductive assignments for quantified swffs. This had proved to be problematic because no formal restriction could be placed upon the aboutness-domain concerning whether items we talk about exist, are fictional, mythical, imaginary or even impossible. It would seem arbitrary, in any case, to place such a restriction upon aboutness-especially since the <u>significance</u> of talk about impossible objects is an issue quite pertinent to more comprehensive significance logics. It was primarily for this reason that, in II, C (p. 432), I introduced three styles of quantifier in CL (hence, in CS-1, also):

(Ux) Ø: for all items, Ø.
(Px) Ø: for some out of all items, Ø.
(∃x) Ø: there are Ø's (Ø's exist).

In this, I follow Routley and Goddard's example (<u>1973</u>, pp. 148-150, 351; also §7.8, p. 520), in explaining the choice as governed by recognition that, if what is talked about is wholly unrestricted, <u>any</u> item is a possible argument to a bound variable of quantification. For example,

(3) qu(Every item sleeps)

is not <u>false</u> of items like virtues, thoughts, forces and purple primes; it is <u>nonsignificant</u> for such instances. Since $qu((Ux) \times sleeps)$ can only yield a truth or falsity relative to $\langle v, w_{ab} \rangle$ on M if $v'_M(x \text{ sleeps}) w_{ab} =$ 1 for <u>all</u> items x such that $\langle v'_M, w'_A \rangle \approx x \langle v, w \rangle$ or $v'_M(x \text{ sleeps})_{w_{ab}} = 0$, for some out of all items x such that $\langle v', w'_{ab} \rangle \approx x \langle v, w \rangle$, then (3) fails to yield a definite statement unless restricted to items of a particular sort or type.

Various systems of restricted quantification for significance logics are examined in Routley and Goddard (loc. cit.)---I propose not to

burgeon the text with a review of them. I sketch instead a tentative version which, in Routley and Goddard (<u>1973</u>, p. 351) has some plausibility, given the view of aboutness I have proposed.

If determining aboutness is always relative to contextual identification under a 'covering' general term, then we can suppose that generalising by quantifying over items is, in context, always restricted to items of a sort or type (ultimately, of course, to 'Item', itself). For this purpose, Routley and Goddard introduce a connective '1' with the matrix (ibid, p. 351):

| 1 | 1 | 0 | - |
|---|---|---|---|
| 1 | 1 | 0 | - |
| 0 | - | - | - |
| - | - | - | - |

which serves to restrict the interpretation of bound variables to items antecedently satisfying some classifying predication.

Thus:
$$(Ax^{1} \emptyset)_{w} = 1$$
, for all $\langle v', w'_{ab} \rangle$ on M
 $\approx x \langle v, w \rangle$ "

is read " \emptyset is satisfied in M by all instances satisfying Ax" or, for short "every A-item is \emptyset ". This connective is not reducible by standard methods eliminating restricted quantifiers, if only because qu((Ux)(Ax $\supset \emptyset$)) suffers from the same significance-failure for nonsense items. In terms of '1', evaluation of quantified swffs, interpreted restrictively relative to context, could be given as follows:

for all $\langle v, w_{ab} \rangle$ on M* and swffs \emptyset (possibly containing 'x') $v((Ux)\emptyset)w_{ab} = 1$, relative to θ , iff for some A ε G, $v'(Ax \land \emptyset) = 1$ for all $\langle v', w'_{ab} \rangle$ $\approx x \langle v, w \rangle$.

$$v((U_{x})\emptyset)w_{ab} = 0, \text{ relative to } \theta \text{ iff for each } A \in G,$$

$$v'(A_{x} \land \emptyset) = 0, \text{ for some } \langle v', w'_{ab} \rangle$$

$$\approx x \langle v, w \rangle.$$

$$v((P_{x})\emptyset)w_{ab} = 1, \text{ relative to } \theta \text{ iff for some } A \in G,$$

$$v'(A_{x} \land \emptyset) = 1, \text{ for some } \langle v', w'_{ab} \rangle$$

$$\approx x \langle v, w \rangle.$$

$$v((P_{x})\emptyset)w_{ab} = 0, \text{ relative to } \theta \text{ iff for each } A \in G,$$

$$v'(A_{x} \land \emptyset) = 0 \text{ for every } \langle v', w'_{ab} \rangle$$

$$\approx x \langle v, w \rangle.$$

Lastly, I claimed in II, C (p. 448), that $qu((\exists x)\phi)$ is always true or false statement-yielding in context. The same effect could be achieved by introducing $qu(E_0 x)$ as a constant-predicate in CS-1, read as "x exists". On intuitive grounds, however, my preferrence is not to treat "exists" as a general predicate. 'Existent' is not a determinate sort or kind of item with a distinctive property. In the main, that is, each assertion of existence is highly context-relative--in the manner in which, say, qu(Forsooth, Hamlet still exists) yields a truth in the context of an <u>ad lib</u> whilst reading Shakespeare; yet the assertion is false of any actual, "existent" item if we enforce rigid ontological criteria. Hopefully, then, little harm is done in supposing such assertions are <u>quantified</u> and subject to the recursion which goes through virtually unchanged from II, C (p. 448):

$$v((\exists x)\emptyset)w_{ab} = 1 \text{ iff } (i) w_{ab}(t_0) \neq (\Lambda, \Lambda) \text{ for } t_0 \in \underline{\text{Term}} \text{ and}$$

$$(ii) v'(\emptyset[x/t_0]) = 1 \text{ for some } \langle v', w'_{ab} \rangle$$

$$\approx x \langle v, w_{ab} \rangle \text{ on } M^*$$

Otherwise, $v((\exists x)\phi)w_{ab} = 0$.

Requirements upon cgm's:

As before, a cgm M* is a quintuple $\langle M, S, C^*, F, f_{\underline{P}} \rangle$ such that

conditions 1) - 5) are satisfied and, in addition:

(6) if S is a set of partial models with domain M (= $\langle D, R, \underline{Ab} \rangle$), then S is a <u>chain under</u> ' \leq p' if ' \leq p' is a reflexive, antisymmetric, transitive relation on S which is defined by the following recursion:

(i) for each atomic monadic swff $\emptyset x$, aboutness-assignment w_{ab} on M and R'_{M} - assignment of subsets of M to predicates of CS-1; if $w_{ab}(\emptyset x) = (x_1, x_2)_2$ then:

if $x_1 \in R'(\emptyset)$, then $T(\emptyset x_1) \leq p F(\neg \emptyset x_2)$ and if $x_2 \in \overline{R'(\emptyset)}$, then $T(\neg \emptyset x_2) \leq p F(\emptyset x)$.

(ii) for each \ll , $\beta \in \underline{St} \times \underline{Ab}$ such that for <u>no</u> swffs \emptyset , ψ , relative to $\langle v_s, w_{ab} \rangle$ on M does $qu(\emptyset)(\theta_M) \forall \propto$, $qu(\psi)(\theta_M) \forall \beta$ hold:

(a)
$$T(\neg \alpha) \leq_{p} F(\neg \alpha)$$
 iff $\alpha \notin S$
(b) $T(\alpha \lor \beta) \leq_{p} F(\alpha \lor \beta)$ iff $\alpha \notin S$ and $\beta \notin S$
(c) $T(\alpha \land \beta) \leq_{p} F(\alpha \land \beta)$ iff $\alpha \notin S$ or $\beta \notin S$
(d) $T(\alpha \rightarrow \beta) \leq_{p} F(\alpha \rightarrow \beta)$ iff $\alpha \notin S$ or $\beta \notin S$

Together, conditions (i) and (ii) on $' \leq p'$ ensure that the chains of S under $' \leq p'$ are \neg -consistent.

(7) A subset S' of chain S with same domain as M is a <u>maximal chain</u> in S if (i) S' is a chain under $' \leq p'$ and (ii) for any M' \mathcal{E} S - S', S $\cup \{M'\}$ is not a chain.

The union of a chain S of partial models is the model $\langle D, R_{\infty}^{m} \rangle$ where, for each prediate Øi,

$$\frac{R_{\infty}^{m}(\phi_{i}) = \bigcup R_{n}^{i}(\phi_{i})}{\langle \mathbf{p}, \mathbf{r}_{i}^{*} \rangle \epsilon s}$$

$$\overline{R_{\infty}^{m}(\phi_{i})} = \bigcup \overline{R_{n}^{i}(\phi_{i})}.$$

$$\langle \mathbf{p}, \mathbf{r}_{i}^{*} \rangle \epsilon s$$

Provided that D is denumerable, (b) (i), (ii) and (7) ensure that the union of each maximal chain in S is complete (see Van Fraassen, <u>1971</u>, p. 186). Notice that the probability measure $f'_{\underline{}}$ will be well-defined only if partial models of CS-1 have finite or denumerable domains. This may be

problematic in so far as the likelihood of some value set being forced eventually in some chain of sets of value-sets (increasingly closer to complete models at the limit-union) will be given by discrete values in [0, 1]. In a <u>finite</u> domain, the set of probability-values assigned by f_{\leq} always adds up to 1, for the completion of the ground model. But, in a denumerable domain, the set of values <u>approaches</u> 1 as membership in S' approaches the closure of <u>St</u> x <u>Ab</u>. Finally, in a <u>non-denumerable</u> domain, it is not clear whether f_{\leq} -values approach either 0 or 1--whence it is not certain that CS-1 possesses any intuitively correct models, owing to lack of closure conditions upon ' \leq p'. Yet non-denumerable models introduce special problems (in the absence of a well-ordering of the continuum) which need not concern us here.

An example from a <u>finite</u> domain illustrates the function of ' \leq_p ' in relation to ' f_{\leq} '. [The example, using matrices for ' \leq_p ' and the probability-measure is taken from W. Hodges, <u>Logic</u>, Penguin, <u>1977</u>, p. 238]. Suppose neither qu(po), qu(qo), nor qu(ro) is statement-yielding in the ground-model M. Relative to a measure (here chosen arbitrarily) of the distribution of values in completions of M, how are we to evaluate whether, if qu(po) yields a truth in some completion of M, then qu(qo) is eventually just as likely to yield a truth as qu(r_o) in those completions (i.e. if $T(\S_{Po})$ is forced in S, then $T(\S_{Po}) \leq_p T(\S_{Po})$?

Accordingly, we begin with a measure, fixed by $f_{\underline{c}}$, for the possible distribution of value sets (relative to $\langle v, w_{ab} \rangle$ on S) in the chain determined by AN(M,S). This can be given by a matrix:

| | | ł | Measure | 633 |
|-----|------|------|-----------|------------------------------|
| 500 | 5900 | 50 | fe-values | |
| 1 | 1 | 1 | - 2 | |
| 1 | 1 | 0 | • 1 | |
| 1 | 0 | 1 | • 2_ | |
| 1 | 0 | 0 | 0 | |
| D | 1 | 1 | •1 | |
| 0 | 1 | 0 | D | These <i>i_</i> -values are, |
| 0 | 0 | 1 | •1 | chosen for atomic |
| 0 | 0 | 0 | •3 | sentential swff of |
| | | Tota | 1 1.0 | US-1. |

Since we are supposing that $T(\S p_0)$ is forced in some completion of M, we need only calculate the value of $T(\S q_0) \leq p T(\S r_0)$ on the assumption that $T(\S p_0)$ is forced already; i.e. the value of $'T(\S p_0) \rightarrow (T(\S q_0) \leq p$ $T(\S r_0)'$ which is calculated in the following matrix

| T(3P0) | Seve | Src | $T(\mathfrak{P}_{\mathcal{P}_{0}}) \to \left[T(\mathfrak{P}_{\mathfrak{P}_{0}}) \leq_{\mathcal{P}} T(\mathfrak{P}_{r_{0}}) \right]$ | | | | | |
|----------|------|-----|--|---|---|-----|---|--|
| 1 | 1 | 1 | 1 | 0 | 1 | 0 | 1 | |
| 1 | 1 | 0 | 1 | 0 | 1 | 0 | 0 | |
| 1 | D | 1 | 1 | 0 | 0 | 0 | 1 | |
| 1 | 0 | D | 1 | 0 | 0 | 0 | 0 | |
| . | | | | | | (*) | | |

Column (*) is calculated by adding up the fraction of the total distribution in which $T(\S q_0)$ is forced, according to the f_{ξ} -measure, and comparing it with the same fraction for $T(\S r_0)$. For $T(\S q_0)$, this comes to $\cdot 2 + \cdot 1 + \cdot 1 + 0 = \cdot 4$ and, for $T(\S r_0)$, .2 + .2 + .1 + .1 = .6. Since $T(\S q_0)$ is forced in fewer completions than $T(\S r_0)$, $T(\S q_0) \leq p T(\S r_0)$ does not hold. This is repeated for each row , and the value of $T(\S p_0)$

 \rightarrow [T(\Sq_0) \leq p T(\Sr_0)] calculated in terms of the regular matrix for entailment. Since <u>all</u> the values are '0', it follows that, even when T(\Sp_0) is forced in some extension of M, the likelihood of T($\$q_0$) being forced, when T($\r_0) is, is nil. Thus, the statement-success of qu(p_0) in some context, for this example, does not affect the statement capability of qu(q_0), qu(r_0) [though separate matrices would be required for the likelihood that T($\$r_0$) or F($\r_0) is forced relative to the likelihood of T($\$q_0$) or F($\q_0), alone]. The result is hardly surprising-reflecting, as it does, that, without further analysis, qu(p_0), qu(q_0) and qu(r_0) are independent atomic swffs.

Finally, it is necessary to describe how C* functions in M*, to delimit contexts for each interpreted swff. The domain of C* is to be a set of contexts <u>qua</u> combination of circumstances $\{C_1, C_2, \ldots, C_n\}$ -where each circumstance (as in II, C) is a non-empty set of complexes. The less value-sets from <u>St</u> x <u>Ab</u> appear in a context, the less "specific" the context is with respect to the aboutness and statement-conditions of an utterance within that context. The essential purpose of the function C*, then, relative to AN(M,S), is to select which value-sets in the chain S of subsets are added to a ground-context to determine the aboutness and statement-conditions for an utterance.

Thus, C* is a function from partial models to partial models whose range is the set of pairs $\langle M', S' \rangle$ where, (i) $M' \in S'$; (ii) AN(M',S')(iii) the union of maximal chains in S' is complete (which ensures the \neg -consistency of each new context accessible from M. Thus, let (M1 θ) be the set of circumstances (sets of value-sets in <u>St</u> x <u>ab</u>) restricted to a partial model M. Then, $C^*(M1\theta) = (S1\theta')$ provided that:

Df. AN: AN(M,S) is a partial ordering over partial models $M = \langle D, R, Ab \rangle$ which is closed under truth-functions, '∠p' and the following conditions: 1) for any \propto , $\beta \in St \times Ab$ such that if (a) $\propto \notin (M1\theta)$ (b) if $\beta \in (M1\theta)$, then (\propto/β) $\notin (M1\theta)$ then (i) if $T(\beta \rightarrow \alpha) \in (M1\theta)$, then AN(M $\cup \{\beta\}, S \cup \{\alpha\}\}$) (ii) if $\beta i \ll \alpha$, then AN(M $\cup \{\beta\}, S \cup \{\alpha\}\}$) (iii) for any A \in G, if T(A) \in M, then AN(M, S $\cup \{\Delta_A\}$)--where $\{\Delta_A\}$ is the set of all value-sets for Δ_A predications in St x Ab.

This completes the formal description of contextually graded models for CS-1, and, thus, concludes this section. In Section E, I will outline several of the problems left open for CS-1 and, by working through examples, show how CS-1 might be extended to accommodate syntactically more complex swffs than the monadic predications and truth-functions thereof in CS-1. In concluding the thesis, I will extrapolate from the separate themes which have been discussed, both in formal and informal terms above, in order to examine once more the relationship between categories, types and significance.

Section E: Towards CS-2; Conclusions:

C5-1 is an interpreted first-order formal language which is intended to explicate a framework in which statement-failing and nonsignificant monadic predications in context fail to yield statements owing to incompatibilities between their aboutness- and statement-conditions. Its contextually graded interpretations provide for a measure of the content expressed by an utterance in terms of the entailments and presuppositions of the statement-conditions upon the utterance across a range of contexts construed as partial models. For all the complexity of the semantic modelling of CS-1, it lacks application, in a number of ways, to kinds of formulae which create special problems for significance logic.

I propose in this final section to examine informally several of the open problems for CS-1 and to anticipate ways in which CS-1 might be extended to CS-2 to provide some resolution of these problems. In conclusion, I will try to extrapolate from the main arguments of the thesis to consider the philosophical importance of a systematic examination of category-mistakes, in relation to type and sortal predications. After detailed exegesis and interpretation, argument and counter-argument, proofs and definitions of such length, my conclusions will be brief. Of necessity, the application of formal techniques to philosophical problems, extrapolation from results achieved in a rigorously delimited framework, must be tentative. The results concerned require careful focus to avoid both oversimplification and the tyranny of methodology in formal investigations.

Nonetheless, I believe that the positions for which I have argued in this thesis cohere to form an approach to significance-in-context which is recommended over others by its fidelity to inferences and claims we make in judging and appraising the significance of one another's speech acts.

(I) Open Problems for CS-1: 1) Relations:

The most obvious omission from the semantics of CS-1 is the absence of any consideration of utterances which are not subjectpredicate in form, but relational. Relational assertions, or n-adic predications (for $n \ge 2$) create complex problems of interpretation especially for the determination of their aboutness. There is a clear sense in which, though it is category-mistaken, the simple subject-predicate assertion qu(Virtue is blue) has a definite aboutness—virtue-- and predicates qu(x is blue) of it. In addition, both subject-term and predicate are themselves free from anomaly--it is the <u>coupling</u> of subject with predicate which generates significance-failure. (This is not true of, say, qu(The river is a brown god), qu(She smiled deductively) or qu(This stone's aunt is supercilious) which require separate treatment for nonsignificant complex predications, adverbial modifiers, or complex subject-terms).

In contrast to a simple subject-predicate assertion, a category mistaken relational assertion provokes its own issues of interpretation:

(1) qu(John frightens sincerity) (Chomsky's example) is a dyadic predication of the form $'R(\underline{a}, \underline{b})'$. The question arises
whether the nonsignificance of (1) should be attributed solely to the coupling of $\langle John, sincerity \rangle$ by the relation qu(x frightens y); or rather that the assertion of qu(x frightens sincerity) of <u>any</u> aboutness item generates the category-mistake.

The first alternative seems mistaken. It is not the presence of the pair $\langle John, sincerity \rangle$ in the extension of a relation which alone generates significance-failure. The same pair may appear significantly in the extension of any number of relations qu(x admires y), qu(x contemplates y), or even qu(x is frightened by y).

On the other hand, to treat (3) as nonsignificant because John is not of a sort of which qu(x frightens sincerity) may be asserted is simply misleading. Reducing the dyadic predication to a monadic preciation makes it appear both as if qu(John) has definite aboutness-conditions and qu(xfrightens sincerity) has definite significance-conditions, and it is the incompatibility of these conditions which leads to nonsignificance. The category-mistaken nature of (3), however, results from the <u>lack</u> of significance-conditions for qu(x frightens sincerity). Thus, the reduction to a monadic predication is unhelpful.

In consequence, (3) must be analysed as dyadic: qu(x frightens y) is asserted of two items which (3) is about; and the nonsignificance of (3) stems from the coupling qu(John) [term] with qu(x frightens y) [dyadic predicate] and qu(sincerity) [term], in that order. Thus, a proper analysis for (3); and, in general, for n-adic predications, should assign ordered n-tuples of aboutness items to the extension of a relation, to each member of which aboutness-conditions attach and for which significanceconditions are <u>indexed</u> to the position of the item in the ordering determined by the form of the relation.

Clearly, this extension of the aboutness-assignments for n-adic predications will involve much greater complexity than the w_{ab}-assignments on <u>Ab</u> of CS-1's models. Just such a treatment of a related, but differently defined notion of aboutness is available in Routley and Goddard (<u>1973</u>, pp. 172-191), I will not repeat it here--though it rests upon the introduction of ranked vector-sets for subject-items, ordered in accordance with the standard (Wiener-Kuratowski) definitions of ordered ntuples: $\langle a,b \rangle = \{\{a\}, \{a,b\}\}, \langle a,b,c \rangle = \langle \langle a,b \rangle, c \rangle$ and so on.

A second question which arises in extending CS-1 to CS-2 to accommodate relational assertions concerns whether the semantic framework can reflect differences in kind between relations based upon the significance of assertions resulting from application of operations from the algebra of relations. For, relations R differ inrespect of whether their composition, converse, iteration, image or resultant is significant, whenever qu(xRy) is significant.

L. Goddard (<u>1966</u>, pp. 154-162) has offered the following valuable classification of relations as a first step to reflecting these differences in a semantics for significance logic.

The standard treatment of relations in classical first-order logic (derived from PM) makes the assumption that every (dyadic) relational assertion about two items has a significant converse, if it is significant. This follows from the implicit acceptance of the doctrine of complete definition in classical logic -- whereby <u>all</u> individuals are permissible arguments in relational assertions: hence, the domain D_R and conv erse domain D_R^{\cup} of a relation qu(xRy) coincide in exhausting the totality of items $(D_R^{\cup} D_R^{\cup} = D)$. Ordinarily, however, there is a primary difference between relations which, for the <u>restricted</u> significance-field $S_R^{\cup} S_R^{\cup}$ of items, over which they are significant, satisfy the condition that <u>arbitrary</u> choices of pairs $\langle a, b \rangle$ from $S_R^{\cup} S_R^{\cup}$ ensure that qu(aRb) is significant, and those relations which do not satisfy this condition. (Note that S_R^{-} , S_R^{-} themselves comprise unions, respectively, of the items for which qu(xRy), qu(xRy) are true or false).

Goddard (loc.cit.,p. 156) calls relations satisfying the above condition homogeneous, otherwise heterogeneous. In particular:

For any dyadic relation qu(xRy), let S_R be the subset of D such that: Sig(qu((Py)aRy)) iff $a \in S_R$; and let $S_R = be$ the subset of D such that: Sig(qu((Px)xRb)) iff $b \in S_R = be$ (I am supposing for the moment that problems over the significance ranges of P-quantified variables are settled).

> Then: qu(xRy) is <u>homogeneous</u> if, for any $\langle a, b \rangle \subseteq S_R \cup S_R$, Sig(qu(aRb)). qu(xRy) is <u>heterogeneous</u> if qu(xRy) is not homogeneous.

An obvious example of a heterogeneous relation, given what has gone before, is the relation 'E' (in the sense of Russell) contextually defoned in PM (*20), within the theory of types. If I is a set of individuals, each elementary function (or matrix) satisfied only by members of I determines a class belonging to type-1 of the heirarchy. The totality of functions containing at least one bound variable ranging over I determines a class of type-1 classes belonging, thus, to type-2, and so on. Hence, for any assertion of the form $qu(x \xi y)$, by the typetheoretic restrictions, $Sig(qu(x \xi y))$ holds only for pairs $\langle a,b \rangle \subseteq S_{\xi} \cup S_{\xi}$ such that a is of type one less than b. Indeed, the purpose of type theory, as explicated in Part I, is to rule out as nonsignificant homogeneous membership-assertions: e.g. $qu(c \xi d)$, when c is of type $\geq d$.

A second example of a heterogeneous relation, again obvious in this context, is 'qu(-) is about ... ' when the level-restrictions of Appendix (B) (IV) are imposed upon the substitution ranges of 'qu(-)'. For Sig[qu[qu(ϕ) is about <u>a</u>)] holds only provided that $w_{ab}(\phi)$ assigns a subset of Ab of level at least one greater than the set of quexpressions of CS-1 (c.f. Appendix (B), pp. xxxii-xxxiii). Thus if S about forms a set \underline{Qu} of qu-expressions in the base-level of the level-heirarchy of <u>Ab</u>, and <u>a</u>, <u>b</u> \in S_{about} U S \bigcup_{about} , then for every $\langle a,b \rangle \subseteq S_{about} \cup S_{about}, \neg Sig(qu(\underline{a} \text{ is about } \underline{b}))$ and 'aboutness' is heterogeneous. Again, the heterogeneity of 'aboutness' with respect to the levels of vocabulary in CS-1 formed by 'qu(-)', is necessary to avoid paradoxes of ungrounded assertion discussed in Appendix (B), (pp. xxxviiiff.). The major examples of these paradoxes (the Liar Paradox, Grelling's paradox) led Russell, in part, to adopt the ramified theory of types (of which the account of the level-heirarchy of Ab for CS-1 is a distant relative) and prompted Tarski to generate the heirarchy of object and meta-languages required for a formal

definition of truth.

Amongst homogeneous relations, Goddard distinguishes four kinds:

(1) <u>Family relations</u>: those relations for which $S_R = S_Q(e.g. in$ the extension of the general term 'Person', family relations are $qu(\underline{a} \text{ is } \underline{b}$'s brother), $qu(\underline{a} \text{ is ancestor of } \underline{b})$, $qu(\underline{a} \text{ is less human than } \underline{b})$, etc. Some of these relations are also family relations in the extensions of other general terms which restrict a common term with 'Person').

(2) <u>Nest relations</u>: those relations for which $S_R \subset S_R$ or $S_R \subset S_R$. Thus, if qu(xRy) is a nest relation, then, if Sig(qu(aRb)), qu(bRa) is sometimes significant, sometimes not. Qu(bRa) will only be significant for items from the common part of S_R , S_R^{\cup} — often determined by some general term covering <u>a</u>, <u>b</u>; i.e. Sig(qu(bRa)) if Sig[qu(aRb)] and $\langle b, a \rangle$ $\subseteq (S_R \cap S_R) \subseteq \{\langle a, b \rangle : a, b \in R_M^{\Theta}(A)\}$, for some R_M^{Θ} -assigned extension on M, relative to context Θ , for a general term A under which both <u>a</u>, <u>b</u> fall. Otherwise, \neg Sig[qu(bRa)].

Such relations form a "nest" in that their extensions form inner and outer significance domains according as $S_R \subset S_R \text{ or } S_R \subset S_R$. An example might be any relation which, say, includes only persons in S_R and material objects in S_R . People stand in various relations to material objects ('trip over', 'fall beneath', 'decompose into', etc.), yet not every predication significant over people is significant over material objects ('thinks of', 'is maternal aunt to') whereas any relation significant over <u>all</u> material objects ('weighs more than', 'gets in the way of') is significant over people. In other words, $S_R \cup S_R$ forms a nest.

(3) <u>Categorial relations</u>: a relation is categorial if $S_R \cap S_{\tilde{R}} = A$, i.e. if <u>a</u> is any item in S_R then $\neg Sig[qu(a\tilde{R}b)]$ for any <u>b</u> in $S_{\tilde{R}}$ and $\neg Sig[qu(bRa)]$. Examples of categorial relations are characteristically intensional dyadic predications (if, indeed, these are "relational" in significance): 'believes that', 'knows how to'; and also the ' ε ' of simple type theory. (4) <u>Partially categorial relations</u>: Goddard (loc.cit.,p.160) lists four conditions for this complex species of relation. The conditions entail that, in effect, the significance-field $S_{\mathbb{R}} \cup S_{\mathbb{R}}$ of such relations consists of properly overlapping families of items satisfying significance conditions for qu(xRy), qu(xRy); i.e. neither is wholly contained in the other. Thus, a partially categorial relation is:

- a) a family relation over $S_R \cap S_R^{o}$,
- b) a nest relation over $(S_R \cap S_R) \cup S_R$,
- c) a nest relation over $(S_R \cap S_R) \cup S_R^{\upsilon}$,
- d) a categorial relation over $[S_R (S_R \cap S_R)] \cup [S_R (S_R \cap S_R)].$

An example of a partially categorial relation (from Routley, <u>1966</u>, p. 207) can be taken from a Cartesian theory of the mind/body relation; i.e. any relation between rational (embodied and disembodied) spirits as the significance domain and extended objects as the significance conversedomain. Since it is always significant, at least, to talk <u>about</u> both embodied and disembodied spirits, then, for example, qu(x contemplates y) will be partially categorial in this domain. For:

a) the common part $S_R \cap S_R \circ S_R$ comprises persons contemplating each other (qua extended objects),

b) the nest over $[(S_R \cap S_{\breve{R}}) \cup S_R]$ comprises persons who <u>also</u> contemplate the deceased (disembodied),

c) the nest over $[(S_R \cap S_R) \cup S_R]$ comprises embodied persons contemplating the Taj Mahal or a grain of sand, etc.,

d) the categorial relation comprises the deceased in eternal contemplation of the 'impersonal' material world to which they no longer belong.

Goddard's classification suffices to indicate some of the complexity involved in accommodating relations in CS-2. Yet, to the extent that the classification is, itself, described in terms of semantic resources many of which are available already in CS-1, then it is to be hoped that further development along these lines should contribute to the classical algebra of relations, through its finer articulation in significance logic.

2) Complex subjects:

If the absence of relations from CS-1 is a glaring omission, full treatment of the aboutness of complex subject terms is a less pressing need. For, the definitions and explications of significance conditions based upon aboutness-assignments in context under covering general terms are themselves sufficiently general to transfer to a treatment of complex subject terms in CS-2.

The syntax of CS-2, however, has to be enriched to include a definite description operator (perhaps defined: $'(\iota x)\emptyset x'$ --read "the \emptyset "), an indefinite description operator $'(\iota x)\emptyset x'$ --read "a \emptyset ", a choice-operator $'(\zeta x)\emptyset x'$ --read "any particular \emptyset ", and abstraction-operators $'(\lambda x)\emptyset x'$ --read " \emptyset -ness", and '{ : \emptyset ...}' for "the class of \emptyset 's", in the formation-rules for swffs. These are needed to express category-mistaken assertions whose nonsignificance derives from the manner in which the aboutness item is picked out; e.g.:

- a) qu(The nothing which is active negates) = $qu(Neg(\iota_X)(Nx \& Ax))$.
- b) qu(A venemous violin vibrates) = qu(Vib([x)[Violx (Venx]).
- c) qu(Tom swallows any (one) of your tall stories) = $qu(Sw[t, (\zeta x))$
- d) $qu(Triangularity exemplifies John) = qu(Ex[(\lambda x)(\Delta x),j]).$

Though there are certainly many issues of detailed analysis requiring answers for the semantic interpretation of these operators, much of the essential syntactic development has already been described in Routley and Goddard (<u>1973</u>, especially pp. 151-172). I shall not reduplicate their efforts, here.

Sx (Tx]).

3) Remaining questions for the development of CS-1 into a full significance logic CS-2 concern, in the main, the merging of existing logical theories with the significance logic framework. For example, CS-1 lacks both quantification over predicates (of second and higher order) and abstraction principles for a formulation of set theory and arithmetic in CS-2. Again, the relevant syntactical outline is available from Routley and Goddard (1973, sections 7.2 - 7.13), though the divergence between my semantic approach and their anticipated significance-range theory (in Vol. II of 1973, forthcoming) may preclude strict parallels in development. Nonetheless, it is beyond my present scope even to foresee how strongly extensional requirements for arithmetic and set theory are to be reconciled with the intensional structures of CS-1's contextually-graded models. Trivially, and uninterestingly, some formulation of ZF-set theory could be simply appended to CS-1 and interpreted only in the set of all classical completions of partial models M* of CS-1. Such a development, however, would reveal nothing of the interaction between contextually relative significance in natural languages and the theoretical structure of number theory and mathematics.

Finally, I have introduced, but not explained in full, the means of <u>restricting</u> quantified swffs in CS-1 by means of the matrix-defined connective $\mathbf{4'}$. This is certainly not the only means in which models for restricted quantification may be defined for a formal language. Other methods are described in Leblanc (<u>1973</u>), Hintikka (<u>1963</u>), and Stevenson (<u>1975</u>, pp. 185-207). Adoption of '**1**' into CS-2 is recommended only by its simplicity - a simplicity that could certainly be overturned by the increasing complexity of detail needed to resolve these open problems.

Conclusions: Types, Categories and Significance:

"The substance of the world <u>can</u> only determine a form, and not any material properties...substance is what subsists independently of what is the case". (Wittgenstein, Tractatus, 2.0231-2.024)

"Essence is expressed by grammar.... Grammar tells what kind of object anything is". (Wittgenstein, <u>Investiga</u>tions, §§ 371-3).

