A DEFENSE AGAINST QUINE'S ATTACK ON ANALYTICITY

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This thesis is an attempt to rescue the analyticsynthetic distinction from the attacks of W.V.O. Quine and his followers. In particular, the notion of synonymy in natural languages is examined along with a critical discussion of attempts to define this notion. Also, Carnap's explication of logical truth is discussed to some length with emphasis on semantical rules, meaning postulates, and statedescriptions.

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> D.C. (1970)

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CHAPTER I

INTRODUCTION

The analytic-synthetic distinction has up to this day been considered by many to be the unproblematic cornerstone of modern analytic philosophy. And while there has been some controversy in the past having to do with this distinction, the legitimacy of the distinction itself was never questioned. Rather, these controversies have had to do with the claims that there is no synthetic a priori or that all necessary truth is analytic; there has also been some question as to whether the statements or propositions of mathematics and the natural sciences such as, e.g., 'F=MA', should be considered analytic or synthetic. But though these questions have probably never been resolved to everybody's satisfaction, it has been generally accepted that the analytic-synthetic distinction is a valid one no matter what the outcome of these other issues. An analytic statement (it has been widely thought) is simply one whose truth is known by mere reflection on the meanings of the terms which occur in the statement without regard to extra-linguistic facts; a statement whose denial is self-contradictory or inconceivable. Hence, though there may have been considerable disagreement over what statements met the above stated criterion, hardly anyone doubted that some statements fell into the class of analytic statements and all other statements fell into the class of synthetic ones.

Recently however, the general charge has been made that this (alleged) distinction is nothing more than "an unempirical dogma of

empiricists"¹; an "untenable dualism"² which can only be maintained as a "metaphysical article of faith."³ The charge in particular is that all the so-called definitions or explications of analyticity have had little or no explanatory value (or. at best. have been circular), for in each case the explicatum or definiens is in as much need of clarification as the notion of analyticity itself. The notion of a meaning, as some entity between word and object, is as obscure a notion as anyone will care to find and can hardly serve to make the notion of analyticity any clearer. Similarly, the notion of self-contradictoriness in the broad sense needed for an explication of 'analytic' is in precisely the same need of clarification as is the concept of analyticity itself, "The two notions are the two sides of a single dubious coin,"⁴ Hence, both Quine and White claim that they have no understanding of what 'analytic' means and will remain without an understanding until a general definition of 'analytic' is provided which does not rest on concepts equally as unclear.

More recently, N.L. Wilson has outdone both Quine and White in professing not to understand many other semantical terms any better than 'analytic', which, he confesses, he does not understand.⁵ For the semantical terms such as, e.g., 'logically true', 'true', 'designates', 'sentence', etc., are as badly in need of a general definition as is the

¹W.V. Quine, "Two Dogmas of Empiricism," reprinted in his book, <u>From a logical Point of View</u>, Harvard University Press, 1964. ²Morton White, "The Analytic and the Synthetic-An Untenable

²Morton White, "The Analytic and the Synthetic-An Untenable Dualism," reprinted in L. Linsky's <u>Semantics and the Philosophy of</u> <u>Language</u>, The University of Itlinois Press, 1952. Quine, <u>Op. Cit.</u>, p. 37. ⁴Quine, <u>Op. Cit.</u>, p. 20.

³Quine, Op. Cit., p. 37. ⁴Quine, Op. Cit., p. 20. ⁵N.L. Wilson, The Concept of Language, University of Toronto Press, 1959, Chapter 1.

term 'analytic'. All that has been provided so far in semantics, with respect to any of these terms, is a definition which holds only with respect to a particular language; we have no definition of any of these terms which holds for all languages in general, not even all artificial languages. More will be said about this problem in Chapter IV.

Now before launching into an examination of the objections raised against the analytic-synthetic distinction, I want to first take up the more modest task of simply putting the notion of analyticity in its traditional context. In doing this, I do not intend to present a history of the analytic-synthetic distinction, but rather only a brief sketch which illustrates the development of this distinction and the notion of analyticity over the last few hundred years. Let me begin this sketch then, with the ideas which Leibniz expressed on the subject.

Leibniz spoke of two kinds of true propositions; there are, he claimed, "truths of reason" and "truths of fact."⁶ These latter truths are about the world and true in virtue of the way the world actually happens to be. Now the phrase 'about the world' is not altogether clear. It could mean that a given statement is in the object language and hence is not about language. However, this is a doubtful interpretation of what Leibniz intended by the phrase. What is a more probable interpretation of the phrase 'about the world' is that empirical observation is required in order to determine the truth of the statement. For example, the statement 'Salt dissolves in water' is true partly because we use the constituent words the way we do and partly because salt does dissolve in

⁶G.W. von Leibniz, "New Essays Concerning Human Understanding," reprinted in The Monadology and Other Philosophical Works, translated by R. Latta, (New York: Oxford University Press, 1945), Book IV.

water. Now though it is a fact that salt dissolves in water, this fact can certainly not be known by an understanding of what the words, 'salt', 'dissolves', 'in', and 'water' mean, prior to experience. What is essential in such a statement is that the denial is not self-contradictory. i.e., it is possible in, let us say, another world.

Truths of reason, on the other hand, are true not only of this actual world but of all possible worlds as well." What Leibniz meant by this is that we cannot consistently conceive of a world in which the denial of any of these statements would be true. The notion of possible worlds has its modern counter-part in Carnap's notion of 'state-description'.⁸ A state-description is a class of sentences of a language which contain, for every atomic sentence, either the sentence or its denial. An analytic sentence then, is one which holds in all state descriptions. Now because the notion of 'possible world' is so similar to that of 'statedescription', it is not surprising that an objection which has been made against Carnap's state-descriptions can also be made against Leibniz' notion. That is, how are we to know beforehand whether or not a possible world is one in which, c.g., some bachelors are married? We know intuitively that the sentence 'Some bachelors are married' is necessarily false, but without a criterion of some sort, we do not know whether this sentence will hold true in a possible world without relying on our intuition. But we cannot rely on our intuition, for that is what we are trying to get clear about in explicating the phrase, 'truth of reason'.

Leibniz does, however, supply us with a criterion. All truths of reason are statements which can be demonstrated by the principle of

7 Leibniz, Op. Cit. 8 see R. Carnap's Meaning and Necessity, The University of Chicago Press, 1947.

contradiction or the principle of identity alone. Incidently, since this criterion corresponds to Kant's criterion of analytic truth (as is given in the <u>Critique of Pure Reason</u>), it is probably fair to say that Leibniz anticipated the modern view that all necessary truths (truths of reason) are analytic. This criterion, though, is not free from at least one serious shortcoming.

The drawback of the above stated criterion "is that in most cases the reduction of a necessary truth to an identity, total or partial, presupposes principles of deduction which are themselves necessary truths but cannot be held to be in turn thus reducible,"9 Many deductions require principles which are equivalences legitimizing substitutions. These equivalences must, of course, themselves be shown to be necessary, since a contradiction is deducible even from the denial of a contingent proposition if we use certain contingent propositions as premises. The problem here is simply that the principle of contradiction and the principle of identity alone are not sufficient to demonstrate all the propositions which Leibniz would have held to be a truth of reason; other logical truths are needed as well. This may tempt one to reformulate Leibniz' criterion to read: a proposition P is a truth of reason if a contradiction is derivable from not-P with the help of logical truths alone. However, even with this reformulation, we are still faced with the problem of showing that the logical truths are themselves truths of reason. We trivialize the above

9A. Pap, <u>Semantics and Necessary Truth</u>, New Haven and London: Yale University Press (1958), p. 8.

criterion if we try to show from it that the logical truths are truths of reason, for in that case we would simply use the negated logical truth T along with T itself as our premisses. Still, whatever the defects of Leibniz' characterization of the two kinds of true propositions which we are apparently faced with, we must admit that he did lay the groundwork on which other philosophers might clarify the apparent difference of statements in a more satisfactory way.

One such philosopher who attemtped a further elucidation of the notion of necessary truth (as opposed to contingent truth) is Immanual Kant. According to Kant, there are two types of knowledge, viz., <u>a</u> <u>priori</u> knowledge and <u>a posteriori</u> knowledge. This latter kind of knowledge, as the name would indicate, can only be obtained after or through experience and the judgments which express such knowledge Kant calls <u>synthetic</u>. A synthetic <u>a posteriori</u> then, insofar as its truth can be known only through empirical observation, corresponds to what Leibniz called truths of fact. <u>A priori</u> knowledge, on the other hand, is independent of all experience. "Necessary and strict universality are thus sure criteria of <u>a priori</u> knowledge, and are inseperable from one another."¹¹

But there are, Kant says, not one, but two kinds of judgments which afford a priori knowledge. For, according to Kant, some judgments such as, e.g., 'All bodies are extended', are analytic a priori, while other judgments are synthetic a priori such as, e.g., '7+5=12'.

¹¹Immanual Kant, <u>Critique of Pure Reason</u>, translated by Norman Kemp Smith, New York: St. Martin's Press (reprinted in 1965), Introduction, Section I, p. 44.

An analytic judgment is one in which the predicate B is coverily contained in the subject A. In contrast to analytic judgments, synthetic judgments are those in which the predicate B cannot be found within the concept of the subject A. Thus the judgment 'All bodies are extended' is an analytic judgment on the ground that the concept of extension is already conceptually contained (somehow) in the concept of the subject. While, on the other hand, the statement '7+5=12' is synthetic on the ground that the concept of twelve is not already thought by merely thinking of the addition of seven to five. Analytic judgments then, "add nothing through the predicate to the concept of the subject, but merely break it up into those constituent concepts that have have all along been thought in it, although confusedly."¹² Now it is well known that there has in the past been much controversy over whether mathematical propositions should be regarded as analytic or synthetic; the controversy rages in some quarters even today. However that may be, I do not here wish to pursue the issue. What is of interest to me here is Kant's use of the word 'analytic'.

Before going further, we might stop to remark on the observation that Kant uses the term 'analytic' with respect to judgments rather than statements or propositions. And just what Kant meant by 'judgment' is not entirely clear. He may have used it in the sense that the term 'proposition' is often used, that is, to account for sentences of different languages (or different sentences of the same

12Kant, Op. Cit., p. 48.

language) having the same meaning. Alternatively, and what seems more likely, Kant may have used the term to refer to the mental act of judging. Thus we judge of a particular object that it has a particular property. This latter interpretation of 'judgment', incidently, might help us to understand why Kant emphasized so heavily the subjectpredicate form of judgments. Finally, perhaps Kant even had both of these interpretations in mind (albeit in a confused way) when he used the term.

But putting aside the problem of interpreting how Kant used the word 'judgment', there are other problems connected with Kant's explication of 'analytic'. Kant does not give one straightforward criterion for distinguishing analytic from synthetic judgments. Rather, he gives two distinct criteria which may or may not be equivalent. One of these criteria we have already mentioned, viz., a judgment is analytic if its predicate is (conceptually) contained in its subject. The other criterion.which Kant gives is that a judgment is analytic if it rests on the law of contradiction alone.¹³ Now the two criteria given by Kant might be said to be equivalent <u>provided</u> we assume that a judgment (in Kant's jargon) can be thought of as a statement. Then we might say that an analytic statement is one in which the predicate term actually occurs in the subject expression such as in, e.g., the statement 'All black cats are black' or, put symbolically,

'(x)((Bx&Cx))Bx)'

13_{Kant}, <u>Op. Cit.</u>, p. 49.

Under this interpretation, to deny an analytic statement would be to violate the law of contradiction. However, it is doubtful that Kant intended such an interpretation. For in his own example of an analytic statement, viz., 'All bodies are extended', the predicate term does not occur in the subject expression. And so, obviously something else is required (like a definition) in order to extract this statement from the law of contradiction.¹⁴

In a way though, it is fortunate that Kant provided two criteria. For his first criterion (that a statement is analytic if the predicate is contained in the concept of the subject) is hopelessly restricted, as there are an indefinite number of statements (or judgments) which do not have the required subject-predicate form. For example, the negative judgment 'No triangle has four sides' would undoubtedly have been considered by Kant to be analytic. Yet the predicate is so far from being conceptually contained in the subject that it actually contradicts the subject.¹⁵ Moreover, existential judgments such as, e.g., 'There are philosophers' would have been considered synthetic by Kant, but the only way of construing the judgment as having subject-predicate form would be to contradict Kant's own thesis that 'existence' is not a predicate. Other kinds of judgments. Thus, insofar as Kaut intended

¹⁴According to Ayer (Language, Truth, and Logic), the two criteria should be considered not equivalent, for "from the fact that one can think of the sum of seven and five without necessarily thinking of twelve, it by no means follows that the proposition '7+5=12' can be denied without contradiction." p. 78. And so, it appears that a judgment which is synthetic by one criterion might be analytic by the other. 15 pap, Op. Cit., p.27.

the analytic-synthetic distinction to exhaust the entire class of true judgments, this criterion fails.

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Kant's other criterion (that a judgment is analytic if it can be extracted according to the law of contradiction) seems then to be more comprehensive. Also, it has the welcome feature of being a <u>logical</u> criterion rather than a psychologistic one. However, this second criterion is open to the same objection which we made against Leibniz' criterion for distinguishing truths of reason from truths of fact. That is, the principle of contradiction is not sufficient to demonstrate all the judgments which we would want to call analytic.¹⁶ Other logical truths are needed as well, but the analyticity of these logical truths cannot be shown on the basis of the above criterion. Hence, we cannot be satisfied with either of Kant's criteria.

In more recent times, especially since the publication of Wittgenstein's <u>Tractatus</u>, philosophers (particularly empirical philosophers) have been inclined to refer to analytic statements as <u>tautologies</u>. And as tautologics, analytic sentences say nothing (factually). Now, for Wittgenstein at least, the word 'tautology' has a specific meaning, viz., a tautology is any truth-functional compound having the truthvalue T regardless of the truth-values of its constituent propositions.¹⁷ This concept of a tautologous truth-function, though it has the virtue of making all questions of logical truth decidable, is nevertheless restricted to truth-functional <u>compounds</u>, i.e., molecular statements

16 Again see Pap, Op. Cit., p. 29.

¹⁷Ludwig Wittgenstein, <u>Tractatus-Logico Philosophicus</u>, trans. by D.F. Pears and B.G. McGuiness, New York: The Humanities Press, 1961, 4.27-4.46.

such as, e.g., 'If (roses are red and violets are blue), then roses are red'. This statement is true regardless of the truth of its component statements. Thus it seems that insofar as we restrict Wittgenstein's definition to the propositional calculus, there is no problem. But what about statements of the functional calculus in which we find the occurance of quantifiers ranging over individual variables? Truthtables will not work when it comes to testing statements which we would want to consider logically true such as, e.g., '(x)(Px)=-(Ex)-Px'.

Wittgenstein apparently thought that his concept of 'tautology' was adequate for the explication of logical truth even for the statements of the functional calculus because he maintained that all propositions are truth-functions of atomic propositions. And Wittgenstein was evidently led to this opinion because he thought that he had found a way to define quantification. The notion of tautologous truth-function is applicable to statements involving quantifiers if the universal assertion of a function ¹⁸ is construed as the assertion of the conjunction of its instantiations. Similarly, an existential statement can have its function reduced to the disjunct of its instantiations. Thus, the definitions of '(x)(Fx)' and '(Fx)' will be formulated in the following way:

(D1) (x)(Fx)=df Fa&Fb&Fc&..., Fn.

(D2) (3x)(Fx)=df FaVFbVFcV...,Fn.

But the crucial objection against these definitions is that,

¹⁸Bither monadic or dyadic.

even though the implication from left to right is in accordance with the principle, "what is true of any is true of all," the implication from right to left is true only if we assume that the individuals signified in the right-hand side exhaust the entire class of individuals in the universe of discourse. For, "we want the left-hand side of (D1) to mean that all individuals are F, and even supposing that a, b, c, ... and n are all the individuals there are, there is nothing on the right-hand side of the definition to <u>say</u> that these are all the individuals there are."¹⁹

It might be suggested that the right-hand side of the definition (D1) can be improved upon by reformulating it to read, (D3) (x)(Fx)=df ((Fa&Fb&Fc&...Fn)&(w)(Fw) w=aWw=bVw=cV...w=n).

