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WINDOWS ON SCIENCE

1. How Science and Myth and Fiction Step beyond the Actual, and

2. Science and Science

RICHARD SYLVAN

WINDOWS ON SCIENCE

1. HOW SCIENCE AND MYTH AND FICTION STEP BEYOND THE ACTUAL, AND SOMETIMES BEYOND THE POSSIBLE

AND

2. SCIENCE AND SCIENCE: RELOCATING STOVE AND THE MODERN IRRATIONALISTS

by

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WINDOWS ON SCIENCE

PREFACE

What this WINDOWS series is intended to do is to put together material elaborating a noneist view ON SCIENCE, that is, a view set within the universal theory of objects which is nonreductionist as well as nonextensional and nonexistential. Some of the main elements of such a noneist philosophy of science have already been presented elsewhere (especially in *Exploring Meinong's Jungle and Beyond*, chapters 10 and 11); but those presentations, which remain somewhat inaccessible, leave much to be done. Some of the further doing will be done in this series.

HOW SCIENCE AND MYTH AND FICTION STEP BEYOND THE ACTUAL

ABSTRACT[1]

Sistology, the general investigation of all items, is explained. It is contrasted with its dwarf subtheory, ontology, which is restricted to existent objects. Sistology cannot be recovered from ontology. Sistology, unlike ontology, assigns nonexistent and impossible objects standing. How this is accomplished logically is outlined; and resulting advantages for linguistics, language and literature are indicated. The theory is then applied to a comparison, in depth, of (failed) science with fiction.

Science and fiction are much more alike than generally supposed. Indeed they do not differ essentially in syntactical ways, and overlap significantly in content. (While they do differ more significantly on technological applications, that is frequently not to science's credit.) To force what differences there are, a detailed characterisation of science is presented, which is then compared point by point with fiction. What emerges is that science and fiction form merging families, with deductive closure and qualified confrontation with experience affording main separation features.

What remains of the gap between science and fiction is bridged by myth, which often provides other cultures' versions of science. Leading features of myth are explained and two styles of myth roughly delineated, anthropic myths and naturalistic myths. The latter merge with failed science.

The resemblances are exploited both in criticizing fashionable accounts of scientific theories and scientific explanation, and in offering new accounts. A scientific theory is a story, which is closed, in particular under deduction, and which suffers qualified exposure to experience, that is, which meets reality requirements. Scientific explanation also fits within the story setting, a covering story replacing the orthodox "covering law" model. The account accommodates what is crucial in much explanation, explanation of what exists by way of what does not exist.

Science is integrally involved not merely with what does not exist, but also with what is impossible. The role of inconsistent theories and other inconsistent objects is indicated, and the emerging theory is applied to certain psychological puzzles concerning thinking the impossible.

[1] This paper is to appear in German, in an issue of Zeitschrift für Semiotik entitled 'Zeichen und Fiktion'

HOW SCIENCE AND MYTH AND FICTION STEP BEYOND THE ACTUAL AND SOMETIMES BEYOND THE POSSIBLE

§0. The framework stretches beyond ontology, to the far reaches of sistology. Sistology is the general study of items in general – whether these things are existent or not, possible or not, universal or particular, dynamic (such as processes and actions) or static, and so on. In theories of items and objects, sistology replaces ontology, which is the study (only) of what exists. Ontology is of course a branch of sistology, but a rather small branch given that many items do not exist. As much, Meinong already said in the object-theory of his later philosophical work, where perhaps the first comprehensive beginning was made on sistology. In fact, most objects do not exist: other worlds, fictional characters, ideal scientific objects, transfinite and infinitesimal numbers, etc. etc., do not exist. Yet most philosophy, ontology-bound, attempts the heroic task of trying to take account of all those (uncountably many) nonexistent objects through the comparatively few (countable many) objects that do exist. Dimensional considerations alone indicate the task is doomed to fail. (The misguided idea of stretching existence founders, among other places, against rock of contradictory objects.)

Sistology, unlike ontology, treats all items as having standing. Nonexistent objects are treated as having standing, of logical and other types, in their own right; they are not refused admission, or removed from view, for instance through large reduction programs, as happens in standard ontology. The familiar discrimination, characteristic of mainstream philosophical positions, directed against nonexistent objects, and most intensely towards impossible items, can be, and is, removed. Like many other forms of discrimination, it is without basis. Yet much of the history of metaphysics can be read as a long history of such discrimination against

nonexistent items.

There is only one, elementary, strategic move required to get sistology started, as a much broader investigation than ontology: and that is the "trick" of granting the obvious, that nonexistent things each have a range of properties, that much is true of what does not exist. Instead then of the massive reduction program confronting any metaphysical theory wedged within ontology, of all those apparent subjects of discourse about what does not exist, there is a much more straightforward task: namely, that of ascertaining which properties nonexistent items do have, what they are <u>like</u>, in all their rich and distinctive diversity.

To avoid the collapse of sistology into ontology, standard logical theory has of course to be modified, since the standard theory incorporates the ontological assumption, that only existent items have genuine properties.[1] Fortunately the first stage of modification of standard logic, enough to get sistological programs substantially under way, can be comparatively slight, and as far as quantification theory (i.e. pure first order logic) is concerned need only involve reinterpretation, that is, the initial changes can be purely semantical. The adjustment called for is essentially a matter of neutral reinterpretation of subject-predicate expressions and of introduction of neutral quantifiers. Subject-predicate (sp) forms such as 'Sherlock Holmes smoked a pipe' and 'the Kinase cascade explains characteristics of malignant cells' do not presuppose (for their truth) that Sherlock Holmes and the Kinase cascade exist. Moreover such judgements entail that, for some x, xp; for example, that Sherlock Holmes does not exist entails that some item does not exist. Neutral quantifiers, like 'for some', replace the standard

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1. Meinong, writing before the rise of modern symbolic logic, did not encounter this logical obstacle. The then prevailing logical paradigm, Aristotelian syllogistic theory, readily admits of, and was sometimes given, ontically neutral interpretation.

   The required stages of adjustment of standard logical theory are worked through in detail in the text JB, which provides the background for this paper.

existentially-loaded quantifiers, such as 'there exists', which can now be defined as 'for some existing', using the predicate 'E' for existence.

To allow however proper logical representation of incomplete objects, such as nonexistent items typically are, and of inconsistent objects, further adjustments to this (ontically) neutral logic are required. The next stage, still within the confines of standard logic neutrally reinterpreted, adds predicate negation, or an equivalent, to quantification logic. No new negation symbol is required (for basic purposes), but s~p, where negation has a predicate occurrence, is a well-formed expression along with the usual ~sp, where negation has a sentential occurrence. Familiar natural language forms of incompleteness and inconsistency can then be represented. For instance, neither K~bald nor K bald is true, where K is the present king of France, so K is incomplete as regards baldness (and, since this is an extensional feature, K is an incomplete object). And both m round and m~round are true, where m is Meinong's round square, whence m is inconsistent as regards roundness (and, since the predicate '(is) round' is extensional or local-worldly, m is an inconsistent object). Of course m~round does not entail ~m round, else inconsistency would infect the applied logical theory, and collapse to triviality would ensue by virtue of the spread features (especially the inference, ex falso quodlibet) of standard logic.[2]

But apparently contradictory objects like Meinong's round square are not explicitly contradictory; and no representation is provided for totally contradictory items, such as the thing (m say) that is round and such that it is not not the case that it is round. Encompassing such items motivates a further major stage of logical development, namely the shift to a paraconsistent logical base, where fully contradictory objects can be

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2. This sketches the core suggestions, for standard logical representation of a theory of objects, of my 'Exploring Meinong's Jungle', University of New England, cyclostyled, 1967 (later absorbed in JB).

non trivially represented. In such a paraconsistent setting (where the inference ex falso quodlibet fails), fully contradictory objects are indeed contradictory; for example, $m^{\dagger}$round and $\sim m^{\dagger}$round. Similarly, the Russell set, the set of all non-self-membered sets, is both self-membered and not such that it is self-membered; but the logical theory does not collapse. Like natural language the matching logical theory absorbs isolated chains of inconsistency without catastrophe (see OP).

It should be already evident that sistology affords a fresh and interesting approach to many problems in linguistics, and especially to issues concerning fictional and theoretical discourse.[3] By admitting linguistic subjects such as 'Sherlock Holmes' and 'a tachyon', which are about objects that do not or may not exist, as perfectly good (logical) subjects, not in need of syntactical analysis or elimination in some "deeper" deformed structure, a great deal of problematic reductive analysis is swept away. Therewith several objections to the representation of natural languages through logical languages[4] are also removed.

The advantages for linguistics flow, more generally, from three sources (with underground connections):-

1. Neutral logic is much closer to ordinary discourse than standard highly regimented logic. As a result many tortuous and dubious analyses of linguistic constructions into standard logical forms are shortcut or avoided. Correspondingly also, there are fewer prospects of going wrong in logical analysis, as logical analyses remain much nearer the surface.
2. A good deal of discourse which cannot be represented at all, or at all

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3. Sistology also offers a fresh perspective on many traditional philosophical problems. But naturally the proof of this lies in the doing, in things, done elsewhere (especially in JB or, a bit differently, in Parsons 80 or Zalta 83). Although sistology is very much theoretical ground, it corresponds to the pulse of much ordinary discourse. Ordinary people uncorrupted by philosophy tend to operate in any case with object-theory, and commonsense accords with object-theory. But mainstream philosophy has generally operated with a much narrower paradigm, finding difficulty with all discourse that is not referential and with everything that cannot be squeezed within ontology.

4. For instance, those advanced by Moravcsik.

satisfactorily, in standard logics, especially highly intensional discourse, can be readily handled in neutral (λ-categorial) logic.

3. Many types of inconsistent and radically incomplete theories - of belief, desires, etc. - held by ordinary speakers that cannot be accommodated standardly, can be represented in neutral theory.