These epigrams from Wittgenstein are paradoxical and deliberately provocative. Their seeming paradoxicality is the same: if the "substance" of the world is constituted by the kinds of thing there are in it, how can this substantive make-up of the world determine only a form and not the actual, matter-of-fact properties things have? And, if "substance" belongs to the world, the most general descriptions of kinds of thing in it, how can it subsist independently of what is the case? Finally, if the <u>essence</u> of something is its nature---its being of a sort or kind--how can <u>grammar</u>, which pertains only to how we talk about things, to our descriptions in language, express the natures of things and tell what kinds of thing there are?

For all their air of paradox, these remarks express several of the themes for which I have argued through this thesis. They encapsulate the transitions I have made: from my concern to reject Frege's doctrine of complete definition, through the exegesis of Russell's gradual recognition that the meaning of a propositional function (predicate) is not determinate unless the range of items which are candidates for satisfying it is specified. This is one form of the Vicious Circle Principle. From this recognition, Russell developed a solution to the logical and semantic paradoxes which requires predicates to divide into a ramified heirarchy, according to the levels of abstraction at which they are asserted, or judged, of items; and to the types of item of which they can be asserted.

The remarks lead also to Wittgenstein's own criticisms of Russell's account of propositional meaning, of the ramified theory and of the epistemology upon which it was based. For, the remarks <u>deny</u> that what belongs to logic--form--requires any special view of the world to underpin it; nor any epistemological account as to how items are judged or perceived. They lead, thus, to the rigid separation of <u>form</u> from <u>content</u> which was Wittgenstein's own explanation of what a type theory tries (and fails) to say: that <u>formal</u> properties, determined by substance, <u>can</u> only be shown <u>in</u> what we say, and not described <u>by</u> what we say. For, formal properties belong to what is common to world and description-logical form--and, thus, are presupposed by both. To cap epigram with epigram, one might echo a radically different view and summarise the doctrine of showing in the claim: "though existence precedes essence, grammar precedes both."

Finally, in these remarks, also, are to be found, at some remove, the connecting links between the predominantly historical tone of my examination of the source and development of type theory in Part I, and the systematic efforts of Part II to provide a formal analysis of contextual significance which explicates the failure of category-mistaken predications to yield true or false statements in context. The links are themselves both historical and systematic. As the success of the

symbolic language of PM led the logical positivists to seek an unification of scientific theories in a common, rigorous ideography--so their stringent verifiability criterion of meaningfulness prompted the rejection of metaphysical assertions as pseudo-statements, cognitively meaningless and, in some instances, as category-mistaken.

If assertions are to be appraised as meaningless or nonsignificant, whether they purport to state metaphysical truths or simple descriptive claims, a systematic requirement is to analyse this concept of nonsignificance and to evaluate the procedures involved in its appraisal. In particular, if type-theoretical restrictions upon the meaningfulness of combinations of symbols in the ideography of PM are to be transferred to a perspicuous language for science--an "ideal language"--then the relationship between type theory and significance has to be explained. The link, again, is provided in Wittgenstein's insistance that a correct system of notation--a language--already possesses a formal structure which differentiates between what has sense (what can be said) and what does not (what is shown; or is <u>Sinnlöss</u>). It is this distinction which prompts the positivists to seek canonical forms to replace the vagaries and ambiguities of ordinary language.

The link between significance and type-theory, as I argued through I: C, D, could never be made so simply as to fulfill the positivist's demand for a rigorous, ideal language. Indeed, it required misinterpretations both of Russell's conception of type-theory and Wittgenstein's conception of "correct symbolism" to suppose that it could. It was not until the discussion of Ryle's use of type-theory, as a theory of

meaningful predication--inadequate as the theory proved to be--that a link more faithful to how we, as language-users, appraise significance in relation to the couplings of subject-terms with predicates, could be forged between type-theory and the nonsignificance of category-mistakes.

What, then, is the relation between <u>types</u>, construed broadly along Russellian lines, and the significance-failure of category-mistakes? To answer this, I return to Wittgenstein's distinction between 'formal' and 'material' properties.

In the genesis of Wittgenstein's Tractarian views (in the Notebooks, 1914-1916 and in letters to Russell), Wittgenstein frequently uses the expressions 'formal' and 'logical' interchangeably. What makes up the theory of types, which Wittgenstein certainly took to be a theory belonging to logic (i.e. PM), are assertions that 'individuals', 'functions', 'properties', 'relations', 'propositions', 'facts' form a heirarchy of 'types'. It is Wittgenstein's criticism that these terms cannot even be used to ascribe features to items; since, as predications, either they are typically ambiguous when asserted of items in different types (whence, their assertion yields nothing definite) or they are redundant (they express what is already shown in the form of the notation for their arguments.) For example, qu(a is an individual of type-0) appears to ascribe some feature to a, whereas it is properly to assert something about the logic of assertions ascribing features to a (namely, that it is nonsignificant to predicate $qu(\underline{a})$ of items of higher type). Properties exhibited in asserting logical features: ' ξ is an individual', ' ξ is a type', '§ is a property', '§ is a relation' are formal properties.

And formal properties are <u>shown</u> in the manner a sign is used (with sense), they are not expressed by assertions we make <u>about</u> individuals, properties, facts, relations, and so on.

What is distinctive about expressions for 'formal' properties, or about the 'formal', as opposed to 'material' concepts they express? The first point to notice is that whatever is distinctive of formal concepts, in Wittgenstein's sense, will be distinctive also of type-theoretic concepts like 'individual', 'function', 'complex', 'relation' and 'type'. For, these terms are amongst the examples Wittgenstein gives:

So one cannot say, for example, 'There are objects' as one might say 'There are books'.../... The same applies to the words 'complex', 'fact', 'function', 'number' etc.../ They all signify formal concepts. (<u>T</u>. 4.1272; see also 4.126, 4.1273).

'Complex' is Russell's term for the basic object corresponding to elementary judgements of perception, and his term for the multiply-relational form of the discursive judgement 'A judges that aRb' (PM, pp. 43-44). The judgement-complex comprises A standing in relations to <u>a</u>, <u>R</u>, <u>b</u> and the form of the complex 'x Ø y' (in the 1913 version of the theory). An elementary judgement is <u>true</u> if there <u>is</u> a perceived complex corresponding to an arrangement of that form ('aRb'), false if not. Similarly, the other words which, for Wittgenstein, signify 'formal' concepts, belong, for Russell, to the genesis of type-theory--the explanation of the source of the logical and semantic paradoxes in illicit, impredicative classifications. The following passage makes this parallel striking:

the appearance of contradiction is produced by the presence of some word which has systematic ambiguity of type, such as 'truth', 'falsehood', 'function', 'property', 'class', 'relation', 'cardinal', 'ordinal', 'name', 'definition'. Any such word, if its typical ambiguity is overlooked will...apparently generate a totality containing members defined in terms of itself... (PM. p. 64).

Russell's claim, thus, is that 'type' words are ambiguous as to level and order. The risk in using them is that of generating viciouscircle fallacies. Wittgenstein's claim is that such words can express nothing: <u>either</u> they are redundant, since what they purport to express about an item is already shown in the symbolic use to which a sign is put; <u>or</u> they risk our asserting of <u>items</u> what, in fact, is not true of items but of the manner in which we represent them through our pictorial use of signs.

Both claims suggest, thus, that there is a considerable risk to be run in trying to say anything very much about formal concepts. Accepting this risk, we can enquire further into what is distinctive about them.

Russell explained the notion of a 'type' via that of a 'range of significance':

A <u>type</u> is defined as the range of significance of a propositional function, i.e. as the collection of arguments for which the said function has values. (Russell, <u>1908</u>, p. 74).

Two rather obvious points emerge: placing items according to type (in a heirarchy) is a process of collecting items together; i.e. types are a distinctive kind of general classification. Secondly, the classifying of items by type is marked by the <u>significance</u> of what can be asserted of them (not simply the <u>truth</u> of what is asserted). Thus, if a type-concept is a formal concept, a mark of the 'formal/material' distinction appears to be that an assertion as to an item's type is made on the basis of classification according to the significance of further assertions about that item. Indeed, in view of the discussion of Pap's account of "type-predication" (in I, D, pp. 302-4), we can mark the distinction more perspicuously: to <u>predicate</u> a formal concept of an item is to assert something which, if significant, is true, otherwise nonsignificant. Assertions like $qu(\underline{a} \text{ is an individual}), qu(\emptyset x \text{ is a function}),$ qu(n is a number) have the property that their significance, by the canons of type theory, guarantees their truth. The expressions substituted for the "gap-signs" in qu(() is an individual), qu(() is a property) must be respectively, an individual name and a function expression. Hence, they are true, if significant--since any other kind of substituend violates significance-conditions; c.f. qu((Being old)) is an individual), qu(Johnis a function), qu(Socrates is a number).

It is this <u>mark</u> of the 'formal'/type-predications which lead Wittgenstein to the claim that such assertions do not express any thing true or false (thus, descriptively meaningful). They do <u>not</u> state properties of items. On the contrary, not stating anything true or false of subjects type-predications express only what is true of the manner in which we talk <u>about</u> separate subject-matters. That is, they express what is already evident if we have a notation which is adequate for distinctions of type: that-qu(<u>a</u>) is an individual-name, that-qu(F(x)) is a function sign of order-1 (contains a gap which can be filled by individual names), and so on.

In sum, the distinction 'formal/material' is intended to mark the difference between what is true of the manner in which an item is talked <u>about</u> and what is genuinely true of the item, itself. They reflect a partitioning made on the basis of what it is significant to say (formal concepts) <u>versus</u> a partitioning made on the basis of what it is true or false to say (material concepts).

Herein lies the motivation for the separation, in Part II (Section B) between what is expressed by a statement-making utterance in context, and the illocutionary success of the act performed; that some statement is yielded. My conclusion works towards justifying this separation in terms of the difference between classifying (or 'categorising') according to <u>types</u>, on the basis of the significance of our utterances; and classifying (or 'sorting') according to <u>kinds</u>, in virtue of the truth or falsity of our statements.

Type-predications, as I have maintained, are distinctive in being true of every item of which they can be significantly predicated. Expressing the matter thus, however, only adds to the mystery of why some classificatory predications have this property and others do not. Something of the mystery, but not enough, is removed by attending to the difference between truths which obtain because of the manner in which items are <u>featured</u> in assertions, and truths which are consequencial upon features <u>of</u> the items themselves.. But how well-founded is this distinction? For, it seems to run counter to a principle in explanations of truth which has the veneration of Aristotle:

The true proposition, however, is in no way the cause of the being of the man, but the fact of the man's being does somehow seem to be the cause of the truth of the proposition on the fact of the man's being or not being. (Aristotle, Metaphysics, I, 1011b, 25).

Am I opposing Aristotle, thus, in claiming that some true assertions, at least, are the 'cause of their own being', in a sense, in so far as, unless they are true, nothing is asserted (no proposition is expressed) by a false type-predication?

My answer, of course, is that there is no genuine opposition. What is true in virtue of the significance of utterances is what is required for us to be able to make true or false statements at all in the utterance of declarative tokens in context. These truths are the 'cause of the being' of statements only to the extent that, in order for our constative speech acts to be successful, in a context, we must be capable of expressing our truth-claims in a form of words significant to others. That grass is green is not a true statement <u>because</u> qu(grass) can be used to designate a phylum of plants of jointed stem and narrow leaves, any more than it is true <u>because</u> it is significant to predicate qu(x is green) of any material stuff (grass included). That-grass is green is true, so I am told, is <u>because</u> that form of herbage has an atomic structure which reflects statistically sufficient radiant energy in the wavelengths 3900A to 7700A to stimulate the human retina--though I find the choice of the preposition "because" odd in such a context.

On the other hand, for us to make a true or false statement in uttering qu(grass is green) requires the utterance to be determinately about something of which colour can be significantly predicated. This

much is quite obvious; but the next step may not be so.

To predicate a type of an item, I have claimed, is a form of classifying. Not all forms of predication are acts of classifying--though some which are, are not type-predications. A type-predicate does not state something true or false of the item it is about, it states something true of the manner <u>in which</u> we talk about the item. This does not hold simply because assertions as to type have a classificatory role. Other assertions, also having that role, do yield truths which, to all appearances, hold of the items themselves--though not in the simple and direct sense in which a colour predication ascribes a property to a spatiotemporally locatable particular. Characteristically, general term predications have a classificatory function. It is amongst general terms that typeexpressions fall.

In describing a predicative assertion as "having a classificatory rôle", I reflect a <u>prima facie</u> distinction in respect of how certain syntactic forms of predications are used. In ordinary predicate logic, no distinction is drawn in the symbolism between the translation of

Nixon is a man. and
Nixon is dishonest.

That is, predicate logic reflects no difference between the forms qu(x is an A) and qu(x is F). Both receive the canonical translation $qu(\emptyset x)$. Yet, from the discussions of distributed and undistributed terms in Aristotle's <u>Prior Analytics</u>, to the medieval <u>sophismata</u>, logicians have pondered the difference between substantive and applicative predications, on the one hand, (the term is Geach's, <u>Reference and Generality</u>, Cornell

U.P., <u>1962</u>, Ch. II) and adjectival or ascriptive predications on the other. One way to mark the difference syntactically is to contrast (1) with (2) in respect of whether the term appearing in the predicate can also function, without transformation, in the subject position: c.f.

(3) A man is an animal.

(4)* (A) dishonest is an animal.,

but the syntactic criterion is rendered spurious if the distinction between subject- and predicate-position is first drawn (as I drew it in II, D, p. 550) in terms of a difference in the rôle of the terms.

Problematic as the search for criteria may be, one can contrast substantive and ascriptive predications in respect of the natural difference in the acts carried out in predicating substantives as in (1), versus predicating ascriptives, as in (2). The natural difference is that (1) is a form of classifying, or allocating to a kind, whereas (2) is a form of characterising or feature-placing. And so to the point: feature placing through the content expressed in predicating qu(x is F)of items <u>a</u>, <u>b</u>, <u>c</u>,... is an activity which typically <u>requires</u> prior classification of <u>a</u>, <u>b</u>, <u>c</u>,... Stated simply: in the order of explanation, the significance of ascribing some feature to an item <u>presupposes</u> the identification which individuates the item and provides criteria differentiating what can have the feature from what cannot. In short--as I shall proceed to argue--taxonomy is prefatory to ascription; or, if one wishes (pace Quine) phylogeny predetermines ontogeny.

This claim is quite vital to my frequent contention through the

thesis that to explicate linguistic meaning, one has to look first to the active contribution of speaker, context and audience to the utterance-significance in the speech act. Thus, I have claimed, no <u>a priori</u> characterisation of utterance-significance can exhaust the creative capacity of speaker and audience to impose significance upon overt nonsense. I shall discuss shortly the manner in which the first claim leads to the second. First, it is necessary to clarify and defend the former claim.

On the face of it, to claim that ascriptions of features (properties, relations, etc.) presuppose antecedent classifications invites immediate rebuff: not only are taxonomies made on the <u>basis</u> of species and family features, but, surely, we can and do make assertions--like qu(this is red), qu(John is lazy), qu(Rex is ahppy) without presupposing the application of some substantive determining <u>this</u> to be a leaf, John to be a person, and Rex a dog? What <u>kind</u> of item John is is surely irrelevant to the statement one makes in asserting that he is lazy?

The first grounds for rebuttal are, in themselves, puzzling. We are often given a classificatory schema which applies sortals and mass terms to items <u>before</u> we discover the truths which determine membership of the sort or kind. Not only do children <u>use</u> and correctly apply general terms in the absence of criterial features for their application; but some scientific advances are paradigms of the application of a system of classification which actually elicits the experimental search for criterial features—one thinks naturally of Mendele'ev's formulation of the periodic table in advance of experimental confirmation of the underlying distribution of atomic bondings. This procedure of "pulling oneself up by one's own bootstraps" is not at all infrequent in description and explanation.

Secondly, the reply has to be that, though we do not have to apply some particular substantive classifier in each and every case in which we refer to something to ascribe some feature to it, nonetheless it is conceptually incoherent to suppose, in general, that placing features in an environment through making statements and verifying or falsifying them, can take place in the absence of prior partitioning of that environment, however minimal.

The reply is based upon the following considerations in which I return to those properties of systems of symbolic representation which I listed in concluding section I, D (pp. 330-331). I shall repeat them in summary, below.

My claim is that a non-iconic, symbolic system of representation, or a language, if it is to embody the possibility of representing both what is and what is <u>not</u> the case--through acts of making true and false claims, has to contain resources which <u>ipso facto</u> demarcate between representing <u>items</u> (designating, referring) and representing <u>situations</u> (factstating, asserting). In other words, a true/false dichotomy requires <u>differentiation</u> of items (<u>a</u> from <u>b</u>) and of items from situations (<u>a</u> from a's being F).

Why should this be so? Notice first that, for a distinction to function as a true/false dichotomy at all (and it matters little how the distinction is drawn, or whether it pertains only to linguistic acts, or whether we call acts "linguistic" which can yield true/false outcomes),

something distinctive must be heralded in deeming a statement true. Provision for this difference requires, in the most general terms, that any feature-placing act which purports to yield a truth <u>about</u> some item contains within it the possibility of <u>both</u> the act's <u>failing</u> (it's yielding nothing at all) <u>and</u> of the act's succeeding, but yielding what is not, in fact, true. In other words, for the difference between truth and falsity to be a <u>genuine</u> difference, it is not enough merely to separate successful from unsuccessful representations--since there would, then, be no difference between representing what is not, in fact, the case, and not representing at all. In brief, for an assertion to yield something true or false of an item, the possibility must exist of that assertion's <u>not</u> being true in virtue of the item so indicated, and not solely in virtue of the act of asserting.

Thirdly, unless a statement's being false is accountable in some way or other to the indicated item's <u>not</u> having the ascribed feature (or having the feature denied of it), then no coherent distinction can be drawn between a statement's being <u>false</u> of that item and its being true of something else.

In sum: a true/false dichotomy between statements, to be coherent at all, reflects a concommittant difference between making two statements about one item--one true, the other false--and making two statements-both true--about different items. Finally, if a true/false dichotomy can only be made when a representational system differentiates what a statement is true <u>of</u> and everything else, then, <u>ipso facto</u>, such a system already contains the classificatory resources necessary to draw this

difference, in principle, <u>independently</u> of the particular truth or falsity of feature-placing statements involved.

The properties of representational systems to which I appeal, here, are essentially those which Wittgenstein ascribed to a logically correct form of notation. They comprise the following:

a) If 'representing' is to take place symbolically, it must be possible to separate 'failing to represent' from 'representing what is not the case'.

b) 'Representing' is not reducible to <u>naming</u>, since to mention something which does not exist is to fail to mention, whereas to represent what is not the case, is not to fail to represent, but to represent successfully. (In other words, though 'naming' can proceed only with "simulation", 'representing' requires "dissimulation".)

c) If a system of representation is to be <u>logical</u>--if it is to articulate differences between what obtains and what doe's not, it has to embody a true/false dichotomy. As such, it acquires the means of representing both items and situations--since it distinguishes what is true of different items (in different situations) from what is true of an item in one situation and false of the same item, in another.

a) - c) are essentially Wittgenstein's conditions upon an adequate representational system. Further conditions stem from the claim of a system of representation to be a <u>language</u>: namely, that it be finitely learnable, without being taught, that it be recursive (generative) and permits free deviations from norms. (This last distinguishes <u>rule</u>constituted systems, like chess, in which to deviate from the rules is

to stop playing chess; from <u>rule-governed</u> systems which <u>have</u> rules which do not proscribe deviations, they merely delimit bounds). The last condition is essential to the account of linguistic creativity and change.

As an illustration, <u>only</u>, of how these distinctions function in my argument to this point: consider what one supposes to be the earliest linguistic act in the neonate's acquisition of language.

So far as I am aware, empirical evidence is inconclusive upon the question whether infants first acquire a linguistic capacity to make statements, rather than to perform other speech acts ("promising" seems doubtful, "commanding" is likely, "threatening" is existentially appealing). The question, in any case, is not entirely empirical. It involves essential consideration of what a 'linguistic act' is. If the neonate's perceptual discriminations (repeated over time and in different contexts) between how much of the otherwise undifferentiated stuff of experience constitutes one 'Mama', whilst the rest is 'not-Mama', then it is arguable that overt linguistic capacities do not post-date this discriminatory ability. (I am excluding here the possibility that embryos have prenatal, genetically encoded linguistic capacities. I do so largely because, though it might be a necessary condition of learning and speaking a language to have a genetic complexity sufficient to develop a large quantity of neo-cortical tissue, the chromosomes concerned do not "speak" a language, do not engage in human linguistic practices, and, thus, do not themselves realise the potential ascribed to them).

The question of <u>which</u> linguistic act comes first is unimportant to my illustration--provided it is recognised that, whichever it is, the

child can repeat the act, and it can be both successful, at sometimes, unsuccessful at others, and correct or incorrect in respect of the appropriate appraisal. That is, if it is a statement-making act, its outcome can be true or false. If it is a command, it can enjoin the relevant behaviour <u>through</u> its representation of the agency and what is ordered thereby.

In this sense, a causally affective stimulus--like crying--is not an essentially linguistic activity, even if it elicits a response appropriate to a command or threat. For undifferentiated crying does not <u>communicate</u> the identity of subject-matter--nor does it "stake a claim" to being true or false, appropriate or inappropriate, correct or incorrect-in sum, significant. This holds, I believe, notwithstanding the <u>inter-</u> <u>pretation</u> of cries by a parent, which can invest them with a complex significance, often heavily tied to the context in which they occur ('before being fed', 'after being fed',...).

The conclusions I wish to draw from this survey of the neonate's linguistic performances--viewed from the perspective of the Wittgensteinian notion of a representational system--is that some classificatory activity is presupposed in the explanation of a linguistic act as one whose performance conforms to conditions upon both success in communication and what I shall call "representational articulation"--the possibility of being successful yet incorrect (false, inappropriate, irrelevant, or whatever).

If classification is, in this sense, a precondition upon making statements, issuing commands, etc., then the chief motivation for

analysing context-relative aboutness—in II, D(A)-- as presupposing the application of general terms is brought to the fore. In this respect, the formalism for CS-1 captures already the distinction I drew above between substantive- and ascriptive-predications and ties the truthconditions of the former to one-half of the determination of an utterance's significance in context--fixing an utterance's aboutness. In sum, the difference is one of function: predicating general terms of items, in order to identify items has the primary purpose of classifying-partitioning the domain to make discriminations within which we feature items through ascription. I have included type-predications amongst substantive predications; but within this group, what distinguishes a classification as to <u>type</u> from the application of very general classificatory terms (ultimate sortals or summa genera)?

Viewed in one way, to partition a domain along type-theoretic lines--into a heirarchy of 'individuals', 'properties of individuals', 'properties of properties',..., and so on--is much like the subdivisions within a taxonomy of natural kinds--species and genera. There are important differences, however. The difference between a natural kind, or sortal term like 'Man', 'Animal', 'Organism', and a 'logical' kind or <u>type</u> is that membership of the latter is determined by the <u>signifi</u>-<u>cance</u> and <u>nonsignificance</u> of predications; whereas membership in the former is determined by the <u>truth</u> and <u>falsity</u> of predications. One of the misleading things about a type theory, if it is taken to resemble a very general taxonomy, is that the classifications it makes appear to depend upon essential features of the items classified. Just as taxonomy produces definitions per genus et differentiae in the form:

x is a Man iff x is an animal & x is rational, bipedal, warm-blooded, and so on;

it might seem appropriate to look for similar "definitions" of "individual", "property", and so on. But it would be entirely inappropriate to phrase type-allocations in the form:

x is of type-n iff x has F_0^n , F_1^n , F_2^n ,...

The misconception lies in supposing that <u>significance conditions</u> are like <u>truth-conditions</u>, when they are not. As I argued at length in I, C (pp. 165-167 and <u>passim</u>), Russell insisted that a type theory--in practice--as it applied to a particular subject matter, is <u>relative</u>. Given that one class of assertions is taken as <u>elementary</u> (as being 'about' individuals), the heirarchy is generated from there by generalisation. This is a profound insight of Russell's: that, in so far as the <u>basis</u> for type theory lies in the forms of the judgements we make with respect to what is elementary, what is obtained from what is elementary, and so on, then it is in the <u>application</u> of a type theory to <u>any</u> subject matter that proscribes, on pain of nonsignificance, violations of this ordering of levels. In other words, type distinctions represent conditions upon how we come to <u>understand</u> and, thereby, form judgements about the subjects of our discourse in advance of placing features true or false of them.

The doctrine of relative types is clouded in Russell's development of ramified type theory, because of the demands of his theory of propositional meaning. To base this latter theory upon a primary epistemological relation 'acquaintance' with those (logical) simples which enter into propositions, risked the presumption that types classified items <u>abso-</u> <u>lutely</u> into kinds. The confusions of what is perceptually <u>simple</u> with what is logically <u>simple</u> infect the accounts of both Russell and Wittgenstein--or, at least, it corrupts the most frequent misinterpretations of their accounts.

For, Russell was led by the demand that analysis arrives at 'logical' simples--not further analysable--to an identification of objects of acquaintance with the primary data of perception--sense data. And the impetus that Wittgenstein's <u>Tractatus</u> gave the positivists of the Vienna Circle can be imputed, in the main, to their (mis)interpretation of "elementary propositions"--the end product of logical analysis--as "protocolsätze"--the simplest form of immediately verifiable observation sentences.

The doctrine of relative types, however, deserves a more sympathetic reading--one which is implicit in the account of 'aboutness' and 'significance' I have given in Part II. For, though a type theory may appear in many forms according to how heirarchies are generated, the crucial component of a type theory is the principle that membership of a type is determined by significance and not truth.

Reflecting upon this principle, it becomes possible to draw together the claims for contextually-relative significance, the description of presupposed general- and type-predications, and the account of categorymistaken predication I have espoused.

There is no hard and fast classification of sorts or types which

is imposed upon our language from without--what is 'individual' with respect to one discourse--i.e. through the conditions governing the aboutness and super-predications of an assertion in one context--may for different purposes, and in different conditions, be treated, say, as a composite of parts (e.g. 'numbers' in Peano-arithmetic are unanalysable elements, in Zermelo Fraenkel set-theory, they are sets), or as a relation between items (e.g. 'justice' in a naturalistic ethic may be a simple quality, in a Platonic citizen , it is the proper balance of the tri-partite elements of the soul), or differently classified, as a property of interracting events, which themselves are four-dimensional space-time manifolds.

Here, then, I locate Wittgenstein's insistance that, if a system of signs is used with sense, then it is the type-theoretic <u>form</u> underlying our classifications of items, and ascriptions of features to them that proscribes category-violations. As he remarks in the Tractatus:

We cannot give a sign the wrong sense (5.4732) Frege says that any legitimately constructed proposition must have a sense. And I say that any possible proposition is legitimately constructed, and, if it has no sense, that can only be because we have failed to give a meaning to some of its constituents. (T. 5.4733)

to which we can add his remark in the Notes Dictated to Moore (April 1914) (Wittgenstein, <u>19**6**9</u>, p. 107):

In order that you should have a language which can express or <u>say</u> everything, this language must have certain properties... Thus a language which <u>can</u> express everything <u>mirrors</u> certain properties of the world by these properties which it must have; and logical so-called propositions stem in a systematic way from these properties .. An illogical language would be one in which e.g. you could put an <u>event</u> into a hole. Here, again, Wittgenstein is reflecting that a language for which the type-classifications did not fix the manner in which signs symbolise properties as opposed to individuals, facts as opposed to relations, would be one in which category-absurdity would be a feature of the world so classified--a world in which events could appear in holes.

This, in essence, was the point at which my conclusion began: "the substance of the world [the types and sorts of items in it] can only determine a form...grammar tells us what kind of object anything is" (ibid., p. 647). The insight is <u>not</u> that what kinds of item there are can be decided by language--for that would be to confuse 'logical' with 'natural' kinds; i.e. it would be to conflate significance- with truth-conditions.

In general, type-predications characterise the manner in which, relative to a subject-matter, once we have determined what is to be 'individual' and what kind of predications are significant over these individuals (express properties of them), then the incompatibility between identifying the aboutness of a predication and the range of application of the predicate can only arise in our crossing over between the categoryallocations determined in the context by the classifications already made. This incompatibility I described in II, D as distinctive of categorymistaken predication---and the measure of its significance-failure was represented by the extent to which our classificatory predications, and the ascriptions consequential upon these, would have to be revised to identify a definite statement such a predication could yield.

For example, there is nothing illogical or category-mistaken in treating, say, qu(Triangularity), qu(Virtue) as about individuals--provided

that, in that context, the aboutness conditions for qu(Triangularity) do not become confused with our more ordinary concept of an individual spatio-temporal particular. For, it is always significant to predicate colour of spatially extended items--but it is category-mistaken to transfer the significance conditions for what may be 'individual' in one context to what is identified as 'individual' in another. From such miscegenation, the common examples of category-mistakes--like qu(Triangularity is blue)---are derived.

Such categorial boundaries are not set in any absolute sense-they are relative to how we classify in the context for the purposes of identifying a subject matter and featuring aspects of it. There can be no fixed standard for placing items according to category--if only because, if a category-mistaken predication is an incompatibility between <u>how</u> an item is identified and what is stated of it, it is always possible to resolve the significance failure by identifying the item in different ways.

There is one further thread to this discussion. Classifying according to type, I claimed, is an activity--it is something <u>we</u> do through the manner in which we introduce the topics of our discourse into a context. Yet, <u>how</u> we classify is not arbitrary. Our actions are constrained by the conditions for their success. It is that which is constitutive of <u>acting</u> rather than behaving according to custom or pattern (where success is conventional).

I argued through II, Section B that statement-making is something we, as speakers, do in expressing a proposition to an audience, in a

context. The success of one's statement-making acts is not conferred by the honesty of one's intent, but by the content expressed to others in what one says.

The effect of this observation is to introduce the notion that utterance-significance is not so much the "passive" content which a sentence, alone, conveys, in the abstract manner of dictionary-entries. Significance-in-context involves a dynamic interaction of speaker, context and audience. That is, understanding what is said is akin to neither passive reception of data, nor to the transfer of a 'message', by means of signals, from speaker to audience. I have emphasized instead through my examination of significance-failure, and the logic of significance claims, that contextual significance, and, thus, communication between speakers, requires interpretative <u>acts</u>, however immediate, which are essentially creative. The significance of what one says hinges both upon the speaker's input--the form of words in the language he speaks-and upon the interplay between speaker, context and audience.

<u>APPENDIX</u> <u>A</u>: "I am sorry to hear that my objection ... paralyses you." (Wittgenstein to Russell: 22/7/13)

The purpose of this Appendix is to examine in detail Wittgenstein's criticisms of Russell's 1913 multiple relation theory of judgement and to seek to show how those criticisms led to fundamental changes in Russell's logical doctrines, including ramified type theory. These changes appear explicitly in the second edition of PM (1927); but they are anticipated in Russell's espousal of Wittgenstein's doctrines of logical atomism (from 1918). The impact of Wittgenstein's criticisms is immediate and direct. Voiced in 1913, they caused Russell to abandon a major work, in manuscript, on the theory of knowledge--a work which incorporated a refined version of the multiple relation theory. Unfortunately, in the literature on Wittgenstein's influence on Russell these criticisms are not well-understood; in particular, it has not been shown why the effects of the critical attack by Wittgenstein were so extensive--leading Russell, as he reports later, to despair that: "I could not hope ever again to do fundamental work in philosophy". More importantly, the effects of the criticisms upon Russell's logical doctrines and theory of meaning have not been fully brought out. Nevertheless, in offering the following speculative reconstruction of Wittgenstein's criticisms and their effects, I have drawn upon several sources which have gone some way towards account for Russell's reaction: to unravel the complex issues of interpretation bound up in Russell's successive refinements of the multiple relation theory of judgement and

belief between 1910 and 1913, I rely upon K. Blackwell's (unpublished) thesis "Wittgenstein's Impact on Russell's Theory of Belief" (M.A. Thesis, <u>McMaster University</u>, 1974); upon lectures by E.R. Eames at McMaster University (1976 and 1977) on the relationship between Russell and Wittgenstein; and upon Eames and Blackwell's excellent bibliographic reconstruction of the unpublished m.s. Russell gave up writing in 1913 as a result of Wittgenstein's attack (reported in "Russell's Unpublished Book on Theory of Knowledge", <u>Russell</u>, <u>Journal of the Russell Archives</u>, 19, 1975, pp. 3-18). Neither Eames nor Blackwell, however, have demonstrated how Russell's abandoning the multiple relation theory affected the manner in which that theory, together with the doctrine of incomplete symbols, was to support the ramified theory of types. It is my intention to examine that question in this Appendix.

