A DEFENCE OF KUHN'S INCOMMENSURABILITY THESIS

By

TIMOTHY ALEXANDER KOSUB

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Abstract

Kuhn's incommensurability thesis is the claim that successive scientific theories often conflict not only logically but also normatively: i.e. they differ both about nature and also about the use of common apparatus, concepts and experimental results, and what are proper scientific goals and methods. Critics commonly object that Kuhn's thesis attacks such traditional scientific values as objectivity and rationality. But their strongest response can be expressed as a dilemma: either, if taken literally, the incommensurability thesis is self-contradictory; or, if that literal reading is rejected, this thesis has no philosophical import. Kuhn claims his critics have misinterpreted his thesis and he maintains both its intelligibility and relevance. The problem is whether his position can be sustained.

In support of Kuhn, I argue that his critics' reading of his thesis is based on a mistaken identification of logic with formal logic and, more generally, of comparability with commensurability. I argue that logical comparison of theories that lack common concepts is possible if one can compare theories directly, as whole to whole, and that such direct logical comparison is actually commonplace in natural languages. I also argue more generally that Kuhn's critics' identification of comparison with commensuration leads to a vicious regress.

My attempt at resolving the dispute between Kuhn and his critics is informed by a simple "hermeneutic" principle: if one view seems either unintelligible or irrelevant to the other, then

both sides probably disagree on the interpretation of shared concepts. Once the focus of the dispute is located, arguments can often be given for preferring one interpretation over another. Thus if I am right that Kuhn's critics' view wrongly equates comparability with commensurability and logic with formal logic, that view clearly must be replaced by one that distinguishes them.

I argue that if those distinctions are made, incommensurability can be seen to represent no essential threat to scientific rationality and objectivity. In this light, I suggest Kuhn's major analytic concepts be viewed as improvements on more traditional notions drawn from formal logic. I also use a historical case study of the original discovery of geometrical incommensurability to illustrate further Kuhn's concepts and to develop a more general notion of a proof of incommensurability that is applicable to scientific theories.

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Chapter 1

Commensurability, Comparability and Logic

1.0 Introduction

In *The Structure of Scientific Revolutions*, T. S. Kuhn marshals much historical, psychological and philosophical evidence for his thesis that successive scientific theories are often both logically incompatible and incommensurable (Kuhn 1970a, 96, 103). Kuhn contends that the existence of incommensurable theories is ruled out by traditional epistemology and philosophy of science; so he concludes that the traditional view must be replaced by a "viable alternative" that can accommodate incommensurability (Kuhn 1970a, 121).

Kuhn's critics have raised many, serious objections to the incommensurability thesis. Most are methodological and "moral" (Scheffler 1967, 8) objections, which claim incommensurability leads to such bogies as relativism and irrationalism. But Kuhn's opponents' deepest challenge is that his thesis is either self-contradictory, if taken literally, or philosophically irrelevant, if not. Thus his opponents take the standard meaning of "incommensurable" to be "absolutely incomparable" and argue that, because logically incompatible theories are comparable, Kuhn's thesis is "self-refuting" (e.g. Putnam 1981a, 114). And they counter that if, in Kuhn's usage, "incommensurable" does not mean "absolutely incomparable," then the evidence he musters for his thesis can have only psychological or pragmatic significance. Nevertheless, Kuhn rejects his critics' analysis and continues to maintain the philosophical importance of his thesis.

In this chapter, I defend Kuhn's rebuttal by showing that his critics' arguments depend on a mistaken identification of logic with formal logic and, more generally, of comparison with commensuration. I argue that Kuhn's thesis is perfectly intelligible if those notions are distinguished. In addition, I provide textual evidence that Kuhn himself distinguishes those concepts in his work. My arguments cannot, by their nature, provide positive evidence for the actual existence of incommensurable theories. Providing such positive evidence is not my aim here since Kuhn has already satisfied this demand, as shown by the vigorous critical reaction to his thesis.[1]

Instead of arguing for actual incommensurability, I seek to remove an important obstacle to a fair evaluation of Kuhn's evidence. But I believe my arguments perform more than that negative function. If I am right that the traditional philosophy of Kuhn's critics precludes incommensurability because it is founded on a mistaken identification of distinct concepts, my arguments also provide positive support for Kuhn's claim that we need a new "epistemological paradigm" (Kuhn 1970a, 121): one that embodies those conceptual distinctions, and thus allows for incommensurability. It turns out that, contrary to what some of Kuhn's critics have claimed, this new epistemology need not eschew traditional scientific values such as objectivity and rationality,

1.1 The Incommensurability Thesis

On the traditional view, science is seen (ideally) to progress through theory change by accumulating facts whose characterization is theory-neutral and by an increasing verisimilitude to a theory-independent reality. The rejection of a current scientific theory in favour of a new one is deemed *rational* on this view if it can be shown *neutrally*--i.e. on grounds common to both theories--that the new theory incorporates the positive results of its predecessor while avoiding its errors and that the new theory has greater explanatory and predictive scope (Collier 1984, 9).

A premise of such neutral comparison, and therefore, of traditional philosophy of science, is that successive scientific theories have the same basic logical structure, at least for those domains in which they compete. Thus to compare successive theories neutrally for empirical adequacy in those domains, the traditional view requires that the meanings of each theory's referring expressions and other non-logical concepts do not change (Newton-Smith 1981, 10). But it is precisely this basic premise of conceptual stability between successive theories that Kuhn claims his historical studies show to be mistaken. In fact, Kuhn's notorious *incommensurability thesis* is the claim that theory change in science is often, if not always, so revolutionary that successive theories lack the common concepts and referring expressions required for neutral logical comparison (Kuhn 1970a, 102).[2]

Historically, Kuhn argues, the cause of an earlier theory's[3] failure to cope with experience often lies not just with that theory's empirical content but also with its concepts, which are its means of expressing that content (Kuhn 1970a, 53). That is, often the discovery of an "*unexpected* novelty" (*ibid.*, 35) is not only empirically, but also conceptually, anomalous because that novel phenomenon appears to fall within the theory's scope, but actually conflicts with assumptions implicit in the theory's categories (*ibid.*, 63-4). The new theory, Kuhn claims, has "to change the meaning of [such problematic] concepts" (*ibid.*, 102) if it is to avoid both of its predecessor's difficulties (*ibid.*, 56, 64).[4]

Moreover, Kuhn also argues that a scientific theory's concepts form an "integrated" whole (*ibid.*, 129) or "conceptual web" (*ibid.*, 149); so the required change of meaning of the particularly problematic concepts also changes the meaning of *all* the other concepts the new theory retains from its predecessor. Such a revolutionary conceptual change between successive theories, he says, makes them "fundamentally incompatible" (Kuhn 1970a, 98): it simultaneously results in both "substantive" (i.e. logical) and "normative" (i.e. conceptual, methodological, observational) incompatibility (*ibid.*, 103, 109).

...paradigms [or, theories; see *n*3] differ in more than substance, for they are directed not only to nature but also

> back upon the science that produced them. They are the source of the methods, problem-field, and standards of solution accepted by any mature scientific community at any given time. As a result, the reception of a new paradigm often necessitates a *redefinition* of the corresponding science (*ibid.*, 103. Italics mine).

That redefinition of science, Kuhn claims, "changes...the standards governing *permissible* problems, concepts, and explanations" (Kuhn 1970a, 106. Italics mine). As a result, not only does the truth of one view imply the other's falsity (*ibid.*, 98)[5] but often the "problems, concepts, and explanations" of one view appear either incoherent or scientifically irrelevant by the standards of the other (Kuhn 1970a, 103-110). In brief: "The normal-scientific tradition that emerges from a scientific revolution is not only [logically] *incompatible* but often actually *incommensurable* with that which has gone before" (Kuhn 1970a, 103. Italics mine).

Kuhn draws the following general logical and epistemological conclusions from the incommensurability thesis: (1) because there are no theory-neutral grounds for determining which of two incommensurable views is true (or false), the concept of truth has an "unproblematic" application only *intra*theoretically (Kuhn 1970b, 264);[6] hence, (2) the traditional ideal of science progressing *toward* an antecedently-understood aim of truth is a chimaera. Still, Kuhn believes that, *despite* the absence of a neutral standpoint, it is possible to compare *current* theory with *previous* ones (Kuhn 1970b, 264). And such backward-looking comparison often shows later theory to be an improvement on earlier ones

(Kuhn 1970b, 264). That suggests, Kuhn says, that we should "substitute evolution-from-what-we-do-know for the [traditional] evolution-toward-what-we-wish-to-know" (Kuhn 1970a, 171) and come to see progress in science as "a process of evolution *from* primitive beginnings" (*ibid.*, 170. Italics in original).

Philosophers have raised various serious objections to the incommensurability thesis: they have claimed that it is simply false; that it is based on a vague theory of meaning; that it makes rational theory choice impossible; that it eliminates the critical impact of experiment on theory; that it leads to idealism and relativism; and that it is otherwise "methodologically and epistemologically undesirable" (Lodynski 1982, 91). But by far the strongest criticism philosophers have made of Kuhn's thesis, and the most revealing of their basic philosophical commitments, is that it is either "unintelligible" because "self-refuting" (e.g., Newton-Smith 1981, 148-9; Putnam 1981a, 126; Lodynski 1982, 91); or philosophically-irrelevant because describing purely "humdrum," psychological facts about scientists (Musgrave 1980, 51) or entirely "modest" conceptual disparities between scientific theories (Davidson 1984, 184). Nevertheless, Kuhn completely rejects both alternatives and the interpretations of his thesis they are based on.[7]

Kuhn's critics' portrayal of the incommensurability thesis, as either self-refuting or philosophically-irrelevant, strikingly resembles those he says generally "characterize discourse between participants in incommensurable points of view"

(Kuhn 1970b, 230; c.f. p5 above). Thus the failure of his critics to find an interpretation of his thesis that makes it both intelligible and philosophically-relevant suggests their views may simply be incommensurable with his (*ibid.*, 230). In other words, Kuhn's view may not be unintelligible or irrelevant *simpliciter*, just so *relative* to his critics' perspective. And since, by definition, incommensurable points of view interpret common concepts differently (see pp3-4 above), we should expect that Kuhn's position, *if it is intelligible or philosophically-relevant*, must involve a different and coherent understanding of those concepts that lead to their argumentative impasse.[8] Before defending the intelligibility and relevance of Kuhn's position, however, I shall first illustrate the historical argument for the incommensurability thesis with Kuhn's discussion of the Proust-Berthollet controversy. This discussion will hopefully provide both a clearer picture of the notion of *logico-normative* incompatibility and an indication of one source of the impasse between Kuhn and his critics: the nature of logic.

1.1.1 History and the Incommensurability Thesis

One historical example Kuhn cites of a pair of "fundamentally incompatible" theories are those of the French chemists J. L. Proust and C. L. Berthollet.[9] Proust held that all chemical reactions take place in fixed, integral ratios but Berthollet insisted that most occur in continuously-varying proportions. On the face of it, these propositions are examples of the more familiar, less global, notion of incompatibility since both seem to be expressible as formal contradictories using common (logical and non-logical) concepts.[10] But Kuhn points out that not only did Proust dispute Berthollet's view of chemical processes, he also differed with him about the meaning of the concept, *chemical reaction*, used in expressing their apparently incompatible claims. Hence he also differed with him about the meanings of the related concepts of chemical compound and physical mixture.[11]

Thus though both chemists allowed that, "when mixing [of chemicals] produced heat, light, effervescence or something else of the sort," a chemical reaction had occurred, only Berthollet took the behaviour of "salt in water, alloys, glass, oxygen in the atmosphere, and so on" as also evidence of a chemical reaction (Kuhn 1970a, 131). Because this latter class of substances can form combinations of smoothly-varying proportions, in ratios from zero to a fixed limit, Berthollet argued that they provided clear evidence for his theory. And since such compounds are more commonly found than those combining in fixed proportions, he concluded that variable compounds were the norm, fixed ones the exception (Meldrum 1910, 5). By contrast, because he rejected the idea that solutions were chemical compounds, Proust denied that Berthollet had proved his point. And he insisted that only those cases of mixing that produced compounds of fixed proportions were chemical reactions.

