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Progress In Scientific Revolutions: The Problem Of Semantic Incommensurability

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PROGRESS IN SCIENTIFIC REVOLUTIONS: THE PROBLEM OF SEMANTIC INCOMMENSURABILITY

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ABSTRACT

If two successive theories are semantically incommensurable, we have no way to make a complete comparison of their contents. If so, we have no way to verify that the highly confirmed content of the successor is greater than that of its predecessor, and we cannot verify that scientific knowledge has accumulated across the theory change. Thus, incommensurability creates a problem for the justification of the standard cumulative conception of scientific progress.

To resolve this problem, I distinguish irresolvable strong incommensurability from weak incommensurability, which is resolvable. I argue that Kuhn's arguments, insofar as they are sound, support only the latter. Cumulative progress is therefore not only possible, but in principle justifiable. Nonetheless, I support most of Kuhn's claims about the incommensurability of successive paradigms.

My argument for weak incommensurability depends on an interpretation of scientific theories which makes the way a theory is understood an integral part of the theory. Both syntactic and semantic approaches to theories fail to deal with the incommensurability problem because they ignore this pragmatic aspect. I offer a context-dependent semantics

based on contemporary pragmatics which can both represent the incommensurability problem and show how it can be resolved.

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INTRODUCTION

THE PROBLEM OF SEMANTIC INCOMMENSURABILITY

The problem of semantic incommensurability arises from the conflict between contemporary historical studies of the development of science (typified by the work of T.S. Kuhn) and the received philosophical treatment of the meta-theory of science (see Stegmuller 1976b pp 147-148). On the Received View, progress in science involves the accumulation of the strongly corroborated content of theories. Rational justification of claims of progress requires semantic comparability across successive theories. The historical evidence, however, suggests that successive theories are neither cumulative nor semantically comparable.

Kuhn's _thesis of semantic incommensurability_, introduced in his seminal book, _The Structure of Scientific Revolutions_ (1970a), states that some competing theories are mutually incompatible but not completely comparable with respect to their content. If this thesis is true of a theory and its successor, they cannot be compared on such factors as agreement with the facts, approximate truth, or verisimilitude. A direct consequence of Kuhn's thesis is that the content of theories cannot be known to accumulate across revolutionary advances. This undermines the doctrine that knowledge accumulates as science progresses: that the true or accurate component of successive theories is largely preserved, and increases with time.

I will argue that Kuhn's thesis depends on the unavailability within a particular historical context of conceptual resources for inter-theoretic translation. I believe that a satisfactory resolution of the conflict will adopt the traditional cumulative conception of progress in science while accepting the contextual relativity of scientific theories insofar as they are historical entities. The Structuralist Approach, represented by Sneed and Stegmuller, is one attempt to meet both of these demands. Structuralism differs from the Received View and its statement-oriented descendents in using informal model theory rather than the syntactic approach of the Logical Empiricists.[1] Although Structuralism is superior in some respects, both of these approaches suffer from their highly abstract treatment of theories. This makes them difficult to reconcile with the history of science, which treats theories as historical entities. In Chapter 5 I outline an approach to theories

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[1] Although semantic notions have syntactic equivalents, these are generally unknown. The advantages of Structuralism are largely pragmatic and heuristic (see Chapter 4 below).

which treats them as structures interpreted through an algorithm in a particular historical context. My approach relies heavily on pragmatics, and although it retains classic philosophical concepts like meaning, the analytic-synthetic distinction, and reduction, these concepts are rendered highly context-dependent. An appropriate name for my approach might be "contextualism". The general position is one of non-subjective relativism which permits traditional notions of scientific truth and progress.[1]

Using this synthesis, I attempt to close the "rationality gap" (Stegmuller 1976a p viii) Kuhn's thesis creates. I will attempt to keep my discussion as close to Kuhn's intentions and philosophical motivations as possible, since I find him to be the most balanced of the writers who favour incommensurability. Though Kuhn often makes statements which seem quite radical, I believe that much of what he says must be understood in light of his own protests against the way he has been interpreted. I assume that, given the general trend of his argument, many of his more controversial statements were intended rhetorically rather than literally. Even if I am wrong, I think the position I attribute to Kuhn is of interest on its own.

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[1] I suspect that my general position is in some respects similiar to C.S. Peirce's pragmatic realism.

This work contains five chapters. The first shows how Kuhn's thesis is an inevitable result of the development of the theory of science in the last century. The roots of incommensurability are to be found in Duhem and Poincare, and can be traced right into the Received View. Problems with the foundations of the Received View open the door to the account of scientific change which results in the postulation of incommensurabilty.

Chapter 2 deals directly with Kuhn's incommensurability thesis. Aside from exaggerations of his position, misunderstandings seem to centre around the incompatibility of competing paradigms. I will argue that the incompatibility involved is not logical, but practical, resulting from our human and historical conceptual limitations. I then formulate strong and weak versions of the thesis, the strong version being insurmountable, while the weak version can in principle be circumvented. Finally, I present my version of Kuhn's argument for his thesis.

In Chapter 3 I describe the accepted view of scientific progress, arguing that it requires the reducibility of the knowledge contained in the old theory to the new theory, and that this in turn requires semantic comparability. I consider the alternative accounts of Kuhn, Laudan and

Feyerabend, and argue that they are either inadequate, or else require semantic comparability as well.

Chapter 4 discusses the Structuralist Approach, and argues that although it can account for much of the historical evidence, and can represent the incommensurability problem particularly well, it fails at a crucial point to mitigate incommensurability, and must, therefore, be supplemented. The arguments given in favour of the non-statement view of theories of the Structuralist approach depend largely on the assumption that if the statement view is inadequate, the non-statement view must be adopted. The failure of the Structuralist Approach to deal adequately with the incommensurability problem is a Kuhnian anomaly at the meta-level, which suggests the need for some new approach.

Chapter 5 draws together the criticisms and resources of the previous chapters to justify a new approach to theories which takes interpretive context explicitly into consideration. On this view, scientific theories have two components: the <u>explicit</u> part of a theory is the articulated formal structure of the theory, while the <u>implicit</u> part of a theory is the largely unarticulated scientific practice through which the theory is applied. Theories so conceived are historical entities, tied to particular practices and conventions. Incommensurability occurs when the implicit parts of two theories overlap only incompletely in the practices by which they are supported and applied.

I will argue that any incommensurability which is historically relevant is in principle eliminable. Since incommensurability results when the overlapping practices of two theories together with their explicit parts don't completely specify the theories, the remedy is to articulate the implicit but non-overlapping parts. This involves a reinterpretation of some of the terms of the theories. This can be accomplished either by creating a new, more inclusive terminology, or, what is more often the case, by using the unexploited potential of a previously existing terminology. The effect of this articulation is the creation of a broader, more inclusive language capable of expressing the content of both theories.

CHAPTER 1

ORIGINS OF THE PROBLEM

> Reason, or the ratio of all we have known,
> is not the same as it shall be when we
> know more.
>
> -William Blake
> (1757-1827)

1.0 Introduction

On what has come to be known as the Received View (Suppe 1977 p 3), the growth of science has been regarded as an accumulation of knowledge based on theory-neutral observation organized and extended to new cases by explanatory theories. A new theory was thought to be rationally warranted if it was compatible with the evidence for its predecessor, and was better confirmed in novel areas. As this metatheory of science was refined, certain problems arose. These came from both internal and external sources: internally from gaps or over-simplifications in the metatheory itself, and externally from apparent inconsistencies with the history of science. Taken separately, these problems might not have been as serious as they became; internal problems were expected to succumb to closer analysis, while discrepencies with the historical development of science might be accounted for in terms of extraneous (nonscientific) influences on scientists. The metatheory of science determines how science should develop in ideal conditions, whereas the history of science describes how it actually develops within a social context.

Kuhn (1970a) has argued that scientific revolutions result when the current theory encounters intractable problems, called "anomalies". In such circumstances, scientists on opposite sides of the "revolutionary divide" do not seem to come to terms or agree on the significance of experiments. Communication across the divide is at best partial. Kuhn and others have hypothesized that the new theory is so different from the old that it is <u>both</u> incompatible with <u>and</u> semantically incommensurable to its predecessor.

In this chapter I will first discuss the historical roots of Kuhn's thesis. I then discuss why later theories of science failed to recognize the problem. This is followed by a review of the major philosophical criticisms of the Received View which bear on the issue of incommensurability. Finally, I describe Kuhnian normal and revolutionary science, and outline his reasons for believing that his account of the development of science requires incommensurability.

1.1 The Roots of Semantic Incommensurability

Incommensurability has its historical roots in two theses, each formulated in a classic work on the theory of science. The first was proposed by Pierre Duhem (1954); an extension of which has come to be known as Duhem's Thesis. The second, while also considered by Duhem, owes its most interesting formulation to Henri Poincare (1905). It has come to be known as Poincare Conventionalism. The development of the theory of science in this century can be viewed from the perspective of attempts to deal with these two theses.

1.1.1 Duhem's Thesis - Pierre Duhem (1954) was one of the first to recognize that scientific hypotheses could be protected from potentially falsifying evidence by a buffer of auxilliary hypotheses. In order to infer observable consequences from a theory, certain assumptions about the measurement apparatus and experimental conditions are required, but few of these assumptions are ever made explicit. If the conclusion of an inference is contradicted by observation, the problem might just as well lie with some auxilliary hypothesis as with the hypothesis in question. Since any particular hypothesis can be saved from disconfirmation by a particular observation, it is natural to generalize Duhem's thesis to state that any theory can be saved in the face of any evidence by a suitable choice of auxilliary hypotheses.[1]

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[1] Laudan (1965) argues that this is not an historically accurate rendition of Duhem's thesis since Duhem applied his thesis to isolated hypotheses only. Against this, Duhem (1954 p 206) states that only when a theory has reached its complete development, so that observational consequences can be deduced from and compared to experiments, can it be refuted. The generalization to whole theories

For example, the anomalous precession of Mercury's orbit does not require rejecting Newtonian theory unless one can exclude unknown Newtonian effects capable of explaining the anomaly. Leverrier's successful prediction of Neptune (using wholly Newtonian principles) lent considerable credibility to this possibility. Whether or not Duhem held this view, later writers (e.g. Grunbaum (1960)) have taken it seriously.

We can define observational equivalence (or, if you like, observational indistinguishability) as follows:

D1: T and T' are observationally equivalent if and only if any observation consistent with T is also consistent with T', and vice versa.

So defined, observational equivalence is an equivalence relation. Transistivity is the only difficult aspect to prove. Suppose that T and T', and T and T'' are observationally equivalent. T' and T'' would fail to be observationally equivalent if and only if there were some observation consistent with one but not the other. We can suppose without loss of generality that such an observation is consistent with T', but not T''. It is consistent with T because of

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is fairly obvious if we consider theories to be conjunctions of statements whose intended applications cannot be completely specified. The generalized version has been supported by Reichenbach, Hanson, Quine and Toulmin (for discussion, see Suppe 1977 section IV-B-1, esp. pp 71-76 and pp 108-109). Strangely, Kuhn makes no reference to Duhem's thesis, although he states a similar thesis (1970a pp 99-100).

the observational equivalence of T' and T, and consequently with T'' by the observational equivalence of T and T''. Thus no such observation exists, and T' and T'' are observationally equivalent.

The generalized Duhemian relation between theories can be defined as:

D2: T and T' are <u>Duhem-related</u> if and only if T and T' have extensions ET and ET' such that ET and ET' are observationally equivalent.

The generalized version of <u>Duhem's thesis</u> can be stated as:

DT: All theories satisfy D2.[1]

The adequacy of DT can be shown as follows: Every theory T must, by DT, have an extension ET which is observationally equivalent to some EO, an extension of the trivial theory O composed only of observations. Hence ET is consistent with O. Consequently, any theory can be saved in the face of any potentially conflicting evidence with a suitable addition of auxilliary hypotheses. This shows that DT yields Duhem's Thesis. On the other hand, every observation must be consistent with some extension of every theory, otherwise some theory would be falsifiable by some observation. So the theory F composed of all possible factual observations is

[1] Although it is inconsequential, the reader may wish to restrict DT to theories with the same set of intended applications.

observationally equivalent to some extension of every theory. Since all of these extensions are also observationally equivalent, it follows by the transitivity of observational equivalence that all theories are Duhem-related. This shows that Duhem's Thesis yields DT.

DT is startling, since it requires that decisions between theories cannot be made on observational grounds alone. It might be objected that auxilliary hypotheses can themselves be tested, allowing a choice between _them_ on observational grounds. This would disallow arbitrarily saving a theory in the face of evidence by adopting a suitable auxilliary hypothesis. In practice, though, since auxilliary hypotheses can't all be made explicit (the range of possible unobserved influences is infinite), any decision to ignore outstanding possibilities is fallible. Another problem is that the testing of auxilliary hypotheses is itself subject to Duhem's Thesis. Nonetheless, it seems likely that at some point the hypotheses required to save a theory will be so _ad hoc_ that there is no reason to accept them except to save the current theory. If such _ad hoc_ rescues are ruled out, DT does not undermine choices between theories on the basis of content.[1]

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[1] Duhem was not unaware of this (1954 Part II Chapter VI section 10).

1.1.2 Poincare Conventionalism - Physical theory organizes and categorizes the observable facts. This allows for divergent systematizations which might be incompatible, and might not categorize observables in comparable ways (Kuhn 1977a) (though they can't be distinguished on the basis of observations alone).

Henri Poincare (1905 pp 166-172) observed that some theoretical laws and principles, such as the principle of conservation of energy[1] and Newton's first law are "protected" from direct testing. These laws play a role more like that of definitions than empirical generalizations. If they are definitions, then alternate theories using different definitions are possible. Nonetheless, in the cases Poincare cites, the choice of laws seems to involve something more substantive. A particularly vexing problem of this sort illustrates the difficulty. Poincare held that the selection of a geometry for space is not imposed by observation; rather, we must choose a geometry which is convenient, and which becomes "the standard ... to which we shall refer natural phenomena" (1905 pp 70-71). If we have a force which is universal in the sense that it affects all bodies in the same manner,[2]

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[1] Suppe (1977 p 11) reports that Hertz commented on this characteristic of energy conservation in 1894, eight years before Poincare.

[2] The usual examples are inertial forces, affecting a body in virtue of its mass alone, and a temperature differential

we cannot distinguish observationally between this case and one in which there is no universal force, but the geometry of space is appropriately different.

For any theory T which proposes G+F, where G is the geometry and F a universal force, there is a theory T' which proposes merely G'. Moreover T' is observationally equivalent to T (assuming the force has no other observable consequences), in the sense that any model of the observable part of T will also be a model of the observable part of T' (and vice versa), although individual statements of T which are syntactically identical to statements of T' may differ in their interpretation. For example the sentence "The geometry of space is G" may be a consequence of T, but not of T'. Thus the two theories certainly <u>seem</u> different, if not incompatible, yet observation alone does not allow us to decide between them.

We could treat such theories as merely different versions of the same theory. Duhem seems to have adopted this view.[1]

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in a world in which all bodies (including the "plenum" in which the others are embedded) have the same coefficient of thermal expansion. Reichenbach (1958 Chapt 1) developed a more elaborate example in which the apparatus for measuring length varies systematically from place to place.

[1] But see (Duhem 1954 p 212), where the overthrow of "universally adopted conventions" is discussed. Duhem remarks that such revolutions can signal remarkable progress, suggesting that he did not regard theories which differ in their conventions as equivalent.

Poincare, on the other hand, held that two observationally equivalent theories were not necessarily different formulations of the same theory. Given the non-equivalence of the theories on other than observational grounds, some sort of choice between them becomes necessary. The necessity of a choice means that the theories are incompatible, and not merely notational variants of the same theory.[1] Poincare cryptically stated that this choice was conventional but not "merely" conventional. Poincare Conventionalism is the thesis that there are observationally equivalent but incompatible theories.

1.1.3 Empiricism and Meaning - Duhem's thesis and Poincare conventionality don't imply Kuhn's thesis, either separately or conjointly. It seems obvious that if we are to establish a conclusion about meaning a further assumption concerning meaning is required. Empiricist theories of meaning base differences in meaning on differences in possible experience, and so accept Peirce's Maxim:

P: If a difference in meaning of terms alters the truth of statements, it must also make a difference

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[1] The meaning of "compatibility" here is problematic. See section 2.2.3 below for an account. All that is required for incompatibility of theories is that there is some form of conceptual inconsistency involved in adopting both simultaneously.

to possible experience.[1]

This consequence of P ensures that a scientist's stubborn commitment to his theory is ambiguous between an openness to ad hoc rescues and an openness to changes of meaning within his theory. His attitude can be most charitably explained by assuming that he has adopted a policy of not being too specific about the meanings of the terms of his theory until new evidence requires him to "tighten them up". The content of his theory is not fully specifiable until all potentially falsifying evidence is accounted for, and this never happens in practice. Consequently, theoretical terms are only partially interpreted in normal scientific practice.' Whether or not theoretical terms have fully specifiable meanings, normal scientific practice gives us no basis for their complete comparison across different theories.[2]

Several assumptions were required for this result. First, we must assume P, secondly, we must assume that Duhem's thesis applies to the theories in question, and thirdly, that the theories are incompatible in some acceptable way.

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[1] Note that the converse of P is generally false. A difference of meaning which affects possible experience may not affect the truth of any statement in the actual world.

[2] It seems that the first person to raise this problem effectively was P.K. Feyerabend (1962, 1965a, 1970a, 1975). Perhaps the reason the problem was not recognized earlier is due to the influence of positivism.

Lastly, there is the assumption that it is possible to speak of theories with terms of incompletely specified and specifiable meaning. All of these assumptions were available at the end of the last century, but the history of the theory of science developed in a way which had the effect of avoiding their joint consequence.

1.2 Avoidance of Incommensurability

Although directly motivated by other concerns, the positivist movement and its descendent, logical empiricism, effectively avoided the conclusion of theoretical incommensurability for some time. There are basically two ways to avoid the problem: either disallow the incompatibility of equivalent or potentially equivalent theories, or else fix meanings in some way which defangs Duhem's thesis. Both solutions are implicit in early positivism, but the final version of the Received View is open to the charge of incommensurability.

1.2.1 Verificationism - the central doctrine of positivism is the verification theory of meaning:

V: A contingent proposition is meaningful if and only if a difference in its truth makes a difference to possible experience.

V has two versions, strong and weak (Ayer 1946 p 50ff). The strong version requires conclusive differences, whereas the weak version requires merely that there be possible observations relevant to the truth or falsity of a meaningful proposition. Ayer explains the requirement of weak verification as the deducibility of some "experiential propositions" from the proposition in question together with certain other propositions, where the deduction is impossible without the proposition in question (1946 p 52). Note that this requires that there be a specific class of propositions which are distinguished as experiential. This led to a distinction between an _observational language_ and a _theoretical language_. Ayer's experiential propositions are ones which can be expressed without using the theoretical language.

Assuming the strong version of V, any apparent incompatibility of supposed Poincare-related theories must be illusory. Since _a fortiori_ two observationally indistinguishable theories must predict the same consequences for experience, it would seem that they cannot differ in meaning, and cannot be incompatible. The weak version of V, however, does not yield this result.

1.2.2 _The Received View_ - Carnap (1936-37) suggested that theoretical terms could be given a partial interpretation in terms of reduction sentences. These reduction sentences do not give a complete definition of the theoretical terms which they introduce, but constrain its meaning within certain limits. It was recognized later that many theoretical terms are not and cannot be introduced via reduction sentences, and the criterion for the cognitive significance of theoretical terms was further weakened to allow the partial definition of theoretical terms by interpretive systems which constrain the use of a set of theoretical terms together (Suppe 1977 pp 23-26).

The open-endedness of the partial definitions provided by interpretive systems opens the door to the arguments of section 1.1.3 above,[1] but resulting incommensurability poses a threat to the traditional view of progress in science only if theoretical terms are interpreted realistically. Instrumentalist and operationalist approaches to theories remain unthreatened. We can determine whether or not there has been an increase in the empirical content of theories across a change in theory solely on the basis of the observational content of the theories involved. Any incommensurability in the theoretical parts of the theories is irrelevant to this determination.

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[1] Aside from Feyerabend, Jane English (1978) also argues effectively that Carnap's partial interpretation creates Kuhnian problems for the theoretical part of theories. I disagree with her claim that these problems are as serious as the incommensurability Kuhn postulates, which extends to the observational content as well.

Given V, observationally equivalent theories are either both true or both false, even though they may differ intuitively in their theoretical parts. Any such difference is treated as a matter of a difference in conventions, and is held not to reflect any difference in objective truth. Two theories which differ only on their conventions can be considered notational variants of each other.[1] Theoretical incommensurability is bothersome only if we require that individual theoretical statements are true independently of the other statements of the same theory. As long as it is possible to determine the meanings of the observational terms of successive theories independently of the theories involved, it is possible to compare their respective empirical contents. The partial incomparability of two competing theories involves only that component of the theories which has no direct empirical significance. Consequently, any failure of complete comparability, while a possible source of confusion,[2] does not rule out the cumulative view of scientific progress. A difficulty for many philosophers, though, is the denial of scientific realism.

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[1] This conventionalist route is a solution (not Hempel's) to the "theoretician's dilemma" (Hempel 1958, Suppe 1977 p 30-36). See section 4.1.2 below for a statement of this issue in the context of the problem of theoretical terms.

[2] The sort of confusion which might arise would involve tendencies to extend notationally variant theories in ways which are more natural for the respective notations.

1.3 Theoretical Holism

In the previous section it was noted that within the Received View, problems resulting from theoretical incommensurability can be avoided if some form of anti-realism is adopted. Instead of providing an explicit definition for each term in a theory, theoretical terms are defined implicitly by correspondence rules (which give a partial interpretation in observational terms) together with the relations of the theoretical terms within the theory (Feigl 1970, Hempel 1971, Brown 1979). This sort of definition is similar to the definitions provided by the Peano axioms and Hilbert's axioms for Euclidean geometry.

This holism, required to avoid undue constraints on scientific theorizing, undermines traditional empiricist theories of meaning. On the one hand, theories have ~~a~~ factual content: they place restrictions on possible experience. On the other hand, the axiomatized form of a theory, from which these restrictions can be deduced, seems to define the meaning of the terms. It is impossible, if we view a theory as a set of sentences, to separate these two functions. Consequently, we cannot distinguish whether or not a given sentence is analytic or synthetic. (The difficulty seems to have been noted first by Quine (1953)). Since the rationale of the Received View is to ensure the cognitive significance of theoretical terms through definitions in terms of observational terms, the failure of the analytic-synthetic distinction undermines its motivation (Suppe 1977 pp 79-80). This, in itself, does not show the Received View false, but it does render it suspect.

Strong theoretical holism includes both theoretical and observational terms. This allows the extension of Feyerabend's thesis to all the terms of a theory, making the contents of theories incommensurable on any basis. Strong holism is supported by Hanson's (1958) view that observations are theory-laden: meaning is built into perception, and that to be understood completely observations must be understood in the context of some theory. For example, Hanson holds the view that Tycho and Kepler mean different things by the term 'the sun', since each conceives it in a different way, according to his respective theory. Thus, when Kepler sees the sun rise, he sees something different from what Tycho sees. On this view, both theoretical and obsevational terms must be defined implicitly. This position is supported by the difficulty in providing natural conditions for the distinction between observational and theoretical terms (Suppe 1977 pp 80-86), as well as by Quinean arguments to the effect that there are no pure observation sentences independent of collateral information (Quine 1960 pp 42-43).

Kordig (1971) has objected to theory-ladenness on the ground that it confuses statements of observations with statements of beliefs about the objects of observations. He points out that although Tycho and Kepler believe different things of the object that they see, there is no sense in which they can be said to see a different thing, as Hanson seems to say. Although Hanson leaves himself open to the attack, I believe Kordig attacks a straw man. The importance of strong theoretical holism lies in the difficulty it presents for the evaluation of the epistemic significance of any particular observation. While it is surely true that there is a sense in which Kepler and Tycho see the same object, their respective observations have a different significance for each to the extent that they would describe their experience in apparently contrary ways: Tycho would describe the sun as rising, Kepler would not.

The nature of strong theoretical holism can be shown without assuming that all observations are theory-laden. Even if, as I suspect, Quine's claim that observation sentences always involve collateral information and Hanson's claim that all observations are theory-laden should prove to be false, theoretical holism can still be shown. Let us say, for the sake of argument, that statements of the form "That pointer is pointing at 2.6" are not theory-laden, and that scientific observations can be put into such an uncontroversial form. In order to interpret the significance of pointer-readings, it is necessary to draw on theoretical considerations which will generally vary considerably from theory to theory. Furthermore, as Duhem made clear, a variety of different auxilliary assumptions, even within the context of a given theory, can give a specific pointer reading a different significance. Since the language of pointer readings is so sparse, and the language of theory is so rich, any given pointer reading is compatible with many possible situations. Thus, it is quite difficult to say what a given pointer reading means without making some specific assumptions.

The real importance of strong theoretical holism (or theory-ladenness of meaning) can be stated as a dilemma. Either the observation language is rich enough to provide evidence for or against a theory without much further interpretation, in which case its terms are theory-laden, or else the observation language is too spare to provide any evidence without further interpretation, which will always depend on some theory. Although we might be able to escape theory-ladenness of our observation language, we cannot escape the theory-ladenness of the significance of our observations to our theories. Thus stated, the theory-ladenness of observation is inescapable. The internal problems of the Received View, together with the issues raised by Poincare conventionalism and Duhem's thesis combine to make Kuhn's position initially plausible. I will consider whether or not it can be justified in the remaining chapters.

1.4 Theory Dislodgement

In scientific revolutions a new theory dislodges the old. The Received View can be easily extended to deal with theory dislodgement: successive theories can be united through the extension of the later ones to include the highly verified applications of the earlier ones. The earlier are thereby reduced to (or reductively eliminated in favour of) their successors. In some cases all or some of the laws of an earlier theory are also absorbed into the later theory as special cases of its laws.[1] The growth of scientific knowledge, then, is two-fold, involving 1) the accumulation of evidence organized under explanatory theories which may be further reduced to more general theories, and 2) the dislodgement of defective theories through reduction. Competing theories are either equivalent in their observational consequences, in which case they could be treated as notational variants of the same theory, or else they differ empirically, in which case a crucial experiment could

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[1] Often, perhaps always, not all of the explanatory successes of the older theory are immediately included under the new theory (Kuhn 1970a p 169, Laudan 1977 pp 148-49). For discussion, see Chapter 3 below.

be performed to distinguish which theory is correct. The dominant discipline, under which others were expected to be eventually subsumed, was basic physics, as exemplified in the following quote from Hempel:

> The division of science into different areas rests exclusively on differences in research procedures and direction of interest; one must not regard it as a matter of principle. On the contrary, all the branches of science are in principle of one and the same nature; they are branches of the unitary science, physics.

According to this model, an entrenched theory, with many explanatory and predictive successes, remains established until it is dislodged by a successor which reduces the successes of its predecessor, and accounts for its failures and ommissions (Scheffler 1967 p 9).

Work in the history of science by Hanson, Feyerabend, Kuhn and others suggest directly that this cumulative account of science is inaccurate. The objection does not stem so much from a failure of succeeding theories to duplicate the success of their predecessors as from the observation that adherents to successive theories often misunderstand each other and "talk through each other", interpreting the same evidence quite differently. Kuhn explains this with the radical view that succeeding theories are so different from their predecessors that reduction is impossible. As I argue in Chapter 3, any account of theory reduction has the minimal requirement that the empirical content of the reduced theory be comparable to the empirical content of the reducing theory. Kuhn's thesis makes reduction impossible unless incommensurability is resolvable.