Firstly, to understand Wittgenstein's criticisms we have to expound in greater detail than is carried out in Section C the refined version of the theory of judgement that Russell had developed by 1913. In addition, we have to reconstruct Wittgenstein's criticisms and their effects--through the period 1913 - 1918--from only the meagrest evidence. A sketch of the background highlights the problem of interpretation involved.

Following publication of the third volume of PM, Russell began work-early in 1913--upon a major book on the Theory of Knowledge.² Working at astonishing speed, Russell had completed about 350 pages in a month (from 7/5/13 to 6/6/13, approx). Substantial historical and bibliographic evidence, compiled by Eames and Blackwell (loc. cit.), establishes that much of the m.s. of this book (the first six chapters) was published subsequently

as articles in the <u>Monist</u> during 1914 and 1915.³ Nevertheless, the rapid progress on the m.s.--of which Russell writes enthusiastically to Ottoline Morrell through the Spring of 1913---up to the development of a revised version of the multiple relation theory applied to 'understanding', 'belief' and 'judgement', is brought to a halt around 6th June, 1913.⁴ Eames and Blackwell (loc. cit., p. 8) document how, in the letters to Ottoline Morrell through this period, Russell writes elatedly of the progress he is making (Letters ## 768, 781; Russell Archives: <u>Archives Catalogue</u> numbering, dated 8/5/13 and 20/5/13). Within a fortnight, however, Wittgenstein had delivered the second of two related criticisms which, Russell reports, "rather destroyed the pleasure in my writing."⁵ Soon Russell is commenting: "I have only superficially and by an act of will got over Wittgenstein's attack"⁶, and two weeks later he has given up writing the book. He confesses to Ottoline Morrell:

All that has gone wrong with me lately comes from Wittgenstein's attack on my work--I have only just realised this. It was very difficult to be honest about it, as it makes a large part of the book I meant to write impossible for years to come probably the first time in my life that I have failed in honesty over work.⁷

At this time, as Eames and Blackwell report (loc. cit., p. 8), Russell contemplated suicide. It is safe to conclude, thus, that Wittgenstein's criticisms must have been fairly devastating--at least as they appeared to Russell.

There is unfortunately considerable controversy over the exact nature and effect of Wittgenstein's criticisms, in philosophical terms. This controversy is largely a consequence of the fact that the criticisms
have to be inferred from brief references in Wittgenstein's letters to Russell (published in G.H. Von Wright (ed) Letters to Russell, Keynes and Moore, Oxford: Blackwell, 1975, letters R1 - R42; extracts in Appendix III of G.E. Anscombe's edited translation of Wittgenstein's Notebooks, 1914-1916 (hereafter, 'Notebooks'), Oxford: Blackwell, 1969, pp. 119-131), from copies of m.s. Wittgenstein sent to Russell--believed to have been collated and arranged by Russell, subsequently (a version of which appears as Appendix I to Notebooks); and notes dictated in October 1913 by Wittgenstein, in Russell's presence, (the so-called 'Notes on Logic') a version of which is now in the Russell Archives.⁸ In addition, as is extensively argued by Blackwell (in Blackwell, 1974), at least part of the devastation Russell experienced as a result of Wittgenstein's attack on his doctrines is attributable to the profound personal effect Wittgenstein, as Russell's student, had upon Russell. Wittgenstein's intense personality, and belligerence in argument, persuaded Russell "that what wanted doing in logic was too difficult for me. So there was no really vital satisfaction of my philosophical impulse in that work (the 'Theory of Knowledge') and philosophy lost its hold upon me."9

It would certainly be unfair to Russell, however, and a distortion of Russell's relations with Wittgenstein through this period, to attribute all of the effects of Wittgenstein's criticisms, in practical terms, to the dominant personality of the latter. In addition, it is scarcely credible to suggest--as Blackwell does (ibid., p. 77)--that Russell was devastated by an objection which "does not seem to apply against the theory advanced in 'Theory of Knowledge'." Instead, then, we have to examine Russell's theory of 1913, reconstruct Wittgenstein's criticisms of it, assess their validity, and appraise the philosophical impact upon Russell's doctrines as these are evidenced by the changes Russell himself made, and by the alternatives that Wittgenstein put forward. In doing so, it has to be emphasised that, through both lack of space and lack of direct evidence, much of the following discussion is highly speculative. Also, there is no guarantee that Russell perceived the full extent of the consequences of Wittgenstein's criticism; so that many qualifying points in the reconstruction arise where Wittgenstein's alternative doctrines, and Russell's interpretation of them diverge. To attempt a fully documented, historical exegesis of Wittgenstein's influence on Russell would certainly demand a thesis in its own right. I shall be content, however, to sketch the main effects as they pertain to changes in type theory between 1910 and 1927.

Wittgenstein's criticisms in the dictated 'Notes on Logic' are directed at four specific doctrines central to Russell's conception of logic: the account of propositions as incomplete symbols, the use of the assertion-sign, the multiple relation theory of judgement and the theory of types. These specific criticisms have to be understood against a general critical attack upon the conception of logic represented in PM. This general attack, and the positive alternatives Wittgenstein develops, issue eventually in Wittgenstein's <u>Tractatus Logico-Philosophicus</u> (1921). The most general effect of this attack upon Russell can be discerned, thus, in Russell's giving up, by 1918, many of the logical and epistemological doctrines of PM, <u>Problems of Philosophy</u>, (1912) and <u>Philosophical Essays</u>, (1910); and espousing what he understood of Wittgenstein's logical atomist

theories of logic and meaning. That this fundamental change in Russell's views was at least begun by the criticisms of the theory of judgement is evident from a passage in a letter to Ottoline Morrell, later in 1916:

Do you remember that at the time you were seeing Vittoz (a Swiss physician) I wrote a lot of stuff about Theory of Knowledge, which Wittgenstein criticized with the greatest severity? His criticism, though I don't think you recognised it at the time, was an event of firstrate importance in my life, and affected everything I have done since. I saw he was right, and I saw that I could not hope ever again to do fundamental work in philosophy. (quoted in <u>Autobiography</u>, vol. II, p. 57).

I have argued (in Section C) that the heirarchy of orders of propositions and functions is given an epistemological foundation in the theory of judgement; whilst the heirarchy of types is given a logical foundation in the doctrine of incomplete symbols. The theory of judgement of the 1913 m.s. is a refined version of that presented in Russell <u>1910</u>, and <u>1912</u>, and summarised in PM, (Introduction, Ch. II, Section iv). Drawing upon Blackwell's account, the refinements to the theory can be explained as follows:

Russell's 1910 theory of judgement (or belief: Russell appears not to have distinguished these explicitly) is very sketchy. When a subject S judges that aRb, Russell supposed, S is acquainted with each of the constituents of the judgement expressed in asserting "aRb". 'Acquaintance'--the presence before the mind of an object of understanding involved in thinking, doubting, believing, wondering and so on--is the basic epistemic relation in Russell's theory. It is explained as a dyadic relation between subject and either a particular, a universal, a logical constant (negation, disjunction, implication,...) or a perceived complex; e.g. "a's-being-R-to-b":

When an object is in my present experience, then I am

acquainted with it ... Suppose I were occupied, like Adam, in bestowing names upon various objects. The objects upon which I should bestow names would all be objects with which I was acquainted ... What distinguishes the objects to which I can give names from other things is the fact that these objects are within my experience ... during the process of naming they appear merely as this, that, and the other. (Russell, 1914, repr. Russell 1956, p. 167)¹⁰

The characteristic of an object of acquaintance for a subject S is, thus, that it be 'nameable'; i.e. S can ostend it by a logically proper name: "this" or "that".

Acquaintance is a dyadic (dual) relation taking two terms, but judgement, belief and understanding take at least <u>three</u> terms. And, in the example of S judging aRb, the judging comprehends <u>four</u> terms--the subject S and the three constituents arranged in the judged proposition.

The explanatory value of the 1910 theory (apart from its role in distinguishing orders) was that it allows for the possibility that S may judge falsely that aRb, even though there is no 'false proposition' which is the object of S's judgement. An elementary judgement of perception is false when the arrangement of objects in the judgement is not as they are in reality.

The 1910 theory, however, had several faults. It did not adequately explain how, in judging aRb, S judges the subordinate relation R to be holding between a and b and not, say, between b and a. In being acquainted with the relation R, Russell had suggested in 1910 (<u>1910b</u>, p. 158), the relation "must not be abstractly before the mind, but must be before it as proceeding from a to b rather than from b to a". Russell called this the "sense" of the relation, and supposed that, in being acquainted with a relation, one was also acquainted with its "sense". What is missing, though, is an account of how the sense relates asymmetrical relations, <u>qua</u> nameable items of experience, to a first, then b; and not b then a.

Much of the unclarity in the 1910 theory results from Russell's ignoring the character of the primary multiple relation ('judging', 'believing',..). If judging is simply S's standing in a multiple relation to objects arranged in the judgement-complex, one wants to add at least that S understand what he judges. One would not think it possible to judge some proposition to be true without understanding the proposition. A key advance, in fact, which is made in Russell's formulation of the 1913 theory, is his recognition that <u>understanding</u> that aRb is basic, presupposed in judging and believing, and that an explanation of understanding is the "first step in the analysis of propositional thought" (1913 m.s., p. 204/ Archive typescript (t.s.) p. 46). Understanding, Russell argues, is fundamentally different from acquaintance:

Understanding ... is presupposed in belief, and can itself be discussed without introducing belief. Understanding and belief, however, are closely akin as regards logical form and raise the same logical problems; whereas understanding and acquaintance ... are very widely different in logical form. (1913 m.s. p. 198/t.s. p. 42).

Introducing 'understanding' as the basic kind of multiple relation involved in 'attitudinal' facts (judging, believing, supposing, etc.) is one refinement on the 1910 theory. A second refinement is to introduce an additional element in the analysis of understanding (hence, judging, believing); namely, acquaintance with the logical form of the proposition understood:

If we are acquainted with a and with similarity and

with b, we can understand the statement 'a is similar to b', even if we cannot directly compare then and "see" their similarity. But this would not be possible unless we knew how they are to be put together i.e. unless we were acquainted with the form of a dual complex (the form 'x ϕ y'). Thus all "mental synthesis", as it may be called, involves acquaintance with logical form. (1913 m.s. p. 190/t.s. p. 36--my insert).

The logical form involved in, say, judging that Socrates precedes Plato (in time), is what we understand when Socrates and Plato and 'precedes' "are united in a complex of the form 'xRy', where Socrates has the x-place and Plato has the y-place. It is difficult to see how we could possibly understand how Socrates and Plato and 'precedes' are to be combined unless we had acquaintance with the form of the complex" (1913 m.s. pp. 185-186/t.s. p. 33). We arrive at a logical form--or a "pure form" as Russell sometimes called it--when we consider the "complete generalisation" of a proposition; i.e. when every expression has been replaced by a variable. For example, we obtain the logical form 'xØy' from a proposition like "Socrates precedes Plato" by replacing each of "Socrates", "precedes" and "Plato" by a variable in such a way as to preserve the sense of the relation as from Socrates to Plato: thus "xøy". Similarly, we obtain the form 'Øx' from a proposition like "Socrates is a man" by analogous generalisation. This characteristic of "complete generality" is what Russell took to be distinctive of the propositions of logic (hence, of mathematics):

Every logical notion ... is or involves a <u>summum genus</u>, and results from a process of generalisation which has been carried to its utmost limit. This is ... a touchstone by which logical propositions may be distinguished from all others ... 'Logical constants' are really concerned with pure form. (1913 m.s. pp. 182-183/t.s. p. 31)

As has been pointed out by D. Pears (Phil. Rev., vol 86, April 1977, pp.

177-196) this view has the implausible consequence that, in so far as we are acquainted with the pure form ' $x \phi y$ ', of a dyadic relation, this alone guarantees the <u>logical</u> truth of the proposition that something has some dual relation to something:

The importance of the understanding of pure form lies in its relation to the self-evidence of logical truths. For, since understanding is here a direct relation of the subject to a single object (the form) the possibility of untruth does not arise, as it does when understanding is a multiple relation (ibid. m.s. p. 250/t.s. p. 73-my insert)

This "touchstone" of logical propositions--their involving "pure form", hence, being 'completely general' and, thus, self-evident--is one consequence of Russell's theory upon which Wittgenstein exerted the utmost pressure. It is a doctrine which had appeared also in the first edition of PM (*12, pp. 162-164) in the form, described in Section C, that all propositions of PM can be obtained from matrices by generalisation. It is therefore noteworthy that this distinctive mark of logical propositions--"complete generality"--is explicitly dropped from the second edition of PM (c.f. Introduction to Second Edition, p. xiii and Appendix A, *8).

In sum, the characterisation of 'understanding' in the 1913 theory, in terms of which judgement and belief are to be explained, is as follows: confining attention to <u>atomic</u> propositions (not involving logical constants or inference):

An atomic propositional thought is defined as a complex formed by a multiple relation of the subject to certain objects, where nothing is involved in the objects which may not occur in an atomic complex, but the 'form' of some atomic complex does occur. (1913 m.s. p. 345/t.s. p. 158) Thus:

What is called 'understanding a proposition' is a relation of a subject to certain objects which are (1) the form of certain atomic complexes, (2) entities of the same logical kinds as the constituents of such complexes, sufficient in number and kind to form one such complex. A given proposition will be the fact (if it is a fact) of there being a complex of the form of an understanding, when the objects are given. (ibid. m.s. p. 346/t.s. p. 158).

This yields the following analysis of the statement that someone S understands the proposition that a and b are similar (from Blackwell, <u>1974</u>, p. 70):

U {S, a, b, similarity, $x \phi y$ }.¹¹

"U" stands for the multiple relation of 'understanding'; "S" for the subject; "similarity" is the subordinate relation with which we are acquainted; and " $x \phi y$ " (or " $\phi(x,y)$ ") is the pure form of the understood proposition.

Judgement and belief differ from understanding in the character of the multiple relation "U", and in being true or false. Thus the analysis of "S judges that aRb" is:

 $J \{ S, a, b, R, \phi(x,y) \}.$

and, provided this is an <u>elementary</u> judgement (of perception), it is true if there is a corresponding perceived complex "a's-being-R-to-b", otherwise false. The heirarchy of orders can thus be built up from elementary judgements 'about' perceived complexes, as described in Section C.

Before proceeding to Wittgenstein's criticisms of the theory, notice how the 'proposition'--qua object of judgement or belief--has disappeared entirely from the analysis. Indeed, the 1913 m.s. contains the clearest statements of Russell's doctrine that propositions are not single entities, but are what we "falsely abstract" from incomplete symbols, which, properly, are only completed in being asserted, believed, or judged true. Analysis reveals that all apparent references to propositions by means of descriptive phrases are removed by the contextual elimination of incomplete symbols:

Now a proposition is, in my opinion, an 'incomplete symbol', i.e. some context (of assertion) is necessary before the phrase expressing the proposition acquires a complete meaning... We must ... say that, in the sense in which propositions are involved in believing and in propositional understanding, there is no difference, as regards reality, between true and false propositions. And this, in turn, since it is repugnant to admit the reality of false propositions, forces us to seek a theory which shall regard true and false propositions as alike unreal, i.e. as 'incomplete symbols'. (1913 m.s. pp. 200-201/t.s. pp. 43-44)

The 1913 manuscript ends in Part II with the analysis of judgement, belief, truth and falsehood, and self-evidence applied to atomic propositions, only. On Eames and Blackwell's reconstructed Table of Contents--(loc. cit. p. 10), this was to be followed by Part III in which the theory was applied to 'Molecular propositional thought', i.e. judgements involving inferences and logical constants (Archives document 210.06556-F1 is an outline, on one page, of the proposed contents of Part III, entitled "Molecular Thought"). It is interesting to note that this pattern of development--beginning with atomic 'elementary' judgements and proceeding through molecular judgements to logical inference--mirrors the introduction of the 'orders of generality' of propositions and functions which is characteristic of the ramified heirarchy of PM. One can at least surmise that in classifying the different kinds of judgement, thus, Russell had in mind the heirarchical classification developed in the ramified theory.

Though Russell had continued with the m.s. after Wittgenstein's criticisms, it is clear that the 'paralysis' Russell felt, as a result of the criticisms, stems from their effect upon the revised theory of judgement as that applied to <u>atomic</u> propositions. It is thus the foundation of the account of different kinds of judgement--the base-level of the heirarchy of orders--that is called into question by Wittgenstein. Before I proceed to state those criticisms, appraise their validity, and examine their effects, the available documentary sources for the criticisms should be marshalled.

Russell reports what is apparently the first occasion of Wittgenstein's criticising the 1913 theory in a letter to Ottoline Morrell of 28/5/13 (he does not give details of the criticisms):

We were both cross from the heat. I showed him a crucial part of what I had been writing. He said it was all wrong, not realising the difficulties--that he had tried my view and knew it wouldn't work. I couldn't understand his objection--in fact he was very inarticulate--but I feel in my bones that he must be right, and that he has seen something that I have missed. If I could see it too I shouldn't mind, but as it is, it is worrying and has rather destroyed the pleasure in my writing--I can only go on with what I see, and yet I feel it is probably all wrong.

We do not know which part of Russell's 1913 m.s. was the "crucial" one Wittgenstein criticised--though Blackwell gives good grounds for identifying it as containing the chapter (I of Parc II) giving the theory of understanding expounded above. Russell carried on writing the m.s., without enthusiasm. Much of his difficulty stemmed from trying to take account of Wittgenstein's criticisms (which he barely understood), which "had to do with problems I (Russell) want to leave to him (Wittgenstein)"¹², and with the difficulty of "not stealing his ideas".¹³ Soon Russell came close to utter despair,¹⁴ and it is of this that Wittgenstein writes (22/7/13):

> I am very sorry to hear that my objection to your theory of judgement paralyses you. I think it can only be removed by a correct theory of propositions. (<u>Notebooks</u>, Appendix III, p. 121).

Contrary to a view held by D. Pears¹⁵--that Wittgenstein had not seen Russell's 1913 m.s. prior to dictating the 'Notes on Logic' in Russell's presence--a view which would entail, surprisingly, that Wittgenstein paralysed Russell by attacking his old theory of 1910 (a view refuted by McGuiness' analysis of the two manuscripts of the 'Notes'¹⁶), Wittgenstein's criticisms have to be construed as applying to the refined 1913 theory of judgement. The dictated 'Notes on Logic' can be more or less confidently dated at October, 1913, and, since the chief source for the detail of Wittgenstein's criticisms comprises these notes, together with a letter of June, 1913, the reconstruction of the criticisms requires the assumption that Wittgenstein was criticising the 1913 version of the theory. Scant additional evidence for the effects of the criticisms -- in terms of Russell's perception of them--is available. I shall refer to a three-page document entitled "Propositions" (Archives # 220.011440), one leaf of which Russell wrote on the verso of a rejected page of the 'Theory of Knowledge' m.s. (Page 197 of the m.s., which forms part of the "crucial" chapter on 'understanding' (!)) and which appears to be a sketch of an unsuccessful attempt to overcome the problems Wittgenstein caused. The remaining evidence of

Russell's reaction (and his understanding of Wittgenstein's criticisms) has to be gleaned from later sources: the 1918 Lectures on the Philosophy of Logical Atomism (repr. in Russell, <u>1956</u>, pp. 175--282), where Russell espoused many of Wittgenstein's doctrines; the 1924 survey article on Logical Atomism (Russell, <u>1956</u>, pp. 323-343) and the changes in PM embodied in the introduction to the second edition (<u>1927</u>, pp. xiii-xlvi). It remains controversial, however, as to how many of the changes in PM are a result of Wittgenstein's influence, and how many result from Ramsey's criticisms of the page-proofs of the new introduction (Russell acknowledges obligations to both in the first two pages of the introduction).¹⁷

The first task, however, is to reconstruct Wittgenstein's criticisms, and to appraise their effects. By far the most fruitful method of doing this is to approach the question from Wittgenstein's point of view. Recall that, in his letter of 28/5/13 to Ottoline Morrell, Russell reports Wittgenstein as claiming "it was all wrong ... he had tried my view and knew it wouldn't work"¹⁸. The only evidence of what preoccupied <u>Wittgenstein</u> in the period before and immediately after the 'paralysing' objection to Russell's new m.s. is that of Wittgenstein's letters to Russell from Summer 1912 to Summer 1913. I believe that, by attention to these letters, the reasoning that led Wittgenstein to criticize Russell's theory and to claim that "he had tried my view and knew it wouldn't work" can be clearly discerned. Of course, piecing together what questions preoccupied Wittgenstein's reports of them (in the absence of Russell's letters to Wittgenstein) is a one-sided, and highly speculative enterprise. Yet the evidence suggests

quite clearly a singular chain of reasoning in Wittgenstein's thinking from the Summer of 1912 to the Spring of 1913 (when he criticised what he saw of Russell's Theory of Knowledge m.s.), and continuing through the later part of that Summer. The thread that connects this reasoning together is the ramified theory of types and Wittgenstein's attempts to make sense of it. (For brevity, I shall employ Von Wright's number R.1.-R.57 with date (occasionally inferred) in referring to Wittgenstein's letters to Russell: see, <u>Wittgenstein's Letters to Russell</u>, <u>Keynes and Moore</u>, ed. Von Wright, Oxford, Blackwell, 1974).

From Wittgenstein's earliest letters R.2, R.3, it is clear that his first preoccupation is with the question of what special nature distinguishes the propositions of logic. As early as June 1912 it is surprising to find Wittgenstein anticipating a central doctrine of the <u>Tractatus</u> (4.0312, 5.4) that the logical signs "v, \supset , \sim ,..." do not denote--there are no objects or functions which are the 'meanings' of logical signs:

Logic is still in the melting-pot but one thing gets more and more obvious to me: The propositions of Logic contain ONLY APPARENT variables and whatever may turn out to be the proper explanation of apparent variables, its consequence <u>must</u> be that there are NO <u>logical</u> constants. (R.2, 22/6/12: Wittgenstein's emphasis).

Gradually, through the Summer of 1912, Wittgenstein comes to concentrate on this problem of the 'meaning' of logical signs--disjunction, negation, and so on (R.4, Summer 1912)--and tries to make sense of there being an elementary <u>complex</u> corresponding to each <u>molecular</u> proposition. (From PM, p. 44, a <u>complex</u> is what makes elementary judgements of the form 'a has the quality q', 'aRb', ' \emptyset (c,d,e)',... true). Thus, the question arises: why did Wittgenstein believe that the problem of the meaning of logical signs was connected to his denial that anything but apparent (bound) variables could occur in the propositions of logic? Addressing Russell, Wittgenstein calls this problem "our problem" (R.5, 16/8/12). The answer to this question, I believe, lies in PM, therefore, and, in particular, in the justification for the ramified theory of types.

Notice first that, for any incomplete symbol of PM to express a proposition in being asserted or judged true (see: PM, p. 44), it can contain only apparent variables if it contains variables at all:

Propositions which contain no functions and no apparent variables may be called 'elementary propositions'. Propositions which are not elementary, which contain no functions, and no apparent variables except individuals, may be called 'first-order propositions'. It should be observed that no variables except <u>apparent</u> variables can occur in a proposition, since whatever contains a <u>real</u> variable is a function, not a proposition. (PM, p. 54)

Thus, in demanding that logical propositions "contain ONLY APPARENT variables" (R.2), Wittgenstein is applying pressure immediately to Russell's conception of the propositions of logic as "completely general" propositions. As we noted above, the mark of a proposition of logic, Russell held, is its generality. This doctrine is, however, ambiguous; for, it could mean either that logical propositions are only concerned with "pure form" (containing only <u>real</u> (free or schematic) variables), or that in the logical propositions of PM (axioms, primitive propositions, theorems) <u>only</u> apparent variables occur. The most problematic effect of this ambiguity involves the ramified theory of types. Concentrating, thus, upon type theory, we can reconstruct how Wittgenstein connected the "apparent variable business"

What troubles me most at present is not the apparent variable business, but rather the meaning of 'v', '.', ' \supset ' etc. This latter problem is--I think--still more fundamental ... If 'p v q' means a complex at all--which is quite doubtful--then, as far as I can see, one must treat 'v' as part of a copula, in the way we have talked over before. (R.4, (Summer 1912 ?)).

It is very hard to reconstruct what Wittgenstein and Russell may have "talked over" in treating "'v' as part of a copula", but I believe the problem involved can be approached as follows:

In Russell's exposition of the formal definition of what it is for items to be of the same type (PM, *9, pp. 128-9), he considers the problem of applying the doctrine of systematic ambiguity across type-orders to logical expressions: disjunction and negation. He concludes:

Hence, negation and disjunction may be treated in practice as if there were no difference in these ideas as applied to different types ... when '~p' or 'p v q' occurs, it is unnecessary in practice to know what is the type of p or q ... The limitation .. to treating .. disjunction .. the same in all types would only arise if we ever wished to assume ... that there is some one function of p and q whose value is always p v q whatever may be the orders of p and q. Such an assumption is not involved so long as p (and q) remain <u>real</u> variables. (PM, p. 128)

The doctrine of systematic ambiguity applies, then, only to expressions containing <u>real</u> variables. As Russell illustrates this point in connection with the logical truth "p .v.~p", the assertion of this truth will hold for all orders because "p" occurs in it as a real (free) variable. Wittgenstein has noticed (I surmise) that this conflicts immediately with the prior claim (PM, p. 54) that what contains a <u>real</u> variable cannot be a proposition; i.e. the assertion "-. p.v.~p" is not a proposition of logic, at all. Failing to perceive the inconsistency, Russell had continued his illustration:

But if we assert " \vdash .(p). p.v.~p" it is necessary ... that "p v~p" should be the value, for the argument p, of a function \emptyset p; and this is only possible ... if p is limited to one type. (PM, *9, p. 129).

As we know from the account of the heirarchy of orders, it is the occurrence of an apparent variable in an expression that confines the proposition expressed to a definite order (through the function's presupposing its range of values). When Wittgenstein requires (R.2, 22/6/12) that, to be propositions at all, logical propositions must contain only apparent variables, he has noticed that, in effect, this would be to confine all the primitive propositions and theorems of PM to a definite order--conflicting with the claim for the "complete generality" of logical propositions. I believe that this conflict led Wittgenstein to suppose that we cannot regard the logical signs 'v', '~', and so on, as 'meaning' (denoting) different functions in each order; hence, that either the logical signs do not denote at all ("there are NO logical constants"), or their meaning must be bound up in the "copulae". By the latter, it would seem, Nittgenstein means that, since it is the predicative part of an expression like "Socrates is human" (i.e. the copula "is human") that, qua propositional function, determines the type of its propositional arguments, then, when propositions occur combined by logical constants like disjunction, their meaning must be "part of" the copulative propositional function, so that their order is fixed. Shortly, however, Wittgenstein recognises the absurdity to which

this version of the doctrine of systematic ambiguity commits him:

Now as to 'p v q' etc.: I have thought that possibility-namely, that all our problems could be overcome by assuming different sorts of Relations of signs to things ... But I have come to the conclusion that this assumption does not help us a bit. (R.5, 16/8/12)

Here, I am identifying "assuming different sorts of Relations of signs to things..." with the doctrine of systematic ambiguity--or, rather, with that version of the doctrine which holds that, if logical truths contain only apparent variables, they must be asserted at each different order and <u>mean</u> <u>something different</u> thereby, because of the differences in the orders of their respective value-ranges. For example, if we regard the 'meaning' of "v" as a function 'Or(\hat{p}, \hat{q})' which takes propositions as arguments, then the range of significant arguments to this function forms a type; and the set of propositions which result from supplying permissable arguments $p_1, q_1, ...$ to 'Or(\hat{p}, \hat{q})' forms a definite order of propositions which are presupposed by 'Or(\hat{p}, \hat{q})'s having a definite meaning. If different orders of propositions are presupposed, the function will differ in meaning (i.e. "Or(p,q)" will mean different functions for different types of argument-c.f. Russell's discussion in PM, p. 39, of " ϕ x" <u>ambiguously denoting</u> its range of values).

There is much that would be unsatisfactory in such a doctrine. The "complete generality" of logical propositions would be lost--the law " \vdash .(p). p .v.~p" would have to mean different things at different orders, and what makes the law generally <u>true</u> would be different at different orders. This would no longer represent a <u>general</u> truth at all.

Wittgenstein persists (R.4, R.5) in attempting to find ways out of

this difficulty through supposing that logical signs have meaning in different ways from ordinary functional expressions. In a sketch of August 1912 (R.5) he <u>seems</u> to be grasping at something like a truth-functional account of logical signs (anticipating, possibly, Sheffer's stroke-function):

For instance: \checkmark (p.q), which is to mean "the complex p has the opposite form of q's form". That means that \nsim (p.q.) holds for instance when p is ϵ_1 (a,b) and q is $\sim \epsilon_1$ (c,d). (R.5, 16/8/12)

The idiosyncratic notation " $\epsilon_1(a,b)$ " expresses the <u>atomic</u> subject-predicate proposition p, through copulating (by ' ϵ_1 '--a copula of type-1) the <u>items</u>, say, Socrates and mortality in the assertion that Socrates is mortal (rather combining the items to form the elementary complex which makes this assertion true). Then, the complex which makes "Socrates is mortal" true is of opposite <u>form</u> to the complex which makes, say, "Plato is not green" true (" $\epsilon_1(c,d)$ "), since the latter is <u>negative</u> in form. Negation is explained, thus, as opposition between <u>forms</u> of complex (c.f. PM, p. 44) and does not need a separate function 'Not(\hat{p})' at each other. Here, in view of the paucity of evidence available, however, my reconstruction of Wittgenstein's thought at this time must be regarded as highly tentative, and perhaps not even coherent.

Thankfully, Wittgenstein does not long remain satisfied with these tentative efforts--recognising, perhaps, that the problem of type-differences in the denotations of 'v', '~', and so on, is reproduced in holding that there are forms of copulae ε_0 , ε_1 ,... of different types:¹⁹

I believe that our problem can be traced down to the <u>atomic</u> propositions. This you will see if you try to explain precisely in what way the Copula in such a proposition has meaning. (R.6, after August 1912)

Wittgenstein has concluded, thus, that the problems of the meanings of logical signs and the apparent variables in logical truths will be resolved if a correct account of the predicative part (the Copula) of atomic propositions and how they mean can be given. He reports (ibid.) that he is now confining attention to the meaning of atomic propositions.

What is this problem of 'the meaning of the Copula'? Basically, it seems, Wittgenstein is considering propositions of the form ' $\emptyset(a)$ '. He knows how the name "a" has meaning--it stands for an individual; but how does the " \emptyset "-part get its meaning? By January 1913, he has got a new view:

I have changed my views on 'atomic' complexes. I now think that qualities, relations (like love) etc. are all copulae! (R.9, January 1913)

Before discussing this 'new view', several points should be noted: R.9 is a long letter connecting the new view of copulae to type theory. It is important to notice that Wittgenstein sees the problem of the meaning of atomic propositions ("atomic complexes") as related to type theory--leading one to interpret much of his preoccupation, in preceding letters, as involving problems arising from type theory. Secondly, R.9 is the last letter discussing logical matters of any substance before Wittgenstein's meeting with Russell in May 1913. R.10 reports his father's death and his departure from Vienna for Cambridge to meet Russell (25/1/13); whilsc R.11, interestingly, reports that Wittgenstein had 'hit a bad patch' and was "as perfectly sterile as I never was, and I doubt whether I shall ever again get ideas." (25/3/13). This indicates that Wittgenstein did not believe he had solved the problem of the meaning of atomic propositions (and, indeed, would not solve it until 1914, when he begins to assemble the Picture Theory of Meaning for elementary propositions). Finally, R.12 (June 1913) postdates his meeting with Russell and contains the "exact" expression of his 'paralysing' objection to Russell's multiple relation theory of judgement. In consequence, it is worthwhile to attend closely to Letter R.9; since, I believe, it contains the most direct evidence for the substance of Wittgenstein's criticism that Russell's theory--displayed in the 1913 m.s.-was "all wrong ... he (Wittgenstein) had tried my view and knew it wouldn't work" (Russell to Ottoline Morrell, 28/5/13). To begin: I quote at length from the letter of January 1913:

I now think that Qualities, Relations (like Love), etc. are all copulae! That means I for instance analyse a subject-predicate proposition, say, "Socrates is human" into "Socrates" and "Something is human" (which I think is not complex). The reason for this is a very fundamental one: I think that there cannot be different Types of things! In other words whatever can be symbolized by a simple proper name must belong to one type. And further: theory of types must be rendered superfluous by a proper theory of symbolism: For instance, if I analyse the proposition Socrates is mortal into Socrates, Mortality and $(\exists x, y) \in_1(x, y)$ I want a theory of types to tell me * that "Mortality is Socrates" is nonsensical, because if I treat "Mortality" as a proper-name (as I did) there is nothing to prevent me to make the substitution the - wrong way round What I am most certain of is not however the correctness of my present way of analysis, but of the fact that all theories of types must be done away with by a theory of symbolism showing that what seem to be different kinds of things are symbolized by different kinds of symbols which cannot be substituted in one another's place. (R.9, January 1913, Wittgenstein's emphasis)

The marked passage "---- *----" is, I believe, the theory Wittgenstein tried and had already rejected when he met Russell in May. It is very close to Russell's theory of the 1913 m.s. Recall that Russell analysed S's judging the proposition that Socrates is mortal into the relational complex: J {S, Socrates, Mortality, $\phi(x)$ }.

where S is the judging subject, and " $\emptyset(x)$ " is the logical form which relates the constituents of judgement in the form of a monadic atomic complex. The difference between Russell's " $\emptyset(x)$ " and Wittgenstein's " $(\exists x,y) \in_1(x,y)$ " would be accountable to Wittgenstein's having treated a copula of this type as having a form which relates two <u>named</u> items (Socrates, Mortality), and to his insistance that only <u>apparent</u> (bound) variables occur in the forms of logical propositions. Thus, if " $\emptyset(x)$ " is the logical form of the judged proposition, then " $\emptyset(x) \cdot v \cdot \sim \emptyset(x)$ " ought to be the <u>logical</u> proposition which expresses that either the judgement or its negation holds. But this 'logical' proposition contains <u>real</u> variables (the <u>logical form</u>, which Russell includes as an object of acquaintance in the 1913 theory, is obtained by replacing <u>names</u> by real variables). As such, it is not a genuine proposition, at all. The variables, Wittgenstein insisted, should be bound--as they are in Wittgenstein's version of the analysis "($\exists x,y$)...".