Kuhn here argues that we should not construe this difference of concepts as a trivial definitional equivocation (Kuhn 1970a, 131). For, he claims, Proust's and Berthollet's respective choices of definition of *chemical reaction* and *compound* had substantive, incompatible consequences for the ways in which they explained their common environments. Thus, because he believed that compounds of variable composition were the norm, Berthollet sought to find instances of them produced by chemical reactions meeting the criteria that Proust also accepted (giving off heat, light, gas, etc.). And such experimental evidence was not hard to find. Thus Berthollet was able to show that "metals such as copper, tin and lead, on heating in air can take up oxygen *continuously* in proportions increasing to a fixed limit, giving a continuous series of oxides, as shown in some cases (e.g. lead) by varying colour changes" (Partington 1957, 156. Italics in original).

On the other hand, because of his commitment to his "definition," Proust repeated Berthollet's experiments and claimed that the *same* operations showed "that these oxides were mixtures of two, or a small number, of definite oxides, and he carefully distinguished between mixtures and solutions...and chemical compounds" (*ibid.*, 156). Nevertheless, the experimental evidence was completely equivocal; "the two men necessarily talked through each other and their debate was entirely inconclusive" (Kuhn 1970a, 132). Thus it seems that Proust's and Berthollet's conflicting definitions of concepts also made their theoretical conclusions incompatible. And, equally, as we shall see, their choice of incompatible theories also seemed to force those scientists to interpret shared concepts in conflicting ways.

Berthollet's claim that saline is a chemical compound, and not a physical mixture, was a natural consequence of his new theory of affinity, which he constructed to overcome the flaws of the older version. Affinity theory took chemical compounds to result from a mutual "elective" affinity between their constituent elements. Thus lumps of elemental substances like silver held together because of the affinity of their corpuscles for each other; mixed compounds formed because the corpuscles of the different elements had a greater mutual affinity for each other than for those of their own sort; and solutions formed because the solute's particles had a greater affinity for those of the solvent than for each other. "In the eighteenth century the theory of elective affinity was an admirable chemical paradigm, widely and sometimes fruitfully deployed in the design and analysis of chemical experimentation" (Kuhn 1970a, 130).

Prior to Berthollet, chemists had combined a belief in affinity theory with an acceptance of the generality of Richter's law of constant proportions (Meldrum 1910, 2). But the old affinity theory was plagued by anomalies: for example, it couldn't explain the phenomenon of "mass action." The chemist Bergman had shown that, for reactions of the form: "$Ad + c = Ac + d$, where Ac is precipitated" (Meldrum 1910, 4),[12] depending on the particular A, d and c involved, from two to six times the *mass* of c was needed to saturate A when combined with *d*, than when A was uncombined. This phenomenon was clearly incompatible with the older affinity theory's acceptance of the doctrine of fixed composition; for under different circumstances forces of affinity seemed to permit the formation of compounds of widely-varying proportions.

Berthollet chose to resolve the older theory's anomaly by keeping the proven notion of chemical affinity but "obliterat[ing] the [traditional] distinction between chemical and physical forces" (Meldrum 1910, 4). Thus he took "*all* forces of affinity [to be] modified gravitational attraction" (Partington 1957, 157*n**). That choice allowed Berthollet to keep intact much of the theoretical apparatus of the older theory of affinity but made it impossible to differentiate any longer between chemical compounds and solutions. Mere physical mixtures were now just those combinations of substances that failed to show cohesion: for example, those whose constituents "could be distinguished by eye or mechanically separated" (Kuhn 1970a, 131). And Berthollet claimed the relatively few compounds of fixed proportions occurred because of the ways in which their specific structures interacted with certain environments. Fixed compounds, he said, "were formed as a result of the interference of extraneous physical forces. For example, certain proportions of the elements could produce a compound which was least soluble or of greatest density (influence of cohesion), or the most volatile (influence of elasticity) of all the possible compounds, and hence this compound was formed in preference" (Partington 1957, 157).

Instead of choosing to keep the time-tested concept of affinity from the older theory, however, Proust opted to retain that theory's law of constant proportions and its distinction between solution (*qua* physical mixture) and chemical compound. However, that left him with no alternative positive account of what forces kept substances like salt and sugar, as mere physical mixtures, in solution; he could only reply that such solutions clearly differed in nature from standard chemical compounds like salt and sugar themselves. So the forces that bound solvent and solute together were *conceivably* very different from chemical forces. Hence, he argued, the behaviour of solutions should not be used as counterexamples to his theory (Partington 1957, 157).

That rebuttal did not initially prove very convincing to chemists because Berthollet's new affinity theory was the only method then available for explaining "mass action" (now understood as "chemical reaction rates" (Day, *General Chemistry,* 1974). Proust's side had to wait for Dalton's atomic theory and the work of Berzelius before they could begin to match Berthollet's account in this area (Meldrum 1910, 8, 12). Dalton's new theory, based on a law of *multiple* (as opposed to either fixed or continuously-varying) proportions had much greater apparent scope and implications than either Berthollet's or Proust's theories. It made sense of a clear demarcation of solutions from compounds; handled the formation of metal oxides and salts in a less equivocal manner than either Berthollet or Proust; accounted for Gay-Lussac's law of combining volumes of gases; *and* was also able encompass the phenomenon of mass action (Meldrum 1910, 12-14). Dalton's "new way of practicing chemistry...proved so rapidly fruitful that only a few of the older chemists in France and Britain were able to resist it" (Kuhn 1970a, 134). Still, it "resolved" (for most chemists, but not Berthollet) the dispute by introducing a new theory and set of concepts that were incompatible with *both* those of Berthollet and Proust. And it also left unsolved a problem Berthollet's theory could handle: the behaviour of solutions (*ibid.*, 131).

In summary, it seems, at least intuitively, that Berthollet's and Proust's (and Dalton's) competing theories of chemical reactions and compounds also involved competing concepts for picking out instances of those reactions and compounds. Kuhn points out a still further aspect of this putative logico-normative incompatibility. Based on conclusions he draws from Gestalt psychology and the work of the philosopher N. R. Hanson, he argues that the views of Proust and Berthollet were not only logically and conceptually, but also *perceptually* or *observationally*, incompatible (see Kuhn 1970a, 112-114). Thus he claims that not only was it impossible for either scientist to understand or formulate the other's theory using his own concepts; it was also impossible for either scientist to perceive the world in the same way as his opponent. Thus "where Berthollet saw a compound that could vary in proportion, Proust saw only a physical mixture" (*ibid.* 132; c.f. *ibid.*, 198). In other words, even their "observation languages" were "theory-laden," and thus incompatible. Because of this putative total incompatibility of their points of view, Kuhn concludes that Proust and Berthollet could not find any common ground to settle their dispute "and their debate was entirely inconclusive" (Kuhn 1970a, 132).

1.2 The Critical Response: Logic and Formal Logics

If Kuhn is right that there are theories, like Proust's and Berthollet's, that are simultaneously logically, conceptually and perceptually incompatible, it seems clear that the logical incompatibility of those theories could not be *formally* demonstrated. This is because: (1) to show on the basis of "logical form" that two entities (e.g. sentences, theories) are incompatible, formal logics require that those entities have common concepts;[13] and (2) the fact that the concepts of two putatively incommensurable theories form distinct, "conceptual webs" means that one cannot *construct* commonly-acceptable concepts from the two theories from specific, local instances of logical conflict or congruence between them. That is, because the meanings of the other, incompatible theory's concepts are given by their *entire* role in that theory, that theory's content cannot be described except by concepts which presuppose that content's truth.[14]

It seems clear, therefore, that *if* there are theories that are both logically and conceptually incompatible, since formal logics apparently cannot demonstrate that incompatibility, the general concept of logical incompatibility must have a larger scope than the specific formal one. On the other hand, however, the fact that Kuhn's critics do not generally recognize this option, suggests strongly that they believe that logical incompatibility is a purely formal notion.[15]

Thus a standard objection to Kuhn's analyses is that, because debates like those of Proust and Berthollet seem stalled largely by their equivocal use of common expressions, Kuhn has not "justified" the claim that such views are *actually* incompatible (Newton-Smith 1981, 149). Instead it is claimed that if they were truly incompatible, it should be possible to analyze further those theories' concepts and express their conflict as a formal contradiction using common concepts. For example, it might be the case that Proust and Berthollet shared tacit, common criteria for distinguishing chemical compounds from physical mixtures, which might be made explicit through formal analysis; in which case, their views would differ only logically, not conceptually. On the other hand, if Proust's and Berthollet's concepts were different, Kuhn's critics infer that, therefore, appearances and intuitions to the contrary, their theories were really compatible (c.f. Newton-Smith 1981, 150).

Kuhn's reply to the first alternative is to note that scientists seldom recognize the results of such formal "analyses" as equivalent with their own usage (Kuhn 1970a, 47; Kuhn 1977, 305). Moreover, he also notes that when a historian like himself tries to analyse the logical bases of older theories, he invariably finds that "phrased in just that way, or in any other way he can imagine, [those logical reconstructions] would almost certainly have been rejected by some members of the group he studies" (Kuhn 1970a, 44). In other words, Kuhn is saying that, in his experience, formal reconstructions of historical theories always misrepresent their content. And he appeals to Wittgenstein's work on "family resemblance" concepts to argue that there need be no tacit, formal structures underlying scientists' reasoning (*ibid.*, 47).[16]

Of course, one might object that scientists' denials of the congruence of their concepts with the results of analysis are beside the point since the issues involved in those reconstructions lie outside their expertise (Collier 1984, 96). To see that this objection fails to address Kuhn's point one must keep separate the following issues: (1) whether or not scientists are necessarily qualified to judge, say, the relations between their own and historical theories or the philosophical implications of their work; and (2) whether or not scientists are exceptionally qualified to determine when formal reconstructions change the meanings of their concepts *in their scientific context*. It seems clear that the answer to the first question is that scientists are not specially qualified by their scientific work in either historical reconstruction or philosophical analysis. But it seems equally clear that the answer to the second question is that scientists are surely the most qualified to recognize the noncongruence of their *own* usage with formal reconstructions (supposing that they understand the formal language). And Kuhn obviously has the second question in mind (along with its supposition) in the above claim (p15-16; see also Kuhn 1977c, 303*n*13). Thus if a "reinterpretation in the broader context" (Collier 1984, 96) seems to a scientist to change his concepts, that belief deserves serious consideration. More important, since the *point* of such reinterpretations is to provide a *neutral* framework for scientific debate, the fact that scientists from both camps would likely find those reinterpretations inaccurate would only defeat the purpose of that supposedly neutral reinterpretation.

Kuhn's counter to the second claim that theories that cannot be reconstructed as formally incompatible are simply different and compatible has been, as we have seen, to try to make the incompatibility of such theories seem intuitively obvious. And, though intuitions notoriously differ, the theories he describes as incompatible are usually also taken as such by his critics (e.g., Newton-Smith 1981, 158). Thus, again, Kuhn's critics' insistence that theories that seem intuitively incompatible are either reconstructible as formally incompatible or not really incompatible suggests that the real issue between them and Kuhn is whether or not the canons of formal logics fully capture the notion of logical incompatibility.

In the following sections, I use comments and arguments from Kuhn's critics to show that, to preserve the intelligibility and relevance of his thesis, he must differ with them not only on the nature of logic but also on the more general notion of comparison. And I give textual evidence to imply that Kuhn would actually want to differ from his critics in the ways I suggest. I then link that dispute about comparison and logic to the ancient problem of universals and argue that the position of Kuhn's critics leads to an infinite regress.

1.3 The Critical Response: Comparability and Commensurability

As we have just seen, one primary source of Kuhn's opponents problems with the incommensurability thesis is their identification of logic with formal logic. But there is a still more basic source of his critics' difficulties: their equation of "incommensurable" with either "absolutely incomparable" or "not inter-translatable."