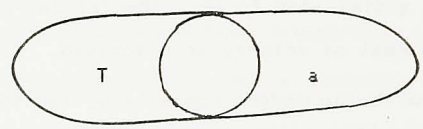
Some of these advantages will be illustrated. For, in what follows, a new look is taken, from the viewpoint of sistology, at fiction and, what closely resembles it and is often assimilated to it, theoretical science, especially <u>failed</u> science, which is most of science as practised. For, though also-rans are often lost sight of with the modern competitive fad for exalting winners and success stories, most scientific theories are (potential) failures; in a field of twenty-three, or a thousand, only one starter in general will win.

Both science and fiction operate essentially with nonexistent objects, often in nonexistent worlds. Theoretical science has as its focus (mathematically-tractable) ideal or perfect objects which remove the complexities and "imperfections" of real world objects (e.g. ideal gases, perfect spheres, inertial frames, electrons with precise unitary charge, etc.), while fiction focusses on characters removed from much of the messiness, irrelevance, and triviality of actual day-to-day living. Though some of what we say concerning these characters and these idealisations is true, the detailed stories concerning them tell of happenings in other worlds beyond the actual. Understanding science and fiction then involves nonexistent objects in <u>two</u> ways: first, as regards what they are frequently about, what their subjects signify, and secondly, as to what their statements hold at and with respect to what their truth-values are assessed.

In short, both fiction and science, and especially unsuccessful science, go beyond what exists, beyond the narrow domain of the actual world, and in two similar ways:- Both are much concerned about things that do not exist. And both are other-worldly, being concerned with the structure of other

imaginatively-accessible worlds. Several of the striking similarities between science and fiction are grounded in these shared semantic features.

The common semantical structure of science and fiction is more clearly grasped by comparing a clearly failed scientific theory with a work of pure fiction. Both tell stories about other worlds (single world a, say, in the simplest case), but worlds that overlap the actual world T, since some statements of the theory or fictional work are true, for instance, those characterising objects of the theory or fiction.



A similar diagram, of overlapping classes, applies to the respective domains of objects. The domain of items, of the fictional work (i.e. d(a)) overlaps the domain of items of world T, but none of the entities of the domain of T belong also to the domain of a, since the work is one of pure fiction (i.e. in symbols, d(a) overlaps d(T)-e(T), where e(T) comprises the existent objects). Now what happens in the special case where a scientific theory has not failed, but is true, is that world a does not merely overlap T but is contained in it. Similarly, then, the individual objects the theory takes to exist do exist (i.e. $e(a) \subseteq e(T)$).

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5. (from previous page) For fiction both these ways are ordinarily taken for granted, though mainstream philosophers have great difficulty in trying to account for these features and reconciling their theories with them. As regards science, the case, at bottom commonsensical, is argued in JB, especially chapter 11.

   The theory of fiction presupposed is also basically a commonsense position, an integrated theory which takes most ordinary claims concerning fiction and fictional characters at face value. The integration is achieved by way of context. There are two main types of context involved, actual world (T) contexts, and contexts which transfer evaluation to the worlds of the works of fiction. In actual world contexts (as opposed to their referential contractions), fictional objects have their ordinary (characterising) features, but not all the features attributed to them by their source, by any means; but in appropriate fictional worlds they do have all the features their source supplies. For fuller explanation and some refinement of this simple integrated picture, see JB, chapter 7 (especially the summary, p.595ff.).

§1. Science is more like than unlike fiction. Science is much more like fiction than most philosophers and scientists would care to acknowledge. And what distance there is between these two forms of discourse is evidently bridged by science-fiction, with scientifically-informed science-fiction often grading into popular science on one side of the sci-fi rainbow. But science-fiction is not the only bridge, only the most obvious one. Another historically significant bridge is myth, which merges into fiction, but from which (as many have recently emphasized) scientific theories have condensed. In myth, as in science, stories are made up or elaborated to account for natural phenomena. In science these stories, though often but sophisticated folk-lore, are elevated to the status of theories.

Some of the most obvious similarities of science and fiction begin from the respective products, which in both cases are predominantly propositional, and presented through a linguistic medium. Bookstores typically stock the tangible products of both, books, sometimes shelved under the respective headings SCIENCE and FICTION, both shelvings perhaps conveniently merging through SCI-FI. Anyone who has looked hard at bookshop and library classifications of subjects will appreciate how arbitrary and artificial these divisions are. To be sure, there are often differences in the format of products, but none appear essential. For example, science texts may contain exercises and problems for the reader, but then so could detective novels. An ideal novel will have a plot, a theme, realistic characters, and so on (as Forster's Aspects of the Novel explains); but then so may an entertaining science book, with the heroes real-life scientists. In this genre, for instance, belongs to Watson's story of the discovery of the double helix structure of DNA, a happening which incidentally helped make plain to a wider public what a five- ring circus mainstream science is. And so on, for other similarities.

What differences remain in the respective products can be further reduced

by lumping together with fiction the partly parasitic enterprise of literary criticism, or by considering more sweepingly the whole of literature, both the object works and critical (meta-)works. The combined product then includes text-books, books with statistical data or surveys, even books with mathematical modellings. Thus, in principle, nothing need differ in the respective formats of science and fiction. The differences, such as they are, are not syntactical in kind, but must lie elsewhere, in content and application.

The differences in content of science and fiction are not so dramatic as has been supposed either. A main reason for this is that both scientific theorizing and literary imagining are other-worldly activities, involving investigation of (strictly limited) worlds beyond the actual. This manifests itself in two apparently less obvious, but very important, similarities between science and fiction. One concerns what the respective stories of science and fiction are about. Science is bound to try to tell a story bearing on some parts of the natural world; fiction is not so bound, though often works of fiction will conform to such a broad requirement. But both will commonly present their stories by talking about and appealing to what does not exist: this is the <u>insistential</u> (or nonexistential) <u>feature</u>. The other similarity turns on the <u>intensional character</u> of the stories involved, what is often put down misleadingly as the conjectural or make-believe character of the stories. The stories or theories concern not the messy real world of everyday complexity, but situations distinct from actual ones – other worlds – that only resemble parts of the real world in respects deemed relevant.

While these similarities in elements of content are important in bringing out features of science that are commonly neglected, features that we shall want to return to consider further, they are not much help in separating science from fiction. Interestingly, to see bigger differences it is to

applications that we must divert, to the involvement of science in industry, in the subjugation of "less advanced" peoples, and in the domination of nature. While much literature may foster similar ideas, and describe things even worse, fiction never gave us the B-52 or the Bulldozer or the Bomb, or the (theoretical) blueprints for them. Even so, much science, like most fiction, has no such application; and there is an ambivalent attitude to the connection of applications with science, especially in the case of purer science. For, on the one side, science resents being dirtied by the applications it has; on the other, applications are a route to money and power.

The upshot is that to reveal the differences between science and fiction - that we all tend to expect, though often we expect to be too easily won - a rather longer and more careful comparison will have to be undertaken. The larger route has the advantage, however, of bringing out significant features of both science and fiction, and rather different and more satisfying theories of both than those to be encountered in the orthodox literature.

Elsewhere, science has been characterised as a study or investigation of a certain sort, distinguished, so far as it is, by its methods - experimental and methodical, and restrictive in the arguments and systematisation permitted - and by its focus - on nature, or more generously, on parts of the actual (normally external) world - and by its mode of presentation - as approximating truth, of giving an account of how (more or less) things really are - and by its apparent goals - truth, explanation and the like.[6] Fiction tends to differ from science on every (italicized) count, though again it is fair to say that the differences have often been exaggerated, and that on each count there are cases and examples from both sides, science and fiction, which begin to bridge the differences. Consider the counts in order.

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6. This characterisation, which reflects that in the literature, is drawn from Sylvan 84.

Firstly, fiction, by contrast with literary criticism, is not really a study of any kind; it is not serious or systematic in that sort of way. Much of it is rather entertainment, rather leisure-time activity than work. However, select groups of fictional works can legitimately be regarded as studies, of the English industrial revolution, of schizoid characters, of grand tragedy in human life, of utopias, and so on. For some of the readers, reading these works will be work, though, seen from the authors' viewpoint, the works have (mostly) not been produced for this sort of objective, or with such a study group envisaged. Moreover, some works of fiction are undoubtedly studies of certain sorts (and might as well be, or are, subtitled as such), especially in the area of social, psychological, or historical studies.

The second contrasting count, method, is more important, though again the contrast can be readily eroded (indeed into no difference at all if we were to take seriously Feyerabend's theme that there are no distinctive methods in science, that anything goes). Of the elements in method, two especially should be singled out in making the requisite contrast with fiction; firstly, the critical, argumentative, commonly deductive, character of science and scientific stories, and, secondly, the empirical control of science and the downgrading or ditching of scientific stories where they do not measure up against recognised empirical testing. Of course what the successors of Popper have rightly emphasized is the extent to which highly approved and widely received scientific stories evade or resist empirical testing and unfavourable results ensuing therefrom, and the extent to which grander scientific theories, such as evolution, resemble ideology, or better mythology. But, despite the erosion of differences in this fashion, it remains true that scientific stories are expected to conform to empirical testing, to be open to quantified falsification to an extent to which literary and fictional stories are not.[7] What would it be like to falsify, or to try to falsify, some piece

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7. Popper's attempt to reduce the whole complex fit of scientific theory against refined experience to falsifiability, is just much too simplistic. The more complex relations of theory to empirical evidence including those of qualified falsifiability - have yet to be adequately described.

of fiction, e.g. Lord of the Rings? So, even if we remain disturbingly vague about what being empirical or satisfying empirical testing amounts to, still there is a solid difference here in that there are tests supposed to apply to scientific theories, which are irrelevant or don't even make much sense for fictional stories.[8]

Nowhere are these methodological differences better manifested than in the widespread phenomena of scientific fraud and deception. What is the main method of fiction, fictionalizing, becomes in science, fraud or the like. This is the fate of a theory like the appealing kinase cascade theory of cancer, where some of the crucial data supporting the theory were simply made up in the style of fiction or science-fiction.[9]

As to the third count, fiction is much more comprehensive than science. The focus of fictional work can be on nature, commonly is on what is taken to be human nature; yet, unlike science, it does not have to be so focussed, it can be on something quite remote and very otherworldly. Doesn't fiction claim not to be about the world? Not always: realistic and historical fiction are often very much about phases of the world. Doesn't fiction mean, at least in origin, if not make-believe, made-up? Perhaps something like this is so. Fiction is not constrained by requirements of truth, by how things are, by the ways of the actual world, as science (and also philosophy) to some extent is. But scientific theories too are, so to say, made-up; scientific theorizing, or fictionalising, is an intensional activity, though again one with many more constraints than fiction proper. And a fictional story, though "made-up" may

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8. Of course there is something of a play-off here with the first count; namely, insofar as a piece of fiction also purports to be a study, so it is open to empirical checks. If it fails, we can say, for instance, 'Well it may be a gripping story, but it doesn't represent the actual situation with so-and-so satisfactorily, and gives only a distorted and misleading picture of such and such'.