As is evident from the quoted letter, the reason Wittgenstein rejected this analysis of the atomic proposition---so analogous to Russell's analysis of the atomic propositional judgement---was that the <u>type-difference</u> (between the <u>items</u> Mortality and Socrates) is destroyed in claiming that, <u>qua</u> constituents of atomic complexes (i.e. <u>qua</u> objects of acquaintance), they can be <u>named</u>. All that can be named--by PM, *9, are individuals and, hence, all of the <u>same</u> type. In other words, Wittgenstein had written, in <u>January</u> 1913, describing an analysis of atomic propositions which is entirely analogous to Russell's analysis of atomic propositional judgement, and <u>had rejected it on</u> the ground that it made it possible to judge nonsense of the kind embodied in a type-mistake (e.g. "Mortality is Socrates"). It is my belief, then, that Wittgenstein's apparent annoyance at Russell's showing him, in May 1913, a "crucial part" of the 'Theory of Knowledge' m.s., derived from his having already attempted that analysis of atomic propositions (perhaps suggested by his acquaintance with Russell's earlier version of the multiple relation theory--in Russell, <u>1910</u>, <u>1912</u> and PM), described the attempt in a letter to Russell, <u>and</u> given a compelling reason for finding the analysis wanting (that it made it possible to judge nonsense). All this had taken place in January, 1913--<u>before</u> Russell had formulated the refined version of the multiple-relation theory. I surmise, thus, that the substance of Wittgenstein's criticism in his stormy meeting with Russell in May 1913 involved the fact that, on Russell's refined version of the multiple relation theory, there is nothing in the analysis of judgement to differentiate:

(1) J $\{$ S, Socrates, Mortality, $\emptyset(x)\}$. from

(2) $J \{ S, Mortality, Socrates, \phi(x) \}$.

whereas (1) is the significant judgement that Socrates is mortal; and (2) is the nonsignificant judgement that mortality is Socrates, i.e. (2) is a type-mistake. This speculation, undoubtedly, has to be regarded as tentative; whether one accepts it or not (and I shall contrast it shortly with Blackwell's claim that the objection of Wittgenstein does not apply to the 1913 theory), the general inference I want to draw, to continue the argument, is that through this period (Summer 1912-Spring 1913), prior to his criticising Russell's theory of judgement, Wittgenstein was preoccupied with how to justify type theory through the analysis of atomic propositions.

The most direct <u>effect</u> of Wittgenstein's criticisms should be, one supposes, the problems they cause for Russell's justification of ramified type theory in terms of the analysis of judgement.

Before discussing the criticisms of May and June 1913 in detail, notice also that following this "exact" formulation of the objection to the theory of judgement (R.12, June 1913), Wittgenstein's preoccupation remains with the theory of types, the nature of logical propositions and the "apparent variable business". In a letter (R.15) that Von Wright dates as "Probably Summer 1913" (close after the letter of 22/7/13 which concerns the generality of the primitive ideas of logic--negation, disjunction, and so on--and problems arising from the "abolition of the real variable"; the same letter as that in which Wittgenstein sympathises with Russell's "paralysis" over the objection to the multiple relation theory), Wittgenstein next discusses the Axiom of Reducibility--a fundamental proposition for ramified type theory--and produces what appears to be a very damaging objection to it. I shall discuss this objection shortly. Similarly, the following letter (R.16, 5/9/13) reports Wittgenstein as "sitting here in a little place inside a beautiful fiord and thinking about the beastly theory of types. There are still some very difficult problems ... to be solved." Thus, from Wittgenstein's point of view, at least, the proper context in which to search for the explanation of the objection to Russell's theory of judgement should be the theory of types.

I mentioned above that my interpretation of the events leading up to Wittgenstein and Russell's meeting in May 1913 contrasts with Blackwell's claim that Wittgenstein's objection "does not seem to apply against the

theory advanced in 'Theory of Knowledge'" (Blackwell, 1974, p. 77). Certainly, Wittgenstein was familiar with Russell's earlier versions of the multiple relation theory (from PM and Russell, 1912). Equally certainly, Wittgenstein was not objecting to Russell's modified version of the theory in January, 1913, because Russell had not then formulated it. My claim is only that, in January 1913, Wittgenstein had come up with an analysis of atomic propositions which anticipates Russell's 1913 version of the theory; and had rejected it on the ground that type-differences between the constituents of the propositions are not accounted for. The question arises, then, whether we should regard Wittgenstein's objection--derived from the January letter--as applying only to Russell's earlier version of the theory, and not that of the 1913 m.s. Evidence for this view can be gleaned from Wittgenstein's meetings with Russell prior to their May 27th meeting, of which Russell writes to Ottoline Morrell that "Wittgenstein was shocked to hear I am writing on Theory of Knowledge - he thinks it will be like the shilling-shocker, which he hates." (Letter #775, 13/5/13, to Ottoline Morrell; the "shilling-shocker" is Russell's pet-name for his Problems of Philosophy of 1912). Shortly afterwards, after completion of the first six chapters of 'Theory of Knowledge', Russell reports that Wittgenstein came with "a refutation of the theory of judgement I used to hold. He was right, but I think the correction required is not very serious. I shall have to make up my mind within a week, as I shall soon reach judgement" (Letter #782 to Ottoline Morrell; my emphasis). Thus: is Wittgenstein's objection-substantially the one made in January, 1913--applicable only to the theory Russell "used to hold"; but avoided by the 1913 version of the multiple

relation theory of judgement? To argue that the objection is <u>not</u> avoided by the 1913 version, I turn now to the detail of the criticisms of May and June, 1913.

For this purpose, Wittgenstein's correspondence with Russell (except R.12) is no longer useful, since there is a gap between March 1913 and June 1913 when Wittgenstein formulated the "exact" expression of criticisms he had made orally in May. Detailed reconstruction has to be inferred from three sources: indirectly, since Russell was still writing the 'Theory of Knowledge' m.s. up to June 1913, he may have tried to incorporate changes in the theory of judgement to accommodate Wittgenstein's objections. In this case, the most valuable source is the three-page m.s. "Propositions"-referred to above--one leaf of which is written on the verso of a rejected p. 197 of the 1913 m.s. Secondly, Wittgenstein sent to Russell in October 1913 a German m.s. from Norway, which is now lost (it is mentioned in R.19, 29/10/13), but of which an English translation by Russell exists, entitled "Wittgenstein", and sub-divided into four parts. This translation was later rearranged by Russell, with new sub-headings, and has been reprinted as the so-called "Costello version" of the 'Notes on Logic' (Appendix I of Notebooks, 1914-1916). Finally, in the Russell Archives, there is the typescript of 'Notes' that Wittgenstein dictated to a stenographer, in Russell's presence, between 2 - 9th October, 1913. Both Russell and Vittgenstein made corrections to this typescript. From these sources, the following passages can be extracted: (for ease of reference I have numbered the passages),

(1) When we say A judges that etc., then we have to

mention a whole proposition which A judges. It will not do either to mention only its constituents, or its constituents and form, but not in the proper order. This shows that a proposition itself must occur in the statement that it is judged; however, for instance, "not-p" may be explained, the question "What is negated?" must have meaning. ("Summary" of 'Notes', p. 2)

(2) In my theory, p has the same meaning as not-p, but opposite sense. The meaning (Bedeutung) is the fact. The proper theory of judgement must make it impossible to judge nonsense. (ibid., p. 4)

(3) The proposition "a judges p" consists of a proper name <u>a</u>, the proposition p with its poles, and <u>a</u> being related to both of these poles in a certain way. This is obviously not a relation in the ordinary sense. (ibid., p. 5)

(4) Every right theory of judgement must make it impossible for one to judge that this table penholders the book. Russell's theory does not satisfy this requirement. (p. 15 of Russell's transl. of German m.s.).

(5) There is no thing which is the form of a proposition, and no name which is the name of a form. Accordingly we can also not say that a relation which in certain cases holds between things holds sometimes between forms and things. This goes against Russell's theory of judgement. ... In aRb, R looks like a substantive but it is not one ... Similarly in $\emptyset x$, ' \emptyset ' looks like a substantive but it is not one; in ' \sim p' ' \sim ' looks like ' \emptyset ' but is not like it. This is the first thing that indicates that there may not be logical constants ... Russell's 'complexes' were to have the useful property of being compounded, and were to combine with this the agreeable property that they could be treated like 'simples'. But this alone makes them unservicable as logical types since there would then have been significance in asserting of a simple that it was complex. But a property cannot be a logical type. ("Costello version", 'Notes', p. 99)

(6) Logical indefineables cannot be predicates or relations. Nor are 'not' and 'or', like judgement, <u>analogous</u> to predicates and relations because they do not introduce anything new. ("Costello version", 'Notes', p. 101)

(7) If the existence of the subject-predicate sentence

Blackwell has interpreted the first three of these passages as embodying two major criticisms of Russell's theory:

Wittgenstein's criticism ... appears to have two major thrusts. One of them is that the theory makes it possible to judge nonsense. The other is that the theory treats expressions of belief, etc., as intensional. (Blackwell, <u>1974</u>, p. 74)

I endorse Blackwell's claim that one thrust of Wittgenstein's criticism was against the fact that Russell's theory does not exclude the possibility of judging nonsense--but I do not believe Blackwell has discerned the source or effect of this objection, or its relation to Wittgenstein's other criticisms. Blackwell continues:

Wittgenstein stated that Russell's theory of judgement could result in a pice of nonsense such as "this table penholders the book". This objection does not seem to apply against the theory advanced in 'Theory of Knowledge' In the formula J $\{S,a,b,R,R(x,y)\}$, "R" is defined as standing for a verb. Since "penholders" is not a verb, it could not be substituted for "R" and produce that kind of nonsensical judgement. (ibid., p. 77)

Blackwell concludes that the "paralysing" effect of Wittgenstein's criticism must derive from its second thrust, which Blackwell interprets as concerning the non-extensionality of propositions occurring in belief-contexts (ibid. p. 79). He adds that, apparently, much of the impact of this 'two-pronged' attack upon Russell "had psychological roots" (ibid., p. 84). It is here that I think that Blackwell's reconstruction goes astray. For I can find little evidence that Russell was very much concerned at the 'discovery' that belief statements apparently have a different logical form, and behave differently, from extensional statements. For, it was <u>Wittgenstein</u>, not Russell, who demanded that all propositions (including those expressing beliefs or judgements) are truth-functions of "elementary" propositions which, themselves, contain only names of simples, and are, thus, extensional (<u>Tractatus</u>, 5.54, 5.5421). That is, for Russell, the problem of the <u>intensionality</u> of belief and judgement contexts (that extensionally equivalent propositions could not be substituted <u>salva veritate</u> in propositions of the form "A believes p", "A judges p") was simply the problem of "enlarging the inventory of logical forms so as to include forms appropriate to the facts of epistemology" (1913 m.s., Part I, Ch. iv). Though he attributes the discovery of this problem to Wittgenstein, there is scant evidence that what Russell referred to as this "difficult and interesting problem of pure logic" (ibid.) could have been the substance of Wittgenstein's paralysing objection.

Thus, if Blackwell is right that the first thrust of Wittgenstein's criticism is not applicable to Russell's 1913 theory, and if Russell found the second thrust innocuous enough to report Wittgenstein's discovery in his 1913 m.s.--without discussing it in detail--we are left with the mystery that Wittgenstein's objection does not seem "paralysing" at all.

What is omitted from Blackwell's account is the fact that Wittgenstein's criticism that "the proper theory of judgement must make it impossible to judge nonsense" (Passage (2)) and his "exact" formulation of the objection to the multiple relation theory: that from "A judges aRb" the proposition "aRb.v. \sim aRb" must follow, "without the use of any further premise" (R.12, June 1913) are really the SAME objection.²⁰ That is, what has been overlooked in the commentaries on Wittgenstein and Russell is that the <u>kind</u> of nonsense with which Wittgenstein was preoccupied in his analysis of atomic propositions through this period--thus, what a proper analysis of atomic propositional judgement must render impossible--is the nonsense that results from violation of type. Russell's 1913 theory does <u>not</u> satisfy this <u>logical</u> requirement; so, the paralysis Russell experienced can be attributed to his realising the following:

(a) that Wittgenstein's objection the multiple relation theory was valid;

(b) that a major effect of the objection was to remove the justification for ramified type theory in the distinctions of kinds of judgement and in the doctrine of incomplete symbols;

(c) that a secondary effect of the objection was to convince Russell that the refinement to the multiple relation theory which demanded acquaintance with logical form as ill-founded;

(d) that a generic effect of Wittgenstein's critical attack was to undermine Russell's conception of the nature of logical propositions, as given in PM, and to lead him to suspect, <u>inter alia</u>, the Axiom of Reducibility and the rationale for ramified type theory; and, finally,

(e) that the formal basis for type theory in PM would need radical revisions to accommodate Wittgenstein's attack upon it.

The first task is to show how the impossibility of judging nonsense,

and the deduction of "aRb .v.~aRb" from "A judges aRb" are the same requirement upon a "proper theory of judgement". There are two questions to be answered:

(i) Why must "aRb .v.~aRb" follow, without additional premises?

(ii) Does Russell's theory satisfy this requirement, or could it be modified to do so?

Several commentators (Pears, Griffin, Blackwell²¹) have noted that to require that "aRb .v.~aRb" follow from "A judges aRb" is equivalent to demanding that, whatever A judges, it be a significant proposition. As described in Section C, Russell had identified 'significance' with 'expressing a true or false proposition', in PM, thus, a judgement is significant when either the judged proposition or its negation holds. This correctly relates the June, 1913 "exact" formulation of the objection to the 'Notes' criticism that a proper theory make it impossible to judge nonsense. But what is <u>wrong</u> with judging nonsense? If judging were regarded simply as a psychological act, there would seem little reason to preclude the possibility that a lunatic may judge (or believe) that he is persecuted by the prime numbers, or that his knife is the square-root of his fork (Pears example). Why should Wittgenstein have held that a "proper analysis" should exclude the possibility of such (albeit 'abnormal') psychological acts?

The answer to these questions, I believe, must be that, in virtue of what Russell <u>intended</u> the multiple relation theory to do--namely, to give an account of the <u>understanding</u> of atomic propositions (and, later, of molecular propositions) upon which to build an account of judgement which <u>justified</u> that ramified heirarchy of judged propositions--his analysis in terms of a multiple relation between a subject and objects of acquaintance must make it impossible to judge a <u>type-mistake</u>. An analysis of 'understanding thatp' must exlude p's being nonsensical, if only because we could not then be said to <u>understand</u> p. In other words, <u>unless</u> the analysis of judgement-- upon which the heirarchy of different kinds of judged propositions is to be built--precludes judgements which violate the type heirarchy, then the ramified theory of types, as a solution to the logical paradoxes, is left without a foundation in Russell's positive theories of propositional meaning. Viewed this way, that "aRb .v.~aRb" follows from "A judges aRb" is the condition for A's judgement <u>not</u> to be a type-mistake; just as Uittgenstein's reason for demanding, in <u>January</u> 1913, that an analysis of "Socrates is mortal" preclude "Mortality is Socrates" is the condition that the constituents of the judged proposition be so analysed as to <u>include</u> typedifferences. Thus, the criticism that it be impossible to judge a typemistake, and that "aRb .v.~aRb" follow without further premises, is one and the same criticism formulated in two ways. This can be demonstrated:

By *9.61 of PM, if $\emptyset \hat{\mathbf{x}}$, $\emptyset \hat{\mathbf{x}}$ are <u>elementary</u> propositional functions (containing no apparent variables) of the same order (all arguments are individuals), then there is a function ' $\emptyset \hat{\mathbf{x}} \cdot \mathbf{v} \cdot \vartheta \hat{\mathbf{x}}$ ' (a function of \emptyset and ϑ). This holds also for elementary dyadic relations ' $\hat{\mathbf{x}} \hat{\mathbf{R}} \hat{\mathbf{y}}$ '. Then, in PM (p. 171), we have the following discussion:

The following proposition is useful in the theory of types. Its purpose is to show that, if a is any argument for which " \emptyset a" is significant, i.e. from which we have \emptyset a .v. $\sim \emptyset$ a, then " \emptyset x" is significant when, and only when, x is either identical with a or not identical with a. (PM, *13, p. 171, my emphasis)

The proposition mentioned is:

*13.3 — :: $\emptyset \mathbf{a} \vee \langle \emptyset \mathbf{a} \rangle$ \Im :. $\emptyset \times \vee \langle \emptyset \times \rangle =$: $\mathbf{x} = \mathbf{a} \cdot \mathbf{v} \cdot \mathbf{x} \neq \mathbf{a}$ Equivalently, for a dyadic function ' $\mathbf{x} \mathbf{R} \mathbf{\hat{y}}$ ', we have:

 $\begin{array}{c} \begin{array}{c} & & \\ & & \\ & & \\ \end{array} : \ aRb \ v \sim aRb. \ \bigcirc : \ xRy \ v \sim xRy. \ \Longrightarrow : \ x=a \ . \ y=b \ .v. \ x\neq a \ . \ y=b \ .v. \\ & & \\ & & \\ & & \\ x=a \ . \ y\neq b \ .v. \ x\neq a \ . \ y\neq b \end{array}$

Thus, in any judgement that aRb, a,b are significant arguments to 'xRŷ' only when the antecedent to this conditional is satisfied. That is, for a, b to be of the same type, it must be revealed in the analysis of "A judges aRb" that a, b are significantly related by R (i.e. that $\hat{x}R\hat{y}$ is confined to arguments of a definite type). And a, b are significantly related by R only when "aRb .v.~aRb" follows directly from what is judged. Were this not to follow, then the antecedent of *13.3 could be satisfied, though the consequent were false. The falsity of the consequent would require either the falsity of "xRy .v. \sim xRy", which is impossible, or the falsity of "(x=a . y=b) v (x \neq a . y=b) v (x=a . y \neq b) v (x \neq a . y \neq b)". The falsity of this last demands the falsity of each disjunct -- whereupon there would be no items a, b such that aRb or not-aRb, contradicting the supposition that the antecedent is satisfied. That Wittgenstein required that the antecedent of *13.3 be derived without further premises, from the analysis of judgement, amounts to the requirement that if the proposition "A judges aRb" is to be an elementary judgement (at the base-level of the heirarchy of orders) then its analysis, alone, must show that the items related in the judged proposition are of the appropriate type-level. Otherwise, that 'aRb' is elementary would depend upon the truth of another judgement -- that a, b are individuals -beginning an 'endless regress' (viz.post). For the judgement to be elementary, the items a, b must be individuals, and the relation R must be defined only over individuals -- and this must be recognised from the form of the atomic complex which makes the elementary judgement true, if it is true (or the 'opposite' complex, if it is false).

A moment's reflection shows that Russell's 1913 formulation of the

multiple relation theory does not meet this requirement -- nor can it meet the requirement without introducing independent reasons for the function 'xRŷ' to be significant only over individuals. This introduces a vicious circularity. That the judgement that aRb was an elementary judgement (corresponding, if true, to a perceived complex 'a--R--b') was to be the reason for classifying the function ' $\hat{x}R\hat{y}$ ' as a "predicative, first-order function of individuals" (PM, p. 162). The order of this function is fixed by the fact that, to understand it, we presuppose only the totality of elementary propositions 'aRb', 'cRd',... (see: PM, p. 39). Thus, that the type of the arguments to $'\hat{x}R\hat{y}'$ is that of individuals depends upon our understanding ' $\hat{x}R\hat{y}$ ' as presupposing only the totality of elementary atomic propositions of the form "xRy". This, in turn, depends upon the incomplete symbol "xRy" expressing a definite, elementary proposition when asserted of each pair of individuals a,b; c,d; ...; or judged true of those individuals. That is, the type of significant arguments to $\widehat{x}R\widehat{y}$ depends upon the kind of judgement made in asserting $\hat{x}R\hat{y}$ of a, b, and so on. And the kind of judgement thus made depends upon what type of argument is related by R in judging that aRb.

This circularity is vicious. There is nothing in Russell's analysis of "A judges that aRb" which guarantees that a, b are of the same type--and further, there is nothing to secure the <u>order</u> of ' $\hat{x}R\hat{y}$ '. The analysis Russell gives is that "A judges aRb" holds when the multiple relation J holds between A,a,b,R and the logical form ' $x\phi y$ ' (the form of a dual complex); i.e.

J {A, a, b, R, $x \phi y$ }.

In this judgement-complex, a, b, and R are merely named objects of acquaintance. This explains Wittgenstein's contradicting Russell (in Passage (5), above) that "in aRb, R looks like a substantive, but is not one". Qua nameable items of acquaintance, the relation R is just as much an individual as a, b. If the type similarity of a, b is to be shown in the analysis of judgement, together with the order of the relational function $\widehat{\mathbf{x}} \widehat{\mathbf{R}} \widehat{\mathbf{y}}$, then it seems that the only candidate for this role is the logical form ' $x \phi y$ ' of the atomic complex 'a--R--b'. This, too, is a named item of acquaintance-prompting Wittgenstein's ironic comment that Russell's 'complexes' were to share the "agreeable properties" of being both simple and complex (Passage (5)). If the logical form ' $x\phi y$ ' is to display that permissable arguments to $\hat{x}R\hat{y}$ are to be of the same type, then this must be a property of the complex 'a--R--b' of which 'xØy' is the form (inducing Wittgenstein to complain, in (5), "But a property cannot be a logical type!"). What 'property' of the perceived complex 'a--R--b' would reveal that a, b are of the same type? Are we acquainted with type-differences? There is nothing in Russell's explanation, in the 1913 m.s., of "acquaintance with logical form"--with the form of a dual complex -- which could account for this 'property'. In his discussion (1913 m.s., Part I, Ch. vii "On the Acquaintance involved in our Knowledge of Relations), Russell is concerned only to show how acquaintance with the form of the complex preserves the "sense" of asymmetrical relations as proceeding from x to y, and not from y to x. Later in the 1913 m.s., Russell is concerned to insist that, inbeing acquainted with the form 'x ϕ y', one is acquainted with the "utmost generalisation" of the proposition that aRb; i.e. the "pure form" of aRb is ' $x \phi y$ " in which each

of the three <u>names</u> "a", "R" and "b" has been replaced by a <u>real</u> variable (1913 m.s., pp. 132-5). As named items, however, a, R and b would all be of the same type with respect to the multiple relation 'J(x,y,z,u,v)'--and, now, the type-heterogeneity of a, b and $\hat{x}R\hat{y}$ has been lost in going from the complex 'a--R--b' to the form ' $x\phi y$ '. The problems for ramified type theory are multiplying:

When we say A judges that etc., then we have to mention a whole proposition which A judges. It will not do either to mention only its constituents, or its constituents and form, but not in the proper order. This shows that a proposition itself must occur in the statement that it is judged;.... (Passage (1))

Later still, in the 1913 m.s., in explicating the self-evidence of logical propositions, Russell commits himself to saying that acquaintance with logical form guarantees the <u>logical</u> ("completely general") truth of the assertion that something has some dual relation with something: (this point has been made by Pears, 1977, p. 180)

The importance of the understanding of pure form lies in its relation to the self-evidence of logical truth. For, since understanding is here a direct relation of the subject to a single object, the possibility of untruth does not arise. (1913 m.s., p. 250)

Now it seems that acquaintance with pure form is acquaintance with a <u>simple</u>-it has become wholly mysterious how such an acquaintance could guarantee the type-sameness of a, b, or even that $\hat{x}R\hat{y}$ is of a definite order. Here, Wittgenstein's criticism of the "complete generality" of logical truths (discussed above) comes to the fore. Pure forms cannot be completely general in containing only <u>real</u> variables, because, then, " $x \phi y v \sim x \phi y$ " (c.f. *13.3) would have to be a <u>proposition</u> of logic; whereas it is not a proposition at all.

In fact, Wittgenstein's criticism, in these passages cited above, goes deeper than this: Russell had suggested that acquaintance with pure form--completely generalised complexes--is acquaintance with a <u>single</u> object, because pure forms have no constituents; i.e. no <u>named</u> objects (1913 m.s., p. 243). This leads him to say that the understanding involved in judging a <u>logical</u> proposition is a <u>dual</u> relation between a subject and a single object--the form (1913 m.s., p. 347). But completely generalised complexes cannot be regarded as simple objects merely because they have no constituents. The <u>original</u> reason for introducing such a form as 'xØy' into the analysis of judgement was to guarantee that the arrangement of objects in the judgement that aRb would be isomorphic with the relational fact a--R--b, through a's being in the x-place, R in the Ø-place and b in the y-place. But what is isomorphic with a complex cannot be simple (Wittgenstein's Passage (5)).

Wittgenstein proceeded to extend these criticisms of Russell's use of 'logical forms' into ramified type theory, itself--as formulated in PM. In a letter closely after the June 1913 report of the "exact" formulation of the objection to the theory of judgement, Wittgenstein applies the above criticism of the "complete generality" of pure forms to the Axiom of Reducibility, and, later, to the Axiom of Infinity. These applications of the objection, viewed as concerning the nature of logical propositions, demonstrate clearly the devastating effects on the theory of types. In addition, that Wittgenstein should have <u>next</u> discussed the Axiom of Reducibility indicates his preoccupation <u>throughout</u> this period with the justification
for type theory. The first such letter opens with the following:

Your axiom of reducibility is $\vdash :(\exists f): \emptyset x \equiv_x f!x$; now is this not all nonsense as this proposition has only then a meaning if we can turn the \emptyset into an <u>apparent</u> variable. For if we cannot do so no general laws can ever follow from your axiom. The whole axiom seems to me at present a mere juggling trick....The axiom as you have put it is only a schema and the real Pp (primitive proposition) ought to be $\vdash :.(\emptyset):(\exists f): \emptyset x \equiv_x f!x$, and where would be the use of that?! (R.15, dated by Von Wright "Probably Summer 1913).

Where indeed would be the use of that? Nittgenstein is clearly referring to PM, *12.1---the Axiom of Reducibility for monadic functions--and has uncovered an inconsistency in the use of the axiom in ramified type theory. The Axiom has been frequently criticised, of course; many of those criticisms have been discussed, above, in Section C (the most important criticism concerning the axiom's role in the logicist reconstruction of real number theory being that of Ramsey, 1925). Few commentators, however, have noticed that, as early as 1913, Wittgenstein criticised the axiom on grounds that, in effect, are those of its later critics: namely, that, intuitively, the axiom does not appear to be a logical truth (that it is contingent); and that the axiom collapses the distinctions of order that ramified type theory built up, (c.f., for example, Quine, 1963, p. 251). As stated in Section C, the axiom is needed to guarantee that, wherever we need to make assertions involving functions of varying orders (whence the assertions, themselves would be of different orders), there will always be an equivalent assertion involving predicative functions only. Thus, *12.1 asserts that, where " $\emptyset \hat{\mathbf{x}}$ " is any monadic function, of any order, there is a predicative function "f!x" which, for every value of x, is equivalent to

asserting $\emptyset \hat{\mathbf{x}}$ of x. The inconsistency Wittgenstein has uncovered can be seen as follows: elsewhere in PM (pp. 162-163), Russell required that all propositions be obtained from matrices by generalisation (see: Section C,). To obtain a proposition, by generalising, <u>all</u> variables in the p. matrix must be turned into apparent (bound) variables. The 'Primitive proposition', then, which states the Axiom of Reducibility (*12.1) is, thus, not even a proposition, because it contains the real variable " ϕ ". And it is essential to the axiom, as stated, that " ϕ " remain a real (free) variable. For, the axiom is intended to assert that any monadic function, of whatever order, is equivalent, for all values, to a predicative function, all of whose arguments are of the same type. To turn *12.1 into a proposition, by generalisation, would require that " \emptyset " be turned into an apparent variable -- as Wittgenstein notes. Once we do that, however, the axiom is confined to a specific order--namely, one greater than the order of the value-range of $\emptyset \hat{\mathbf{x}}$. Since the revised axiom no longer holds generally, but has to be reasserted at each order, for each function $\emptyset \hat{\mathbf{x}}$, and must mean different things at each other (by the systematic ambiguity of "Øx" across orders), then there is little sense in regarding the axiom as a "completely general" proposition of logic, at all. Where would be the use of an axiom which was confined to each separate order in the ramified heirarchy?

Within a few months, Wittgenstein has extended this criticism of the "completely general" status of the Axiom of Reducibility to the Axiom of Infinity. Like the Axiom of Reducibility, the Axiom of Infinity is required to preserve the definability of arithmetical concepts in PM. Roughly stated, the Axiom asserts that there are at least $N_{\rm o}$ (denumerably

many) things. Russell expressed grave reservations in the <u>logical</u> truth of the axiom--apparently under Wittgenstein's prompting that, since e.g. " $(\exists x)$. x = x" (there is at least <u>one</u> thing) is "really a proposition of <u>physics</u>" (Letter R.23), so:

The same holds for the Axiom of Infinity: whether there exist \mathcal{N}_0 things is a matter for experience to determine ... But, now, as to your Axiom of Reduction (Reducibility): imagine we lived in a world in which nothing existed except \mathcal{N}_0 things and, over and above them, ONLY a single relation holding between infinitely many of the things and in such a way that it did not hold between each thing and every other thing and further never held between a finite number of things. It is clear to me that the axiom of reducibility would certainly <u>not</u> hold good in such a world. But it is also clear to me that whether or not the world in which we live is really of this kind is not a matter for logic to decide.. (R. 23, November or December, 1913).

To the extent that such a world is logically possible, yet lacking in the requisite predicative functions to be equivalent to the infinitary relation, then the axiom of reducibility is contingent. And, if the possibility of such a world is not decided by logic, then, also, logic cannot decide whether there are infinitely many things. (Note: where we confine <u>logic</u> to first-order predicate logic, Wittgenstein's argument, here anticipates the modern Compactness Proofs for a logical system: that there is no set of sentences of the logic which, though all finite subsets of the set are satisfiable, the (possibly infinite) set is not itself satisfiable).

The overall effect of the above discussion of Wittgenstein's criticisms has been to show that, in relation to the <u>specific</u> criticism that type-differences are unexplained in Russell's analysis of judgement, there is a massively penetrating series of general consequences of the criticisms which Wittgenstein drew in sustaining his attack on ramified type theory. Wittgenstein returns, again and again in the 'Notes on Logic' and, later, in the Notebooks, 1914-1916, to these questions concerning 'logical form', how it could not be a 'constituent' of judged or asserted propositions, how type-differences could not be judged or asserted of the facts (complexes), or constituents of facts, to which judged propositions correspond (see: Passage (7)); and, in the end, how a Theory of Types must be impossible, because it tries to state what can only be shown in the symbolism for propositions (that the statements of type theory are self-refuting). Some of these general consequences I discuss in Section D. In concluding this Appendix, however, I shall concentrate only upon those specific effects of Wittgenstein's criticisms for which there is some evidence that Russell perceived them. Thus restricted, the effects are nevertheless devastating in their scope (they involve many of Russell's logical and epistemological doctrines) and in their penetration (leading Russell to despair that "what wanted doing in logic was too difficult for me" (Letter to Ottoline Morrell, 1916, quoted in Russell, 1967, vol ii. p. 57)).