As noted above (p6), often Kuhn's opponents' strategy is to confront him with a dilemma. With respect to the concept of incommensurability itself, they commonly argue that either (1) if "incommensurable" is read 'literally'--as meaning "absolutely incomparable"--Kuhn's thesis turns out to be the self-contradictory claim that certain theories are both (absolutely) incomparable and comparable;[17] or (2) if that *prima facie* reading is rejected, his thesis is not self-contradictory, but "incommensurability" turns out to be only his misleading expression for what are merely psychological or pragmatic problems in comprehending, comparing or translating alien views.[18]

It is easy to see why Kuhn's opponents' interpretation of "incommensurable," as meaning literally either "absolutely incomparable" or "not intertranslatable," makes his thesis seem unintelligible. For, *given their interpretation*, since it is trivially obvious that to call two theories incompatible is *both* to compare *and* contrast them,[19] it follows that Kuhn could not *possibly* find historical examples of successive theories that are "not only incompatible but often actually incommensurable" (Kuhn 1970a, 103). Further, *on this interpretation*, since in his writings Kuhn repeatedly translates, in a common (natural) language (contemporary English), theories he claims are incommensurable with contemporary ones, it follows that he could not possibly use *this* work to prove those theories were truly incommensurable.

Donald Davidson, who takes "incommensurable" to mean "not intertranslatable" (Davidson 1984b, 190), makes exactly this last point when he remarks ironically: "Kuhn is brilliant at saying what things were like before the revolution using--what else? --our post-revolutionary idiom" (*ibid.* 184). Even a philosopher as sympathetic to Kuhn's overall view of science as Hilary Putnam (c.f. Putnam 1981b, esp. 69-78) finds his thesis to be "self-refuting" (Putnam 1981a, 114), and for exactly the same reasons as Davidson (*ibid.*, 116). Thus Putnam observes that, since "incommensurable" means "not intertranslatable" (*ibid.*, 114), for Kuhn (or Paul Feyerabend) to "tell us that Galileo had 'incommensurable' notions *and then to go on to describe them at length* is incoherent" (Putnam 1981a, 115. Italics in original).

Kuhn's critics' identification of "*in*-commensurable" with "absolutely *in*-comparable" (and "*not*-intertranslatable") provides a clue to the deep source of their dispute with him: it shows that they take "comparable" (and "intertranslatable") to mean "commensurable," which itself means literally "measurable [broadly: comparable] *by a common standard*" (*Concise Oxford English Dictionary*, s.v. "commensurable"). But "comparable" and "comparable by a common standard" are not self-evidently synonymous expressions. Further, it also seems possible to distinguish the locutions "comparable in a common *natural language*" (intertranslatable) and "comparable in a common *system of concepts*" (commensurable) (c.f. *n8*, *n14*); for *natural* languages may not be fully uniform conceptual systems. So whether "commensurable" should be conflated with, or distinguished from, either "comparable" or "intertranslatable" seems to be a significant philosophical issue. And Kuhn has gone on record as denying that "incommensurable" means either "incomparable" or "not intertranslatable" (e.g., Kuhn 1970b, 267; Kuhn 1976, 191; Kuhn 1970a, 201-4).

On the other hand, however, if one took comparability to require commensurability, because logical incompatibility clearly involves (logical) comparability, he would naturally also conclude that logically incompatible theories cannot be incommensurable. Thus we find that one of Kuhn's main critics, Dudley Shapere, who explicitly equates "incommensurable" with "absolutely incomparable" (Shapere 1981, 55), also says that "in order for [two theories] to be inconsistent with one another, they must be formulated, or at least formulable, in a *common language*." (*ibid.* 44n10. Italics mine). Shapere concludes that "it is difficult to see how one could construct a theory which, while differing in the meanings of all its terms from another theory [i.e., is incommensurable with it], can nevertheless be inconsistent with that other theory" (*ibid.*, 44).

As the following passage reveals, another prominent opponent of Kuhn's views, Israel Scheffler, also identifies "comparable" with "commensurable"; thus he wonders how paradigms that are

> ...based in different worlds, and address themselves to different problems with the help of different standards ...[can be] said to be in competition? To declare them in *competition* is, after all, to place them within *some common framework*, to view them within *some shared perspective* supplying, in principle at least, comparative and evaluative considerations applicable to both. It is in fact to consider them as oriented in different ways toward the same purposes, as making rival appeals from the standpoint of scientific goals taken to be overriding and with respect to a common situation taken as a point of reference. (Scheffler 1967, 82. Italics mine).

Thus both Shapere and Scheffler imply that Kuhn's assertion that incommensurable theories can be in competition is self-contradictory for they claim that the very notion of incompatibility presupposes that of commensurability. Importantly, however, neither Shapere nor Scheffler give an *argument* for their claim; instead they simply assert it as though its truth were self-evident.[20]

According to Ian Hacking, the deepest criticism of incommensurability is that of Davidson (Hacking 1983, 73-4; Davidson 1984b, 183-198). Davidson associates the idea of incommensurability with the more commonly accepted view that there can be more than one "conceptual scheme" (*ibid.*, 185, 190) but denies that this idea makes sense *at all*: that is, even when applied to theories or schemes that aren't competing (*ibid.*, 198). Like Shapere and Scheffler, he understands "incommensurable" to mean "absolutely incomparable" (*ibid.*, 184)[21] and, as we have also seen, "not intertranslatable" (*ibid.* 190). Davidson gives a subtle argument, based on his philosophical studies, to try to show that we cannot sensibly attribute *any* theories, beliefs or concepts to others unless we can first (exactly) translate what they are saying "into a familiar tongue" such as English (*ibid.* 186).

But Davidson's argument is "deeper" just because it is more explicitly committed than those of Kuhn's other critics to an equation of comparability with commensurability. Thus, while he acknowledges that the examples Kuhn (and others) point to of conceptual change and disparity are "[occasionally] impressive" (Davidson 1984b, 184), he insists that they "are not so extreme but that the changes and the contrasts can be explained and described *using the equipment of a single language*" (Davidson 1984b, 184. Italics mine). And this claim shows that Davidson implicitly equates intertranslation (successful explanation and description of alien views in a single, natural language) with commensuration (their successful explanation and description in a neutral system of concepts: "the equipment of a single language.")[22] However, as I have argued above (p20), it is just this putative identity of intertranslatability and commensurability that Kuhn denies.

That Kuhn's critics implicitly identify "comparable" (and "intertranslatable") with "commensurable" is also made clear by the only alternatives they seem willing to consider if their first construal of "incommensurability" is not what he has in mind. Thus they commonly counter that if, contrary to first appearances, his notion isn't intended to imply absolute incomparability or untranslatability, then the theories he terms "incommensurable" must, *in the literal sense*, be commensurable. But, in that case, they say, Kuhn's thesis cannot have any philosophical import; at most it can have only either psychological or pragmatic significance.

For example, Alan Musgrave says that if comparison or translation of 'incommensurable' theories is possible, "incommensurability has ceased to be a logical affair and [its import] has become a purely psychological...matter" (Musgrave 1982, 50): it merely refers to the commonplace *empirical* fact that old scientists often have considerable difficulty in accepting and even understanding new theories.[23]

Similarly, Davidson argues that examples Kuhn and others adduce to show that, "what comes easily in one language may come hard in another," *can* only reveal "modest," pragmatic difficulties in securing adequate translations; they cannot provide evidence for conceptual discontinuities (Davidson 1984b, 184). It follows that Kuhn's term "incommensurable" is either a misleading expression for a non-logical issue (if it does not mean "not intertranslatable") or embodies a confusion (if it does): "if translation [of another's conceptual scheme] succeeds, we have shown there is *no need* to speak of two conceptual schemes, while if translation fails there is *no ground* for speaking of two" (Davidson 1980b, 243).[24]

As already noted (p20), Kuhn denies his opponents' charge that by "incommensurable" he ever meant "absolutely incomparable" or "not intertranslatable"; moreover, he continues to insist that his thesis has logical and epistemological repercussions, not merely psychological or pragmatic ones (e.g. Kuhn 1970b, 232-3). But philosophers are often reluctant to let go of even a straw man-opponent. Thus Kuhn's critics have taken his denials to show simply that he is "but a pale reflection of the old, revolutionary Kuhn"; that, in the face of their cogent criticism, he beat a full-scale retreat from his original, "challenging" ideas towards the traditional canons of rationality that he originally slurred (e.g. Musgrave 1981, 51).[25] Kuhn's retort is that the debate between him and his critics has the characteristic "talking-through-each-other" feature of arguments between practitioners of incommensurable paradigms like Proust and Berthollet (Kuhn 1970b, 231-2).

Other philosophers are more sympathetic to Kuhn's view than Davidson and Musgrave but also imply, like Davidson, that the difficulties he terms "incommensurabilities" are not logical and epistemological but arise only from *temporary* pragmatic problems in precisely formulating the relations between scientific theories. Incommensurability would be solely a pragmatic, and thus temporary, issue if, for example, the lack of common concepts was only apparent and removable by analysis; or if "metascience" were able to find other common elements than concepts or phenomena for neutral theory comparison.[26]

As noted above (pp16-17), John Collier takes the first option: he suggests that we can make articulate the "implicit part of a theory" through "the creation of a broader, more inclusive language capable of expressing the content of both theories" (Collier 1984, 8-9). Dale Moberg takes the second approach: he argues that incommensurability because of "radical meaning variance" may simply point to a need to develop a new "theory of theory comparison" (Moberg 1979, 261). Though there are no conclusive *a priori* reasons why such projects *must* fail, we have seen that Kuhn argues in effect that there are no *a priori* reasons why they must succeed either; and there are plenty of *empirical* reasons to believe that they will not (c.f. pp16-17 above).

Still, it is misleading to see the dispute between Kuhn and his opponents as purely empirical. As we have seen (pp19-20), his critics' philosophy makes incommensurability a logical impossibility; so that philosophy can have no criteria for deciding whether a given pair of theories were or were not commensurable. Kuhn's opponents' often unspoken, but consistent, identifications of comparability with commensurability; and logical incompatibility with formal logical incompatibility show that those identifications are not arbitrary, but built into their whole philosophical approach. Davidson makes this normally tacit commitment explicit when he says at the beginning of his critique of Kuhn: "Different points of view make sense, *but only if there is a common co-ordinate system on which to plot them*; yet the existence of a common system [standard] belies the claim of dramatic incomparability" (Davidson 1984, 184. Italics mine). And Davidson, like Shapere and Scheffler, never argues for this assertion; and he tacitly assumes that philosophers like Kuhn also accept it.[27]

1.4 Interim Summary

We have seen that the incommensurability thesis is the claim that successive scientific theories are often both logically and normatively (i.e. conceptually, operationally, observationally) incompatible. We have also seen that Kuhn's critics find his thesis either unintelligible or philosophically irrelevant because they equate logic with formal logics and comparability and intertranslatability with commensurability. This leaves us with two (mutually-exclusive) options: (1) either those notions cannot be meaningfully distinguished; in which case, Kuhn's thesis is "impaled on the dilemma"; or (2) it is possible to distinguish those notions significantly; in which case, Kuhn "escapes between the horns" but the traditional "epistemological paradigm" loses its philosophical impetus.[28]

In the following section, I show how to make a philosophically-significant distinction between comparison and commensuration; and, as proof my interpretation is on the right track, I give textual evidence to indicate that Kuhn also makes such a distinction in his philosophy. I then give a more detailed analysis of the presuppositions of systematic, formal logics, like the propositional and predicate calculi, to help pinpoint those presuppositions which specifically embody a commitment to commensurability. That analysis will hopefully yield a clearer picture of what a notion of logic that is compatible with the incommensurability thesis would be like.[29]

1.5 How to Distinguish Comparability from Commensurability

As already mentioned (p20), the most obvious potential difference between "comparable" and "commensurable" is that the latter expression involves an adverbial qualification of the first: that is, "commensurable" means "comparable *by a common standard*." And if the adverbial clause "by a common standard" is not redundant, then "incommensurable" means "*not* comparable by a common standard," not simply "not comparable *at all*" (nor: "*absolutely* incomparable"). Hence, if certain things were comparable, but not commensurable, they would be comparable, though not by a common standard.