9. On this example of scientific deception, and also more generally on the extent of fraud in science, see Broad and Wade. The matter of literary fraud, which concerns authority and authenticity, is for the most part different.

give a good account of, or good insight into, how things are with part of the actual world, especially with people.[10]

The fourth count may look decisive, since the mode of presentation of mainstream science, as approximating truth or making some claim to approach the truth or how things are, differs from that of usual fiction, which does not claim to tell a true story, or to say, except obliquely, how natural things really are. Again appearances are a bit deceptive. Historical fiction may present, or claim to present, a true or reliable picture of how things were in a given past period. And once literature and the surrounds of fiction such as myth are admitted, any attempt to distinguish science just through its mode of presentation becomes entirely biassed. Myth, for instance, does pretend to offer some account of the way things are, and why, though the mode is typically historical.

It may be thought that the fifth count clinches the differences, but such an assumption would again be wrong. People's goals in writing fiction and cooking-up scientific stories - explanations, theories, hypotheses, etc. - are decidedly various, especially when personal motivation is duly taken into account. The goals include self-advancement, status, increased or continued income or grants, and the like. A goal formerly suggested as distinctive of the (young) scientist, wonder, comes well down the list, and for many professionals hardly figures at all. But what about motivation for the whole enterprise, science or fiction respectively? In fact the main motivation for science now appears to be that of applications, through applied science, especially military but also industrial applications. As observed, this certainly helps distinguish science from fiction, which has no such awful, or sometimes worthwhile, (by-)products; but again it is not the way most

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10. Most of literature is conspicuously human chauvinistic, and concerned with the celebration of things human, to which the natural world is a mere backdrop. Sometimes even, as with Hollywood scripts, natural things virtually disappear from sight, under layers of human artifice.

scientists would like to see their enterprise distinguished. Moreover it only helps in demarcation, and will hardly serve as the distinguishing criterion, because much of science, especially of pure science, lacks any very direct application or even, whatever might be said in grant applications, any application. Even here science merges into science fiction (which can have indirect application of a sort).

It remains to consider the noble goals, truth and the like, regrettably often lost sight of in dirty industrial science. A good deal of fiction does aim for a certain authenticity, and more serious science fiction tries for and sometimes acheives a depth of psychological insight which social sciences seldom manage. But fiction seldom aims for explanation of natural phenomena in the way or with the persistence that "better" science does, though again nothing stops some fiction from doing some of this. It is however in the vicinity of these more noble goals that importance differences are reflected. Achievement in particular is different, a thing easily brought out by holistic application of semantic terms. A work of fiction, for instance a novel, is not applauded, when it is, as true or false; but a successful theory can hardly be downright false. While individual statements about Mr Pickwick may be true or false, The Pickwick Papers does not even aspire to truth; nor would it be dismissed as false, unless by a mainstream philosopher or a someone who mistook it for history. But though most of them are false, worked-out scientific theories have normally aspired, in their active phases, to truth or to approximating truth. This takes the matter of noble goals back to the mode of presentation: these counts tend to coalese.

What emerges, then, is that science and fiction are not sharply separated, but form merging families. However the significant features of science discerned, when taken together, do demarcate the main body of science from that of fiction. It is amusing that recent anti-realist positions concerning science try to dissolve the very features that

distinguish science from fiction; in particular these positions bizarrely attempt to dissolve the linkages of science with truth and existence, with the real empirical world.

§ 2. Exploiting the resemblances in theories of science and fiction. Though they diverge in the ways explained, science and fiction have, quite enough in common for the philosophical investigation of each to be very illuminating as regards the other. Indeed to go further (beyond this bit of Forties' account of science than that delivered by the usual accounts,which have, by and large, erroneously looked to more exact practices such as mathematics and logic for illuminating accounts.

Consider, for example, the matter of scientific theories. Positivistically-inclined theorists typically tried, and still try, to explain scientific theories as deductive theories of mathematical type, or, since the theories mostly failed to measure up to the standards prescribed, as degenerate members of the deductive type. Such was the usual view of theories of those preaching the restricted hypothetico-deductive method in science. It is curious, given the breakdown of such an account by way of deductive theories (which can be sloppy enough, e.g. as regards underlying logic), that repairs favoured by formalistically-inclined philosophers involved even more exact structures; namely, in syntactic terms, formal systems, or, differently, in semantic terms, models. However the problem lay not with insufficient exactness in explication, but with too much! Scientific theories, especially in the "softer" sciences, do not display an exactness even approaching that of deductive theories. Too often the paradigm of sciences has been physics, which is hardly representative; and, in any case, the live theories of physics are very far from assuming formal system form . Moreover, in physics and elsewhere in science, trying to force theories into this sort of canonical, strait-jacketed, form may involve quite a struggle and much original work; so

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11. The comparison and contrast method used reflects that of Wisdom, rather than Wittgenstein. But the family notion deployed derives from Wittgenstein.

the strait-jacketed result, if obtained, will not be the same as the original theory, which was representative of the object under investigation.

The recent semantical replacement for (consistent) formal systems is, of course, models. However the fashionable proposition that theories are models or sets of models is strictly nonsensical because it identifies items of quite different types – stories, which are propositional, with classes of set-theoretical objects, which are not propositional. The fashionable proposition is a confused empiricist way of getting at the point that theories hold at worlds beyond the actual. Holding at worlds becomes truth in models, and the theme of theories being true in models is then contracted to the misleading slogan that theories are models, since given a model, what is true at it is determined (usually recursively). From an object-theory viewpoint, these conversions hardly represent an improvement, for two important reasons:- Firstly, sets and models do not exist any more than the possible worlds they are surrogates for, despite the empiricist fiction that these objects do exist (JB, p.735ff.). Secondly, models (like standard possible worlds) will only serve to represent consistent and complete theories; but theories, like stories, are characteristically incomplete, and often inconsistent. The shift from models to sets of models does something to avoid these failings (and others)[12], but does not escape the objections that sets of models are decidedly limited representations (by no means the same as the objects under investigation) and that only consistent theories are represented. Yet significant scientific theories have been, at important stages in their histories, inconsistent (a fact much emphasized recently by the London LSE School).

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12. Such a shift is made by van Fraassen and other earlier empiricists he cites, p.64ff. An analogous shift is made in attempts to obtain possible world representation of propositional attitudes, as e.g. in Cresswell. The more primitive thesis that theories are (represented) by models simpliciter (at least in the case of fundamental theories) may be found in Cartwright, p.10.

No, like fiction, a theory is also a story - a scientific story where the theory is scientific, of course. A story is a connected, and usually integrated, set of statements; too often, when theories are defined in logic, the important, but difficult-to-specify, requirement of connectedness is left out. Furthermore, a story gives an account of something. Scientific stories, or theories, are distinguished in two ways.

The first thing that distinguishes the stories of science is that they are closed under various requirements, in particular deduction. Here is what is right about the attempt to transform scientific theories into axiomatic deductive form, namely that they are deductively closed stories (by contrast fictional stories and worlds are not in general so closed: see JB, p.547, p.551). The fact that a theory is deductively closed does not imply, however, that it admits of precise (or easy) formulation; that is, deductive closure does not guarantee the positivist perception of scientific theories as exact deductive systems. For the assumptions may be inexact and indeterminate in a range of ways. From the story made up, for example, by a social scientist, to account for this or that, it may be difficult to extract even the leading assumptions; that may be something for a later and more refined stage of theorizing. Not for nothing was it sometimes said that the positivists captured theories in their final stages, or even dead, after rigor mortis had set in.

Scientific theories are often vague and messy in their earlier formulations, so that someone outside a given theory, or a formaliser, may have to study several different presentations in order to winkle out, if it can be done, the assumptions, even the leading ones (that condense into axioms). In this respect science, especially early science, again parallels parts of fiction, and especially myth. In each case outsiders may work - may have to work - through several presentations in order to discover the central claims of a putative science, such as biodynamics, or to determine the core

of a myth and its variations and embroidery (consider, e.g., the problem with the Sisyphus myth of determining whether Sisyphus rolled the stone right up to the top of the hill or just short of the top). As it is with Sisyphus and the Faerie Queen, so it is with ideal observers, rational economic man, gravity, etc.

The second thing that distinguishes the stories of science is that they are subject, to some extent, to checks and tests, such as qualified falsifibility. This feature derives from the requirement that a scientific story gives an account of something in the (natural) actual world (even if it appeals to other worlds in doing so). Empirical testing in what ties the account to this world. Fiction does not suffer such qualified exposure to experience.

From these two reality requirements, strong inferential closure and sensitivity to empirical truth, flow other important features, distancing theories and theoretical objects from fictions and fictional objects. The further differences concern, in particular, the extent to which the standard logic of relations can be applied (after neutralisation), for instance the legitimacy of such relational shuffles as passive conversion, replacement of extensional identicals, etc. The standard (neutral) theory turns out to be a special case; though appropriate for objects that meet the reality requirements cited, it does not apply generally, in particular to fictional objects, which conform to a more general logic of (reduced) relations. Many of the familiar objections to straightforward theories of fiction depend in fact on applying the restricted standard logic to fictional objects, on the mistake, that is, of treating fictions as if they were real. Fictionalism, according to which scientific theories are fictions, makes the reverse mistake, of treating scientific theories and theoretical objects as if, like fictional analogues, they were immune to reality requirements and what follows from them.[13] Nearer the mark would have been the theme that science is

a mythology.