1.5 Normal v.s. Revolutionary Science

1.5.1 Normal science - Kuhn divides science into two kinds: normal and revolutionary. Normal science is practiced on a foundation of past scientific achievement, using methods which have had enough success to "attract an enduring group of adherents away from competing modes of scientific activity"; nevertheless the methods are sufficiently open-ended to leave a large number of problems unsolved (Kuhn 1970a p 10). Kuhn originally called such a foundation a paradigm, since it exemplifies a commonly shared practice of a community of researchers. He later introduced the term 'disciplinary matrix' to avoid confusion with the particular solutions to problems which serve as exemplars within the disciplinary matrix (1977a p 463). Other components of the disciplinary matrix include symbolic generalizations and models, or preferred analogies. The problems of normal science (i.e. what scientists work on during periods of normal science) are best characterized as puzzles, since investigation is carried out within the (expected) scope of the paradigm.

Puzzles can be of different sorts. One type results from attempts to extend a theory to new areas, a variety of empirical problem. This involves determining special laws (such as Hooke's Law in particle mechanics) which apply to special types of systems. A second kind of puzzle is conceptual, most commonly the improved formalization of relatively intuitive ideas, such as Euler's revision of Newton's second law into differential form; this comprised a natural extension of Newton's own work on mechanics and the calculus. A third kind of puzzle is also empirical, but involves testing the confirmation of a theory in areas to which it has already been successfully applied. Scientists spend much time devising tests to confirm the current theory under more stringent conditions. A side effect of puzzle-solving activity can be the appearance of anomalous (i.e. unexpected) results, which may eventually lead to the overthrow of the theory.

The restriction of problems to a paradigm generally ensures the existence of a solution, and the paradigm provides resources, methods, and criteria for an adequate solution. The gradual extension of a paradigm to new areas which are expected to be tractable has been called the "Art of the Soluble" (Medawar 1967). Normal science fits the traditional view of science as an accumulation of knowledge: solved puzzles are an extension of the paradigm to new applications. It is a "highly cumulative enterprise" (Kuhn 1970a p 52).

1.5.2 Revolutionary Science - Normal science corresponds to a widespread image of science, but does not seem to account for a recognized phenomenon in the history of science: novelty in the form of radically new theories (Kuhn 1970a p 52). Kuhn argues that if the novel aspect of science is to be reconciled with normal science, the roots of novelty must be contained in normal science, i.e. the source of the shift to novelty is in the problem-solving process itself. Certain puzzles resist solution. These anomalies don't force the abandonment of the current paradigm, since the resistance might be due more to our lack of insight than to anything inherent in the problem itself; nevertheless, anomalies do cast some doubt on the suitability of the paradigm.

Three varieties of anomaly can arise, corresponding to the three varieties of puzzle-solving mentioned above. Kuhn offers many examples of anomalies leading to radically new theories. He describes the process as follows: The initial state is one of relative agreement among scientists, typified by normal science. Anomalies appear which resist incorporation into the paradigm. Attempts to solve the problem or problems lead to a proliferation of perhaps promising but unsatisfactory theories, often incompatible with each other.

The crisis is resolved by the adoption of a revolutionary new theory which becomes accepted as the new paradigm.

There are two well-known difficulties with the pattern Kuhn describes. First, periods of revolutionary science overlap periods of normal science in the same discipline. Kuhn (1957 p 182) admits that the Copernican revolution took an extended period to occur, and proceeded by degrees. Second, competing theories may persist for centuries with no clear winner. One striking example is the competition between the corpuscular and the undulatory theories of light. Certain research traditions may die out entirely, but many merely wither and lie dormant to grow again from their roots when conditions are favourable. These two difficulties do not strike me as particularly relevant to the problem I am dealing with. Even if "revolutions" occur over extended periods of time, it is still true that a radically different theory dislodges the previous one, no matter how long this process takes. In the case of contemporaneous competing theories certain scientists are likely to shift from one school to the other, which has the same effect for them as theory-dislodgement. The case of dormant theories requires the shift of allegiance of the majority of scientists from one paradigm to an other. These shifts in allegiance can be thought of as personal revolutions in thought.

Kuhn's treatment doesn't apply to minor shifts in scientific viewpoint. These shifts can usually be understood in terms of more fundamental principles which are part of an established and unchanging paradigm.

For example, in the field of molecular biology anomalous results were encountered in research on the regulation of lactose hydrolysis in E. Coli. According to the accepted theory, the activity of the gene producing the equipment to produce the enzyme activating lactose hydrolysis is repressed by a repressor protein in conjunction with an inducer (corepressor). Induction in this case (not surprisingly) is governed by the presence of lactose. It was observed, however, that the presence of glucose inhibited lactose hydrolysis. These observations were first suspected, since the technique involved is delicate. Once they were verified, attempts were made to explain the glucose inhibition by accepted mechanisms (e.g. glucose inhibition of the entry of lactose into the cell), but these attempts were unsuccessful. A lac-promoter was suspected and then discovered. This promoter is required for hydrolysis even in the presence of an inducer, and requires for its operation the presence of a substance whose production is inhibited by a metabolite of glucose. Although all of the chemical mechanisms are not yet known, the lac-promoter explanation solved the apparent anomaly. This explanation was certainly startling to the scientists who were aware of the problem. The solution, though, was quite readily accepted, since the principles involved were firmly based in the prevailing understanding of biochemical principles.

Major revolutionary changes in science seem to involve shifts of theory which are not readily comprehended in terms of a more fundamental theory. It is useful to distinguish cases of theory dislodgement which take place within an encompassing paradigm from cases which do not. It is only the latter <u>global revolutions</u> which are subject to Kuhn's thesis.

1.5.3 <u>Paradigms and Paradigm Shifts</u> - A paradigm is something that one learns at least in part by <u>doing</u>. The articulated (formal) part of a paradigm is generally (though not always) learned directly. But the articulated part does not completely determine the paradigm; it must be interpreted through its application to specific problems. A student learns certain methods and extends those methods to other areas. The extension of the methods is largely by analogy rather than explicit deduction from principles. For example, a standard exemplar in Newtonian Mechanics is the simple pendulum. This case is idealized and used as an example of simple harmonic motion. The understanding of the pendulum exemplar, gained by the student through demonstrations of pendula and working out problems involving pendula, is extended to other applications which are analogous in exhibiting some approximation to simple harmonic motion. The analogy is extended further with the help of Fourier analysis to a wide range of periodic and aperiodic phenomena. A second example is the exemplar of colliding billiard balls, which is idealized to the interactions of point particles and extended to the interaction of other sorts of bodies, including sub-atomic phenomena and light. Attempts were even made to apply the analogy to gravitational force. Typically, the understanding gained through working out the exemplars is extended in its relevant parts to the new applications.

The exact form of the extension will generally be determined by a number of factors including the student's skill, availability of funding, interest in the area, familiarity with required techniques, and others. These factors are generally hard to specify, but constrain the means through which a given abstract theory is in fact interpreted. These pragmatic considerations in effect constrain how the theory is to be understood, and place constraints on the interpretation of the theory which go beyond purely linguistic constraints. Which of these constraints are legitimate and which are not is established through the operation of a scientific community, which in effect determines how a given theory is to be understood at any given time. A student learning a paradigm also learns a technique for interpreting the terms it contains. The two processes are inseparable.

In global scientific revolutions, the methods of the new and old paradigms differ and cannot generally be compared in terms of universal scientific methods. Since meanings are determined by a paradigm, and involve an unarticulated component through which the formal, articulated part of the paradigm is interpreted, the direct comparison of meanings is often prevented. Kuhn argues that although competing global theories are incompatible with each other, no complete comparison of meaning across paradigm shifts is possible, since there is no higher authority than the scientific community making up a discipline to determine the correct interpretation of the terms used by the discipline.

Kuhn's thesis was widely perceived as undermining the rationality and objectivity of scientific progress (see for example Scheffler 1967 p 19, and Shapere 1966 pp 383-4). However, there are a number of intermediate positions between the Received View and irrationality. Although both Kuhn (1970b) and Feyerabend (1970b, 1977) reject the charge of irrationality, it is not exactly clear which of these intermediate positions they subscribe to. Kuhn (1970a p 126) has admitted he is unable to completely relinquish the traditional view of a neutral observation language based on sensory experience until an acceptable alternative is forthcoming, suggesting that he would look most favourably on the minimum revision which will do the job. This strikes me as good sense. In the subsequent chapters I will attempt to formulate and defend a modification of the traditional approach.

CHAPTER 2

THE CASE FOR INCOMMENSURABILITY

> Rational conciousness...is but one special type of conciousness, whilst all about it, parted from it by the filmiest of screens, there lie potential forms of conciousness entirely different. We may go through life without suspecting their existence, but apply the requisite stimulus, and at a touch they are there in all their completeness.
>
> William James
> (1842-1910)

2.0 Introduction

In this chapter I first deal with several misconceptions about Kuhn's incommensurability thesis, then define it and present what I take to be Kuhn's argument in its favour. I treat theories as concrete historical entities, rather like a functioning (implemented) computer program. Semantic incommensurability, where it exists, is a direct consequence of differences in the way competing theories are understood by their proponents. It cannot even be represented without explicitly taking the context of the theories into consideration.

2.1 Kuhn's Models of Incommensurability

Kuhn (1970a) tends to argue analogically rather than directly, and uses several analogies to explain various aspects of incommensurability. These are: change of paradigm, change of world, gestalt shifts and translation across different world-views. The salient features of these models are discussed in Appendix I.

Several points about incommensurability can be taken from these models. First, the psychological aspects of paradigm shifts are similar to perceptual gestalt shifts, being subject to factors beyond our immediate control, including our biology and past experience. Our actions during paradigm shifts need be neither rational nor deliberate. The gestalt model, however, fails to explain incommensurability, since (at least for the perceptual examples usually given) it is possible to compare conceptual gestalts in terms of the perceptual components on which they are based. Incommensurability requires that there are no such common components. Although paradigm shifts are in a sense—revolutionary within their discipline, only global shifts are candidates for incommensurability.

Second, it is necessary for incommensurability that the pre- and post-revolutionary theories deal with the same world, or else comparison of competing theories with respect to the knowledge they give us about the world, or their ability to allow us to interact with the world, or any other aspect of their relation to the world is pointless. Even if incommensurability holds, competing theories share a common basis of uninterpreted observations which, when interpreted, become evidence for those theories. These observations can be referred to demonstratively without prejudice as to which description of the observation is correct.

Finally, the translation analogy stresses the importance of tacit, unarticulated knowledge to the interpretation of the terms we use. An adequate semantic comparison of two theories must be able to render explicitly the differing tacit components involved in understanding the theories, such that at least their taxonomies are preserved.

2.2 Comparability and Incompatibility

Incommensurability has been notoriously difficult to characterize.[1] Kuhn's critics have misunderstood his views on i) the relation between incommensurability and inter-theoretic comparison, ii) the strength (or, rather, weakness) of the incommensurability thesis, and iii) the force of Kuhn's arguments. Underlying each misunderstanding is a failure to recognize the essential role of practical factors in Kuhn's thesis.

[1] Kuhn once fascetiously remarked that there seem to be two Kuhn's, one known to himself, and the other, who he can never meet, known to his critics.

2.2.1 Incommensurability and Comparability - Both critics and friends have rendered Kuhn's thesis too strongly. The most common error has been to assume that Kuhn meant incommensurable paradigms to be incomparable on any grounds whatever (e.g. Scheffler (1967), Feyerabend (1970b, 1977), Rorty (1979)[1] Szumilewicz (1977), Moberg (1979)).

Feyerabend (1977) contrasts his view with Kuhn's. Denying that incommensurability requires the impossibility of rational comparison of paradigms, he maintains that all he ever claimed was the deductive disjointness of successive theories. Comparison by content is out, but other criteria are available, such as comparisons of form or value. Theory selection in revolutionary circumstances is guided by unconventional standards, which must be introduced by what amounts to clever propaganda, drawing on values which lie outside of the range of the debate.[2]

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[1] Rorty recognizes Kuhn's reluctance to endorse the position, but thinks that his interpretation of Kuhn is the more interesting one. While I don't deny that Rorty's understanding of the significance of Kuhn's work deserves attention, I will restrict myself to the problem which is close to Kuhn's heart: how can we give a rational account of scientific progress which is consistent with the history of science?

[2] Feyerabend (1977) describes his philosophical predecessor as Kierkegaard, though given Kierkegaard's criticism of rationality (1959) and praise of irrationality and subjectivism (1954, 1941), it is hard to see how this supports Feyerabend's claim that he has not abandoned rationality in the evaluation of scientific theories.

Feyerabend states that Kuhn, on the other hand, has a notion of incommensurability which precludes comparability. He thinks Kuhn believes that the interaction of a) deductively disjoint concepts, b) differing perceptions and c) differing methodologies results in incomparability. On the contrary, not only does Kuhn believe that theories can be compared despite (b) and (c), but he doesn't even support (a). Although Kuhn holds that intertheoretic comparison must have non-rational components (1970a, particularly in Sections X and XII), Feyerabend is mistaken for the following reasons: First, Kuhn is willing to consider puzzle-solving ability as a basis for inter-theoretic comparison (1970a, 1983b). If so, he (obviously) allows comparison across revolutionary change. Second, Kuhn believes that certain internal characteristics of theories, such as accuracy, consistency, scope, simplicity and fruitfulness can be used to compare theories, though they must be balanced against each other, and allow room for individual scientist's preferences (1977c, 1970a p 206). His allowance for individual preferences does not necessarily undermine rationality, since following preferences during revolutionary science can lead to a satisfactory new theory, whereas sticking to accepted methods can block consideration of anything outside of the current paradigm. Finally, Kuhn believes that partial communication can occur across the revolutionary divide. Partial communication undermines deductive disjointness.[1]

Given that the main proponents of incommensurability, Kuhn and Feyerabend, think rational comparison is possible, it would be question-begging to define incommensurability as the failure of rational comparability of successive theories, as found in Szumilewicz (1977), or as the absence of any objective standards of comparison, as in Moberg (1979).

2.2.2 Degrees of Incommensurability - Even some who recognize that Kuhn allows rational comparison of competing theories still interpret him too strongly. The most extreme case has Kuhn holding that all competing paradigms are necessarily incommensurable. This interpretatation is suggested by the following passage, which refers to Proust and Berthollet:

> ...the two men necessarily talked through each other, and their debate was entirely inconclusive (emphasis mine). (1970a p 132)

but most likely Kuhn means here merely that incommensurability, where it exists, is a necessary consequence of the conditions which lead to it. He also says, "the proponents of competing paradigms practise their trades in different worlds", but this must be taken with a grain of salt, given Kuhn's misgivings

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[1] Kuhn (1970b p 250) stresses that he has regularly talked of "partial communication" which is "a problem to be worked on, not elevated to inscrutability". See also (Kuhn 1970a p 149, 1977b and 1983a).

about talking about "change of world" (see A.I.2). Although he speaks of communication across revolutions as "inevitably partial" (1970a p 149), the context of his statement is a discussion of major revolutions such as those of Copernicus, Newton and Einstein. Elsewhere, Kuhn suggests that incommensurability is not the inevitable result of paradigm change:

> The normal scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with what has gone before (emphasis added). (1970a p 103)

As I noted above, it is only dislodgements of global theories which exhibit incommensurability.

Allowing that Kuhn does not believe that every case of theory change involves incommensurability, there is still the question of whether incommensurability is mere empirical fact, or a necessary feature of some types of theory change.[1]

Suppose that a given theory T, held by a scientist or group of scientists S, is replaced at some time by another theory T'. If S lacks the resources at the time in question to semantically compare T and T', we might want to say that T and T' are incommensurable for S at this time. Even if this were so, it is still possible that some other being,

[1] It is possible that Kuhn would see no difference. He appears to have taken a progressively weaker position on the necessity of incommensurability, with the strongest statements found in his (1970a).

or even S at some other time, might have the necessary resources. This sort of incommensurability is quite weak, being relative to certain available resources. This technique must apply to at least the two theories in question.[1] Furthermore, semantic comparability fails to just the extent that the required techniques are unavailable. Thus, the absence of a technique for comparison between competing theories is both a necessary and sufficient condition for their incommensurability.

A technique, as I understand it, is somewhat similar to what Rescher (1977) calls a "method", except that it is somewhat less general. Rescher describes two essential aspects of methods: teleology and generality. Both methods and techniques are justified instrumentally, i.e. in terms of their effectiveness in achieving some specific end. There are two sorts of generality required by methods. Only the first of these is required of techniques: methods and techniques are both inherently generic, being capable of repetitive application. The second characteristic of methods, not shared with techniques, is that they are (unlike talents) impersonal; a method will work the same way for anyone who uses it: it

[1] Kuhn originally thought that semantic comparison requires a general method, but under the influence of Stegmuller (1976b) he has recognized that all that is required is a technique applicable to the particular theories in question (1976, note 11).

can be written out as a set of explicit instructions which can be followed by any normal person. Techniques, on the other hand, must work reliably for anyone who has mastered them, but not everyone may be capable of mastering a given technique. We can say that someone has a technique if he has mastered it, so that he can reliably get similar results in similar situations.

Techniques are a kind of algorithm, since they are ways for producing a specific output for a given input. Just as not every algorithm can be implemented on a given device, not every person can master every technique. Algorithms are usually assumed, via Church's thesis, to be able to represent only those functions which are decidable by a Turing machine (Rogers 1967 pp 1-5, 18-21). If we assume Church's thesis applies to techniques, then for every technique there must be a Turing machine which gives the same results in each situation as the technique does. While it is difficult to see how this could be false, it is not obviously true, either. Fortunately, its truth turns out to be irrelevant to Kuhn's thesis (see the next sub-section). If we assume further that human beings are capable in principle of performing any procedure a Turing machine can perform, then for each technique a corresponding method is possible. The Turing thesis is almost certainly true.

A technique for semantic comparison must give a means for unambiguously correlating the languages of two theories in a way which preserves meaning. Discussion of the obstacles to this task, and the requirements for a successful correlation will be deferred to the last section of this chapter and to Chapter 5.

The degree of incommensurability depends on the difficulty in obtaining the necessary techniques for comparison. The following grades of incommensurability seem to capture the relevant alternatives:

Weak (relative) incommensurability: T and T' are weakly incommensurable for S at time t iff S has no technique at t to semantically compare T and T'.

Strong (absolute) incommensurability: T and T' are strongly incommensurable for S iff it is not possible (at any time) for S to obtain a technique for the semantic comparison of T and T'.

Logical (universal) incommensurability: T and T' are logically incommensurable iff there is no technique for the semantic comparison of T and T'.

Each of these three grades of incommensurability has a correspondingly strong form of the incommensurability thesis.

Once this ambiguity in incommensurability is clarified, we can describe the misunderstanding between Kuhn and his critics. The presumption that Kuhn must have meant logical incommensurability has led to unfair rejection of Kuhn's thesis (Watkins 1970, Kordig 1971, Scheffler 1967, Katz 1978, 1979). This presumption is probably based on the assumption that the capabilities of observers are irrelevant to objective science. I find this assumption dubious, since it is not at all clear how the concepts involved in a theory could exist without someone to conceptualize. Kuhn explicitly invokes this "subjective" element through his notion of a disciplinary matrix.

Although most commentators have assumed that Kuhn meant at least strong incommensurability, Kuhn (1970a pp 198-199) says that he does not claim that there is no recourse in cases of communications breakdowns, and that, indeed, recourse must be possible. Given the context of this statement, which specifically concerns comparisons of meanings, it is difficult to understand what Kuhn could have meant unless he believed that semantic comparison could be established somehow, though the means might not be readily available.[1]

2.2.3 Incommensurability and Incompatibility - A common objection to the incommensurability thesis is that two paradigms cannot be both incommensurable and incompatible. This objec-

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[1] Admittedly, Kuhn (1983a) still believes that certain aspects of translation must remain incomplete.

tion is raised by Watkins (1970). Kordig (1971) and Shapere (1966) also criticize Feyerabend on the same grounds. I will argue that the criticism has no force against Kuhn's conception of the incommensurability thesis, but might be effective against Feyerabend.

If incompatibility is logical inconsistency, then this criticism is a strong one. Watkins (1970) argues that if the Genesis account of Creation is incommensurable with scientific theories of evolution, then they have different universes of discourse, and cannot be inconsistent. No theoretically neutral observation could decide between the views, thus they could not differ on the facts. On the other hand, if two theories are logically inconsistent, there is some statement that one deems true and the other deems false. If this is to have any meaningful content, there must be some empirical test to decide which is correct. This would require semantic comparability.

Kordig argues that incommensurable paradigms cannot be incompatible because there is no place where they specifically disagree; if there were, this would constitute a neutral basis on which to compare the them. If such a neutral basis exists, then *a fortiori* the paradigms can't be incommensurable (1971 p 56). This objection was raised in response to Feyerabend's (1965b p 233) attempt to base incompatibility on non-isomorphism: that it might be possible to establish certain areas of local agreement where sentences are directly connected to observation procedures. Kordig correctly points out that this directly undermines deductive disjointness, Feyerabend's necessary and sufficient condition for incommensurability.[1]

A problem shared by Kordig's and Watkins' arguments is that it is possible that two consistent theories are jointly inconsistent but the inconsistency is not derivable from their conjunction. The Robinson Consistency Theorem (Chang and Keisler 1973 p 88) requires that if T is a complete sub-theory of T' and T'' in the intersection of the languages of T' and T'', then the conjunction of T' and T'' is consistent in the union of their languages. If there is no such complete theory, then the theorem cannot be proven, since it relies on the derivability of a contradiction. This seems to allow the possibility of underivable inconsistencies between some theories.

Given this possibility, incommensurable but inconsistent theories might exist. On the other hand, such cases seem

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[1] Feyerabend's conception of incommensurability is much more radical than Kuhn's. He often talks as if any change in the meaning of any term will change the meaning of all other terms in the theory (see, for example 1962, 1965b p 231, and 1975). This has come to be known as the thesis of radical meaning variance, which has often been confused with Kuhn's incommensurability thesis.

rather artificial, and it isn't clear how they could occur in scientific practice.[1] In any case, since scientific progress involves just the comparability of the empirically verified content of successive theories (see Chapter 3 below), and since at any given time the empirical ground consists of a finite set of measurements describable in the neutral language of pointer readings, the verified content of two theories is in principle comparable in some way.

The only way that two theories could be mutually inconsistent but incommensurable is if our articulation of the theories is not rich enough to allow us to derive the inconsistency. The extreme case of this possibility is the one mentioned above in which the inconsistency is not finitely deducible from the conjunction of the two theories. A less extreme case would be one in which the inconsistencies were sufficiently subtle that we lack the time or intuition to find them. In such cases, by hypothesis, we would not be able to recognize the inconsistency, consequently we would be unaware of the incompatibility. But if Kuhn's thesis is

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[1] The only case that I can think of which might involve this sort of incommensurability is the relation between corpuscular and plenum theories in physics (see, e.g. Hooker 1973). To the best of my knowledge, it is still an open question whether or not there is a common representation theorem for both types of theories. In fact, I don't think there is yet a representation theorem for field theories in general. To discuss this case in the depth it deserves would take me far beyond my present project.

relevant to rational accounts of scientific progress, the incompatibility he refers to must be fairly easily recognizable. Since deductive disjointness makes the incompatibility of theories intractably elusive, Feyerabend errs in basing incommensurability on it insofar as the incommensurability thesis is significant for scientific progress.

The incompatibility Kuhn is interested in is manifested when the application of a theory within the disciplinary matrix of another theory results in confusion about colleague's meanings, misinterpretation of the purposes of their experiments, and even the appearance of inconsistency or outright unintelligibility of some of their statements and actions. Such evidence allows us to recognize pragmatic incompatibility, which depends on the particular way scientists understand the theories they consider.

In many cases pragmatically incompatible theories can be reconciled by showing that the conflict can be avoided, either by restricting their applications to different domains, or else by using available techniques to reinterpret both under a common paradigm. To illustrate, consider a possible case of Poincare conventionality (see section 1.1.2 above): Restricting consideration to point particles and their trajectories, let T be the theory that the world has Euclidean geometry E and a universal inertial force field F. Let T' be the theory that the world has a Reimannian geometry R, and no universal inertial fields (i.e. F=0). Assume further that there is no empirical test for the existence of a universal inertial force field independent of T and T'. With a suitable choice of R, the two theories are empirically equivalent. On a naive interpretation, these two theories cannot both be accepted simultaneously, since one says F=0, while the other says F≠0. If there were some time when scientific authorities lacked the correct correlation of T and T', then the theories would have been incompatible at that time. Someone who tried to adopt both theories simultaneously would confuse his peers, and probably himself as well. In such a situation we might perform a sort of "therapy", which would involve pointing out that the two theories cannot be mixed freely, and that one or the other should be chosen, or that at least it should be made clear which theory is being used at a given time. The alternative is to give a sophisticated interpretation of the theories which renders them compatible.

Another example is the Schrodinger and Heisenberg theories of quantum phenomena. These were at first thought to be in competitition as alternative theories, and it was not at all clear how they could be compatible. Once they were shown to be equivalent, however, it was recognized that they were quite compatible. They became known as "pictures", that is, representations of a common idea. It is fairly easy to imagine what might have happened if their equivalence was not shown so quickly. They would have remained in competition as alternate theories.

Pragmatic incompatibility itself is rather unobjectionable, and need not be very significant. It does not entail incommensurability, nor is it particularly surprising.

Watkins' argument is undermined if the incompatibility of theories is pragmatic. Recall that his argument goes as follows: (1) If two theories are incompatible, then they must disagree on the truth or falsity of some statement. In this case, (2) they cannot be incommensurable. (3) If incommensurable, they must have different domains of discourse, and (4) they cannot be incompatible. Therefore, (5) no two theories can be both incommensurable and incompatible. Watkins' first line of argument, from (1) and (2) to (5) fails because premise (1) need not be true. It is quite possible for two theories to be pragmatically incompatible but not differ on the truth or falsity of any statement.

Watkins assumes that any test of the incompatibility of two theories must allow us to decide which is correct. This position is overly verificationist. While any incompatibility between two theories should be testable in some way, it does not follow that a test must allow a decision as to which theory is correct. Tests devised under each of two competing theories may appear to confirm their own theory and disconfirm the other. This situation gives at least a prima facie reason for doubting whether the two theories are compatible. This does not rule out the possibility that the theories can be rendered compatible at some later date.

Watkins' second argument, from (3) and (4) to (5) is also unsupportable. The opponent of the incommensurability thesis might allow that T and T' are pragmatically incompatible, but still require that incommensurability entails that they have different universes of discourse (premise (3) of Watkin's argument). If so, they could not really be in competition, and pragmatic incompatability is not important for theory change.[1] This reply will not work, since actual observations can be referred to demonstratively, or in terms of observational practices, which are theory-neutral. The reason why evolution and literalist versions of Genesis are incompatible is that they both purport to explain this world.

[1] Kordig (1971 p 56) also raises this objection against Feyerabend (1965b).

Watkins-style objections look in the wrong direction. What is incompatible about competing theories is not necessarily to be found in their content alone.

2.2.4 Summary - Kuhn's thesis has two components, incommensurability and incompatibility, which have an essential pragmatic aspect. Incommensurability arises because of the technical limitations on our ability to compare the contents of theories, due to the manner in which we grasp, or understand them. Theoretical incompatibility, on the other hand, arises from two possible sources: logical inconsistency or else practical problems in applying the theories in the same context. Only the latter is relevant.