To get a more specific idea of what comparison without common standards would be like, first consider the notion of a standard of comparison. Trivially, a standard of comparison is a *norm, paradigm, pattern,* or *rule* for comparing things (*The Concise Oxford Dictionary*). And two objects are comparable just in case those objects are related in a *particular* way; that is, just in case they are both instances of a *particular* binary relation: e.g. one of *similarity, difference, incompatibility, relevance, entailment, length, intensity, volume,* etc. Two objects will be commensurable, therefore, just in case they are instances of a particular relation that is determinable by a common standard. And, by contrast, two objects will be comparable and incommensurable just in case they have such relations, but those relations are *not* determinable by a common standard.

There are (at least) two distinguishable cases of commensurability: (1) a single standard is able to compare a given pair of objects because each object is a simple, hence irreducible or *direct,* instance of its categories; and (2) a single standard is able to compare a given pair of objects because each object is a complex, hence "definitionally-eliminable" or *indirect,* instance of more primitive categories embodied in that standard. In both cases, however, the comparison of one object with the other by the standard is *indirect*: each object is separately compared with the standard to determine how they compare with each other. In other words, commensuration is comparison of two things by their comparison with a third: their common standard. Thus, since incommensurability means that there are *no* common standards of comparison, a further condition on our notion of comparable and incommensurable objects is that each such object must be compared *directly* with the other to determine their interrelations.

However, definition (1) does not, as it stands, give us the resources for making a useful distinction between commensurability and comparability; for, since, by hypothesis, the objects that we are considering are comparable, they could also be *made* "commensurable" by a trivial technique compatible with that definition. Thus even though they shared no common structural elements, and so were incommensurable by our earlier definition (p4 above), those objects would be, in a weaker sense, comparable by a common standard: one constructed from their mere union.[30]

Definition (2) is more helpful. Thus, on the basis of (2) and our earlier definition of "incommensurability," we may say:

> Definition 3: *Two structurally-complex objects are comparable and incommensurable when they have relations that are not determinable on the basis of more primitive elements common to both.*[31]

And since incommensurable objects' relations cannot be determined *indirectly* as a function of common parts, their relations can only be determined *directly*. That is, the relations between such objects--whether of part to part; part/whole to part/whole; whole to whole; etc.--can be determined and determinable only by the simple *juxtaposition* of one object or its aspects with the other. And (if such direct comparison makes sense) that spatial, temporal, etc., pairing will be enough to show that those objects constitute an irreducible instance of a particular comparative relation.[32]

In summary, then, the intelligibility of the notion of comparable but incommensurable objects depends on the intelligibility of the notion of direct comparison. This attempt to bifurcate the notion of comparability into direct comparability and commensurability has potential philosophical significance: it suggests that comparison of structurally complex entities need not rely on the presence of elements common to both; it need not rely on common standards of comparison.

To make this rather abstract discussion more accessible, consider the application of my formulation to commensurability and incommensurability in geometry, where those notions were first introduced. In geometry, two lengths are said to be commensurable (by length) if one can construct a common unit length that will divide both exactly. Thus a stick that is ten centimetres long is commensurable with one seven centimetres long because, for example, a one centimetre length will divide both without remainder. And both lengths are, in a sense, 'definitionally-eliminable' since each length can be reduced to multiples of ("logical constructions on") that common unit (see p28 above). Thus the ratio of the length of the first to the second stick can be broken down further to the ratio 10 : 7.

On the other hand, as is well known, the hypotenuse and side of a right angle isoceles triangle are incommensurable: it is impossible to construct a common unit length that will divide both hypotenuse and side exactly. Nevertheless, those lengths are directly comparable, since by mere inspection of the triangle one can see immediately that the hypotenuse is *longer than* the side; and from similar triangles one can also see directly that those lengths form a constant, and irreducible, *ratio* (c.f. Von Fritz 1945, 261). The side and hypotenuse are thus both incommensurable and directly comparable as in my definition. Moreover, it is also possible to compare directly the parts of those lengths: e.g., every part of the hypotenuse is (approximately) 14,142/10,000 times the length of a part of the side, if both those parts are in their original ratios.[33] Moreover, in line with the comments I made on definition (1) (p29 above), there is also a simple way in which those lengths can be made "commensurable." That is, there is a "common standard" for comparing the lengths of the hypotenuse and side: the real number "system"; and according to that common system, the ratios of those lengths is $\sqrt{2} : 1$. But since that real number system is simply the union of two disjoint sets: the rational (e.g. which contains 1) and irrational numbers (e.g. which contains $\sqrt{2}$), their "commensurability" in that system does not entail their comparability by more basic elements common to both.[34]

Before I extend my model to the problem of logically incompatible and incommensurable scientific theories, I shall first present evidence that Kuhn also makes use of a distinction between (what I have called) direct and indirect comparison in his philosophy.

1.5.1 Kuhn and Direct Comparison

The most explicit example of a distinction between direct and indirect comparison, and thus comparability and commensurability, in Kuhn's writings is in his notion of a similarity set (see Kuhn 1970a, 192, 200; Kuhn 1977c, 305-18). However, that notion is intimately related to his analytic concept of a scientific paradigm, which he introduces to replace the traditional, formal notion of a universal generalization. And since I shall discuss Kuhn's alternative to the traditional formal schema in chapter two, I shall here use more suggestive evidence of the above distinction.

As the following passage shows, Kuhn apparently believes that recognition of relations between different points of view does not require a common, neutral basis of comparison.

> What occurs during a scientific revolution is not fully reducible to a reinterpretation of individual and stable [i.e., theory-neutral] data. In the first place, the data are not unequivocally stable. [A Bertholletian compound is not a Proustian physical mixture], nor is oxygen dephlogisticated air....[Thus] rather than being an interpreter [of "fixed" data], the scientist who embraces a new paradigm is like [a] man [who undergoes a gestalt switch]. Confronting the same constellation of objects as before *and knowing that he does so*, he nevertheless finds them *transformed through and through* in many of their details (Kuhn 1970a, 121-2). Italics mine).

1.6 Commensurability and Formal Logics

Thus far I have claimed, without much elaboration, that formal logics require that the linguistic entities they compare or analyse be expressible within a common vocabulary (e.g. p14): in other words, that those entities be commensurable. I now want to suggest that formal logics be taken quite strictly as logical "measures." If I am right, this means entities that can be formally compared are *logically* commensurable because they are comparable by a common logical measure. By contrast, in my use of "formal comparison," those entities that cannot be formally compared are logically incommensurable.[35] Of course, if one is to take the metrical analogy seriously, formal logics have to have "units" of logical measure: i.e. they have to have specific means for determining logical relationships. In what follows, I suggest that those units are the so-called *logical concepts*. And I argue that the view that language divides into strictly logical and non-logical concepts, which is essential to formal systems, is incompatible with the truth of the incommensurability thesis.

There are two basic uses made of formal logics like the predicate calculus in traditional philosophy of science: (1) they are used to describe the internal logical structure of a theory; and (2) they are used to formulate the logical relations between scientific theories (e.g. Popper 1968, 32 *et. seq.*). The second use clearly depends on the first, since individual theories have first to be formalized before their logical interrelations with others can be determined.

Now, invariably, formal logics divide their object languages into logical and non- or extra-logical concepts (e.g. Suppes 1957, 3, 43, 48, 68; Mates 1972, 16, 45; Boolos and Jeffrey 1980, 97). For example, in the predicate calculus the standard logical concepts (words, terms, symbols) are: "not, "and," "or," "if..., then," "if and only if," "all," and "some," and their synonyms. And philosophers who apply formal logics to scientific languages also commonly speak of the logical and non-logical vocabularies of those languages (e.g. Suppes 1957, 68).[26] The implication of this division of language is that all (or at least the clearest. c.f. *n*13) logical work in that language is done by just these concepts.

Such a division of language seems to be a pre-condition for any formal, (non-trivially) systematic logic (c.f. Mates 1972, p16). Unless logical relations between, for example, propositions and theories were based only on the meanings of a relatively small number of concepts, it would be generally impossible to determine those relations formally. For, in that case, the logical relations between many pairs of entities would given by the meanings of those entities alone; so there would often be no common feature linking various instances of a particular logical relation like incompatibility. Nevertheless, I shall now argue that the incommensurability thesis presupposes that logical relations are in fact *largely* non-systematic and non-systematizable.

It must be emphasized, however, that this presupposition does not imply the patently absurd claim that there are *no* systematic logical relations nor that such logics are not important and powerful *tools* of science (c.f. Kuhn 1977, 313). Rather, it simply means that such systematic relations only constitute a small proportion of logical relations proper.

I argue for two claims: (1) that the relation of logical incompatibility *between* incommensurable theories cannot be captured by formal systems; and (2) that, if there were incommensurable theories, it could not generally be true that every individual scientific theory could be *analysed* by formal logics. Both arguments work by *reductio ad absurdum*. I have tried to make the arguments completely general and not dependent on the features of particular formal systems like the predicate calculus.

One should keep in mind that, by definition (see pp4, 29), incommensurable theories, or incommensurable parts of theories, have no common concepts. And because we are here considering the hypothesis that formal logics can either compare or analyse incommensurable theories, it is also therefore assumed that incommensurable theories share the same logical concepts but have no common *non-logical* concepts.

1.6.1 Argument 1: Logical Relations Between Theories

Here we consider the hypothesis that formally comparable theories can also be incommensurable. By the hypothesis of formal comparability, the logical relations between any pair of elements, e.g. (t1j;t2k), of any pair of such theories, e.g. (*T1;T2*), must depend only on the meanings of the logical concepts. This means that one such element is equivalent to a logical function, *f*, of the other: e.g. *t1j* = *f*(*t2k*); and that means, since the theories share the logical concepts, the theory (*T1*) that contains the first element (t1j) also contains the second element (t2k). Which contradicts the hypothesis of incommensurability.

1.6.2 Argument 2: Formal Analysis of Individual Theories

Here we consider the hypothesis that any individual scientific theory can be formally *analysed* even though there are pairs of theories that cannot be formally *compared* because of their incommensurability. Consider a pair of incommensurable theories: since, by hypothesis, each is fully analysable formally, each will share the same logical concepts; since those theories are incommensurable, they will share no non-logical concepts. Now, by hypothesis, those theories are logically related. And, clearly, those logical relations must be due either to the meanings of the theories' logical or non-logical concepts. By argument 1, those relations can't be due to the logical concepts, since that would imply, contrary to the hypothesis of incommensurability, that the theories share common non-logical concepts. On the other hand, if those logical relations follow from the meanings of the non-logical concepts, then the non-logical concepts aren't (at least absolutely) non-logical. Which is a also a contradiction. Hence if there are incommensurable pairs of theories, the languages of scientific theories do not divide into strictly logical and non-logical concepts. In other words, if there are incommensurable theories, logic is not identical with formal logic.

For each of the above arguments it is possible to retort that logic just is formal logic and that, therefore, the incommensurability thesis is false. Or instead it might be claimed that individual theories form different logical systems; and *each* such system does divide further into different purely logical and non-logical concepts (e.g. Goodman 1984, 44-5; but c.f. *ibid.*, 94-5); hence, formal logics don't preclude incommensurability. But, with respect to this second option, I have not here claimed that Kuhn's thesis implies that there are no relations based on logical form or even that no theories may be formally analysed. I have only said that the incommensurability thesis means that formal logics do not *exhaust* logic proper. In any case, even if each of an incommensurable pair of theories constituted a different *formal* systems would still leave unexplained--i.e. unformalized--the logical relations *between* those discrete systems.

With respect to the first retort, it should be noted that there are instances of statements in ordinary language whose logical relations seem to arise from the meanings of their "non-logical" concepts. For example, the claim, "George is a bachelor and George is a married man," is not contradictory because of its form but because of the meanings of *bachelor* and *married man*. Moreover, to turn this claim into a formal contradiction by first defining *bachelor* as *unmarried man*, presupposes that one already knows those concepts to be synonymous, and so begs the question of the source of the incompatibility of *bachelor* and *married man*.

Examples of such meaning-dependent logical relations are not mere curiousities. The so-called natural logician, Steven Thomas, claims that "samplings indicate [that]...more than 90% of deductively valid inferences in natural language...depend on the *semantics*, rather than on the logical form or syntax, of the premise(s) and conclusion" (Thomas 1986, 448. Italics in original).[37] Moreover, Davidson himself is also aware of deep problems formal approaches have with the most common features of natural languages (like that of science), though with the opposite conclusion to Thomas.