Wittgenstein's specific illustration of the nonsense permitted by Russell's analysis of judgement is not perspicuous--so we should not suppose that Russell perceived immediately the relationship between the criticism and the ramified theory of types. In Passage (4), Wittgenstein requires that a "right theory of judgement must make it impossible for me to judge that this table penholders the book." Blackwell (loc. cit. p. 77) has argued that Wittgenstein was wrong if he believed this example could be generated from Russell's 1913 theory. He notices that, in the formula, J {S, a, b, R, $x \phi y$ }.

"R" is defined as standing for a verb. He continues "Since 'penholders' is not a verb, it could not be substituted for 'R' and produce that kind of nonsensical judgement" (ibid). The response misses the point--which is not that 'R' has to be a verb, which 'penholders' is not--but that, as a constituent of the complex 'a--R--b', R is being singled out as a nameable item (object of acquaintance). As such, it is just as "individual" as a penholder, table or book. Wittgenstein's point is that, for the primary relation "J" (or "U" in the case of understanding (to relate the constituents of the complex to the subject, we have to suppose that J is a many-place relation (indeed, J has to have as many places, plus two (subject + form), as there are constituents of the complex). Since Wittgenstein recognises (by Passage (6)) that, like 'not', 'or' and logical signs, such a relation as is expressed by "J" must extend over heterogeneous types (i.e. be systematically ambiguous), then this cannot be "a relation in the ordinary sense" (Passage (3)), unless we suppose that its relata are typically homogeneous. But that supposition collapses type-differences. Indeed the judging relation "J" simply cannot be systematically ambiguous across types if the differences in kind between judgement-complexes is to justify the classification of judged propositions into the heirarchy of orders (PM, pp. 44-45). This is the most direct effect of Wittgenstein's criticism.

That Russell perceived immediately this effect of the criticism is difficult to discern--though there are indications in the three-page m.s. "Propositions" (Archives #220.011440) which suggest that he did. The "Props" m.s. (as I shall refer to it) is evidently an attempt by Russell to accommodate some aspects of Wittgenstein's criticisms. Blackwell dates the m.s. close after Wittgenstein's second meeting with Russell--27/5/13-because its first-page appears on the verso of a page of the 1913 m.s. (p. 197)--a point in writing at which Russell may well have arrived by late May, 1913. "Props" begins with discussion of the correspondence between a judged proposition and the positive or negative fact (complex) which verifies or falsifies it.

Three objects x, R, y form one or another of 2 complexes xRy or \sim xRy. The prop(osition) xRy points to either indifferently: both contain nothing but x and R and y ... It <u>looks</u> as if there actually were a rel(ation) of x and R and y whenever they form either of the 2 complexes and as if this were perceived in understanding. If there is such a neutral fact, it ought to be a constituent of the +ve (positive) or -ve (negative fact.. ("Props", p. 1)

Russell is grappling, here, with a decisive point Wittgenstein makes later (in published 'Notes') in connection with negation (see: Passage (1)). Referring directly to Russell's theory of judgement, Wittgenstein argues

"... a proposition itself must occur in the statement that it is judged; however, for instance, 'not-p' may be explained, the question 'What is negated?' must have meaning". (Passage (1)).

One of the chief advantages of the multiple relation theory of judgement-as Russell perceived it--was that it dispensed with true or false propositions as <u>objects</u> of judgement, belief, understanding, and so on. Much of the 'paralysis' Russell experienced, indeed, may have resulted directly from his recognition that, if the proposition had to be re-introduced as a genuine constituent of judgement, belief or assertion, all of his previous problems (pre-1905) over the 'subsistence' of false propositions,

as "transcendent" objects, might recur. By 1913, of course, Russell's old realist theory of the proposition had given way to the doctrine of incomplete symbols -- that apparent referential committment to propositions denoted by phrases like "That Socrates is human", "The discovery of the atom", "The belief that angels exist" could be contextually eliminated by analysis in terms of the Theory of Descriptions. Indeed, the multiple relation theory supplements the doctrine of incomplete symbols in purporting to show why, after analysis, an elementary proposition becomes nothing more than an incomplete symbol which corresponds or fails to correspond to a perceived complex in being judged or asserted. Since judgement and belief no longer needed a 'special' object, and the Theory of Descriptions accounted for other apparent references to propositions as 'special' true or false objects, Russell could conclude, so he thought, that he could dispense with commitment to such 'mysterious' entities as true and false propositions. The "Props" m.s., however, indicates clearly that Russell is dismayed to find that propositions may have to be re-instated to account for negated judgements-that something (a "neutral fact") must correspond to what is understood in order for a negative judgement to be true:

It <u>looks</u> as if there actually were always a relation of x and R and y ... and as if this were perceived in understanding ... (ibid).

The notion of a 'neutral fact' to which a judged proposition (positive or negative) points to "indifferently" is fraught with difficulty. First, though, it is worth indicating why Russell may have felt compelled, by Wittgenstein's criticisms, to contemplate such a notion. The key, I believe, is 'negation', and Wittgenstein's earlier discussions of the 'meaning' of logical signs.

Suppose that p' is a proposition of a definite order (say "elementary"). Russell had regarded 'not-p' as a function of p of order one greater than that of the values which result from replacing the variable in "Not $(\hat{\mathbf{x}})$ " by a propositional sign (incomplete symbol). Judging that not-p' is thus the assertion of the function Not $(\hat{\mathbf{x}})$ of a permissable arguement "p'". (Recall that "assertion of a propositional function" is a primitive idea of the first edition of PM). How would "A judges that notp'" be analysed by the multiple relation theory, and in what relation would it stand to "A judges that-p'"? Suppose p' is elementary and of the form "F(a)". Then "A judges that-F(a)" is given the form:

(a) $J \{A, a, F, \phi(x)\}$.

On the other hand, the judgement that not-p' is what is made in asserting Not(\hat{x}) of p' (c.f. PM, p. 6--the Contradictory function). This must be a different <u>kind</u> of judgement from (A), since a propositional function is involved essentially. Now, Russell did not give an account of molecular propositional judgement in the 1913 m.s.--it was scheduled to be discussed in the abandoned Part III. I surmise, though, given the primitive idea of PM of "assertion of a function", that the Contradictory function Not(\hat{x}) would have to enter into this judgement as a constituent:

(b) $J \{A, a, F, Not(\hat{x}), f(\phi(x))\}$.

The logical form " $f(\phi(x))$ " differs from that in (A) since it is the generalised form of the complex 'not--a--F' which corresponds to the value of the Contradictory function for the argument "F(a)".

The inadequacy of this account is clear--as is the ground of

Wittgenstein's criticism that it should always be possible to ask "What is negated?" Unless the function 'Not(\hat{x})' is asserted <u>of</u> a definite argument (namely, the proposition which <u>is</u> negated), it is unclear how Russell could get from the different analyses (A), (B) to the obvious requirement that, in judging that not-p', we judge not to be the case whatever it is that we would have judged in asserting, instead, p'. That is, there must be some answer to the question "What is negated?"

"Props" attempts to resolve this difficulty by supposing that judgement ...

... involves the neutral fact. Judgement asserts <u>one</u> of these (positive or negative) facts. It will still be a multiple relation but its terms will not be the same as in my old (1913) theory. The neutral fact replaces the form. ("Props", p. 2)

On this view, we are no longer acquainted with "pure forms" but with neutral facts which are "positively" or "hegatively" directed to whichever of the positive or negative facts obtains. Calling "+J(xRy)" the judgement of the positive fact that xRy--"+(xRy)"; and "-J(xRy)" the judgement of the negative, Russell continues:

The correspondence in judgement is between

+J(xRy) and +(xRy) -J(xRy) and -(xRy) +J(xRy) and +(xRy). ("Props", p. 3)

Soon, however, Russell recognises the hopelessness of the modification:

This correspondence, however, entails the old difficulties: it seems not intimate enough. And it suggests dangers of an endless regress. It can't be quite right.

There will only be a neutral fact when the objects are of the right types. This introduces great difficulties. ("Props", p. 3)--my emphasis. The underlined passage, above, indicates that Russell perceived the problem that type-differences are unexplained in his analysis of judgement--though it gives no clue as to how extensive Russell perceived those "great difficulties" to be. In any case, the suggested modification--introducing 'neutral facts' to replace 'logical forms' as constituents of judgement-brings back problems which the multiple relation theory tried to avoid (the "old difficulties"). The lack of 'intimacy' in the correspondence, to which Russell refers, is, I believe, the problem Russell found with "Heinongian" explanations of the truth or falsity of judgements as constituted by a relation between a mind and an 'Objective' (discussed, <u>inter</u> <u>alia</u>, in Russell, <u>1910b</u>, pp. 149-152):

If we allow that all judgements have objectives, we shall have to allow that there are objectives which are false ... (This) leaves the difference between truth and falsehood quite inexplicable. We feel that when we judge truly some entity 'corresponding' .. to our judgement is to be found outside our judgement, while when we judge falsely, there is no 'corresponding' entity.

This "old difficulty" is a dilemma one horn of which is "Plato's Beard": if judgement is a relation of mind to fact, to judge falsely is to stand in a relation to nothing. But we cannot "be related to what is not". The other horn of the dilemma is that, if there are 'objectives' corresponding to false judgements, they are just as "objectively real" as objectives of true judgements. The difference between true and false judgement remains unexplained.

The modification of the multiple relation theory suggested in "Props" threatens a recurrence of the above dilemma. (I am not convinced that the dilemma is genuine; only that Russell thought it so). If the negative judgement is a correspondence between a subject and, amongst other things, a neutral fact, how are we to distinguish the manner in which the neutral fact 'points to' the corresponding positive or negative fact (negative if the judgement is true, positive if it is false). The intimacy of the correlation between <u>actual</u> items of acquaintance and constituents of perceived complexes has been lost.

An additional threat posed by the "Props" modification is the recurrence of an "endless regress" in analysis, which Russell had tried to avoid in the original 1913 theory. If the neutral fact is to be a constituent of judgement replacing the 'logical form' then:

There would have to be a new way in which it, and the ... other constituents are put together, and if we take this way again as a constituent, we find ourselves embarked on an endless regress. (1913 m.s., pp. 182-3)

The context from which the above quotation is taken is one in which Russell is denying that a "logical form" is just another "constituent" of the judgement--it is a "logical object" with which we are acquainted, but it is not a "thing" (ibid. p. 186). The distinction between "logical object" and "thing" is not a clear one--but, in any case, replacing the logical form by a "neutral fact" abolishes the "logical objecthood" of this latter item and re-introduces the threat of an endless regress.

Finally, the whole modification of the theory involving facts as neutral between positive and negative, as Russell perceives, does not even approach the problems concerning the type-differences between the constituents of judgements which I have discussed in detail, above. I have argued, there, that there is every reason to suspect that what Russell regarded, sometime late in May, 1913, as introducing "great difficulties"--the lack of an account of type-differences in the multiple relation theory--was precisely what induced his 'paralysis' reported in June to Wittgenstein, at about the time he abandonned work on the 'Theory of Knowledge' and fell into despair.

To sum up, it may suffice to note that, of the changes in PM between the first and second editions (1910-1927)---which are discussed at the close of Section C---many can be attributed directly to that sustained critical attack on ramified type theory which Wittgenstein began in 1913 with the criticisms of the theory of judgement. The changes need only be listed to record their debt to Wittgenstein, and to indicate the extent to which Russell revised his logical and epistemological views to accommodate what he understood of Wittgenstein's new doctrines as to the nature of logical propositions, the meanings of logical signs, the generality of logic, the basis of type theory, and the contingency of the Axiom of Reducibility:

(i) In the introduction to the second edition (p. xiii), Russell explicitly attributes to Wittgenstein the recommendation to abolish real (free) variables from the propositions of PM--the chief significance of this change being noted to be its effect upon the account of 'general judgements' (involving quantifiers), in the ramified theory of types.

(ii) Russell also explicitly attributes to Wictgenstein the change in attitude towards the Axiom of Reducibility. As discussed in Section C, an intuitive justification of the axiom was offered in the first edition (p, 58). This is abandonned in the second edition and replaced by the comment "This axiom has a purely pragmatic justification: It leads to the desired results and no others" (PM, Second Edition, p. xiv)

(iii) Wittgenstein's "extensionality thesis" (that functions of propositions are always truth functions) is

recommended as an <u>alternative</u> to the axiom of reducibility-and Appendix C is added to the second edition to examine some of the difficulties attending this thesis (amongst which is that "it requires us to maintain that 'A believes p' is not a function of p" (p. xiv). The alternative, of dropping the axiom, but retaining intensional propositional functions--an alternative embraced by Chwistek in "The Theory of Constructive Types" (1923)--is said to "compel us to sacrifice a great deal of ordinary mathematics" (ibid., p. xiv).

What is to take the place of the axiom of reducibility in the second edition is the assumption that "a function can only occur in a propositional matrix through its values" (p. xxix). The effects of this fundamental change in the theory of propositional functions upon the theory of types is discussed more appropriately in relation to Ramsey's simplification of type theory (above, Section C). It has been shown, however, by Myhill (<u>1974</u>), that this amendation runs into severe difficulties when, in Appendix B to the second edition, Russell attempts to save inferences by mathematical induction from failure, through the lack of predicative functions of the requisite orders, once the axiom of reducibility is removed. It can thus be said that the ramified theory of types never recovered from the attack upon it begun by Wittgenstein in 1913.

Appendix (B); Type/token, Use/mention and quotation

(I) Of any given linguistic expression, one might wish to talk about, remark on, mention or refer to its syntactic features or roles, its semantic properties, its ordinary use or its pragmatic effects. In all these cases, one may wish to distinguish the expression qua mark, inscription or sequence of sounds (token-expression) from the expression qua symbol which has such tokens as instances (type-expression). Quotation marks, which are commonly employed to signal the difference between talking about (mentioning) and using an expression, are typically deployed in logic only in contexts where purely syntactic features of expressions are being mentioned. As such, they prove unsuited to the variety of features which can be attributed to an expression (syntactic, semantic, pragmatic) and ambiguous as between token and type. Such ambiguity is removed by the widely insisted upon practice, in logic, of construing quotation-marks as forming, in all cases, a proper-name of the linguistic expression to which some feature is being ascribed. Thus, for example, an utterance of the sentence:

(a) Nuts are nutritious.,

in a context, may make a statement; whereas, the occurrence of line (a) in:

(b) "Nuts are nutritious" is grammatical.,

is not an occurrence of (a) as a sentence but as an improper part of a name--'"Nuts are nutritious"'--which is formed by concatenating first a left-hand quotation-mark '"', then an 'N', then a 'u',, and concluding with a '"'. A name so constructed stands for the linguistic expression (qua

i

syntactic object) appearing on line (a), i.e. the sentence. As such, what appears on line (a) is no more a functioning part of the name on line (b) than "hill" is a word functioning as part of the name "Churchill". This difference is explained, then, in saying that, whereas the sentence "Nuts are nutritious" is <u>used</u> on line (a) (perhaps to make a statement), the sentence is mentioned on line (b) by a proper name for that sentence, i.e. '"*N*u*t*s* *a*r*e* *n*u*t*r*i*t*i*o*u*s*"'--where '*' represents a concatenation-operator.

In brief, this standard account yields the following principles governing the mentioning of expressions by means of quotation¹:

- A. No expression is both mentioned and used at the same time.
- B. All mentioning is of one sort--picking out a syntactic object (letter, word, phrase or sentence) by means of a name.
- C. Uniform syntactic mention is best accomplished, notationally, by setting an expression within quotation-marks to form a quotation-expression.
- D. Quotation-expressions (formed by quoting and concatenating) are propernames for the expressions within the quotation marks.

All of A-D are false. Moreover, their persistance as a standard view of use/mention serves only to obscure complex issues which concern the proper bearers of semantic properties like truth, significance and semantic success.

A general objection to the account embodied in A-D concerns its insensitivity to the type/token distinction. Consider a number of common examples in which expressions are mentioned, recognised in ordinary language by setting the appropriate expression within double-quotes:

- (a) Arrange "table", "chair", "cupboard" in alphabetical order.
- (b) (i) Give a synonym for "brother".

- (ii) Translate "Der Schnee ist weiss".
- (iii) Do you understand what I mean when I say: "Eventsare uniquely ordered under time"?
- (c) Galileo said "The earth moves".

Each of these examples is ambiguous with respect to type or token. In (a), do we arrange three tokens, or three types of which the instances mentioned are tokens, or three other tokens of the <u>types</u> mentioned in (a)? What would be the difference between these tasks? Clearly, the quoted expressions in (a) are not being used—I am not being asked to arrange furniture—moreover, only syntactic features are being mentioned; for, the command can be obeyed without knowing what the mentioned expressions mean, or even what parts of speech they are.

In the (b) group examples, however, type/token ambiguity engenders more disquiet. In (b)(i), am I to find a synonym for the <u>token</u> mentioned-as if it were to be unambiguously used in a context, or do I give synonyms only for a <u>type</u> of which (b)(i) mentions a token, conceding that, in this case, the type is (out of context) ambiguous? Similarly, am I to translate a German type in (b)(ii), inferring thereby that every instance of that type is univocally correlated with the English type of which my particular translation is a token? Or, do I translate a <u>token</u> and rely upon the (questionably) normal contextual invariance of translation to ensure that there is some English type which univocally translates the German type?

(b) (iii) poses a more complex question. Under the assumption that the <u>actual</u> semantic features of the mentioned utterance may diverge markedly, in its context, from the context-free semantic features of the utterancetype, then answering the question posed in the example seems to require

that I be taken as mentioning an actual token. But which token? It is a poor joke to reply to my asking: "do you understand what I mean when I say 'S'?", by saying: "Well, go ahead and say S; I will see if I understand!" For, the context, one supposes, is one in which my asking that question requires that you interpret the mentioned sentence as if I had used it. And, what I use, on a given occasion, is an utterance-token. Can a token, however, -- construed as a single unrepeatable event--be mentioned in a context like (b)(iii)? or, should what is mentioned in (b)(iii) be identified as another token of the same type as the token I would have used if I had said, simply, "Events are uniquely ordered under time"? In either case, to answer my question, you have, as it were, to read inside the quotation-marks to assess the significance of the token I have mentioned. This marks a difference between examples (a) and (b). For, though in all the examples being considered words and sentences are mentioned, neither the commands in (b) are fulfilled nor the question answered unless non-syntactic features of the mentioned expression are taken into account. The syntax of each mentioned expression (whether token or type) is, of course, pertinent to fulfilling the command or answering the question; but it is so precisely in the way in which the syntax of any expression as used contributes to the semantic features of the expression in context, along with the contribution of contextual and pragmatic features.

Compare the above [(b)(iii)] with (b)(ii): in the latter, the word "brother" is clearly being mentioned, but we cannot <u>give</u> such a synonym as is requested unless we <u>read inside the quotation</u> as if the word (token) were being used in a determinate context (one which disambiguated "brother"

between "male sibling", "member of a monastic order" and "member of a guild or trade-union").

It follows that the mentioning in (b) examples cannot be merely syntactic. For the mentioned items, <u>qua</u> syntactic objects, simply do not have those features which would accompany an appropriate <u>use</u> of the expression mentioned. Consequently, it would become entirely mysterious how we fulfill such commands or questions simply by addressing the mentioned items as syntactic objects. In such examples, the customary sharp distinction between use and mention cannot be sustained. What is involved in these cases of what I call 'semantic mentioning' pertains to features of the <u>use</u> of the mentioned expression. This, at least, suffices to refute thesis B of the standard account: that all mentioning is of one sort; namely, 'syntactic mention'.

Examples like (c) introduce a further complication for the standard account. Clearly, in directly reporting Galileo, we are not mentioning his actual token-words, nor are we mentioning a <u>type</u> of which Galileo's words were a token. For Galileo did not speak English. Are we, then, mentioning a translation-type of his token? or something else, i.e. the statement he made? It is highly implausible to claim--as on the standard view--that, if we mention a translation (token or type), the quotation expression in (c) functions as a proper name. For, it is not inconceivable that the circumstances of the utterance of (c) may have been the very <u>first</u> appearance in English of "The Earth moves" as a translation of "Eppur si muove". As such, the name '"The earth moves" has to be construed as naming a type which has not been independently used, or a token which appears for the first time

within those same quotation marks. In a sense, such a token or type does not (yet) exist and the name "the earth moves" is empty (just as 'Elizabeth I of England' is empty prior to 1559). Alternatively, the token occurrence of 'the earth moves' within that quotation-expression has to be construed as a significant use, as a translation, within a context in which it is being mentioned (as with a baptismal act). To do so, however, is to admit that such an occurrence is one in which an expression is both mentioned and used, at the same time. To the extent, then, that the analogy between quotationexpressions and proper-names breaks down in such cases, thesis D. of the standard account: that quotation-expressions function as names, is rendered objectionable.

Thesis C.: that uniform mentioning is best accomplished by means of quotation-expression, can be accepted, of course, if purely 'syntactic mention' of the kind illustrated in (a), is distinguished from other varieties of mentioning, and quotation is reserved for this task. C. is false, however, if taken in conjunction with B., since the use of <u>one</u> device for mentioning expressions will then be ambiguous between syntactic and semantic mention.

It might be claimed, in response to these criticisms, that much of the unclarity surrounding 'use' and 'mention' can be removed by specifying more exactly what type-expressions are, what tokens are, and what relations hold between them. In so doing, however, it has to be conceded that, if there is a genuine distinction to be drawn between type and token, then there is a consequential distinction between mentioning a type and mentioning a token, which the standard account of quotation, allegedly capturing the difference between use and mention, fails to mark. Reichenbach $(\underline{1947})$, for example, has avoided this shortcoming by supplementing the standard view with 'token quotes' (forming a name of a token-occurrence of an expression). Such an <u>ad hoc</u> repair to the standard view, unfortunately, does not explain <u>how</u> we decide which kind of mentioning applies in any given case.

R. Routley and L. Goddard, in a recent article (Routley and Goddard, <u>1966</u>), have offered an alternative account of the use of "quotationexpressions" (expressions occurring within quote-marks, or within the scope of a quotation-operator--in contrast to the 'quotation-names' of the standard account). It is their view which I summarise briefly, below. In addition, I offer replies to the customary criticisms of the use of quotation-operators in formal languages, which are derived from Tarski, <u>1936</u> (repr. in Tarski <u>1956</u>, Ch. VIII). Finally, I describe a language which admits a quotation-operator and a semantics for the language in terms of which semantic consistency has been proved (by Grover, <u>1973</u>). I report an open-problem for the language described, yet argue that the language (called "Lqu") is not susceptible to the traditional semantic paradoxes. It is the quotation operator "qu(----)" thus introduced and defined (within Lqu) that I use in II Sections C, D, E in formulating the Logics CL, CSi and CS2.

Routley and Goddard's account of the qu-operator, token/type and use/mention distinctions can be summarised as follows: The standard treatment of quotation (from Tarski, <u>1936</u>) treats a <u>given object</u>-language as the universe of discourse of a meta-language in which expressions of the

object-language are mentioned by forming names of them. Any names, uniformly chosen, <u>could</u> function as names of expressions in the object language; though the customary method for forming such a name is to reproduce (by means of a concatenation-operator) a structural-descriptive string of symbols isomorphic with the nominatum and enclosed within quote-marks. Thus, "'J*o*h*n*'" is a name for the expression "John". Rather than treat all mentioning as analogous to naming, Routley and Goddard concentrate, not upon what objects are picked out by quotationexpressions (hereafter "qu-expressions"); but upon how we <u>use</u> or <u>feature</u> items (which may or may not be linguistic expressions) for the purpose of making various assertions about them:

> When we speak of type-, token- and genus-features we mean to refer to those features of a given object which we use as the basis of a given classification for a given purpose, and with respect to some stateable, if not stated, criterion. (R & G, <u>1966</u>, p. 8).

Just as to refer to the book on my desk, in front of me, for example, as a philosophy book is to feature its similarity to most books in my library (and its dissimilarity from other items--e.g. pencils--in my library); so to refer to the word which begins the first sentence of this Appendix as a preposition is to feature grammatical properties it has in virtue of which it is similar to the ninth, eleventh and fifteenth words of that sentence (and dissimilar from

the third and fourth words). On the other hand, to refer to the book on my desk as the one with the coffee-stain on page 56 is to feature its individual properties, in virtue of which it is my particular copy of Wilson (1959). So, to refer to the preposition at the start of the first sentence of this Appendix as containing a capital "0" and preceding a token "any" is to feature its token-properties, in virtue of which it is one individual occurrence of "of". There are not two things on my desk; a philosophy book and a copy of Wilson (1959) with a coffee stain. There is one item classified in two ways--with respect to its genus-features and token-features. Being Wilson's The Concept of Language (1959) is an additional type-feature of the book which it shares with all other copies (of the 1963 reprinting). Analogously, there are not two words at the start of page (i)-a preposition and a token "of". There is one item with genusfeatures (being a preposition-word) type-features (being an "of") and token-features (being an "of" with capital "O" and preceding an "any").

In brief, then, Routley and Goddard's systematic account of qu-expressions allows that there may be various <u>ways</u> in which we feature expressions for the purposes of talking about them. For example, token-features of written and spoken expressions may be their location, occurrence and graphic or phonetic qualities; type-features may be their philology, etymology, grammar and significance in context; and genus-features may be their being words, sentences of English or Croatian etc. The emphasis of the account, then, is properly upon the manner in which items may be mentioned for diverse purposes of ascription and classification--an emphasis to which the standard account--subsuming all mentioning (whether of token or type) as a species of naming--is insensitive.

Let us call the occurrence of an expression e along with its relevant quote-marks a <u>quotation-expression</u>—hereafter 'quexpression'—of e. Then, relative to the kind of mention involved, the function of qu-expressions is far less like that of proper names than it is like the manner in which a variablebinding operator (abstraction-, description-, or choiceoperator) forms a term in predicate logic, relative to the assignments of arguments to its constituent variables. For example, the description-operator '(η_X)(... χ ...)' defined over sentential functions '... χ ...' forms, for given arguments to the variable χ , a term whose value is <u>the</u> unique object \overline{a} , if such there be, satisfying '... χ ...'. Similarly, a quoperator yields a qu-expression as the result of applying an operation to expressions (as arguments) to form a term with a constant value (if any) for each item in the syntax of the formalism. Unlike a proper name, therefore, it is a composite expression of which the relevant kind of quote-marks (operans) and constituent expression (operandum) are functioning parts.

I proceed now to list the forms of quotation discussed above--several of which have little use, save as illustrating points made in the discussion. In subsequent sections only the last quotation operator--'qu(--)', introduced in (v) will be employed. My remarks on the first three will, therefore, be brief.

(II) Mentioning tokens:

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(i) syntactic mention: a token occurrence of an expression may be mentioned for the purpose, say, of featuring some particular syntactic property exhibited by its single occurrence on a specific occasion. Mention of the expression can be accomplished by forming <u>from the antecedent use of</u> <u>the token</u> an alphabetically invariant qu-expression concatenating each letter in the expression. The result is a 'structural descriptive name' (in the sense of Quine, <u>ML:1951</u>) which is, itself a token, mentioning the token from which it is formed.

Because a token qu-expression mentions an expression only in relation to an antecedent token, it is unsuited to the general mentioning of expressions. It has a use, perhaps, only in characterising certain kinds of token reflexivity: for example, we may contrast the syntactic functions of the two occurrences of a "had" in: (k) John had had five dollars.,

by observing that:

(1) the *h*a*d* follwing *J*o*h*n* is a tense auxiliary; whilst the *h*a*d* preceding *f*i*v*e* is the main verb in (k).

Structural descriptive names for tokens are needed in this example since, without further explanation, it makes little sense to describe a word-type as 'preceding' or 'following' tokens [in a left-to-right ordering].

(ii) semantic mention: a semantic feature of a token expression is exhibited, usually, within a particular context of its use. A mention of that token--for the purpose of featuring some semantic property it has-should therefore be indexed to a description of those contextual circumstances which contribute to the expression's featured property. This could be indicated, say, by flanking a token qu-expression (structural descriptive name) with a constant abbreviating the given contextual description. For example, if we wish to assert of the *J*o*h*n* on line (k) that it refers, in that use, to John Smith, one could write:

(m) *J*o*h*n* (C,) refers to John Smith.

Token qu-expressions of tokens are unrepeatable--each has an appropriate value only for <u>one</u> antecedently used token. From their formation, however, one can generalise to the formation of qu-expressions mentioning types.

(iii) type-mention: a type qu-expression singles out, for syntactic or semantic mention, any of a class of tokens, by featuring its similarity

(alphabetic or whatever) to some explicit, <u>located</u> token (namely: to the token 'within' the quotation-expression). In forming such an expression, therefore, an abstraction operator is being applied to the denotata of token qu-expressions, partitioning them into equivalence classes. For example, an initial clause of a legal agreement often explicitly classes recurrences of a given token expression throughout the agreement:

(n) The party of the first part--hereafter "Lessor" agrees Further occurrences of token "Lessor's throughout the document then designate the same individual through the stipulative type qu-expression. Such restricted type-mention, relative to a particular class of tokens, contrasts with the more general mention of any of the class "Lessor" simply say, for the purpose of spelling. In this latter case, orthographic similarity, and not occurrence in a document, would be the sole condition upon membership in the argument-range of the type qu-operator. A suitable notation, thus, for type-mention should indicate its operation as forming equivalence classes of tokens similar in some respect to the operandum. Unlike a structural descriptive name, therefore, that it successfully mentions an expression is not contingent upon some antecedent occurrence of a token, but upon its having determinate membership conditions. So, a type quexpression is the value of a constant function, sometimes indexed to context, from tokens to equivalence classes, membership being determined by some featured condition. That is, we can contrast:

(o) *L*e*s*s*o*r* (C) has six letters.,
--which asserts truly of a token on line (k) that it has six letters; with

(p) [Lessor] (c) designates the party of the first part which asserts

of any token occurrence of a "Lessor" in the agreement that it stands for the party of the first part.

(iv) variable mention: it is often necessary to mention expressions to feature properties they exhibit as a type, without singling out any specific class of tokens. Notice, however, that any occurrence of an expression <u>in use</u>, or within quotation marks, is a <u>token</u>-occurrence, related to the expression-type only through some featured equivalence to any of a class of tokens. The alternative---to regard expression-<u>types</u> as both useable and mentionable--is to be committed to that Platonism in the standard account of type and token which reifies classes of expressions as items existing over and above the mere collection of their instances.

In addition to the general mention of expression types, some means of treating quotation-contexts containing <u>variables</u> is needed in order to state general theses about the result of substituting (arbitrary) tokens of <u>whatever</u> type for a variable qu-expression. Here, a technical difficulty-considered by Tarski (<u>1933</u>, transl. <u>1956</u> pp. 161-2) as jeopardising <u>any</u> attempt to introduce qu-operators into formal languages--arises in connection with how variables function in quotation contexts.

Suppose we wish to generalise Tarski's so-called "Convention T" (a condition upon the definition of "truth"):

(T) 'Snow is white' is true if and only if (iff) snow is white. To generalise (T) to assert of <u>any</u> sentence that it is true if and only if what it states in fact obtains requires the mention of the result of substituting any token-sentence for a quoted variable. [Although I do not subscribe to Tarski's formulation of the truth-convention-because sentences, alone, are not true or false, though the statements they yield might be--I shall employ (T) as an illustration of the general difficulty]. Tarski claims that to generalise (T) as:

(T') for all p, "p" is true iff p--where 'p' is a sentential variable-is inadequate because, on the standard account of mentioning by quotation, (T') asserts that the 16th letter of the alphabet is true if and only if P (for any P). Tarski's claim holds, however, only when we construe the quotation marks in (T) and (T') as forming a name of an expression, which assimilates all cases of mention to syntactic naming--a view already rejected. Indeed, on the standard account of mention, (T), itself, is unacceptable if (T') is. For what we require of the quoted P in (T') is that it retain its function as a variable (i.e. as a use of a variable) even in a quotation context. Similarly, in (T) "'Snow is white'" has to be read as stating a truth, as used, even though the sentence, itself, is being mentioned qua syntactic object. In any case, on the standard account of use and mention, (T') violates all the conventions for mentioning, since, to quantify into a quotation-expression is to regard the constituent expression as a functioning part of the whole; whereas the standard account treats the whole as a proper-name.