Incommensurability of a purely logical sort is not relevant, since its historical role is doubtful. Nor does incommensurability undermine all possibility of intertheoretic comparison. Even semantic comparison between incommensurable theories may be possible, although not by the scientists holding the theories during the historical period in question. Although incommensurability of competing theories requires incompatibility (see 2.3.3 below), the converse is false: a person may be able to compare the semantic contents of two theories but still not be able to use the theories discriminately. On the other hand, incommensurability is likely to lead to the sort of confusion which makes theories incompatible.

2.3 Kuhn's Argument for Incommensurability

2.3.1 Theories - A theory in the Kuhnian sense is a mathematical structure interpreted in a specific way by a group of subjects in an historical context. Unlike purely formal accounts of theory, the interpretation of the structure is not just a correlation of symbols with their reference, but is given tacitly through the exemplars together with the preferred analogies, or models. Exemplars provide a connection between the abstract structure and the actual world by being successful applications of the theory which can be transmitted to students as practical examples to be imitated in other applications. Together, the analogies and practices learned through working out the exemplars determine the range of intended applications of the theory, albeit tacitly, and rather loosely.

The particular way the exemplars and preferred analogies determine the set of intended applications is important in determining the identity of the theory. It isn't enough to define a function from the mathematical structure to intended applications, since cognitively incompatible exemplars or analogies would require different theories even if the formal structures and intended applications were the same (unlikely though this eventuality is). We must consider, in identifying a theory, not only the function from mathematical structure to intended applications, but also the instantiation of this function.

When scientists learn a theory T they acquire a technique for determining which situations are among the intended applications of the theory. This technique can be called an <u>interpretation algorithm</u>. It need not give a decision for every candidate,[1] but it does select a set of situations from among those possible. Since the algorithm is not fully articulated, it is possible that scientists cannot give any deeper explanation for some of their choices than that they seemed right. Participants in a disciplinary matrix share at least the common parts of the matrix, which constrains the algorithms they can use. Nonetheless, there is still much room for individual variation, and for variation in a given individual from time to time.

A theory, then, can be defined as a pair of a mathematical structure and a class of (largely) compatible algorithms. This definition is still unnecessarily abstract, involving as it does an equivalence class of algorithms, which can't be directly embodied, rather than a particular algorithm,

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[1] In such cases the output of the algorithm is "no decision" or some equivalent.

which can. This abstractness can be avoided taking the interpretation algorithm of a theory to be embodied in the (partly duplicated) techniques of a community of scientists. There is no requirement that any particular scientist should be able to make a complete interpretation of the theory. The interpretation of a theory, and hence a theory itself, becomes essentially a social entity. This conception of a theory is quite in keeping with Kuhn's approach to science. Aside from allowing incommensurability and incompatibility to co-exist, it has the advantages of making theories much more like historical entities and allowing for the institutional nature of science.

2.3.2 <u>Compatibility of Theories</u> - Two competing theories T and T' will have some applications in common. This need amount to no more than that they attempt to describe the physical properties of some system, such as the Earth-Sun system, referred to demonstratively. Two theories are compatible unless they appear to differ on the truth of some statement about an application within their shared set of applications. The most obvious way that this could happen is that the theories are mutually inconsistent. A more subtle way theories can be incompatible is by appearing to differ on the truth of some statement because similar terms in some statement play different roles in the theories, as in the artificial example of section 2.2.3 one theory held that $F=0$, while the other held that $F\neq 0$. Although the two theories can be shown to be consistent, there is no immediate reason to think they are. If we apply the interpretation algorithm appropriate to one theory to the situation described by the other, we would evaluate the truth of the statement made in the second theory differently than if we use the algorithm appropriate to the first theory. It would be preferable to have a definition of theory incompatibility which grades from pragmatic incompatibility to logical incompatibility. The following definition has this property:

O: The <u>overlap</u> of two theories is the set of situations, as referred to demonstratively, which are among the intended applications of both of the theories.[1]

I: Two theories T and T' are <u>incompatible</u> relative to an interpretation algorithm A iff there is at least one uninterpreted sentence, well-formed in the languages of both T and T', which when evaluated using A is in the overlap of T and T' and the truth-value of the sentence under the interpretation A differs for T and T'.

In order to avoid problems with different versions of the same theory in different languages (e.g. Newtonian Mechanics

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[1] A proposition is in the overlap if its extension is a subset of the overlap.

stated in French and English), I assume that if there is an available translation for any term or sentence in the language of a theory then its image under the translation is also in the language of the theory. If A is an interpretation algorithm which appropriately interprets any structure (a universal interpretation algorithm), these conditions imply that T and T' are inconsistent. If not, it is possible that T and T' are consistent, but that one of them has been misconstrued through the use of an interpretation algorithm which is inappropriate in the context. For example, it might be legitimate to use A to interpret T and its consequences, but not necessarily T'. If so, it might be possible to take account of the different contexts. This can be done by interpreting both T and T' within a common context, or else using an A* which is different for different contexts.[1]

2.3.3 The Incommensurability Thesis - If two theories are compatible with respect to some interpretation technique A and have a non-empty overlap, they can be understood jointly by some person who has A, thus they are not incommensurable for that person. Consequently, incompatibility is a necessary condition for incommensurablity of overlapping theories. Since competing theories can be assumed to overlap, semantic

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[1] Both of these techniques are illustrated in section 5.2.1 for the example used in this section.

incommensurability is sufficient to establish Kuhn's thesis. I propose the following definition (keeping in mind that techniques are embodied in people or groups of people):

Two theories T and T' are <u>incommensurable</u> at time t iff:

1a) Given that A and A' are the interpretation techniques of T and T', T and T' are incompatible at t, relative to A and/or A'.

1b) There is no available technique at t for representing T and T' which is compatible with both A and A'.

2a) When interpreted with their own techniques, T and T' do not differ on the truth of any observational statement about any application they have in common.

2b) There is no available technique at t for interpreting T and T' in terms of underlying inconsistent theories on which T and T' are based.

Condition 1a ensures the incompatibility of T and T', while 2a ensures that a decision between the theories can't be made on their own grounds. Condition 1b ensures that T and T' cannot be understood in the same way, while 2b ensures that T and T' cannot be shown to presuppose inconsistent theories.

An omniscient being with a universal interpretation technique (a Watcher) would find that all of its theories are either inconsistent or compatible, and would experience no incommensurability. The possibility of Watchers shows that logical (universal) incommensurability is not necessary for any pair of theories. The Watcher's nature ensures that neither of the b clauses holds. It has both a fully articulated interpretation of any theory, and a full articulation of the nesting relationships of all theories. If any version of Kuhn's thesis is true, we are not Watchers.

2.3.4 <u>The Argument for Incommensurability</u> - Two competing theories which satisfy Kuhn's thesis must be incomparable on empirical grounds (embodied in condition 2) as well as on conceptual grounds (embodied in condition 1). This involves both incompatibility (the a clauses) and incommensurability (the b clauses).

The basic line of argument for condition 2 is that the history of science shows us that good scientists often differ on the significance of experimental evidence. If condition 2 holds, this sort of disagreement would be quite likely. It could be argued, then, that condition 2 is the best explanation for these differences. The explanation goes as follows: Given the under-determination of theory by its evidence, any theory can be saved against potentially falsifying evidence E by choosing suitable auxilliary hypotheses. Although this might result in a slight modification of the interpretation of the theory (by changing the set of intended applications), it will not affect the interpretation of the core of the theory unless E undermines one of the exemplars. This is quite unlikely for a mature science, since the exemplars are by definition well-confirmed. Any difficulties are likely to occur in applications which are extensions of the basic theory. The nature of the implicit, largely analogical reasoning from the exemplars to the more peripheral applications makes it more likely that this reasoning is faulty than that the core of the theory is false. (It will certainly be more suspect if only because the reasoning has been less explicit, and less closely checked.) The effect of this situation is the resistance of core theories to empirical falsification.

Although theories can't be compared directly through empirical evidence, it might be possible to compare the underlying auxilliary hypotheses required to protect them from potential falsification (Stegmuller 1976b). Kuhn (1976) doubts this can be done. The difficulty in responding to his challenge is evidence that at least weak incommensurabiity holds. To establish the stronger form, though, it must be shown that Stegmuller's solution can never work. There is some reason to believe this. The testing of auxilliary hypotheses itself involves auxilliary hypotheses, either forcing a retreat to even more basic theories, or involving the original theories. This seems to lead either to a regress or a vicious circle. The latter would be fatal. A regress would probably be cut off at some point because the required revisions are implausible. Nonetheless, one would like to know just where the cutoff would come. Furthermore, the well-known arguments against the observational-theoretical distinction suggest that a vicious circle is more likely than a regress.

The argument for condition 1a requires Kuhn's notion of a theory as something which is interpreted through the scientist's practical understanding based on standard examples. Given this notion, competing paradigms are always incompatible, since i) they attempt to explain the same evidence (where the sameness of evidence is established by demonstrative reference to the relevant observations and procedures), and ii) they must differ under some interpretation technique, or they would not be competing theories.

Since conditions 2a and 2b apply to all theories (if they apply at all), and condition 1a applies to any competing theories, incommensurable or not, condition 1b is the most significant condition for semantic incommensurability. Condition 1b is often violated by competing theories which satisfy the other conditions. This will happen whenever competing theories are embedded in a common encompassing discipline. For weak incommensurability, all that is required for this 1b to hold is that the competing theories have not in fact been reduced to a more encompassing theory. For example, 18th Century chemistry allowed miscommunication in applications of the terms 'mixture' and 'element', but when chemical concepts came to be understood in terms of more basic physical theories the different usages of the chemical terms could be distinguished by reference to the common physical theory. Strong incommensurability can hold only between theories which cannot be be embedded in some common theory, either because they have different presuppositions at the more basic level, or because there is no more basic level. Dynamical theories such as Newtonian Mechanics and General Relativity are possible candidates of this sort. If all sciences are reducible to basic physics, then basic dynamical theories are the only candidates for stong incommensurability. This is, I think, part of the explanation for why so much of the discussion of incommensurability has focused on the transitions from Aristotelian to Newtonian dynamics, and from Newtonian dynamics to relativity and quantum theory.

Crucial to (1b) is the fact that the meanings of the terms in disciplinary matrices are defined only implicitly; no strict definition, which would give the sense of the term, is given. Kuhn (1983a), in replying to Lewis's (1970, 1972) argument that theoretical terms can be defined by Ramsey sentences, says:

> If there is one and only one referential realization of a given Ramsey sentence, a person may of course hope simply by trial and error to hit upon it. But having hit upon a Ramsey-defined term at one point in the text would be of no help in finding the referent of that term in its next occurence. The force of Lewis's argument depends therefore on his further claim that Ramsey definitions determine not only reference, but also sense, and this part of his case encounters difficulties closely related to but even more severe than the one just outlined.

In brief, inter-theoretic comparison of concepts cannot be construed in purely referential terms, but also requires a common means of grasping the concepts involved. These means cannot be established by purely formal methods, due to the unarticulated status of much of our understanding of the terms we use.

Several candidates have been proposed for a neutral language. One possibility is a neutral observation language common to all theories. The existence of a pure observation language is now considered highly doubtful, since there seems to be no distinction between theoretical and observational terms which is not theory relative (for a discussion of this see, for example (Suppe 1977), and (Stegmuller 1976b pp 23-29)).

A second possibility for a neutral language is the language of experimental and observational procedures. If we could give operational definitions of theoretical terms such as 'mass' we could compare shifts of meaning across revolutions. The problem with this method is that i) operational definitions are seldom if ever entirerly explicit, because of the way scientists learn theories by practicing with exemplars, ii) new theories and extensions of old theories both provide new methods for operationally defining theoretical terms, and iii) it is the relation between the terms of a theory and observational procedures which is in question as far as matters of interpretation are concerned, so procedures themselves can't provide a neutral ground unless we assume that there is a way to represent that relation in a theory-independent way, which is just to beg the question.

A third possibility is to express the differences of meanings of the terms of competing theories in ordinary non-technical language, which is presumably theory-neutral. There are three problems with this approach. First, it is not obvious that ordinary language is genuinely theory-neutral. Our usual way of speaking probably embodies all kinds of pre-scientific and defunct scientific notions (for example, we still talk of the sun rising). It is often necessary to bend conventional usage quite a bit to express new scientific theories. Second, there is no particular reason to assume that ordinary language is sufficiently precise to articulate the differences in meaning between the terms of highly refined theories. It is just because of the imprecision of everyday language that science students must learn theories by way of exemplars: the student must actually formulate new concepts which can't be conveyed directly. The third problem is that correct usage of the terms of ordinary language is determined by authorities. In the case of terms of ordinary language which are the subject of scientific theorizing the scientists are the authorities. If scientists disagree about correct usage there is no higher authority to appeal to.

A fourth possibility is based on Katz' (1978, 1979) effability hypothesis. According to him, all concepts are innate, so any problems of translation are merely practical. The problem is that incommensurability may be a practical problem. Even if the effability hypothesis is true, we still might be incapable of translation. The posession of concepts is of little value if we can't use them appropriately. I see no more reason to say that the innateness of concepts makes us capable of translation than that molten glass is capable of being broken. We may have the potential to translate, as molten glass has the potential to solidify and become breakable, but potential is not capability.[1]

Summarizing the argument: general revolutions in mature sciences involve strong incommensurability because 1) the underdetermination of theory by evidence allows revisions

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[1] For a more detailed discussion, see Appendix II.

in the interpretation of a theory to protect it from falsification, 2) the theory-ladenness of observation prevents the exclusion of some sorts of revisions by appealing to more basic theories on which the competing theories depend, since revisions also affect the interpretation of the lower-level theories, 3) the pre- and post-revolutionary paradigms are not merely conventionally or notationally different, since they are incompatible, and 4) the four possibilities for a neutral language given above are exhaustive, so there is no neutral way to represent both theories.

Although I agree with the first three premises of the above argument, I find (4) questionable. It seems possible to me that although there may be no way to compare theories at a given time, using the resources available at that time, it might be possible to create a suitable technical language which can represent both theories adequately for their comparison. The appropriate language for each case will be determined by differences in the presuppositions between the two disciplinary matrices. In order to allow comparison, these presuppositions must be made explicit. Since that can not be done within the current version of each matrix, new conceptual resources must be created which are adequate to the task.

CHAPTER 3

MEASURES OF SCIENTIFIC PROGRESS

> A new scientific theory does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it.
>
> \- Max Planck

3.0 Introduction

The traditional view of scientific progress requires cumulativity, the justification of which requires reducibility. Reducibility is incompatible with strong incommensurability. So, if strong incommensurability holds, the traditional view of scientific progress is unworkable.

I will argue that progress can be measured by means other than direct comparison of theoretical content, but it cannot be entirely independent of content. If scientific progress is an entirely internal property of theories, and strong incommensurability holds, either both truth and empirical adequacy are irrelevant to science, which is implausible, or else standards of progress are theory relative, and loops of successive theories can be progressive, which is absurd. Barring some unmentioned criterion of progress which is proper to science, and does not require semantic comparability, but is not theory relative, strong incommensurability rules out scientific progress. While I cannot reject the possibility of some such criterion, I find it unlikely.

3.1 Cumulativity

Kuhn, Laudan and Feyerabend all argue that the requirement of cumulativity cannot be met, and that it should be dropped from considerations of scientific rationality. Laudan (1977 pp 147-150) argues that cumulativity is directly contradicted by the historical evidence. I think, on the contrary, that the standard view can accomodate Laudan's examples if we carefully distinguish between what is perceived as progressive and what is in fact progressive. Although genuine problem-solving capability, not perceived solutions, is the basis of progress in the traditional view, we can't evaluate problem solving capability directly. All we can require is that the dislodging theory have a <u>reasonable expectation</u> of duplicating the explanatory successes of its predecessor. Of course, judgements of problem-solving capability don't guarantee actual problem-solving capability, on which progress depends. Almost inevitably, a new theory will be adopted before it has achieved successes in all of the areas a well-established predecessor has accounted for. If perceived success were the appropriate indicator of theoretical progress, few new theories would be adopted.[1]

Laudan's interpretation of cumulativity requires that <u>all</u> of the successes of the predecessor theory be duplicated by the successor. He gives strong textual evidence that this is, indeed, what many traditional philosophers of science have required. He mentions Whewell, Peirce, Duhem, Collingwood, Popper, Reichenbach, Lakatos, and Stegmuller:

> ...these thinkers argue that a necessary condition for one theory, T', to represent progress over another, T, is that T' must solve <u>all</u> the solved problems of T. (emphasis in the original) (1977 p 147)

There is little doubt that such a high level of success is seldom achieved, at least not until a theory has been accepted for some time.

Laudan suggests several counter-examples to cumulativity: the shift in geological problems in the early nineteenth century to a restricted set of problems in stratigraphy; the failure of Newtonian optics to account for the problem of refraction (explained by Huygen's optics); the failure of caloric theories to explain heat convection

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1 Feyerabend (1975 p 55ff) notes that "no theory ever agrees with all of the facts in its domain, yet it is not always the theory which is to blame. Facts are constituted by older ideologies, and a clash between facts and theories may be proof of progress." His use of 'fact' is, of course, non-standard.

and generation; problems in chemistry solved by elective affinity theory, but not Dalton's atomic chemistry; the failure of Franklin's theory of electricity to explain the repulsion of similarly charged particles, which Laudan says was solved by the earlier vortex theories, and remains unsolved today. Laudan proposes that such situations can be accounted for by allowing for the relative importance of various empirical problems:

> Knowledge of the relative weight or the relative number of problems can allow us to specify those circumstances under which the growth of knowledge can be progressive even if we lose the capacity to solve certain problems. (1977 p 150)

I will argue later (section 3.5) that this view itself requires semantic comparability. For the moment I wish to reject Laudan's counter-examples.

The fact that some problems resist immediate solution does not require that the new theory cannot solve them eventually; the difficulty might be due to practical problems in applying the new theory, or errors in the old theory. A case could be made that elective affinity did not really solve many of the problems Dalton's theory had difficulty with, and that scientists who believed the solutions were adequate were simply mistaken. Similarly, the vortex theory of electrical repulsion left much to the imagination. Although both of these explanations were accepted as adequate, the possibility is surely open that the scientists who accepted them were mistaken. From a modern perspective it is also doubtful that Newton's theory of light was a real advance over Huyghen's wave theory, or that the caloric theory of heat was a real advance.

The evidence we have available does not tell us directly whether or not a given problem is in _fact_ solved, but only tells us whether or not scientists have at some time _judged_ it solved. What should accumulate across theory changes is genuine problem solving capability, not accepted solutions.

There are other cases, like the introduction of stratigraphic uniformitism in geology, in which the new theory fails to even address problems which the old theory solved. The theory of stratigraphic uniformitism replaced the view that geology was primarily the result of catastrophic events such as floods, earthquakes and vulcanism. Catastrophism was influenced by the importance of catastrophes in the Bible, as well as by the patent visibility of their effects. Uniformitism, on the other hand, involved the more subtle perception that gradual, long-standing cyclic processes of weathering and sedimentation could result in major alterations in geomorphology. Once the existence of these processes was accepted (which involved accepting a much longer geological time-scale than had been assumed before), the attention of geologists shifted to details of stratigraphy which had been overlooked under the old paradigm. Uniformitism did not deny the existence of catastrophic processes, but down-played their importance.

Laudan argues that uniformitism is not a cumulative advance, since catastrophism solved problems which uniformitism did not. If we look at the domain in which catastrophism is successful and uniformitism is not, however, we find that there is no reason to believe that uniformitism applies. The nature of the uniformist paradigm determines the domain of its application. Although it is true that geologists generally lost interest in the catastrophe paradigm, it is questionable whether it was in genuine competition with uniformitism outside of the domain of the latter. The appropriate domain for comparing uniformitarian geology with its predecessor should be restricted to the domain that they both have in common.

The "failure" of uniformitism to explain facts outside its domain of application is hardly relevant to its success. Laudan (1977 p 149) admits that the two theories have quite different problem areas. Even though one did push the other out of active consideration by most scientists there was no theory-dislodgement in anything other than a sociological sense. Uniformitism was progressive in its own domain, and did not require the rejection of older theories in other areas. If there was a paradigm shift in Kuhn's sense, it was localized to the domain of sedimentary processes.

A different argument against cumulativity is raised by Feyerabend (1975). He points out that the numerical predictions of a dislodged theory are often not duplicated exactly by its successor. Consequently, the old theory can't be reduced to the new one. For example, Newton's predictions for falling objects do not agree numerically with Galileo's, because of the finite radius of the Earth. Feyerabend also points out that the experimental data used to confirm many theories does not actually fit the predictions of the theories within the limits of experimental error (1975 Chapter 5). There is an "ocean of anomalies" which surrounds every theory. He suggests that the sloppiness of fit between theory and its evidence allows us to focus on the evidence which fits the new theory, and ignore the rest in an *ad hoc* manner (1975 pp 176-179). Rather than being cumulative, theory changes are merely selectively attentive.

This argument strikes me as weak. Focusing on one area in the domain of the old theory is not necessarily illegitimate. A new theory can be progressive in some restricted area, but not generally. Secondly, sloppiness of fit doesn't rule out cumulativity. If data are indeed selected in a biased manner, this does not preclude a rationale for doing so. This rationale can be independently tested. Lastly, disagreements in numerical predictions as slight as those between Newton's and Galileo's were too small for the available experiments to detect. It would be unreasonable to require cumulativity of content beyond the limits of experimental accuracy, since this would take us beyond the empirical basis for our theories. Even if numerical differences exceed experimental error, approximate reduction might be sufficient. Schaffner (1967) gives an account of reduction which takes this into consideration.[1]

Cumulativity, as I see it, requires that the actual explanatory successes of the dislodged theory must be duplicated by the succeeding theory (within the limits of experimental accuracy) over the (non-empty) domain of intended applications the two theories have in common. Cumulativity is an ideal which is not always recognizable in practice. All we need to be justified in accepting a new theory is a reasonable expectation of cumulativity. The essential fallibility of science can allow nothing else. The expectation of cumulativity would be unreasonable if it could in principle never be verified.

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[1] See A.III.5 for discussion. Moulines (1976) has developed a more detailed theory of approximate reduction which deals with the particular case in question.

3.2 Progress and Reduction

In the context of the Received View, if T and T' are respectively a theory and its successor, there has been scientific progress only if the highly confirmed empirical content of T has been reduced to the empirical content of T'. Science progresses because it is cumulative: the highly confirmed content accretes across revolutions as well as during normal science. If the empirical contents of theories can't be compared, progress across revolutions can't be recognized, since we cannot find evidence for a reduction relation between the incomparable contents of T and T'.

Strong incommensurability is sufficient to deny us access to any reduction relation which might exist. Even after the problem of establishing the existence of semantic comparability and the possibility of reduction is solved it may still be difficult to find a reduction. The practical problem, though it might reflect on the usefulness of the Received View as a guide for scientific research, presents no insurmountable difficulties for the truth of that view. Nonetheless, it may still be advisable under many circumstances not to assume comparability and/or reducibility. Non-traditional conceptions of scientific progress might prove valuable even if they must ultimately be justified in terms of increasing content.

As discussed in section 1.3, there are several reasons for believing that a hard and fast distinction between theoretical and observational terms, on which the Received View rests, cannot be motivated, and that the Received View should be rejected. This failure of the Received View doesn't require, though, that the traditional account of progress is unsupportable. If it is possible to maintain some sort of distinction between theory and data relative to a theory,[1] and it is possible compare the data, as conceived under the two theories, progress can still be defined in terms of the reducibility of the empirical contents of T and T'.

3.3 Theory Reduction

Since theory reduction is so central to cumulative progress, it is useful to examine it in more detail, and determine whether or not it really does require semantic comparability. Schaffner (1967) has characterized four different approaches to reduction found in the literature. He also provides a general paradigm of reduction which accomodates some of the difficulties raised by Kuhn and Feyerabend. One important

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[1] Van Fraassen, for example, argues for a distinction at the level of phenomena rather than at the level of vocabulary (1980). Stegmuller (1976a) and Sneed (1971) argue for an observation-theory distinction which is theory-relative. Since van Fraassen's distinction makes what is considered observable dependent on current theory, his distinction is theory-relative.

result of Schaffner's work is a clarification of the issue of ontological reduction in terms of synthetic identities. This notion will play a central rôle in Chapter 5.

In Appendix III I descibe the various forms of reduction and show that each requires semantic comparability. Since the cumulative view of progress requires reduction, it also requires comparability. Are there plausible accounts of scientific progress which do not?

3.4 Kuhn's Conception of Scientific Progress

Although he has since become more optimistic about the possibility of content comparison across radical theory change (1970a p 203, 1976, 1983), Kuhn originally thought that content comparison was either impossible (1970a p 133), partially impossible (1970a p 149), or very difficult (1970a pp 151-153). Consequently, he looked for grounds other than reducibility to explain revolutionary progress, and even suggested that we might have to give up the notion of progress except in normal science.

Kuhn's normal science inevitably fits the traditional notion of progress perfectly, since it is our exemplar of scientific activity. Despite the fact that revolutionary science doesn't resemble normal science, Kuhn argues that the winners of theory clashes will see their side as progressive, even if they don't see their work as merely an extension of the new theory. Unless we assume that innovative theoretical scientists are hopelessly naive about progress, how can these two views possibly be reconciled? Kuhn suggests the rather unsatisfying conclusion that "scientific progress" might come to be seen as redundant: science is progressive just because it is science (1970a pp 160-161).

Kuhn notes that not all revolutions in science are scientific revolutions. If non-professional authority arbitrates debates, the outcome might still be a revolution, but it wouldn't be a scientific revolution. This would rule out the adoption of Lysenkoism in the Soviet Union, or an adoption of creationism in American biology imposed by fundamentalist politicians. The power to choose among paradigms, says Kuhn, must be vested in a special kind of community (1970a p 167). Membership in this community requires 1) the scientist must be concerned with solving problems about the behaviour of nature, 2) this concern may be global, but the problems must be problems of detail, 3) the solutions must be widely acceptable in the community, and 4) the community must share standards of evaluation.[1] Kuhn draws these condi-

[1] Kuhn (1983b pp 567-569) has now concluded that necessary and sufficient conditions for membership in a discipline cannot be given. Disciplines, he says, must be distinguished in contrast to other disciplines. Even so, we need some basis for this distinction.

tions from the practice of normal science, since as the exemplar of scientific activity it determines the conditions for membership in the scientific community. Since members of the community will deem any newly adopted paradigm progressive (the winners are hardly likely to admit that their changes are regressive!), if they are the ultimate arbiters of progress, any revolution will be progressive.

This is hardly satisfactory, as Stegmuller points out (1976a pp 220-221). In normal science progress occurs in two ways, by an expansion of the applications of the theory, and by further restrictions on the theory through special laws and constraints. The dual of progress, "setback", occurs when physical systems are excluded from the applications of the theory, or when special laws or constraints are rejected. There is no counterpart to normal science setbacks in the Kuhnian conception of revolutionary progress. Specifically, there are no criteria for differentiating between cases of theory dislodgement which are progressive, and ones which were overly hasty or simply mistaken. Even if the scientific community has within its power to dictate change, this change is not necessarily progressive. Although the scientific community is probably our best detector of scientific progress, progress depends on something external to that community.