> *Since I think there is no alternative*, I have taken an optimistic and programmatic view of the possibilities for a formal characterization of a truth predicate for a natural language. But it must be allowed that a *staggering* list of *difficulties* and *conundrums* remains (Davidson 1984, 35. Italics mine).

What my arguments, Thomas's statistics and Davidson's "conundrums" suggest is that the logical relations between incommensurable theories must likewise depend on the semantics of their "nonlogical" concepts. That should not be surprising if we recall Kuhn's characterization of incommensurable theories as "fundamentally incompatible" (see p4); for that logico-normative hybrid notion is, in retrospect, clearly at odds with any rigorous division of the logical from the non-logical. And if we also remember that Kuhn takes scientific theories to be holistic, "conceptual webs," we should also expect that the logical relations between those "webs" will have to be a consequence of the semantics of those systems as conceptual wholes.

It is true that sentences are normally held to be the unit of logical relations such as incompatibility; so it will undoubtedly appear peculiar to speak of wholesale theoretical incompatibility. But if we state the notion of logical incompatibility quite broadly as: "if *this* were true, then *that* could not also be true," then the idea of direct theoretical incompatibility may not seem so odd. Thus one could say that two holistic theoretical systems are (directly) logically incompatible just in case, if the whole world were as in one theory, it could not also be as in the other theory.

Still, given the obvious limits of the human mind, since theories are entities with very complex internal structures, it might seem impossible that their incompatibilities could be determined directly. To help his readers get an intuitive understanding of the unfamiliar idea of primitive theoretical incompatibility, Kuhn employs the idea of a "switch in visual gestalt" as its "elementary prototype" (Kuhn 1970a, 111).[39]

1.6.3 A "Gestalt-Switch" Model of Theoretical Incompatibility

A classic example of a visual gestalt switch is the response to the so-called *duck-rabbit* figure. Surprisingly, this structurally-complex figure is seen *alternately* as *wholly* either a duck or a rabbit, though never both at once. Hence, since it is clearly impossible to see the figure simultaneously as both a duck and a rabbit--a duck is not a rabbit and *vice versa*--these seem to be *directly incompatible* construals of the same domain. The duck-rabbit figure thus appears to provide a very good model for explicating how the direct logical comparison of (putative) incommensurables can take place without implying their commensurability.

The Duck-Rabbit

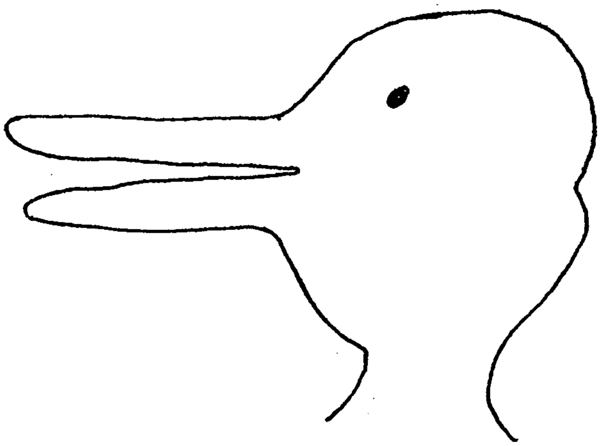

Figure 1.

Thus, having experienced both gestalts, we can directly compare and contrast the duck and rabbit views as follows. The upper bill of the duck perspective "is" the left ear of the rabbit perspective; the left duck eye of the duck perspective "is" the right rabbit eye of the rabbit perspective; the throat of the duck perspective "is" the nape of the rabbit perspective; the notch on the back of the head of the duck perspective "is" the mouth on the rabbit perspective; and so on.[39]

I have used shudder-quotes around the word "is" in each case to emphasize that, while the respective parts are in a sense co-extensive, it is nevertheless impossible that they both obtain (from either perspective). For ducks have no floppy ears and rabbits no rigid beaks, and so on. Notice further that while both the duck and rabbit are in a sense *composed of* their constituent bills, ears, etc., these parts are what they are because they are parts *of the whole object.* Thus it is more accurate to say instead that the duck and rabbit *divide into* their constituent parts.[40]

For Kuhn, then, on analogy with this "elementary prototype," the comparison or contrast of incommensurable theories must ultimately involve their direct comparison as whole to whole. That those theories are wholly incompatible, however, does not mean that their parts cannot be compared or even found to be similar. It just means that any comparison of those parts will involve the juxtaposition, not of theory-independent terms, concepts and sentences, but of those entities as theory-dependent subdivisions. Thus each element in the comparison or contrast pairs will be there as a representative of the *whole* theory from which they are abstracted. For example, just as the bill *qua bill of the duck* can be seen directly to be co-extensive with the ears *qua ears of the rabbit,* so also a chemical compound *qua Bertholletian substance of greatly variable proportions* can be seen directly to be (often) co-extensive with a physical mixture *qua Proustian solution.*

Two objections have been raised against the use of the gestalt model for theory change (Collier 1984, 109). The first objection--that the duck-rabbit example involves representations, not real objects--is, I think, rather easily disposed of. For, *mutatis mutandis,* that example works just as well if we think of the object as either a *picture-duck* or a *picture-rabbit*, which we do in any case (c.f. also Feyerabend 1975, 258 & *n*98).

The second objection is more serious. Thus it is claimed, with some (I think mistaken) encouragement from Kuhn (Kuhn 1970a, 114), that in the duck-rabbit case, there is a neutral base of comparison: "the lines which make up the figure" (Collier 1984, 109). If so, the duck and rabbit views would be alternative, but compatible construals of a common domain. But just as it is impossible to see the figure simultaneously as both a duck and a rabbit, it is also impossible to see the figure simultaneously as both *mere* lines and as either a duck or a rabbit. Thus, while the drawing is, in a sense, composed of the lines on paper, and while one may draw the figure oneself, seeing those lines as either a duck or a rabbit, or vice versa, must also take place "as a relatively sudden and unstructured event" (Kuhn 1970a, 122).

This second objection is a more specific version of such claims as: "what the duck-rabbit shows is that reality can have alternative or complementary representations, each of which grasps some aspect, but which are not only not in conflict but are each needed if one is to get 'the whole picture' (so to speak)" (Savitt 1989, 2; see also Hacking 1981, 4). But this more general claim itself contains an implicit demand for commensurability in making judgements about conflict. Thus it suggests that unless there are common standards for showing views to be in conflict, those views are compatible *by default*.

On the other hand, as we have seen, there are two basic ways in which one might judge views to be either conflicting *or complementary*: by indirect or direct comparison. But in cases like the duck-rabbit figure, and incommensurable theories, I have argued, there is no neutral perspective for making an indirect judgement. Therefore, in such cases, the judgement must be made by direct comparison; and *such* comparison shows those perspectives to be clearly incompatible. That is, it is impossible, *from one point of view*, to have the other point of view; and there is no third standpoint from which one might have both. Hence to call incommensurable views compatible by default is to imply that comparison is commensuration. But as I shall now argue, such an identification leads to absurdities.

1.7 Direct Comparison: A Thought Experiment

From the fact that commensuration involves the comparison of two things by their comparison with a *third* thing (see §1.5, p28-29), it should be clear that to define "comparison" as "commensuration" would yield a vicious regress.[41] In what follows, I argue that to demand that to establish similarity and difference of reference; to determine similarity and difference of meaning; and to recognize logical incompatibility always requires common elements leads in each case to a vicious regress. On the other hand, I argue that if Kuhn's critics were to agree that such comparisons will at some point have to be direct, but hold that this point is ostensive definition, a "common co-ordinate system"

(Davidson 1984b, 184), or formal logic they must offer an independent argument to prove that this is so. And, therefore, if the *prima facie* evidence is that no such system does or could exist, that evidence will have to be taken at face value.

First, if the co-reference of two terms always depended on each one's being co-referential with some common third term, then we should also require a still further term to mediate co-reference between each of the first two terms and the third; and so on. On the other hand, if the co-reference of the first two terms with the third can be determined directly, we have to be given a reason why this could not also be the case for the first two. For example, consider the following co-referential expressions, "Brian Mulroney" and "the prime minister of Canada." If we hold that to establish their co-reference we first have to show that each is co-referential with a "more primitive" expression like a demonstrative pronoun (e.g. *this*), we shall also need a still "more basic" expression to mediate the co-reference of both "Brian Mulroney" and "the prime minister of Canada" with that demonstrative pronoun.[42] On the other hand, if these last relationships can be determined directly,[43] we need a reason why this could not, in principle, also be done with "Brian Mulroney" and "the prime minister of Canada" alone.

Second, if the similarities or differences of meanings of two concepts always required a "common co-ordinate system" (Davidson 1984, 184) to determine those relations, then we should also require a still further co-ordinate system to show that those concepts were instances of that first system; and so on. Again, if it is admitted that the relations the two concepts have with the co-ordinate system must be determined directly, we need a further reason why this could not also be the case for the relations between the concepts themselves.

Finally, if the incompatibility of two concepts, sentences, theories, etc. always had to be determined by a standard like logical form, then we should also need still further standards to show that one such entity is identical in meaning with the negation of the other. And, on the other hand, if it is countered that the recognition that one such entity is synonymous with the negation of the other itself needs no standard, we need an independent reason why their incompatibility could not be judged directly.

The foregoing exercise is not meant to establish that all uses of terms, concepts, sentences, and theories involve no common standards. That is clearly false. Appeals to standards, paradigms, rules, tradition, etc. often play an extremely important role in science, law, literature, art, etc.[44] My argument only makes the obvious point that such appeals must end *somewhere*; and so even those practices that involve appeals to standards must at some point involve direct comparison. Therefore, when historical, anthropological, sociological and conceptual evidence shows that certain communities and traditions use apparently quite different standards of, say, *truth*, *meaning*, or *justice*, and determined attempts to attribute more basic, common standards to those groups are always "rejected by...members of the group" (Kuhn 1970a, 44), that evidence must be taken at face value.

The ability to compare things directly, however, should not be thought of as a "mysterious" (Shapere 1980, 32) capacity to intuit their relations (see also *n*28). That would be tantamount to introducing intuition as a commensurating faculty; but as we have just seen, the demand that any such standards are *necessary* leads to absurdities. Wittgenstein puts this last point as follows: "If intuition is an inner voice--how do I know *how* I am to obey it? And how do I know that it doesn't mislead me? For if it can guide me right, it can also guide me wrong. ((Intuition an unnecessary shuffle.))" (Wittgenstein 1953, §213). In the final analysis, then, all that can be said to explain direct comparison is simply to remark that it is a basic fact of human "natural history" (*ibid.*, §25) that we group certain things as similar, identical, different, incompatible, and so on. No more can be said without redundancy.

1.8 Summary, Synthesis and Conclusion

In this chapter I have tried to show that the argumentative impasse between Kuhn and his critics on incommensurability arises from a fundamental difference about the interpretation of two concepts: logic and comparison. Thus it turns out that Kuhn's critics implicitly identify logic with formal logics; but his holistic notion of theoretico-conceptual incompatibility conflicts with the idea, basic to formal logics, that there is a completely rigorous distinction between the logical and non-logical. More deeply still, it turns out that Kuhn's critics identify logic with formal logics because they identify comparison with commensuration. But I showed, by pointing out that commensuration is comparison of two things by their comparison with a third, that such an identification would make commensuration itself impossible (see pp28-9, 44-6).

One proof that the distinction between (direct) comparison and commensuration is on the right track is its ability to resolve a residual puzzle Kuhn feels about the characteristics of the new "epistemological paradigm" (see 1.0 above). Kuhn says that "I am convinced that we must learn to make sense of [seemingly paradoxical] statements that at least resemble these... though the world does not change with a change of paradigm the scientist afterward works in a different world" (Kuhn 1970a, 121). These apparently contradictory claims can be made sense of as follows. On the one hand, just as direct comparison of the duck and rabbit views shows them to be coextensive (p41), so too direct comparison of incommensurable theories shows that their domains are often largely overlapping; so in one sense those theories describe the *same* world. On the other hand, just as direct comparison of the duck and rabbit views does not reveal a further, common *duck-rabbit* view, one can also say that incommensurable theories describe *different* worlds.