§ 3. Bridging the gap, through myth. Much closer to science than most fiction is myth. Though comparisons of science with pure fiction do reveal significant divergence, the differences begin to evaporate when science is compared with myth. Myth, like science, and unlike fiction, often purports to be more or less true. (Of course, though a work of fiction as a whole does not usually pretend to truth, many statements in such a work may be true.) And it takes the consequences of this, such as deductive closure and a certain exposure to experience. Furthermore, unlike pure fiction (where virtually no-one is an insider), many people adhere to myths, instead of, or as well as, received science. Indeed for other cultures myth is commonly the analogue of science an analogue science tends to displace upon cultural contact, because of some impressive technological applications and much accompanying propaganda or force. While myth sometimes damagingly mixes together science and religion, the same held, at least until recently, for science; and parts of science are more like a religion, or a substitute for religion, than is mostly admitted. Consider, for instance, the roles of evolutionary theory, 19thC mechanism, and 20thC economics. (And certainly in much philosophy of science the attitude to science is aptly described as fundamentalist: cf. Feyerabend). In modern times scientific theories have increasingly substituted for religious accounts; even so leading scientific theories, such as evolution, function pretty much as myths, scientific myths.

Myth shares several major features with science, features which are only occasional or incidental in regular fiction. In the first place, myth offers explanation, especially of the conspicuous puzzles that children are inclined to ask about, such as, Where did this world come from? Who made it? How will it end? What happens to dead people? Moreover, the ways in which answers are attempted tend to take the same broad lines. Objects are discerned, or

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13. For elaboration of these points and further background, see JB, p.604 and earlier.

borrowed from previous stories, and woven into explanatory stories. What is more, the objects are frequently copies, or analogues, of the things they are introduced to explain. Graves illustrates the matter in some detail in illustrating the 'constant rule of mythology ... that whatever happens among the Gods above reflects events on earth' (p.vii). In scientific explanation the analogues are simplified mathematically-tractable objects, such as ideal gases, perfect planes, dimensionless particles, perfect inelastic spheres ("billiard balls"), etc., with behaviour approximately reflecting behaviour of flawed real-life analogues. As with the objects of science, so the objects of myth are frequently not real. This is true even of the earliest work given mythic interpretation, palaeolithic material; a leading feature is that none of the figures appearing is altogether real ('not one entirely corresponds to reality': Laquet, p.2).[14]

Another leading method of explanation in mythology, through appeal to occult forces, is also replicated in science, though the forces involved are natural rather than supernatural. In both, these forces can exercise, commonly through natural phenomena, benign or harmful influences. According to mythology, and to a lesser extent contemporary social sciences, 'it is a function of ritual practices or ceremonies to encourage the former [benign] influence and prevent or neutralise the latter' (Laquet, p.1).

A further major function of myth is to justify a given social system and to account for the rites and customs, usually traditional, in it (cf. Graves, p.v). Western science has the same function, especially parts of the social sciences, such as economics, which are appealed to in support of a sweeping range of often damaging social practices. A striking recent example, fortunately seriously flawed, is sociobiology, which has been presented as justifying leading (but shabby) features of Western "advanced" capitalism.

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14. One reason is that 'real animals are partially deformed in order to avert the hostility which might be aroused in them were their exact resemblance drawn'; Laquet, p.2.

Explanation and social justification and control are of course not the only functions of myths. In modern times, mythology has been interpreted in these and many other ways:

> as a primitive, fumbling effort to explain the world of nature (Frazer); as a production of poetical fantasy from prehistoric times, misunderstood by succeeding ages (Muller); as a repository of allegorical instruction, to shape the individual to his group (Durkheim); as a group dream, symptomatic of archetypal urges within the depths of the human psyche (Jung); as the traditional vehicle of man's profoundest metaphysical insights (Coomaraswamy); and as God's Revelation to His children (the Church). Mythology is all of these (Campbell, p.382, italics added).

The main fault of each of these judgements is their single-track character, along with the fashionable tendency of some of the judges to denigrate myth. As Campbell continues,

> The various judgements are determined by the view-points of the judges. For when scrutinized in terms not of what it is but of how it functions, of how it has served mankind in the past, of how it may serve today, mythology shows itself to be as amenable as life itself to the obsessions and requirements of the individual, the race, the age.

Myths continue to adapt to modern needs and prejudices; they pervade modern thought. The idea that myth-making and myth-elaboration came to an end, were driven out, with the rise of modern science is false, a myth (in a derivative sense). Science has not replaced myth, but developed alongside it, shattering much primitive mythology, but adding naturalistic myths of its own. For much of the newer meandering or speculations of science resemble, or amount to, myth. And outside science, in more popular culture, myths flourish in abundance, if no longer in the static traditional fashion.

How can this be so? Doesn't myth always involve appeal to the super-natural, which respectable science nowadays eschews? No. Scientific work, even in respectable anthologies, continues to include allusions to cosmic design and the like. And though traditional myths usually, and perhaps typically, involved supernatural persons, this is not essential to myth. It is important to roughly delineate two styles of myth.

In the early history of Western theorizing concerning the cosmos and its contents, a major shift took place in the style of myth offered by way of explanation, unification, etc. This is the shift from traditional anthropocentric myths to naturalistic myths. The historical shift, of which there are virtually no details, took place between the time of Hesiod and Homer and that of the Milesian philosophers, who flourished about the 6thC B.C. - after which time the two styles of myth ran into tandem, as they do in modern times, with, for instance, creationist and evolutionary myths competing.

In anthropocentric myths, phenomena are explained on the pattern of agency and design, by appeal to humans, writ large. Gods, with (virtuous) human features on a grand scale, account for what happens. Virtually all traditional mythologies, as well as the creation stories of main religions, take this form. With naturalistic myths, by contrast, stories are told which do not depend upon human involvement in any guise, and there patterns of chance and selection become significant, and begin to displace design and agency. Such are the cosmologies of ancient Greece, which run in partial opposition to the stories of the Gods. There is , however, no sharp division between these two styles of myth, because anthropic considerations can occur to qualified extent, as in some parts of contemporary physics.

There is nothing in the internal structure of naturalistic myths which separates them from scientific theories. Myths may be as complex and intricate as theories of social science, with matching deductive architectonics. True, few have anything in the order of the mathematical complexity (not to say empirical power) of contemporary physics; but, then, nor do other branches of science.

Although with naturalistic myths a much closer resemblance to modern scientific theories is obtained, and although what differences there are in style of stories can be eroded or removed by intermediate "myths" (whether

contrived, or drawn from subjects like psycho-analysis), still a significant difference remains, namely that concerning the way myth and science answer to experience. While early myths were not entirely severed from experience, and stories were elaborated and adjusted, as the Ptolemaic theory was, to allow for and account for new observation, the impact of experience was soft; there was no substantial experimentation or hard testing of theories. That sort of thing only really emerged in the modern period and is still not rigorously enforced, else much cherished scientific theory would be threatened. So even now these differences in answering to experience are a matter of degree; many contemporary theories only meet experience to a qualified extent, and some would be imperilled if subject to hard testing. That is why, though there are differences in empirical degree, there is no sharp division between myth and science.

But aren't myths fictitious? Only, again, in much the same ways as scientific theories, particularly failed theories. Myths can be arrived at in rather similar ways to the elements of scientific theories; devised, concocted, dreamed, inherited, happened upon, and so on (in ways subject to some controls, empirical and historical). Like theories, myths are, so to say, imaginary items, not something you could run into or kick. And it would be prejudicing issues, for instance, against contemporary religions and ideologies, to assume that all myths are false, that all are like entirely failed and discredited theories. That is a complete outsider's view, not an insider's impression. Insiders to a myth or theory, while not perhaps adherents, are to some extent caught up in the theory, feel its pull and appeal, and allow that it has things of merit to convey. Someone who is fully inside a theory treats it differentiy, as if it were true or approximating or getting towards what is true. But an outsider, while perhaps sympathetic, sees it differently, as likely false - and in this respect fictitious - and not something by which to orient part of one's life.

Part of the answer, then, to the complex question as to why many people

adhere to myths is the same as that to why they continue to adhere to flawed and failing science. Many people are insiders. But why believe what is lacking adequate evidence? What counts as adequate evidence is often a contentious matter, and, whatever it amounts to, a good deal of received science likely does not measure up to it. More important, there appears to be in many people, for (so far) little understood psychological reasons, a need to believe, to have some system to fall back upon, that can be accepted, even some need to adopt an authority framework, such as entrenched myth or science supplies. These belief systems typically extend far beyond what is required for day-to-day living and decision-making, but not beyond what may seem required for wider life-orientation and explanatory purposes.

Since not uncommonly such systems appear to lack requisite support, it is tempting to suggest that they should be abandoned, or at least reduced to what does have support (whatever needs they provide for).[15]

§ 4. Explanation in science and myth and elsewhere. Pure fiction is not much concerned with explanation of physical or even social phenomena; it typically tells a story without aiming at an account of actual phenomena. By contrast, one of the major shared goals of myth and science, and also philosophy, is explanation. In particular, all are much concerned, in overlapping ways, with explaining, and thereby also understanding, the actual world, its origin and history, its features, (especially some of its more peculiar and anomalous features), its inhabitants, and so on. The route to explanatory theory is generally similar as well,

> basically the quest for unity underlying apparent diversity; for simiplicity underlying aparent complexity; for order underlying

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15. But this would be a large and contentious claim, in effect taking one side of an old dispute between Clifford and James concerning the will to believe (the need to believe adding a further psychological dimension), and a newer dispute between inductivists and hypothetico-deductivists. For a presentation and assessment of the old dispute, which bears on the newer dispute, see Mackie, p.204ff.

apparent disorder; for regularity underlying apparent anomaly (Newton, p.132).

A widespread assumption however, which has jeopardised much clarification, is that such explanation must proceed in terms of what exists, that only through further existent things can what exists and features of what exists be explained. In some respects this is a surprising assumption, since, on the face of it, even scientific explanations very often proceed to account for features of what exists in terms of what does not; for example, by way of idealised objects such as perfect spheres, frictionless surfaces, dimensionless particles, and so forth.[16] A familiar example is the explanation of the behaviour of gases, and the empiricalprinciples they approximately conform to such as Avogadro's law, using the ideal gas law:-

> This law is called ideal because it follows from the assumptions of the kinetic theory, which describes an ideal gas. The molecules of an ideal gas have no volume and have no attraction for each other. The molecules of a "real" gas obviously have a volume and there is some interaction between molecules, especially at high pressure and low temperatures where the molecules are pressed close together. Fortunately, at normal temperatures and pressures found on the surface of the earth, gases have close to "ideal" behaviour (Malone, p.201).