Kuhn (1970a p 171) suggests that scientific evolution may have proceeded with no set goal, no fixed notion of scientific truth. Theoretical change in science is away from anomalies rather than towards some goal. It may be that scientific progress is not a consequence of rational activity directed towards some uniform goal, but it doesn't follow that there needn't be some property of theories which increases as science progresses. It seems that any type of progress must have some monotonicly increasing property, or else we run the danger of progressive circular succession.

This doesn't require, though, that we are able to pursue the maximization of this property as a goal. In order to determine that progress has occurred in science, we need to know whether or not some basic property P underlying progress has increased. If strong incommensurability holds, and P involves semantic comparability, we cannot determine whether progress has occurred. If so, it makes no sense to hold P as a goal. There may be a maximization of P, but we cannot determine this unambiguously.

Kuhn mentions two criteria which are used for theory evaluation: first, the new theory must seem to resolve some outstanding and generally recognized problem, and, second, the new theory must promise to preserve and extend a relatively large part of the problem solving ability accrued to science through its predecessors (1970a p 169). With two words, "seem" in the first condition, and "accrued" in the second, he gives the game away. The use of the word "seem" suggests that the scientific community might be mistaken in its acceptance of a new paradigm. The requirement that problem-solving ability should accrue suggests that progress through revolutions is cumulative. Kuhn hastily retreats from this position, pointing out that the ability to solve problems is neither a unique or unequivocal basis for paradigm choice. He does not, however, offer anything else,[1] and suggests that the investigation of problem-solving will lead to a refined notion of scientific progress which does not require, and might be incompatible with, the notion that scientific revolutions carry science closer and closer to the truth (1970a p 170).

3.5 Progress without Commensurability?

Larry Laudan (1977) has pursued this problem oriented approach to scientific progress. He attributes to scientific endeavour the goal of solving empirical problems.[2] Scientific

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[1] Kuhn does mention several internal properties which can be used to compare theories, but these allow room for individual scientists preferences, and presumably vary systematically from one paradigm to another. They cannot, therefore, be used for an objective measure of progress.

[2] As Feyerabend (1981) points out, both he and Kuhn had explored this approach earlier, contrary to Laudan's claims.

progress is achieved by maximizing explanations of empirical problems while minimizing anomalies and conceptual problems generated in the process. Rational choices are those which are progressive in this sense. He suggests that this definition of scientific rationality does not presuppose anything about the veracity or verisimilitude of the theories we judge to be rational or irrational (1977 pp 124-125). Even if he is correct about this, I believe that it must still be in principle possible to compare content.

Laudan has two arguments which he believes show that even if the incommensurability thesis is true, scientific progress as he defines it is possible. The first he calls the argument from problem-solving. In order that semantic incomparability does not arise at the level of problems, we must first explain how we are able to talk about the same problem within different theories. Laudan suggests that we can speak of the same problem if the theoretical apparatus we use to characterize the problem is different from the theories we use to solve it (1977 p 143). For example, the problem of relating the incident with the reflecting angle of light can be specified in geometrical optics, though various theoretical explanations of the problem have differed widely. Laudan recognizes that many problems cannot be characterized except under the theory that solves them, but problems which can be independently characterized can be joint problems of competing theories (1977 p 15, p 143).

There are two problems with this argument. First, it is not obvious that for all theories, especially sufficiently general ones, which purport to encompass all of the laws of nature (such as the mechanical world-view), there are any problems which can be independently specified; the most disturbing cases are of this sort. Second, if two theorists can agree that they are tackling the same problem, it is not at all obvious that they will be able to compare solutions. If the incommensurability thesis is correct, each side will not even understand the other side's solution, or recognize it as a solution (see Kuhn 1970a p 108, p 154). The inability to compare solutions to problems undermines Laudan's conception of progress as much as the inability to compare problems.

If an explanation were nothing but a deduction of the explananda from a theory, as Laudan seems to think, then we would have an objective way of determining whether a given theory solves a certain empirical problem. This notion of explanation, though, is at once too strong and too weak. Few acceptable solutions to empirical problems come even close to deductive adequacy. Given a specific theory, which can be interpreted only through largely unarticulated auxilliary hypotheses which represent the facts of scientific practice, the resources required for a strict deduction are simply not available. If we look at actual cases of scientists attacking the same problem from different paradigms, we find there is often disagreement over what constitutes a solution.

One example of scientists rejecting one another's explanations is found in the history of the controversy between mechanism and animism. During the period in which the mechanical philosophy was in ascendency, Aristotelian animistic explanations of biological processes were berated as trivial or tautological (King 1972 esp pp 12-15, Brown 1974 p 186). Descartes was of the opinion that vitalistic explanations were entirely superfluous (1972 p 113). He was quite disdainful of vitalistic explanations of non-cognitive activity of the body:

> Admittedly, it is hard to believe that the mere arrangements of the organs is sufficient to produce in us all the movements that are not determined by our thoughts. That is why I shall try to prove it here, and to explain the whole machine of our body in such a way that we shall have no more reason to think that our soul excites the movements - those which we do not experience to be presided over by our will - than we have to judge that there is a soul in a clock which causes it to show hours. (1972 p 115)

It is interesting that Galen, who favoured vitalistic explanations at a much earlier date, thought much the same thing about mechanistic explanations:

> Here, then, we must praise Epicurus for the respect he shows towards obvious facts, but find fault with his views as to causation. For how can it be thought otherwise than extremely foolish to

suppose that a thorn which we failed to remove by digital traction could be drawn out by these minute particles? (1916 p 86)

Animists who followed the period during which the philosophy of Descartes and Gassendi was predominant accused the mechanists of using viciously circular theories which could not provide adequate physiological explanations (Moravia 1978, also Brown 1974 pp 45-60). The problems which doctors deal with are presumably defined in medical practice, common to both the mechanists and animists. In fact both groups shared the same therapeutic practices. Nonetheless, they did not recognize each others methods, let alone their solutions.[1]

Laudan calls the second argument the argument for progress. It is of particular interest, since it attempts to retain theory comparability despite radical semantic incommensurability. According to the argument, even if two theories or research traditions cannot be said to deal with the same problems, they can still be compared with respect to their progressiveness or regressiveness (as measured by the number of empirical problems solved, while minimizing anomalies and conceptual problems). This comparison can be carried out even if the research traditions are utterly

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[1] For an intriguing account of how Descartes rendered long-standing animist ac counts mechanistically kosher, see Hall (1970 pp 53-79). Descartes found the details of the animist descriptions satisfactory, but did not accept their explanations, or at least their language.

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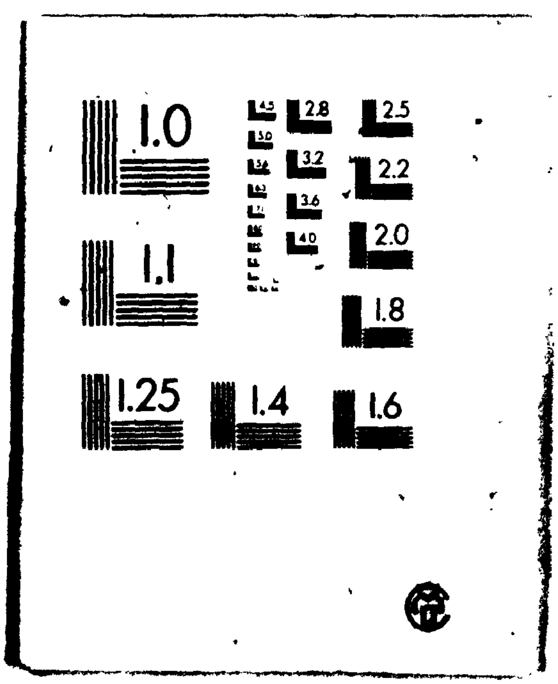

incommensurable with respect to the substantive claims they make about the world, since all we need to compare are numbers (Laudan 1977 pp 145-6).

The problem, of course, is who counts solutions, the partisans of one paradigm, or the partisans of the other? Or, perhaps some neutral body of historians of science? There is no reason to believe that all groups will come up with the same answer, nor is there any reason to expect any of these groups to accept the authority of one of the others, since each believes that they are right. Even if we could come up with some reliable way of enumerating problems solved by a given theory, it is not at all obvious that the result is comparable across theories in the way required for accounting for scientific progress. It might be rational for a chemist to become a geolgist because research in geology is more progressive (in the sense of problems solved) than chemistry, but it would not be rational to replace chemistry with geology.

Laudan suggests that his method of comparison could be extended to any internal criterion for theory progressiveness. On the contrary, I think that the objections to Laudan's notion of progress can be extended. If radical incommensurability holds, there is no guarantee that the measures of progressiveness are comparable, since the theories involved may have entirely different subject matters. The possibility of semantic comparison is essential to any meaningful comparison of theories. The fact that two theories are in competition does not, in itself, guarantee that they have the same subject matter. The competition might be quite unrelated to scientific considerations (e.g. creationists who regard evolutionary theory as faith-destroying). The necessity of semantic comparability, of course, does not rule out a variety of methods for evaluating scientific progress. But by themselves they are not enough; the methods of evaluation of scientific progress are not necessarily constituitive of scientific progress, and perhaps in fact never are.

The cumulative view of scientific progress requires semantic comparability, but so do alternative views of scientific progress. Nothing is gained in this respect by rejecting the traditional view of science.

3.6 Feyerabend on Scientific Progress

Feyerabend (1975) argues that every methodological rule is violated at some time or another in the history of science. He claims further that this is necessary for the growth of knowledge (p 23). By-and-large I agree. Scientific discovery should not be limited by prior conceptions of what is right, if for no other reason than because false or incoherent theories can contribute to eventual progress in science. What I do disagree with is Feyerabend's view that any innovation which eventually contributes to scientific pogress is itself progressive.

Feyerabend argues that standards for scientific progress stifle innovation, and, if strictly adhered to would make scientific progress impossible. I agree with the first thesis, and, to a limited degree with the second. Almost any view expressed strongly, no matter how bizarre, can stimulate progressive responses.[1] On the other hand, I think that there must be some standard for scientific progress which applies to all scientific theories. (This standard might have a number of components.) If, as Feyerabend argues, any theory is potentially progressive (rather than merely capable of stimulating progress), and any standard of progress is acceptable (1975 p 27), it is possible to have a theory T' replace T, and later T replace T'. Any adequate account of scientific progress requires the monotonic change of some property or properties.

Feyerabend seems to recognize the necessity of a monotonic standard of progress in his appeal to hedonism:

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[1] For example, current Creationist attacks on evolution have helped to force biologists to re-examine the foundations of evolution.

> It seems to me that the happiness and the full development of the individual human being is now as ever the highest possible value. This value does not exclude the values which flow from institutionalized forms of life... It rather encourages them, <u>but only</u> to the extent that they can contribute to the advance of some individual... Adopting this basic value we want a methodology and a set of institutions which enable us to lose as little as possible of what we are capable of doing and force us as little as possible to deviate from our natural inclinations. (1970c p 210)

On this view, humanitarian values, not truth or empirical adequacy, provide the ultimate basis of scientific progress.[1]

Progress in science does not imply progress towards humanitarian goals, nor vice versa. This is not to say that humanitarian considerations can not and should not influence progress in science. Although the word 'progress' is loaded with good connotations, it is not analytic that scientific progress is good for society, or even good for science itself. Likewise, progress in integrating science with other areas of intellectual endeavour, such as religion and philosophy, does not necessarily represent scientific progress, as

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[1] Hooker (1972) remarks that Feyerabend's view of science is highly humanistic, and relates Feyerabend's advice to abandon his philosophy of science if it was not a guide in his life. This standard presumably extends to Feyerabendian evaluations of science, and does not strike me as irrational, but it does seem likely to cause confusion. Rather than consider any influence on the acceptance of scientific theories to be necessarily scientific, I think it is preferable to distinguish scientific and non-scientific influences, keeping in mind that the latter are not necessarily undesirable or irrational. Scientific advances are not <u>a priori</u> good from a humanistic perspective, as Feyerabend's criteria of progress would make them.

Laudan (1977 pp 61-64) and Feyerabend (1975 Chapter 4) argue. Science is a rather restricted intellectual endeavour which is only a part of human activity. Despite its ascendent role in current culture, scientific progress is not the same as progress in other intellectual areas, or of human development in general. It is possible that scientific progress could be detrimental to progress in a wider context.

3.7 Summary

Assuming that the institutional function of science is to expand our knowledge of the natural world, progress in science requires the accumulation of justified true belief, even across revolutionary changes. This requires the reduction of the knowledge contained in the old theory to its successor, which in turn requires the semantic comparability of the two theories, or, at least, the justified belief that such reduction is possible.

Alternative views of science either misrepresent the nature of science, or else focus on the indicators of progress, rather than progress itself. Kuhn's account of progress is either inconsistent, confuses the signs of progress with progress itself, or else gives up the notion of scientific progress altogether. Laudan's attempt to base progress on problem-solutions does not, as he believes, circumvent the incommensurability problem, and, inasmuch as his views are correct, his view really reduces to the more traditional view of progress as an accumulation of knowledge (in the form of problem-solutions). Feyerabend's position that there is no standard for scientific progress undermines the independence of science as an institution, an effect he desires, but whose results can be achieved by less drastic means. His attempts to subsume scientific progress under general human progress tends to blur the special characteristics of the scientific endeavour.

Cumulativity is essential to scientific progress. Since it is undermined by the strong version of the incommensurability thesis, either science does not progress across revolutions, or else the strong version of Kuhn's thesis is false.

CHAPTER 4
THE STRUCTURALIST APPROACH

> But who will lead me into that still more hidden and dimmer region where Thought weds Facts, where the mental operation of the mathematician and the physical action of the molecules are seen in their true relation? Does not the way to it pass through the very den of the metaphysician, strewed with the remains of former explorers and abhorred by every man of science?
>
> Clerk Maxwell
> (1831-1879)

4.0 Introduction

Although the Structralist Approach to scientific theories developed independently of the work of Kuhn and the historians (as Stegmuller stresses in his more recent writings (1979a, 1979b), it offers a powerful method for explicating and dealing with Kuhn's ideas, including the distinction between normal and revolutionary science, the resistance of theories to falsification, the theory-ladenness of observations, various aspects of theoretical holism, and the comparability of paradigms. Nonetheless, despite claims to the contrary (Stegmuller 1976a, Stegmuller 1979b, Sneed 1976, Balzer 1979), the intertheoretic reduction relation developed by Sneed and Stegmuller is not adequate to meet Kuhn's argument for incommensurability. Stegmuller (1979a section 11) has recognized the difficulties involved, but remains optimistic that further extensions of the Structuralist approach will be able to resolve them.

I will argue, on the contrary, that there is a fundamental flaw in the Structuralist treatment of theories which renders it incapable of adequately representing, let alone resolving, the problem of semantic incommensurability. As Stegmuller (1979a, 1979b) acknowledges, the Structuralist approach does not require a Kuhnian view of science, and Kuhn's views must be supported further by historical evidence. This is in line with the dual historical and metaphysical arguments for Kuhn's position. I believe that the failure of the Structuralist approach to completely explain Kuhn's views stems not only from the need for additional historical evidence, but also from the inability of the Structuralist approach to deal with how scientists grasp theories, which is essential to the argument for incommensurability given in section 2.3.4 above.

This chapter explains the Structuralist approach, contrasting it with the Received View, and showing how it can be used to represent and justify many of Kuhn's ideas. Various reduction relations are described, and the Structuralist formulation of incommensurability is discussed. In the final section I show why these can not deal adequately with the incommensurability problem.

4.1 The Statement vs. Non-Statement Views

The Structuralist approach is an application of informal set theory to axiomatizations of scientific theories, analogous to the Bourbaki program in mathematics. Stegmuller (1979a p 1, pp 4-5) considers it to be an extension of that approach. It has two stages, the first of which is the direct application of the Bourbaki methods to the description of the purely mathematical aspects of physical theories, begun by E.W. Adams and P. Suppes.[1] The second stage, begun by Sneed (1971), considers the relations of a physical theory to outside objects by adding an informal semantic theory with pragmatic aspects (Stegmuller 1979a Section 2). This stage represents an extension of the Bourbaki approach, whereas Suppes' work was carried out within the limits of the Bourbaki approach. The Sneed approach should be contrasted with the formal language approach of Carnap, which attempts to axiomatize theories in a precisely defined formal language.

Sneed's refinements to the Suppes approach are designed to meet Schaffner's objection to Suppes reduction. Since Suppes' work is restricted to the axiomatization of the mathematical part of physical theories it isn't an advance

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[1] See A.III.3 for a description and criticism of their approach to inter-theoretic reduction, and (Stegmuller 1979 Section 1) for the relation of the Suppes approach to the Bourbaki approach.

over axiomatizations in formal languages, except that the informal approach is more practical to carry out. It is at best equivalent to the formal-language approach,[1] and fails to put as strong conditions on inter-theoretic reduction. The Sneed-Stegmuller approach refines the notion of a theory considerably through the introduction of a notion of theoreticity relative to a theory T, allowing a two-level structure for theories, which leads to a complex hierachical organization. This extra structure permits a much more refined definition of reduction. Even so, this more subtle picture can be represented using the statement view (for details, see Pearce 1982a). Admitting this, Stegmuller states that the advantages of the non-statement view are largely psychological and practical. He argues that it is an empirical fact that scientists use a version of the non-statement view, and that this must be accounted for by philosophers of science. Given the practice of scientists, the statement view is not tenable as a description of science. Any objections should be directed to the scientists themselves, and not the proponents of structuralism (Sneed 1976 p 132, Stegmuller 1979a p 22).

[1] This objection is also raised by M. Friedman against van Fraassen.

4.1.1 Outline of the Structuralist Approach - The Structuralist notion of a theory involves a core K and a set of intended applications I. The empirical claim of a theory is that its core bears a certain relation (to be defined below) to the intended applications. The core is further broken up into four sets: Mpp, Mp, M and C. M is the set of models of the complete theory, including all of its laws. It is the extension of the set-theoretic predicate "is an S". These models are now called theory elements. Mp is the set of possible models of M with the laws dropped out. The Mp's are potential models of T, in the sense that they are systems about which T can be reasonably expected to say something. Any element of Mp must contain all of the theoretical and non-theoretical functions, relations, etc. These elements are now called theory element matrices (Sneed 1976 p 123). Mpp is the set of partial potential models; obtained by dropping all of the theoretical apparatus from Mp. C is a set of constraints, a subset of the power set of M, which ensures that the same objects will be assigned the same values for the same theoretical function. The use of M, Mp, and Mpp allows for a hierarchical structure of theories, the lower levels of which have theoretical terms which are not theoretical with respect to the higher levels. This allows for quite complex inter-relationships between various theories. The set C adds quite a bit of structure to K, and restricts considerably its possible applications. Further structure is added by the manner in which the core is extended to special applications. The over-all form of the "theory" is greatly enriched in its final form, called a theory-net (N). Further details are given in Appendix IV.

This approach relies heavily on giving an adequate definition of theoreticity. Rather than defining theoretical terms in relation to observation terms, as in the Received View, the Sneed approach makes theoreticity relative to a given theory T (T-theoreticity) (see Stegmuller 1976a Chapter 3). It turns out that T-theoreticity is rather difficult to define. Intuitively, a function is T-theoretical if it cannot be measured without employing the theory T. Sneed (1971) proposed that this should require that a term t is T-theoretical iff in every application of T every method of measurement for t presupposes T. Tuomela (1973) proposed to substitute "there is an application" for "in every application". Stegmuller (1979a p 23) has more recently proposed that a theory T employs exactly those functions as T-theoretical for which no representation theorem can be proved (i.e., no reduction of the T-theoretical terms can be performed in terms of the lower level theory).

Balzer and Moulines (1980) have clarified the notion of T-theoreticity considerably. The ideal would be to give a definition of T-theoreticity entirely in terms of M and Mp; any invocation of the Mpp's runs the risk of making the demarcation between theoretical and non-theoretical arbitrary.[1] Unfortunately Balzer and Moulines (1980 p 492) have found this ideal difficult to attain. One problem they consider is that of the theoreticity of the mass function in classical particle mechanics (CPM). Intuitively, this function is CPM-theoretical, however, mass can also be measured within collision mechanics and rigid body mechanics. They argue that the latter two theories are clearly dependent on CPM, and suggest that T-theoreticity depends on the presupposition of T or else some theory reducible to T. A further refinement they find necessary is to restrict T-theoreticity to the existing formulation of T. This makes formalization more difficult, but is in line with the move towards considering theories as historic rather than "Platonic" entities.

Assuming that the notion of T-theoreticity has any content, that is, that the division into Mp's and Mpp's is warranted, some of the more troublesome aspects of the theory-ladenness of observation can be mitigated. While it is probably true that all observations are theory-laden, it is not necessarily true that all of the observations on which a given theory depends are laden with that particular

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[1] Stegmuller's proposal would not satisfy this criterion.

theory. Measurement of T-theoretical functions will be T-theory-laden, but measurement of non-T-theoretical functions will have no dependency on T. This allows us to separate the various parts of our understanding of the world in terms of their empirical dependency, but not in so fine-grained a way that we can give theory-independent verification conditions for every predicte. To what extent the assumption of the first sentence of this paragraph is justified will be considered in section 4.4 below.

4.1.2 <u>The Rationale for the Non-Statement View</u> - By <u>non-statement view</u> Stegmuller means a family of concepts which must be distinguished in order to avoid confusion. His arguments against the Received View are directed at a pair of theses which collectively make up what he calls the <u>statement view</u>. These arguments, in general, take the form of a dichotomy: for each thesis of each set, either it or its corresponding thesis is true or advisable to accept as a methodological principle, while the other is either false or inadvisable; in each case the statement view is not supportable, hence the non-statement thesis should be accepted.

The first version of the statement view (st.v.1) holds that individual scientific theories are best studied as sets of sentences whose terms are defined in a precise formal language. The corresponding non-statement view (n.st.v.1) holds that theories are best treated using informal set theory. The argument for n.st.v.1 is methodological. We must accept either the formal approach, or else the Structuralist approach (by assumption). The formal approach is simply not a realistic alternative to the Structuralist approach, since we do not yet know how to precisely formulate a given theory in a precisely formulated artificial language, but we do know how to deal with theories using informal set theory. Stegmuller claims to have no quarrel with the power of modern logic, but believes that the supporters of the statement view have over-estimated "our human abilities to handle this powerful tool" (emphasis in the original) (Stegmuller 1979a p 5).

It is certainly true that a purely formal approach is not yet capable of dealing with modern physics, since as Stegmuller (1979a p 6) points out, the mathematical devices used by modern physics, such as tensor analysis, partial differential equations, or even the theory of matrices, have not themselves been formalized. This argument is effective against the present complete application of the statement view to the philosophical analysis of particular scientific theories, but is not obviously effective against the application of the statement view to scientific theories in general.

Stegmuller calls the belief that the statement view is adequate for the general theory of scientific theories st.v.3. To establish that st.v.3 is unsuitable for the philosophy of science, Stegmuller uses a different argument, again methodological. Although it might be admitted that the statement view is currently inadequate for the study of specific theories, it might be argued that the general treatment of theories is best carried out under the (not strictly true) assumption that theories are classes of sentences or propositions. Stegmuller objects that this would be to proceed as if the statement view were feasible for individual theories, which it is not (1979a p 46). Furthermore, the statement view gives an empirically false description of what scientists in fact do (see the next sub-section). Consequently, st.v.3 is irrelevant to science as it is practised. He also gives nine specific reasons why st.v.3 "has brought little advantage", "is extremely misleading", and "presents a hopeless undertaking" (1979a pp 46-49).

I find Stegmuller's argument against the value of st.v.3 weak. First, even if st.v.1 is descriptively false, it might still be able to play a normative role in guiding the methodology of science. St.v.3 may not be able to give a complete account of what scientists do, but as a methodology it can place some constraints on how we think of theories. Stegmuller does not argue that st.v.3 is necessarily false;

in fact he accepts that in its ideal development it is true (1979a p 49). Consequently, he should accept that theories are classes of sentences or propositions, or are at least representable as such. There are two ways to see the relationship between the statement and non-statement views. On one hand, we could see both as true, but that each claims to be methodologically superior to the other. On the other hand, we could see each as claiming that the other is false, and that they are mutually exclusive. Stegmuller seems to vacillate between the two. I think we can reconcile the two positions if we assume that when he regards the statement view as false, Stegmuller means that it does not accurately describe the <u>process</u> of science, whereas when he regards both views as true, he is thinking of theories as being <u>representable</u> in terms of one or the other of the views. But then, even if the statement view gives a false description of the historical development of science, representation of theories in terms of the statement view may give insights about theories which are not derivable in the non-statement view. Even if the statement view is potentially misleading and open to abuse, it may still have a useful role to fill.

Stegmuller's case against the adequacy of the statement view (as opposed to his case that it is completely replaceable) is quite good. It might seem that the unformalized parts of mathematics he mentions could be avoided in any comparison of theories, given that the theories can generally be stated without this apparatus. However, in order to compare the contents of the core statements of two competing theories we must be able to determine the meanings of the terms used in these statements with sufficient rigour to assure that any shifts in meaning can be accounted for. In order to determine the meaning of a given theoretical statement (or set of statements) we must be able to provide an interpretation. But in order to provide an interpretation, we need some technique for either translating statements of the two theories into some common language, or else for relating the statements to some common experience. Given the demise of the observation language, together with Kuhn's arguments against the existence of a higher authority in major scientific revolutions, the existence of a common language is doubtful. The alternative, to relate theoretical statements to some common experience, such as observational and experimental practice, is blocked within the statement view just because it is impractical to formalize the relations between a theory and its applications. Thus there is no practical way, using the statement view, to compare the contents of successive theories.

The basic flaw in Stegmuller's reasoning is that he restricts the choice between methodologies to either the statement or the non-statement views. His strongest argument takes the form: the statement view is false, therefore the non-statement view must be true. This argument is backed up by a growing body of successful applications of the non-statement view. These successful applications do not, of course, guarantee the adequacy of the non-statement view. We might well come up against some central problems in the philosophy of science which resist solution by the non-statement view, a meta-level Kuhnian anomaly. I will argue in the final section of this chapter that the incommensurability problem presents just such an anomaly, and that the non-statement view is inadequate to resolve it.

4.1.3 <u>The Descriptive Falsity of the Statement View</u> - There is a further version of the statement view which deals with the nature of the empirical claims of theories. This view (st.v.2) states that the empirical hypotheses of theories are potentially infinite classes of sentences (Stegmuller 1979a p 22). The contrary non-statement view (n.st.v.2) is that the empirical content of a theory is "one single big claim, indivisible into smaller parts". Stegmuller believes that, given the practices of scientists, st.v.2 is logically untenable (1979a p 49), and that n.st.v.2 is the only alternative.

The argument for n.st.v.2 involves the problem of theoretical terms. One aspect of this problem is referred to by

Hempel as "the theoretician's dilemma" (1958). The theoretician's dilemma arises out of the assumption that the purpose of theoretical terms is to establish definite connections among observable phenomena. If they serve this purpose, they can be dispensed with, since any chain of laws and interpretive statements establishing the connection can be replaced by a law which directly links observations. Thus:

> If the terms and principles of a theory serve their purpose they are unnecessary, ... and if they don't serve their purpose they are surely unnecessary. But given any theory, its terms and principles either serve their purpose or they don't. Hence, the terms and principles of any theory are unnecessary. (1958 pp 49-50)

Hempel goes on to argue that the first premise of the dilemma is false, i.e. that the purpose of theoretical terms is not to establish definite connections among observable phenomena. Theoretical terms aid inductive systematization, economy, and heuristic fertility (1958 p 87), functions which cannot be performed by observational terms alone. In addition, Hempel suggests, theoretical terms are necessary inasmuch as scientists wish to refer to unobservable entities which are the cause of observable phenomena. The necessity of theoretical terms generates the problem of theoretical terms: how can we decide which theoretical terms are apppropriate, and how can we justify statements involving them?