The right-hand side of (D3) does tell us that a, b, c, ..., and n are all the individuals of the universe of discourse. However, unfortunately the definition (D3) is circular because we are using in the right-hand side the very type of expression which we are trying to define, viz., the universal quantifier. Thus, Wittgenstein's definition of 'analytic' is not satisfactory because it is not applicable to the full range of sentences which intuitively we would want to call analytic.

I hope then that by the above I have placed the problem of the analytic-synthetic distinction in the proper historical perspective. I think we can fairly say that though the attempts of Kant, Leibniz, and Wittgenstein to clarify the distinction may have provided some pen-

¹⁹N.L. Wilson, Loc. Cit., p. 106; see also Pap, <u>Semantics and</u> <u>Necessary Truth</u>, pp. 143-149.

etrating insights into the nature of some kinds of statements, none of their criteria or definitions, with respect to the analytic-synthetic dismtinction, can be considered to be fully satisfactory. Let us then, turn to the problem as it faces us today, that is, is there really a clear distinction at all between those statements which we call analytic and those statements which we call synthetic.

CHAPTER II

BEHAVIOR, LANGUAGE, and FORMALIZATION

In discussing the problem of defining analyticity (or, for that matter, any theoretical term having to do with languages) we are faced with a far more basic problem. That is, the problem of determining just how it is we are going to treat languages. Without sounding too obscure, we must determine the ontological status of a language before we can set about trying to define any of its semantical or theoretical terms. For quite naturally, the method with which we seek a definition of, let us say, analyticity or synonymy, will largely be determined by our view of what a language essentially is. Hence, it is not at all surprising that Quine, for example, since he understands a language to be "first and last a system of dispositions to observable behavior,"¹ believes that those who are interested in understanding theoretical linguistic terms should concern themselves with definitions of such terms (as that of analyticity) based on linquistic behavior.² Nor is it surprising to find that Carnap is puzzled that Quine should require pragmatic (i.e., behavioristic) definitions for terms which he thinks to be semantic in nature,³ seeing that for Carnap semantical rules constitute a semantical system.⁴ And even

1_{W.V.} Quine, "On a Suggestion of Katz," Journal of Philosophy, (1967), p. 52. Quine also seems to take this stance in his book Word and Object p. 27, "We are concerned here with language as the complex of present dispositions to verbal behavior..."

²Quine, "Carnap and Logical Truth," P.A. Schilpp, ed., <u>The</u> <u>Philosophy of Rudolf Carnap</u> (Library of Living Philosophers), Evanston, p.403.

though Carnap does take up the challange of Quine and attempts to give behavioristic criteria for analyticity and synonymy,⁵ he does not intend to provide a <u>definition</u> of analyticity in terms of linguistic behavior but only a criterion of a behavioristic sort for purposes of testing when an expression is analytic for a user of a particular language. More will be said about this later.

Still, we must emphasize the point which some philosophers have apparently overlooked.⁶ This is the simple fact that to give a behavioristic criterion for the purpose of determining, let us say, when two expressions are synonymous is not to <u>define</u> synonymy in terms of linguistic behavior. In other words, the definition of synonymy, if there is one, may still well be on a semantical level. Behavioristic criteria may well be indispensable for testing the empirical adequacy of definitions for such terms as 'analytic', but this is by no means to say that behavioristic criteria are indispensable for the defining

³See Carnap's reply to "Carnap and Logical Truth," which is also in Schilpp's volume on Carnap.

⁴See Carnap, <u>Foundations of Logic and Mathematics</u>, International Encyclopedia of Unified Science, volume I, p. 7. Incidently, it is worth mentioning that Carnap, in the same monograph, also says that "a language, as, e.g., English, is a system of activities or rather, of habits, i.e., dispositions to certain activities, serving mainly for purposes of communication and of co-ordination of activities among the members of a group." p. 3. The reason for this apparent discrepancy is perhaps that Carnap, as so many philosophers do, mistakenly or not, makes a distinction between natural languages such as, e.g., English, and formalized "artificial" languages which Carnap prefers to call semantical systems. I would call semantical systems languages and vice versa, but more will be said of that later.

5Carnap, "Meaning and Synonymy in Natural Languages," Philosophical Studies, vol.VI (1955), p. 33.

6 Including Quine himself! See "On a Suggestion of Katz."

of theoretical terms.

Nevertheless, it does seem to be consistent for one who thinks that a language is to be defined in terms of behavior, to think that the theoretical terms such as that of analyticity and synonymy will be defined in terms of behavior also. So, perhaps it can be more easily seen why a behaviorist such as Quine (and perhaps Morton White as well) will contend that they do not understand what 'analytic' means just as they do not understand what 'synonymous', 'necessary', 'possible', or 'contradictory' mean. 7 Quine says this because he has not been shown how these terms are reflected in behavior; these terms have not been defined in terms of linguistic behavior. And Quine is not content with, indeed, he is not even concerned to dispute, the formal correctness of the definitions of these terms in pure semantics. "He doubts whether there are any clear and fruitful corresponding pragmatical concepts which could serve as explicanda."⁸ It seems then, that it is not so much that Quine does not understand 'analytic'; he understands what Carnap, at least, is saying about analyticity. Its just that Quine refuses to allow that there are any sentences to which we can apply the predicate in a non-arbitrary way. The word 'analytic' then, is not gibberish for Quine, it is more like the word 'unicorn'; meaningful but not true of anything.

We should, of course, not assume that Quine is saying he does not understand these terms (whereas most other philosophers do) because he is exceptionally stupid. The contention is that nobody else understands them either, even if they think they do. 8 Carnap, "Meaning and Synonymy," p. 35.

Conversty, it seems correct for one who does not think a language has anything <u>essentially</u> to do with verbal behavior to maintain that behavioral criteria are really not crucial to definitions of semantical terms and certainly the behaviorial criteria (if there are any) are not themselves definitions of theoretical linguistic terms.⁹ This is, of course, not to say that behavioral criteria are not crucial in <u>testing</u> the adequacy of definitions. Anybody interested in the analyses of natural languages will certainly want to discover ways of testing whether or not two expressions are synonymous, or whether a certain sentence is analytic. However, what is more to the point (at least in semantics) is the formulation of a definition of analyticity which will give us the means of deciding whether a given statement in a language, regardless of whether that language is a natural one or is constructed artificially, is analytic.

It is indeed one of the major tasks of philosophy to give a clear and complete account of just what precisely a language is, and I have no intention of undertaking such a task here. However, I think we can see that there are certain fairly obvious difficulties in maintaining <u>either</u> that a language is a system of rules formulated in the metalanguage or a system of dispositions to observable behavior.

First of all, let us consider the view that a semantical

⁹I am not necessarily attributing this position to Carnap, for, as has been seen, Carnap's view of natural languages is similar to that of Quine. However, Carnap uses artificial languages, or what he would call "constructed semantical systems" to define the theoretical concept of analyticity and it is precisely this explication which Carnap defends against Quine's attack on analyticity.

system is a system of rules formulated in the metalanguage. Now the rules here being referred to are sentences of the metalanguage which define 'sentence in L', 'designates in L', 'true in L', etc. Now, as Wilson has pointed out, "if 'L' is the (metametalinguistic) name of a system of rules and if 'L' appears in the rules, then we have in effect - of all things! - a confusion between use and mention."¹⁰ For if 'L' is the name of the system of rules, it must be the name of the conjunction of rules or the class of rules. But in either case 'L' should not appear in the rules themselves. It seems more plausible then, to hold rather that the rules define the language rather than constitute it.

To take, however, the view that language is a system of dispositions or habits to behave in certain verbal ways is to take up even more difficulties, although these difficulties may be less readily admitted to. For example, Latin is as much a language today as it was in the time of Cicero, yet we certainly cannot identify the observable linguistic behavior of the few professors who speak Latin today with the behavior of the masses who inhabited Rome in 120 B.C. Moreover, this kind of identification makes a language vary as linguistic behavior varies. But if language varied so frequently, as indeed it would if this identity held, what would be the point in writing grammar books? What is the linguistic relevance of such features of speakers as memory, manners, morals, temperament, health, and so on?¹¹ Yet these features affect the disposition or babits of the

¹⁰Wilson, "The Philosophy of Rudolf Carnap," <u>Dialogue</u> Vol. IV, 1965, No. 1.

¹¹See J. Katz, "Unpalatable Recipes for Buttering Parsnips," Journal of Philosophy, Vol. LXV, No. 2 (1968), p. 45.

linguistic behavior of the speaker.

Behavioristic norms are just not refined enough to adequately characterize a language. "One might say that behavioristic, semiotic sociologism has come to replace mentalistic, epistemological psychologism as the prevalent threat to the drawing of precise distinctions in logic (and language)."¹² And, once again, this is not to detract from the importance of sociology or psychology. It is just that unambiguous characterizations or descriptions of languages are not to be obtained merely by behavioristic norms.¹³ "If we were to use a logic founded on an empirical concept of (let us say) synonymy, as Quine seems sometimes to suggest, we should be as helpless in trying to prove a mathematical theorem as a court of law would be in trying to convict a defendant of transgressing a folkway."¹⁴

There is a view similar to the one which maintains that a language is a system of dispositions which is often taken up by the so-called "ordinary" language philosophers partly because they doubt the philosophical value of formalized languages.¹⁵ The view I refer to is the one which holds (or, at least, insinuates) that a language, e.g., English, is the use of English. This view, which, since Wittgenstein, certainly has its adherents, seems to me to be quite wrong.

12_{Herbert Bohnert,} "Carnap on Definition and Analyticity," in Schilpp's volume on Carnap, p. 419, (italics are mine).

¹³Quine himself, of course, realizes this and this is precisely why he maintains that there is no generic difference between analytic and synthetic statements. Verbal behavior is just too crude to permit of a generic distinction.

14Bohnert, Op. Cit., p. 419.

¹⁵It was, in large part, Wittgenstein's contention that discourse about language involved an attempt to convey something essentially unsayable which prompted Carnap to construct his "artificial"languages which clearly distinguish the object from the metalanguage.

The bankruptcy of this position can be decisively demonstrated by an argument which is a modification of an argument used in an unpublished manuscript by N.L. Wilson.¹⁶ If English is identical with the use of English, then if Jones speaks English, Jones speaks the use of English. But if Jones speaks the use of English, he speaks the use of the use of English ... and so on. And this is what the philosopher must say who prides himself on restricting his attention to the language which the man on the street uses!¹⁷

Actually, we want to distinguish not only languages and their use but also the competence and the performance, i.e., the specific exercises in that competence of a language user or speaker. This is a distinction within use. Neither the competence or the performance of a speaker should, of course, be identified with a language but since this distinction tends to be passed over it is worth emphasizing. To use Wilson's analogy, "we want to distinguish the Each Chaconne, Menuhin's ability to play the Each Chaconne, and a specific performance of the Chaconne by Menuhin."¹⁸ Correspondingly, we want to distinguish any language from both the ability to speak the language and the specif... ic instances of the actual speaking of the language, i.e., the performance. And though we perhaps are still no closer to knowing what a language is, at least we know what it is certainly not.

One might rightly ask at this point just why it is that some

¹⁶"What Precisely is English?" In this paper Wilson directed his argument against the view that English <u>is</u> the ability to speak English.

17To be fair, we must say that there certainly are some ordinary language philosophers of the Oxford variety who trouble themselves to draw a distinction between a language and its use. See, for example, P.F. Strawson's "On Referring".

¹⁸N.L. Wilson, "Linquistical Butter and Philosophical Parsnips," Journal of Philosphy, Vol. LXIV, No. 2, (1967).

philosophers are so prone to identifying a language with its use, or with a system of dispositions, or habits, etc. These philosophers certainly cannot be ignorant of the advances made by Tarski, Carnap and others in the development of the so-called "artificial" languages. Yet these languages have never been identified with their use: most likely because nobody uses them. Carnap himself, as has already been said, seems to make a distinction between natural languages and artificial languages, i.e., constructed formalized languages. Indeed, he refrains from even referring to the latter as languages and instead calls them "semantical systems", Other philosophers take an even dimmer view of the formalization of language such as, c.g., Grover Maxwell, who thinks the attempt to formalize languages is little more than a positivistic pipe dream.¹⁹ Just what is the basis for this apparently widespread view that formalized, "artificial", semantical systems do not quite deserve to be called languages with the full strength of the term?

The objection is raised that whereas with "artificial" languages the semantical rules are laid down first, natural languages are spoken first and then, if a semanticist has the inclination, a natural language may be described or characterized by laying down the semantical rules of that particular natural language. Thus, the so-called "rational reconstruction" of a language such as, e.g., English, is only, indeed, <u>can</u> only be undertaken after the language is already in use.

¹⁹See "The Necessary and the Contingent," <u>Minnesota Studies</u> in the <u>Philosophy of Science</u>, Vol. III.

so, what point is there, after all, in trying to treat natural languages as one would want to treat a simple artificial language? To paraphrase what Wittgenstein once said, we may lay down the rules for English if we want to; for none have so far been drawn. But that never troubled us before when we used English.

This argument reminds me of an argument I once had with someone who maintained that an example of a synthetic <u>a priori</u> sentence is "Team A cannot compete in the second semi-final." This particular person held that this statement is synthetic and gave us <u>a priori</u> knowledge because though it is universally and necessarily true, it is not analytic on account of the fact that the subject term ('team A') cannot be seen to include what is asserted in the predicate term ('cannot compete in the second semi-final'). This seems to me to be a very superficial view of what is actually happening. For if this statement is indeed necessary and universally true, it must be derived from the rules of some kind of team tournament which is set up in the following way:

TEAM PAIRS OF FIRST ROUND IST SEMI-FINAL AORB FINAL CORD (AORB) OR (CGRD) CORD (AORB) OR (CGRD) EVS. CORD (SORH) EVS. EORF (ECRF) OR (GORH) GVG. GORH

Now, once we see the schema which explicitly and unambiguously sets out the rules for us, we can understand how someone can hold that the

statement mentioned above is necessary and universally true. On the other hand, we can also gain no factual information from the statement (other than the fact that a certain tournament proceedure has been accepted) simply because by 'team Λ° we mean that team which competes with 'team B' in a game of which the winner competes in the first and only the first, semi-final.²⁰ But these rules, even though they had been tacitly assumed all the time, were somehow ignored by the person who maintained that the sentence 'Team A cannot compete in the second semi-final' is synthetic.

The similarity I see between the argument I have described above and the objection raised by those who hold that natural languages are used before any rules are drawn is that in both cases the fact that certain rules (rather than others) are tacitly accepted all the time is conveniently ignored.²¹ Of course, early man did not hold a powwow to lay down the rules of the language they were going to use. But this does not mean that there was not some agreement about the rules of their language, rules which are evident to all of us through behavior. It is undeniable that there is a very high degree of regularity in our use of, e.g., English. And this regularity can only be the result of our acceptance that certain expressions are to signify certain (non-verbal) entities and no others.

²⁰Needless to say, the rules which determine which semi-final team A can compete in could be such that/would exclude the 1st semifinal. The reason why this simple fact might be overlooked is that we take for granted the fact that rules are normally contrived for the sake of simplicity and practicality.

²¹Perhaps it is more precise to say that we do not accept rules, but rather adopt a particular language whose rules we conform to.

Of course, with use the words and expressions, or at any rate, <u>some</u> words and expressions take on new meanings or lose their old ones. "We warp usage gradually enough to avoid rupture."²² Because of this fact a second objection might be raised against the view that we should treat formalized semantical systems as languages, i.e., natural languages. It may be argued that a semantical system with formalized semantical rules should be distinguished from a natural language such as, e.g., English, just because of the fact that whereas the semantical rules of a formalized language are static and fixed so that the expressions of such a system can never take on new meanings, a language such as English is continuously in flux. Words take on new secondary and tertiary meanings as the language evolves through usage.