What is needed, therefore. is a means of mentioning whatever (arbitrary) token of whatever type, over which a quoted variable ranges, results from substitution into the scope of a quantifier. The proper reading of the quantifier in (T') will then be <u>substitutional</u>²--asserting of whatever expression is substituted for the quoted variable, that it yields a truth if and only if what it states is, in fact, the case.

The following formulation of a quotation operator 'qu(--)' is designed to meet these requirements:

(v) the quotation-operator (qu(--)): when we display mathematical functions in the form: 'x²', 'Log x', '2x + x', we represent that the values of the functions depend upon the values assigned to the Ex's. It would be absurd to say that, in such expressions, what is being mentioned , are Ex's--for we do not square the letter Ex, nor do we square the variable x. What we square is any value assigned to the variable x. Similarly, what the quoted Pee in (T') indicates is not that the letter Pee, nor the sentential variable p, is being mentioned, but that whatever expression is substituted for the variable is being mentioned. That is, the quoted 'snow is white' in (T) occurs as an argument-expression (substitution-instance) to the type qu-expression--indicating that, whatever token of this type, as used, yields a truth if and only if snow is white. Such a type quexpression is therefore a complex term whose values are token assignments from a class of sentences lexicographically similar.

Secondly, in (T'), p functions as a place-holder, within a complex term, which, in conjunction with a quotation-operator indicates that, whatever expression from the substitution range of p is substituted for the variable, the result is a type qu-expression mentioning one of a class of tokens (of any sentential type) which yields a truth, in <u>use</u>, if and only if what it states is in fact the case.

This suggests the following initial convention for the formulation of a qu-operator:

<u>Qu</u>: where M is schematic for variables or constant expressions, whatever argument-expression is assigned to M, the value-expression of qu(M) is to be the type qu-expression of the argument assigned to M.

Thus, we may rewrite (T), (T') as follows:

(T) qu (snow is white) is true iff snow is white,

(∀p)(qu(p) is true iff p)--where the quantifier
(∀--) is substitutional and has as scope the entire equivalence.

Each substitution for an expression from the substitution class of the variable in (T') will then yield a case of (T) as an instance (of which the cited (T) is one instance).

More generally, we can define the qu-operator for occurrences of variables within compound contexts, by modifying the original convention to read:

<u>Qu</u>: where M is schematic for variables or constant expressions within a context (...M--), whatever argument-expression is assigned to M, the valueepxression of qu(...M--) is the type qu-expression of the context with M replaced by its argumentexpression.

Formal bases for logics which admit a qu-operator and which permit quanitification into quotation contexts have already been formulated in works by N. Belnap and D. Grover (in Leblanc, <u>1973</u>) and by Grover, alone (also in Leblanc, <u>1973</u>). I offer a sketch of such a formal language in concluding this Appendix, together with a Grover-type semantics for the language and known (semantic) consistency results. At this point, however, it is necessary to examine whether the qu-operator is free of those general criticisms of quotation operators which, Tarski has argued (loc. cit. pp. 161-2), threaten any formal system admitting quotation-clauses.

(III) Criticisms of qu-operators:

Tarski has four objections to the introduction of qu-operators into formal languages:

(i) "Quotation-operators do not have a sufficiently clear definition"Tarski claims that, if we admit qu-operators, we are forced to admit:

"certain linguistic constructions whose agreement with the fundamental laws of syntax is at least doubtful, e.g. meaningful expressions which contain meaningless expressions as syntactical parts (every quotation name of a meaningless expression will serve as an example)." (loc. cit. p. 162).

It is clear that this criticism is not directed at the standard account of quotation (that, in a sufficiently rich meta language--one which replicates the object language--quotation-expressions function as proper names of the appropriate object language expressions). For, on the standard account, the constituent of a qu-expression is no more a syntactically functioning <u>part</u> of the qu-expression than "Plato was bald" is a sentence functioning syntactically in "The teacher of Plato was bald".

It seems that what Tarski supposes is in 'doubtful agreement with the fundamental laws of syntax' is the inference that, if 'qu(--)' is a meaningful expression of the metalanguage and any result of substituting expressions for the place-holder in 'qu(--)' is meaningful, then 'qu (borogroves are mimsy)' is meaningful even though "borogroves are mimsy" is not. Yet it can conflict with no "fundamental law of syntax" to mention a nonsignificant expression in a suitable way in order to assert (truly) of it that it is (literally) nonsignificant. On the <u>standard</u> account of quotation, however, the truth of "Borogroves are mimsy" is nonsignificant' derives not from the fact that the mentioned expression lacks a literal interpretation (in standard contexts), but from the <u>absence</u> of any expression formed by concatenating, in order, the 2nd letter of the alphabet, with the 15th letter, with the 18th letter,, with the 25th letter, followed by a period, from the class of admissable syntactic forms in the object language. On this view, then, one accepts implicitly a restriction upon the syntax of an object language to exclude nonsignificant expression at outset. It follows that the metalinguistic predicate 'M is significant' is true for every argument, from this restricted object language, for which it is defined (cf., above, Part I, Sect. C -- the discussion of Russell's type theory as imposing <u>syntactic</u> restrictions upon the formation-rules of a language).

To accept such a restriction on an object language is, therefore, to accept that all cases of failure of significance are best subsumed under syntactic ill-formedness or ungrammaticality--a view found objectionable, already. This is not to deny that, for most purposes of classical logic, it is preferrable to disregard the natural difference between sentences violating grammatical or syntactic rules and sentences which are grammatical, but lack significance (i.e. lack a literal interpretation in context). Nevertheless, there is nothing 'fundamental' about such <u>ad hoc</u> restrictions upon an øbjest language. When one's focus is upon 'significance features' and 'semantic failure', in general, such restrictions are self-defeating.

The assumption I have made, thus far (in Sect. A), that the universe of discourse of significance logic comprise all grammatically and well-formed sentence- and expression-types concedes thus, that some arguments to the

qu-operator may be nonsignificant, referentially unsuccessful, sortally incorrect, i.e. semantically unsuccessful for a variety of reasons. The apparent validity of the argument: if 'qu(borogroves are mimsy)' is a meaningful expression, so is 'borogroves are mimsy'; whereas this latter is not meaningful, turns on an implicit acceptance of only one variety of meaningless expression. Within the context of significance logic, however, the argument is clearly invalid. For, it proceeds from the premise: "'qu(borogroves are mimsy)' is a meaningful expression of the metalanguage (in the sense of being syntactically admissable)" to the conclusion "hence, 'borogroves are mimsy' is significant (in the sense of being semantically successful, i.e. statement-making)". Now, whilst the premise is true in virtue of its subject term mentioning a (paraphrase of a) token-sentence by Lewis Carroll's Jabberwocky, its conclusion is false if the context is confined to literal interpretation. In this respect, the domain of significance logic merely extends that of classical logic to include features of sentences and statements not normally treated in classical logic. Such an extension follows the lead of N. Chomsky who includes within the purview of the semanticist sentences wholly grammatical from the perspective of syntactic structure; but whose nonsignificance derives from their violation of semantic principles. The most noted example of such a sentence is Chomsky's :

(c) Colourless green ideas sleep furiously.

To conclude, therefore, the 'fundamental laws of syntax' with which, Tarski claims, qu-operators are in conflict amount to little more than the predilection of logicians to remain within the confines of classical logic. (ii) "The use of a qu-operator is ambiguous as to the kind of expression being mentioned" (paraphrasing p. 162, loc. cit., third paragraph):

I am, of course, in complete agreement with this criticism of the standard account of quotation. I have argued above $(A_{pp}dx. C,(I))$ that this ambiguity in the standard account is best resolved not by differentiating between <u>kinds</u> of mentioned expression (token/type), but between featured ways in which expressions are mentioned for diverse purposes.

(iii) "Quotation-operators are not extensional" (loc. cit. p. 161)

Tarski acknowledges that the set-theoretic notion of extensionality is not applicable to operators of this kind (because they are not defined in terms of membership). Instead, Tarski bases his criticism on the observation that the following canon of sentential logic fails for a quoperator:

(F) for any sentences \emptyset, ψ and operator f defined over sentences: $[\emptyset \equiv \psi, \supset f(\emptyset) = f(\psi)].$

It is clear that contexts where a qu-operator appears are 'referentially opaque' (in the sense of Quine, <u>1960</u> p.148). That is, intersubstitutivity of identicals fails; for example:

(I) Tully = Cicero, but qu(Tully ≠ qu(Cicero), which is unsurprising, since difference of objects need not follow from difference of sign. It is not clear, however, whether Tarski's (F) should be taken as definitive of <u>extensional</u> contexts, since so few operators defined over sentences satisfy it. In effect, (F) restricts 'extensional' operators to those which, in the semantics of sentential logic, are definable solely in terms of mappings $f: \left\{0,1\right\}^n \rightarrow \left\{0,1\right\} \text{ of truth-value assignments onto the two-element set}$ $\{0,1\}$ of truth-values. Such functions--mappings from truth-value sets to truth-values--are precisely those "truth-functions" the representation of which, in sentential logic, guarantees the intersubstitutivity of materially equivalent sentences in all truth-functional contexts. Yet, apart from the sentential 'connectives' '~, \lor , \supset , &, \equiv ' explicitly defined in terms of truth-value assignments, examples of operators over sentences, or sentential forms (open-sentences), which are not 'truth functional', in this sense, are not hard to find. In any infinite domain, a quantifier $(\forall_{\eta}) \phi_{\eta}$ is not uniquely definable in terms of mappings from n-tuples of truth-values to truth-values, unless infinite sets of truth-values are admitted. Otherwise, the modal operator 'Nec(\emptyset)' which is true of a sentence \emptyset whenever \emptyset is true in all possible worlds, the tense-operator 'GØ', true of \emptyset if \emptyset always will be true, or the doxastic operator 'BxØ' true of \emptyset if x believes \emptyset , all fail to satisfy (F) . One need not, even, investigate examples from non-classical logic (tense, modal or doxastic) to find sentential operators whose failure to satisfy (F) does not mark them as 'non-extensional' in any distinctive sense. Consider, as a trivial example, the operator ' (ϕ) ', explicitly defined over sentences of sentential logic, whose values are given by that function which assigns 0, resp. 1, to a sentence ϕ according as the least assignment of truth-values in which ϕ is satisfied is of cardinality at least one greater than $||\phi||$ --where ' $||\phi||$ ' stands for the (finite) cardinal number of distinct atomic variables in \emptyset . Such a function partitions the sentences of sentential logic into 'basic' and 'inessential variants' in the sense of assigning a designated value only to those sentences whose constituent variables number less than, or equal to, the rows of truth-table which suffice to make \emptyset true. Thus:

$$\mathbf{*}(\boldsymbol{\emptyset}) = \begin{cases} 1: & \text{iff } \|\boldsymbol{\emptyset}\| \leq \|\boldsymbol{\mu} \mathbf{v}_{\mathbf{i}} : \mathbf{v}_{\mathbf{i}}(\boldsymbol{\emptyset}) = 1 \|, \\ & \text{for } \mathbf{v}_{\mathbf{i}} \in \mathbb{V}, \text{ where } \mathbb{V} \text{ is the set of classical} \\ & \text{valuations of sentences } \boldsymbol{\emptyset}. \end{cases}$$

 $*(\emptyset)$ fails to satisfy (F) and is, thus, 'non-extensional' in Tarski's sensefor let \emptyset be '(p &~q)', and let ψ be '(r v~r) \supset (p &~q)'. Then, \emptyset is materially equivalent to ψ (they are interderivable), yet $*(\emptyset) \neq *(\psi)$, since $||\emptyset|| = 2 = ||f\mathbf{r}\mathbf{v}_i : \mathbf{v}_i(\emptyset) = 1||$, whence $*(\emptyset) = 1$; whereas $||\psi|| = 3$ and $||f\mathbf{r}\mathbf{v}_i : \mathbf{v}_i(\psi) = 1|| = 2$ (assign 1 to 'p' and 0 to 'q', then the value of 'r' does not matter), whence $*(\psi) = 0$.

Not too much weight should be attached to such counter-examples unless we construe Tarski's appeal to (F) as definitive of some property of sentential operators which separate them as of a kind which requires distinctive treatment. His intention, it is clear, is to confine logic to consideration of functions and operators which satisfy Russell's maxim (\underline{PM} , Intro., 2nd edition, p.××i×) that a "[sentential] function..enter into a proposition only through its [truth]-values" (Russell included also, of course, propositional functions from individuals or sets, to propositions). What is surprising with respect to Tarski's criticism, though, is that the much stronger principle (II) fails in modal, tense, doxastic and quotationcontexts, also, and, in this case, such failure <u>is</u> indicative of the 'intensionality' of such contexts. (II) is the principle of the indiscernibility of identicals:
(II) where $\emptyset x, \forall y$ are sentences which are alike save that $\forall y$ contains occurrences of a term y, wherever $\emptyset x$ contains occurences of x, then, for any function f, defined over sentences (or open sentences) $(x = y . \supset f(\emptyset x) = f(\forall y)).$

Failure of a modal operator 'Nec(\emptyset x)' to satisfy (II), for example, derives from the well-known observation that, unless all the 'possible worlds' in which \emptyset x, ψ y are interpreted contain the same individuals, the contingent identity of x, y does not guarantee the sameness of x, y across all possible worlds, required for the simultaneous satisfiability of \emptyset x, ψ y. Analogously, since identity of orthography across linguistic contexts, as it were, is required for 'qu(\emptyset x) = qu(ψ y)' to be true, mere coincidence of reference is insufficient (indeed, even <u>necessary</u> identity of reference is insufficient for identity of sign!). In view of this shared property of a wide variety of non-truth-functional sentential operators, we have to conclude that Tarski's criticism of qu-operators--that they fail to satisfy (F)--is insufficient, of course, to declare them 'non-truth-functional').

(iv) "The use of qu-operators exposes us to the danger of becoming involved in semantic antinomies" (loc. cit. p. 161).

Tarski's final criticism--that qu-operators generate antinomies--is the most frequently cited ground for declaring qu-operators inadmissable. This is doubly surprising, since:

(a) that an antinomy is derivable within a formal calculus is as much attributable to its deductive strength (its axioms and inference-rules) as to its expressive power (i.e. the number and kind of operators over formulae definable, or primitive, within the calculus);

(b) in the particular derivation of an antinomy in the language

Tarski considers, he appeals to inferences based upon principles one would not expect a fully formalised language with a qu-operator (for example, the language Lqu, from Grover 1973, sketched below) to contain.

Tarski derives an antinomy involving the qu-operator as follows: [I am indebted to N.L. Wilson for this formulation of Tarski's antinomy:]

Let C abbreviate the sentence-token entered on line (a) below:

(A)
$$(\forall p)(C = qu(p) : \supset \sim p)$$

We obtain as an empirical matter:

(a)
$$C = qu((\forall p)(C = qu(p) . \supset \sim p)).$$

We should expect that identity of quotation-expression guarantees material equivalence of mentioned expression, i.e.

(3)
$$(\forall p) (\forall q) [qu(p) = qu(q) . \supset p \equiv q].$$

Derivation of the antinomy is now immediate:

Since we have derived (A) $\vdash \sim$ (A) and \sim (A) \vdash (A), we have the antinomy '(A) $\equiv \sim$ (A)'. That a consistent language (Lqu) <u>can</u> be formulated, however, in a way which blocks the derivation of the antinomy will be shown below. For the moment, however, the following observations upon the derivation of the antinomy must suffice:

Notice, first, that in the introductory definition of the qu-operator ((v), above), quantification over variables occurring both within and without a quotation context is permitted, but is to be substitutional in both cases. Implicit in this requirement is the assumption that the qu-operator is being introduced into a partially formalised metalanguage M which contains some well-defined object language 0 as a proper part. (0 may be, perhaps, English or some formalised fragment of English). The set of expressions of O constitutes the substitution class for the bound variables of M. Thus, the schema of M "($\forall p$) ~ F(p)" reads "for every result of replacing p in '~ F(p)' by some expression e (of M) from the substitution class of p, $'\sim F(e)'$ is true." Yet, quantification over quoted variables (of M) is also permitted in M, and this quantification is also construed substitutionally: the schema " $(\forall p) \notin (\mathbf{au}(p))$ " reads "for each result of substituting some closed term t_0 of 0 for p in 'qu(p)', the value of 'qu(-)' for the argument t_0 is \emptyset ". The antinomy arises from allowing some expression e (of M), belonging to that proper part of M which constitutes 0, to appear both in the substitution class of an unquoted variable and in the value-range of qu(-). Belonging to M, it is quite possible that such an expression e is built up from quantifiers, connectives and the qu-operator. But, in its capacity as a term of 0, e may also be a value of qu(-), in which case, within 0, it designates that

that very expression of M mentioned by 'qu(e)' as a substitution instance of " $(\forall p)(...qu(p)_)$ ". Choosing e, then, to lack some property F (In M) if and only if it has the property Ø (in 0), we simply construct the matrix 'Ø' in 0 to yield a truth 'F(t₀)' for every term in the substitution class of p, including e (as in line (A), above). When $t_0 = e$ (which we verify empirically), e lacks F when and only when e has F--which is paradoxical.

There may be many ways in which 0 and M can be formalised to block the inference to the paradoxical conclusion; but, recalling the discussion of the vicious-circle principle (VCP) in Part I, Sect. C, perhaps the most satisfying line of reasoning would proceed as follows:

For e to be in the value range of 'qu(--)', e must belong to the class of expressions of O--the well-defined part of M. That is, the complex "qu(e)" is well-defined in M, provided e (<u>qua</u> well-formed token) occurs among the constants, terms or sentences of O. So, in introducing quantification in M to assert something of <u>whatever</u> results from replacing the placeholder in 'qu(p)' by a well-defined expression of O, the presumption is that the substitution class of 'p' is already semantically determined, independently of the new quantification. If we allow, now, that, amongst the substitution instances of '(...p___)', there appear expressions of O built up from quantifiers, connectives <u>and</u> the qu-operator of M, then, in part, membership of p's substitution class is fixed only when the set of welldefined arguments to 'qu(--)' is fixed. But, membership of the argument set of 'qu(--)' is fixed only when some means of determining the well-defined expressions of O is given (say, by enumeration or by recursive definition). Substitutional quantification over a variable within 'qu(--)' is, thus, well-defined if substitutional quantification over unquoted variables is well-defined, which, in turn, is well-defined if the set of expressions of 0 <u>including</u> those containing quantification over variables within 'qu(--)' is well-defined. Clearly, we have now come full circle and it will be impossible to determine membership of qu(--)'s argument-set except in terms of the expressions of 0, which are detetermined, in part, by membership in the argument-set of 'qu(--)'.

That this process is circular is obvious. That it is viciously circular follows from the observation, above, that it is because the expression e (i.e. line (A) in the derivation) functions simultaneously as a substitution instance of " $\langle \forall p \rangle (\dots p_{_})$ " (derivation; steps 1)-2) and $(6)-7 \rangle$ and as an argument to "qu(p)" (derivation: line 9), i.e. (d)) that the antinomy is derivable. What is required of a solution to the antinomy, therefore, is to restrict the substitution class of bound variables in M, in terms of which quantification in M is defined. Such a restriction should accord with the general principle upon definitions that they be non-circular. (A circular definition, after all, fails to yield a well-determined meaning for the expression-type it introduces). In concluding this section I shall sketch a formal language containing a qu-operator (based upon suggestions of D. Grover (loc. cit.)) which embodies just such a natural restriction.

In sum, I have argued that, excepting the final criticism concerning the "quotation-antinomy", Tarski's criticisms of qu-operators--as properly construed syntactic devices for quotation in formal languages--are not well founded. Much of the force of Tarski's criticisms is removed by noticing that many of the problems associated with quotation derive from the shortcomings of the standard account, which interprets qu-expressions as semantically autonomous names, rather than syntactically composite terms. Once the ambiguity between mentioning linguistic items <u>qua</u> tokens or types is removed, an important consequence is that, amongst the various ways in which expressions may be talked about, referred to and mentioned (by description: 'what you said to me in the library', 'the third line on page 33' ...; by ostension: '<u>this</u> sentence...', 'the next phrase you utter...'), quotation, alone, explicitly displays (a token of) the mentioned expression. This consequence is important in an analysis of speech-acts in formulating distinctions between the utterance used, in a context, the statement made, and the significant content expressed. This will be the role of the quoperator in the context- and significance-logics, developed.

(IV) A formal language admitting quantification into quotation contexts:

One way to ensure the consistency of languages containing a quoperator is to formulate their syntax in a rigorous manner and provide a suitable interpretation. If a formal language is interpretable with respect to a given model-structure m, it is said to be <u>semantically consistent</u>, guaranteeing that for no assignment of values to formulae \emptyset , mF \emptyset and m $\hbar \sim \emptyset$. In concluding this section, I shall contrast the main features of such a formal language 'Lqu' with the orthodox resolution of semantic paradoxes (due to Tarski) in terms of language-heirarchies.

The language Lqu has the following characteristics:

1) It is free from semantic (and logical) paradoxes (the latter follows trivially since Lqu contains a cumulative type-ordering of variables along the lines of Church's formulation of the simple theory of types (Church,

1956, §53)).

2) Lqu contains a qu-operator and, thereby, the means of mentioning elements of its own syntax (--certain kinds of 'self-reference', then, are permissable. That such self-reference never leads to vicious circles follows from the manner in which Lqu 'accumulates richer vocabularies, heirarchically', in a sense to be explained, below).

3) At each syntactic level in the heirarchy of Lqu's vocabulary, the language is supplemented by a denumerable set of 'expression-term parameters' (analogues of 'Henkin-constants'; see L. Henkin, "Completeness in the Theory of Types", JSL, 15 (1950)).

4) Quantification within and without qu-contexts is permitted in Lqu, and is substitutionally interpreted; i.e., in general, $(\forall \mathbf{n}) \emptyset'$ will be true just in case, for each well-formed expression e belonging to the substitution class of \mathbf{n} , $\emptyset[\mathbf{n}/\mathbf{e}]$ is true--where $'\emptyset[\mathbf{n}/\mathbf{e}]'$ represents the simultaneous substitution of e for all free occurrences of \mathbf{n} in \emptyset .

5) Lqu contains '=' and n-ary predicate variables, to be interpreted as properties of expressions. (Thus, Lqu is, itself, essentially 'metalinguis-tic', containing some well-defined, but non-specified, object-language as a proper part).

The orthodox approach to the semantic paradoxes--which is the only approach to have been worked out in any detail--is that which leads to the celebrated 'heirarchy of languages' of Tarski (loc. cit.). Briefly, let Lo be a formal (object-) language comprising the usual operations of predicate logic, with a stock of primitive predicates, variables and constants, and expressively adequate to represent its own syntax (by means of some such device as Gödel-numbering of its formulae). Tarski <u>proved</u> that such a classical language cannot 'define' (i.e. represent within it) its own <u>truth-predicate</u>. That is, in Lo, there is no recursive procedure for specifying satisfaction functions generating all sentences of the form 'T $L_o(\phi)$ I. $\equiv \phi^1$ '--where 'T_{Lo}(x)I' represents (in the Gödel numbering) the predicate 'x is a true formula of Lo, relative to the interpretation I', and ϕ^1 is the canonical translation or structural-descriptive name of ϕ . Consequently, Tarski infers, semantic predicates like "true-in-Lo", "designates-in-Lo", have to be defined in a higher-order (hence, 'expressively richer') language L_1 --the metalanguage for Lo--which may either contain Lo as a proper part, or may contain an enriched vocabulary suitable for mentioning all expressions of Lo. With respect to L_1 , this reasoning can be reiterated (for "true-in-L₁"), leading to a sequence of languages Lo, L_1 , L_2 , ..., in each of which semantic predicates of expressions and formulae of the preceding language are represented and defined.

It is only recently that this orthodox approach has been subject to criticism--notably in works by Van Fraassen, Martin, and Kripke⁴. I do not intend to pursue their criticisms here, save to note that the alternative resolutions of the semantic paradoxes to which they give rise suffice to refute that interpretation of Tarski's proof which construes it as <u>prohibiting</u> the formulation of languages containing the means of mentioning expressions of their own syntax and of expressing various properties of their own formulae within their syntax. For brevity, I cite only Kripke's remarks on this topic:

"Various writers speak as if the 'heirarchy of languages' or the Tarskian approach <u>prohibited</u> one from forming, for example, languages with certain kinds of self-reference, or languages containing their own truth-predicates...there are <u>no prohibitions</u>; there are only <u>theorems</u> on what can and cannot be done within the framework of ordinary classical quantification theory. Thus Gödel <u>showed</u> that a classical language can talk about its own syntax; using restricted truth-definitions and other devices, such a language can say a great deal about its own semantics...." [Kripke, op. cit., p. 694, footnote 9].

In the spirit of Kripke's tolerance, then, Lqu represents an alternative to

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construing all mentioning of expressions (by means of quotation) as belonging to a separate language from that in which expressions appear in use.

Given this motivation, instead of dividing <u>languages</u> into a heirarchy of levels, Lqu adopts a device to block the formation of paradoxical expressions, through the imposition of level-restrictions on the <u>substitution</u> <u>classes</u> of closed terms and sentences in the interpretation of Lqu. In this way, a heirarchy of levels appears only in the (substitutional) interpretation of bound-variables, i.e. in the semantic, rather than in the syntactic, part of the language. Nevertheless, since quantification in Lqu is <u>substitutional</u> level-restrictions apply properly to <u>expressions</u> of Lqu (and not to the things they denote), i.e. to the expressions belonging to the proper part of Lqu which is the (unspecified) object language. The effect, then, is essentially the same as applying <u>syntactic</u> restrictions, similar to Russell's indexing of bound variables to "orders" (c.f. Part I, Sect. C), to the expressions of the object-language.

The guiding principle behind the heirarchy of 'expression-levels' is to confine the interpretation of qu-expressions in Lqu to a level <u>below</u> that of variables appearing in unquoted contexts. This inversion of the normal ordering of levels (whereby, ordinarily, a qu-expression of an expression e appears only at levels 'higher than' e) is accomplished through the adoption of a procedure first used by Henkin in proving the completeness of simple type theory ⁵, and applied to substitutional quantifiers by Leblanc and Meyer ⁶. Henkin established that the semantic consistency (i.e. 'soundness') and, hence, satisfiability, of any set of sentences at a given typelevel (whose bound variables have an index not exceeding that level) is

unaffected by introducing into the interpretation of those sentences a denumerable list of special constants of that type (called "Henkin constants"), disjoint from the parameters and variables of that type. This procedure ensures that each existential closure of a satisfiable sentence of that type can be matched with a value (the 'denotation' of the constant), new to the interpretation. When applied to a substitutional interpretation of quantifiers, the procedure guarantees that, by introducing denumerable lists of 'expression-term parameters' into Lqu, disjoint from the substitution class of any bound variable and indexed in a suitable way to a cumulative ordering of expressions in the proper part of Lqu, no value of a quoperator need coincide with a substitution-instance of an unquoted bound variable of that level to issue in the kind of self-reference that leads to paradoxes. (It is in the addition of expression-term parameters that Lqu "accumulates richer vocabularies" in the sense intended above--2) p. xxx). Relative to its interpretation, then Lqu allows the possibility that any well-formed expression, of whatever level in the proper "object language" part of Lqu, may be mentioned in several ways (as a value of the qu-operator, or as the denotation of an expression term parameter). This consequence, however, is innocuous, since a substitutional interpretation of quantifiers already requires that there be denumerably many expressions (terms) at each level, which, when substituted for a universally bound variable of that level, generate true substitution instances (see: Leblanc and Meyer, loc. cit. p. 79).

The above informal description of the formalisation of a paradoxfree quotational language may appear to complicate beyond feasibility the

intuitive basis for the claim that self-referential paradoxes are <u>not</u> the inevitable consequence of employing qu-operators. It may indeed appear, in view of the complicated interplay between the semantics of Lqu and the syntax of its proper part, that this is a case where the "cure is worse than the complaint"! I would claim, on the contrary, that, in its formalisation, Lqu represents, in a natural manner, the intuitive diagnosis of the source of paradoxes in violations of the vicious-circle principle. This claim can be made clearer in the formal outline of Lqu--to which I now proceed:

A (formal) language consists of a syntactic part (primitive and defined expressions) and a semantic part (a model-structure, comprising a universe of discourse and operations defined over it, and a set of "interpretation-functions" which assign 'meanings' to the expressions of the syntactic part, by mapping them, in a generative, i.e. recursively enumerable, way onto items in the model-structure). The syntactic part of Lqu is a sextuple (So, V^n , Q^n , F^n , S^1 , {qu, ~, v, \forall , =, (,)} comprising: 1) a denumerable stock So of atomic sentence parameters (never occurring bound). (I use M, \triangle as schematic for members of So). 2) for each $n \ge 0$, a denumerable stock V^n of sentential variables with index n. (I use 'pⁿ', 'qⁿ' as ranging over members of V^n). 3) for each $n \ge 1$, a denumerable stock Q^n of expression term parameters (atomic terms) with index n. (I use E^n , E^n_1 for members of Q^n). 4) for each $n \ge 1$, a denumerable stock F^n of n-place predicates (for which I use schematic F, G). [In general, indices on sentential variables restrict their substitution range; indices on terms restrict admissable assignments of denotation]. 5) the set {qu, \sim , v, \forall , =, (,)} of primitive logical and non-logical operators. 6) the set S^{1} of (defined) terms and sentences of Lqu given by:

(a) the set S^1 is the smallest set (of terms and sentences) containing So, Vn, Qn, and such that:

- (b) if \mathbf{X} is a term or sentence, $qu(\mathbf{X})$ is a term,
- (c) if t_1 , t_2 are terms, $t_1 = t_2$ is a sentence, (d) if t_1 , ..., t_n are terms and F an n-place predicate, F(t_1 ,..., t_n) is a sentence,

(e) if \emptyset, ψ are sentences, so are \emptyset , $(\emptyset \lor \psi)$, and $(\forall p^n) \emptyset$.

Definitions of bound and free occurrences of variables are the familiar ones of predicate logic; I omit them. Notice that (i) a term or sentence is said to be "closed" if all occurrences of variables in it are bound, (ii) an occurrence of p^n in $qu(p^n)$ is a free occurrence. I use "expression" as generic for terms and sentences, and "closed expression", accordingly. The abbreviations ϕ, ψ, \star are schematic for sentences, X, Y for expressions, and Vⁿ for sets of closed expressions (with index n).

It remains to give the definitions of "level", "vocabulary of a level" for the syntactic part of Lqu, and to discuss the semantic part.

Df I: Call an occurrence of an expression X in Y transparent provided that X, in that occurrence, does not fall within a well-formed part of Y of the form qu $(\ldots X_{-})$.

Otherise, call an occurrence of X in Y quotational. [I also talk of "transparent (quotational) contexts" to mean the "transparent (quotational) occurrence" of an expression in that context].

Df II: The level of an expression X is

a) zero: if there are no sentential variables occurring

- in transparent contexts in X,
- b) max $(n_1, ..., n_k) + 1$: if $n_1, ..., n_k$ are all the

distinct indices on variables occuring transparently in X.

[Notice that Df II inverts the normal 'heirarchy of level's and places quotational occurrences of variables at the zero-level].

Df III: where n is the level of all of a set of closed expressions, I call the <u>n-level vocabulary</u> Ve of Lqu the set of closed expressions whose level is n.

It follows from Dfs. I-III that the difference between V_{ρ}^{n} and V_{ρ}^{n+1} , for each n, is that the latter has, in addition to all the closed expressions of Vⁿ_a, closed expressions containing some transparent occurrences of at least one sentential variable 'pⁿ' of level n. Lqu comprises, therefore, "cumulatively richer" vocabularies at each level.

The semantic part of Lqu comprises mappings of the closed terms into closed expressions (understood as the proper "object-language" part) and of the closed sentences into $\{0, 1\}$, the two-element set of truth-values 'false', 'true'. Such mappings (interpretation-functions) are determined by a basis-assignment of closed expressions to term parameters E^{0} , and a truth-value assignment of 0, resp. 1, to atomic sentences. When I is a basisand truth-value assignment (I = $\langle I_{E0}, I_{S0} \rangle$), I is a <u>level</u>-assignment (i.e. "level-preserving basis assignment") when I maps term parameters with index n + 1 into V_{ρ}^{n} and closed atomic sentences of So into $\{0, 1\}$.