One line of response to this problem is instrumentalism. On this view, theoretical terms and the laws connecting them to each other and to non-theoretical terms constitute a calculating device for making predictions on the basis of available evidence. There are a number of variants on this approach, notably those of Bridgmann (1938) and van Fraassen (1980). Arguments against this approach are found in Hempel (1958, 1971), Boyd (1983), Glymour (1980), and Putnam (1974). The gist of these responses is that working scientists direct their investigations as if the claims which their theories make about non-observable entities have a truth-value, and the assumption of the truth or falsity of these claims has an over-riding influence on their methodology, and in guiding their intuitions.[1] Whether or not the assumption of realism is ultimately justifiable, it is a matter of fact that the majority of successful scientists have adopted it with respect to at least the better confirmed parts of their theories. The burden of proof, then, is on the side of the philosophical anti-realists. They must convince scientists that their

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[1] One notable example is the prediction by Dirac of the positron on the basis of certain solutions to his equations of motion for the electron (this example was suggested to me by Patrick Enfield). Another example might be the prediction of the neutrino. More recently, the search for quarks, magnetic monopoles, and tachyons are evidence that scientists take the reality of their theoretical claims seriously.

current understanding of their methods and assumptions is both false and unnecessary.

To investigate this issue in detail is beyond the scope of this dissertation. I assume that a correct account of the theory of science must accurately represent the realistic assumptions of scientists. If this assumption should prove false, I assume that what I say can be rendered in a form compatible with a fictionalist approach to scientific theories. This should not lead to severe error, since there is a problem as well for the translation of the terminology of different fictional accounts of the world. My feeling is that without the guiding intuition of realism, this problem is quite unresolvable.

Stegmuller argues that the following three sentences are inconsistent (1979a p 18):

i) The empirical claims of physics are of the form "a is an S".[1]

ii) Physical theories contain irreducible theoretical terms (e.g. 'force').

iii) Claims of the type "a is an S" are empirically testable.

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[1] This embodies the realist assumption of the previous paragraph.

Using the example of force in classical partical mechanics, Stegmuller shows that any attempt to verify statements involving the force function will inevitably lead in a circle. Using a Duhem-style argument, he shows that any discrepency between predictions and measurements can be accounted for by the postulation of perturbational forces. This can be avoided by stipulating that "a is an S", but this makes it analytic, undermining (iii). He finds this result unacceptable because it would undermine science as an empirical enterprise. The denial of (ii) would undermine Hempel's solution to the theoretician's dilemma, as well as requiring that scientists reconceive what they are doing when they theorize, undermining the descriptive adequacy of the theory of science. The only possible escape is that (i) is false (Stegmuller 1976a Chapter 4).

The solution the Structuralists propose is to replace statements of the form "a is an S" with their corresponding Ramsey sentences (Stegmuller 1979a p 21, Stegmuller 1976a 58-639). The empirical content of a physical theory is an indivisible claim about the nature of the systems to which it applies. It is impossible to divide this claim into individual statements about supposed theoretical relationships between entities represented by the theoretical terms of the theory. "The empirical claims of physical theories must be interpreted holistically as single comprehensive claims". (Stegmuller 1979a p 24).

In adopting the Ramsey sentence formulation of theories (and its later "Sneedification"), we give up the literal realism of sentences like (i), but we don't adopt an anti-realist position. What is preserved in the Ramsey approach is the structure of the theory. The empirical claim of the theory, then, is that a certain entity has a certain mathematical structure.[1]

4.2 Kuhnian Aspects

The Kuhnian aspects of theory nets divide up into those which are direct consequences of the Sneed formalism, and those which are not consequences, but are representable using it. The aspects which are involved in revolutionary science are not direct consequences of the Sneed approach, but rather natural extensions of that approach. This is an almost trivial result of the fact that there is no formal reason

[1] For a discussion of precursors to this view in the early part of the century, see Demopoulos and Friedman (1982). Sneed (1983) takes an instrumentalist view of structuralism. Empirical claims are interpreted in terms of more basic structures, which are interpreted in turn. This regress must be infinite, circular, or grounded in some uninterpreted structure. Only the last provides justification for belief in a theory, but Sneed prefers the first two possibilities. His preference undermines one of the main reasons for adopting his approach, that it more accurately represents what scientists do.

why there must be more than one basic core. I will defer discussion of those aspects involving incommensurability and reduction to the next section. The other major aspect of revolutions is their non-cumulativity. This has a natural interpretation in terms of the replacement of the theoretical apparatus of the dislodged theory with a new apparatus (Stegmuller 1976a). The theoretical components are peculiar to a given theory net, and there is no particular reason to expect that they will be duplicated in successor theories. This, however, is not what has generally been meant by non-cumulativity (though it may explain some such talk). Rather, what has usually been meant is a non-cumulativity of even the non-theoretical structure across revolutions (see Chapter 3 above). I do not find the fact that there is no natural representation of this in the Sneed formalism disturbing, because I do not think that this sort of non-cumulativity is very likely.

Of the other Kuhnian concepts, most of those which are representable using theory nets but are not consequences of the formalism are discussed in conjunction with the notion of a Kuhn theory in A.IV.2, viz. the set of exemplars Io and the frame for a basic core of a theory. Stegmuller (1976a) relegates the Kuhnian notion of preferred analogies to areas outside of the theory of science. This move is evasive and undesirable, though a natural one for someone who believes in the adequacy of the Structuralist approach. The notions which Stegmuller ignores are the very ones which are crucial for understanding theory change.[1]

Given this serious deficit, it is surprising how many of Kuhn's ideas are direct consequnces of the Sneed formalism, especially when it is considered that many of the results were arrived at independently (Kuhn 1976). I will give only a brief summary, since I have already discussed most of the salient features.

Resistance to falsification in normal science is explained by the open-endedness of the set of intended applications, and by the distance of the central core from most specializations. The ability to play around somewhat with laws and constraints because of the holistic nature of empirical claims also aids resistance to falsification.

Holism itself is an integral part of the Structuralist approach, though it is somewhat restricted. (See Sneed (1976) for explicit comments on this.) The theory-ladenness of observation which gives rise to holism (see section 1.3 above) is restricted to the measurement of T-theoretical functions: a measurement which is theory-laden in one theory might not be in another. This relativization of

[1] Harper (1977) adopts a somewhat different semantic approach, but explicitly mentions this deficiency (p 478).

theory-ladenness to a theory, consequent on the notion of T-theoreticity, is perhaps Sneed's most important single contribution.

The nature of normal science as a puzzle-solving enterprise is easily understood in the Sneed approach as a process of core-expansion, as is the cumulativity of normal science through the same notion. The progressiveness of a theory is built right in to Sneed's notion of holding a theory (though some will find this arbitrary). In general, the "fit" between Sneed's work and Kuhn's notion of normal science is quite remarkable.

4.3 Reductions, Revolutions and Incommensurability

Sneed (1976 p 135-144), Balzer and Sneed (1977-78), Stegmuller (1976a p 216 and 1976b p 170) all have argued for and attempted to formalize the notion that a Sneedian reduction relation can be used as a criterion of progress across scientific revolutions. Stegmuller (1979a p 69) has since retreated somewhat from this position, calling it a "daring philosophical hypothesis" which could be proven wrong. Under the influence of Kuhn (1976), he has come to realize that the reduction relation is inadequate, and now believes that some aspects of incommensurability can be given a precise formulation in the Structuralist approach. He distinguishes a particularly severe (but common) form of incommensurability he calls "empirical incommensurability", which undermines the reduction relation (1979a p 77). He conjectures that such incommensurability results from underlying incompatible theories, and is in principle resolvable. Balzer (1979) has also attempted to formalize incommensurability within the Sneed formalism. This section examines the reduction relation, Stegmuller's conjecture, and the attempts to formalize incommensurability.

4.3.1 Reduction Relations - Sneed's discussion of reduction relations (1976 pp 135-144) is quite clear. Reduction relations are based on reductive correspondence, which is a one-many relation from theory-net N' to N, and an associated relation R* which extends R to the power sets of N' and N. The simplest form of reduction is called weak reduction. R weakly reduces T' to T just when R is a reductive correspondence between the non-theoretical structure in K' and K, for each element of A(K') there is an R corresponding element of A(K), and <I',I> is in R*. A strong reduction relation can be formed at the theoretical level which is the analogue of weak reduction, such that whenever a set of reducing structures satisfies the laws and constraints of the reducing theory, the reduced structures satisfy the laws and constraints of the reduced theory. Weak reduction is a consequence of strong reduction. In Schaffner's terms (see Appendix III) weak reduction is an indirect reduction, like Kemmeny-Oppenheim reduction, whereas strong reduction is direct, like Nagel-Woodger-Quine reduction.

Reduction between theory elements requires that for any specialization of the reduced theory, there is a specialization of the reducing theory which reduces it. This makes reduction somewhat too easy; we want to exclude arbitrarily contrived theory cores, created just to ensure reduction. To strengthen the notion of reduction we can relativize it to a particular pair of theory nets, such that the same reduction relation serves to reduce every pair of elements of the respective theory cores (Sneed 1976 p 140). Net-relative reduction has the advantage over an even stronger form which reduces every pair of theory nets in that it allows reduction in cases where the reducing,theory develops later than the reduced theory, but the reduced theory has a much more extensive set of expansions (Sneed 1976 p 142). Further refinements of the notion of reduction have been carried out by Mayr (1976, 1981). He considers the requirements for approximative reduction, desirable for phyiscal theories which are recognized to be approximately true, but not precisely accurate (Moulines 1976, see also A.III.5).

4.3.2 Reduction and Incommensurability - A standard Structuralist conjecture is that whereas the strong reduction relation is appropriate for reduction within a scientific tradition, weak reduction is the relation of theories across revolutions, e.g. (Balzer and Sneed 1977 p 204). Although Kuhn (1976) is quite enthusiastic about the ability of the Structuralist approach to effectively identify and analyze theory change by replacement as well as growth, he is quite critical of the reduction relation as the solution to the incommensurability problem. In fact, he considers it a virtue of the Sneed formalism that it can be used "to localize the problem of incommensurability" (Kuhn 1976 p 190).

Kuhn's rejection of the weak reduction relation is based on his conception of the incommensurability problem as being in part one of translation. In the next section I will use my explication of the incommensurability thesis from Chapter 2 above to show the inherent limitations of the Structuralist approach. Here, I will describe Kuhn's direct criticism of the weak reduction relation.

Kuhn's basic objection is that the Structuralists' use of the reduction relation begs the question. Reduction requires the reducibility of the corresponding cores K' and K. This in turn requires the reduction relation to uniquely associate each member of M'pp with a member of Mpp. The question is: how is this to be done? Unless there are uncontroversial techniques for identifying partial possible models of different theories, differently characterized, the weak reduction relation is too weak to serve its purpose. Any arbitrary many-one correlation would satisfy the formal requirements. The problem of finding techniques for identifying differently characterized applications of successive theories is exactly equivalent to the incommensurability problem.

This problem can be rendered acute by examining the comparison of classical particle mechanics with relativistic mechanics (Kuhn 1976 pp 192-193, Stegmuller 1979a p 71).[1] As Stegmuller points out, the Mpp's of the two theories are different. In classical mechanics the laws are Galilei-invariant, whereas in special relativity they are Lorentz-invariant. This places quite different conditions on the kinematics underlying each theory. The relationship between these kinematics is still quite controversial. This rules out any current direct comparison of their contents.

If the contents of Galilean and Lorentzian kinematics cannot be directly compared either there must be some common ground on which they are comparable, requiring a common

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[1] I ignore Kuhn's example of copper in eighteenth and nineteenth century chemistry (1976 p 192). It is too vague to be convincing.

theory underlying both, or else they are incommensurable. If the latter, then the dynamical supertheories must also be incommensurable, since the presupposition of the reduction relation is undermined. The alternative, supposing the existence of an underlying theory, begs the question of incommensurability unless the existence of the common theory is argued for independently. This Stegmuller fails to do.

4.3.3 Attempts to Define Incommensurability - Having admitted that incommensurability is a genuine phenomenon, some Structuralists such as Stegmuller (1979a section 12), and Balzer (1979) have attempted to define it using the Sneed formalism. I will deal with Stegmuller's attempt first.

Stegmuller (1979a p 71) defines theoretical incommensurability as the relation between two theories which claim to explain the same phenomena but differ in their theoretical structure. This is akin to the Carnapian incommensurability associated with the early Feyerabend, discussed above in section 1.2.2. As I pointed out there, this form of incommensurability need cause no problems for explicating progress in empirical science. Likewise, the Structuralist reduction relation can resolve any potential problems of rational comparison arising from theoretical incommensurability alone. As mentioned above, this form of reduction relies on the possibility of identifying the Mpp's of successive theories in a natural way. By focussing on the theoretical aspect of incommensurability, Stegmuller evaded the deeper Kuhnian issue which involves the incommensurability of not only theories, but also their empirical evidence. Theoretical incommensurability, though necessary for Kuhnian incommensurability, does not exhaust its content. The comparison of theories via Mpp's invokes a neutral language of comparison which Kuhn argues cannot exist. Kuhn (1976) has pointed out that even the Mpp's of competing theories can be different. The existence of the required neutral language is questionable.

To his credit, Stegmuller has recognized this problem (under some prodding by Kuhn). He has defined an even more severe kind of incommensurability which he calls <u>empirical incommensurability</u>. Two theories are empirically incommensurable if they have different Mpp's such that the presupposition for applying the reduction relation is not satisfied (1979a p 72). He suggests that the resolution of this sort of incommensurability requires going down to theories underlying the Mpp's (p 77) in order to find underlying inconsistencies. In the case of Newtonian versus Relativistic kinematics, he states that the underlying physical geometries are not incommensurable, but inconsistent, and that one of them can be established as correct on empirical grounds.

Even Stegmuller's empirical incommensurability fails to adequately represent Kuhnian incommensurability. To test empirical geometries we need either a test which depends on the theories supported by the geometries, or one which is independent. The Kuhnian position is that no independent test is possible, not because the referents of the two geometries are disjoint, but because our only means of grasping the geometries is through the incompatible disciplinary matrices to which they belong. Incommensurability cannot be resolved merely by resorting to underlying physical geometries which are capable of a common representation in some abstract geometrical theory.

Balzer (1979) has given definitions of two sorts of incommensurability based on Sneed's approach which are somewhat closer to the Kuhnian sort than Stegmuller's. The first sort holds between two theories which have disjoint languages, where the language of a theory is the set of non-logical symbols obtained by describing the theory in higher order logic. Different terms are presumed to be represented by different symbols. Balzer also requires that the two theories share a common paradigmatic application. This last condition is stated so that the applications need not contain the same objects, but any object in an application of one theory will, through rearrangement of its parts, correspond to some object in the corresponding application of the other theory. This definition has the advantage of taking the languages of the theories directly into consideration, but the disjointness condition is too broad. Two languages might share no terms in common but still be inter-translatable. Balzer's definition would require that Newtonian theory stated in English is incommensurable with the same theory stated in French. We need the additional pragmatic condition that the resources for inter-translation of the two languages are lacking.

Balzer's second form of incommensurability applies to theories which share terms in common. This situation corresponds to Kuhn's "partial communication". Theories which are incommensurable in this sense share common compatible parts, but have theoretical terms of the same set-theoretic type, satisfied by the same objects, which nonetheless cannot be unified in a way which satisfies the laws of both theories. For example, the impetus function of impetus theory and the force function of Newtonian theory are of the same type, since in some applications impetus is regarded as a force. Despite this, the intuitive pictures of the two theories cannot be unified into one common picture satisfying both theories, since impetus is active in constant velocity motion, whereas force is not. Balzer further requires that the objects dealt with by one theory are more extensive than those dealt with by the other. Aside from the ad-hocness of this last condition, which is required to rule out intuitively comparable cases like Keplerian and Newtonian theory, the notion of unification is too vague. Balzer appeals to intuitive differences between impetus and force in his example, but his formal definition overlooks the possibility that the functions might be unifiable by giving different interpretations to some of the common terms of the two theories despite the mathematical dispartiy which results if all common terms are presumed to have the same meaning. Here again it seems necessary to take the actual interpretation of the terms in specific contexts explicitly into consideration.

4.4 Limitations of the Structuralist Approach

The main failing of the Structuralist approach is that it does not take into consideration how the concepts of a theory are grasped and altered. As Feyerabend (1977) suggested, concepts are treated as given, and their intuitive relationships are presumed to be known. Since Kuhnian incommensurability involves the inability of scientists to intuitively compare the concepts of successive theories, this deficit of the Structuralist approach leaves it incapable of even expressing the incommensurability problem, let alone resolving it. Structuralism does, however, localize the incommensurability problem by making it clear exactly what it is that must be compared. It is possible to use

Structuralist techniques to show what other, lower-level theories a given theory is based on, thereby specifying more closely what concepts need to be correlated in order to allow comparison of the overlying theories. The Structuralist approach can make a considerable dent in the incommensurability problem, but it is the wrong tool to crack it open.

The Balzer-Sneed (1977) conjecture that weak reduction can allow satisfactory semantic comparison across theory-dislodgements has been proven false by the observation that the partial possible models of Newtonian Mechanics and Special Relativity are not the same, and cannot be identified in any intuitive way. Stegmuller's (1979a) conjecture that incommensurable theories are underlain by incompatible theories is promising, but in order for this to be useful the underlying theories must be semantically comparable. Naively assuming comparability because of common terminology is risky, since the way in which terms are grasped by scientists working in different disciplinary matrices may be different. This problem will not arise if the understanding of the lower level theories is independent of the understanding of the theories that they support. This assumption is supposed to be justified by the Sneedian notion of T-theoreticity, but the justification is inadequate on two grounds.

First, a given function may be T-non-theoretical because it can be measured independently of T, yet scientist's actual understanding of the function depends on their understanding of T. If so, a measurement of the value of the function which contradicts the predictions of the theory could lead scientists to revise what they think they are measuring rather than accepting the veracity of the measurement. This would defeat any attempt to compare meanings of terms referring to the function in question, since the meanings of the terms would shift according to the experiments performed, the theories held, and the degree of commitment of the scientists involved to those theories. This shift would be quite opaque to the scientists, who are presumed to be the authorities on the meanings of the scientific terms they use.

The second problem is that there may not be any relevant T-non-theoretical terms. If T is a global theory, even the most fundamental scientific concepts are altered in some respect. In revolutions such as the shift to Relativity, in which consideration of such a high-level theory as electrodynamics led to a shift in fundamental notions of space and time it is quite questionable whether intuitively lower level spatio-temporal concepts are in fact T-non-theoretical.

The statement view, though it suffers from an overly optimistic view of human capabilities and an inflexible crite-rion of meaning, is superior to the Structuralist approach in at least one respect: it recogizes that the representations scientists use must be correlated with the objects about which scientists theorize. The Structuralist approach can show the structure of the content of a scientific theory, but is incapable of dealing with its form except insofar as the form is affected by content. Part of this failure is a result of ignoring the role of preferred analogies in the development of a theory. It is largely through these analogies that a theory's scope is extended to new areas. It seems likely that in order to extend the interpretation of a theory to make it comparable to other theories that these analogies will have to be either revised or extended. Since they play a central role in the interpretation of a theory, they certainly will need to be considered in dealing with the incommensurability problem, which involves techniques of interpretation.

Chapter 5

Pragmatic Incommensurability

> If you want to find out anything from the theoretical physicists about the methods they use, I advise you to stick closely to one principle: don't listen to their words, fix your attention on their deeds.
>
> - Albert Einstein (1879-1955)

5.0 Introduction

To compare the meanings of scientists' utterances we must consider the relation between syntax and semantics. Correlation between the purely formal aspects of two theories is justifiable only in terms of some common semantics. Syntactic approaches must be supplemented by an account of how reference is determined on pain of begging the question of whether diverse theories share a common semantics. The Received View fails to account for scientific revolutions because paradigm shifts change the context of interpretation. Since much of the shift is in tacit assumptions, the nature of the change may be indeterminable given only the resources of the competing theories. The Structuralist Approach tries to alleviate this difficulty by resorting to semantic conceptions of theory. The intended applications of two theories can be compared on the basis of the identity of their structure. This move fails because comprehension of the structure of the applications of a theory can be achieved only by means of some language or other form of representation. Consequently, any trans-revolutionary identification of applications begs the incommensurability question unless there is a system of representation which encompasses the applications of both the pre- and post-revolutionary theories.

To resolve incommensurabilities, it is necessary to expand the contexts of interpretation of the pre- and post-revolutionary theories until differences between them can be made explicit, allowing determination of the identity of the applications of respective theories. In this chapter I will first describe an approach to semantics which allows explicit consideration of the context of interpretation. I will then render Kuhn's argument of section 2.3.4 into this approach. Finally, I will argue that any incommensurability which does exist between theories on opposite sides of a revolutionary divide must be of the weak form.

5.1 Context-dependent Semantics

In the present section I introduce and develop some technical apparatus which is required to refine the ideas of context, interpretation algorithm, compatibility and incommensurability introduced in Chapter 2. This apparatus will serve not only to sharpen understanding of these concepts, but will also provide a basis for evaluating Kuhn's argument for his incommensurability thesis.

5.1.1 Context and Presuppositions - Context provides additional information about the interpretation of statements and the terms that comprise them over and above the constraints of grammar. There are two basic ways that context can do this. First, context may restrict the domain of discourse by determining certain facts which remain unquestioned, and which constrain the possible interpretations of each utterance. This form of context-dependence has been studied by Stalnaker (1978, 1981), Lewis (1979), van Fraassen (1979 pp 134-137). The second form of context-dependence includes facts about the use of terms in the context. It concerns the method of representation rather than what is represented, the domain of the first form.

This distinction is complicated by the fact that they can constrain each other. For example, some terms can only be used appropriately with certain types of subject matter, and also indicate that a particular attitude or style of thinking is being used. The English word 'thou' is generally restricted to Biblical contexts or contexts closely associated with the Bible, involving a certain religious viewpoint. It would be inappropriate to use 'thou' instead of 'you' in most other contexts. The word 'thou' serves not only to refer, but also is an indicator that a particular context is being presumed. Within this context it is appropriate to talk about certain matters and not others, and the discussion is expected to take a certain style.

Scientific contexts are determined by disciplinary matrices. Although they do not have such obvious context-indicators as the use of 'thou' in certain religious contexts, various subtle linguistic and non-linguistic signs tell us what paradigm is being presumed. The paradigm determines what locutions (and other activities) are appropriate, and how they are to be understood. The range of appropriate locutions limits the applications which can be considered, but its primary role is to facilitate communication by providing a common ground for discussion of the matters at hand.

For example, within Newtonian science it makes perfect sense to talk of the unqualified simultaneity of distant events, but not within the Relativistic paradigm. Likewise, in Relativistic science it is sensible to talk of mass varying with acceleration, whereas in the Newtonian paradigm masses can vary only through addition or deletion. In order to discuss the possibility of mass varying with acceleration it is necessary to go outside of the Newtonian paradigm. The fact that 'mass' is used in a certain way in Newtonian science places restrictions on the hypotheses which can be legitimately considered. Thus, the usage of terms can involve presuppositions about matters which, if viewed from some other (generally broader) context, are substantive. Within the Newtonian disciplinary matrix, however, these limitations are merely limitations on correct usage, not factual matters which can be investigated empirically.[1]

Confusion between the two forms of context-dependence has led Kordig (1977 p 63) to criticize the notion of paradigm and the related notion of theory-ladenness of observation. He argues that if observation O presupposes theory T, if T is false O is neither true nor false. Consequently a false prediction cannot be used as a basis for rejecting T. Thus, if O presupposes T, if O is either true or false, T is true. This, Kordig remarks, would be disastrous for empirical science. He concludes that observations cannot presuppose the theory they are evidence for. Since Kuhn's paradigms involve this sort of presupposition, his account of science is false.

While it is true that paradigms involve presuppositions, and that scientificly useful observation must be carried out within a paradigm, the truth of observations doesn't

[1] A.J. Ayer (1946 pp 126-130) argues that many core scientific statements are analytic in their usual usage, but could factual if understood differently.

presuppose theories in any way that damages empirical science. The disciplinary matrix determines how scientific terminology is used, including the way observations must be represented. If this were the whole content of a theory, then theories would, as Kordig argues, be non-empirical. In addition to determining correct usage, however, the disciplinary matrix also determines under what conditions observations are compatible with the theory. The conditions a particular observation satisfies is an empirical matter. The empirical content of a theory is not contained in the statements of the theory itself, but in what Giere (1979) calls "theoretical hypotheses". These are hypotheses that a particular physical system satisfies the theory.[1] Only when viewed from some broader context does a theory make factual claims rather than merely determining authoritative scientific usage. In this broader context, of course, the observations don't presuppose the theory. In order to circumvent incommensurability, it is necessary to show how a broader context can be constructed which will allow treatment of the theory as an empirical hypothesis rather than as a stipulation of correct usage.

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[1] Theoretical hypotheses need never be articulated. The application of a theory to a particular situation involves the implicit presupposition that the theory applies to the situation. Acceptance of the theoretical hypothesis that s is an application of theory T need involve nothing more than the application of T to s.

This requires a theory of context which allows us to represent both contexts and their relationships.

5.1.2 <u>A Theory of Contexts</u> - In context-free languages (if any exist), a given <u>grammatical</u> string has a specific content. Since there is no variation of the content from instance to instance of the string, it makes no difference whether we take string-types or string-tokens to have contents. Furthermore, the only reason to distinguish between strings and their contents is that two strings can have the same content. In context-dependent languages, however, the content of a given string-type can vary from instance to instance, hence it is necessary to distinguish string-types from string-tokens. String-types do not have a content until a context is specified, having instead a range of possible contents, one of which is selected for each context. A string-token is a string-type completed by a specific context. The elements of a context are a set of indices. Examples are time, location, speaker and language. These indices may be represented explicitly in the string, or implied by the circumstances of its utterance.

We can distinguish grammatical strings into terms and sentences. Sentences are composed of strings which when completed by a context make a statement which is either true or false (I ignore sentences which do not make assertions, such as questions). Terms, when completed, are names, predicates, and various logical and non-logical operators. The content of a term-token is a concept. Since sentences are composed of terms, the content of a sentence-token is a relational structure of concepts. Specific theories of context-dependence vary somewhat on what concepts are, but generally agree that they are intensional objects of some sort. I will describe two leading theories and make rather free use of aspects of both in describing a theory of context-dependence applicable to scientific theories.

The first theory, due to Stalnaker (1976, 1978), is based on two-dimensional modal logic (Segerberg 1973) using possible worlds. On this approach a two-dimensional matrix of possible worlds replaces the usual vector used in standard modal and intensional logics. A given sentence-type corresponds to a different proposition in each possible world of a set called the <u>context-set</u>, which represents the presuppositions of the discussion in question. Terms and concepts can be given a similar treatment. Propositions are themselves functions from possible worlds to truth-values, so a context-set containing n possible worlds will form an n x n matrix of truth values with the columns corresponding to the proposition expressed by the sentence in each of the worlds of the context-set. The presuppositions represent the common ground of the discussion, whether it be explicitly known or merely determined implicitly.[1] This common ground is composed of the possibilities which are treated as relevant to the discussion. A proposition is presupposed only if it is true in all of the members of the context-set.