The point that natural languages change with use is a good one and certainly is not to be dismissed. But whether or not acceptance of this point commits us to maintaining an essential difference between artificial and natural languages is doubtful. For if we allow, as I think we must, that all languages (both artificial and natural) have rules (either explicitly lajsd down or tacitly agreed to), then the change or evolution that any natural language undergoes will be manifested, if that language is rationally reconstructed or formalized, within the semantical rules. But if we have, after formalization of, let us say, English, two different sets of semantical rules for time T1 and time T2, then we are justified in maintaining that what we really have are the systems of rules for two different languages, even if we refer to both languages by the word 'English'. For if semantical

system A and semantical system B both contain an expression which is used in system A to signify something different than in system B, then obviously the vocabularies of the two systems are different. But since the identity of a language is constituted by, among other things, its vocabulary, we must consider A and B to be two distinct languages. "To give a certain sign-design a different definition than that already given to it in language L is to move to a new language L'."²²

I would now like to guickly comment on one last objection which has also been raised against the view that there is no difference in principle between artificial and natural languages. It has been objected that artificial languages really cannot be compared with natural languages because they (the artificial languages) are so simple. How can we hope, it is asked, to benefit by comparing a natural language with all its complexities to an artificial language which contains a paltry four or five expressions? In response to this objection I must say that I really do not see why complexity should be considered an essential property of languagehood. Natural languages may be wore complex than some artificial languages but so what? We use languages to refer to or talk about things of the world; the more complex the language we use, the more we are able to talk about. But the fact that a semantical system has limited resources really does not mean that there is any fundamental difference between it and a more complex semantical system. What all languages must have are such things as expressions whose meanings are determined by designation rules and truth conditions, formation and transformation rules. But these have very little to do with questions of simplicity or complexity.

22 Bohnert, Op. Cit., p. 426.

After what has been said above in the last few pages, I think it is indeed probable that "the difference of a formalized language from a natural language lies not in any matter of principle, but in the degree of completeness that has been attained in the laying down of explicit syntactical and semantical rules and the extent to which vaqueness and uncertainties have been removed from them."²³

However, even if it is granted that there is no difference in principle between artificial and natural languages, why should we show so much concern over this point? "All right," one might say, "so perhaps artificial and natural languages are, in principle, alike, If this contention is sound, what philosophical value is there to it?" If in answer to this question we were forced to reluctantly admit that really there is nothing to be gained (philosophically) by showing the similarity of the languages - it's really just a curious fact that we are able to construct semantical systems which are like ordinary languages - we would look a bit silly after exerting ourselves so strenuously to establish the point. Actually though, the situation is quite the opposite. There is an enormous amount of value in constructing formalized languages, especially if they are similar in relevant respects to natural languages. And there are three important points, I think, which demonstrate this worth,

First of all, the point previously raised which was that artificial languages should be contrasted with natural languages because they are simpler can be turned around to show the very value

²³A. Church, "The Need for Abstract Entities in Semantic Analysis," <u>Proceedings of the American Academy of Arts and Sciences</u>, Vol. LXXX, (July, 1951), p. 100-101.

of artificial languages (once it is shown that the simplicity of artificial languages is not a sufficient reason for maintaining that two languages are essentially different). For, because of their simplicity, they are far more manageable than the unwieldy natural languages in attempting to formulate definitions of semantical terms or explicating philosophical notions. Most of the work which is done in semantics requires a language with relatively few expressions and these expressions can be enumerated quite easily within the welldefined limits of an artificial language. On the other hand, it would be practically impossible to enumerate all the unitary expressions of a language such as English.

Secondly, the simplicity of artificial languages allows for the explicit and unambiguous semantical rules to be laid down, which, in turn, allow for the precise explication of theoretical concepts such as that of, e.g., truth or logical truth. Without a division between the object language and metalanguage, a consistent definition of truth is impossible.²⁴ But Tarski was able to overcome this difficulty only by constructing an an artificial language whose metalanguage is clearly distinguishable from its object language. Any attempt to make such a distinction in a language such as English would be very difficult indeed.

The last point I want to make here with respect to artificial

²⁴See Alfred Tarski, "The Semantic Conception of Truth," reprinted in Feigl and Sellars (eds.), <u>Readings in Philosophical</u> Analysis, New York: Appleton-Century-Crofts, Inc. (1949).

languages is one which illustrates, I think, the worth that accrues when artificial languages are considered to be, in principle, no different than natural languages. If we can think of a natural language as being essentially similar to artificial languages (at least in their purely logical and descriptive resources), then we can consider the explications of philosophical concepts, which were formulated only for the artificial languages, to be analogous in the case of natural languages, whose cumbersomeness has not permitted explications of comparable worth. To take an example already alluded to, Tarski's explication of truth, which was possible only within an artificial language, should perhaps be considered to be much like an explication of truth would be for a language such as English, if we were able to arrange a hierarchy of languages for English, i.e., change the "closed" nature of English, which, unfortunately, is very difficult if possible at all. Similarly, perhaps we are justified in considering Carnap's explication of analyticity to be analogous to what an explication of analyticity would be like in a natural language if the rules of that language were explicitly and unambiguously layed down.

In the above pages I have tried to give reasons why we should not be inclined to make a distinction in kind between the artificial and natural languages just because of the fact that, among other considerations, natural languages are not formalized. On the other hand, we cannot just turn our heads and ignore the obvious difficulty of formulating the rules, or rather, formulating a description of a natural language. Even if there are no essential differences between

formalized artificial languages and natural languages, there are, nevertheless, some differences which deserve attention. I suppose the most obvious difference stems from the fact that whereas we are free to stipulate the rules as we please when constructing artificial languages, we are not so free in formalizing the rules of a language already in use such as, e.g., English, if we want adequately to reflect in our formalization the English language which is familiar to all of us fluent in that tongue. The reason for this, of course, is that any rational construction of English must be based on linguistic behavior, for linguistic behavior is all we initially have when dealing with natural languages. And this fact leads Quine, for example, to believe that the notions of synonymy and analyticity are not at all clear with respect to natural languages. For, if we will remember, according to Quine, there are great difficulties which stand in the way of any explication of these notions which is not circular, i.e., which does not rest on other notions equally as unclear. Let us now examine these difficulties.

CHAPTER III

SYNONYMY and ANALYTICITY

However useful Kant's definition of analyticity may have been, it is, as we have seen, certainly plagued with some serious shortcomings. Kant's use of the word 'contained' is, at best, on a metaphorical level. Also, his definition is applicable only to statements of the subject-predicate form. A more modern and rigorous approach to define analyticity is given by Quine¹ who suggests that statements which are analytic fall into two classes.

First, there are those statements in which only the logical terms occur essentially. That is to say, the statement remains true under any and all interpretations of the descriptive terms, e.g., "No unmarried man is married". Given the logical constants, which in this case are 'no', 'un', and 'is, the statement is true under all interpretations of the descriptive terms 'married' and 'man'. We have seen that even this formulation is not free of difficulty, for statements of the form ' $(\exists x)(\exists y)(x \neq y)$ ' are left unadequately accounted for. Here, only the logical terms occur essentially, yet it is a contingent matter of fact, apparently, that there are at least two individuals. This has prompted at least one philosopher to say that a statement is analytic if and only if both the logical terms only occur essentially and

¹Quine, "Two Dogmas of Empiricism," p. 22-23.

it is necessary. However, as I think we shall see, the notion of necessity, if appealed to, as in the above definition, creates more problems than it attempts to solve.²

A second class of analytic statements which I will refer to, following N.L. Wilson, as <u>descriptively analytic</u>, are typified by the following statement,

'No bachelor is married'.

We see here at once that the logical terms only do not occur essentially. In other words, we can interpret the descriptive terms in such a way that the statement will be false. Yet, it is evident also that it is not just a matter of fact that no bachelor is married, for the statement is known to be true at once, without empirical evidence, if only the words 'bachelor' and 'married' are understood.

However, the descriptively analytic statement 'No bachelor is married' can be transformed into a logical truth by putting synonyms for synonyms; that is, by replacing the word 'bachelor' with the expression 'unmarried man'. This may seem, then, to be a happy resolu-

³Mill described these statements as being true by essential predication.

 $^{^{2}}$ N.L. Wilson, in Chapter VI of his book The Concept of Language, has offered a fairly satisfactory explanation of how theorems can be both logically true and involve existential assumptions. It turns out that such a theorem is only contingently significant, but if significant, then it is necessary. Wilson argues that if such a theorem was not true it would not be false but insignificant, since a condition of language is a world of individuals. It must be granted that if Wilson's account is correct, theorems such as '($\exists x$)($\exists y$)($x \neq y$)' are curiosities rather than serious problems.

tion but Quine argues that our notion of synonymy is at least as unclear as our notion of analyticity. if we are to explain analyticity by an appeal to a pre-analytic notion of synonymy, then what is required is a clarification of synonymy.

If we are to rely upon our notion of synonymy to clarify analyticity, we are not so concerned with logically true statements as we are with descriptively analytic statements or those statements which can be transformed into logical truths by putting synonyms. To those who find comfort in asserting that synonymous expressions are so by definition, Quine presents some formidable arguments.

First of all, how is it that we find 'bachelor' to be defined as 'unmarried man'? If we appeal to the dictionary, we put, according to Quine, the cart before the horse. For the lexicographer's report in the dictionary is the report of an empirical scientist who records in the dictionary those expressions which he believes are <u>already</u> used synonymously. How then, can the dictionary be taken as the ground for synonymy?

There is another problem which should perhaps be considered as being related to Quine's objection regarding the use of the dictionary as the ground for synonymy, although Quine himself does not explicitly bring the problem up in "Two Dogmas". The problem I am referring to stems from the fact that not all words or expressions have synonyms, even though they are "defined" in the dictionary. For example, the word 'gold' does not have a synonym, yet it is, at least, entered in the dictionary. The definition of gold in part, is that it is the
metal with the atomic weight of 197.2. Does this mean then, that the statement, 'Gold has the atomic weight of 197.2' is analytic? It seems not, for it is a contingent matter of fact that gold has that particular atomic weight. Moreover, it is absurd to suggest that one must know the atomic weight of gold in order to know what the word 'gold' means. At any rate, the statement 'Gold has the atomic weight of 197.2' is neither logically true nor can it, seemingly, be trans-formed into a logical truth by putting synonyms for synonyms.⁴

To get back to the notion that definition is the ground for synonymy, Quine argues that even the explicating of a word, although an activity not limiting itself to the reporting of pre-existing synonymies, nevertheless, rests on other pre-existing synonymies. For any explication must be synonymous with the favored contexts of the definiendum, taken as a whole in its antecedent usage. Perhaps a good example of the kind of explication Quine is talking about here is Tarski's explication of truth. Tarski, in his explication, was trying to account for an important way in which people always have used the word 'true'.

According to Quine, the only kind of definition which does not rest on prior relations of synonymy are those definitions which are created for the sake of abbreviation. Unfortunately, however, Quine does not think that an understanding of abbreviation helps in clarifying our notion of pre-existing synonymy.

⁴See Kant, <u>Op. Cit.</u>, p. 586, for a good discussion of this problem.

In response to these objections raised by Quine, I think we should first make a distinction between definitions and sentences <u>about</u> definitions. Quine presents us with three different kinds of definition and attributes to each a different status with respect to the analytic-synthetic distinction. He speaks of lexicographical (or reportive), explicative, and abbreviational definitions. Quine concedes that abbreviational definitions, since they do not rest on antecedent usage, but rather are stipulations for the sake of convenience, are analytic.⁵ The analyticity of the other two kinds of definitions is problematic, according to Quine, because they rest on ante-cedent usage, i.e., the allegedly synonymous expressions which occur in them are known only through the observation of linguistic behavior.

However, because certain synonymous expressions cannot be known without the observation of linguistic behavior, it does not follow that the definitions formulated with such synonymous expressions, viz., lexicographical and explicative definitions, are of a different logical nature than the abbreviational kind. It seems rather that the different names of these definitions point to the fact that definitions can be used in different ways. "A single definition, e.g., of 'aspirin' as acetyl salicylic acid, may be reportive for the lexicographer, and abbreviational for the chemist; it may constitute an explanative definition for the student and may not 'serve as a definition' at all for a child."⁶ Hence, the disagreement over the analyticity of these dif-

> ⁵Quine, "Two Dogmas of Empiricism," p. 26. ⁶Bohnert, <u>Op. Cit.</u>, p. 424.

ferent "kinds" of definition is perhaps really about the uses we may put these definitions to. So, it is not a <u>reportive definition</u> which is synthetic but rather a report <u>about</u> a definition, e.g., that a certain community accepts it - a sentence of very different form than the definition itself; and it is not an explicative definition whose analyticity we question, but rather the contention that a given definition constitutes an explication.⁷

All definitions are not, and should not be, sought purely for purposes of abbreviating longer locutions already in use; but all definitions, if they are indeed to be considered definitions, must have the logical features we are willing to attribute to abbreviational definitions. These logical features include complete determination of the meaning of the defined term by the definiens (without regard to psychological associations carried over from previous use), establishment of synonymy "by fiat", and eliminability in favor of defining terms with no loss (or gain) of expressive power. In the sciences there seem to be some definitions for abbreviational purposes such as, e.g., 'conductance' defined as the reciprocal of resistance. There seems to be little or no disagreement over the analyticity of such definitions and these definitions, which are admittedly analytic, can be taken as the logical core or, at least, as part of the logical core of science. The analytic status of other, more disputed, definitions would be determined as the sciences are more explicitly formalized.

7 Ibid.

Quine sanctions synonymy when "the definiendum becomes synonymous with the definiens simply because it has been created expressly for the purpose of being synonymous with the definiens."⁸ And this sort of synonymy by fiat seems to act more easily on words previously unfamiliar. Apparently then, Quine does not object to definitions used for purposes of sheer abbreviation because of the lack of prior psychological associations for the word being defined. But meanings are not psychological as is evidenced by the fact that words are intersubjective. Rather, words are meaningful in virtue of the semantical rules governing their use. And these rules are objective.

Not content with definition as the key to synonymy and analyticity, Quine attempts a second time to determine the nature of synonymy by examining the suggestion that synonymy, or rather, the synonymy of two linguistic forms, consists simply in their interchangeability in all contexts <u>salva veritate</u>. The question which we must now ask ourselves is whether or not interchangeability <u>salva veritate</u> is a strong enough demand to make of synonymy.

Of course, we can easily construct contexts in which we cannot preserve the truth of a statement by interchanging what appear to be synonyms. For example, the statement ""Bachelor' has fewer than ten letters," when we substitute ''Unmarried adult male'' for ''bachelor'' becomes "'Unmarried adult male' has fewer than ten letters," which is clearly not true, even though we started with a true state-

⁸Quine, "Two Dogmas," p. 26.

ment. However, I think we will all grant that 'bachelor' is a different word than "'bachelor''. And even though 'bachelor' may be synonymous with 'unmarried adult male', we are by no means justified in asserting that ''bachelor'' is synonymous with ''unmarried adult male''. At any rate, Quine, if only for the sake of argument, does not consider this to be a telling argument, though he does count on our prior conception of wordhood to present difficulties of its own, Indeed, there are other problems beside those having to do with the names of synonymous expressions. For example, is the word 'brothers' in the title of the novel Brothers Karamazov synonymous with the expression 'male sibling'? If so, then because the sentence "Male Siblings . Karamazov is the title of a Russian novel" is false, we are faced with another context in which truth is not preserved after substituting synonyms for synonyms. But leaving aside these problems, it is still doubtful, according to Quine, whether interchangeability salva veritate is an adequate characterization of synonymy.

Quine tells us that interchangeability <u>salva veritate</u> is meaningless until relativized to a language whose extent is specified in relevant respects. If we consider a language which contains oneplace and many-place predicates with variables which allow us to construct atomic sentences, **a**nd further, that the rest of the language is logical, complex sentences being built out of atomic ones through the use of truth functions, we find ourselves with an extensional language. This means that any two predicates which have the same extension, i.e., range over the same individuals, are interchangeable

salva veritate.

But in an extensional language, Quine says, interchangeability <u>salva veritate</u> does not necessarily mean that we are dealing with synonymous expressions. As Quine says, "That 'bachelor' and 'unmarried man' are interchangeable <u>salva veritate</u> in an extensional language assures of no more than that 'all and only bachelors are unmarried men' is true."⁹ In an extensional language, the words 'bachelor' and 'unmarried man' need be no more synonymous than the expressions 'creature with a heart' and 'creature with a kidney'.