What are gas laws <u>true of</u>? Ideal gases. That is the correct answer; the answer that informed chemistry students will give. But, as they all know, ideal gases do not exist. And what serves to explain the behaviour of ordinary terrestial gases? As they know, ideal gases and their properties. The existent is explained by the sufficiently analogous nonexistent.

Nonetheless the common assumption is deeply entrenched and virtually ubiquitous, and, broadened to encompass all reasons, has obtained principle status, as the Ontological Principle, in Whitehead's mature philosophy. According to this principle, 'the reasons for things are always to be found in the composite nature of definite actual entities ... The ontological principle can be summarised as: no actual entity, then no reason' (PR, p.25).

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16. What follows complements the argument of JB, p.810ff.

Such is the received status of this sort of principle that Whitehead did not even bother to argue for it, or, apparently, feel any obligation to defend it.

The main reason why defence is felt unnecessary is that the Ontological Principle derives from the Ontological Assumption, which is an integral part of the prevailing referential paradigm.[17] Under the referential paradigm discourse must be at bottom referential, always comprising transparent judgements about clearly existent objects. Only such aseptic discourse is clear and distinct, in order as it stands. All other intensional and inexistential discourse requires analysis into this approved logico-linguistic form: such is analysis into classical logical form and into linguistic deep structure. According to the Ontological Assumption, which encapsulates one side of the referential theme, truth always concerns, after requisite analysis, what exists. Now genuine explanation calls for fall-back explanatory hypotheses which are true. But by the Ontological Assumption, then, no such hypothesis can be about nonexistent objects. Hence genuine explanations cannot proceed by way of nonexistent objects, so yielding a suitable version of Whitehead's Ontological Principle.

It is really because of the Ontological Assumption, and the referential paradigm underlying it, that philosophers are prepared to go to prodigious lengths to try to transform theories and laws into something they are not, and to put up in all seriousness themes that are patently false. Deeply entrenched the referential paradigm may be (more American and Western than apple-pie), pervasive the Ontological Assumption may be in theoretical work; nonetheless these things represent the grandest mistakes of mainstream philosophical tradition, and they have left a great trail of damage and

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17. There is a subsidiary agreement of importantce for the Principle. Namely, it derives from the assumptions that all explanation is at bottom causal, and that causes must always involve existents. As religion tends to elevate agency to the sole mode of explanation, so mechanistic science tends to elevate causality to the sole form (cf. Mackie p.237) whence the main assumption. But the assumption is false; much satisfactory theoretical explanation is not causal.

unnecessary problems. That is a large set of hotly contested claims to set down and leave without further defence. Unlike Whitehead, there is an excuse, that the requisite defence has been given in detail elsewhere (especially in JB).

Once the Ontological Assumption and its referential setting are duly left behind, the genuine work, of explaining how myth and philosophy do explain, can be tackled. A worthwhile beginning has in fact been made on this by those who, dissatisfied with the standard constrictions on what is called "scientific explanation" (and especially the narrowness of the deductive covering-law model), have sought a broader notion of generic or systemic explanation.[18] At a first approximation, a systemic explanation fits or tries to fit, a thing to be explained into a system, or framework, in such a way as to make it clear or intelligible, at least to those appropriately caught up in the system (system insiders). Or, slightly better, to explain something, to someone (or some audience), is to invoke (introduce, recall, etc.) some part of a story, to which that person (or persons) is an insider, from which the thing to be explained can be suitably demonstrated, in particular how it happens, illustrated or, more generally, argued, in ways admitted by the person (or persons). Thus a characteristic form of (interstatemental) explanation of C goes as follows: there are statements A, B, etc., set in background story S (to which those to whom the explanation is directed are insiders) such that C derives by suitable (admitted, and followed) argument from A, B, etc.; in brief, C be-cause of S.

Main elements of the covering law model are all represented, but in relaxed form: instead of a law there is a covering story, into which the phenomena to be explained is fitted or set; instead of a tight deductive relation, there is some looser relation of yielding or showing how the phenomena to be explained results. But what then distinguishes such use of a

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18. Several (rather vague) accounts of this type are assembled in Yolton, pp.205-7.

story as an explanation? That it fulfils, to a suitable audience, an appropriate explanatory role, such as making clear or intelligible or producing understanding. The narrower accounts sought to remove such an epistemic component from explanation, in favour of a purely logical account, perhaps in the hope of logically reproducing the epistemic component. In this respect, however, as in others, the narrower accounts failed: "explanation" was severed from normally expected epistemic connections. In the account outlined these connections are restored in the requirement of a suitable derivation or demonstration, which the intended audience can follow. (Thus relevant elements in explanation of making clear, or intelligible, or providing understanding, are here explicated, Wittgenstein fashion, by way of following.)

It is clear, as the covering story account duly allows, that myths afford explanations, at least to their insiders, in much the way that sciences do. All that differs in scientific explanation is that the stories are those of science – received science to which it is assumed the audience is (or ought to be) an insider – and that the suitable arguments are those received scientific methodology sanctions. Exactly which arguments are sanctioned is still a matter of some controversy, but at least deductive arguments and certain statistical arguments are, whence Hempel's rather minimal account of scientific explanation. But such minimal accounts are merely that, necessary conditions when other requirements of suitability are met, but very far from sufficient.

There is nothing in the commonsense notion of explanation or in such refined explanations of explanation as the covering law or covering story accounts, which demands that explanations must proceed in terms of what exist. Nor – fortunately, given the shape not merely of myth but of much received science – is there anything which requires that the stories involved be more or less true, though to fulfil their intended role they must be acceptable to

the audience addressed. Now, even on standard logical perceptions, false but accepted theories or stories may be about what does not exist (in a way that is not as problematic as it is with true theories), so such theories or stories can provide explanatory cover involving nonexistent items. Naturally the explanation resulting will hardly be as satisfactory as those covered by true theories, where they can be had; but we scientific beggars can't be chosers.

But some of the most likely covering laws or stories will be, and are bound to be, about what does not exist. Take, for example, the relativistic inertial law concerning particles not acted upon by external forces. Though such particles do not exist, the law can fulfil its explanatory functions. And should we be lucky enough to hit upon a cosmological myth which gets the broad outlines of cosmic origin more or less correct, the same would be true, since the myth would have to explain either how what was always there got there, or, more likely, how something came from nothing. In short, some of the explanation would have to go by way of what does not exist.

§5. The dialectical turn: inconsistent theories and other inconsistent objects. The essential role of nonexistents in explanation is not the worst of the bad news for the establishment; blacker news concerns the possible truth of inconsistency. Much as several different features come together to push theoretical matters beyond the actual, so several factors combine to force a satisfactory theory further still beyond the actual and into the impossible. A first and most conspicuous moving-force is the fact of inconsistent theories, including major scientific and mathematical theories. These theories are themselves very obvious inconsistent objects (no doubt objects of "higher order", but objects nonetheless). The number and variety of these theories has been indicated elsewhere (by associates of the LSE School, such as Lakatos and Feyerabend, and also in OP). Inconsistent theories are by no means the only inconsistent objects that have figured, sometimes

prominently, in the advance of science. Infinitesimals and sets are others; Bohr atoms and Dirac δ-functions are still other less famous historical examples.

A direct consequence of the fact of nontrivial inconsistent theories is that standard logic and standard models are inadequate for more than a partial account of scientific theories, or of matters where they feature, such as "the logic of science". However, there are alternative logics which do not suffer the fate of standard logic, in which inconsistent theories do not yield any and every statement. Some of these paraconsistent logics can provide a suitable vehicle for a logic of science. Strangely this remains unknown to Feyerabend, who, despite his advertised width, is remarkably narrow as regards logic (see e.g. p.211). Logic, which he repeatedly denigrates, is identified with standard logic. Since standard logic fails with inconsistent theories, logic fails; it is too naive and simple-minded to reflect science (p.211). But the argument - Feyerabend does not give away informal argument, which he might have suspected admitted of some formalisation - is evidently fallacious. For an indictment of standard logic, which alternative logicians might make, is no indictment of logic.

A second moving-force lies in the nature of contradictory objects themselves. Contradictory objects really are contradictory - or ought to be in any account true to the facts. The attempt to accommodate genuinely contradictory objects in a consistent theory, for instance of augmented standard type, is evidently a subterfuge, though an appealing one given the establishment horror of contradictions. A comprehensive theory of science will not exclude such objects, on pain of conspicuous incompleteness (regarding the historical, logical and linguistic data). Thus a comprehensive theory of science, like that of myth and fiction, will assume a nonstandard logical base, that of paraconsistent logic.[19]

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19. For more details and much further requisite argument, see OP.

It is hardly surprising that, in this more generous logical setting, many things they (the establishment especially) said couldn't be done can be done. For the setting provides a way, not of doing, but of thinking the impossible, a way, that is, of doing what can be done, but that standard theory would entirely exclude. Things of some relevance that can be so accomplished include an almost naive theory of fictions (accommodating cases where the source supplies contradictory specifications, such as that Mr Fathin is both fat and thin) and a non-Tarskian semantics for simplified natural languages which include such semantical predicates as the truth predicate. But for illustration some psychological paradoxes which have long entertained philosophers will serve well. These puzzles concern thoughts, wishes, and the like, that we seem perfectly able to have, but which are excluded on pain of contradiction: so that it may look, and has looked to standard logicians, as if there are some surprising limits to thought.

Consider, for example, the thought that nothing is now being thought, or your thought that what you are thinking is itself not so, or a thought about a thought that it is not about itself, or ... . Or, for a case involving a different intentional attitude, consider the situation where my only wish is that your wish comes true, but your wish is that nothing I wish should eventuate. These defective or paradoxical intensional objects, as they are called, exhibit the same sort of rich variety as the semantical paradoxes they parallel.