Stalnaker places three conditions on rational discourse, of which the third is most important here: an utterance must express the same proposition relative to each possible world of the context-set. If this condition is violated, the meaning of a given utterance would depend on the truth of other statements, violating the spirit of the positivist condition that truth and meaning must be independent for the purposes of communication. Stalnaker acknowledges that it is a matter of fact that words mean what they mean and that changes in the values of indices can alter the reference of an utterance. In order to accomodate these facts and still maintain his third principle, he suggests that the content of an assertion should be taken as the proposition formed by taking for each possible world the truth-value the assertion would have if it were uttered in that world. He calls this proposition the <u>diagonal</u> <u>proposition</u>. As long

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[1] Stalnaker refers to presuppositions as "common background knowledge". This suggests that speakers are conscious of their presuppositions. I see no reason why this need be so. Someone can presuppose something unconsciously by failing to consider the alternatives, and consequently acting and communicating as if he had knowingly accepted it. Prejudice, for example, generally functions in this way.

as the participants in the discussion have the same or nearly the same context-set, the diagonal proposition exists. Stalnaker argues that even if a discussion starts off with diverse context-sets (a situation he calls a defective context), the context-sets will quickly converge until any differences are unimportant. This eventuality is exactly what does not happen in cases of incommensurability. Discussions involving incommensurable theories necessarily take place in defective contexts. Consequently, diagonal propositions are of limited value in analyzing incommensurability.

A somewhat different approach to pragmatics has been developed by Montague (1968, 1970) and extended by Kaplan (1978, 1979) and Perry (1977). Kaplan deals with explicit indices he calls "demonstratives". On this approach the usual relation between a term and its referent is divided into two functions called character and content. Character is a function from strings and contexts to contents, while content is a function from sets of possibilities to actuality. Contexts are defined by the values given the indices in an expression. The content of a given string-type can vary according to the context. Each string expresses the same content only if the values of its indexical expressions are the same. This conflicts with Stalnaker's third principle. Kaplan names proper contexts those in which the indexicals have the values they would have if the string were uttered.

The range of truth-values of a sentence in its proper contexts is equivalent to the diagonal proposition (if there is one) of Stalnaker's system.

Observing that the diagonal proposition might be true in all possible worlds, while the content of the same sentence, though true in each world in which it is uttered, could be false in some other world, Kaplan urges that certain sentences such as "I am here now" are analytic but not necessary. They are analytic because they are true in any context in which they are uttered (barring special conventions about their meaning), although what they express is possibly false. Each sentence-token of the sentence-type when asserted must be true, but what is asserted is not true in all possible worlds. This permits a split between truth by meaning and truth by metaphysical necessity which is not permitted in Stalnaker's system.

Kaplan's approach has two advantages over Stalnaker's. First, it does not require that all participants in a discussion share the same (or nearly the same) presuppositions in order for a coherent proposition to be expressed by an utterance. Although it makes the analysis of situations involving shared presuppositions more complex, it is useful for dealing with cases involving diverse presuppositions, such as competing disciplinary matrices. Secondly, Kaplan's approach allows statements to be true in virtue of their meaning without being necessarily true. Although this at first seems somewhat paradoxical, it seems to be exactly what happens in the case of high-level theoretical statements such as Newton's second law. Despite these advantages, Kaplan's theory is deficient in one respect: since it deals only with explicit indices it has no way of representing presuppositions except by assigning values to the indexical expressions in a string. When the presuppositions are implicit, as in disciplinary matrices, there is no way to syntactically distinguish sentences which are context-dependent from those which are not.

An obvious extension to Kaplan's approach is to allow implicit as well as explicit indices. Presuppositions can be represented in terms of values of indices, including the truth-values of certain (usually very general) statements. These can be understood as propositions of the form "index i has value v", where v might be a range of values. If proper contexts are restricted to those contexts compatible with the presuppositions in force, then only the restricted range of values can be considered as candidates for filling the context position in the character function. This has the effect of restricting the potential meanings for utterances in such contexts. It is natural to define a notion of <u>context-dependent</u> <u>analyticity</u> relative to the presuppositions in force (which determine the context-set). Thus, a certain sentence used in a situation with a particular context-set might be analytic even though it is neither analytic for every other context-set, nor necessary.

To summarize, like Kaplan I take the content of a given string-token to be the content in the world in which it is uttered, rather than the diagonal proposition. This decision was motivated by the fact that defective contexts are likely to be encountered in cases of competing paradigms, where the presuppositions of rational discourse may have already broken down. Context-sets are determined by the acceptable range of values of both explicit and implicit indices. A range of proper contexts, restricted by the context-set, corresponds to Stalnaker's diagonal proposition, if it exists. A sentence is necessary if its content is true for every possible situation, and analytic if it is true for all permissible proper contexts.

5.1.3 Application to Disciplinary Matrices - One of the major roles of a disciplinary matrix is to provide a context in which normal science can be carried out. All terms used in a disciplinary matrix, whether theoretical or non-theoretical, can be indexed to the disciplinary matrix. Similar terms used in different disciplinary matrices might be identical, non-identical but comparable, or incommensurable. Making the interpretation of terms relative to a disciplinary matrix avoids begging the question of which condition holds.

A disciplinary matrix involves both explicit and implicit presuppositions. The explicit presuppositions are provided by the formal structure of the theory: its laws and constraints as well as those of underlying supporting theories. Examples of explicit presuppositions are Newton's second law, the second law of thermodynamics, and the evolutionary principle of the survival of the fittest. Within their respective disciplinary matrices, these laws are accepted as true: any data encountered in the process of normal science will have to conform to them in some way, or else the presuppositions of normal science would be undermined. Since within the disciplinary matrix the theoretical structure is presupposed, in every possible particular application of the theory its central statements will be true. As a result they are analytic as used in the disciplinary matrix. They need not be necessary, however: the presuppositions they correspond to might be false if the world were different.

The distinction between analyticity and necessity resolves a puzzle about the nature of general laws in science which I have discussed before. The three examples mentioned above: Newton's second law, the second law of thermodynamics and the law of survival of the fittest, have all been accused of being tautologies, and devoid of empirical content. Similar claims have been made about other scientific laws, some of which are mentioned in section 1.1 above. Despite their apparent tautological nature, there seem to be possible alternatives to these laws, and they are regarded as being supported by empirical evidence. This paradox can be resolved if we recognize that the laws are analytic relative to their normal disciplinary matrix, but aren't necessary. For example, the survival of the fittest is guaranteed by the interpretation it is given in Darwinian normal science. 'Fitness' is understood in such a way that those characteristics which tend to enhance survival are most fit. Nonetheless, it is not necessary that those characteristics enhance survival, since if the world were different they might not enhance survival: if an evil demon were to kill all organisms which develop the characteristics which we consider fit, fitness would not enhance survival. Evil demons are ruled out by the Darwinian paradigm, so no application of that paradigm is subject to such a counter-example. Similar arguments can be used to show that $F=ma$ and $dS/dt \geq 0$ are not tautological, though they are analytic relative to their normal context of use.

Stegmuller's concern (see section 4.2.2) that if scientific claims are of the form "a is an S" are analytic then science is undermined as an empirical enterprise is met by recognizing that analyticity does not entail necessity. His use of the Ramsey-sentence formulation of theories is not incompatible with the analyticity of theoretical statements as represented in the statement view. The statement and non-statement views are compatible not only (as Stegmuller admits) in the abstract, but also as descriptions of what scientists is fact do. In normal science, scientists treat statements like 'F=ma' as analytic, but the presuppositions selected by such statements (which are semantic entities representable in informal model theory) are not necessarily true. Not only is it possible (as I argued in section 4.2.1) that the statement view can represent accurately some aspects of science which the non-statement view cannot, it does.

The fallacy in Kordig's argument against Kuhnian paradigms (see section 5.1.1) can now be shown directly. He argues that if observations presuppose the theory they are supposed to support, the theory cannot be false, contrary to the empirical nature of science, which requires that T can be false. If O presupposes T, then the contexts which are permissible are just those in which T is analytic. Other contexts are inadmissable, but they still exist. In these contexts T might be false; consequently, it is not necessary that T is true. The empirical nature of science is preserved by the existence of other possible disciplinary matrices in which the proposition represented by T is false. If the commitment of scientists to the current disciplinary matrix were total, Kordig's claim that Kuhnian science is not empirical would have some force. The possibility of anomalies, crisis and revolution undermines his argument.

The second way that context enters a disciplinary matrix is through the practices of scientists in applying theories. The context is restricted in the following way: for an application to fall under the scope of a disciplinary matrix it must be related to one of the exemplars by the preferred analogies or through a chain of preferred analogies. Applications which fall outside this restriction are not candidates for consideration within the disciplinary matrix.

5.2 Context and Incommensurability

In this section I render the argument for incommensurability from section 2.3.4 into the language of the theory of context outlined in the previous section. I then point out several features of the incommensurability issue which I have mentioned previously, including the role of scientific authority in Kuhn's argument, the importance of the implicit assumptions of the disciplinary matrix, and the role of preferred analogies in interpreting scientific theories.

Next I specify the conditions required for resolving incommensurability when it has occured. I argue that strong incommensurability does not occur across scientific revolutions, and that the conditions required for resolving incommensurabilities that do occur can be satisfied.

5.2.1 <u>Incommensurability of Disciplinary Matrices</u> - Two theories satisfy the thesis of semantic incommensurability if and only if they satisfy the conditions described in section 2.3.3. As discussed in section 2.3.4, conditions (2a) and (2b) depend on the theory-ladenness of observation and on the globality of incommensurability theories, topics dealt with in Chapters 1 and 4. Condition (1a) is commonplace for competing theories. Condition (1b) is the significant one, on which I shall concentrate.

According to definition I of section 2.3.2, T and T' are incompatible relative to an interpretation algorithm A if and only if there is at least one sentence-string in the language of both T and T' which when evaluated by A i) is in the overlap of T and T' and ii) the truth-value for the sentence differs for T and T'. A natural way to understand A is as an algorithm representing the character function which takes as arguments the sentence-string and the indices explicit or implicit in the theory (or other context). Representing the context of theory T by T, the interpretation of sentence-string S under A is the value of the character of S completed by the indices defining T, viz. A(S,T) is the proposition meant by S in the context of T.

To illustrate, consider again the example of Poincare conventionality described in section 2.2.3:

a) T is a theory which says space has Euclidean geometry E and there is a universal inertial force field F.

b) T' says that space has Reimannian geometry R and no universal inertial force field.

c) R = E + F

d) F has no observable effects except on the trajectories of particles.

Theories T and T' are empirically equivalent, but intuitively incompatible. If A is the usual interpretation algorithm for T and S is 'The trajectory t of particle p is a geodesic.', A(S,T) and A(S,T') are both in the overlap of T and T' if p exists, since the trajectories of particles are among the intended interpretations of both theories and it is possible to point to <u>that</u> particle following <u>that</u> path. In general A(S,T) is a different proposition from A(S,T') since if a particle follows a geodesic of R it does not follow a geodesic of E, and <u>vice versa</u>. If t is in fact a Euclidean straight line, then A(S,T) is true and A(S,T') is false, since the natural interpretation of a geodesic, the shortest distance between two points, is a Euclidean line in the context of T but not in the context of T'. One way to resolve the incompatibility would be to choose an A* such that 'geodesic' in S picks out the same trajectory for both T and T'. We might choose an A*, for example, which interprets "geodesics" as trajectories of curvature 0. Alternatively, we could reinterpret both T and T' into a context T* such that the image of S under the reinterpretation is different for the two theories, viz. $R(S,T)=S^*$ and $R(S,T')=S^{*\prime}$, where R is the reinterpretation function, $S^* \neq S^{*\prime}$, and $A(S^*,T^*) \neq A(S^{*\prime},T^*)$, $A(S^*,T^*)$ asserting that t has curvature 0. R can be treated as part of a new interpretation algorithm A* such that $A^*(S,T)=A(R(S),T^*)$. It is also possible to combine aspects of both techniques to obtain $A^*(S^*,T^*) \neq A^*(S^{*\prime},T^*)$.

The above interpretation of incompatibility provides an interpretation of condition (1a): T and T' are incompatible relative to the interpretation algorithm actually used for at least one of them. So far there is no need to distinguish between the interpretation algorithm and the character function. This is required by condition (1b): there is no available technique for representing T and T' in a common language. This condition is satisfied if and only if there is no available A* such that for all S in T and all S' in T' $A^*(S,T)=A(S,T)$, $A^*(S',T')=A'(S',T')$, and $A^*(S,T)=A^*(S',T')$

if and only if $A(S,T)=A'(S',T')$. In other words, A^* interprets each sentence of T and T' as it would be interpreted using the accepted technique for interpretation in its respective theory, and assigns the same semantic value to a sentence of T and a sentence of T' if and only if they represent the same proposition under their accepted interpretations in their usual contexts.

The above interpretation of condition (lb) would not be satisfactory if the A's were functions rather than algorithms. Use of a common interpretation algorithm requires that if two sentences select the same proposition that proposition is known to be the same, since use of the same interpretation algorithm guarantees that it is grasped in the same way, making the two sentences synonomous. If A^* were merely the character function it might be that $A^*(S,T)=A^*(S',T')$, yet S and S' are not in a common language since their meanings are not grasped in the same way. A^*, for example, might be just the union of A and A', freely mixing the two languages. To use an example of Kripke's (1979 p 254ff), someone might be able to give adequate meanings to "Londres est jolie" and "London is not pretty" yet not be disposed to say that the first is true if and only if the second is false. Pierre, a speaker of French only, who had never left France, might believe from hearsay that London is a beautiful city, whereas, after going to England and picking up English his experience leads him to believe that London is a dirty, vile place. Kripke finds this problematic, since he accepts the disquotational principle: the proposition a person believes can be determined from his sincere statments by disquoting them. This principle is false if the statement has implicit presuppositions, since disquotation will capture only the explicit part of what is actually expressed.[1] If Pierre grasped the meanings of the two sentences within a common context, his presuppositions would be the same, and his mistake would be impossible.

Kuhn's argument for condition (1b) has two parts. The first part establishes, through examples of failures of communication together with reasons for believing that this is a possible consequence of the nature of disciplinary matrices, that some competing theories use different languages for which the scientists involved have no complete translation. The second part of the argument appeals to scientists' authority in their field to establish that the incommensurability established by the first part is unresolvable. I accept the first part, but not the second. My reasons will be laid out in the next sub-section. The remainder of this sub-section gives an elaboration of this argument in the terms of the present chapter.

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[1] See Marcus (1981) for further reasons for believing the disquotational principle is false.

As mentioned in section 2.3.4, in order to have a common language in which to represent two theories we need a common means of grasping, or understanding both theories. Given that certain analogies based on the exemplars are used to interpret the theory in new applications, the interpretation of the theory is governed by these analogies. The context of the theory, then, is restricted not only by the presupposition of the central statements of the theory, but also by its exemplars and preferred analogies. The latter are generally not explicitly formulated (partly because it is difficult to formulate what is relevant to a particular case) either within the theory, or in any other context. If two theories have different exemplars and/or preferred analogies, they will not be directly comparable, since the means of interpreting the theories are at least partly disjoint. Complete semantic comparison is impossible without further articulation of the implicit aspects of the respective interpretations. Two such theories are at least weakly incommensurable.

Consider, for example, the relation of wave and particle theories in Britain at the beginning of the 19th Century.[1] The particle theorists (largely Scots) used mechanical models based on a version of Boscovichian atomism, which involves point-like particles and associated force fields, together

[1] Some sources of historical information on this case which consider Kuhnian issues are Frankel (1976) and Cantor (1975).

with geometric methods of problem-solving (Olson 1969). Although early versions of the wave theory were based on models analogous to water and sound waves, recognition that light waves could not be longtitudinal led to the abandonment of this model. The version of wave theory which eventually succeeded was introduced into England and popularized by Cambridge mathematicians, who used mathematical models based on the calculus of fields (Cantor 1975). The particle and field models are apparently incompatible, but their exact relationship has eluded analysis to the present day (Hooker 1973). Despite our inability to understand the exact relationship between the two theories, indicating that we don't have a fully explicit understanding of one, the other, or both, it is still possible to apply the theories to concrete applications in the appropriate context. We must have an implicit understanding of those parts of the theories which we can't articulate. No common interpretation of the theories is available, so if the other conditions of incommensurability are satisfied, which seems plausible, the two approaches not only were incommensurable in the 19th Century, but still are.

The unavailability of a common language for two competing theories may either be remediable or not. Kuhn states that there is no higher authority on the meaning of the terms scientists use than the scientists themselves. He may mean by this merely that scientists have no authority to compare meanings across disciplinary matrices, thereby ruling out one possible route for semantic comparison. He seems to be making a stronger point, however: given that T and T' are not comparable on the basis of A and A', and no A* providing a common interpretation of T and T' is available, no such A* is possible, since such an A* would require reinterpreting the terms and statements of T and T' in a new, expanded context. This would change the meaning of at least some of the terms involved. But since scientists are authoritative on the meanings of the terms they use, no reinterpretation of those terms outside of their disciplinary matrix is permissible. Consequently, comparisons across disciplinary matrices, if unavailable, are impossible. Weak incommensurability requires strong incommensurability.

5.2.2 Resolution of Incommensurability - The fault with the second part of Kuhn's argument is his reliance on the authority of scientists. Some source, be it a person, institution or text, can be authoritative for either of two reasons: bestowal or special knowledge. Authority can be invested in a certain source by conventional means, through institutional structure, popular agreement, or personal decision. Such a source is authoritative if it is the highest authority on the particular issue involved: there is no source which will, under any circumstances, be treated as more reliable in its domain. This condition is seldom satisfied by sources which are treated as authoritative. Most people, for example, take a dictionary to be authoritative on the uses and spelling of words. Dictionaries, though, are themselves influenced by common usage: a dictionary which did not reflect common usage would be regarded as incorrect. This suggests that common usage is authoritative. A moment's reflection shows that this suggestion is false. Common usage can change in a variety of ways which might not be sanctioned by current usage; consequently, common usage is not authoritative for usage in general.

Similar arguments show that many other sources which are treated as authoritative aren't generally authoritative. The laws passed by a legislature can be found unconstitutional by the courts, and the findings of one court can be later over-ruled by another. Are there any authoritative sources which aren't subject to external revision? One possible case is a source which is procedurally invested with the authority to make decisions which no other source can revise, such as the Academie Francaise. The Academie Francaise is a body which has been invested with the right to determine which words are and are not part of the official French language. Someone speaking French may use words not sanctioned by the Academie, but his usage would not be officially correct.

If there were a body of scientists with the official power to declare a particular interpretation of a theory the correct one, there would be no possibility of revision except by the official body. Since the presence of weak incommensurability between disciplinary matrices would permit no direct reason to revise the current interpretation, it seems likely that such a body would declare current usage correct, and alternatives incorrect. This would be sufficient to secure Kuhn's conclusion. The problem with this argument is that there is no such body. The institutional structure of science requires that in principle <u>any</u> theory or approach to scientific issues can get a hearing, whether it is in accord with current practice or not. The fact that established scientific interests may retard or block such input does not affect the principle. The very existence of scientific revolutions indicates that there is no body of scientists with the authority to establish official scientific usage.

The second form of authority is the authority one gets from having a special understanding of a particular subject matter. For example, many philosophers believe that people are authoritative about what thoughts they are having at any given time. This authority derives not from some official procedure, but from the fact that people are in a special position to tell what thoughts they have. Even if they could be mistaken, it is not (currently) possible for anyone else to have as good or better evidence what their thoughts are than they do. Experienced scientists working within a disciplinary matrix are in a position of authority about what is and what is not appropriate within the disciplinary matrix because they are more familiar with it than anyone else. There is some question whether or not this authority extends to all changes within the disciplinary matrix. I suspect not, since the exact interpretation of a theory in new applications will depend on how the preferred analogies are applied in the particular case. This will depend on what aspects of the preferred analogies are considered relevant to that case. Since relevance conditions are hard to specify explicitly, there is room for varying interpretations, each of which must be evaluated on its own merits, irrespective of the preferences of the individual scientists involved. Nonetheless, it seems plausible that for at least the central applications of a theory, experienced scientists working _within_ a disciplinary matrix are authoritative.

The authority of scientists on issues which extend _beyond_ the disciplinary matrix within which they work is quite a different matter. Since they have no special experience with, or knowledge of the issues, there is no reason to believe that they have any special authority. Just as it would be wrong for a patient to object to his psychiatrist's psychiatric interpretation of his behaviour on the grounds that it did not correspond to what the patient was thinking at the time, it would be wrong for a scientist experienced in a particular disciplinary matrix to object to a philosopher's reinterpretation of his terminology within a broader context on the grounds that the reinterpreted form is neither what he meant nor corresponds with how he uses the terms involved. It is only if the scientist familiarizes himself with the broader context, understands the principles of reinterpretation, and still fails to see a correspondence between his usage within the disciplinary matrix and the reinterpretation in the broader context that he is justified in rejecting the reinterpretation. Kuhn's appeal to the authority of scientists fails because scientists are authoritative only within the context of the disciplinary matrix they participate in. This undermines the major argument for strong incommensurability.

A weaker argument for strong incommensurability is that the leading candidates for providing a neutral language for inter-theoretic comparison have fatal flaws. Assuming these candidates are exhaustive of the relevant possibilities, there is no neutral langauge for comparison, and strong incommensurability prevails. Four such candidates, a neutral observation language, the procedures of working scientists, ordinary language and rationalist accounts were considered and rejected in section 2.3.4. It seems highly unlikely that there is a single neutral language for comparing any two competing theories. If such a neutral basis exists, we neither have te capacity to use it or identify it at present. Any resolution of incommensurability will have to be through the extension of existing techniques of interpretation, or through the creation of new ones.

Since contexts are distinguished by their presuppositions, unifying them requires modifying the presuppositions of one or both contexts until the presuppositions of both are the same, and there is a single context of interpretation rather than two. The explicit presuppositions, though formally articulated within the disciplinary matrix, cannot be directly compared, since their interpretation depends on the implicit parts of the disciplinary matrix.

Some guidelines are needed for deciding which presuppositions are more dispensable than others. I advocate four which I believe are necessary, though they cannot be sufficient. First, it is more reasonable to modify the explicit presupositions of a disciplinary matrix to bring them in line with those of competing theories, if this is possible. For example, retaining Galilean relativity and giving up Einstein's Principle of Relativity would preclude comparison of Newtonian Mechanics and Special Relativity, so this is not a desirable move. It would be better to establish a context in which Galilean relativity is recognized as one of a number of possible transformation principles than to retain the context relative to which it is analytic.

Second, when there is a choice between a reinterpretation of some theoretical function which retains theoretical relationships between that function and others in the theory, and one which retains the operational procedures used to measure the value of that function, the second choice is preferable, if it can be made. For example, the mass function of Newtonian Mechanics can vary only through the addition or deletion of matter. In Special Relativity mass can vary with relative velocity, but the rest mass of a particle varies in the same way as the Newtonian mass. If we pay attention to the conceptual role of the mass function, it is reasonable to identify Newtonian mass with the relativistic rest mass (Field 1977). On the other hand, if we pay attention to the procedures for measuring mass used within Newtonian Mechanics, it is the velocity-dependent mass which would be measured, not the rest mass. Further procedures are required in order to measure the rest mass, like bringing the particle to relative rest, or performing additional calculations.

This second guideline is justified by the fact that conceptual role can vary according to the understanding one has, which is dependent on the disciplinary matrix one accepts, whereas measurement procedures are types of actual events which do not change immediately when there is a shift of theory. Furthermore, although theoretical functions like mass cannot be completely defined operationally, operational definitions are more closely tied to what can actually be done than is the conceptual role of a function within a theory. Whereas the theory is a representation or system of representation, a measurement is something which actually occurs, and necessarily contains a factual component.

A related third guideline is that functions which are more directly observable are less subject to revision than those which are less so. Its invocation may require dividing a function or concept which was previously taken as unitary into a number of cases. Simultaneity in Newtonian Mechanics must be distinguished into local simultaneity and simultaneity at a distance. The former is more directly observable, and less subject to revision. In the limiting case, a sufficiently fine-grained distinction within the concepts of two theories will establish a set of sub-concepts whose extensions are sufficiently directly observable that any differences in measurement procedures between theories will be negligible, permitting identification of the corresponding sub-concepts. This does not guarantee comparability between theories, since there may be a variety of ways of interpreting each theory in terms of this restricted set of relative observables. A motivated semantic comparison requires selecting the correct interpretation for each theory, but which interpretation is considered correct may vary according to the respective disciplinary matrices, the interpretations being incompatible. This in itself doesn't preclude semantic comparison, but given that the interpretation of a theory in terms of a common set of relatively observable properties may involve the implicit part of a disciplinary matrix, it may in fact be impossible to do an explicit comparison in terms of the selected interpretations. Nonetheless, it is likely that guideline three places some constraints on inter-theoretic comparison which might be over-looked if the relative observability of the extensions of sub-concepts is not considered, especially if concepts which are normally treated as unitary remain that way. Furthermore, application of the guideline to justify dividing concpts into sub-concepts allows more possibilities of comparison. Kuhn (1977a) has argued that one source of incommensurability is that the different exemplars and different analogies of competing disciplinary matrices lead to different ways of classifying things. These classifications are not comparable in terms of the systems of classification. If two theories divide up the world differently, a finer-grained classification can circumvent this effect.

A fourth condition for comparing meanings across theories is that inasmuch as possible the terms of theories should be interpreted to preserve the principles of the theories. This aids the unification of contexts by retaining as much as possible of the core of the old theory without modification, making it easier to ensure that $A(S,T)=A^*(S,T)$. This guideline is particularly powerful when two theories share a common central principle. For example, the formula F=ma occurs in both Newtonian Mechanics and Special Relativity. If the Newtonian mass is interpreted as the rest mass of Special Relativity, the 'm' in the equation has a different meaning in each theory, and the principle referred to is not the same. Born (1962 p 277) has argued that in order to preserve the simple form of 'F=ma' in Special Relativity, the Newtonian mass should be taken to correspond to the variable relativistic mass, not the rest mass. The convergence of this condition with the requirement of condition two makes a convincing case for Born's identification.

These four constraints on the comparison of interpretation and comparison of meanings of terms in the explicit parts of competing disciplinary matrices are not sufficient for comparability since they might be impossible to satisfy. The problem lies in the implicit part of the disciplinary matrix. In the case of the first guideline, it may not be possible to render the explicit presuppositions of a theory into a context in which they are comparable with those which play the same role in another theory, because no such context is available. Finding such a context requires either a bit of good luck, or else a careful articulation of the implicit assumptions of the disciplinary matrix through which the presupposition is interpreted, at least to the extent that these implicit assumptions differ from those of competing disciplinary matrices.

The same problem, the lack of explicit articulation of many of the presupositions of a disciplinary matrix, limits the effectiveness of guideline three. Again, articulation of these presupositions, at least inasmuch as they differ from those of competing disciplinary matrices, would mitigate the problem. A necessary condition for the resolution of incommensurability, then, is that the implicit presupositions of competing disciplinary matrices can be articulated, or rendered explicit, at least inasmuch as they differ. If this condition can be satisfied, and the four guidelines above are adhered to, semantic comparison is possible.