For I a level-assignment, the <u>level-interpretation</u> I_e <u>based on I</u> is the (unique) function satisfying the following conditions:

Df IV (1) I_e(X) = I (X), for any <u>closed</u> expression X in the domain of I, (2) If X is qu(Y), I_e(X) = Y, (3) If X is t₁ = t₂, then I_e(X) = 1 iff I_e(t₁) is I_e(t₂), (4) If X is ~Ø, then I_e(X) = 1 - I_e (Ø), (5) If X is (Ø v ¥), then I_e(X) = max (I_e(Ø), I_e(¥)), (6) If X is (∀pⁿ) Ø, then I_e(X) = 1 iff for each closed sen- tence ¥ ∈ Vⁿ_e, I_e(Ø[pn/¥]) = 1, where Ø[pn/¥] is the result of replacing every free occurrence of 'pⁿ' in Ø with (closed) ¥.

[In effect, clause (6) defines V_e^n as the level-substitution class of the variable 'pⁿ' occurring transparently in some closed sentence, of level n + 1].

The remaining task, at this point, is to show that for an arbitrary level-assignment I, there is a unique extension I_e satisfying (1)-(6). There are two parts to this task: that of showing the existence of I_e based on I,

and of showing the <u>uniqueness</u> of each I_e based on (some) I. Proofs of these parallel, essentially, the proofs of existence and uniqueness of interpretation functions given in Grover (loc. cit. pp. 106-8) for her substitutional languages ρ , γ and σ --so I shall be content merely to sketch them here.

Effectively, it has to be shown that, for an arbitrary level preserving assignment I (to closed terms and sentences), when I'_e , I''_e are <u>level-interpretations based on</u> I which agree on I, and satisfy (1)-(6), then (i) I''_e , I''_e agree on one another (uniqueness) and (ii) there always is such an I'_e (= I''_e) existence).

Taking (ii) first, it suffices to show by induction on levels $\leq n$, there is a set of assignments I^{n+1} , coinciding with I on the evaluation of closed terms and sentences (Hypothesis); and such that if I^{n+1} satisfies (1)-(6) of Df IV for all terms and sentences of complexity $\leq k$, it satisfies (1)-(6) for terms and sentences of complexity k + 1. By double induction, then, first on level, then on complexity within each level, the requisite level-interpretation I_e is defined as the union of all those I, I', I"....'s which agree on I and satisfy (1)-(6). [Each I, I', I"...., is simply a <u>basis</u> assignment I at each level of vocabulary V_e^o , V_e^e , $V_e^{"}$ Since only <u>closed</u> sentences are assigned truth-values and <u>closed</u> terms denotations, (1)-(6) guarantees that level is preserved by the indexing of closed terms, and that truth-conditions are preserved by truth-functional composition].

The main steps in the uniqueness proof--which is somewhat more complicated-are set out in Grover (1973, pp. 108-110). I believe--though it remains an open-problem for Lqu--that her proof can be reconstructed in Lqu. From these proofs, we have the following theorem for the semantic part of Lqu: THEOREM: For every level-preserving basis-assignment I, there is a unique extension I_e of I which satisfies Df IV, (1)-(6), and preserves level of vocabulary.

As a result, it has been shown that Lqu possesses well-defined substitutional interpretations based on level-assignments--from which we can infer the semantic consistency of Lqu. In general, therefore, the claim that Lqu is free of semantic paradoxes involving substitution into the quoperator is substantiated. The particular demonstration that Tarski's quotation antinomy, the paradox of the Liar and the paradox of heterologicality cannot be derived from some set of closed sentences of Lqu, I leave to the summary, as illustrations.

(V) Summary and the Paradoxes, again:

A systematic and detailed account of the mentioning of expressions through quotation and other means is vital to the investigation of utterances, their significance in context, the statements they yeild, and intraand inter-contextual identity conditions for statements. In this section, I have presented arguments against the customary account of the use/mention, type/token distinctions (I), preferring an analysis which emphasises the variety of <u>kinds</u> of mention, conditional upon ways we <u>feature</u> items for different purposes of classification and individuation. The manner in which I redrew these distinctions was intended as a solution (or 'resolution') of that specific instance of the problem of the relation between universal and particular which pertains to linguistic entities. Assuming (in II) an ontology of token-expressions, describing the ways in which they are featured led to the introduction of several kinds of quotation-operator. For the last of these--the operator 'qu(--)'--a rigorous formalisation in a quantificational language, Lqu, was presented (IV) together with a soundness proof based upon the existence and uniqueness of (substitutional) interpretation-functions. It was argued that Lqu adequately formalised the intuitive conviction that antinomies arising from informal use of a qu-operator are the result of viciously circular definitions.

The logical paradoxes appear to demonstrate that there is a conceptual inconsistency in holding jointly (i) that to each predicate defined over objects, there corresponds a class (its extension) comprising every object of which the predicate is true; <u>and</u> (ii) that classes, themselves, are objects falling within the extensions of predicates. Similarly, the semantic paradoxes appear to demonstrate an analogous inconsistency in presuming that (i) to each (semantic) predicate defined over expressions, there corresponds an assertion true of all expressions of which the predicate is true; <u>and</u> (ii) that assertions, themselves, are (yielded by) expressions falling within the extensions of those predicates. The soundness of the quotational language, Lqu, guarantees its freedom from paradox. It is illustrative of Lqu's application, however, to show how the techniques employed block the formulation of semantic paradoxes.

Each of various formulations of the Liar Paradox requires us to suppose that, among denials of the truth assertions, there is an assertion denying truth of itself. For example, consider line (L)

(L) The assertion on line (L) is false.

qu (the assertion on line (L) is false) is true if and only if the assertion in line (L) is false (Tarski's 'convention T'); therefore the assertion on line (L) is false. If line (L) is false, however, it asserts truly of the assertion on line (L) that it is false; therefore it is true. Thus, line (L) is true, if false, and false, if true; i.e. paradoxical.

Similarly, Grelling's paradox of heterological predicates asks us to consider, among applicable predicates, the applicability of a predicate not applicable to itself. Thus, for example, the predicate qu (x is long) is not long, and qu (x is monosyllabic) is not monosyllabic. Call such predicates "heterological". Then:

(G) x is heterological only if x does not apply to itself. By (G), qu (x is heterological) is heterological only if x is not heterological. So, qu (x is heterological) does not apply to itself; i.e. it <u>is</u> heterological. Thus, qu (x is heterological) is heterological if and only if not heterological; i.e. paradoxical.

In each of these paradoxes, whether quotation is employed or not, their formulation requires some mention of an expression (assertion or predicate) which also occurs in <u>use</u> in the paradox. Some have dubbed this characteristic of semantic paradoxes their "distinctive self-referentiality" (c.f. Russell, Part I, Sect. C, above).

It should not be thought, however, that semantic paradoxes <u>require</u> this kind of self-reference (mentioning an expression in using it) for their formulation. There are formulations of the Liar paradox (or something very like it) due to Kripke (Kripke, 1975, pp. 691-2), expressed without quotation, lacking intrinsic self-reference, and leading to the paradoxical conclusion, only contingently--given certain empirically unfavourable circumstances. There is nothing intrinsically wrong, for example, with assertions (D), (N), below:

- (D) [asserted by Dean]: "Most (i.e. a majority) of Nixon's Watergate assertions are false."
- (N) [asserted by Nixon]: "Dean's Watergate testimony is all true".

Yet, if (D) is all of Dean's testimony (or the rest of his testimony is true) and supposing Nixon's assertions about Watergate matters to be evenly balanced between truth and falsity, except for (N), then, if (D) is true, (N) is false (to put Nixon's false assertions in the majority); whence (D) is false. Conversely, if (D) is false, since fifty percent of Nixon's assertions are true, (2) must be true (to provide the requisite majority). So (D) is true (since a part of Dean's testimony). Therefore, (D) is true if and only if false, i.e. paradoxical, given such unfortunate circumstances. In the given context, it is impossible to assign either truth-value to (D) and (N), simultaneously, so (N) or (D) (inclusively) will fail to yield a true or false statement when the empirical context is sufficiently unfavourable. Should (N) and (D) be asserted in a different context, however, both may be true--consider a context in which (N) is the only truth amongst the many assertions Nixon has made about Watergate matters.

The necessary failure of a paradoxical sentence to yield a true or false statement, relative to the truth or falsity of certain other statements (the contextual circumstances) is distinctive of what have been called "ungrounded" assertions (by Herzberger, Van Fraassen, Kripke, <u>et. al.</u>). ^{ty} Only Kripke has provided a <u>formal</u> criterion for the ungroundedness of assertive sentences (Kripke, loc. cit.), and, then, only for such as involve a truth-predicate in an essential way. In a subsequent section (\mathfrak{D}) , I shall argue that "ungroundedness" is analogous to certain features of utterances which fail to be significant when contextual circumstances are unfavourable, (for example, when their failure to yield a statement is attributable to their necessarily not being 'about' some determinable item, in the context, or to their predicating of an item what is not defined for items of that kind). Notice, however, that 'ungroundedness' is a property of an utterance in a context--not of an utterance or sentence alone. Such paradoxicality cannot then be attributed to some intrinsic feature of the utterance.

The examples above suggest that the attempt to provide some criterion for semantic paradoxicality, intrinsic to the sentences, themselves, which formulate the paradox, fails for paradoxes of ungrounded assertion. The orthodox treatments of semantic paradoxes (Tarski's heirarchy of languages, Russell's ramification of syntactic orders) are built upon the supposition that such an intrinsic criterion is appropriate. It is the inadequacy of this supposition that motivates the formulation of level-preserving semantics for Lqu.

The notion of 'level' (with respect to quotational or transparent occurrences of bound sentential variables) is intended to capture an intuition somewhat of the following kind. In order for mention by quotation to be a well-defined operation on expressions, any assignment of terms as denotations of qu-expressions cannot--on pain of vicious circularity--include

closed expressions containing occurrences of the quotation operator. So, the base-level in Lqu, V_{ρ}^{0} , comprises a stock of expression term parameters (the denumerably many atomic terms and sentences of the 'object language' part of Lqu, not involving quantification). As basis assignment I maps each result of applying 'qu(--)' to a term-parameter of V_e^o onto an expression in V_e^1 . But, V_e^1 contains, in addition to closed expressions of V_e^o , closed expressions containing unquoted bound occurrences of sentential variables 'p⁰' (of level-one, by Df II). So, the level substitution-range of these variables comprises closed qu-expressions of V_{ρ}^{o} , and term-parameters of V_{ρ}^{1} . Generalising over all such expressions requires that the result of applying 'qu(--)' to expressions in V_e^1 yields an assigned I-value in V_e^2 , which, together with all closed expressions containing unquoted variables 'p¹' (of level-two), comprises the substitution class for bound-variables. As this accumulation of new vocabulary proceeds, of course, each $I_e^$ interpretation based on an assignment I generates denotations for complex terms, and evaluates compound sentences at each level according as I assigns 0 or 1 to their atomic components.

In terms of this cumulative structure of vocabularies in the semantic part of Lqu, the Liar paradox is avoided by never including any sentence containing a (bound) variable 'pⁱ' in the level substitution range of 'pⁱ', unless it occurs in a quotational context. But, then, in that context, 'qu(pⁱ)' will always be assigned a value at a level of vocabulary exceeding that of any sentence formed from the closure of 'qu(pⁱ)'. [Recall that 'pⁱ' is free in 'qu(pⁱ)']. That is, the Liar-sentence:

(L) $(\psi_p^i)(qu(p^i) = C \supset \sim p^i)$ (compare: line (A) p. xxv)

will never appear among the closed substitution instances of V_e^i which evaluates (L) as true. Thus, (L) can indeed be true of all sentences in which sentences of level \underline{Z}_i are mentioned, but never, itself, occurs in the substitution class of the bound (unquoted) occurrence of ' p^i , in (L).

Similarly, with respect to Grelling's paradox, though

(G) qu (F) is heterological

can be true of all predicative occurrences of qu (F) in some V_e^i , the predicate qu(heterological) will then be assigned an extension in V_e^{i+1} , so that

(G') qu (heterological) is heterological is assigned no value at V_e^i (and this holds for each i = 0, 1, 2....).

In this respect, finally, the ungroundedness of paradoxical assertions corresponds in Lqu to sentences being assigned no value at any V_e^i , and, thus, not appearing in any level-preserving vocabulary interpreting Lqu.¹⁶ That some ungrounded assertions are paradoxical only relative to unfavourable contextual circumstances is reflected in Lqu by the fact that assignments of level to closed expressions in the 'object-language' part of Lqu is left open (i.e. not syntactically determined)--allowing closed terms and sentences to find their own level consistent with the assigned values and substitution ranges of the basis-assignment.

FOOTNOTES

Introduction

- 1. Sources for each of these claims are as follows:
 - self-referential: B. Russell, "Mathematical Logic as based on the Theory of Types", <u>American Journal of Mathematics</u>, v.30, 1908, pp. 222-262.
 - analytic: R. Carnap, <u>Logical Syntax of Language</u>, orig. Vienna, 1934; English transl. London and New York, 1937.
 - contradictory: L. Wittgenstein, <u>Tractatus Logico-Philosophicus</u>, First English edition: Routledge & Kegan Paul, 1922, T.4.461.
 - evaluative: A.J. Ayer, <u>Language</u>, <u>Truth and Logic</u>, London: Gollancz, 1936.

metaphysical: R. Carnap, (1934).

- 2. C.W. Morris, <u>Signs</u>, <u>Language</u> and <u>Behaviour</u>, Englewood Cliffs, N.J.: Prentice-Hall, 1946.
- . 3. This is problematic for the technical developments of part II. For certain semantic systems, the class of <u>significant</u> sentences is not recursively characterisable, e.g. let S be the formal system of the relation R(x,z) s.th. $z = Ay T_1(x,x,y)$ --where Ay $T_1(x,x,y)$ is Kleene's function (see S. Kleene, <u>Introduction to Metamathematics</u>, Amsterdam; North-Holland, 1952, p. 325). S will adequately formalise R(x,z) iff S contains a formal relation $A(\overline{x},\overline{z})$ --where ' \overline{x} ', ' \overline{z} ' are Godel Numbers of formulae of S, s.th.
 - (i) $r_{S} T_{1} \xrightarrow{A}(\overline{x},\overline{z})$ iff R(x,z) is true, and (ii) $r_{S} g_{i9} \xrightarrow{A}(\overline{x},\overline{z})$ iff R(x,z) is significant.

Now, if R(x,z) is significant iff $\bigwedge y T_1(x,x,y)$ is defined in S, then Kleene proves (loc.cit.) that the class of Tarski-sentences for S would be recursively enumerable. But, $\bigwedge y T_1(x,x,y)$ is not general recursive; so, there can be no recursive algorithm for deciding whether R(x,z) is significant. Thus, the class of demonstrably significant wffs of S is not recursive. In particular cases, then, a recursive grammar cannot provide recursively specified significance-rules.

4. N. Chomsky, <u>Aspects of the Theory of Syntax</u>, Cambridge, Mass.: M.I.T. Press, 1965, pp. 148-160.

- 5. P. Ziff, "About Ungrammaticalness", Mind, LXXIII, 1964.
- 6. Certain experiments of H.H. Clarke and J. Begum, "Left-to-Right Processing in Grading Utterances", <u>British Journal of Psychology</u>, v.36, 1965, suggest speakers of English frequently judge anomalous utterances as progressively diminishing in intelligibility. For example, "The hat lassoed the calf" is judged less intelligible than "The calf lassoed the hat", though most speakers marked both as anomalous.
- 7. In C.W. Morris' early work--"Foundations of the Theory of Signs" <u>Foundations of the Unity of Science</u>, Volume 1, Number 2,--the distinction between semantic and pragmatic investigations is drawn with respect to the objects of investigation: semantics describes relations between signs and objects (the world); pragmatics deals with relations between signs and speakers.
- 8. Answers to this question are discussed in detail in Part I, where Chomsky, and Katz and Fodor's theories of category-mistakes based upon selection restrictions in the semantic component of a language are criticised. For the moment, it suffices to note that to the extent that Chomsky and Katz regard semantic interpretation as based exclusively upon 'deep structures' of the syntactic component, it follows immediately that semantic interpretation has to be 'contextfree'.

PART I: Section A: (I) Survey of Background:

- 9. J. Lukasiewicz, <u>Elementary Logiki Matematycznej</u>, Mimeo: Warsaw, 1929, English trans1: by O. Wojtasiewicz, New York: McMillan, 1963.
 - G. Ryle, "Categories", PAS XXXVIII, 1938, pp. 189-206.
 - F. Sommers, "Types and Ontology", Phil. Rev. v.72, 1963, pp. 327-363.
 - R. Routley & L. Goddard, The Logic of Significance and Context, Edinburgh: Scottish Academic Press, 1973.
- G.W. Leibniz, <u>Philosophical Papers and Letters</u>, (ed) L. Loemker, Dordrecht: D. Reidel, 1969,
 Part I "Dissertation on the Art of Combinations" (1666), p. 73-84. Part II "On the General Characteristic" (c.1679), pp. 221-228.
- R. Carnap, "The Elimination of Metaphysics" in Logical Positivism, (ed) A.J. Ayer, Glencoe, Ill: The Free Press, 1959, p. 68.
- 12. B. Russell (Russell, 1906), "Review of H. MacColl's 'Symbolic Logic and its Applications.'", <u>Mind</u>, New Series, v.XV, 1906, pp. 528-33.
- G. Frege, <u>Grundgesetze der Arithmetik</u>, vol. ii, Section 56, Jena, 1903, repr. in <u>Translations from the Philosophical Writings of Gottlob Frege</u>, (ed. and transl.) P. Geach & M. Black, Oxford and New York, 2nd. Revised Edtn., 1960, p. 159.

- 14. A. Tarski (Wahreitsbegriff, Polish 1931, German 1936 with additions), "The Concept of Truth in Formalised Languages", transl. from German and repr. in (ed) J. Woodger Logic, Semantics and Mathematics, Papers from 1923 to 1938 by A. Tarski, Oxford: The Clarendon Press, 1956, pp. 152-268.
- 15. W.V. Quine, <u>Word and Object</u>, Cambridge, Mass: M.I.T. Press, 1960, (Quine, <u>1960</u>) D. Davidson, "Truth and Meaning" <u>Synthese</u>, 17, 1967, pp. 304-232. S. Kripke, "Outline of a Theory of Truth", <u>J. Phil.</u>, LXXVIII, 1975, pp. 690-716.
- 16. Ibid. Kripke, 1975, fn. 1, p. 691.
- B. Russell (Russell <u>1908</u>), "Mathematical Logic as based on the Theory of Types", repr. in (ed.) R. Marsh, Logic and Knowledge, London: Allen & Unwin, 1956, p. 88.
- C.f. the attack upon various versions of the verifiability criterion in C. Hempel, "Problems and Changes in the Empiricist Criterion of Meaning", Rev. Int. Phil., IV, 1950, pp. 41-63.
- For example: A.J. Ayer, <u>Language</u>, <u>Truth and Logic</u>, London: Gollancz, 1936.
 R. Carnap (Carnap <u>1934</u>), <u>Logical Syntax of Language</u>, orig. Vienna, 1934.
 A. Pap, Semantics and Necessary Truth, New Haven: Yale U.P., 1958.
- 20. For this criticism see: W.P. Alston, <u>Philosophy of Language</u>, Englewood Cliffs, N.J.: Prentice-Hall, 1964, pp. 76-83.
- 21. This criticism is, perhaps, only fair to the extent that we do not consider Carnap's formal systems of Carnap (1934) and (1939) as adequate to the positivists' claim. For a fuller discussion of this objection, see R. Rorty's Introduction to <u>The Linguistic Turn</u>, Chicago: Univ. of Chicago Press, 1967, especially pp. 4-12 and the references given there. See also the main arguments of F. Waismann, "Verifiability", PASS, 19, 1945, pp. 119-150.
- 22. This is not to say that philosophers of the Oxford School ever held that many of the common utterances of everyday discourse are nonsensical; rather, that the inferences and constructions that philosophers are tempted to draw from familiar usage often generate spurious theses unless due attention is paid to the rich variety and detail of context and use.
- L. Wittgenstein, <u>Philosophical Investigations</u>, transl. G. Anscombe, Oxford: Blackwell, 1953, Section 464, (Wittgenstein, <u>1953</u>).

- 24. It may be objected that implication does not hold between sentences, anyway. This merely shifts the argument onto the statements made. For, if it is agreed that, in its customary sense, implication holds between truth-bearers, it becomes equally unclear how ill-formed non-sentences could bear a truth-value (even a "don't care"--c.f. p. 21, above.).
- 25. G. Ryle, (Ryle, <u>1938</u>), "Categories", <u>PAS</u> XXXVIII, 1938, pp. 189-206. (Ryle, <u>1945</u>), "Philosophical Arguments", Inaugural Lecture as Waynflete Professor of Metaphysical Philosophy; Oxford Univ. Press, 1945, pp. 1-20; repr. in <u>Logical Positivism</u>, (ed) A.J. Ayer, Glencoe, II1: The Free Press, 1959.
 " (Ryle, <u>1949</u>), <u>The Concept of Mind</u>, London: Hutchinson, 1949.
- 26. Thus, the most elegant formulation of simple type theory in a purely formal language is A. Church's "A Formulation of the Simple Theory of Types", J.S.L., 1940--which adopts type-indices 'o, i' as primitive syncategorematic symbols, and recursively generates wffs of each type in the formation-rules of the language.
- 27. T. Drange, Type Crossings, Moulton: The Hague, 1966.
- 28. Ryle, 1938.
- 29. Ryle, 1949, p. 8.
- 30. Ryle, <u>1954</u>, <u>Dilemmas</u>, The Tarner Lectures, Cambridge Univ. Press, 1954, p. 9.
- 31. One can compare this classical account of <u>reductio</u> proof with the Intuitionistic analogue which adopts ' \mathcal{A} ' as a primitive symbol for "absurdity" and permits the inference from the provability of ' $p \rightarrow \mathcal{A}$ ', where p is a thesis, to the provability of the Intuitionistic negation of $p - - ' \neg p'$.
- 32. My emphasis at this point is to draw attention to (i) the centrality of this claim for the main argument of the thesis; (ii) reservations to be frequently expressed in Part I that a proper account of significance for utterances cannot ignore this possibility and does so in treating "meaning" as a context-free, permanent feature of sentence-types.

(II): Section A: Preamble, the strategy of the argument.

1. J. Passmore, Philosophical Reasoning, London: Duckworth, 1961, p. 137.

 The criticisms of type theory, impredicativity, and VCP I discuss in Section C appear in the following:

F.P. Ramsey, "The Foundations of Mathematics", Proc. London Math. Soc., series 2, 25, 1925, pp. 338-84.

 K. Gödel, "Russell's Mathematical Logic", in P.A. Schilpp, ed., <u>The Philosophy of Bertrand Russell</u>, Library of Living Philo-sophers, Chicago: Northwestern U.P., 1944, pp. 123-154.
 W.V.O. Quine, <u>Set Theory and its Logic</u>, Cambridge: The Belknap

Press, 1963, Chapter XI.

- G. Cantor, <u>Gesammelte Abhandlungen mathematischen und philosophischen</u> <u>Inhalts</u>, ed. E. Zermelo, Berlin, 1932, (the relevant quotation appears in Cantor, <u>1895</u>).
- B. Russell, <u>Autobiography</u>, <u>Volume I</u>, London: Allen and Unwin, 1967, p. 147.
- 5. B. Russell, <u>The Principles of Mathematics</u>, London: Cambridge U.P., 1903, see espec. C.s II and V.
- 6. Ibid., Section 43, p. 39.
- 7. Ibid., Section 22, p. 19.
- 8. Ibid., Section 23, p. 21.
- 9. Ibid., Appendix B, Section 497, p. 523.
- Ibid., (Second Edition, London: Allen & Unwin, 1937), Appendix B, Section 497, p. 523.
- 11. Ibid., Appendix B, Section 499, p. 524.
- 12. Ibid. (Second Edition), Revised Introduction, p. xiii.
- 13. Russell's dissatisfaction with his first formulation of type theory-as it appears in Appendix B, 1903, can be inferred from the fact that in Appendix A, in a note, he recommends Frege's solution to the paradox in preferrence to his own (though Frege's "way out" turns out not to remove the inconsistency-see below, Section B).
- 14. These debates--some of which are discussed in Section C--are documented in the following:
 1904, 1906 -- Russell and Couturat: see Couturat, <u>1904</u>, <u>1905</u>, <u>1906</u>.
 1905, 1906 -- Russell and Hobson: see Hobson, <u>1905</u>, <u>1906</u>.
 1904, 1906 -- Russell and Böcher: see Böcher, <u>1904</u>, Russell, <u>1905</u>.

and see also references in Russell, <u>1905</u>, <u>1906</u>, <u>1908</u>, <u>1910</u>, <u>1913</u>, <u>1967</u>; and Correspondence: Russell \leftrightarrow Jourdain, Russell \leftrightarrow Böcher, Russell \leftrightarrow Couturat, <u>Russell Archives</u>, McMaster University.

- 15. This is discussed further in Section C where reference is made to Russell, <u>1905</u> for the theory of descriptions; and Russell, <u>1910</u>, for the doctrine of incomplete symbols.
- 16. B. Russell, 1903, (Second Edition, 1937, p. xiv).
- 17. B. Russell, <u>1908</u>, repr. in Marsh, ed., <u>Logic and Knowledge</u>, London: Allen & Unwin, 1956, p. 59; but Russell's appeal to 'common sense' at this point is qualified in the following line "...common sense is far more fallible than it likes to believe."
- B. Russell, Introduction to Second Edition, <u>Principia Mathematica</u>, London: Cambridge U.P., 1927, Sections IV and V.
- B. Russell, <u>1903</u>, Revised Introduction to Second Edition, <u>1937/8</u>, p. xiii.
- 20. B. Russell, "Review of Ramsey's Foundations of Mathematics and other Essays", Mind, n.s. 46, 1931, pp. 476-512.
- B. Russell, <u>My Philosophical Development</u>, London: Allen & Unwin, 1959, pp. 79-80.
- 22. Quine, 1963, pp. 255-256.

Section B: The impredicativity of Frege's semantic theory.

- 1. B. Russell, <u>1910</u>, vol I, p. 1.
- 2. Russell, <u>1959</u>, p. 76.
- 3. As Poincaré expresses it, somewhat tersely: "There is no actual infinity; the Cantoreans forgot that and they fell into contradiction." H. Poincare, "La logique de l'infini", <u>Revue de Metaphysique</u> <u>et de Morale</u>, 17, pp. 451-82; transl. in <u>Last Essays</u>, ed. J. Bolduc, 1963, p. 47.
- 4. G. Frege, <u>Gg</u>, vol i, <u>1893</u>, Appendices I and II publ. with vol ii, <u>1903</u>, transl. by M. Furth, <u>The Basic Laws of Arithmetic</u>, (First

discipline." (E. Zermelo, "Untersuchungen über die Grundlagen der Mengenlehre I", <u>Mathematische Annalen</u>, 65, 1908, p. 261; transl. in J. Van Heijenoort, <u>1967</u>, pp. 199-215.)

and from Mirimanoff:

"Burali-Forti and Russell have shown by different examples that a set of individuals need not exist, even though the individuals exist. As we cannot accept this new fact, we are obliged to conclude that the proposition which appears evident to us and which we believe to be always true, is inexact, or rather, that it is true only under certain conditions." (D. Mirimanoff, "Les Antinomies de Russell et de Burali-Forti et le problème fondemental de la théorie des ensembles", L'enseignement mathématique, 19, 1917, p. 38).

- 8. J. Von Neumann, Collected Works, vol. i, 1961, see p. 37.
- 9. That this was Frege's view can be inferred from arguments he gives in <u>Gg</u>. vol ii, Apdx II, pp. 255-256, which object to tentative solutions on the grounds that "the generality of arithmetical propositions would be lost." That this remained Russell's view is considered further in Section C.
- 10. Frege, Reply, p. 127.
- 11. Frege, Gg. vol ii, Appdx II, 1903.
- 12. Frege, Gg. vol i, Section 20, p. 36.
- 13. For the inconvenience of the reader, I have transcribed, wherever possible, Frege's elegant, but little known notation into a more modern symbolic ideography. This is almost certainly unfair to the rigour and the inter-connectedness of Frege's Begriffsschrift notation with his semantic and ontological views; I disclaim, at this point, the distortions which arise as a result of these transcriptions.
- 14. Frege, Gg., vol ii, Appdx II, pp. 260-261.
- 15. Frege, Gg., vol ii, Appdx. II, p. 262.
- 16. The inconsistency of Frege's proposal in any domain of <u>Gg</u>. which satisfies '(∃x)(∃y) (x ≠ y)' was first described in lectures by S. Lesniewski, in 1938--so reports Sobocinski, "L'analyse de l'antinomie russellienne par Lesniewski", <u>Methodos</u>, vol. i, 1949, pp. 220+.
- 17. Frege, Gg., Section 146 and footnote *.

- 18. By Russell's "mature conception", I signify the formulations of logicism as a philosophy of mathematics which postdate the formulation of the doctrine of incomplete symbols (1905) and the stimulus this gave to changes in Russell's theory of the proposition and the nature of logic (partly through the formulation of type theory (1908), the use of the theory of incomplete symbols (PM 1910) and the adoption, somewhat later, of Wittgenstein's tautology theory of logical truth (1918, 1927). Before this, Russell's 1903 conception of logicism duplicated Frege's in all essentials--as Russell acknowledges in the Preface to Russell, 1903, First Edition).
- 19. Russell, 1903, Preface to First Edition, p. i.
- 20. For example, see <u>G1</u>., Section 4.
- 21. Frege, Preface to Gg., and also Geach and Black, 1960, pp. 137+.
- 22. Frege, <u>G1</u>., Sections 60-69.
- 23. Frege, Gg., Sections 21-23, pp. 72-78.
- 24. I am espousing, here, what I consider to be the currently accepted view of Frege's 'concept/object' distinction in relation to his theory of reference. It is best explained by M. Dummett in Frege, Philosophy of Language, London: Duckworth, 1973, Chapter 7. The view is that a concept or function is the reference of an incomplete predicate-expression; whilst an object is the reference of a name or sentence. On this view, ascription of reference to conceptexpressions consists in their always resulting in a complete expression when their gap-sign is filled by an appropriate type of complete expression. This view contrasts with that given by Carnap, "The Logicist Foundations of Mathematics", Erkenntnis, 1931, pp. 91-121, and by W. Marshall, "Frege's Theory of Functions and Ojbects", Phil. Rev., LXII, 1953, 374-390; which construes a concept as the sense of a predicate-expression, the reference being the extension of the concept. I will not enter the dispute over these interpretations, save to note that the evidence for the Dummett view (also espoused by M. Furth, 1964) is apparently quite overwhelming, given the newly-released unpublished writings of Frege in Nachgelassene Schriften, ed. Hermes, Kambartel & Kaulbach, Hamburg, 1969; and G. Gabriel's Aus dem Nachlass., Hamburg, 1971.
- 25. Of course, any first-level function or concept is properly defined for all arguments (see below: the doctrine of complete definition), so Frege also gives stipulations of values for these functions for nonsentential arguments.
- 26. Frege, Gg., Section 5, p. 38, footnote 15.

- 27. Ibid., Section 37, p. 96.
- 28. Ibid., Section 21, p. 73.
- 29. Ibid., Section 29, p. 84.
- 30. Frege, Gg., Section 56, vol ii--see Geach & Black, 1960, p. 159.
- 31. Throughout the exposition of Frege's theory of Complete Definition, I am indebted to Dr. David Bell, for points raised in discussion, and from his Ph.D. dissertation: "Frege's Theory of Judgement", <u>McMaster University</u>, Department of Philosophy, 1976.
- 32. Frege, FUB, pp. 32-33.
- See: P.F. Strawson, "On Referring", <u>Mind</u>, n.s. LIX, 1950, pp. 320-344.
- 34. See: Frege, Ged, repr. in Klemke, 1968, p. 511.
- 35. In his reply to Russell's original letter describing the paradox, Frege objects to Russell's formulation of the paradox and states his own version informally and in the symbolism of <u>Gg</u>. (<u>Reply</u>, p. 127).
- 36. Ibid., p. 128.
- 37. Frege, Gg., vol ii, Appdx II, pp. 133-4 of Furth, 1964.
- 38. Subsequent to G1. (1884) in SUB, (1892).
- 39. <u>G1</u>., Section 62.
- 40. See: W.V.O. Quine, "New Foundations for Mathematical Logic", American Mathematical Monthly, 44, 1937, pp. 70-80.
- 41. Frege, <u>Gg</u>., Section 31, p. 87.
- 42. In anticipation of discussion in the next section, it is worth noting that, in criticising Russell's diagnosis of the paradox as a violation of VCP, Ramsey (1925) cites as instances of 'harmless' quantification over impredicative totalities (which violate VCP) only instances formed by a description operator (Ramsey's example: "the tallest man in the room"). In view of the remarks in the text, Ramsey's criticism is beside the point, since it is not the assumption of closure under a description operator which generates an impredicative totality.