But what has seemed especially perplexing is that these intentional objects resist disposal, in particular disposal of types apparently got away with in the case of the objects of the semantical paradoxes. It isn't very plausible to claim, for instance, that no one can have the thought that nothing is now being thought here, is it? What do we say then? This . That the mirror image of the familiar incompleteness of objects, such as theories and thoughts, is overcompleteness or paradoxicalness. And that some theories, such

as a complete theory of paradoxicalness, and some thoughts, such as those of the problems that Mally posed for Meinong, are, what they seem, paradoxical. Mally's thought about a thought which is not about itself is at once about itself and not about itself (cf. JB, p.501). My thought that whatever the status of the other claims in this paper this last claim is mistaken, though mistaken, is not mistaken.

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SCIENCE AND SCIENCE

ABSTRACT

Science is compared with business, and the dirtier side of science glanced at, as well as the broad range of fashionable activity. Philosophy of science typically abstracts from the wider activity to concentrate upon the propositional product, but even that product is many-stranded. In particular, within recognised (or cited) science, received or mainstream science can be distinguished and this normally differs from science proper i.e. true science, so far as it is known, which it only overlaps.

Many of the recent disputes as to the character of science, notably that between Stove and the "modern irrationalists", can be explained through the proposition that one side (Stove) concentrates upon science proper while the other (Popper and company) is primarily concerned with received science. The opposition is systematic and runs through related epistemic notions such as knowledge, discovery, solution, etc. Not only is there much evidence that the opponents have respectively sighted different parts of the larger scientific package, but that evidence helps reveal underlying reasons why the remainder is accorded at most some noumenal role (the contrasting reasons are roughly: sociology as unscientific vs. scepticism based on deductivism). The resulting limits to the integration of the opposing positions into the single wider perspective of science are investigated, in a way intended to illuminate further features of science.

SCIENCE AND SCIENCE:

relocating Stove and modern irrationalists

Science is like business. It is now a widely approved enterprise, but, in part for just this reason, increasingly difficult to demarcate, and contained within decidedly fuzzy boundaries. Like bigger business, science is a complex process, conducted for the most part by a professional elite, with products, some of them marketable; it tends to concentrate upon such products. For it is attracted by size and money and power. It is state supported and encouraged, and closely integrated with military and larger industrial enterprises, especially big science. It is increasingly authoritarian, not merely in its hierarchical structural arrangements, but in some of its methods, which include suppression of dissent, and appeal to authority figures and experts whose backing is merely ideological. In some of its dirtier practices, it is far removed from the favoured enterprise discussed in standard philosophy of science fairytales.[1] Much as the polluting factories of impressionist France were carefully avoided in impressionist painting, from angles selected for scenes on the Seine, so too the views on science philosophers choose to represent and haggle about obscure the extensive seamy sides of science. So it is also with our philosophers, Stove and those he calls (in 1982) the 'modern irrationalists'; it is over a cleaner producer-independent fairyland science that Stove and some of the Popperian opposition joust, over attractively-packaged products in brightly-lit stores. In trying to clarify and alter their contest[2], we too can

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[1] As to the visible part of the iceberg of suppression, fraud, and the like, in science, see s.g. Martin and others, Broad and Wade, Arditte and others.

[2] For most recent reports on this contest from participants' viewpoints, see Stove and Watkins, as cited in the reference list.

abstract, naturally with loss of detail, firstly from those who produce and consume science, to the initially-refined enterprise itself - in somewhat the way semantics abstracts from the language users of semiotic activity. It is the next stage of abstraction, of narrowing of focus, down to the acclaimed products of this diverse and dynamic enterprise, the output presented in books and papers, that calls for more care, especially if the contest is to be relocated.

Like business, science is now fashionable, to the point almost of becoming another sacred cow. Indeed, despite sporadic and much-attacked anti-science attitudes, science has become such an approved activity that many partly-empirical subjects seeking further respectability have built-in the title 'science'. Thus appear such recent offerings as 'food science' and 'wine science' and 'sports science', and such renovations as 'geographical science' for 'geography' and 'organisational science' for 'administration'. 'Science' sounds better and attracts more support than 'studies', which is what these subjects mostly amount to; these days the title 'science' works better no matter how the systematic and empirical character of the studies concerned are emphasized. This recent academic bandwaggoning, which deserves some of the anti-science strictures, is not exactly a new phenomenon but a new wave of an older phenomenon. Already in the 19th century such "social sciences" as political science, the study of politics, were not so much discovered as named. Science in its stricter (newly-refined late 18th century) sense is more discriminating in the methods admitted and the theoretical structure expected. Sciences which conform to stricter standards, sciences which have for instance a plausible law or two of their own (unlike, say, Say's law), are often distinguished nowadays as 'hard sciences',[3] from other systematic studies of actual

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[3]A term which is not only prejudicial, but is being made to carry sexist overtones: cf. Easlea.

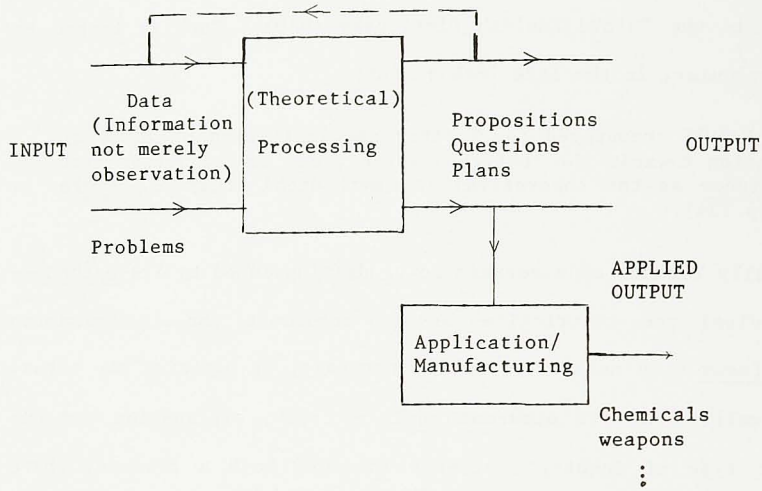
things which are merely 'soft sciences'.

Part of the significant modern evolution of the term 'science' is evident from the listing for science in the Oxford English Dictionary (OED). More is traced by Raymond Williams: in particular, in the late 18th century,

> Changes in ideas of NATURE encouraged the further specialization of ideas of method and demonstration towards the 'external world', and the conditions for the emergence of science as the theoretical and methodical study of nature were then complete (p.234).

Science is then essentially a study of a certain sort, distinguished by its methods - experimental and methodical and restrictive in the arguments and systemization permitted - and by its focus - on nature or, more generously, on parts of the actual (external) world - as well as by its apparent goals - truth, explanation and the like. As a study (or type of inquiry), science involves both a process, where admissible methods are applied to data and items within the focus, and a product, an output which is primarily propositional, comprising theories, claims, questions, conjectures, and the like (see figure 1). There is no serious loss of generality, however, in treating the output as entirely propositional, with questions duly reformulated propositionally (e.g. 'it is inquired whether ...') and indeed in treating the product as consisting of theories (generously construed). Moreover, for much of what follows (not for other purposes), there is no great loss in abstracting further in the fashion of standard philosophy of science, and in treating science in terms of its output, as a propositional product - though really science consists of processes, with such a product.

FIGURE 1. Simple black box model of initially-refined science (and its applications).



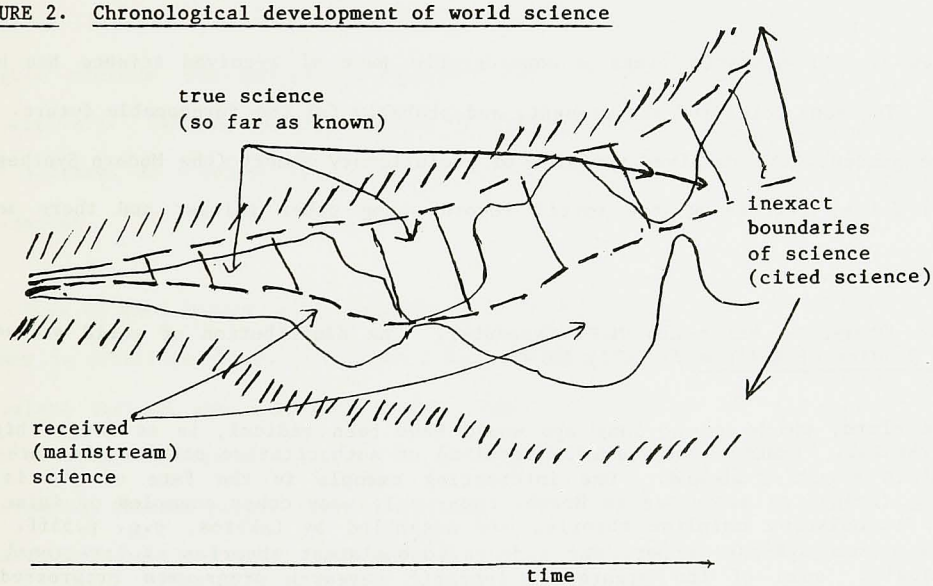
Note: the dashed feedback loop assumes especial importance (and solid status) for those who erroneously see the data as essentially theory-dependent.

At any given time, science as a product consists not only of a bundle of received, or generally accepted, theories covering its different reaches (physics, molecular biology, economics, etc.), but also of competing or alternative theories which have not been definitely rejected. For instance, there are several rival cosmological theories (for the situation in 1964, see North). And differently, though quantum theory has a generally accepted and taught core within the Copenhagen theory, there are as well several competing interpretations or elaborations of the usual formalism, e.g. hidden variable, alternative logic, and multiple world interpretations. But perhaps the most remarkable example in physics is the persistence of the classical program; the former paradigm has not been entirely

abandoned, but now lives a largely peripheral life (for examples of this program, see Sansburg, Barnes and others, and most noteworthy, The Journal of Classical Physics.)

Science is not then a single theory (or, in a narrow sense, a single thing) but a system of theories in various stages of development. A graphic picture of the development of science over time is provided by that of an expanding rope or widening river (see figure 2). Parts of the river can be broadly distinguished, in the way depicted. The schematic picture concerns of course the propositional product generated by scientific activity; once again the picture abstracts from the scientific process itself and still more from science more broadly conceived as a socially organised instrument.