Can the implicit assumptions of competing disciplinary matrices be rendered explicit? Given the failure of Kuhn's authority argument, I believe so. One possible objection is that there may be no common presupositions of two competing disciplinary matrices, and any articulation of their implicit presupositions could go on without end. I think this situation is impossible if, as required in revolutions, the two disciplinary matrices are based on the same body of observations and experiments (where these are understood as actual activities or events, not as descriptions of these activities). The experimental procedures of a science are not directly affected by changes of theory (although they may eventually be affected), and can be described in quite neutral terms (such as body movements, apparatus set-ups and pointer readings, to take an extreme case). These procedures provide at least some common ground underlying competing disciplinary matrices. As a bottom line, any two competing disciplinary matrices share the constraints imposed by their common experimental basis. Any chain of articulation of deeper and deeper implicit assumptions which does not terminate either on its own or in some experimental procedures has no relevance to empirical truth, and is of dubious meaningfulness. Such cases either do not occur, or are not relevant to scientific progress.

A second objection, which is more serious, is that the theoretical resources and the current understanding of language (linguistic theory or Philosophy of Language) and mathematics is not rich enough to systematically articulate the implicit assumptions without reaching a dead end before a common ground is reached between competing theories. This situation could be reached as follows: In order to articulate implicit assumptions systematically, some theory of articulation must be adopted. Typically, this theory will itself be only partly articulated. It is possible that a point might be reached where not only do the implicit assumptions of the disciplinary matrix under study resist further articulation within the current linguistic paradigm, but the implicit presupositions of that paradigm also resist further articulation when the linguistic theory is applied to itself. This effectively terminates any further systematic investigation. Even if this were to occur, and I don't find it at all implausible that it might, incommensurability still might be resolvable through the introduction of a new linguistic paradigm which is able to articulate the anomalous cases. Granted, such an event would be fortuitous, but no more so than a scientific revolution. Given that the positive argument for strong incommensurability based on the authority of scientists is fallacious, as I have shown, the burden of proof is on the supporter of strong incommensurability to show that not only must a dead end be reached, but that no new linguistic paradigm will appear which can resolve the anomalies of the previous one. Such an argument, if based on current linguistic theory would be self-defeating, since by hypothesis new paradigms need not be commensurable with the current one. Consequently, I don't think that a coherent argument in favour of strong incommensurability can be made out. If strong incommensurability holds between successive theories, we will never know it.

5.3 Summary

I have argued that the terms of theories can be treated as indexical, requiring specification of the disciplinary matrix in which they are used to determine their meaning. Since disciplinary matrices are not fully explicit, the comparison of meanings across disciplinary matrices for which there is no available underlying common language (a context into which both can be rendered accurately) is impossible unless the differing parts of the implicit aspects of the disciplinary matrices can be made explicit. If this can be done, four conditions on the interpretation of the explicit part of competing disciplinary matrices constrain their semantic comparison.

The main positive argument against the possibility of articulating the implicit part of disciplinary matrices is that such an articulation would change the meaning of the terms used in the disciplinary matrices by changing the context of their interpretation. This (the argument goes) violates the condition that scientists must be authoritative on the meanings of the terms they use. If so, no alteration in the meanings of those terms is justified.

My response to this argument is that the authority of scientists on the meaning of the terms they use derives not from some institutional power invested in them to determine meanings, but from the fact that they are in a special position to know the meanings of the terms they use within their disciplinary matrix. Outside of this restricted context, however, there is no reason to attribute any particular authority to them. Since any inter-theoretic comparison of meaning or reinterpretation of meanings in a broader context takes us outside of the disciplinary matrix, it also takes outside of the domain of the scientists' authority. Consequently, scientists have no special authority to determine the meanings of the terms they use except in the context in which they in fact use them as experienced specialists.

Since there is no strong reason to believe that the articulation of the differing implicit components of competing disciplinary matrices is impossible, and since competing disciplinary matrices always share a common basis of experimental practice, it is reasonable to accept that competing disciplinary matrices will share a common set of presuppositions which can be used to provide a context for their comparison if their differing implicit presuppositions can be made explicit. The only argument that this might not be possible is self-defeating, since it must start from current linguistic theory. Even if it can be shown that current linguistic theory is inadequate for complete articulation of the differring implicit presupositions, there is no reason to believe that every future theory will be equally inadequate, especially if the current theory any new theory which might replace it are incommensurable. I conclude that no argument can establish that incommensurability cannot be resolved. Since the burden of proof is on the proponent of incommensurability to show that it is irresolvable, there is no good reason to accept strong incommensurability.

Weak incommensurability, however, is a natural consequence of the globality of some theories and the way in which scientific terms are interpreted, together with the fact that scientific innovation tends to precede its complete justification. This latter fact does not make the efforts of innovative theoreticians irrational, since they may have good reasons to expect that their innovative theories are progressive even if they cannot state them explicitly. Weak incommensurability does not undermine traditional conceptions of scientific progress so much as to make it somewhat more difficult to determine. Until any incommensurabilities are resolved, typical indicators that a new theory is progressive must be relied on to justify its adoption rather than a demonstration that it can account for all of the highly corroborated content of its predecessor.

APPENDIX I

MODELS OF THEORY CHANGE

This appendix describes several models of theory change which have been associated with incommensurability. None of these models is completely accurate, but collectively they help to bring out some more characteristics of incommensurability. The greatest problem with these models is that they can engender resistance through too literal interpretation.

A.I.1 Change of Paradigm - Kuhn's use of 'paradigm' takes on a rather broad meaning. The linguist, Margaret Masterman (1970) has identified, with perhaps more enthusiasm than was necessary, no fewer than 21 different uses. Kuhn admits the problem, and has attempted to refine his usage into two distinct senses. The first is global, embracing the shared commitments of a scientific group. The second sense is part of the first, referring to the concrete problem solutions whose adequacy is accepted by the scientific community in question. Kuhn now calls the first sense disciplinary matrix and the second exemplar.

In order to avoid circularity in the definition of 'disciplinary matrix', Kuhn defines a scientific community as the practitioners of a certain specialty, bound together by common elements of education, pursuing a common set of goals. They are characterized by a "relative fullness of communication within the group, and by relative unanimity of the group's judgement in professional matters" (1977a p 461). As such, scientific communities have certain similarities to political parties, religious groups, and other cults. A disciplinary matrix is analogous to an ideology or mythos.

Kuhn holds that all paradigm changes involve necessary and irreconcilable differences. These differences are, because of their irreconcilability, revolutionary. He argues that political changes which seem part of the normal political process might well be viewed as revolutionary to those directly involved. Revolution is relative to perspective. Likewise, a religious conversion or psychiatric cure might appear revolutionary to the subject, but might seem part of the person's normal development to his friends or his psychiatrist. On paradigm shifts, Kuhn says:

> (A paradigm) can simultaneously determine several traditions of normal science that overlap without being coextensive. A revolution produced within one of these traditions will not necessarily extend to others as well (1970a p 50).

Also,

> Scientific revolutions...need seem revolutionary only to those whose paradigms are affected by them. To outsiders they may, like Balkan revolutions of the early Twentieth century, seem normal parts of the developmental process. (1970a)

Scientific revolutions are relative to a certain discipline or sub-discipline; what is seen as revolutionary science from a perspective within a certain discipline may be seen as normal science from without.

The relativity of revolutions presents serious problems for the view that all paradigm shifts involve absolute incommensurability. Unless we assume that incommensurability is relative to a given subject, two theories could be in the same sense both incommensurable and not incommensurable. We must reject at least one of the following:

a) All paradigm shifts are revolutionary.

b) Some revolutions are relative to a discipline.

c) All paradigm shifts involve incommensurability.

d) Incommensurability must be absolute or stronger.

Kuhn rejects (d), and is somewhat ambiguous about (c), but accepts both (a) and (b). Statement (b) is clearly supported by the second quote above. Support for (a) is somewhat more questionable, but seems to be in line with the bulk of Kuhn's writing. The revolutionary nature of paradigm shifts results form the guiding role of exemplars in normal science. Any rejection of previously established exemplars involves a change in scientific practice which goes outside the bounds of normal science, at least for the discipline which uses those exemplars (1970a pp 93-94). Such a change is, by definition, revolutionary for the discipline involved.

If we assume a nesting of disciplines and sub-disciplines, normal scientific activity in some discipline may involve revolutionary activity in some sub-discipline. This activity will appear revolutionary only from the perspective of the sub-discipline. The existence of the core exemplars of the encompassing discipline provides a reference from which to evaluate changes in the sub-discipline. Activity which is revolutionary in one sub-discipline may merely provide new data or methods for other sub-disciplines, and have no revolutionary impact. The nesting of disciplines allows the possibility of revolutionary change in a discipline which is not itself nested in any other stable discipline (i.e. one which has a normal science). Only paradigm shifts which are global are candidates.

A.I.2 Change of World - Hanson (1958) argues that in revolutionary changes the pre-revolutionary and post-revolutionary scientists do not see the same thing. For example, Kepler and Tycho don't see the same object when they look at the sun, since one sees a moving object, while the other sees a stationary object. The actual objects in their worlds are different. Kuhn often speaks in the same manner, viz.:

> ...after the assimilation of Franklin's paradigm, the electrician looking at a Leyden jar saw something different from what he had seen before. The device had become a condensor... (1970a p 117)

and,

> Lavoisier...saw oxygen where Priestly had seen dephlogisticated air and others had seen nothing at all. (1970a p 117)

also,

> Pendulums were brought into existence by something very like a paradigm-induced gestalt switch. (1970a p 119)

and finally,

> In so far as their only recourse to that world is through what they see and do, we may want to say that after a revolution scientists are responding to a different world. (1970a p 111)

At other times (e.g. 1970a pp 114-115) he is more careful, and speaks only of the objects involved being seen differently. Kuhn is aware of the difficulties involved in saying that the world changes with a paradigm shift, but thinks that we must learn to make sense of statements like "the world does not change with a change of paradigm; the scientists afterwards work in a different world" (1970a p 121).

If the world really does change in a scientific revolution, then incommensurability is a non-issue. Watkin's argument (section 2.2.3) applies: the two theories involved have disjoint universes of discourse, and can't be incompatible. Since this consequence of the change-of-world model contradicts Kuhn's views on incompatibility and incommensurability, I think it fair (especially given his misgivings) to assume that Kuhn was speaking metaphorically in the above quotes. We should read references to seeing as as-if-seeing, and references to being brought into existence as as-if-being-brought-into-existence. The alternatives are either to dismiss Kuhn as fundamentally inconsistent, which would be counter-productive, or to try to reinterpret references to incompatibility and incommensurability in some way which makes Kuhn consistent with the change-of-world model. I doubt that this can be done without yielding a far more radical failure of communication than Kuhn's talk of "partial communication" implies.

If Hanson is right, the subject matter of a given science changes completely with each scientific revolution. If so, it would be just as absurd to call a new theory better than the old as it would be to call geology better than psychology. Hanson must be mistaken. He thinks that perception is necessarily interpreted and theory-laden. Consequently, since Tycho's theory says the sun doesn't move, Tycho actually sees a stationary object (or rather, the object which Tycho sees is a stationary one). Since the object which Kepler sees does not move, Tycho and Kepler cannot be seeing the same thing. Aside from the assumptions of theory-ladenness and meaning holism, the essential assumption for Hanson's argument is that interpreted perception is the relevant form for determining identity of objects.[1]

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[1] For a detailed (but question-begging) account of Hanson's position, see (Kordig 1971 pp.3-13).

While I think a good case can be made that interpreted perceptions are the basis for epistemology,[1] I doubt that interpretation is relevant here. Tycho and Kepler can both point to the what they are observing, though each might interpret what they point to quite differently. Demonstrative reference itself is not interpretation relative. It may not be sufficiently rich to provide a basis for epistemology, but it can provide a basis for co-temporal identity. The possibility of referring to "this" world is sufficient to block the possibility of disjoint worlds and universes of discourse. Even Hanson (1958 pp 8,18,182 note 6) admits there is a sense in which Tycho and Kepler see the same thing, i.e. share parts of their visual experience. Their worlds at worst overlap. The change-of-world model doesn't seem to support any special reason to think that different paradigms should be incommensurable.

A.I.3 Gestalt Shifts - A model of theory change strongly suggesting incommensurability is the gestalt-shift model. This model was put forward and defended by Hanson (1958), and later taken up by Kuhn (1970a pp 111-114). In a gestalt shift, certain elements of experience are re-interpreted and perceived differently. The most obvious examples involve

[1] Otherwise how could perceptions form the evidential basis for beliefs, which are intentional? (Brown 1977)

visual perception, but the concept of gestalt extends to the recognition of patterns in other sensory modes, and other parts of experience.

Gestalt theory distinguishes the "figure", which is the interpreted object of perception, from the "ground", which is made up of the perceptual elements which are otherwise ignored. Attention is focused on the figure through its interpretation as an object of a particular kind. The background serves only to delimit the figure. The recognition of an interpreted figure standing out from its ground is called a gestalt. According to the theory, any perception involving conscious attention requires such a distinction of figure and ground, with a concomitant interpretation of the figure. Examples from nature would include the gestalt of a predator among the shadows of a tree or bush. The pattern of splotches of light and dark is ignored as insignificant, while a certain part of the pattern is unified through its recognition as a predator. Individual subjects vary widely in their tendencies to form gestalts in general, and to form particular gestalts. The former may be biologically based, but the latter seems to depend on past experience. Another significant aspect of gestalt shifts is that they are "all or nothing" events; they don't occur piecemeal (though they aren't instantaneous, either).

Kuhn remarks that scientific training often involves developing the ability to have certain gestalts:

> Looking at a contour map, the student sees lines on paper, the cartographer a picture of a terrain. Looking at a bubble chamber photograph, the student sees confused and broken lines, the physicist a record of a family of sub-nuclear events. Only after a number of such transformations of vision does the student become an inhabitant of the scientist's world, seeing what the scientist sees and responding as the scientist does. (1970a p 111)

This transformation is usually more gradual than textbook gestalt shifts, and is generally irreversible. Kuhn suggests that this will lead to the <u>appearance</u> of incommensurability when the normal-science tradition changes:

> (The student's world) is determined jointly by the environment and the particular normal-science tradition the student has been trained to pursue. Therefore, at times of revolution, when the normal-science tradition changes, the scientist's perception of his environment must be re-educated - in some familiar situations he must learn to see a new gestalt. After he has done so the world of his research will seem, here and there, incommensurable with the one he had inhabited before. (1970a p 112)

Experiments indicate that we cannot form two gestalts simultaneously from the same ground. To illustrate with some famous examples, we can't simultaneously see the duck-rabbit figure as both a rabbit and a duck, nor can we have a gestalt of both a beautiful young woman and an old hag when we observe the wife/mother-in-law figure, nor can we see the Necker cube simultaneously with the bottom corner proturuding and receding. Try as much as you want, the best you can do is to alternate the two gestalts rapidly. The incompatibility of the duck and rabbit gestalts is a consequence of the function of our means of perception, which are themselves influenced by our past experience and training. The extent to which incompatible gestalts can be rendered compatible depends on the plasticity of our perceptual apparatus, which is at least partly an empirical question.[1]

Kuhn (1970a p 113) remarks that Hanson (1958 Chapt 1) and other colleagues have argued that supposing that scientists occasionally experience shifts of perception like gestalt shifts could make the history of science more sensible, but he is careful to note that the psychological experiments of the gestalt theorists only suggestive.

There are two problems with the gestalt model as a model of theory dislodgement. First, the examples which form the basis of the gestalt-switch literature are artificial. The "objects" which are perceived are not real; they are only representations of real objects. In fact, the duck-rabbit

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[1] Some perception is biologically determined: try as we might, we can't eliminate the perceptual bend of a partially immersed stick which is known to be straight. Other perceptions are apparently fixed at an early age, but depend on experience. Assuming the analogy between gestalts and theories, one might wonder whether certain beliefs are biologically determined or fixed at an early age. I won't discuss this interesting possibility.

is neither a rabbit nor a duck, but a two dimensional figure on paper. Consequently, it is impossible for anyone looking at it to really see either a duck or a rabbit. It isn't obvious that a gestalt-switch can occur without at least one of the interpretations being mistaken, generally by presuming more than is really evident. The Necker cube, for example, must be falsely assumed to be a three-dimensional object for a gestalt switch to be possible. These false assumptions can be exploited to create intriguing illusions (sometimes called "penroses"), such as the famous "three-pronged framis", which is square and two-pronged at one end, but round and three-pronged at the other, or the marvellous waterfall drawing of M.C. Escher. These illusions occur spontaneously; we cannot control them. Incompatible gestalts might always involve illegitimate (and perhaps unconscious) inferences from the data which would be excluded in good scientific practice.

The second problem bears directly on the issue of incommensurability. In the gestalt-shift examples given above, it is possible to focus attention on the lines which make up the figure of the gestalts. These lines are neutral to the incompatible gestalts, and provide a reference to which the subject can relate his interpretations; he is able to realize that he perceives certain lines both as a duck and a rabbit. If a neutral basis exists for scientific observations, then competing paradigms could be recognized as alternative accounts of the same observations, which may be treated as though they were compatible; this is the gist of Quinean conventionalism.

Kuhn's solution to this second problem leads directly to a fundamental argument in favour of incommensurability. He admits that if the gestalt analogy holds here, then there would be an external standard which remains constant through the shift, allowing comparison. He does not think, however, that the gestalt analogy does extend to the existence of such a common standard:

> In scientific observation, though, there is no recourse above or beyond what (the scientist) sees with his eyes or his instruments. If there were some higher authority, that authority would itself become the source of his data. (1970a p 114)

Furthermore, scientists' commitment to the new paradigm obscures the shift, since once the new paradigm is adopted scientists state their beliefs in its terms. A convert to Copernicanism does not say that he used to see a planet, but now sees a satellite; he says that he once took the moon for a planet, but was mistaken (1970a p 115). Acceptance of the new paradigm, incompatible as it is with the old, precludes the possibility of accepting both. Thus, the scientist does not notice the "shift of scientific vision". We can determine that there has been some sort of mental transformation not from direct testimony, but through "indirect and behavioural evidence that the scientist with a new paradigm sees differently from the way he had seen before." This is reminiscent of Einstein's advice that to understand physical theory one must pay attention not to what the physicist says, but rather to what he does. The lack of an external standard not only results in incommensurability, but actually disguises the basis of the incompatibility.

This explains why the study of theories as abstract calculi related logically to their evidence has ignored the aspects of theory change Kuhn describes in the historical parts of. Unless a philosopher of science is well versed in the history of science, he will have to rely on scientific colleagues (or his own scientific training) to understand the theories involved in a revolutionary change. In doing this, he implicitly adopts the current paradigm, and is in no better position than scientists to recognize the obscured transformation.

Kuhn remarks that it was only after immersing himself in Aristotelian science that he was able to recognize that the Aristotelian approach was not obviously incorrect, but an approach that might even be rendered acceptable (1977b). Kuhn maintains that in order to properly understand a paradigm one must be prepared to go native:

To translate a theory or world-view into ones own

> language is not to make it ones own. For that one must go native, discover that one is thinking and working in, and not simply translating out of, a language that was previously foreign. (1970a p 204)

This transition is spontaneous; Kuhn describes it as a conversion. A complete explanation of scientific change must give not only both a translation and good reasons for the change, but must also describe how the translation and reasons are used by the scientist to choose a new theory.

Abstract treatment of theory change not only obscures shifts in understanding; it doesn't even permit their consideration. If an account of the way a scientist grasps a theory is required to give a complete account of scientific change, as Kuhn's arguments imply, then the Received View is doomed to failure. Kuhn and his supporters have amassed a large amount of evidence that the Received View does not in fact accord with many significant aspects of theory change. This failure does not prove Kuhn's account is correct, but it does open the door to it. If traditional views of scientific progress are to be retained we need an account of conversion which does not require incommensurability.

A.I.4 Translation and Change of World View - Although the world doesn't change during a scientific revolution (except in the obvious way), our perspective on it, to use a common metaphor, does. A shift of paradigm is analogous to a shift of physical perspective. We see the same thing, but we see it differently. As in the case of gestalt shifts, changes of physical perspective can be both recognized and compared through the isolation of the common elements perceived from both locations. Furthermore, the actual physical shift can be measured, and geometric methods can be used to project what the view of the object will be from various positions. This possibility is exploited by computer programs which allow engineers to represent what an object will look like from a number of angles.

A possibility for comparing competing theories is suggested by the use of geometric methods for comparing perspectives: perhaps we can find some method for projecting representations from one theory to another. What's required here is something like a translation from the language of one theory to the other by which the translated representation is understood as if it were in fact a representation of the second theory.[1] Though the problem of semantic comparability is essentially one of translation, it differs from translation between natural languages in that many terms are shared. Kuhn rejects the idea that our knowledge of the interpretation of our theories can be fully articulated in terms of explicitly

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[1] It is amusing, and indicative of the deep roots of the change of world-view metaphor, that 'translation' derives from the Latin word for 'transfer', and is still used in physics to mean change of physical position.

formulated rules. Learning a paradigm involves learning (along with a mathematical structure) a set of techniques through the solution and extension of the exemplars. This knowledge, unlike the contents of the theory itself, is largely tacit, unarticulated, and merely implicit in the practices involved. To translate from one theory to another we must represent the exemplars of the first theory in terms of the second. Since knowledge of the exemplars in both cases is at least partially implicit, it isn't possible to give an explicit translation without making this implicit knowledge explicit in terms of some neutral language.

Although Kuhn allows that it is possible to compare theories with respect to reference, he denies that this sort of comparison amounts to translation. This is in line with his earlier protests that he always insisted that communication was partial. Even if it is possible to define the reference of the terms of a foreign theory in terms of the home theory, this definition is coloured by the implicit meanings of the home theory, and complete translation is thwarted. In order to reduce the implicit component of meaning one needs to study the exemplars of the home theory in their proper context (i.e. "go native"). This isn't enough to allow translation, however, unless what we learn can be rendered explicitly. Kuhn points out that the difficulties of learning a second language are far less problematic than the difficulties of translation: translation "can present difficulties even to the most adept bilingual" (1970b p 267). He holds that translation requires that the both theories can be rendered in terms of some neutral position:

> In applying the term 'incommensurability' to theories, I had intended only to insist that there was no common language in which both could be fully expressed and which could therefore be used in a point-by-point comparison between them. (1976 191)

Although complete translation might involve differences in nuances and connotations, these do not seem to be relevant to semantic comparison. These aspects of language can vary more between two users of the same language than between users of different languages. The important aspect of translation for semantic comparability is the existence of a language which is neutral with respect to the two theories in question, but can represent each without distortion. Kuhn (1983a) requires that taxonomy is a factor which must be preserved in an adequate representation.

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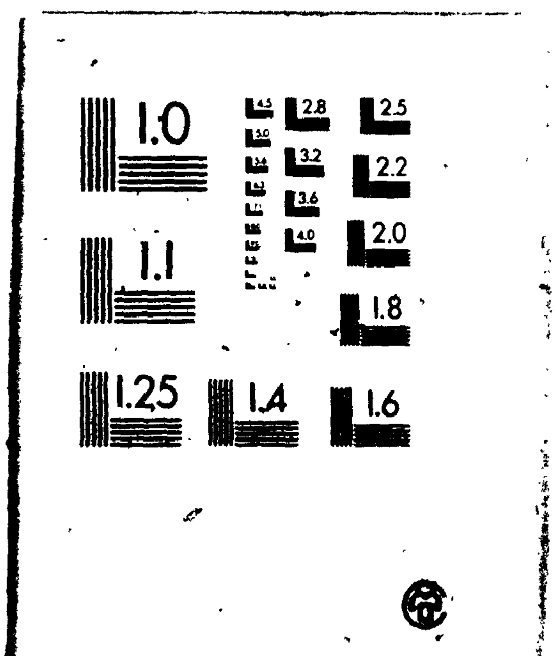

APPENDIX II

THE EFFABILITY HYPOTHESIS

According to the rationalism of Chomsky, Fodor and Katz all meanings are "effable" through a species-specific set of linguistic universals. Possible experience includes not only sensory experience, but also cognition. Katz (1978 p 202) assumes that propositions and senses are identical, and argues for the effability hypothesis:

> EH: Each proposition can be expressed by some sentence in any natural language (1978 p 209).

EH can be stated more simply as "Anything which can be thought can be said." The effability hypothesis is based on the genetic disposition to constrain language learning with a narrow set of linguistic universals. On the semantic side these determine a conceptual space common to all language using humans, consisting of semantic primitives and combinatorial rules to which each proposition, or sense, can be reduced. On the syntactic side the universals determine a grammatical space in which every system of surface to depth structure can be represented (1978 p 218).

The reducibility of senses to a given set of semantic primitives permits atomism (each statement is individually reducible), and allows scientific realism, since the meaning of theoretical statements is determined by their relation to the linguistic universals, not by some loose connection to observation. Differences of meaning always make a difference to possible experience, since they involve different cognitive experiences. Intertranslatability of natural languages is explained by effability, since for arbitrary languages L and L', if S is a sentence of L, and A is the sense of S, then A is also the sense for some S' in L'

For the case of inter-theoretic comparison Katz speaks best for himself:

> For rationalists, cases of failure to translate theoretical sentences represent only a temporary inability of speakers, based on their lack of knowledge of the relevant sciences, to make the proper combination of primitive semantic concepts to form the appropriate proposition. That is, the failure represents a temporary vocabulary gap (rather than a deficiency of the language) which makes it necessary to resort to paraphrase, creation of technical vocabulary, and metaphorical extension, etc. in order to make translations actual in practice, as well as possible in principle. (1978 pp 219-220)

Powerful results from powerful assumptions.

Katz' argument for the effability hypothesis is based on the need to explain the rapidity with which children learn language, as well as the creativity of language use, which has three components: 1) unboundedness, 2) stimulus-freedom, and 3) appropriateness. Although effability is the strongest expressability principle on which a semantic theory might be based, since it makes every natural language equally expressive in every respect, Katz regards anything weaker as arbitrary (1978 p 224). Katz' other argument (aside from the elegance of his view) is the absence of evidence for failures of translatability (1978 pp 220-223).

Although I agree with Katz' explicit consideration of cognition, I find his position inadequately argued for, and evasive of some of the real problems of Kuhnian incommensurability. Although he does not explicitly consider any of Kuhn's examples, he classes translation across conceptual revolutions in science together with a number of examples drawn from linguistics in which the main problem is the absence of superficial grammatical counterparts in different languages. It seems to me quite possible that the set of species-specific linguistic universals might extend to cover differences in the basic surface grammars of natural languages, but not extend nearly so far as to cover conceptual differences in abstruse sciences, whose immediate comprehension has little survival value for the individual, and which in any case have not been around for a biologically significant time. The "temporary vocabulary gap" Katz mentions may be indicative of the absence, in the current state of the development of the language, of the capability to form the required concepts. The resort to metaphor, etc. may be a truly creative activity dependent on fortuitous environmental stimulus which has little to do with conscious cognitive activity.[1] If, for example, Piaget's (1952) account of the development of intelligence is even roughly correct, intellectual capabilities develop in hierarchical succession, under biological control, but also contingent on environmental stimulus. At a given stage of development there are certain things a child cannot conceptualize. This inability is not just a matter of lack of experience.