Kuhn's rejection of a primary role for (formal) logic in analysing science has been construed by many critics to involve "denying objectivity to the processes by which scientific theories are critically evaluated" (Scheffler 1967, 88). By contrast, I suggested early in this chapter that Kuhn should be understood instead as offering an *alternative* construal of scientific objectivity and rationality that is not based on formal logic, and thus commensurability. In this regard, Scheffler also notes, though with some perplexity, that "Professor Kuhn himself concludes by reintroducing the very [evaluative] ideas he has been at pains to deny in the main tendency of his work...[for example] he opposes received notions of falsification, but himself introduces the concepts of anomaly and crisis, which have a *parallel* function in his account" (*ibid.*, 89. Italics mine).

The main reason Kuhn rejects a primary analytic role for formal logic in scientific epistemology is, as we have seen, that he believes theories are often incommensurable. In the next chapter, I shall bring out still other reasons Kuhn has for the demotion of formal logic. Thus in chapter two I discuss Kuhn's notions of paradigm, normal science, anomaly, crisis, thought experiment and scientific revolution and show how they parallel, and diverge from, more traditional categories drawn from formal logic. I shall also show there how Kuhn embodies a distinction between commensuration and what I have called direct comparison in those alternative analytic notions.

Notes

1. If Kuhn had not made a persuasive case for incommensurability, it is doubtful that opposing philosophers would have made such concerted efforts to respond to his work. Israel Scheffler, a noted critic of Kuhn, admits that, at first glance, Kuhn's results seem undeniable: "For the claims in question are supported by detailed considerations to which we ourselves are inclined to assent, at least initially" (Scheffler 1967, 22).

2. In this paper, I shall include under the descriptions "traditional image," "received view," etc. not only the classic statements of the Logical Empiricists and Karl Popper but also more recent variations formulated in response to Kuhn's (and other like-minded philosophers') criticisms. Thus the philosophies of Lakatos, Laudan and Newton-Smith, for example, may be seen as "traditional" for my purposes since they also seek what Kuhn argues does not *and need not* exist: theory-neutral criteria for evaluating progress in science. Of course, such a schematic account of 'the' traditional view is bound to be overly simplistic and lump together incompatible philosophies. Still it is safe to conclude that, whatever the differences of those philosophies, they are united in being attempts to find theory-neutral comparative criteria, and thus in their rejection of incommensurability.

3. I shall usually use the term "theory" and Kuhn's preferred expression "paradigm" as though they were synonymous. This is in keeping with Kuhn's usual practice, though, given what he means by "paradigm," it does not agree with his opponents' understanding of "theory." Thus theories traditionally are thought to be universal generalizations (c.f. Giere 1983, 271); but Kuhn holds that his concept of a paradigm, or "concrete scientific achievement" (Kuhn 1970a, 11) is more basic to science than is that traditional conception (*ibid.*, 182). I shall discuss paradigms in chapter two.

4. By contrast, if the empirical anomaly does not implicate the theory's categories, theory change need not involve conceptual change, and so also need not involve incommensurability. For example, the theory's concepts may allow for the formulation of different empirical laws, so the replacement of the false law with a more adequate one can leave the basic theory unchanged. On the traditional view, however, since theories are taken to be generalizations, such a change of laws would also be taken as a change of theory. But, as I pointed out in note 3, Kuhn takes paradigms to be the basic unit of science. And he suggests that

change of that putatively more basic unit almost always produces the conceptual and evaluative change he calls incommensurability (Kuhn 1970a, 103).

5. It is occasionally claimed that Kuhn cannot really mean that incommensurable theories are *logically* incompatible; so he must have some other notion of incompatibility in mind. Thus Newton-Smith says that "Kuhn...inconsistently explicates the notion of competition [between incommensurable theories] in terms of the notion of logical incompatibility" (Newton-Smith 1981, 159). Newton-Smith kindly offers what he believes to be a more suitable notion of incompatibility to get Kuhn's intentions across: "*pragmatic tension*" (*ibid.*, 159). But, *pace* Newton-Smith, Kuhn explicitly states that the theories he deems incommensurable--e.g. Newtonian and Einsteinian dynamics--are also *logically* incompatible: "Einstein's theory [Kuhn argues] can be accepted only with the recognition that Newton's was wrong" (Kuhn 1970a, 98; see also *ibid.*, 97). In any case, in this paper I shall take Kuhn at his word and try to give an interpretation of the notion of "fundamentally incompatible" theories that coheres both with Kuhn's explicitly-stated intentions and with good sense.

6. That is, though Kuhn allows that in comparing two incommensurable theories one can see that, *from the vantage of one theory's standards*, that theory is true while the other is false, he claims there is no *common, theory-independent perspective* for settling which of them are in fact true or false.

7. Kuhn also denies most of his critics' other claims: viz., that his position means that experiment plays no critical role in science (e.g. Kuhn 1970a, 132); that it leads to idealism (Kuhn 1970b, 2); (strong) relativism (*ibid.*, 264-5); irrationalism (*ibid.*, 263-4); and he also believes that *theories* of meaning play no essential role in these issues (*ibid.*, 266*n*2). In this paper, however, I shall not discuss these more specific questions in any detail since I believe that they are a reflection of the more general differences Kuhn has with his critics over logic and comparison. That is, I hope it will become clear that Kuhn does not reject the fundamental concepts of empirical science, only particular construals of them.

8. Kuhn suggests that when seeking the source of incommensurability between two views that one "can first attempt to discover the terms and locutions that, used unproblematically within each community, are nevertheless foci of trouble for inter-group discussions" (Kuhn 1970a, 202). Kuhn is here offering a hermeneutic principle that is similar to that of Quine: "The maxim of translation underlying all this is that assertions startlingly false

on the face of them are likely to turn on hidden differences of language" (Quine 1960, 59). Kuhn's principle differs from Quine's, as we shall see, in not requiring that the translated claims make sense from the standpoint of the home language's current concepts (c.f. Feyerabend 1987, 77 and note 14).

9. Kuhn describes their views as being "fundamentally at cross-purposes" (Kuhn 1970a, 132). Recall that "fundamentally incompatible" means "both logically incompatible and incommensurable." Also note that, in this essay, I shall only be concerned with those putative cases of philosophically-interesting incommensurability: those which also involve logical incompatibility; thus theories here described as incommensurable will also be assumed to be logically incompatible.

10. For example, if we let R and C stand for, respectively, the 1-place predicates "is a chemical reaction" and "takes place in constant (integral) proportions," and if we take the predicate "takes place in continuously-varying proportions" to be logically equivalent to $\neg C$, then it seems that we can represent Proust's and Berthollet's positions as (formal) logical contradictories using the same concepts:

Proust: $(x)(Rx \rightarrow Cx)$

Berthollet: $\neg(x)(Rx \rightarrow Cx) \equiv (\exists x)(Rx \,\&\, \neg Cx)$

11. Alternatively--in the "material mode"--Proust disagreed with Berthollet over which processes were chemical reactions and over which things were chemical compounds as opposed to physical mixtures. The three concepts, *chemical reaction, chemical compound* and *physical mixture* are logically-interrelated as follows: a *chemical compound* is formed when two or more chemical substances are mixed together if and only if they undergo a *chemical reaction;* otherwise they constitute a mere *physical mixture.*

12. *A, d,* and *c* are chemical substances; *Ad* and *Ac* are compounds of those substances.

13. This has been noted by most of Kuhn's critics, though their arguments are usually expressed as the contrapositive of mine. That is, Kuhn's critics generally infer that, since formal logics would show no logical incompatibility between putatively-incommensurable theories, those theories *could not* be logically incompatible (e.g. Shapere 1981, 43-4; Newton-Smith 1981, 149, 159; Scheffler 1967, 82; Davidson 1984, 184). However, granting that formal logics would not show incommensurable theories to be in-

compatible, one may argue instead that those logics are simply inadequate to describing logical relations between incommensurable theories. I shall provide a more complete analysis of the source of conflict between Kuhn's thesis and assumptions of formal logics in sections 1.6 *et. seq.*, where I shall also argue that the existence of incommensurable theories means that formal logics could not, *as a general rule*, give the logical structures of all individual theories.

14. The problem (2) of finding common concepts for incommensurable theories is similar to the problem of translating between certain natural languages. Thus translators often find that for many words of one language there exists no single word, or well-defined set of words, that matches the meaning of the other in all circumstances of its application; to translate they must adjust their choice of word from situation to situation. But since those situation-variant choices often proceed according to an alien pattern of thought, such translations often result in claims that seem bizarre to the speakers of the home (translating) language (c.f. Quine 1960, 57-9; Nida 1964, 92; Hacking 1983, 69-71). And if, pace Quine (Quine 1960, 58), such translations are correct, but seem bizarre only because they represent non-standard usage of existing concepts, this last fact suggests that translation may alter the home language by introducing into it concepts that are incommensurable with current ones (see also Feyerabend 1977 and note 8).

15. This is not strictly true. Thus, to borrow a locution of Kuhn's, those critics occasionally "dimly recognize" that there are phenomena that seem incompatible with their views. For example, the logicians Patrick Suppes and Benson Mates acknowledge as a "subtle and complicated matter" the fact that some logical relations seem not to be purely formal, but to derive from the meanings of 'non-logical' expressions (Suppes 1957, 68; Mates 1972, 14-5, 80). And Shapere wonders briefly if "Feyerabend [and Kuhn have] in mind some special sense of 'inconsistent' (though [they] claim not to be abandoning the law of noncontradiction) or 'meaning'" (Shapere 1981, 44). But such brief acknowledgements of problems are invariably taken back; for such philosophers immediately go on to disparage unformalizable logical intuitions as irrelevant to logic (Mates 1972, 9-10); or irredeemably vague (Suppes 1957, 4); or far too "broad" (Shapere 1981, 54).

16. Because it often seems impossible to analyse formally actual scientific theories, Kuhn offers his own alternative which he believes is more accurate. I shall discuss Kuhn's alternative analytic schema in chapter two.

17. Correlatively, Kuhn's critics also believe that, read at face value, "incommensurable" means "not intertranslatable," and that on this construal, therefore, his thesis amounts to the self-contradictory claim that certain theories are both not inter-translatable and intertranslatable.

18. Formally, Kuhn's critics' argument is a "complex constructive dilemma" (*Encyclopedia of Philosophy,* s.v. "Traditional Logic," and "Glossary of Logical Terms"). Thus, on the following symbolization,

c_1: *prima facie* construal of the incommensurability thesis: "incommensurable" means either "(absolutely) incomparable" or "not intertranslatable."
c_1: alternative construal: theories can be both incommensurable, comparable and intertranslatable.
I: the incommensurability thesis is intelligible; in particular, not self-refuting.
R: the incommensurability thesis is philosophically-relevant; in particular, it requires changes in the received, philosophical view of science.

the argument of Kuhn's critics is:

$$\begin{array}{ll} 1. & c_1 \rightarrow \neg I \\ 2. & c_2 \rightarrow \neg R \\ \underline{3.} & \underline{c_1 \vee c_2} \\ \mathrm{C.} & \neg I \vee \neg R \end{array}$$

As I argue later (sections 1.4, 1.5 and 1.5.1), Kuhn's response to his critics amounts to an attempt to "escape between the horns of the dilemma": he effectively denies that either construal captures his intentions.

19. Trivially, two views are incompatible if they make *opposing* claims about *the same* situation. Kuhn also accepts this definition (c.f. Kuhn 1970a, 122).

20. Scheffler acknowledges that Kuhn may be right about incommensurability between the standards of scientific theories at a "first-order" (i.e. intratheoretical) level; but he hypothesizes that evident conflict between two theories presupposes a "second-order reflective and critical level of discourse," which "presupposes a certain sharing of standards at the second-order [intertheoretical] level" (Scheffler 1967, 83).