FIGURE 2. Chronological development of world science



In the approved scientific literature, that included in citation indices and the like, received theories predominate. But even in cited or recognised science,

alternative theories are to some extent included (through the "wilder" ones tend to be represented outside the more respectable literature.) Thus the data base for sociological investigations provided by scientific citations does not provide a pure picture of mainstream science; an additional filter is required to catch the impurities generated by unorthodox alternative theories, rival supplementary theories, and the like. Accordingly, the inaccuracy of starting points for the sociological investigation of science such as the following should be evident:

> We prefer to refer to the data base as representing world science, or 'mainstream science' - e.g. the most heavily utilized science.[4]

Mainstream science is a proper part of world science, a part roughly distinguished in fact by its heavier utilization.

Now in all historic times a considerable part of received science has been false.[5] The same holds for the present, and probably for the foreseeable future. As for the present, the received revision of evolutionary theory (the Modern Synthesis) appears false, refuted by the fossil record among other things; and there seems

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[4] J.D. Frame, F. Nairn and M.P. Carpenter, 'The distribution of world science', Social Studies of Science 7 (1977) 501-16.

[5] This claim, which not so long ago would have been radical, is no longer highly controversial. Examples of formerly received or authoritative studies that are now recognised as false abound. One interesting example is the fate of the island universe theory, as discussed in North, chapter 1; many other examples of false and (even) inconsistent mainline theories are assembled by Lakatos, e.g. p.55ff. As Lakatos argues against Popper, who rejects inconsistent theories as irrational (or uncritical), 'some of the greatest scientific research programmes progressed on inconsistent foundations' (p.147).

More problematic than past false theories is the position of currently received science which is at the same time acknowledged as false (or even as highly dubious). However the "irrationalists", Kuhn and Lakatos especially, have again explained how this can occur, how old and partly discredited paradigms or degenerating research-programs can be clung to: through entrenchment, scientific conservatism, investments of senior scientists, lack of workable alternatives, etc.

little doubt that sizeable parts of advanced quantum theory and of general relativity theory are defective.[6] Certainly parts of cited science are false, since rival theories included are incompatible. Science proper however not merely aspires to truth, but is true. Insofar as it constitutes genuine knowledge, it must be true. Hence, science proper is only a proper part of recognised science, of the historically "determined" and sociologically and critically investigated product; and similarly received science only overlaps science proper. The situation is effectively acknowledged in dictionaries (such as the OED) which disjoin 'knowledge' with 'study' in the main senses given for 'science'. However the main emphasis at any given time is upon received science at that time. To put the point paradoxically, received science is more science than true science. The current dialectic between historical and logical approaches to the philosophy of science roughly mirrors the different focusses on received science and true science respectively. Just as a fuller theory of science takes account of both received and true science, so a fuller philosophy of science includes both historical and logical approaches.

Much of what Popper and successors have to say about science, and what Stove has to say in criticism of these "modern irrationalists" (especially in his 1982), can be explained through the proposition that Popper and company are primarily concerned with received science of one sort or another (and Kuhn is entirely), while Stove is considering science proper (which received science ought to be so far as feasible and otherwise ought to try to approximate). A similar proposition holds for related epistemic notions such as knowledge, discovery, solutions, etc. (cf. the other

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[6]All these assertions are of course controversial and much debated, especially that concerning the Modern Synthesis. For a fragment of the latter debate, see Ruse's response to Popper's criticism of evolutionary theory (Ruse, chapter 3).

success-terms listed in Sp.7). Thus the knowledge supplied by received science, is not knowledge proper (or absolute knowledge), but received knowledge; and what it guarantees is not absolute truth, but received truth, what is accounted true in the orthodox scientific community, but constitutes at most an approximation to what is true absolutely.[7] There is some temptation to go along with Stove because we tend to use, or like to think we are using, terms like 'knowledge' absolutely; hence too the irregular resort of the "irrationalists" to quotation and other devices to signal their usage. By contrast, wider usage of the term 'science' is well established, especially in these days of renewed scepticism about and criticism of science, and growing recognition of the conservative and authoritarian character of mainstream parts.

No doubt some of the company will be tempted to argue that Stove cannot be considering science proper, because neither Stove nor any one else, God apart, can identify elements of science proper or distinguish them from things that are merely

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[7]This is neatly modelled in a pluralist way, using the semantical modelling of Sylvan. Absolute truth is given by what holds at the factual world T, received truth by what holds, or is accepted, at the mainstream scientific world rT. What holds in recognised science is what is pluralistically (i.e. discursively) true as determined through scientific base worlds (which include T and rT).

Of course this pluralistic way of approaching the matter presupposes that relativism is mistaken, that we can distinguish - at least in hindsight and, ideally, right now - enough components of what is true absolutely and what is false absolutely. It assumes that we do not have to fall back, in the end, on what is received or the consensus or the main convention to determine what is true or false, that for instance some individuals can undertake independent checks of their own, if they care to , e.g. through observation, experiment and reflection. Otherwise, while we can discern the split between true and received science in the past, we shall be precluded from doing so in the present. By contrast with stricter conventionalists, such as those influenced by Wittgenstein, modern "irrationalists" like Popper do not pose this sort of relativistic problem, since in their view truth is absolute even if science is not, by virtue of epistemological scepticism again. In short, the problems with "science proper", which is largely inaccessible according to such sceptics, are essentially epistemic.

part of (critically) received science.[8] That is not so: many components of science proper can be suitably identified, and distinguished from merely received science. This is the situation with much of the physics of ordinary bulk matter - as opposed, for instance, to some of the physics of transitory sub-atomic particles. As Feinberg argues, Thales' problem as to the composition and behaviour of ordinary bulk matter is largely solved, properly solved. To explain these properly known things, furthermore, no more knowledge is required than what is supplied by a hard core of contemporary physics (duly restricted). Of course, many of the company (operating with a high redefinition of 'knowledge', premissed on a purely deductive methodology) would deny any such access to science proper and knowledge; insofar as they would acknowledge science proper at all, they are bound to regard it as virtually inaccessible to us. Here we have do enter a substantive area of disagreement between the sceptical company and the broad band of nondeductivists - though again one with a substantial linguistic component, concerning terms like 'knowledge', 'inference', 'valid' and 'rational'.

There is much evidence supporting the thesis that Stove and what he presents as the irrationalist opposition are focussing upon different parts of the fuller recognised scientific package. The evidence includes all the following items set out seriatim (in fact the core of Stove's initial criticism of the company):-

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[8]This paraphrases part of what Watkins had to say in response to an earlier version of this paper. Combining Watkins' claim with the familiar "no identifying reference, no object", would lead to the conclusion that there is no such thing as science proper. But the theme is mistaken (cf. Routley), and especially dubious where the objects are deductively closed theories whose consequences cannot be effectively specified. The correct alternative is to allow for the object, a perfectly proper object of investigation. But for the company who so so, the object will be a decidedly residual one, a noumenal thing, not open to direct investigation, but whose very limited accessible features are determined only indirectly by inference.

1. The matter of the cumulative view of the development of science. Assuming things are duly recorded and not lost, science proper cumulates; normally knowledge grows.[9] But received science does not grow in this simple way. Sizeable parts of theories may be excised at times of scientific revolution; consider for instance the breakdown - or, seen differently and perhaps more accurately, contraction - of Newtonian theory.

All of world science, received science and science proper have in fact grown enormously in the last 400 years. Whether they will continue growing is another matter - which depends on one's theory of propositions and theories, and perhaps then on such things as research expenditure and effort, and nuclear war. Suppose, to take a doubly extreme case, propositions are assumed to require actual instantiation and a prolonged nuclear winter removes all instantiations; then the propositional product of "science" will reduce to zero, growth will be totally reversed.

What is important, that Stove latches onto, is that various of the company (e.g. Watkins) cannot grant that knowledge proper has grown or, in a straightforward way, that science proper has grown. For by Humean scepticism, which the company tends to embrace, knowledge proper of the external world is now, as then and always, zero. (Zero probabilities for scientific laws come out of the same sceptical box.) Thus knowledge proper has not increased, despite the modern scientific explosion. Nor has there been, or can there be, any straightforward growth in science proper, growth an intuitionist would be happy to recognise, in the form of identifiable propositions

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[9]Thus intuitionistic semantical frameworks, which reflect accumulation of information, provide an appropriate development model. Dynamic modelling of the growth of world science and its parts may be obtained by combining pluralistic models (mentioned in the previous footnote) with models like intuitionistic ones which can reflect change over time.

added to science proper; for these would increase scientific knowledge.[10]

There are various criticisms one can, and should, make of such sceptical themes, but Stove's criticism of irrationality is questionably one. For, in contrast to epistemological irrationalism (as explained, e.g., by Gardiner), not all scientific and epistemic methods are taken as inherently defective or suspect; rather, deduction is (wrongly) elevated to the method, the sound method among otherwise failed or unreliable methods. Moreover, there are certainly clearly articulated reasons for the sceptical theme, which have impressed even great thinkers as good reasons, and the reasons cohere, more or less, with a wider philosophy: in short, the standard requirements for rationality are (superficially) met.[11] Scepticism need not be obviously irrational.

The alleged irrationality involved - strictly the epistemological scepticism concerned - cannot be simply sheeted home to deductivism, because there are other

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[10] In a more oblique classical way, the company can, and must, allow for growth of science proper, as Watkins has pointed out. This can happen because classical science proper, in contrast to knowledge proper, is closed under logical consequence.

[11] There remains, however, as is well-known, the marked discrepancy between theoretical scepticism and day-to-day practice not in conformity with scepticism. The extent to which broad epistemic scepticism meets the coherence requirements of rationality can be properly challenged on this ground. Along these lines the pragmatic irrationality of Hume-Popper scepticism can be argued (cf. Sp. 90ff.)

assumptions involved which cannot be eliminated because arguably also false.[12] However it is a fair response that Popper and company accept the other assumptions: but then they also accept deductivism, and are (wrongly) unmoved by Stove's counter-claims and counter-examples.