It appears then, that interchangeability <u>salva veritate</u> is not a strong enough demand to make of synonymy. We do not want merely that the statement, 'All and only bachelors are unmarried men' to be true, but to be analytic as well.

Now it might be important to note that we can explicate the notion of synonymy quite easily if we assume analyticity. For then, to say that 'bachelor' and 'unmarried man' are synonymous is only to say that the statement 'All and only bachelors are unmarried men' is analytic. Quine's contention, of course, is that we need an account of synonymy which does not presuppose analyticity.

Similarly, interchangeability <u>salva veritate</u> would be an adequate explication of synonymy if we could use the modal operator 'necessarily'. Then the statement 'All and only bachelors are unmarried men' would be <u>necessarily</u> true and could thus be distinguished from statements which are only factually true.

⁹Quine, "Two Dogmas," p. 31.

However, this is, once again, only to chase our tails, for to use the adverb 'necessarily' is to presuppose the notion of analyticity and that is precisely what we are trying to explicate.

It seems then, that any attempt to explicate synonymy by appealing to the notion of interchangeability <u>salva veritate</u> is going to be frustrated simply because we cannot draw a line fine enough to seperate or distinguish difference of extension from difference of intension or meaning. The problem, in other words, is that even though any two terms which have the same meaning will have the same extension, two terms may have the same extension and yet not have the same meaning. So, it seems either we must continue our search for those mysterious entities called "meanings" in that realm which lies between words and their extensions, or we must look for some other way to explicate synonymy. Let us then, examine an attempt by Nelson Goodman, ¹⁰ in which the explication of synonymy rests on extensions rather than on intensions or meanings. He argues in the following way:

Since there are no unicorns or centaurs, all unicorns are centaurs and all centaurs are unicorns. For two classes are identical if and only if they have the same members. And unicorns and centaurs belong to the same class, namely, the null class. Moreover, since we have a theorem in logic which says that if all A's are B's, then all the things that bear the relation R to an A are things that bear the relation R to a B, it appears that 'All uncles of centaurs are uncless' of unicorns' and 'All feet of centaurs are feet of unicorns' are true

¹⁰Nelson Goodman, "On Likeness of Meaning, "reprinted in Linsky's-Semantics and the Philosophy of Language, The University of Illinois Press at Urbana, (1952), p. 67-74.

statements.

However, we cannot say that all pictures of unicorns are pictures of centaurs. Goodman argues that this is not to say that we have violated the above cited theorem of logic, but rather goes to show that 'picture of' is not always a relation term like 'foot of' or 'uncle of'. If x is a foot of a centaur, then x bears the relation 'foot of' to some y that is a centaur. Hence, if there is anything which is a foot of a centaur, then there is a centaur. But "if there is something which is a picture of a centaur - as indeed there is we cannot infer that there is some centaur - as there certainly is not."¹¹

Now the expression 'centaur-picture' differs from 'unicornpicture' for we apply them to different objects just as we apply to different object the terms 'chair' and 'desk'. So then, the terms 'unicorn' and 'centaur' are applied to no objects and hence have the same extension; Goodman calls this the <u>primary</u> extension. But the terms 'unicorn-picture' and 'centaur-picture' have different extensions; this is the secondary extension.

Goodman's claim is that any two expressions which differ in meaning will either have different primary extensions or different secondary extensions or both. In other words, if two terms have the same extension and if we compound these terms with identical predicates which make up expressions which also have the same extension, then the two simple terms have the same meaning.

11Goodman, Op. Cit., p. 70.

What is unusual, and perhaps unsatisfactory, is that Goodman claims to have shown that no two different words have the same meaning. For, given any two predicates, we can always show that their secondary extensions differ. To use one of Goodman's examples, the secondary extension of the words 'acrid' and 'pungent', even though their primary extensions are the same, will differ. For, to the inscription of 'a pungent odor that is not an acrid odor', the predicate 'pungent odor description' applies, while the predicate 'acrid odor description' does not apply. Also, since Goodman claims to have shown that no two predicates are interchangeable in all non-intensional contexts <u>salva veritate</u>, ¹² it may be that not only is interchangeability <u>salva veritate</u> too weak a condition, as Quine has shown with respect to extensional languages, but is also too strong a condition.

The startling conclusions drawn by Goodman, viz., that no two terms are synonymous and no two distinct terms ever have the same extension, are, however, mitigated by the following considerations. First, Goodman has succeeded in demonstrating these revolutionary propositions only by changing the meaning of "extensional interchangeability" in such a way that the term ranges over the secondary extension of the predicate term as well as (what Goodman refers to as) the primary extension. Hence, if we mean by "extension" the sum of the primary and secondary extensions, then perhaps distinct terms cannot have the same extension. And it is only against such a background that Goodman can argue that, e.g., the terms 'triangle' and 'trilateral'

12Goodman, Op. Cit., p. 73-74.

have different extensions and hence are not synonymous. For, since the class of triangle descriptions includes the description 'triangle that is not trilateral' which is not included in the class of trilateral descriptions, 'triangle' and 'trilateral' differ in secondary extensions.

Actually though, Goodman has proved nothing except that two distinct terms are, after all, distinct (and even on Goodman's hypothesis, that's a tautology). For to assert that 'triangle that is not trilateral' is a triangle description and not a trilateral description is to beg the question whether or not 'triangle' is synonymous with 'trilateral'. For if these terms are indeed synonymous, then the above inscription is both a triangle and a trilateral description.

Goodman, on the other hand, might intend the terms 'triangle description' and 'trilateral description' to be regarded as syntactic sign-designs. This approach might explain why Goodman seems to beg the question of whether the terms 'triangle' and 'trilateral' are synonymous. But if this is Goodman's intention, then, since the terms are syntactic, they have no extension at all and Goodman is only talking about the actual configurations of ink marks, i.e., he is making the very trivial claim that 'triangle' is distinct from 'trilateral'. Goodman's attempt, then, to show that no two different expressions are ever completely synonymous, is not at all successful.¹³

And to regress a bit, we may even question Quine's conclusion that interchangeability salva veritate is not a strong enough a demand

¹³These points against Goodman were made by Pap in <u>Semantics</u> and <u>Necessary Truth</u>, p. 295-301.

to make of synonymous expressions. For, as Benson Mates argues, "In general, it seems natural to regard two expressions as synonymous in a language if there is no way in the language of distinguishing between their meanings. Thus it is natural to regard synonymity of terms as relative to the language in which the terms occur."¹⁴ Probably, the reason we find it so odd that expressions such as, e.g., 'creature with a heart' and 'creature with a kidney' should be considered synonymous in an extensional language, is that we are accustomed to using such expressions within a language that is rich enough to provide modal or intentional contexts. And, of course, in such contexts, or rather, in such a language, these expressions would not be synonymous, for we could find contexts in which we could not preserve truth after interchanging one expression with the other.

Judging from what has been said so far in this chapter, it seems as if Quine has failed to show the inadequacy of either definition or interchangeability <u>salva veritate</u> in explicating synonymy. For, as we have seen, all definitions must share the logical properties of abbreviatory definitions and since Quine admits that abbreviatory definitions create synonymy by fiat, it seems that all definitions might be considered to be analytic. Also, it is not clear that expressions which are co-extensive in an extensional language should not all be regarded as synonymous. Perhaps this does not mean that we have found an adequate characterization of synonymy, but, at the very least,

¹⁴Benson Mates, "Synonymity," reprinted from University of California Publications in Philosophy, Vol. 25 (1950), in Linsky's Semantics and the Philosophy of Language, p. 123.

we can say that it is an entirely open question as to whether or not analyticity can be explicated by an appeal to synonymy. So, Quine notwithstanding, it does not seem implausible both that interchangeability <u>salva vertate</u> is a satisfactory criterion of synonymy and that descriptively analytic statements can be reduced to logical truths by definition.

Nevertheless, Quine does seem to have a point in demanding that empirical, behavioral criteria be given for the testing of such concepts as synonymy (and analyticity). For even supposing, as we have argued, that there is only one kind of definition, though definitions may be used in different ways, how are we to determine for a language such as, e.g., English, that 'All and only bachelors are unmarried men' is true by definition on the basis of linguistic behavior? And let us not forget that linguistic behavior is all that is given to us in our initial investigation. Also, though it might be the case that the expressions 'creature with a heart' and 'creature with a kidney' are synonymous in an extensional language, surely we do not regard these expressions as synonymous in English. But how is a lexicographer, who is investigating a community of people who speak English, to determine on the basis of linguistic behavior that 'creature with a heart' and 'creature with a kidney', though ranging over precisely the same individuals, are not alike in meaning? If there is indeed a difference in meaning between these two expressions, how does this difference reflect itself in linguistic behavior? Quine maintains that to assign an intension or meaning to an expression on the basis of a previously determined extension can only be to choose any one of many

possible properties which coincide with the extension of the term. Because there may be several alternative intensions which can be assigned to the term, all of which fit the extension, we may be guided in making our decision by a consideration of simplicity. But our assignment of intensions can never be at odds with the facts, according to Quine, for there are no facts to be at odds with. "The finished lexicon is a case evidently of <u>ex pede Herculum</u>. But there is a difference. In projecting Hercules from the foot we risk error but we may derive comfort from the fact that there is something to be wrong about. In the case of the lexicon, pending some definition of synonymy,¹⁵ we have no statement of the problem; we have nothing for the lexicographer to be right or wrong about,"¹⁶

Carnap, however, believes that it is possible to provide a behavioristic, operational proceedure for the testing of synonymy and analyticity, although he does not hold that a semantical concept (such as, e.g., synonymy) must necessarily possess a prior pragmatical counterpart in order to be fruitful¹⁷ Semantical concepts may produce results solely through its application in the development of language systems. However, since Carnap believes that there is a prior pragmatical concept corresponding to the semantical concept of synonymy (even if it is a somewhat vague one), he thinks it worthwhile to show a way to em-

¹⁵I think Quine here means by 'definition' behavioral criterion or, perhaps, pragmatic definition (as opposed to semantic definition).
 ¹⁶Quine, "The Problem of Meaning in Enguistics," reprinted in From a Logical Point of View, p. 63.
 ¹⁷R. Carnap, "Meaning and Synonymy in Natural Languages,"
 Philosophical Studies, Vol. VI, 1955, p. 33-47.

pirically test such concepts. And this might be done in the following way.

Suppose that two linguists, after studying the speech behavior of a person, who for convenience sake, we shall call 'Karl', disagree over a translation. One of them writes in his dictionary:

(1) Pferd, horse

while the other linguist writes:

(2) Pferd, horse or unicorn.

Now, since there are no unicorns, even though the two intensions which have been assigned to the words 'Pferd' are different, the extensions are identical. Apparently then, Quine maintains that since the extension is the same for both (1) and (2), Karl could make no response which would confirm one hypothesis while infirming the other. And hence, there would be no empirical way to test for the correctness of either (1) or (2). Carnap maintains, however, that we are not limited to actual cases in our investigation of Karl's linguistic behavior. We must find, according to Carnap, the responses Karl would make to possible cases as well; we must determine some of the possible cases in which Karl is willing to apply the predicate 'Pferd'. This can be accomplished by putting questions to Karl using modal expressions corresponding to the English expression 'possible case'.

Now as we have mentioned, Quine will not be satisfied with any explication of synonymy or analyticity which must rely on terms such as 'necessary' and its modal counterparts, 'possible' and 'impossible'. The reason for Quine's dissatisfaction with such terms is that their very use presupposes the distinction in question, viz.,

the analytic-synthetic distinction. So perhaps we may immediately feel inclined to wonder just how adequate Carnap's proceedure will be. On the other hand, it may be that Quine's conditions of adequacy are so strong that no adequate definition is possible. He will not allow any definiens of 'analytic' to contain any term belonging to a family circle of terms such as, e.g., 'necessary' (and its modal counter-parts), 'meaning', 'understanding', 'conceivable', <u>etc.</u>, unless that term is itself defined independently of terms within the circle. But is this not like trying to define the word 'ought' in ethics without using the terms 'good', 'bad', 'right', 'wrong', 'duty', 'moral approval', <u>etc?</u> ¹⁸

Actually though, Carnap does not think we need to get embroiled over the use of modal expressions. For though he finds no objection of principle against their use, he maintains that they are not necessary. All that is necessary is that the linguist merely describe a unicorn (in German) as a thing similar to a horse but having only one horn in the middle of the forhead; or the linguist might just point to a horse and describe the modification. Alternatively, the linguist may just point to a picture of a unicorn. After any of these three approaches, the linguist could test the empirical hypothesis of (1) and (2) by asking Karl (in German) if he would be willing to apply the term 'Pferd' to such cases. An affirmative or negative answer will constitute a

¹⁸See Benson Mates, "Analytic Sentences," <u>Philosophical Re-</u> view, Vol. 60 (1951), p. 525-534.

confirming instance for (2) or (1) respectively. And this illustrates the empirical nature of such hypothesis.

Once we have a proceedure for the testing of a given hypothesis concerning the intension of a predicate for a speaker, we may give a concise characterization of the concept of intension. Carnap characterizes this concept roughly as follows:¹⁹ the intension of a predicate Q for a speaker X is the general condition which an object Y must fulfill in order for X to be willing to ascribe the predicate Q to Y. Also, since this testing proceedure for intensions is not only applicable to predicates, but to expressions of other types, including whole sentences, Carnap is able to formulate pragmatic definitions for 'analytic' and 'synonymy' based on his characterization of the concept of intension.

Two expressions are synonymous in the language L for X at time T if they have the same intension in L for X at T.

A sentence is <u>analytic</u> in L for X at T if its intension (or range or truth-condition) in L for X at T comprehends all possible cases.

The objection might be raised here that, with respect to empirically testing a speaker X to determine the intension of a particular predicate, it is not clear that X's answers to the questions put to him by the linguist will not be based on his knowledge of the facts. For example, suppose a linguist wants to determine the intension for the predicate 'gold' for X. Let us suppose that the linguist

¹⁹Carnap, Op. Cit., p. 42

has already determined the extension of 'gold'; he has found that X applies this word to any object which has the atomic weight of 197.2. But, as we have seen, to determine the extension of a word is not to determine the intension. And so, if the linguist wants to find the intension of 'gold', he must further question X by asking him of possible cases. The linguist might ask X, "Suppose I have a metal which is like gold in all respects except it does not have the atomic weight of 197.2. Do you apply the word 'gold' to this metal?" Now X might answer in two ways. If he is a physicist or a chemist, he might answer negatively. On the other hand, if X does not know much of the relationship between metals and their atomic weight, X might answer in the affirmative, thinking that other properties are more essential.

Now if the answer to the linguist's question is negative, the linguist can conclude (tentatively, at least) that the intension of 'gold' is, in part, 'atomic weight 197.2'. And it does not matter that the negative response given by X was formed on the basis of past experience as a physicist or chemist. What does matter, though, is that he would apply the word 'gold' to no object which does not have that atomic weight, i.e., he will not allow of any case in the past, present, or future in which he would not apply that predicate only to metals with that atomic weight. On the other hand, if the answer to the linguist's question is in the affirmative, the intension of 'gold' is not 'atomic weight 197.2'.

Now it is not clear whether or not Quine accepts Carnap's solution to the problem of testing empirically for intensions (or

meanings). What is apparent though, is that Quine's objections to synonymy have, to at least some extent, been mitigated and the analytic-synthetic distinction still seems to be a plausible one. Also, the notion of synonymy seems to be, contrary to what Quine says, an understandable one, even on the pragmatic level. Putting aside synonymy then, let us examine the attempt to define 'analytic' on the basis of semantical rules.