21. Davidson's actual expression is "dramatically incomparable."

22. What Davidson is referring to with the locution "*the* equipment of a single language" are a language's putative "referential apparatus...predicates, quantifiers, variables, and singular terms," and its "equipment" for dealing with the truth of "whole sentences," as displayed in Tarski's Convention T (*ibid.*, 193-4). In other words, Davidson believes there is a strong resemblance between natural and formal languages (c.f. Davidson 1984a, 29-30). In fact, much of Davidson's philosophy of language is based on a belief that natural languages *must* have an underlying structure very much like that of formal ones (c.f. *ibid.*, 3, 17, 30, 55). It is true that, *if* natural languages had the structure formal languages are supposed to have, they *would* form a single system of concepts, and translation would imply commensuration (See section 1.6 *et. seq.*). However, the Chomskyan argument Davidson offers for this *a priori* necessity (*ibid.*, 3)--that languages would not be learnable without formal recursive rules--is a version of the demand for commensurability, and so begs the question.

23. It is ironic that, though scientists often disparage the concerns of their opponents as "mere metaphysical speculation" (Kuhn 1970a, 103), Kuhn's philosophical critics often slander his work as "mere empirical speculation."

24. Recall that Davidson links the ideas of distinct conceptual schemes with that of incommensurable scientific theories.

25. Musgrave somewhat disappointedly concedes that "Kuhn's 'I never said it' ploy often convinces." *ibid.* p51. For similar views on Kuhn's 'retractions' see, Putnam (1981a) p126 and W.H. Newton-Smith (1981) p103.

26. One should keep in mind here that, though Kuhn believes incommensurable theories address a common world, he does not hold that this commonality can be expressed in terms acceptable to both theories. That means, in particular, that even so-called "ostensive definition" is not neutral between theories. See section 1.7.

27. This should already be clear from his equation of Kuhn's notion "incommensurable" with "dramatically incomparable" and "not intertranslatable." In fact, the strategy of Davidson's much discussed paper, "On the Very Idea of a Conceptual Scheme" (Davidson 1984b, 183-198), is not to show that comparison and translation entail common standards, and thus meet Kuhn head on; instead, he argues that *other* standards for individuating conceptual schemes, putatively independent of translation, turn out to

presuppose the ability to intertranslate those schemes: "Studying the criteria of translation is...a way of focusing on *criteria* of identity for conceptual schemes (*ibid.*, 184)....Can we then say that two people have different conceptual schemes if they speak languages that fail of intertranslatability?...My strategy will be to argue that we cannot make sense of total failure, and then to examine more briefly cases of partial failure" (*ibid.*, 185).

28. The traditional epistemological paradigm would lose its point because there would then be no reason to struggle to find elusive, common bases for evaluating scientific theories. For if I am right, rational theory choice would not need those common bases.

29. I should caution that the following discussion, while intended to be illuminating and compelling, must nevertheless be considered as programmatic. It would, in any case, be foolhardy to expect that proposals for modifying currently-accepted concepts will not themselves have to be refined continually to facilitate what Nelson Goodman calls "the *transition* from static absolutism to dynamic relativism in epistemology" (Goodman 1984, 19. Italics mine).

30. This *relativity* of commensurability is not, however, a completely undesirable result. Thus Kuhn notes that "scientific revolutions...need seem revolutionary only to those whose paradigms are affected by them. To outsiders they may, like the Balkan revolutions of the early twentieth century, seem normal parts of the developmental process" (Kuhn 1970a, 92-3). And Kuhn also suggests that "we may come to see ['the transition from Newtonian to Einsteinian mechanics'] as a *prototype* for revolutionary reorientations in science" (*ibid.*, 102. Italics mine). Thus we may come to construct prototypes or standards for comparing theories by using theory pairs like that of Newton and Einstein as irreducible instances of the relation of, for example, *fundamental theoretical incompatibility.*

31. The expression "structurally-complex" is included in the definition to exclude from the class of incommensurable objects things like phenomenal colours. Thus, without that qualification, it might be argued that, since one cannot determine the relations between, say, blue and yellow on the basis of their more primitive parts--they have none--blue and yellow are incommensurable. But, at least in this paper, speaking of colours as incommensurables would introduce unnecessary complexities. On the other hand, since incommensurability is clearly related to irreducibility, it might be possible to include such irreducible things as phenomenal colours as *limiting cases* of incommensurable

pairs of objects (c.f. note28).

32. It should be noted that direct comparability does not mean *untutored* comparability; that is, I am not implying that people who know nothing about, say, particular scientific theories could just look at them and judge their relations. In fact, I am implying just the opposite. To be able to compare directly two theories one clearly must first have an independent, sympathetic, full understanding (*verstehen*) of each. (I owe a recognition of this problem to Rudy Vogt.)

33. This last remark may be more familiar as the claim that the ratio of the hypotenuse to the side of a right isoceles triangle is approximately 1.4142 : 1. Incidentally, it should not be thought that, because there is a rule for approximating $\sqrt{2}$ with rational numbers, this rule makes them commensurable. For the series of approximations always remains different from $\sqrt{2}$; and the operation (multiplication) by which the approximation is compared with $\sqrt{2}$ is also disjoint in its separate applications. Thus, for example, one compares $\sqrt{2}$ with its approximation by squaring both numbers. But while the multiplication(1) of $\sqrt{2}$ by itself is simply *defined* to be 2; the multiplication(2) of any rational number (ratio of integers) by itself can be broken down further as the ratio of two numbers, each produced by *successive addition*. That is, though multiplication of irrational and rational numbers are formally *similar*; the first is a primitive operation, whereas the second is really successive addition.

34. I am here ignoring the construal of real numbers as either "the totality of infinite decimals," as "[infinite] sequences of nested intervals" or as "Dedekind cuts" (see Courant and Robbins 1958, 68-75) These construals represent attempts to analyse the notions of rational and irrational number in terms of putatively-more basic notions; and if they were successful, they would show that rational and irrational numbers (or pairs of lengths) are commensurable (in the sense of "reducible"). However, a discussion of those attempts is both irrelevant to my present concerns and beyond my present expertise. Let me just remark that they involve the (for me and many philosophers) suspect notion of actual infinity; and it is also doubtful that the "analyzed" notions of rational and irrational number are the same as the original ones.

35. In the very general sense of "formal comparison" I use--two things are formally comparable just in case they are comparable by some common feature--entities that aren't formally comparable are incommensurable.

36. Suppes there says: "Fortunately the systematic deductive development of mathematics or theoretical science can proceed without explicit recourse to sentences *whose truth follows simply from the meanings of the predicates used*" (Italics mine). But see (Kuhn 1977, 303 & n13) for an alternative view of the accuracy and real significance of such formalizations and a specific reference to Suppes.

37. This claim can clearly be extended to cover other logical relations and objects than valid arguments, premises and conclusions. It should also be clear here that Thomas does not mean by "semantics" either "formal semantics" or "model theory" (*ibid.* 449-52).

38. Incidentally, the seemingly-irrefutable psychological evidence that people do reason holistically has led some brain theorists (e.g. Michael Arbib) and philosophers (e.g. Mary Hesse) to develop non-computational models of brain function that parallel such semantic behaviour. Thus Arbib and Hesse try to correlate what they call *semantic nets or schemas* with what they term *neural nets or columns* (Arbib & Hesse 1986, 34-41, 69-72).

39. I take this last parallel from Craig Dilworth's *Scientific Progress* (Dilworth 1986, 73). Dilworth develops what he calls a "perspectivist" view of incommensurability between scientific theories by extensive use of the *duck-rabbit* figure, and he shows how (what I call "direct comparison") can be used to favour one aspect over another. Thus he notes that one can argue for the greater accuracy of the rabbit perspective by noting that an "indentation" on the duck's head is somewhat anomalous from the duck point of view while the rabbit-mouth interpretation is to be expected on the rabbit view (*ibid.*, 73).

40. The idea of defining a part by abstraction from a whole should not seem strange to those familiar with Frege's work. Thus Frege derived the constituent predicates (concepts) of a sentences by abstracting away those sentences' singular terms. For example, the particular sentence, "John loves Mary," divides into the one-place predicates, "x loves Mary" and "John loves *y*," and the two-place predicate, "x loves *y*."

41. I have in mind here the "third-man" argument against Plato's theory of ideal forms and Wittgenstein's more general rule-following considerations which attack the idea that universals must be determined by formal definitions: e.g. by necessary and sufficient conditions. That Wittgenstein's work on rules is relevant here should not be surprising since Kuhn's basic idea of the par-

adigm, which presumes direct comparison (see chapter two), is a specific adaptation of Wittgenstein's notion of family resemblance. But Wittgenstein's discussion of family resemblance concepts in the *Investigations* just precedes an extensive discussion of rule-following which serves as a propaedeutic to his still more extensive discussion in §§143-242.

42. Here it may be useful to keep in mind Wittgenstein's critique of the primitive nature of ostensive definition (Wittgenstein 1968, §§28-30). There he points out that ostensive definitions are not essentially clearer in meaning than other referential expressions. For example, if someone points to a certain tall animal and says: "I'm referring to *this*," he may be referring to the animal, its shape, colour, age, etc. The general point about determining coreference is simply that at some point one must just see, without any further grounds, including ostension, that two terms are coreferential.

43. "Determined directly" does not mean here: "determined by 'direct reference' to a common object." That is, I am not claiming there must be some sort of unmediated perception of objects à la Kripke with which coreference can be determined. Rather, I am simply arguing that the recognition that two different ways of designating some object are coreferential must, at some point, not require any further means of determining reference. Thus I am actually denying that there *need* be any privileged means, such as ostension, for determining coreference. See also note 42.

44. For example, Kuhn notes that the systematic, focusing character of standards (paradigms) in science permits "scientists to investigate some part of nature in a detail and depth that would otherwise be unimaginable" (Kuhn 1970a, 24).

Chapter 2

Kuhn's Alternative Analytic Concepts

2.0 Introduction

In this chapter, I show how Kuhn's concepts for analysing science perform many of the same evaluative functions as traditional notions drawn from formal logic, but without precluding either incommensurability or, as we shall see, the historical character of scientific theories. I shall devote particular attention to Kuhn's analysis of the "dialectical" role of thought experiments in science because that analysis illuminates his logico-normative conception of theories and provides the basis for developing in the next chapter a generalized proof of incommensurability.

In what follows, I describe Kuhn's alternative to the traditional picture of scientific theories as universal generalizations. I there show that Kuhn believes his counterpart notion, the *paradigm*, is the more basic both because paradigms give scientific generalizations their empirical meaning and because this last process must be repeated anew for novel situations. It thus turns out that by making the notion of a paradigm basic to science, Kuhn also makes theories into historically-evolving traditions, not timeless, formal structures as on the received view. Paradigms also explain theoretical holism and make room for incommensurability.

2.1 Paradigms and Universal Generalizations

Ronald Giere notes that "since Aristotle it has been assumed that the overall goal of science is the discovery of *true universal generalizations* of the form: All A's are B" (Giere 1983, 271. Italics in original). However, the notion of a universal generalization is an elementary category of formal (syllogistic and predicate) logic. Hence, to make such a notion, and thus the logical system of which it is a part, the basic analytic unit for science would also, as we saw in chapter one, make incommensurability impossible (see §1.6).[1] But historical evidence of *actual* incommensurability and conceptual evidence of its *possibility*, suggests that universal generalizations cannot play such a fundamental part in science. There is more specific evidence for that fact: the empirical and formal indeterminacy of scientific concepts.

Science commonly contains many generalizations. For example, Newton's first law of motion begins with a claim about the behaviour of "*every* body" (*Enc. of Phil.*, s.v. "Newtonian Mechanics and Mechanical Explanation." Italics mine). And such claims are easily rendered formally as, e.g., $(x)(Fx \rightarrow Gx)$. But for generalizations not to be purely formal,[2] but also to have empirical implications, their constituent concepts (here, F and G) must have determinate empirical meaning *prior to* their comparison with nature. For unless it were clear what a generalization said in a given instance, no conclusions could there be drawn about its empirical truth or falsity (see also Kuhn 1977d, 284). It follows that if empirical indeterminacy in new cases were the rule, normally the "overall goal of science" would be to remove that indeterminacy by discovering how to extend the scope of concepts; it could not, in any non-trivial sense, be "the discovery of true generalizations." For, at any given time, scientific generalizations would be true only of those situations to which their concepts have already been extended and indeterminate for those to which they have not.[3]

A surprising example of empirically indeterminate generalizations are Newton's formulations in the *Principia* of the laws of motion and gravitation. Even though these laws are often cited as *the* scientific model of both precision and generality,[4] they frequently proved noncommital or vague in their import for previously-unexplored situations (Kuhn 1970a, 32). Therefore, since it was often impossible to predict the implications of Newton's laws for relevant situations, its concepts were not empirically determinate. Hence, its later applications were not strictly contained in Newton's original formulation.[5] "These limitations of agreement left many fascinating theoretical problems for Newton's successors...[For example,] Euler, Lagrange, Laplace and Gauss all did some of their most brilliant work on problems aimed to improve the match between Newton's paradigm and observations of the heavens" (Kuhn 1970a, 32).