There is, as is well-enough known (underlining is not here required), an extremely severe problem as regards the assessment and appraisal of theories about external world for anyone who accepts Humean scepticism (as above), and accordingly cannot identify any of science proper or straightforwardly acknowledge it. Of course a conventionalist approach may look possible; but that is parasitic on others, notably practicing scientists, acknowledging science proper, which scepticism should exclude as misguided.

2. The neutralization of success words by the company. That something is included in science proper is a genuine cognitive achievement, but not that it is included in mainstream science. That is however an achievement of another sort, and may win its authors a Nobel prize, etc. That p is knowledge entails p, that p is received knowledge (conventional wisdom, etc.) does not entail p is true. Etc.

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[12]The point is made tellingly by Fox, p.101. The assumption of empiricism, common to Stove and the older company, which Fox singles out for special mention, is criticised in detail in Routley, p.745ff. Nonetheless deductivism is a root problem; but eradicating it really requires some more convincing logics than inductivists have so far managed to assemble. Fortunately, there is now under way a very different approach to nondeductive logics from those Watkins takes note of, an approach being elaborated (e.g. through non-monotonic logics) because of experience in artificial intelligence, an approach which ought to force some movement on the logical front.

Hence Lakatos's quotation tic, and the other devices Stove enumerates.[13]

3. The sabotaging of logical words by suitable epistemic embedding, which is frequent practice of the company. This is inevitable when one stands back from received science, as distinct from being immersed in it and accepting its claims. For while pRq as a proposition of science proper guarantees pRq, with respect to received science it doesn't offer such a guarantee. All one gets is: "in received science pRq", or "in such and such a theory pRq". This is really a result of the removal of cognitive success.

'Received' can indeed be tacked on, as a sort of internal accusative, to most of the key epistemic and semantic terms, including 'truth', 'falsity', etc. But since what is received varies from one time to another, strictly 'received at time t' should be tacked on. Kuhn's strange talk of anachronisms (made fun of by Stove, Sp.17) appears to derive from taking this stricter form for granted, and thus also operating within the vocabulary of his own theory (cf. Sp.18).

4. The status of "the logic" of science. Stove claims that Popper and company despite all their talk of 'the logic of science' undermine any such logic (e.g. Sp.2?). This is not so: there is, for instance, a logic of received science. In fact it is much the same as that of science proper, since both are theories. Certainly some results are removed in the case of received science because cognitive success is removed. But it is alright to write of 'the logic of mainstream

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[13] One of the strange things in Watkins, in his response to Stove, is his preference for the assumption that standard uses are neutralised, and that success has to be pumped back and highlighted with italics. Strange, but not surprising given his thesis that we have no knowledge of the external world.

scientific discovery', i.e. received discovery. Incidentally, the history of received science is not a history of cognitive success (pace Sp.13); it is the history of a fair bit of failure also, as superseded science shows. Furthermore, because, as explained, both received and recognised science are false - in part, and so in sum - "the" logic of science cannot be classical logic, at least under major determinates for science. In particular, the conditional cannot be the material conditional, else received science is trivialised. (For the larger reasons why the logic cannot be classical see RLR, chapter 1.)

Because they are concentrating on different parts of world science, the disagreement between Stove and the company would tend to dissolve were both sides to recognise what is happening. And from a wider perspective we can accommodate both sides (as in figure 2). But disagreement does not disappear entirely, because of the Humean scepticism some of the company endorses but Stove and we do not. Because of their scepticism many of the company would prefer to omit science proper from the picture, but they cannot do so in any clear way without abandoning an important part of the sort of philosophy of science they want to maintain, e.g. scepticism about inductive inference along with the absence of any scientific need for such reasoning. Science proper remains on the margins of the picture, but in a decidedly noumenal fashion, as virtually inaccessible to us.

Part of what happens is neatly illustrated in terms of the claim with which Stove begins (in 1982), namely

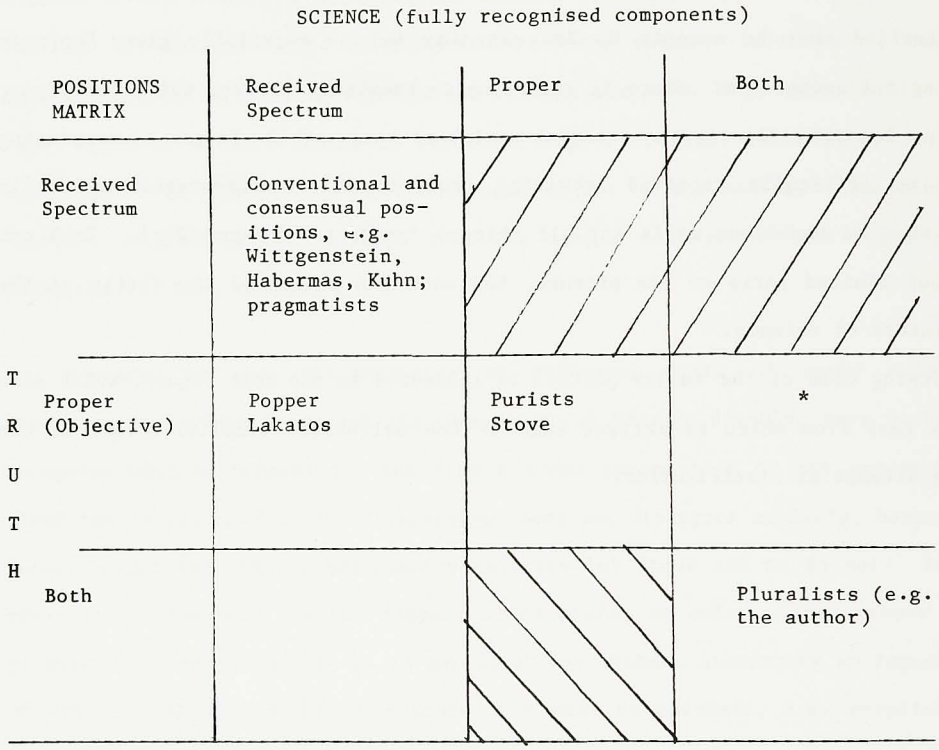
H. There has been a great accumulation or growth of knowledge in the last four hundred years.

Stove insists upon this, and castigates Popper and the company - as irrationalists -

because (he claims) they refuse to believe it. But Popper has responded (to a somewhat naive interviewer, Temple) that he does believe in the growth of knowledge and has implied that he accepts H. How can this be? - especially given Popper's epistemological scepticism? Stove is referring to knowledge proper, which for Popper remains (at least as regards the external world) at zero - while Popper is responding in terms of critically received knowledge (roughly duly corroborated scientific belief), what he sometimes calls (of all things) 'conjectural knowledge'. Both are right about limited parts of the picture, but each has presented too little of the fuller picture of science.

Something else of the fuller picture is indicated in the next figure, which also affords a base from which to correct some of the deliberate simplifications of the preceding attempt at clarification.

FIGURE 3. Positions, and some of their occupants.



The ascending shaded positions are empty, because recognition of science proper leads immediately, by steps not even philosophers would normally refuse to take, to truth proper. The descending shaded position is empty because recognition of both received and proper truth leads to recognition of similar variations for science (as soon as the role and objectives of science are understood). The starred position was formerly occupied by the author; for the longest time, I had great difficulty in seeing how anyone could be a pragmatist and adjust "truth" to what is or proved useful or good for business. And I still consider the purely received positions, which render science almost business, not merely mistaken, but liable to pernicious

exploitation. But for present purposes it is enough to discern these received positions, without advancing a critique. In any case the lines of refutation are well marked: Science is significantly unlike business, in that it should answer to truth proper, not to practical consequences such as expected profit.

As the figure is supposed to suggest, the term 'received science' as hitherto deployed really covers a spectrum, spread along a descriptive to evaluative scale. Again received science is like business, as those whose practical affairs have been scrutinized by a tax commissioner will realize. Towards one end is the descriptive position of the later Wittgenstein, according to which everything is more or less in order as it is; towards the other is the critical (revisionist) position of Popper, which would (rightly) question or reject some of what is ordinarily received such as parts of psychoanalysis or economics. That is, what is accepted under revisionist science is only what passes critical appraisal, critically-controlled received science.

Here enter, in this critical evaluation, such familiar but widely disputed criteria for scientificness of a theory as falsifiability, comprehensiveness, strength, simplicity, and so on. The underlying idea (however misguided) is that these demarcation criteria enable some sort of approximation to science proper - only that cannot be claimed by some of their main proponents who, blinded by epistemological scepticism, cannot recognise science proper. What they can suggest, where they acknowledge objective truth, is that better theories are nearer the truth. However, it is far from evident that satisfaction of some of the criteria, such as simplicity, increases approximation to truth proper, as distinct from what may be more acceptable. So really reliance on these idealist criteria (other than falsifiability) depends upon sliding back to the received spectrum. And, in any, case, a satisfactory account of verisimilitude, nearness to truth, needed to

elaborate the suggestion, is still conspicuously lacking. Thus far then the suggestion remains a dubious conjecture.

A final corollary of the preceding disentanglement of positions is that rather different answers can plausibly be offered to the "historical" questions to which Stove addressed himself (in 1982). These questions are:

Q1. How have the company succeeded in making scepticism (in Stove's terms, irrationalism) about science acceptable to their readers?

Q2. What intellectual influences led the members of the company themselves to embrace scepticism about science proper?

The answer to Q1 is not simply that the company is confusing readers about what they are saying, for instance through sabotage of success words, logical terms, and so on (Stove's answer), but rather that the company is, not illegitimately, primarily concerned in one way or another with received science. And the answer to Q2 is not simply "deductivism": deductivism is a fairly integral part of a larger, erroneous empiricist ideology, which, taken in by sceptical arguments, includes anti-foundationalism. Ironically, some of the company 'have had most to do, recently, with discrediting the empiricism that is at the root of Humes' scepticism' (Fox p.10); their historical investigations have shown the extent to which the development of science, received science especially, violates empiricist canons.

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[14]I have been much helped by comments from Peter Forrest, Brian Martin, Philip Pettit, Charles Pigden, David Stove, and John Watkins.

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