CHAPTER IV

SEMANTICAL RULES

During the thirties and forties, Carnap devoted much of his time to the working out of an adequate explication of analyticity via L-truth (or logical truth). His explications were generally formulated for a constructed semantical system defined by semantical rules which are themselves formulated in the metalanguage. The metalanguage itself is a suitable part of the English language containing translations of the sentences and other expressions of the object language. Now semantical concepts are defined in the metalanguage, and so, it is to the metalanguage we must go for Carnap's explication of analyticity or L-truth.¹

Carnap's most recent definition of L-truth² is formulated with the help of what are called <u>state-descriptions</u>. As we remarked in the Introduction, the notion of state-description was anticipated by Leibniz' concept of possible worlds. A state-description is defined by Carnap as a class of sentences in a language L which contains for every atomic sentence either it or its negation, but not both. Thus, a stateI.

¹As Quine points out (in "Two Dogmas"), there is a difference between analyticity and L-truth which raises special difficulties. Carnap attempts to meet these difficulties by introducing what he calls "meaning postulates". However, for the moment, let us ignore both the difference and the difficulties which it gives rise to.

²See Carnap, <u>Meaning and Necessity</u>, (Chicago: University of Chicago Press, reprinted in 1960), Section 2.

description in L is a complete description of a possible world with respect to all the properties and relations expressed by the predicates of L.

Now semantical rules are needed to determine whether or not a given sentence <u>holds</u> in a given state-description. To say that a sentence S holds in a given state-description is to say that S would be true if that state-description (that is, the class of all sentences belonging to it) were true. Carnap gives, as examples which show the nature of these rules, the following:

> (1) An atomic sentence holds in a given state-description if and only if it belongs to it.

(2) Not-S holds in a given state-description if and only ifS does not hold in it.

(3) SiVSj holds in a state-description if and only if eitherSi holds in it or Sj or both.

(4) $Si \equiv Sj$ holds in a state-description if and only if either both Si and Sj or neither of them hold in it.

(5) A universal sentence (e.g., '(x)(Px)') holds if and only if all substitution instances of its scope ('Pa', 'Pb', 'Pc', etc.) hold in it.³

All of the semantical rules together (of which we have just listed five) determine, what is called by Carnap, the <u>range</u> of any sentence of L. The range of a sentence S is defined as the class of all those state-descriptions in which S holds. To distinguish the

3Ibid.

rules which determine ranges from other semantical rules such as, e.g., designation rules, the former are called rules of ranges. Together with the rules of ranges, the rules of designation for the predicates and the individual constants provide an interpretation for all sentences in L, "since to know the meaning of a sentence is to know in which of the possible cases it would be true and in which not."

Now though Carnap is providing a formal explication far more precise than traditional ones, his explication turns out to be in accord with what has often been given as an informal characterization of analyticity, viz., truth based on meaning alone. For the meaning of a sentence, i.e., its interpretation, is determined by the semantical rules alone. And it is for this reason that Carnap requires of any explicatum of analyticity that it fulfill the following condition: <u>Convention</u>: A sentence S is L-true in a semantical system L if and only if S is true in L in such a way that its truth can be established on the basis of the semantical rules of the system L alone, without any reference to (extra-linguistic) facts.

Let us keep in mind that the above convention is not a definition but rather an adequacy condition. The actual definition is inspired by Leibniz' conception that a necessary truth must hold in all possible worlds. Since state-descriptions represent all the possible worlds (with respect to all properties and relations expressed by predicates of the system L), the following definition suggests itself: <u>Definition</u>: A sentence S is L-true (in L) =df S holds in every statedescription (in L).

To illustrate that this definition fulfills the condition required by the given convention, let us consider this example. Suppose we are given the sentence 'PaV-Pa'. Now we know from the rules of range listed above that since 'Pa' holds in certain statedescriptions and '-Pa' in all others, the disjunct 'PaV-Pa' holds in every state-description. And if the sentence 'PaV-Pa' holds in every state-description, then it holds in the true one, i.e., that statedescription in which all sentences are true. Hence, the truth of the sentence 'PaV-Pa' is established on the basis of the semantical rules of L alone.

If S is a sentence which does not hold in every state-description, then we know that there is at least one state-description in which it does not hold, i.e., in which S would be false if it were the true one. Now since we cannot tell which state-description is the true one without knowledge of the facts, we cannot determine whether S is true solely on the basis of the semantical rules of L. Hence, S is not L-true but rather, it is synthetic.

Now Quine raises several objections against semantical rules and does not restrict his attack to the kind which are given in <u>Meaning</u> <u>and Necessity</u>. The first of the various forms of semantical rules which Quine examines are those, which in a language L, specify (by recursion or otherwise) all the analytic statements of L. Quine maintains that the trouble with this kind of semantical rule is that it contains the word 'analytic', which is what we are trying to understand. Quine's point is that before we can understand a rule, (in this case,

'A statement S is analytic for language L if and only if ...'), we must understand the words or expressions which constitute the rule (and in this case, we do not understand the expression 'analytic' or 'analytic for' except, perhaps, in a pre-analytic way, i.e., intuitively).

Against this objection, I think two things may be said. First of all, a semantical rule of the form 'S is analytic for L if and only if' is essentially different from rules which give an explicit enumeration of the sentences of L which are to be called 'analytic'. If the rule was of a kind which simply enumerated the analytic sentences, Quine would be right in maintaining that we have no understanding of the term 'analytic'. For then, the list of sentences which are to be called 'analytic' would seem to be just an arbitrary selection and we would be in the dark as to why these sentences, rather than some others, have been so specified. Carnap, however, does not simply give an enumeration, but provides a criterion for the application of the term 'analytic'. And since this kind of semantical rule is not enumerative, the class of analytic sentences for L will be much less determinate. That is, the rules will not give us with a nice, clear, closed list of the analytic sentences for L. Quine seems to lose sight of this point when, for example, he says that he does not know whether the sentence 'Everything green is extended' is analytic or not on account of the fact that he does not understand 'analytic'. Quine is not forced to make such a claim, for "one may understand a predicate without being able to decide whether given cases fall under it."4 For example,

⁴Benson Mates, "Analytic Sentences," p. 528-529.

consider the term 'theorem' which is defined as 'the last line of a proof'. Given this definition, we would be able, given a complete set of proofs in a system, to select all the theorems of the system as well. Hence, the criterion for applying the term 'theorem' is that there be a proof of the statement in question. But although the proof proceedure for general quantification theory is complete,⁵ we do not have an effective decision proceedure for that theory.⁶ And so. we may never find out whether some sentences are theorems or not. For if a sentence is a theorem, we shall eventually find a proof of it, but if it is not a theorem, we have no way of knowing that it is not just our lack of ingenuity which keeps us from discovering the appropriate proof. Yet, this in no way means that we do not understand the term 'theorem'. Similarly, with the term 'analytic', although the criterion for its application does not enable us to decide in all cases whether or not a given statement is analytic, it does tell us what would constitute an effective test for analyticity.

Secondly, Quine's charge that appeal to semantical rules is useless in gaining an understanding of 'analytic' because it presupposes such an understanding seems to be a bit unreasonable. For we are not trying to obtain an understanding of analyticity from the semantical rules. What is sought is a clarification of the term 'analytic', a term <u>already</u> in use. Hence, an appeal to prior understanding seems relevant, if not necessary, if we are to determine the adequacy of an

⁵See Leon Henkin, "The Completeness of the First-Order Functional Calculus," Journal of Symbolic Logic, Vol. 14 (1949), p. 159-166. ⁶See A. Church, "A Note on the Entschiedungsproblem," Journal of Symbolic Logic, Vol. 1 (1936), p. 40-41.

explication or definition.

The second major objection (and perhaps a more telling one) made by Quine, and applied in a more general way be N.L. Wilson,⁷ is that the expression 'analytic-for-L' is a unitary phrase which should be taken as a single word which is totally distinct from the expression 'analytic' or 'analytic for', Wilson points out that we can no more replace 'L' in the expression 'analytic-for-L' with the variable 'L₁' than we can replace the '6' in '64' with a numerical variable.⁸ If this is the case, and I think it is, then we might as Quine points out, simply use the letter 'K' (untendentiously) instead of the expression 'analytic-for-L' so as not to seem as if we are really explicating the interesting word 'analytic'.⁹ To quote Quine, "By saying what statements are analytic for L we explain 'analytic for L' but not 'analytic', not 'analytic for'. We do not begin to explain the idiom 'S is analytic for L' with variable 'S' and 'L', even if we limit the range of 'L' to the realm of artificial languages."¹⁰

What we are looking for then, is a definition of 'S is analytic for L' with variables 'S' and 'L', 'L' ranging over, at least, all artificial languages. That is to say, we want a definition that does not eliminate 'analytic-for' and 'L' at the same time, as is the case when we define 'analytic-for-L' as a unitary expression. Wilson calls

⁷ See <u>The Concept of Language</u>, Chapter I.
⁸ <u>Ibid</u>.
⁹ Quine, "Two Dogmas," p. 33.
¹⁰ <u>Ibid</u>.

this the double-climination difficulty and points out that we not only have this problem with 'analytic' but with other important semantical terms as well such as, e.g., 'designates', 'true', 'sentence', <u>etc</u>.¹¹ More will be said about the double-climination difficulty later but for the moment I will make only one comment which might mitigate againet the objection that we should replace the unitary expression 'analyticfor-L' with the untendentious expression 'K' in order not to mislead people into thinking we are shedding light on the old philosophical notion of analyticity.

Perhaps it should be conceded that because our explication of 'analytic', in so far as it must be relativized to a particular language, lacks the generality of a completely satisfactory explication or definition. But this, it seems to me, is certainly not to say that we have gained nothing from our explication of 'analytic-for-L'. Cannot we say that by explicating even the unitary expression 'analytic-for-L' we have bettered our understanding of analyticity in general? Carnap, in his explication of 'analytic-for-L', has drawn an analogy which carries over to languages in general. To be an analytic sentence in some language is to bear a relation to the rules of that language similar to the way certain sentences in L bear a relation (analytic-for-L) to the rules of L.¹² Of course, Carnap could, if he wanted, replace the expression 'analytic-for-L' by 'K' (or some such other expression), but Carnap wants to indicate by his particular choice of expression that

11Wilson, Op. Cit.

12 See Benson Mates, "Analytic Sentences," p. 533 and R.M. Martin, "On 'Analytic'," Philosophical Studies, Vol. 3 (1952), p. 42-47.

he is explicating the familiar philosophical notion of 'analytic'. "Similar remarks apply equally well to the semantical truth concept. We could call the precise new term 'frue' as Tarski suggests somewhere, rather than 'true', so as not to seem to throw light on the age-old, hoary concept of truth. But this would be to miss the point of the explicative power of the new definition."¹³

The third major difficulty is brought out by Quine when he considers the kind of semantical rule Carnap uses in Meaning and Necessity. This is the semantical rule which says not that such and such statements are analytic but that such and such statements are included among the truths. This kind of rule does not have the drawback of containing the un-understood word 'analytic', but since this rule stipulates that only a certain multitude of statements are true, analyticity is explicated in the following way: a statement is analytic if it is (not merely true but) true according to the semantical rules. The difficulty here, Quine contends, is that now we are appealing to the unexplained expression 'semantical rule', "Semantical rules are distinguishable, apparently, only by the fact of appearing on a page under the heading of 'Semantical Rules'; and this heading is itself then meaningless,"14 Furthermore, not every true statement which asserts that sentences of a certain class are true can be counted as a semantical rule - otherwise all truths would be analytic in the sense just noted.

To this last objection, it has been replied that the semantical rules of a system do not provide a basis for determining all the true

> 13_{Martin}, <u>Op. Cit.</u> 14_{Quine}, "Two Dogmas," p. 34.

sentences of that system, but only those sentences whose truth can be shown through the resources of first-order logic.¹⁵ If, however, we take Quine's suggestion and view semantical rules as being analogous to postulates, then the selection of any set of semantical rules is only significant in relation to a given inquiry. "But from this point of view no one sign**a**lization of a subclass of the truths of L is intrinsically more a semantical rule than another; and, if 'analytic' means 'true by semantical rules', no one truth of L is analytic to the exclusion of another."¹⁶

With regard to semantical rules, Martin also says that even though Quine claims not to understand what semantical rules are, insisting that they are in need of clarification, Carnap is, nevertheless, quite explicit in what he regard semantical rules to be. They are either (1) definitions in the metalanguage, or (2) semantical axioms in the metalanguage. Thus, says Martin, "in either case, we know quite well what the semantical rules **Are** To object to them in the sense (1) is to object to semantical truth definitions of the Tarski kind. And to object to them in the sense (2) would seem tantamount to objecting to the very kind of general semantics Quine is demanding !"¹⁷

The above remarks by Martin and Quine's reply would seem to indicate that the two are talking past one another. Martin is anxious to make clear precisely what a semantical rule is and how we can demonstrate the truth of analytic sentences through the resources of firstorder logic on the basis of semantical rules, in order to show why it

> ¹⁵Martin, <u>Op. Cit.</u> ¹⁶Quine, <u>Op. Cit.</u>, p. 35. ¹⁷Martin, <u>Op. Cit</u>.

is that not all true sentences which says that sentences of a certain class are true can be considered to be included among the semantical rules. Quine, on the other hand, not disputing the validity of what Martin is saying, simply does not see why we should recognize some sentences as semantical rules and not others, i.e., why should some sentences be axioms in the metalanguage and others not. If this (what I have just said) is an accurate characterization of the dispute, then it seems to me that Quine is confusing the issue by saying that he does not understand the term 'semantical rule'. Certainly Quine understands what 'semantical rule' means as is evidenced by his reply to Martin. Quine does not seem to want a clarification of 'semantical rule', but rather a rationale for accepting one set of sentences as semantical rules instead of other sets. Now whether or not we can give reasons for adopting a certain set of semantical rules, 18 I think it should be clear that this is not a question that can be answered within semantics. And regardless of whether or not we can give reasons for the adoption of a certain set of semantical rules, we should not lose sight of the fact that we do know what semantical rules are and what they are supposed to do.

¹⁸For a discussion of this problem see Carnap's "Empiricism, Semantics, and Ontology," reprinted in Linsky, <u>Semantics and the</u> <u>Philosophy of Language</u>, p. 208-228. The considerations which will be relevant to the adoption of a particular set of semantical rules will depend upon the task we will want them to satisfy. We might adopt them arbitrarily if the choice we make is irrelevant to the study of a particular language. Alternatively, we might want the semantical rules to be of a certain kind in defining a language which is used in a technical field such as, e.g., physics. Also, in the case of natural languages, we may adopt a certain set of rules because we believe it preserves and reflects a prominent usage already in existence. As was mentioned earlier, however, we must keep in mind that considerations of this kind takes us beyond the bounds of semantics into pragmatics.

Of the three major difficulties pointed out by Quine which we have discussed, the one I find to be the most forceful is the one related to the double-elimination difficulty. Because this difficulty seems to be the basis of Quine's most forceful objection and because the objection is applicable (with some modification) not only to analyticity but to semantical concepts in general, it will be worthwhile to examine the nature of the difficulty in greater detail.

Apparently, the semantical rules of any language are required to fulfill two functions. First, they are to implicitly define semantical terms such as, e.g., 'true', 'designates', 'analytic', 'sentence', <u>etc</u>. for the language in question. But, in addition to this, semantical rules must also function as contextual definitions for the connectives and quantifiers.¹⁹ And this is more than can reasonably be expected of semantical rules.²⁰ In general then, semantical rules are to specify the meanings of signs in the object language and also serve as postulates which implicitly define various semantical terms occuring within them. The problem is that we need a way to define the semantical terms such as, e.g., 'analytic', independently of semantical rules.

If we could use, for example, '... designates-in-L ...' as a three place predicate ('L' ranging over languages), then we could be

¹⁹For example, the rule 'Si&Sj is true if and only if Si is true and Sj is true' interprets, i.e., contextually defines the sign '&'.

²⁰See N.L. Wilson, <u>The Concept of Language</u>, Chapter I, for an excellent exposition of this problem.

satisfied with a general definition of 'designates in'. However, just as is the case with 'analytic-for-L', the 'L' cannot be replaced. And so, we really have only defined the two-place predicate '... designatesin-L ...', i.e., 'designates' has only been defined for a particular language.