Even if all concepts are innate, not every concept can be used freely. An innate concept is useless for translation if there is no capacity to use it. To say that someone has the capacity to use a concept merely because it is innate and can be "triggered" by the appropriate circumstances is as foolish as to say that molten glass has the capacity to break because if it were cooled and solidified it could be broken. The capacity to use concepts must be acquired. The rationalistic effability hypothesis is useless for the purposes of resolving incommensurability unless an appropriate account is given of how the capacity to use concepts for comparing meanings across disciplinary matrices can be

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[1] Stich (1979 esp. pp 330-334) distinguishes two Chomskian hypotheses: i) rigid rationalism and ii) anti-empiricism. The first entails the second, but not vice versa. Stich argues that the linguistic evidence can be accounted for by (ii) just as well as (i), to which EH is equivalent. He then describes a developmental position which is anti-empiricist, but not rigidly.

acquired. Even if we accept the rather extravegant rationalist principles, the problem of semantic comparability still exists. The problems which Katz rejects as merely practical are of the essence of the real issue.

APPENDIX III

FORMS OF REDUCTION

A.III.1 Kemeny-Oppenheim Reduction - The first paradigm of reduction fits the Received View. Schaffner (1967) calls it the KO paradigm (for Kemeney and Oppenheim). This is a form of indirect reduction, since reduction is carried out only between the observational parts of the theories. An example might be the explanation, by Lavoisier's oxidation theory, of all of the observable facts of the phlogiston theory. In a KO reduction, the term 'phlogiston' has no correspondent in the new theory. Assuming that the theories are axiomatized, we can formally represent KO reduction as:

1) T' has among its primitive terms terms not in T.
2) Any part of the data of T is explained by T'.
3) T' is at least as well systematized as T.

In the last clause, systematization is a combined measure of strength and simplicity. The KO paradigm is also suitable for reduction in cases where the theory-observation distinction is not neutral, but is theory-relative, as long as the data for T are comparable to a subset of the data for T' (within their common range of intended applications). Both versions of the KO paradigm require semantic comparability of the observational components of theories, at least.

A.III.2 Nagel-Woodger-Quine Reduction - The second version of reduction is called the NWQ paradigm (for Nagel, Woodger and Quine). This reduction is a direct reduction since all of the basic terms and entities of T are related to terms and entities of T'. An example might be the reduction of thermodynamics to statistical mechanics. Formally, the NWQ paradigm is as follows:

1) All primitive terms a,b,c...n of T appear in T' or are associated with one or more terms of T' by a reduction function f such that:

   a) f is 1-1 between individuals or aggregates of T and T'.

   b) All order-n primitive predicates of T are effectively associated with an open sentence of T' in n free variables such that the predicate is satisfied by an n-tuple of values if and only if the open sentence is satisfied by the corresponding (under f) n-tuple of values.

   c) All reduction functions cited in (a) and (b) must be specifiable, and have empirical support.

2) T is a deductive consequence of T' together with the reduction functions specified in

(a), (b), and (c).

Condition (2) requires the semantic comparability of T and T'. It should be evident that the NWQ paradigm is the appropriate reduction relation for the case in which there is no observation-theory distinction. It is also appropriate for cases of the intermediate sort in which the theories do not share the same observation language, but the non-theoretical parts of the theories are amenable to NWQ reduction.

A.III.3 Model-Theoretic Reduction - The third reduction paradigm Schaffner considers he calls the Suppes paradigm (he notes that E.W. Adams has worked out some of the consequences of this approach). On this paradigm, reduction is effected if a model is constructed from a model of the reducing theory which is isomorphic to a model of the reduced theory. Schaffner points out that in this form, the reduction is not adequate, since two physical theories might be formally isomorphic, but quite different, e.g. hydrodynamics and heat flow theory. The problem of unwanted isomorphisms indicates that model-theoretic reduction has its limitations. Some means of delimiting the intended applications is required. I will discuss the Stegmuller-Sneed elaboration of the Suppes paradigm, which tries to satisfy this requirement, in Chapter 4 below. Inasmuch as their account of reduction is adequate, it also requires semantic comparability.

Schaffner shows that Suppes reduction is a necessary condition for NWQ reduction. It is possible to modify the Suppes reduction to make it a necessary condition of the KO reduction by taking models of only the observational part of a theory. This modification is also appropriate for the intermediate case.

Schaffner suggests that the major advantage of the model-theoretic approach is that it elucidates the methodology of reduction rather than its logic. I find the distinction somewhat artificial.

A.III.4 <u>Popper-Feyerabend-Kuhn</u> <u>Reduction</u> - The last form of reduction Schaffner considers is not properly a reduction at all, at least not on the view of its authors. He names it the PFK paradigm (after Popper, Feyerabend and Kuhn). On this view, T is reduced to T' if T is deducible from T' plus certain counterfactual premises which would in certain experimental contexts not be experimentally falsifiable, given the state of science. An example would be Galileo's law of free fall (Feyerabend), according to which the distance an object has fallen is proportional to the square of the time of its descent. This is derivable from Newtonian mechanics together with the counterfactual assumption that the earth's radius is infinite. The derivation of T from T' is approximate, since the assumption is close to being true for the experimental context in which Galileo worked: the radius of the earth is much larger than the distance he dropped his balls. If the reduction is adequate, it should provide more accurate experimentally verifiable predictions than T, and should explain why T failed (e.g. by overlooking some crucial variable) while explaining why T worked as well as it did. This would, of course, require semantic comparability.

Kuhn and Feyerabend, in particular, would resist accepting the PFK paradigm as a form of reduction. Kuhn would argue that the "crucial variable" was not overlooked, but excluded by the prior paradigm. The relevant premises are not counterfactual, but are part of the presuppositions of the dislodged paradigm. Feyerabend is quite direct:

> ...the remark that we explain "by approximation" is much too vague and general to be regarded as the statement of an alternative theory. As a matter of fact, it will turn out that the idea of approximation cannot any more be incorporated into a formal theory, since it contains elements which are essentially subjective. (1962 p 48)

Both Kuhn and Feyerabend, of course, assume incommensurability.

A.III.5 Schaffner Reduction - Schaffner's version of reduction takes elements of the NWQ paradigm and the PFK paradigm. Basically, the reduction is an NWQ reduction of a theory *T to T', where *T is a theory "strongly analogous" to T which produces numerical predictions "very close" to those of T, but more accurate experimentally. *T must be able to explain both the failures and successes of T.

Both the KO and NWQ reductions require a motivated relation between the reduced terms. In the case of the KO paradigm the referents of the related terms are actually identical, since the terms are terms of the same observation language: the identity is analytic. Under the NWQ paradigm, terms are related by the reduction function, which is a synthetic identity of properties and individuals. It is essential to the meaningfulness of this identity that the terms related can be compared in the experimental context, otherwise any proposed identity will be somewhat arbitrary.

Under Schaffner's reduction paradigm, the construction of *T guarantees its reducibility to T'; any difficulties hang on the relation of T to *T. This relation involves a strong analogy between the theories, and a numerical closeness of predictions. Numerical closeness is simple enough to determine (it requires either an explained difference, or a difference within the limits of experimental accuracy). If there is any problem, it is with the notion of strong analogy. Identity is the strongest form of analogy available, but it is too strong for the case at hand. If we weaken the notion of analogy, we are in danger of falling into a vague notion of similarity, which has all the dangers of model isomorphism discussed above. We need some device to ensure that the analogy holds only between the intended concepts, i.e. that the point of the analogy is the correct one. The concepts related by the analogy must be semantically comparable, otherwise there is no guarantee that we have anything more than formal similarity.

APPENDIX IV

The Sneed-Stegmuller Formalism

The current version of the Structuralist approach was influenced by a suggestion of Balzer to replace the extended cores of (Sneed 1971) and (Stegmuller 1976a). It is found in (Sneed 1976), and discussed by Stegmuller (1976b, 1979a). The implications of the Sneed approach are listed in some detail in (Stegmuller 1976a pp 241-246), and modified in section 8 of (Stegmuller 1979a).

A.IV.1 The Current Version of the Structuralist Account - A scientific theory is a conceptual structure which can generate a variety of claims about a loosely specified range of applications (Sneed 1976 p 120). This is in contrast to the view that a scientific theory *is* a set of claims, at least some of which are empirical. The basis for the conceptual structures called theories is a kind of set-theoretic structure called the core of a theory element, which can be used to make statements about another set-theoretic entity - the range of intended applications of a theory (p 121). Theories are composed of a basic theory element together with the means to construct a variety of nets of theory elements. Each theory net is a specialization of the basic core, and corresponds to an empirical claim about a range of intended applications, the whole network corresponding to a complex empirical claim about the whole range of intended applications. Overlapping applications are connected by constraints, which add further complexity. The over-all complexity, according to Sneed, gives theoretical concepts a "concreteness", and is the only way to provide "determination methods" (1976 p 121). The complexity of the structure is supposed to overcome the problem of unintended applications raised by Schaffner as an objection against Suppes-reduction (see A.III.3). In fact, Stegmuller turns the tables on the statement view, holding that the specification of the reference of theoretical terms within the statement view requires formal techniques which are incapable of being formulated in practice, and that the resulting ambiguity undermines its application.

A Sneedian theory is a rather diffuse structure, built up of various elements, and subject to variation in its components. the basic component is a theory element, which is composed of a theory element core, K, and a set of intended applications, I. K is itself composed of Mp, Mpp, M and C, which were described in section 4.1.1. For a given K we can define A(K), a class of subsets of Mpp, such that a subset of Mpp is in A(K) if and only if theoretical components can be added to each member the sets making up A(K) in a way which yields a subset of M (satisfying the laws of T), and such that the whole array of theoretical components satisfies C (satisfying the constraints on T). Intuitively, A(K) is composed of those classes of physical phenomena which are not ruled out by the laws and constraints of the theory. The empirical claim of a theory element <K,I>, then, is that I is a member of A(K).

The set of intended applications is rather difficult to define, partly because it is open-ended. Whether or not a given physical system is an element of I is a matter of decision, and will depend on the current state of the development of the theory. It may be that at a given time, for a given system, it is impossible to tell whether or not it is in I. Given that a given physical system s is the sort of thing the theory has been applied to in the past, it will generally be the case that s is in I. If it were discovered that some s of a quite different sort is a member of a subset of A(K), it might be decided that s is a member of I, thus widening the scope of the theory, or it might be decided that the correlation is merely coincidental, and that s is not in I.

Somewhat more interesting cases occur when an s which was thought to be in I, or would have been thought to have been in I otherwise, turns out not to satisfy the conditions of the theory. We then have two choices. We can either conclude that I is not an element of A(K), and reject the theory element, or else we can deny that s is a member of

I. Which choice is appropriate will depend on the circumstances of the particular case, and may not be fully determined for any given case.

The rejection of I as an element of A(K) for some theory element <K,I> doesn't require the rejection of the whole theory, since <K,I> may be some specialization of a more central theory element. A specialization is a more restricted application, using special laws and constraints, and generally having a smaller range of intended applications. A theory net is a set of theory elements ordered by the specialization relation. It is a partially ordered set, with the lower elements more specialized. We can also talk about parts of theory nets, and define a core net induced by a net N, called N*. We can then define A(N*), and make very general empirical claims of the sort 'I is an element of A(N*)' for a theory net N based on <K,I>. A theory based on <K,I> can develop both by more narrowly specifying the intended applications already determined as falling under the theory, and by enlarging the theory net under <K,I> (an expansion), extending the set of determined intended applications.

A.IV.2 <u>Holding Kuhn Theories</u> - Two parts of a Kuhnian disciplinary matrix, symbolic generalizations and exemplars, have natural representations in the theory net formalism. The symbolic generalizations are determined by the basic core of the theory. (The actual form they take is that of a "frame"; see (Stegmuller 1976a pp 107-109).) The exemplars are obviously a specific subset of the intended applications. The other component of a disciplinary matrix, models, has no obvious correlate in the formalism. This component plays a rather important role in the development of the theory, providing preferred analogies which have a heuristic function, and also form the basis for the ontological commitment of the theory. Stegmuller (1976a pp 177-180) recognizes the deficit, but argues that these factors affecting theory development belong to the study of psychology and sociology, not the metatheory of science.

The set of exemplars is naturally represented as a set Io of extensionally described (i.e. listed) Mpp's which is associated with a "paradigm theory net" Np. I for Np is composed of those applications which are "sufficiently like" Io (Sneed 1976 p 30, Stegmuller 1976a p 175). Np must be a sub-net of any larger net resulting from the theory's development. With these concepts, we can define a Kuhn theory as composed of a basic theory element Tb composed of a basic core Kb, the basic conceptual structure of the theory, and I, the full range of intended applications based on Io, and the set of all expansions of Np, E(Np). A Kuhn theory remains stable whereas the empirical claims are relative to the theory net one chooses from the expansion (Sneed 1976 p 131). The empirical claim of a Kuhn theory is characterized in the usual way. Sneed takes some pains to point out that the theory net formalism is neither restricted to, nor entails the existence of Kuhn theories.

A person p is said by Sneed to hold a theory T if and only if there is a basic theory element <K,I> with an expansion Et such that p believes at t that I is in A(Et), p has observable data supporting this belief, and p believes that Et can be further expanded (Stegmuller 1976a pp 169-170). A person p holds a Kuhn theory T=<<Kb,I>,Io,Np,E(Np)> at t if and only if p holds T at t, chooses Io as his set of paradigm examples, and believes at t this set is a subset of the applications he believes at t to have been determined by the theory (modified from (Stegmuller 1976a pp 194-1959). A further elaboration is the Kuhnian notion of hoding a theory, which relativizes a theory to a scientific community SC, and a time t (Stegmuller 1976a). A theory is then a quadruple <K,I,SC,t>. Stegmuller (1979b) later replaced time with a history h, further pragmatizing the notion of a theory.

These definitions are probably too strict in some respects, since the set of paradigms, Io, can change with time. Io is usually added to, but during the theory's development some applications may be dropped from Io as better ones are found. This relativisation of Io to time should present no serious difficulties.

Kuhn (1976) finds no reason to object to Sneed's account of a Kuhnian theory, except for the account of revolutionary change. Feyerabend (1977 p 360), on the other hand, objects that the Structuralist approach doesn't leave enough room for changes within the theory core, such as occured during the early development of Quantum Theory, when central ideas such as energy conservation were dropped temporarily and then picked up again. Inasmuch as the understanding of the theory was highly unstable at the time he refers to, I don't think his objection carries much weight except as an indication that the manner in which a theory is grasped needs to be taken into consideration. Since Feyerabend was arguing the advantages of the statement view, this may have been what he intended. He uses a nice turn of phrase to indicate his concern that the Structuralist approach is too abstract and ahistorical:

> ... metascience demands that [concepts] spring into the world like Pallas Athene from the forehead of Zeus (1977 p 362)

BIBLIOGRAPHY

Asquith, P. and T. Nickles

(1983) PSA 82 Vol. 2

Ayer, A.J.

(1946) Language, Truth and Logic, Penguin

Balzer, W.

(1979) Incommensurability and Reduction Acta. Phil. Fennica 30: 313-335

Balzer, W. and C.U. Moulines

(1980) On Theoreticity Synthese 44: 467-494

Balzer, W. and J.D. Sneed

(1977) Generalized Net Structures of Empirical Theories Studia Logica 36: 195-211, 37: 167-194

Born, M.

(1962) Einstein's Theory of Relativity, New York: Dover

Boyd, R.

(forthcoming) Realism and Scientific Epistemology, London: Cambridge U. Press

Bridgmann, P.W.

(1938) Operational Analysis Phil. Sci. 5:114-131

Brown, H.I.

(1979) Perception, Theory and Commitment, Chicago: U. of Chicago Press

Brown, T.M.

(1974) From Mechanism to Vitalism in Eighteenth Century English Physiology Journal of the Hist. of Biol. 7: 179-216

Butts, R.E. and J. Hintikka

(1977) Historical and Philosophical Dimensions of Logic, Methodology and Philosophy of Science, Dordrecht-Holland: D. Reidel

Cantor, G.N.

(1975) The Reception of the Wave Theory of Light in Britain: A Case Study of the Role of Methodology in Scientific Debate Hist. Stud. Phys. Sci. 6: 109-132

Carnap, R.

(1936-37) Testability and Meaning Phil. Sci. 3: 420-468; 4: 1-40

Chang, C.C. and H.S. Keisler

(1973) Model Theory, Amsterdam: North-Holland

Cohen, R. and M. Wartofsky

(1965) Boston Studies in the Philosophy of Science 2, Dordrecht: D. Reidel

Colodny, R.G.

(1965) Beyond the Edge of Certainty, Englewood Cliffs

(1966) Mind and Cosmos, Pittsburgh: U. of Pittsburgh Press

(1969) The Nature and Function of Scientific Theory, Pittsburgh: U. of Pittsburgh Press

Demopoulos, W. and M. Friedman

(1982) The Concept of Structure in Early Twentieth Century Philosophy of Science, University of Western Ontario, Centre for Cognitive Science, Cogmem 9

Descartes, R.

(1962) Treatise of Man trans. T.S. Hall, Cambridge: Harvard U. Press

Duhem, P.

(1954) The Aim and Structure of Physical Theory, New York: Atheneum

English, Jane

(1978) Partial Interpretation and Meaning Change J. Phil 75: 57-76

Feigl, H.

(1970) The 'Orthodox' View of Theories: Remarks in Defense as Well as Criticism, pp 3-16 in Radner and Winokar (1970)

Feigl, H., M. Scriven and G. Maxwell

(1958) Minnesota Studies in the Philosophy of Science, Vol II Minneapolis: U. of Minnesota Press

Feigl, H. and G. Maxwell

(1962) Minnesota Studies in the Philosophy of Science, Vol III, Minneapolis: U. of Minnesota Press

Feyerabend, P.K.

(1962) Explanation, Reduction and Empiricism, in Feigl and Maxwell (1962)

(1965a) Problems in Empiricism, in Colodny (1965)

(1965b) Reply to Criticism, in Cohen and Wartofsky (1965)

(1965c) On the "Meaning" of Scientific Terms J. Phil 62: 266-274

(1969a) Science Without Experience J. Phil 66

(1969b) Problems in Empiricism, Part II, in Colodny (1969)

(1970a) Against Method, in Minnesota Studies in the Philosophy of Science, Vol IV, Minneapolis: U. of Minnesota Press

(1970b) Philosophy of Science: A Subject with a Great Past, pp 172-183 in Stuewer (1970)

(1970c) Consolations for the Specialist, in Lakotos and Musgrave (1970)

(1975) Against Method, London: Verso

(1977) Changing Patterns of Reconstruction Brit. Jour. Phil. Sci. 28: 351-369

(1981) More Clothes from the Emperor's Bargain Basement Brit. Jour. Phil. Sci. 32: 57-94

Field, H.

(1977) Logic, Meaning and Conceptual Role J. Phil 74: 379-409

Frankel, E.

(1976) Corpuscular Optics and the Wave Theory of Light: The Science and Politics of a Revolution in Physics Social Studies of Science 6: 141-184

Galen

(1916) On the Natural Functions, trans. A.S. Brock, Cambridge: Harvard U. Press

Glymour, C.

(1980) Theory and Evidence, Princeton: Princeton U. Press

Grunbaum, A.

(1960) The Duhemian Argument Phil. Sci. 11: 75-87

Hanson, N.R.

(1958) Patterns of Discovery, London: Cambridge U. Press

Hall, T.S.

(1970) Descartes' Physiological Method: Position, Principles, Examples Jour. Hist. Biol. 3: 53-79

Harper, W.

(1977) Rational Conceptual Change, pp 462-494 in PSA 1976, 2

Hempel, C.G.

(1958) The Theoretician's Dilemma, pp 37-98 in Feigl et al (1958)

(1971) The Meaning of Theoretical Terms: A Critique of the Standard Empiricist Construal, in Suppes et al (1971)

Hooker, C.

(1972) Critical Notice of _Minnesota Studies in the Philosophy of Science, Vol IV (Part 2)_ _Can. Jour. Phil._ 1: 489-509

(1973) Metaphysiscs and Modern Physics, pp 174-304 in Hooker, ed., _Contemporary Research in the Foundations and Philosophy of Quantum Theory_, Dordrecht: D. Reidel

Kaplan, D.

(1978) 'Dthat', in P. Cole (ed) _Syntax and Semantics_, Vol 9

(1979) On the Logic of Demonstratives _J. Phil. Logic_ 1979, pp 81-98

Katz, J.

(1978) Effability and Translation, pp 191-234 in F. Guenther and M. Guenther-Reutter, _Meaning and Translation_, New York: New York U. Press

(1979) Semantics and Conceptual Change _Phil. Rev._ 88: 327-365

Kierkegaard, S.

(1941) _Concluding Unscientific Postscript_, trans. D.A. Swensen and W. Lowrie, Princeton: Princeton U. Press

(1954) Fear and Trembling, in W. Lowrie (trans) _Fear and Trembling and Sickness Unto Death_ Princeton: Princeton U. Press

(1959) _Either/Or_ trans. M. Swensen, Princeton: Princeton U. Press

King, L.S.

(1972) Medical Theory and Practice at the Beginning of the 18th Century _Bull. Hist. Med._ 46: 1-15

Kordig, C.R.

(1971) _The Justification of Scientific Change_, Dordrecht: D. Reidel

Kripke, S.

(1979) A Puzzle About Belief, in A. Margalit (ed) _Meaning and Use_, Dordrecht: D. Reidel

Kuhn, T.S.

(1957) _The Copernican Revolution_, Harvard: U. of Harvard Press

(1970a) _The Structure of Scientific Revolutions_, 2nd edition; Chicago: U. of Chicago Press

(1970b) Reflections on My Critics, pp 231-278 in Lakatos and Musgrave (1970)

(1976) Theory-Change as Structure-Change: Comments on the Sneed Formalism _Erkenntnis_ 10: 179-199, reprinted in Butts and Hintikka (1977)

(1977a) Second Thoughts on Paradigms, in Suppe (1977), reprinted in Kuhn (1977d)

(1977b) The Essential Tension, in Kuhn (1977d)

(1977c) Objectivity, Value Judgement and Theory Choice, pp 320-337 in Kuhn (1977d)

(1977d) _The Essential Tension_, Chicago: U. of Chicago Press

(1983a) Commensurability, Comparability, Communicability, in Asquith and Nickles (eds) (1983)

(1983b) Rationality and Theory Choice _J. Phil_ 80: 563-570

Lakatos, I. and A.E. Musgrave

(1970) _Criticism and the Growth of Knowledge_, London: Cambridge U. Press

Laudan, L.

(1965) Grunbaum on the 'Duhemian Argument' Phil. Sci. 32: 295-299

(1977) Progress and its Problems California

Lewis, D.K.

(1970) How to Define Theoretical Terms J. Phil 13: 427-446

(1972) Psychophysical and Theoretical Identifications Aust. Jour. Phil. 50: 249-258

Marcus, R.B.

(1981) A Proposed Solution to a Puzzle About Belief, in Midwest Studies in Philosophy VI, Minneapolis: U. of Minnesota Press

Masterman, M.

(1970) The Nature of a Paradigm, pp 59-90 in Lakatos and Musgrave (1970)

Mayr, D.

(1976) Investigations of the Concept of Reduction I Erkenntnis 10: 275-294

(1981) Investigations of the Concept of Reduction II Erkenntnis 16: 109-129

Medawar, P.

(1967) The Art of the Soluble, Penguin

Moberg, D.

(1979) Are There Rival Incommensurable Theories? Phil. Sci. 46: 244-262

Montague, R.

(1968) Pragmatics, pp 102-122 in Klibansky (ed) Contemporary Philosophy: A Survey Florence, reprinted in Montague (1974) pp 95-118

(1970) Pragmatics and Intensional Logic Synthese 22: 68-94, reprinted in Montague (1974) pp 119-147

(1974) Formal Philosophy, ed. R. Thomason, Yale: Yale U. Press

Moravia, S.

(1978) From Homme Machine to Homme sensible: Changing Eighteenth Century Models of Man's Image Jour. Hist. Ideas Jan. 1978

Moulines, C.U.

(1976) Approximate Reduction of Theories: A General Explication Erkenntnis 10: 201-227

Olson, R.

(1969) Scottish Philosophy and Mathematics 1750-1830 Jour. Hist. Ideas 32: 19-44

Pearce, D.

(1982a) Stegmuller and Kuhn on Incommensurability Brit. Jour. Phil. Sci. 33: 389-396

(1982b) Logical Properties of the Structuralist Conception of Reduction Erkenntnis 18: 107-334

Perry, J.

(1977) Frege on Demonstratives Phil. Rev. 86: 474-497

Putnam, H.

(1974) The Refutation of Conventionalism Nous 8: 25-40

Quine, W.V.O.

(1953) Two Dogmas Of Empiricism, in Quine From a Logical Point of View, Cambridge: Cambridge U. Press

(1960) Word and Object B, Cambridge: MIT Press

Radner, M. and S. Winokur

(1970) Minnesota Studies in the Philosophy of Science, Vol IV, Minneapolis: U. of Minnesota Press

Rescher, N.

(1977) Methodological Pragmatism, New York: NYU Press

Rogers, H.

(1967) The Theory of Recursive Functions and Effective Computability, New York: McGraw-Hill

Poincare, H.

(1905) Science and Hypothesis, New York: The Science Press

Reichenbach, H.

(1958) The Philosophy of Space and Time, New York: Dover

Rorty, R.

(1979) Philosophy and the Mirror of Nature, Princeton: Princeton U. Press

Schaffner, K.E.

(1967) Approaches to Reduction Phil. Sci. 34: 137-147

Scheffler, I.

(1967) Science and Subjectivity, Indianapolis: Bobbs-Merrill

Segerberg, K.

(1973) Two-Dimensional Modal Logic J. Phil. Logic 2: 77-96

Shapere, D.

(1966) Meaning and Scientific Change, pp 41-85 in Colodny (1964)

Sneed, J.D.

(1971) The Logical Structure of Mathematical Physics, Dordrecht: D. Reidel

(1976) Philosophical Problems in Empirical Science: A Formal Approach Erkenntnis 10: 115-146, reprinted in Butts and Hintikka (eds) (1977)

Stalnaker, R.

(1976) Propositions, in A. MacKay and D. Merrill (eds) Issues in the Philosophy of Language, Yale: Yale U. Press

(1978) Assertion, in P. Cole (ed) Syntax and Semantics 9: Pragmatics

(1981) Indexical Belief Synthese 49: 129-152

Stegmuller, W.

(1976a) The Structure and Dynamics of Theories, New York: Springer-Verlag

(1976b) Accidental (Non-Substantial) Theory Change and Theory Dislodgement: To What Extent Logic Can Contribute to a Better Understanding of Certain Phenomena in the Dynamics of Theories Erkenntnis 10: 147-178, reprinted in Butts and Hintikka (eds) (1977)

(1979a) The Structuralist View of Theories, New York: Springer-Verlag

(1979b) The Structuralist View: Survey, recent Developments and Answers to Some Criticisms Acta. Phil. Fennica 30: 113-129

Stich, S.

(1979) Between Chomskian Rationalism and Popperian Empiricism Brit. Jour. Phil. Sci. 30: 329-347

Stuewer, R.H.

(1970) Historical Perspectives of Science Minnesota Studies in the Philosophy of Science, Vol V, Minneapolis: U. of Minnesota Press

Suppe, F.

(1977) The Structure of Scientific Theories, 2nd ed., Urbana: U. of Illinois

Suppes, P., L. Henkin, A. Joja and C.G. Moisal

(1971) Logic, Methodology and Philosophy of Science 4, Amsterdam: North Holland

Szumikwicz, I.

(_977) Incommensurability and the Rationality of the Development of Science Brit. Jour. Phil. Sci. 28: 345-350

van F~aassen, B.C.

(1980) The Scientific Image, Oxford: Clarendon Press

Watkins, J.

(1970) Against 'Normal Science', pp 25-38 in Lakatos and Musgrave (1970)

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