- 43. A. Fraenkel, "Zu den Grundlagen der Cantor-Zermeloschen Mengenlehre", Mathematische Annalen, 86, 1922, pp. 230-237; and
 - T. Skolem, "Einige Bemerkungen zur axiomatischen Begründung der Mengenlehre", <u>Matematikerkongress</u>: <u>Helsingfors</u>, den 4-7 Juli, 1922: Den femte Skandinaviska matematikerkongress, Redogörelse, pp. 217-232; repr. in J. Van Heijenoort, 1967, pp. 290-301.
- 44. This interpretation of the Skolem-Löwenheim Theorem--that it renders the notion of 'cardinal number' relative to the interpretation of set theory (or predicate logic)--was Skolem's own construal of the 'paradox'; see:
 - T. Skolem, "Logisch-kombinatorische Untersuchungen über die Erfüllbarkeit oder Beweisbarkeit mathematischer Sätze nebst einem Theoreme über dichte Mengen", <u>Videnskapsselskapets skrifter I</u>, Matematisk-naturvidenskabeligklasse, 4, 1920,; also repr. in J. Van Heijenoort, 1967, pp. 252-263.

Section C: The Theory of Logical Types.

- 1. The totalities given as examples lead respectively to the paradox of the Liar; a version of Grelling's paradox of impredicable properties; and Russell's paradox of non-self-membered classes.
- 2. The criticisms of ramified type theory and its basis from these authors appear in the following:

(i) F.P. Ramsey, <u>The Foundations of Mathematics</u>, London: Routledge, and Kegan Paul, ed. R. Braithwaite, 1931, pp. 1-61; This article first appeared in 1925 in <u>Proceedings of the London Mathematical</u> <u>Society</u>, 25, pp. 338-384--I refer to it in the text as Ramsey, <u>1925</u>, with the pagination of the Braithwaite edition.

(ii) K. Gödel, "Russell's Mathematical Logic", in P. Schillp, (ed), <u>The Philosophy of Bertrand Russell</u>, Library of Living Philosophers, Evanston: Northwestern U.P., 1944.

(iii) W.V. Quine, <u>Set Theory and its Logic</u>, (Revised edition), Cambridge, Mass: Belknap Press, 1969, Ch. XI. Quine's criticisms have appeared elsewhere (e.g. Klemke, <u>1970</u>), but, in the main, I have referred to this edition of Quine, 1963 and 1969.

3. See: Ramsey, <u>1925</u>, pp. 32-49: I should add at this point that, contrary to the customary view, I shall show later that Ramsey's modification of type theory did <u>not</u> consist of replacing RAMIFIED type theory by SIMPLE type theory, as Quine seems to suggest (in Quine, <u>1963</u>, pp. 255-6). "Russell's theory,..., came to be known as the 'ramified theory..'; and Ramsey's position was that it should be reduced to the so-called 'simple'...theory..." This interpretation, endorsed by Church (1956), is historically inaccurate. Ramsey retained the ramified heirarchy of "orders" of propositions, rejected the 'semantic' paradoxes as of no concern to logic, and redefined the type-heirarchy in accordance with his revised notion of a ppnal function in extension. (Ramsey, 1925, 32-49). The simple theory of types (which abolishes the heirarchies of "branching" orders of propositions and functions) seems to have appeared first in Chwistek, 1921--but this article was ignored until Church and Carnap's respective formulations of type theory in Carnap 1934 and Church 1956. The history of this misconception is excessively difficult to unravel.

- 4. C.f. Gödel's comments in Godel, 1944 (above, (ii)), pp. 133-145.
- 5. Gödel, <u>19/4</u>, pp. 136-7.
- Correspondence Russell to Jourdain (1906)--quoted in I. Grattan-Guiness, <u>1972</u>, pp. 103-10. Also, see Grattan-Guiness, <u>1978</u>, pp 30 ff. and p. 78.
- 7. Russell's successive attempts to describe the nature of propositional functions more precisely, and to come up with a uniform notation for them appear in a host of writings--published and unpublished--between 1904 and 1910. See, for example, Russell, 1904a, b, c, and d (unpubl. m.s. Russell Archives); Russell, 1905, 1906b, 1906b, and 1908. These writings contain several different versions of function-expressions and discussions of how they denote. In many of these versions, Russell is frequently unclear on the distinction between the use and the mention of expressions.
- 8. Quine, <u>1963</u>, pp. 255-6.
- 9. The argument reproduces, in effect, Frege's difficulty over referring to concepts (or functions) by substantival phrases of the form "The concept" which--being a singular term--refers to an object. This was discussed briefly in Section B.
- 10. See Quine, 1960, pp. 240-245 and passim.
- 11. See Appendix A and the sources there cited. There is currently much work being done on the reconstruction from m.s. in the <u>Russell</u> <u>Archives of changes in Russell's theory of propositional meaning</u> pre and post-1905.
- 12. The essay cited here is the rewritten third section of a 1906 article "The Nature of Truth", <u>Mind</u>, n.s., XV, pp. 528-533. It is worth noting how the change in the theory of judgement--to the multiple relation theory from the dyadic relation theory ('subjectjudging-proposition') of Russell, <u>1903</u>, <u>1904</u>, --postdates the Theory

of Descriptions and the doctrine of incomplete symbols--which dispense with propositions as autonomous entities.

- 13. Let a, b, c, be some (denumerable) list of constant names. For any R_n(x1...x_n) in <u>AT</u>, each x_i (1 ≤ i ≤ n) is called a "particular term" or "constituent" of the proposition. "R_n" is called a "uni-versal term" or "component" of the proposition (PM2, p. xix). So, for each component R₁, R₂,...every member of <u>AT</u> can be generated by replacing constituents of R_j(x₁...x_j) k-many times in R_k(a,b,c,...,k) (J ≤ k). For example, if j = 1,2,3,..., then R_j(x₁), R_j(x₁,x₂) are in <u>AT</u> and, for k = 2, replacing a, b by "x₁, x₁", "x₁,x₂", "x₁,x₃", ..., "x₂,x₁", ... "x₂,x₃", ... generates 2^k many members of <u>AT</u> for each j. The result of making all such replacements in any R_n--for all finite n--yields denumerably many members of <u>AT</u>.
- 14. As Russell argues in PM2, p. xxii, general propositions cannot be regarded as obtained by step-by-step generalisation of matrices-since the order in which variables are bound by generalisation affects the order of the functions that are thereby determined at each step. For this reason, generalisation of matrices is described as the <u>simultaneous</u> prefixing of quantifiers for all variables to an elementary matrix--followed by re-arrangement of the prefixes to have the whole matrix as scope.
- 15. See the discussion in Quine, 1963, Ch. XII and Section 45.
- 16. By (i), if for a negated atomic p_k , (p_k/p_k) were in U_{ω} , then p_k is in U_{ω} and must be equivalent to $\emptyset a_i$ for some atomic function $\emptyset \mathfrak{A}$ and individual a_i . But U_{ω} contains all results of negating atomics thus, some subset of U_{ω} contains $\emptyset a_i$ and $\sim \emptyset a_i$ and is simply inconsistent. Since all such atomics are in <u>AT</u> of RTT (see footnote 13), then RTT would be simply inconsistent.

By (ii), since $\neg \emptyset a_1 \cdot \neg \emptyset a_1 = \emptyset a_1 / \emptyset a_1$, and all results of combining atomics by the stroke function are members of <u>Mol</u> of RTT, if, for all x, $\emptyset x. \equiv . \neg \emptyset x$, then some two atomics would be incompatible in RTT. But the interpretation of RTT requires that atomic propositions can equally be assigned 'True' or 'False', arbitrarily. At some point, however, if U_{ω} were simply inconsistent, RTT would be Post-inconsistent (an arbitrary atomic proposition would be derivable). Post-inconsistency implies simple inconsistency.

17. It is not to the point to reply that, if there are denumerably many names in RTT, then there are denumerably many things--namely, just these <u>names</u>, <u>qua</u> individual items. RTT does not contain the means of mentioning items in its own syntax. Even if it did, the problem would recur in guaranteeing that infinitely many <u>mentionable</u> names exist!

- 18. The theorem that every segment of the real number series has at least upper (greatest lower) bound is required to demonstrate that "gaps" corresponding to Dedekindian cuts along the series of rationals can always be filled; i.e. that the real number continum behaves "like" a well-ordered series. With respect to the definition of least upper bounds in PM2, however, the bound of any set of segments (which are, themselves, ordered classes of ordered couples of integers) has to be identified with the class of all reals which belong to at least one segment of the set (PM, *210-*214). So, the irrational real which bounds a set of reals in a segment (e.g. all those with squares less than 2) has to be defined as a class of real numbers--hence of one type-level above that of the reals it bounds (the function defining it is of order one greater then its arguments). Elements in the continuum thus become heterotypical. To avoid this requires the Axiom of Reducibility which identifies the function "least bound of a bounded class \hat{x} of reals" with its elementary equivalent -- restoring the type homogeneity of the continuum.
- 19. The following appears to be the fullest Beth-consistency tree for (1)-(3):

| | (1) (x) ; $\phi x \equiv \phi a$ | | |
|------------|--|---|------------------|
| | (2) $\varphi a \rightarrow (\exists y) [(\gamma) (\psi y \equiv \psi a), \gamma$ | (44) | |
| | (3) (3) (3) (4) (4) (4) (4) (4) (4) (4) (4) (4) (4 | | |
| (4) | $(x, \gamma) (x)$ | (٤) | |
| (5) | qa ≡ va | (4-) | |
| (6) (7) | pa ~pa ~pa | | |
| (8) ~ pa | $(\exists \gamma)[(\gamma)(\gamma \gamma \equiv \gamma \alpha), \gamma \neq j](\lambda)$ | | 7 (1) |
| (9) X | $(\gamma)(\gamma b \equiv \gamma a) \cdot \gamma b \sim \phi a (8)$ | $(\exists y) L(y) (\forall y \equiv \forall a)$. | ~ \$ y] (2) |
| (10) (6) | abb (q) abb $b \equiv ba (1) abc$ | (y)(yb = ya). v ~qh | фb (8) (9) |
| (11) | φ6 ~ φ6 | $\phi h \equiv \phi a$ | (1) |
| | ga ~ga | | ~ ub |
| | \mathbf{X} $\sim \dot{\mathbf{\varphi}}\mathbf{A}$ | ф19 Фа | rça |
| | (10) | X | ~ (p C ~ 12 d |
| | , | (10) | i i |
| | | | 6 1 |



Section D: Types and Categories in Ordinary Language.

- By 1918, at least, Russell had advocated (in his Introduction to Wittgenstein's tractatus, <u>1922</u>, and Lectures on Logical Atomism, <u>1918</u>) that a "logically perfect language" would comprise a syntax in which every well-formed sentence would be meaningful. Carnap's similar claim appears in Carnap, 1934 and 1947, Ch. 1.
- A more detailed discussion of the distinction between 'pictorial' and 'representational' form in Wittgenstein's <u>Tractatus</u> appears in J. Griffin, <u>Wittgenstein's Logical Atomism</u>, Oxford U.P., 1964, (Ch. VIII, pp. 95+).

- 3. The evidence is cited in Appendix A, where the relations between Russell's multiple-relation theory of judgement, his doctrine of incomplete symbols, and his theory of meaning are examined in the light of Wittgenstein's criticisms of 1913-1914.
- 4. In particular:

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- L. Wittgenstein, "Some Remarks on Logical Form", <u>PASS</u>, vol. 9, 1929, 162-171. ", <u>Wittgenstein und der Wiener Kreis</u>, - shorthand
 - notes of conversations with F. Waismann, M. Schlick, ed. B. McGuiness, London: Blackwell, 1967.
 - , The Blue and Brown Books, Oxford: Blackwell, 1969.
 - , Philosophical Grammar, Oxford, Blackwell, 1969.
 - , Philosophical Remarks, Oxford, Blackwell, 1964.
- 5. This depends, of course, upon an interpretation of the <u>Tractatus</u> which is not uncontested. It receives its best defence, I believe in Griffin's Wittgenstein's Logical Atomism, Oxford, 1964.
- See: A. Kenny's discussion in Ch. 6, pp. 103-119 of Kenny, <u>Witt-genstein</u>, Cambridge, Mass.: Harvard U.P., 1973.
- 7. That is, they do not share features distinctive of the ungrammaticality of, say:
 - a) * Mon frère et ma soeur sont beaux.
 - b) * If may the were it.
- 8. See, e.g. a Church, "A Formulation of the Simple Theory of Types" J.S.L., 5, <u>1940</u>; and W.V. Quine, <u>Set Theory and Its Logic</u>, Cambridge, Mass.: Belknap Press, 1963, III, xi, Sect. 36.

9. This principle has appeared, in various forms, in:

- R. & V. Routley, "Categories expressions or things", <u>Theoria</u>, 35, 1969.
- T. Drange, Type Crossings, Paris: Mouton & Co., 1966, pp. 11-13.
- R. Thomason, "A Semantic Theory of Sortal Incorrectness", J.P.L., vol. 1, 1973.

I am indebted to Dr. N. Griffin for bringing these to my attention.

- 10. If "*p" is introduced as a 'special' negation, distinct from classical "not-p", and "p & *p" is true for some p, then "(p v *p) v not-p" cannot hold for all p, if "v" is classically defined.
- 11. Notwithstanding that there may be <u>other</u> reasons (failures of reference, presupposition, future contingents) for non-bivalent logics; and separate controversies over 'excluded middle' (Intuitionistic Logic).
- 13. Max Black, "Comments on Arthur Pap's 'Types and Meaninglessness'", read before the American Philosophical Association, Eastern Division, December 1957, as part of a symposium with Pap.
- 14. Since, classically, the significance of "p" is presupposed in its having a truth-value.
- 15. Some of the critical points raised in the discussion which follows are drawn from Wilson, 1967, pp. 55-68; and Katz' reply to Wilson in Katz, 1972, pp. 73-76. I am indebted to points raised in discussion with Dr. Wilson for explication of his criticisms of Katz.

Appendix A: Part I, Section C:

- Russell to Ottoline Morrell, Letter of early 1916, commenting upon Wittgenstein's criticisms in 1913 (repr. Russell, <u>Autobiography</u>, <u>Vol Two</u>, 1968, p. 57)
- "(Theory of Knowledge)", unpublished manuscript, n.d., f.345; Russell Archives, <u>McMaster University</u>.
- 3. The articles published in the Monist, 1914, 1915, were as follows:

"On the Nature of Acquaintance. Preliminary Description of Experience". <u>Monist</u>, 24, Jan. 1914, pp. 1-16. "On the Nature of Acquaintance. II Neutral Monism". <u>Monist</u>, 24, April 1914, pp. 161-87. "On the Nature of Acquaintance. III Analysis of Experience". Monist, 24, July 1914, pp. 435-53.

These three were subsequently collected in Russell, <u>1956</u>, ed. R.C. Marsh, <u>Logic and Knowledge</u>, London: Allen and Unwin, 1956, pp. 125-74.

"Definitions and Methodological Principles in Theory of Knowledge". <u>Monist</u>, 24, Oct. 1914, pp. 532-93. "Sensation and Imagination". <u>Monist</u>, 25, Jan. 1915, pp. 28-44. "On the Experience of Time". <u>Monist</u>, 25, April 1915, pp. 212-33.

4. The revised version of the Multiple Relation Theory appears in Part II, Chs. i-v of the unpublished m.s.--according to Eames and Blackwell's reconstructed Table of Contents: see K. Blackwell and E. Eames, "Russell's unpublished book on Theory of Knowledge". <u>Russell, Journal of the Bertrand Russell Archives</u>, 19, Autumn 1975, pp. 3-18.

- 5. Russell to Ottoline Morrell, Letter #787, 28/5/13: I employ the numbering of the <u>Catalogue</u> of the Russell Archives for Russell's Letters to Ottoline Morrell; the date given is usually that of the postmark.
- 6. Russell to Ottoline Morrell, Letter #783, 1/6/13.
- 7. Russell to Ottoline Morrell, Letter #811, 20/6/13.
- My source for the differences between these versions of the 1913 "Notes" has been B.F. McGuiness' "Bertrand Russell and Ludwig Wittgenstein's 'Notes on Logic'." <u>Revue Internationale de</u> <u>Philosophie</u>, 26, 1972, pp. 444-60.
- 9. See fn. 1, Russell to Ottoline Morrell.
- From the reprint in Russell, <u>1956</u>, of the <u>Monist</u> articles of 1914, believed to be the first six chapters of the 'Theory of Knowledge' m.s.
- 11. In the 1913 m.s. p. 220/ t.s. p. 58, Russell also supplies an arrowdiagram of the multiple relation "U"--see: Blackwell, 1974, p. 70.
- 12. Russell to Ottoline Morrell, Letter #792, late May, 1913.
- 13. Russell to Ottoline Morrell, Letter #793, 1/6/13.
- 14. See: Russell to Ottoline Morrell, Letter #811, 20/6/13.
- Pears' view is given in his discussion article "The Relation between Wittgenstein's Picture Theory of Propositions and Russell's Theories of Judgement"., <u>Philosophical Review</u>, v. 86, April, 1977, pp. 177-96; see especially p. 184, para. 3.
- 16. Based on McGuiness' analysis of the 'Notes' (see fn. 8) and the fact that Wittgenstein visited Russell, apparently, between October 2nd and 9th, 1913: see Letters R. 17-18 and Notes by Von Wright in Wittgenstein, Letters to Russell, Keynes and Moore, ed. G.H. Von Wright, Oxford: Blackwell, 1974, pp. 28-29.
- 17. <u>Principia</u> <u>Mathematica</u>, second edition, <u>1927</u>, Introduction, pp. xiii-xiv.
- 18. Russell to Ottoline Morrell, Letter #787, 28/5/13.
- 19. The evidence for Wittgenstein's using subscripts on Copulae ${}^{t}\epsilon_{n}$ (which is an analogue of the membership-relation) as indicating differences in type-level is <u>extremely</u> slim--this is little better than a guess. In R.3 (1/7/12) Wittgensten uses " \vdash .(x) Øx.

 $\mathcal{E}_{0}(a)$ follows $\emptyset(a)$ " with a zero-subscript to mean " $\vdash \emptyset_{X} \supset_{X} \emptyset_{X}$. \emptyset a follows \emptyset a"--thereby suggesting perhaps that the form of a <u>monadic</u> elementary proposition is $\mathcal{E}_{0}(a)$, in which the elementary, type-0 argument is displayed as a combination of a name a with a copula (e.g. "this" with "being red" to make the complex "thisbeing-red"). In the following letter (R.4, Summer 1912), he uses " $\mathcal{E}_{1}(x,y)$ " to mean, perhaps, that ' \mathcal{E}_{1} ' is the next <u>type</u> of copula- connecting items of different types. Also in a footnote to R.9, he notes "* Pps which I formerly wrote $\mathcal{E}_{2}(a,R,b)$, I now write R(a,b)" indicating that " \mathcal{E}_{2} " was a different type of copula because it relates a, b to a mentioned relation (of type-1)--so the whole combination is of type-2.

- Blackwell does connect the two formulations of the criticism, and reports Pears' and Griffin's comments that they are the same criticism--but he does not demonstrate why they are the same criticism (see: Blackwell, 1974, pp. 76-77).
- D. Pears, <u>Bertrand Russell and the British Tradition in Philosophy</u>, New York: <u>Random House</u>, 1967; see especially Ch. XIII, pp. 212-241.
 J. Griffin, <u>Wittgenstein's Logical Atomism</u>, Oxford, Univ. Press, 1964, pp. 113-114.

K. Blackwell, "The Impact of Wittgenstein on Russell's Theory of Belief" Unpublished M.A. Thesis, <u>McMaster University</u>, 1974, pp. 76-77.

FOOTNOTES II

PART II: Section A, Preliminaries:

- 1. see: M. Cresswell, (<u>1973</u>), Logics and Languages, Cambridge: U.P. D. Lewis, (<u>1970</u>), "General Semantics", Synthese, 22, 18-67. R. Montague, (<u>1970</u>), "English as a formal language", in <u>Linguaggi nella</u>. Societa nella Tecnica, Milan: Edizione di Communita, 189-223. N.L. Wilson, (<u>1974</u>), "The Ontology of General Semantics", Mimeo: read to Canadian Phil. Assoc., Dec. 1973.
- 2. The point is not whether it is methodologically desirable to make this distinction between semantical and pragmatic investigations; but whether all aspects of sentential significance are accountable to the former. My argument will be that <u>removing</u> the pragmatic component has the effect of idealising the notion of significance. (The distinction syntax/semantics/ pragmatics originates with C. Morris (<u>1946</u>)).
- 3. It should be noted that {t, f, n, i} is a derived range for the sentential variables of Routley and Goddard's CL (1973, pp. 110-111). The primary range comprises an unspecified class of sentence-tokens.

II: Section B: Sentences, statements, utterances and propositions:

- Treating sentences (rather than statements) as truth-bearers is often regarded as a necessary, though undesirable simplification; occassionally disclaimed in a footnote: see e.g. Kripke (<u>1975</u>, p. 691, fn. 1).
- E. Lemmon (<u>1966</u>), "Sentences, Statements and Propositions", in <u>British Analytical Philosophy</u>, ed. B. Williams and A. Montefiore, London: Routledge & Kegan Paul.
- 3. Quine, <u>1960</u>, p. 181: his charge is part of a sustained attack upon 'analyticity' based upon 'sameness of meaning'.
- 4. This, of course, is relative to the tensing system of English. I am told that not all languages shift the tense of a mentioned statement in this way; e.g. Russian.

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- 5. J.L. Austin, 1962, pp. 98-9.
- 6. See: Quine, <u>1953</u>, "Two Dogmas of Empiricism", in <u>From a Logical</u> Point of View, Cambridge: Harvard U.P.
- 7. Quine, 1960, pp. 58-72.
- 8. Quine exempts truth-functional tautologies and "stimulus analytic" sentences from translational indeterminacy (ibid: 42-44, 57-58).
- See Wilson, <u>1959</u>, "Substances without Substrata", <u>Rev. Met.</u>, xii,
 4, p. 532.
- In D. Davidson, <u>1969</u>, "Truth and Meaning" in (ed.) J. Davies (<u>et. al.</u>) Philosophical Logic, p. 237.
- 11. In Quine, 1960, §§ 40-42.
- 12. L. Carroll, "What the Tortoise said to Achilles", Mind, IV, 1895, p. 232: in brief, Carroll generates a regress from supposing that, for A to infer q's truth from $p \supset q$, p, A would have to know the truth of $p \supset q$, p and $((P \supset q) \& p) \supset q$. But, then, to know that q's truth followed from these, A would have to know ' $p \supset q$ ', 'p', '($(p \supset q) \& p) \supset q$ ' and '[($(p \supset q) \& p$) & ($(p \supset q) \& p) \supset q$ '... and so on.
- 13. Austin, 1962, p. 98.
- 14. Originally, Austin conceived that a locution possessed illocutionary force and perlocutionary effect. In "Performative-Constative", he came to recognise that performative utterances were not alone in carrying out an <u>act</u> in being uttered, constative utterances also perform illocutionary acts. Austin, 1958, p. 271-304.
- 15. Austin, 1962, Lecture X and pp. 120-1.
- 16. Above p. 399, a).
- 17. The technique of giving a definition of x in terms of identityconditions for x's originates, I believe, in Frege's definition of number within the context of equations (a definition he puts forward only to reject, subsequently). See Frege <u>G1</u>, <u>1884</u>, transl. J.L. Austin (<u>1953</u>) Oxford: Blackwell, §§ 62-69.
- 18. Significance, thus, is presupposed in some truth-claim being made in the utterance of a sentence. This is one reason not to treat sentences as both significance and truth-bearers; since the values 'significant', 'true' belong to different semantic levels.

- 19. The objection is not that S does not have a belief that-a unless he has the requisite intention to state that-a and elicit recognition of his intent in us. Rather, the objector claims that <u>ascrip-</u> tions of belief to S are based upon evidence of his intentions (and not vice-versa).
- 20. Roughly: the contrast between Davidsonian theories of 'meaning' as 'truth-conditions' and Gricean theories of 'non-natural' meaning.
- 21. J. Searle, (1969), pp. 43-9.
- 22. H. Grice, (1957), p. 385-6.
- 23. P. Strawson, (1964) in Searle (ed.) (1971) pp. 32-3.
- P. Ziff, "On H.P. Grice's account of Meaning", <u>Anal.</u>, xxviii, <u>1967</u>, pp. 1-8.

II: Section C: Statement Identity in CL:

- Formulation of CL is, in a large part, derived from the context logic CL of Routley and Goddard, (1973), Part I. In particular, much of the syntax of my CL--qu(-), Y, §(-)--follows Routley and Goddard, though the semantic account is my own.
- '§(-)', clause-forming on sentences, is primitive in CL--but its value-range, the domain <u>St</u> of statements is characterised algebraically in II, C (III).
- This represents a possible solution to the regress which Carroll posed for Russell's account of conditionals ("implication"). C.f. II, B (p. 395).
- 4. 'Aboutness' is defined in II, D (A). Content G (for "Gedanke") is identified in terms of statement- and significance-conditions in II D (B-C).
- 5. See Appendix (B).
- 6. The relation "Yields" is analogous to the G-relation (for "Gives") in N. Wilson, (1959), p. 47. This in turn elaborates on certain suggestions of Carnap in 1947, pp. 71-73.
- 7. Defined below in (IV-V).
- 8. See f.n. 1 (II, C).

- C.f. Routley and Goddard, <u>1973</u>, p. 65, following a suggestion of W. & M. Kneale The <u>Development of Logic</u>, Oxford: U.P., 1952, p. 539.
- 10. It is not properly the Sheffer stroke $\emptyset / \psi = \sim (\emptyset \& \psi)$, since it admits "truth-value gaps".
- 11. C.f. B. Van Fraassen, 1969, pp. 477-487.
- 12. This requirement <u>could</u> be relaxed to make CL a "free logic"; but the additional complications needed to achieve this are disadvantageous.
- 13. Ibid.
- 14. The issue is taken up again briefly--in connection with "utteranceaboutness" in CS-1 (II, D, (A)).
- 15. Some of the following relies upon Strawson, 1974, Ch. 1.
- 16. Marking the distinction in terms of whether the term is negatable dates back at least to Aristotle (Categories).
- 17. The analogy is not exact--but the term 'forcing' itself carries an additional connotation with respect to a statement's being <u>made true</u>. I record here my debt to B. Van Fraassen (private communication; May 1976) for his advice on the use of 'C₁ forces C_j', (see Van Fraassen, 1969).
- 18. See J.M. Dunn (<u>1966</u>) "The Algebra of Intensional Logics" Ph.D. dissertation, Univ. of Pittsburgh.

A complete, distributive lattice is a non-empty partially ordered set A such that for all a, b, c ε A (i) a ∩ b, a ∪ b ε A (ii) a ∩ (b ∪ c) = (a ∩ b) ∪ (a ∩ c) (iii) a ∪ (b ∩ c) = (a ∪ b) ∩ (a ∪ c) (iv) $\overline{a} = a$ (v) if aRb, then \overline{bRa} (where R partially orders \widehat{A}) (vi) T is a filter on A which is consistent (for no a ε A, a ε T and \overline{a} ε T) and complete (for all a ε A, a ε T or \overline{a} ε T).

- 19. Ibid.
- 20. see: Anderson and Belnap, (1975), Ch. 25.1.
- In D. Davidson, (<u>1967</u>), "Truth and Meaning" repr. in J. Davies, (1967) Philosophical Logic, Amsterdam: D. Reidel.

II: Section D: Significance and Context: (CS-1).

- For example, all of the QS-logics of Routley and Goddard (1973, Ch. 2) exhibit, to some extent, an inference-structure for significance logic-- despite the absence of a fully articulated semantics of significance-range theory for these systems. See also, for a rival account based upon 'supervaluations': R.Thomason, "A Semantic Thoery of Sortal Incorrectness", J.Phil.Log, 1,1972, pp. 209-258.
- 2. This notion of a super-predicate appears in Routley, (1966), where a characterisation of the 'subordination' relation is given formally. I have borrowed the term, and provided some indication of how it is interpreted in relation to the algebras of aboutness and general terms, despite the fact that our uses of the notion differ widely.
- 3. Of course, this argument assumes that, <u>unless</u> there is some formal explication of <u>how</u> a relevant entailment relation proscribes the "Paradoxes of material implication" of which "§¬q → §(¬p∧¬q)v¬q" is an instance, then defining the aboutness of antecedent and consequent, ingeneral, for a truth-functional <u>implication</u> is vulnerable to this argument.
- II: Appendix (B): Type/token, Use/mention and Quotation.
- '1. This summary is taken from Routley and Goddard, (1966).
- 2. See the discussion in Belnap and Grover , "A Substitutional Interpretation for Quotation Languages", in H. Leblanc and R.K. Meyer, (1973).
- 3. N. Comsky, (1957, p.37) and (1965, p. 131).
- See: Van Fraassen (1969), and references in Kripke, (1975, pp.692-3) to R. Martin and Herzberger's work on paradoxes of "ungrounded assertion".
- L. Henkin, "Completeness in the Theory of Types", in Hintikka (ed.) (<u>1969</u>), <u>The Philosophy of Mathematics</u>, (Oxford Readings in Philosophy), Oxford: U.P.
- 6. H. Leblanc and R.K. Meyer, (1973), pp. 76 83.

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Only thoseworks to which reference is made in the text are noted (together with sources from anthologies, reprintings, etc.). Works are listed by author alphabetically and then chronologically by date of publication. Thus, as the text gives a reference to 'Quine, 1960, p. ...' the work is located below under 'W.V.O. Quine, (1960), Word and Object', etc. Unless otherwise stated, book references are to the first edition, and paginationreferences to the first edition or stated source.

In the text the abbreviation:

| OED | appears for: | Oxford English Dictionary |
|------------|--------------|---|
| PM | appears for: | B. Russell and A.N. Whitehead, (1910-1913), |
| | | Principia Mathematica, Cambridge: U.P. (First |
| | | edition) |
| PM2 | appears for: | Russell and Whitehead, (1925), Principia |
| | | Mathematica, Cambridge: U.P. (Second edition, |
| | | with new Introduction and Appendices). |
| <u>T</u> . | appears for: | L. Wittgenstein, (1922), Tractatus Logico- |
| | | Philosophicus, (transl. of "Logisch-philosophische |
| | | Abhandlung" first publ.: Annalen der |
| | | Naturphilosophie, <u>1921</u> , Transl. by D.F. Pears |
| | | & B.F. McGuiness with parallel German text, |
| | | 1961 edition, London: Routledge & Kegan Paul). |

A list of abbreviated references to works by Frege is to be found in footnote 4, Part I, p. vii.

The following is a list of journals and a schema for their abbreviated reference as they appear below.

Acta Philosophical Fennica: <u>APF</u> Analysis: <u>Anal</u>. Australian Journal of Philosophy: <u>AJP</u> American Journal of Mathematics: <u>AJM</u> British Journal of Psychology: <u>BJ Psych</u> Erkenntnis: <u>Eknts</u> Journal of Philosophy: <u>JP</u> Journal of Philosophical Logic: <u>JPL</u> Journal of Symbolic Logic: <u>JSL</u> Logique et Analyse: <u>Log. An</u>. Mathematischen Annalen: <u>Math Ann</u> Mind: <u>Mind</u> Monist: Monist Notre Dame Journal of Formal Logic: NDJFL Philosophy: Phil Philosophical Review: Phil. Rev. Proceedings of the Aristotelian Society: PAS Proceedings of the Aristotelian Society (Supplementary): PASS Proceedings of the London Mathematical Society: Proc L. Math Revue de Metaphysique et de Morale: Rev. Met. Mor Revue Internationale de Philosophie: Rev. Int. Phil Review of Metaphysics: Rev. Met. Synthèse: Synth. Theoria: Theoria. Nous: Nous Zeitschrift fur Philosophie und philosophische Kritik: Zeit P.K.

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