Another problem that is brought up by the double-elimination of the expression 'designates-in-L' is that since a language is never specified apart from its designation rules, any attempt to define 'designates', rather than the unitary expression 'designates-in-L', will ultimately depend on an antecdent understanding of the term 'designates'. For if we try to define 'designates', a valid question for a person to ask would be, "Which is the language for which you are defining 'designates'?" The answer would have to be something like, "It is the language in which - among other things - 'a' <u>designates</u> Chicago." And the problems here with 'designates' are also problems for other semantical terms such as, e.g., 'analytic'.

In passing, we may make Wilson's point that logical truth, even if we retreat into syntax, is not free from the double-elimination difficulty. For the initial rules are supposed to define a particular calculus, say K, and the expression 'direct consequence' for that calculus. But this does not give us a general definition of 'direct consequence for K' where 'K' ranges over all calculi. So we see that the double-elimination difficulty can be viewed not only as a semantical problem, but as a syntactical one as well.

There is, incidently, another problem which Wilson brings up

which is related to the double-climination difficulty.²¹ This problem, referred to as the <u>pedagogical difficulty</u>, reveals a major obstacle in the way of anybody who hopes to understand such terms as 'analytic', 'true', 'designates', and 'language' in a philosophic way. That is, in a way that does not presuppose an antecedent understanding of these above terms. Let us illustrate this problem with the term 'designates'.

Suppose we wish to teach a German (who, for convenience sake, we shall call 'Hermann') the language L, which is, in fact, a reconstruction of English. We have the rule book for this language, but it is written in English. And so, we have to translate parts of it into German for Hermann's benefit. Now we should not have to translate the word 'designates' because it is defined in M (the metalanguage for L). So we begin giving Hermann an enumerative definition of 'designates' for L, telling Hermann, "'Cologne' designates in der Sprache L Köln; 'the moon' designates in der Sprache L den Mond," and so on. When we have finished with our enumeration, it is doubtful whether or not Hermann understands what 'designates' means; for all he knows, it may be just an arbitrary relation in extension. If we give him a hint and say, "Hör mal zu Hermann, jedes mal wir benutzen das Wort 'designates' das heißt 'bedeuted'," then we are trading on Hermann's antecedent understanding of 'bedeutert' and this kind of understanding is not the philosophical kind we are looking for when we try to explicate or define 'designates' in L.²² And so, Hermann has really not been taught our

21 Wilson, Op. Cit.

²²Bohnert may have something like the pedagogical difficulty in mind when he says, "We may, if we wish, make stipulations or commands to 'Speak English' or 'Speak LO' but the commands will contain a curious regression if what English is or what LO is cannot itself be specified by simple declaratives." ("Carnap on Definition and Analyticity," p. 418)

semantical definition of 'designates' and this shows our apparent inability to explicate a semantical term without relying on some intuitive notion of the term that is being explicated.

R.M. Martin makes a similar comment to the one that Wilson makes regarding the double-elimination difficulty,²³pointing out that what Quine is demanding of analyticity, viz., a general definition, has not even been given for truth. The difference is that whereas Wilson seems to be saying that Quine should have demanded more, Martin maintains that Quine is asking for too much.

Martin argues that since we do not have a general definition of truth, even for artificial languages, Quine should be willing to say that our distinction between truth and falsity is a myth, another dogma, apparently not only of empiricists, but of people generally. Indeed, at least a definition of 'analytic' is not <u>bound</u> to be inconsistent in a natural language (as is a definition of truth). To ask for a definition of analyticity for natural languages is exorbitant, Martin says, while it is at best premature to demand a definition of 'analytic' for even artificial languages.

We have, up till now, examined what I think to be the three most important objections raised by Quine with respect to the use of semantical rules in explicating 'analytic'. And I think we can fairly say that two of his objections are indecisive. However, the objection which raises the problem of the double-elimination difficulty seems to

23_{Martin, Op. Cit.}

be both valid and crucial. And, in itself, this objection would cast more than a shadow of doubt on the notion of 'analytic', if it were not for the fact that the double-elimination difficulty is an obstacle which stands in the way of an adequate explication of the other semantical terms as well. But since this difficulty is one which plagues not only 'analytic', but other, more respectable, semantical terms as well, we would not be any more justified in dismissing, as a "metaphysical article of faith," the analytic-synthetic distinction than we would be in dismissing the distinction by ween truth and falsity. However, if we are going to insist on bringing 'analytic' to court to answer to the charge of "unintelligible," then it is going to drag a lot of its (semantical) cronies along in the process. So, it seems as if the notion of 'analytic' has squiggled out from under the noose of Quine's objections or, at least, it does not have to face up to the charges alone. However, there is one further difficulty which must be overcome if the explication of 'analytic' (as is offered by Carnap) is to be considered satisfactory.

The difficulty I am referring to stems from the fact that, strictly speaking, Carnap does not define 'analytic' (in either <u>Intro-</u> <u>duction to Semantics or in Meaning and Necessity</u>) but rather 'L-true'.

In <u>Meaning and Necessity</u>, for example, Carnap's definition of 'L-true' seems to provide a perfectly good explication of <u>logical truth</u>. However, if Carnap wants to make this definition do for 'analytic' as - well, then the definition can only work for languages whose atomic statements are, unlike 'John is a bachelor' and 'John is married', mutually independent. "Otherwise there would be a state-description

which assigned truth to 'John is a bachelor' and to 'John is married', and consequently 'No bachelors are married' would out to be synthetic rather than analytic under the proposed criterion."24 To put the matter in another way, following the above explication, how are we to determine beforehand that a state-description containing both of the atomic sentences 'John is a bachelor' and 'John is married' is impossible? How are we to know that the properties which are designated by the predicate terms 'bachelor' and 'married' are logically incompatible without some prior knowledge? We have no way of knowing that the terms 'bachelor' and 'married' are dependent, let alone in what way they are dependent. There are also problems with relational terms such as, e.g., 'warmer than'. How do we know whether or not a statedescription is possible which contains the atomic sentence 'Wzz', where 'W' is interpreted as 'being warmer than'? Relation terms are even more serious than one-place predicates due to the fact that the logical dependencies of the one-place predicate terms can be avoided by replacing them with independent predicates with the same expressive power, whereas this cannot be done with many-place predicate terms.

Carnap recognizes the problem and attempts to overcome the above stated difficulty by introducing into L what he calls "meaning postulates".²⁵ In order to show the logical dependency of the oneplace predicates 'bachelor' and 'married', the following meaning postulate is laged down:

²⁴Quine, <u>Op. Cit.</u>, p. 23. ²⁵R. Carnap, "Meaning Postulates," <u>Philosophical Studies</u>, Vol. III (1952), p. 65-73.

(P1) '(x) (Bx==Mx)'.

Notice that the designation rules are not given for 'B' and 'M'. According to Carnap, the designation rules are not necessary for the explication of analyticity. All we need to know about the meanings of 'B' and 'M' is that they are predicates of logically incompatible properties.

Now with regard to the two-place predicate'W', which designates the relation Warmer than, it will have to be shown that the statements 'Wab & Wbc & -Wac', 'Wab & Wba', and 'Waa' are false in virtue of their meanings, since 'W' is transitive, irreflexive, and hence, asymetric in virtue of its meaning. So then, three postulates will be required for the predicate 'W':

(P2) (a) '(x)(y)(z) (Wxy & Wyz = Wxz)'
(b) '(x)(y) (Wxy = -Wyx)'
(c) '(x) -Wxx'.

Now in using the meaning postulates to define 'analytic', we let P be the conjunction of all the meaning postulates which have been accepted for the system L. Then, keeping in mind the definition of 'L-true' given at the beginning of this chapter, we define 'analytic' in the following way:

(D1) A sentence S in L is L-true (analytic) with respect to

P =df S holds in all state-descriptions in which P holds. Alternatively, we may let L be the system without meaning postulates and L' be the system constructed out of L by adding the meaning postulates P. In that case the state-descriptions in L' will be defined as
those state-descriptions in L in which P holds. And we may define 'analytic' as follows:

(D2) S is L-true (analytic) in L' =df S holds in every statedescription of L'.

Carnap seems, then, to have found his way out of the difficulty of reflecting in the system L the logical dependency of oneplace predicates and the structural properties of primitive two-ormore-place predicates. But how do we know when there is a meaning relationship between two or more one-place predicates? Or how do we know when a primitive relation predicate has structural properties which must be mirrored in the object language with the help of meaning postulates? It seems we are back to the same old problem the meaning postulates were employed to solve, viz., that without prior knowledge we have no way of determining whether or not predicates such as, e.g., 'bachelor' and 'married', are logically dependent. Carnap, however, can avoid (or at least he attempts to avoid) getting into this bind again by claiming that the determining of whether or not a predicate or a group of predicate terms require a meaning postulate is not a matter of knowledge at all, but of decision.²⁶

According to Carnap, if our wish in constructing a system is to reflect some of the meaning relations of English, then our knowledge or belief that the predicates 'bachelor' and 'married' are usually understood to be incompatible will influence our decision. However, ' the dependency of 'bachelor' and 'married' is relatively clear and in

²⁶Carnap, "Meaning Postulates," p. 68.

other cases the dependency will be far more problematic. For example, if in our system the predicates 'B1' and 'R' correspond to the words 'black' and 'raven', then because the dependency between the two predicates is not so clear-cut, we will just have to make up our mind (in a somewhat arbitrary way) whether or not we want to give 'R' a meaning so strong that it cannot possibly be predicated of any non-black thing.

It is obvious that Carnap is not too concerned with the problem of how to find a pragmatic criterion to test the correctness of our decision to use a particular meaning postulate. And because Carnap would remain unmoved by the charge that the choice of meaning postulates (and hence his definition of 'L-true') is arbitrary, it does little or no good to point out the oddities which result from the adoption of certain statements as meaning postulates, which are, indeed, to be found in abundance. We cannot force Carnap to do pragmatics when his interest is only in semantics and let's not criticize semantics for not doing the job of pragmatics.

Still, can Carnap simply ignore the pragmatic issue, i.e., the demand to provide criteria for the testing of sentences to determine whether or not they should be included in the class of meaning postulates and, at the same time, hope to shed any light on natural languages? We are supposed to distinguish declarations which are to count as meaning postulates from those which do not. But apparently, this is done simply by attaching the label "meaning postulate" over some sentences and not over others! How are we to reflect in our description of, e.g., English, the necessary truths when our decision proceedure, as to what are to count as meaning postulates, seems so arbitrary?

This is indeed a difficult problem, but I think Carnap does have an answer. Namely, we just have to ask ourselves whether we want to make a resolution to the effect that we will apply no predicate term to any object unless that object satisfies a certain condition. Our decision must take into consideration not only actual cases, but possible cases as well. The reason the meanings of words are unclear to us, at times, is that we have not considered all the (logically) possible cases and hence, have not fully determined in our own minds how we would apply the term under unusual circumstances or conditions. This lack of clarity does not normally bother us because we become aware of possible cases only rarely, and they have little practical importance for us.

CHAPTER V

GRADUALISM

Quine, in bringing the analytic-synthetic distinction under suspicion as a mere dogma of empiricists believes also to have thrown the notions of meaning and cognitive synonymy under an unfavorable light as well. Quine has argued, with respect to meanings, that probably on account of a confusion between meaning and reference. there resulted a felt need for meanings as entities. However, "once the theory of meaning is sharply separated from the theory of reference. it is a short step to recognizing as the primary business of the theory of meaning simply the synonymy of linguistic forms and the analyticity of statements; meanings themselves, as obscure entities, may well be abandoned."1 This point, though well taken, does not, however, get at the way many empiricists have attempted to explicate (cognitive) meaning. Modern empirisists have spoken of the meaning of a statement as the method of empirically confirming or infirming it; two statements are synomymous, according to this yiew, if the method of verification is the same for both and an analytic statement is one which is confirmed no matter what,² As is to be expected, any theory, such as the

¹Quine, "Two Dogmas" p.22, see also "On What There Is" p.11 and "The Problem of Meaning in Linguistics" p.48, both in From a Logical Point of View .

As Quine points out, though this is an account of synonymy for statements only, we can derive the concept of synonymy for other linguistic forms on the basis of this account; any two linguistic forms are synonymous when the putting of the one form for an occurance of the other in any statement yields a synonymous statement.

verification theory of meaning, which pretends to explicate synonymy as well as analyticity, is going to be viewed with a very critical eye by Quine. For if the verification theory of meaning is correct, then the notion of analyticity is saved after all.

Now what is essential to the verification theory of meaning is a relation which holds between statements and the experiences which increase or decrease the likelihood of their truth (and thereby estab. lishing the cognitive meaning of any particular statement). The nature of this relation is seen by Quine as the other dogma of empiricism, viz., radical reductionism. Actually, radical reductionism, inits naive form, antedates the verification theory of meaning, for the view that all ideas originate in sense experience is characteristic of the early British Empiricists. Now, I do not have the space or time, nor do I care to report on the vicissitudes of reductionism; however, the view held by many modern empiricists that there is a unique set of experiences, which together constitutes the cognitive meaning of any particular statement, takes for granted, according to Quine, that the meaning of a statement can be determined in isolation from other statements of the language in which it occurs. It seems then, as though we are led from reductionism via the verification theory of meaning to the analytic-synthetic distinction. For if we can speak of confirming or infirming statements in isolation, it seems plausible to posit a kind of statement which is confirmed no matter what experiences we have; and such statements are analytic. The plausibility of this view derives from the fact, evidently, that the

truth of statements depends not only upon extra-linguistic facts, but upon language as well. "Even so factual a sentence as 'Brutus killed Caesar' owes its truth not only to the killing but equally to our using the component words as we do."³ An analytic sentence, then, is one which has no factual component.

As we have seen, Quine maintains that the distinction between the analytic and the synthetic resists any such straight-forward drawing. And, what is more, Quine argues that a clear line will never be drawn because the alleged distinction is based upon the two related but mistaken notions that (1) there are two distinct and sep**a**rable components to a statement upon which its truth rests and (2) the meaning of any statement can be determined in isolation from other statements of a language. Philosophical progress was made when the term by term empiricism of Hume was discarded and the statement was made the unit of empirical meaning and confirmation. But even in taking the statement as the unit of meaning, we have, according to Quine, drawn our grid too finely. The unit of empirical significance is the whole of science - "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body."⁴

I have argued in the preceeding chapters that the arguments leveled by Quine against the analytic-synthetic distinction are by

> ³Quine, "Carnap on Logical Truth", p.386. ⁴Quine, "Two Dogmas", p.41.

no means so devastating as he would have us believe. I have tried to show in a clear way Carnap's method of distinguishing analytic from synthetic statements and, in so far as I have succeeded. I think we can view Carnap's explication of the notion of analyticity as a fruitful and enlightening enterprise. And so, although, according to Quine, the reductionist "dogma" is, at root, identical with the other dogma of empicicism,⁵ viz., the analytic-synthetic distinction, I feel no inclination to vindicate either the verification theory of meaning or reductionism. I would, of course, maintain that in so far as Quine is correct in linking the analytic-synthetic distinction with the verification theory of meaning and reductionism, the latter alleged dogma is every bit as valid as the former, for I do believe that the analytic-synthetic distinction is a valid one. However, it has certainly not been decisively shown, Quine not withstanding, that the reductionist thesis and the analytic-synthetic distinction are so intimately connected as to be identical. At any rate, it is by no means obvious, that Carnap must rely on the reductionist thesis in order to explicate the concept of analyticity. Hence, what I hope to accomplish in this chapter is not a further defense of the analyticsynthetic distinction by trying to show the soundness of the verification theory and reductionism, but rather, I intend to examine the thesis which Ouine presents in place of the view that we can isolate any true statement and determine to what extent its truth is factual and to what extent linguistic. This thesis, which Quine (along with others)

⁵Ibid

argues for has come to be called the "gradualist thesis".

The gradualist thesis is so-called for it asserts that there is really no generic difference between statements which we call 'analytic' and statements which we say are synthetic. Rather, there is a gradual continuum which includes all statements: some statements are held to be more certain than others but none are solely analytic or solely synthetic, i.e., factual. The only indication that a statement is closer to the analytic side of the continuum seems to be the reluctance we have in abandoning it as true. Thus, the analytic-synthetic distinction is seen from the point of view of the gradualist to be only a matter of degree. What it is a degree of is difficult to say, since the thesis is argued for in a variety of ways. Both Morton White⁶ and Nelson Goodman,⁷ for example, present their gradvalism in a different way than Quine does. White apparently bases his gradualism on the contention that we have no sure way to determine (behavioristically) what terms are not just co-extensive, but also co-intensive. Goodman, on the other hand, is a gradualist because he thinks that no two distinct terms are ever identical in meaning, i.e., cognitively synonymous, but only approach synonymy in degrees.

We have seen though (in chapter 3) that Goodman's argument supporting his claim that no two terms are synonymous does not stand up after analyses. And in so far as his argument fails, so does his gradualism. For he claims that we can only have degrees of synonymy

⁶"The Analytic and the Synthetic - an Untenable Dualism," in Linsky's <u>Semantics and the Philosophy of Language</u>. "On Likeness of Meaning," in Linsky.

and degrees of analyticity (in non-repetitive statements) just because we have no two distinct terms which are fully cognitively synonymous, i.e., which can be interchanged in all non-intentional or belief contexts <u>salva venitate</u>. So much then for the gradualist thesis as proposed by Nelson Goodman.

The gradualism posited by Morton White also seems to the result of a confusion or two. At one point.⁸ White offers the statement. "If we were presented with something which wasn't a rational animal. we would not call it a man", as an example of the kind of statements some philosophers use to clarify the notion of analyticity. White suggests we test the effectiveness of such a statement in distinguishing analytic statements from synthetic ones by trying it on,"All men are featherless bipeds", which is by hypothesis, not analytic. He then says that those who use this statement ("All men are featherless bipeds") as a criterion would have to deny that if we were presented with an entity which was not a biped or not featherless we would not call it a man,⁹ But since we do withhold the term 'man' from those things which we know to be either non-bipeds or non-featherless, White supposes that this criterion must be faulty. What is faulty, though, is White's analyses. For in accepting that the statement "All men are featherless bipeds" is not analytic, we are not forced to deny that if we were presented with an entity which was not a biped or not featherless we would not call it a man. What we would be forced

> ⁸White, <u>Op. Cit.</u> p.326. ⁹White, <u>Op. Cit.</u> p.326.

to deny is the utterly different assertion that for any entity which is neither featherless nor bipedal, it is necessarily not a man.

In developing his "argument," this same kind of error crops up again. White envisages a tribe which has in its vocabulary the words 'man', 'rational', 'animal', 'featherless', and 'biped'. After being informed (or rather, told) by anthropologists, who have visited this tribe previously, that 'man' is synonymous with 'rational animal' for that tribe, where as 'featherless biped' is merely co-extensive with 'man', we set out to confirm the anthropologists' report. Now because the people of this tribe withhold the term 'man' when presented with entities which are not rational animals; and, moreover, all those entities presented which are not rational animals are not featherless bipeds either, White concludes that we are no more justified in holding that 'man' and 'rational animal' are synonymous than we are in maintaining (on the basis of our test) that 'man' is synonymous with 'featherless biped'. However, at the risk of repeating what was said earlier (see chapter 3), we must point out that the intension or meaning of a term can never be determined on the basis of actual cases alone; the people of this hypothetical tribe must be asked whether or not they would apply the terms 'rational animal' and 'featherless bipeds' to precisely the same possible¹⁰ cases as well. If an entity can be described to which a member of the tribe would apply the one term while witholding the other, then the terms are obviously

¹⁰I use the model term 'possible' here only for convenience; as was said in Chapter3, this term can be eliminated if it is thought to be objectionable.

not alike in meaning.

But White, not able to determine the intensions and hence the synonymy of the terms 'man' and 'rational animal' because he restricts himself unnecessarily to only the actual cases, finally does, apparently, resort to possible cases. He allows that in pursuing the natives in another way, we might ask them: "Would you call something a man if it were not a featherless biped? To which they answer in the negative. Would you call something a man if it weren't a rational animal? To which they answer no again,"¹¹ Since there is no difference in the natives response to the questions put to them, White presses the natives further by asking them, "Aren't you surer in concluding that something is not a man from the fact that it is not a rational animal, than you are in concluding it from the fact that it is not a featherless biped? "12 If in response to this question, the natives answer affirmatively, then, White asserts, though we have a criterion to distinguish the difference between synonymous terms and other terms, it is a distinction only of degree. "Not being a rational animal is simply a better sign of the absence of manhood than is the property of not being a featherless biped, just as the latter is a better sign than the property of not successing a derby hat."¹³ Thus we see the "gradylism" of Morton White against which I would like to make two points.

First of all, if the natives of this community were indeed

11<u>op. Cit.</u> p.327. 12<u>Ibid</u> 13<u>Ibid</u>

to answer negatively in response to both the questions "Would you call something a man if it were not a rational animal?" and "Would you call something a man if it weren't a featherless biped?". then there is good reason to suppose that both the expressions 'rational animal' and 'featherless biped' are synonymous with the term 'man'. and the anthropologists' report is incorrect. This is assuming, of course, that the natives understand very well that we are speaking of all logically possible cases as well as what might be called physically possible cases. Once the natives are clear, though, as to what is meant by logically possible cases, i.e., cases which can be described without regard to factual considerations, then, if they still refuse to apply the term 'man' to all possible entities which are not featherless bipeds or rational animals, then we have no other alternative than to conclude that the said terms are synonymous with 'man'. And it is completely beside the point (in sofar as we are concerned with synonymy to ask the natives, "are not your reasons for not calling something, which is not a featherless biped, a man different than your reasons for not calling something which is not a rational animal a man?" Of course the reasons are different. In one case, the term 'man' is not applied because the supposed or given entity is not a rational animal, whereas the reason for withholding the term in the othercase is that the supposed or given entity is not a featherless biped. This brings me to the second point I wish to make against White's argument.

White asserts that because the natives say that they are surer in concluding that something is not a man from the fact that it is not a rational animal than they are in concluding it from the fact that it is not a featherless biped, we have the makings of a criterion, "though it is a criterion which makes of the distinction a matter of degree,"14 Yet, I cannot see why this criterion should have anything at all to do with synonymy. It is as if someone were to claim that 'bachelor' is only synonymous with 'unmarried man' to a degree because in some cases it is very difficult to find out whether a particular man is married or not! Contrary to what White says, if all and only featherless bipeds are rational animals, then even though the property of being a rational animal is essential to manhood (whereas the property of being a featherless biped is not), it may still be easier to use the fact that a certain entity is a featherless biped as a criterion for testing whether or not it is a man. This is so just because, as a matter of fact, most or all men are featherless bipeds and this property may be more obvious than the property of being a rational animal. Similarly, we may check the marital records at city hall to find out whether a man is married or not, though 'bachelor' and 'name not listed in marital records' are certainly not synonymous. Nor does it mean that 'bachelor' and 'unmarried man' are only synonymous to a degree. I think we may then, in light of the above, conclude that the gradvalism as propounded by Morton White is a bit misguided. So let us now turn to a look at the gradualism of Quine which is, by far,

14 Ibid

bolder and more interesting.

It is not easy to give a clear picture of what Quine is asserting in maintaining his gradualist thesis, for unfortunately he himself does not meet the standards of clarity which he demands of the proponents of the analytic-synthetic distinction, but rather, relies heavily on metaphorical description. Our knowledge, he says, is a "man-made fabric which impinges on experience only along the edges. Or to change the figure, total science is like a field of force whose boundary conditions are experience."¹⁵ When we have a conflict, then, between a prediction, call it P, and experience. readjustments must be made somewhere in our conceptual scheme; and in reevaluating P we must reevaluate other statements insofar as they are connected or intertwined within the fabric of our conceptual scheme. However, according to Quine, there is no question of right or wrong when it comes to the readjustment of statements within our conceptual scheme, occasioned by conflict at the periphery of experience. Rather, we are guided only by considerations of simplicity and conservation,¹⁶ and hence, we have a wide choice as to which statements we will adjust, i.e., give new truth values to, especially as we move to areas within our system which are not directly connected with sense experience. aside from simplicity, there is no more to ask of a theory other than that it be able to explain facts and predict future events; there is no other criterion to test the "truth" of a theory.¹⁷

> 15 Quine, "Two Dogmas" p.42. 16"Two Dogmas" p. 43.

¹⁷Thus Quine believes that the only difference between a theory which points physical objects and one which posits Homer's Gods is that one is more effacacious in explaining the facts.

Naturally, when a prediction concerning a particular set of sense experiences fails more than a few times, we view that theory with suspicion which led us to make our prediction and we strive to revise those statements within our theory upon which the prediction was based. And so it is that theories are rejected or revised when their predictions fail. However, Quine also points out that a theory may consist in such firmly conditioned connections between the statements of that theory and the predictions it gives rise to that it withstands the failure of a prediction or two.¹⁸ We are always more inclined to explain the failure of a prediction as due to a mistake in observation, unexplained interference, or even hallucination, rather than give up the theory, even though it indevcloped in order to save appearances. In such cases, "the tail thus comes, in the extremity, to wag the dog."¹⁹

Quine's point then, is that it is senseless to speak of the empirical content of a statement, especially a statement which is remote from the experiential periphery of the conceptual field, for the meaning of any statement can only be determined within the context of a theoretical system which includes logical laws as well as sentences reporting immediate experience. And if we accept that within every statement the factual and the logical are intimately and inseperably connected in proportions depending upon the systematic import of other statements in the conceptual scheme, then the analytic-synthetic

> ¹⁸Quine, Word and Object p.18. Quine, Word and Object p.19.

distinction appears to be one of degree and not of kind. For a statement is only analytic because it happens to be central to our conceptual scheme, i.e., it is not linked directly to sense experience, and hence, we are more likely to change the truth values of those statements which are more directly associated with a recalcitrant experience. However, if for the sake of over all simplicity we find it more fruitful to give up even the central statements as true, then we are certainly justified in doing so, analytic statements or not. "Physical laws can be challanged, and revised without ever being themselves subject to test in any particular experiment."²⁰ So, Quine argues it is very difficult, if not impossible, to show how these physical laws which apparently have the status of analytic definitions, differ in any essential way from other scientific hypothesis. For while physical laws such as, e.g., F=MA, do have a preferred status epistemologically, they can, as any hypothesis can, be overthrown if we adopt a system (for the sake of overall simplicity) which includes statements incompatible with other statements previously held to be true by definition, e.g., 'F=MA'. Even revision of the law of the excluded middle, Quine argues, has been proposed in order to simplify quantum mechanics.

The net result of Quine's thesis is twofold. On the one hand, "any statement can be held true come what may, if we make drastic enough changes elsewhere in the system."²¹ As was mentioned above, we may even hold on to the truth of a statement which is relatively close to the periphery of experience such as, e.g., 'There are no

> ²⁰Putnam, "The Analytic and the Synthetic". ²¹Quine, "Two Dogmas" p.43.

flying saucers from other planets', by pleading hallucination in the face of conflicting experience. On the other hand, Quine maintains that no statement is immune to revision. Thus, if what Quine says is correct, the distinction between truths by stipulation, i.e., analytic statements and truths by experiment, i.e., synthetic statements, is one which can scarcely be as clear as Carnap makes it out to be. Now, we will want to examine these extraordinary, if not to say revolutionary, conclusions very carefully, buy first I want to make a few preliminary remarks.

To begin with, we have seen that because Quine does not believe we can attribute meaning to single statements, be argues that we must view the whole of scientific knowledge as the unit of significance. But why should we think that the whole of our scientific knowledge is so closely related as to be considered the basic unit? Why not a part? "In what way, for instance, does my discovery that I have forgotten whether I boarded the train at 12:12 or 12:13pm, affect the sunspot theory of economic crises?"²²Certainly the onus is on Quine to show the relationship between the statements of our language(which are at first sight totally unrelated) in more detail.

Also, just because a statement is synthetic on Tuesday, it does not mean that the same statement was not analytic on Monday; just because a statement "becomes" synthetic, it does not mean that there is no distinction between the analytic and the synthetic, and

²²Albert Hofstatder, "The Myth of the Whole", <u>The Journal of</u> Philosophy (1954) Vol. 5, p.397-417.

it may be an unblurred one at that. For though once we have explicitly reconstructed a particular language L1, and hence, are able to determine all the analytic sentences of that language, the reconstruction of L_1 (and hence the ability to determine all the analytic sentences of L_1) does not mean that we cannot give up L1 and adopt another language, say L2, in its place, even though though the analytic status of some or all of the sentences of L_1 will be changed. Furthermore, strictly speaking, a sentence S_1 in a language L_1 will never change its analytic status, for a sentence can only be defined with respect to a given language. And so, the most we would be justified in saying is that a certain sign-design is common to two different languages L_1 and L_2 ; in L^1 the sign-design along with its interpretation is the sentence S_1 , while in L^2 it is the sentence S_2 . And though both S_1 and S_2 may be analytic or synthetic, they are not identical sentences. More will be said about this in the concluding chapter but let us say, in passing, that Quine does not seem to be taking these factors into account when arguing for the gradualist thesis.

Quine maintains that an analytic statement is really only a statement which is central to a conceptual system. But a question which needs to be answered is why it is just these particular statements have been deemed central, for after all, there is a great deal of agreement over which sentences are or are not analytic. If all Quine can say in answer to this question is that we choose those statements as central which afford the simplest conceptual/scheme consistent with the facts, then the gradualist is open to his own

questions, viz., what determines what the facts are? However let us pass over this question and see whether or not the thesis that any statement can be held true come what may or that no statement is immune to revision can really stand up after closer examination.

First, let us consider the claim that any statement can be held true come what may. In asserting that any statement can be held true come what may, provided we make drastic enough adjustments elsewhere in the system, Quine is subscribing to the Duhemian thesis that every empirical statement S is linked inductively with an auxiliary set of assumptions A, which makes it impossible to conslusively refute S.²³ This means, in effect, that if an empirical statement S linked together with A entails a certain set of observational consequences O, then the failure of O to appear, i.e., some other set of observational consequences are the result instead, say O¹, does not by itself entail the falsity of S. For supposedly, we may be able to modify A in such a way that together with S, the adjusted A, call it A¹, does entail O¹. The most drastic cases of adjustment are, with respect to central statements, revision of logical laws, whereas, with respect to statements at the periphery of experience, the plea of hallucination.

Of course, this thesis can be made trivial if we count as an alteration of assumptions the changing of the meanings of words which occur in S. The statement 'Gold dissolves in water', for example, can be maintained as true if our "adjustment" is the stipulation that

²³Pierre Dubem, <u>The Aim and Structure of Physical Theory</u>, N. J., Princeton University Press, 1954, p. 183-190.

by 'gold' we mean table salt. But certainly, Quine is not concerned with cases such as these. This thesis can also be made trivial if we are forced to plead hallucination to an excess, for any theory which can be maintainced only at the cost of people constantly pleading hallucination is, Quine will certainly concede, hardly to be regarded as useful.

Incidently, Quine's use of the word 'hallucination' seems to be a bit extraordinary. Normally, when we speak about hallucinations, we are talking about somebody's impression of an entity (or a property of an entity) which nobody else has. If a person sees pink elephants and nobody else does, we say he is hallucinating. Thus our evidence for saying that somebody is hallucinating is largely that nobody else has the appropriate impression. However, an occurance of the kind of hallucination Quine is suggesting we plead (to save a particular theory) is not confirmed in such a way. His kind of hallucination everybody would suffer and the only "evidence" (if we can call it that) which we have is that it does not contradict theory. Furthermore, whenever anybody does suffer an hallucination, there is a cause for its occurance which many times can be determined. But what kind of explanation can be given for an occurance of the kind of hallucination Quine speaks of? There seems to be no explanation even though we would especially want one in a case where everybody suffered the same hallucination.24

²⁴These points were suggested to me by N.L. Wilson in conversation.

To return to the Duhemian thesis, Quine is apparently maintaining that for any S and A which imply or entail O, if a disconfirming set of observational results O' is encountered, then there is a (non-trivial) A' which, together with S, theoretically explains or implies O'. The important question here is not the psychological one of whether or not scientists possess the ingenuity to discover (or construct?) such an A'. Rather, the crucial question is the theoretical one of whether or not there is, in principle, for every S a nontrivial A' which in all cases preserves the truth of S in the described manner. Judging from what has been said, such a claim appears to be a non-sequitur. For it does not follow from the Duhemian thesis that S cannot be conclusively refuted on the basis of conflicting observational data, that there is such an A', i.e., a modified set of auxiliary assumptions, which can preserve the truth of S in all cases. Evidently, Quine takes it on faith that there always exists such a modification of theory. But this question cannot be considered closed and the onus is again on Quine to show in greater detail how it is that the truth of any sentence can be preserved "come what may,"

Let us now consider the claim that no statement is immune to revision. It is Quine's contention that since analytic statements, i.e., definitions, act as premisses in predictive inference, the decision to alter these in the face of a conflict between prediction and observation differs in no essential way from our decision to alter an empirical hypothesis. However, as Bohnert asks, "What can it be to disconfirm or revise a <u>definition</u> (on any grounds)?"²⁵I agree with

25 Bohnert, "Carnap on Definition and Analyticity," p. 426.

Bohnert that it cannot be done. For since every definition of a term in a language L figures indirectly in the formational and transformational rules constituting that language, to assign that term, i.e., sign-design, a different definition than that assigned to it in L is to move to a different language L'. And although it may seem a bit eccentric to maintain that we move to a different language when defining a sign-design in a different way, it is as simple an explanation as any other consistent alternative. Now in denying that, strictly speaking, definitions can never be revised, we do not mean to say that physicists, for example, do not aim at the same intuitive concept in adopting a new definition to replace one that, for some reason, is unsatisfactory. It is just that to adopt a new definition is to adopt a new language. I hope to make this point clearer later on in the conclusion.

This brings up the question as to whether we ever revise logical laws. Quine contends that revision of the law of excluded middle has been proposed as a way of simplifying quantum mechanics. Let me say at the outset, first of all, that I have not been convinced that there is any observational data in quantum mechanics, or anywhere else, which would necessitate, or even indicate, that we should revise or abandon the law of the excluded middle. But for the sake of argument, let us assume that some experimental evidence has motivated a scientist to declare the law of excluded middle false. In this case, the law would not be revised but abandoned, or to be more precise,

the language in which the law is taken to be L-true would be abandoned. The scientist would adopt a different language which better serves his purposes, i.e., does not contain an L-true sentence which has been empirically disconfirmed. This would not mean, though, that the deductions made on the basis of this law within that language (in which it is included) have been invalidated any more than the proofs of Euclidean geometry have been invalidated with the advent of non-Euclidean geometries, even though Euclidean geometry has been empirically overthrown.

On the other hand, our scientist may merely want to say that the law of the excluded middle has to be restricted, that is, it can not be applied in certain contexts such as, e.g., in quantum mechanics. Here though, I cannot see how such a restriction can be considered a revision, if by 'revision' we mean a change in the truth value of a sentence (which is what I take Quine to mean when he says that no statement is immune to revision).

Perhaps it will be argued, to take a different line, that apparent revisions or disconfirmations of definitions have really been revisions or disconfirmations of hypotheses. These hypotheses such as, e.g., 'energy = $\frac{1}{2}MV^2$ ', were simply so highly confirmed empirically that they had come to be spoken of (wrongly perhaps) as definitions. This tendency to call scientific hypotheses 'definitions' is probably the result of an unsound analogy between mathematics and the empirical sciences.²⁶ But due to the empirical nature of these

²⁶Bohnert, "Carnap on Definition and Analyticity" p.427.

so-called "definitions" they should not be treated as immune to revision if observation calls for it. In fact no sentence not purely logical, i.e., one in which only the logical terms occur exsentially should be treated as immune to revision, but rather should be treated simply as an empirical hypotleses.

In response to this argument, let us say first of all, that even if we do decide to treat only logically true sentences as analytic (and regard all others as empirical hypotheses), the analytic-synthetic distinction is in no way destroyed or even "blurred". If anything, the distinction becomes sharper than ever; all and only logically true sentences are analytic and all the rest are synthetic. The above argument, if valid, could only show that more sentences are synthetic than previously supposed.

However, there are good reasons why we should <u>not</u> decide to treat only logically true sentences as analytic. "Not only does there seem little to be gained by holding open the possibility of disconfirming the hypotheses that a meter is a hundred centimeters in length or that electric field strength is the gradient of electrical potential, but a serious loss of control in communication would result from the abandonment of definitions or other devices for producing analytic sentences other than logical truths."²⁷ If a language has certain rules which are immune from revision, the language, to an extent is frozen, and this enables the speakers of a language to communicate better because they share a common basis intrinsic to their language.²⁸

> 27 Ibid 28H. Putnam, "The Analytic and the Synthetic" p.383.

Another attraction, related to the above, is that with fixed rules the language is more easily taught. Finally, it is just more convenient to use a word like, e.g., 'bachelor', rather than, e.g., 'unmarried, adult, male, never married before, etc.'

But entirely aside from the above considerations, what could possibly motivate anyone to revise a statement such as, e.g., 'all bachelors are unmarried'? What kind of theoretical adjustments does Quine envisage which would prompt a revision of such statements. We will never find any natural laws conflicting with such a statement because the class of bachelors is defined by ignoring all aspects except a single legal one. It seems that any kind of revision of these kinds of statements will only be the trivial sort which arises from unintended and unexplained historical change in the use of language.²⁹

It seems then, that even if we grant that many statements which are now thought to be analytic are really synthetic or, at least, are not immune to revision, we still have statements such as, 'No bachelor is married,' to keep the distinction in tact. Hence, I conclude on the basis of what has been said that the gradualist thesis, as propounded by Quine, in no way "blurgs" the analytic-synthetic distinction.

29 Putnam, Op. Cit.

CHAPTER VI

CONCLUSION

In chapter II we remarked that the so-called "natural" languages lack the precision and intensional clarity of those languages which have been explicitly constructed. Undoubtedly, part of the problem in ascertaining whether a given statement is analytic or synthetic in, say, English is due to the fact that English is not in all respects unambiguous or, at any rate, the "meanings" of all its expressions have not been specified to a sufficiently high degree to allow for an easy determination in all cases.¹

This problem is compounded when we consider that English, because it is used, is constantly evolving, that is, the meanings of the expressions are not static. Rather they change with usage, so that even if we can precisely describe English at time t1 on the basis of the usage of speaker P(and thus be able to determine for every sentence whether it is analytic or synthetic in English for P at t_1), there is no guarantee that that description would be correct

¹Perhaps it would be more correct to say, following Wilson, that it is not English which lacks precision and intensional clarity but our use of English; English itself is as precise as any other language. See The Concept of Language, Chapter VII.

for the language P uses at time t_2 .² A new description may very well be required of the language P uses at t_2 or, if not a new description, at least a modification of the initial one. Now this does not mean, as we have tried to show in the last chapter, that a statement can be analytic at t_1 and synthetic at t_2 . But it may mean that a certain statement may <u>appear</u> to be analytic at t_1 and synthetic at t_2 . Actually, if a statement appears to be analytic at t_1 and synthetic at t_2 on the basis of observing P's linguistic behavior, it is a good indication that P is using a different language.

One might think that I must be awfully desperate, in wanting to maintain the analytic synthetic distinction, if I have to resort to the plea that though a sentence appears to have changed its analytic status, it is really only that P has adopted a different language. I can hear someone saying that this is an awfully fine point on which to rest such a case. My response to these doubts is however, that fine though our point may be, the fineness of it in no way detracts from its force. And the force of the point comes from the fact that anyway you look at it, the "meanings" of the expressions of a language are governed by the rules formulated in the metalanguage; the meaning postulates precipitate out of the metalanguage. If of two metalinguistic systems one sanctions the use of a certain meaning postulate while

 2 And this is assuming that P is not a person we would ordinarily describe as being bilingual.

the other has no such meaning postulate, we are totally justified in concluding that these two <u>distinct</u> metalinguistic systems define two <u>distinct</u> object languages. And it makes no sense to say that a sentence which occurs in one of the object languages is identical with a sentence which occurs in the other, since a sentance can only be defined with respect to the language in which it occurs.

Let us illustrate this point by an example. Suppose a lexicographer is studying a community of speakers (which, for convenience sake, we shall call S) in an attempt to determine which statements for these speakers are analytic and which synthetic. Now to determine which statements are analytic and which synthetic for S at t1, the lexicographer proceeds by the method outlined at the end of Chapter III. That is, the lexicographer puts the appropriate questions to S and takes note of their verbal responses in order to determine the intension of any given predicate term which S uses. Thus the lexicographer can find out the general condition any entity must meet in order for S to be willing to apply a particular predicate to that entity. Now let us suppose further that at t_1 , on the basis of the linguistic responses of S, it is established that in order for S to apply the term 'Raven' to any object, that object must be, among other things, black. The lexicographer will note then in his description of the language of S (Ls) that the statement

(1) All ravens are black.

is analytic, and since the sentence is descriptively analytic, i.e., one or more of its descriptive terms occur essentially, the meaning

relationship of the predicate terms 'raven' and 'black' will have to be described also. So the lexicographer, accordingly, records that for S at t_1 the sentence

(2) $(x)(Rx \supset Bx)_{o}$

is a meaning postulate, i.e., (in the language of <u>Meaning and Neccessity</u>) it holds for all state-descriptions or, in other words, its necessity is simply a consequence of its metalinguistic translation which tells us that (2) is true.

After asking many more questions and noting the responses of S, our lexicographer finally arrives at a fairly complete description of L_s at t₁. This description, let us not forget, includes, among other things, the sentence (2) as a meaning postulate. Now we may remark here that there are at least two ways in which the sentence(1) has come to be analytic for S at t_1 . It may be that S has a prior conception of what it means to be a raven in much the same way that we have a prior conception of what it means to be a unicorn. That is, prior to our ever having seen anything which we would call a unicorn, we know that if we were to call something a unicorn, it would have to have one horn in the middle of its forehead. Similarly S may know or have known prior to ever having seen a raven, that if anything is correctly to be called a raven, it must be black. Undoubtedly, this is the ground for a great many statements being held true "come what may," however, it is not the reason when it comes to the analyticity of other statements. It may be, and is more likely, that S had no idea of what a raven is prior to seeing one, 1.e., S had no prior conception at all of even the accidental properties, let alone the

essential ones, of a raven. But at some moment or othe, S discovered this particular kind of object which was not altogether like anything it had seen before and so S decided to use a tern which was not applied to any other kind of object. Hence, S used the term 'raven' to refer to entities of such a kind. Now S may not have been very interested in the first ravens they saw; not even interested enough to notice that all the ones they had seen were black in color. But gradually S became aware of the fact that every raven which it had observed was black; the blackness of the ravens was indeed one of the few properties which was common to all ravens. And so, that property became so closely associated with ravenhood that (1), consciously or not, "became" analytic. Or, to be more precise, S adopted a language, consciously or not, in which (1) is analytic.

Now let us suppose that one day S came across an object, call it y, which was like a raven in all the essential respects except that it was not black; rather it was brown. Except for the color though, anything which could be said about a raven could be said about y. The problem then for S, is what are they to call y? It seems as if there are two alternatives open to S. Either S can stand by its previous convention signified by (2) and classify y in a different category as an object which is very near to being a raven but not quite, or S can abandon (2) and decide that y should be classified along with other ravens. If 5 chooses the latter alternative, S is conceding that the property of being black is not essential to being a raven, and so they will have no use for a language in which (1) is analytic.

It so happens that S is not particularly prone to standing by its conventions, and so (2) is given up as a meaning postulate. But to give up (2) is to give up a language, viz., that language in which (2) is a meaning postulate. If our lexicographer returns to study L_S once again and, after putting questions to and receiving responses from S, finds that (1) is not analytic, his completed description of L_S at t2 will not include (2) among the meaning postulates. This is to say that he will have described a different language. Hence, though (1) is analytic for S at t_1 and synthetic at t_2 , we have no justification to conclude that (1) at t_1 is identical to (1) at t_2 . They are sentences of different languages just as the sentence "ich bin krank" in German is different, i.e., not identical, with the sentence "Snow is white " in English. I hope then that we are clear as to why a statement can never become analytic or become synthetic.³

Now the charge has been made that the choice of meaning postulates is arbitrary.⁴ This charge, though, is only partially valid. Indeed, the choice of meaning postulates can and may be arbitrary if we are constructing a language solely for the purposes of a semantical investigation. In such a case we are free to choose any meaning postulates at all only on the condition that their adoption

³It should go almost without saying that L_S before S discovered ravens was also different than L_S at t1, since at that time (1) was not a sentence at all for S.

⁴See N.L.Wilson, The Concept of Language p.132. "Where do these meaning postulates come from? Do we just pull them out of the air?" What makes Wilson's charge particularly strange is that on the same page he seems to approve of Carnap's method for the behavioristic testing of sentences to determine whether or not they are analytic. Actually, this is the same method which would be employed to determine which are the meaning postulates for a speaker.

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does not result in an out and out contradiction. Of course, we must forfeit this freedom if our formalization of a language is to be presented as an accurate description of a language already in use. But to forfeit our freedom, in this respect, means that our selection of meaning postulates cannot be arbitrary. For example, in our hypothetical case presented above, I don't see how we can call the choice of (2) arbitrary. Our lexicographer has come to select (2) only after many hours of hard work observing the responses of S to questions which he has carefully thought out and put to S. And so, his choice of <u>that</u> meaning postulate is really no more arbitrary than is the decision of a tourist visiting Germany to translate 'Wasser' into the English word 'water'.

To conclude, let me give a short summary of the conclusions I have come to in light of the previous chapters. First, and most important, there is an analytic-synthetic distinction. This distinction reflects the difference between sentences which are true solely in virtue of the semantical rules (and since the semantical rules, along with meaning postulates, specify the meanings of the primitive descriptive words, it is correct to say that these sentences are true solely in virtue of the meanings of their terms) and sentences which are true partly because the world is the way it is, i.e., the truth of these latter sentences depends not only upon their meaning, but also upon the facts being what they are. And though it is true that we cannot, as of . yet anyway, provide a general definition of analyticity, as 'analytic'can only be defined with respect to a given language, this in no way shows

that the analytic-synthetic distinction is a myth. For all the other semantical terms, which are defined in such a way that the doubleelimination difficulty is not avoided, are in the same need of a general definition.⁵

What Quine seems to be saying then, though I think he could have been clearer in saying it, is not somuch that there is no analyticsynthetic distinction, but rather that there is no sure way of determining, for a speaker, what sentences are analytic and what sentences are synthetic. In other words, Quine believes that it is not possible to provide behavioristic criteria, if no definitions, for the testing of semantical concepts such as, e.g., analyticity and synonymy. I think, however, that though Quine is probably pointing to the weakest spot in semiotic, in his demand for befavioristic criteria of semantical notions, he is not justified in believing that the task of providing these criteria can not be fulfilled. For I think that Carnap has outlined

⁵We should point out here that apparently wilson has, in The Concept of Language, provided us with general definitions for a number of semantical terms not including 'analytic'. He does this by defining his language without using semantic terms, thus avoiding the double elimination difficulty. However, because he is evidently suspicious of meaning postulates, he did not attempt to incorporate them into his system. And since meaning postulates, or something like them, seem to be essential in order to specify the meaning relationships of different primitive terms, it is not surprising to find that no general definition of 'analytic' is forthcoming from Wilson, Wilson's suspicion of meaning postulates arises primarily from the fact that we have no "requirements, comparable to the completeness and consistency requirements for ldgic" (p.132), for them. It is significant though, that Wilson does not conclude that there is no analytic-synthetic distinction, but rather that something is wrong with designation rules;

a proceedure which, even if it is not developed in enough detail, is at least a step in the right direction towards a clear pragmatic criterion. And though the technical difficulties are enormous which stand in the way of correctly applying such a method as Carmap has outlined, there is nothing, in principle, which should keep us from carrying it through. Thus, I think Quine's main contribution in coming out against the analytic-synthetic distinction is neither his belief that the distinction is a myth nor his proposal that we accept the gradulist program; rather it is the effect his attacks have had in spurring out of complacency all those philosophers of language who mistakenly thought they had a sound pragmatic foundation for descriptive semantics.

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