Kuhn says that he "introduced the term 'paradigm' to underscore the dependence of scientific research on concrete examples that bridge what would otherwise be gaps in the specification of the content and application of scientific theories" (Kuhn 1970d, 284). Therefore, since paradigms specify "the content and application of scientific theories," a paradigm is conceptually "prior to the various concepts, laws, [sub]theories, and points of view that may be abstracted from it" (Kuhn 1970a, 11). From the preceding discussion of empirical indeterminacy, therefore, one would also expect that, while use of paradigms might *help* to specify the content of theories by setting broad parameters, it could not rigorously determine that content. Kuhn claims that analogical reasoning based on paradigms, or "concrete scientific achievements" (*ibid.*, 11), helps to specify the content of theories in just this rough way. Thus if he is right, this analogical reasoning is more basic to science than the traditionally-preferred formal deduction from generalizations, auxiliary hypotheses and initial conditions.

2.1.1 Normal Science as a Process of Paradigm Articulation

Since for Kuhn, a scientific theory's generalizations get their content from "concrete examples," his philosophy inverts the traditional view of the relation of universal to particular. Instead of starting out as *completed* generalizations containing all their implications, Kuhn says that theories begin as "selected and still incomplete examples" (Kuhn 1970a, 23), or *paradigms*, which are continually *made more general* by refinement and by developing still further instances of successful applications (Kuhn 1970a, 30). It is just these given particular applications that show how the theory's concepts, laws, and generalizations can be applied to "some [new] concrete range of natural phenomena" (*ibid.*, 46). Exploitation of these accepted applications provides scientists with interpretations for the theory. Where Kuhn's account differs from more traditional model-theoretic notions of interpretation is that, for him, at no point is the theory's interpretation fixed for all relevant cases. Instead of being *actual* generalizations, therefore, it is more accurate to say that, for Kuhn, theories are *programmatic* generalizations. For he claims the creative input of scientists doesn't end with the initial expression of a theory or at any stage when it is still considered viable. Rather, scientists are forced to adapt continually their current theoretical resources to handle new problems. This ongoing, ampliative process of realizing a theory's promise by constant exploration and refinement, Kuhn calls *paradigm articulation* or *normal science*.

> *Normal science* consists in the actualization of that promise, an actualization achieved by *extending* the knowledge of those facts that the paradigm displays as particularly revealing, by *increasing* the extent of the match between those facts and the paradigm's predictions, and by further *articulation* of the paradigm itself (*ibid.*, 23-4. Italics mine).

Kuhn claims his studies show scientists usually do not abstract from existing applications explicit rules for determining further applications (*ibid.*, 46). Instead they use previous achievements as suggestive models of how to approach new problems. In so doing, they intentionally replicate the entire conceptual structure of their theory in the new situation (Kuhn 1982, 566). Thus analogical reasoning based on paradigms naturally leads to holism. "Scientists...never learn concepts, laws, and theories in the abstract and by themselves. Instead, these intellectual tools are from the start encountered in a historically and pedagogically prior unit that displays them with and through their applications" (*ibid.*, 46).

It is in his detailed account of how scientists "extend," "increase" and thus "articulate" their paradigms without explicit rules for guidance that Kuhn evinces most clearly a distinction between the concepts of (direct) comparability and commensurability, and thus also comparability and incommensurability. He gets this account by drawing a close analogy between his idea of scientific theories as paradigms and Wittgenstein's notion of the family resemblance concept.

2.1.2 Paradigms, Commensurability and Direct Comparability

Famously, Wittgenstein has pointed out that it is often impossible to find a set of common characteristics informing many of our ordinary concepts. For example, when we search for a determinate set of features common to all and only games, upon careful scrutiny we find instead "a complicated network of similarities overlapping and criss-crossing: sometimes overall similarities, sometimes similarities of detail" (Wittgenstein 1968, §66). Thus we discover that some games are merely amusing, while others are seriously competitive; some involve skill, but others depend only on luck; some involve very strict rules, though others are flexible and spontaneous; and still others contain various combinations of these features; and so on. Because there is no fixed set of similarities common to all games, but only a growing (*ibid.*, §68) family of resemblances connecting particular instances, in learning the concept of game, examples (paradigms) must play an essential, not simply illustrative role. "Here giving examples is not an *indirect* means of explaining--in default of a better [e.g. like an explicit definition]" (*ibid.*, §71. Italics in original). It follows that the extension of the concept of game is, at any given time, "uncircumscribed" (*ibid.*, §70), since it is determined by nothing more basic than the particular games themselves.

Kuhn puts Wittgenstein's notion of family resemblance to work in the following way. He says that scientists obtain new applications "by the *direct* inspection of paradigms," (Kuhn 1970a, 44. Italics mine) and that these new applications are linked with each other and the original paradigms "by [family] resemblance and [direct] modeling to one or another part of the scientific corpus" (*ibid.*, 45). Because a scientific paradigm represents a more complex conceptual structure than the simpler notion of a game, such paradigms might instead be called: *family resemblance conceptual systems*.

Etymologically, the term "paradigm" naturally lends itself to a discussion of incommensurability, since it means "standard of comparison." Thus one would expect that the concepts and laws of incommensurable theories would get their respective meanings from incompatible "manipulative procedures and paradigm applications" (Kuhn 1970a, 142-3). But Kuhn's account of paradigms also has deep parallels with my discussion of comparability and commensurability in chapter one. Thus because each new application employs the same basic set of common structural elements (i.e. concepts) as its *paradigms*, each is in effect *commensurable* with any other such application. Kuhn also recognizes that commensuration itself rests on direct comparison. For each application is obtained by a *direct* comparison with either the paradigms themselves or one of their current applications, which link them to those paradigms by a "network of overlapping and crisscross resemblances" (*ibid.*, 45).

Like Wittgenstein, Kuhn argues that the similarities linking the various applications themselves form no fixed, standard set; so those similarities cannot provide an interpretive standard more basic than the paradigms (examples) themselves.

> Though a discussion of *some* of the attributes shared by a *number* of [exemplary solutions to problems] often helps us [as a temporary heuristic] learn how to employ [the theory], there is no set of characteristics that is simultaneously applicable to all members of the class [of solutions] and to them alone. (*ibid.*, 45)

In other words, because it cannot be known in advance which similarities will prove fruitful in extending a paradigm, no rule can be abstracted from existing similarities that will determine later extensions. So such similarities cannot determine scientists in *finding new applications*; rather, scientists must obtain new applications by *finding fruitful similarities*. Because the network of similarities cannot play the role of an interpretive standard, Kuhn calls the resemblances between paradigms and applications "direct." Paradigms and applications, therefore, constitute a primitive *similarity set*, since they are all similar to each other, *simpliciter*. As just noted, Kuhn claims that such unmediated patterning of new problems on old solutions is basic to the scientific enterprise.

> The practice of normal science depends on the ability, acquired from exemplars [paradigms], to group objects and situations into similarity sets which are primitive in the sense that the grouping is done without an answer to the question, "Similar with respect to what?" (Kuhn 1970a, 200)

Kuhn provides several historical examples of what he means by "direct modeling" (*ibid.*, 47). For instance, Galileo learned to see a (point) pendulum's cycle as importantly similar to the seemingly very different situation of a ball rolling down one incline and up another (*ibid.*, 190). Galileo showed how, in both cases, the moving objects could *ideally* be seen as starting from a certain height, passing through a lowest point, and returning to their original elevation. Later, Huyghen's found an important analogy between Galileo's treatment of (imaginary) point pendula and the behaviour of (real) extended ones. By construing a real pendulum as a cluster of point pendula that could each swing independently upon release, Huyghens was able to solve the problem of finding a real pendulum's centre of oscillation (*ibid.*, 190). Daniel Bernoulli later adapted Huyghen's model to treat "the flow of water from an orifice" (*ibid.*, 190).[6] The only generalization common to each of these solutions is the "Principle of *vis viva* ...Actual descent equals potential descent" (*ibid.*, 191). But each situation--balls rolling down inclines, point and real pendula, water flowing from an orifice--does not at first glance seem relevantly similar to any of the others. And until Galileo, Huyghens and Bernoulli found specific ways to see these new circumstances as instances of "vis viva"--until they articulated their concepts--they could not apply the formal techniques of their earlier applications to them (c.f. Kuhn 1977, 305).

Perhaps the most striking illustration Kuhn gives of direct modelling and resemblance is the set of articulations developed from a formal statement of Newton's second law. These extensions make his point in a particularly dramatic way since they show that even formal generalizations, whose use supposedly involves merely mechanical manipulations, often require creative articulation before they can be used for physical problems. Moreover, the purely formal resemblances such articulations have with each other and the original equation is often hard to discern. For example, the standard (since Gauss) expression of the second law is $f = ma$. But that law can also be applied to such seemingly distinct situations as: freely-falling bodies, where it takes the form $mg = m\, d^2s/dt^2$; simple pendula, where it becomes $mg\sin\theta = ml\, d^2\theta/dt^2$; interacting harmonic oscillators, where it is expressed with two equations, one of which is $m_1 d^2s_1/dt^2 + k_1s_1 = k_2(s_2 - s_1 + d)$; "and for more complex situations, such as the gyroscope, it takes still other forms, the family resemblance of which to $f=ma$ is still harder to discover" (*ibid.*, 189).

2.1.3 Direct Comparison of Competing Paradigms

For Kuhn, then, each scientific theory consists of a small set of paradigms and an expanding set of applications derived from them by "direct modeling."[7] These additional applications are all *indirectly* related to each other but are *directly* related to the set of paradigms: their common standards. And because that last relation is direct, *there are no further standards* more basic to a scientific theory than its paradigms and articulations (c.f. Kuhn 1970a, 11).[8] It follows, therefore, on this model, that scientific theories that employ different paradigms won't share some common standards, and so will be (at least partly) incommensurable.[9]

In chapter one, I claimed that incompatible, yet incommensurable theories were conceivable if it were possible to recognize those theories' incompatibility by the direct comparison of one theory with another. Such a direct recognition of incompatibility would depend on being able to see, by directly comparing how each whole theory used its set of referring terms, concepts and laws, that those terms were often co-extensive and that their respective theories said incompatible things about their common domain. We have just seen how direct comparison works in practice *within* the scope of a paradigm. Still, though direct comparison between and within paradigms are very much alike, they are also importantly different.

On Kuhn's "gestalt" model, incommensurable theories can be directly compared by finding pairs of similar, but distinct concepts (terms, laws, etc.) from each theory and displaying their similarities and differences by simply *juxtaposing* them in a "disciplined" (Kuhn 1977d, 267) manner. For example, one could display the similarities and differences between the Newtonian concept of *mass* and its Einsteinian counterpart in the following way.[10] *Newtonian mass* equals the ratio of force to acceleration, and is constant; (at low relative velocities) *Einsteinian mass* also (roughly) equals the ratio of force to acceleration, and is also (roughly) constant. *Newtonian mass* is a property of bodies that is a measure of their inertia; *Einsteinian mass* is also a property of bodies that is a measure of their inertia. By contrast: "Newtonian mass is conserved; Einsteinian is convertible with energy. Only at low relative velocities may the two be measured in the same way, and even then they must not be conceived to be the same" (Kuhn 1970a, 102).[11]

Clearly, these comparative juxtapositions of the concepts of incommensurable theories bear a strong 'family resemblance' to Kuhn's idea of arranging paradigms and applications in a similarity set. Nevertheless, there is an important difference between the external and internal sorts of comparisons. For while scientists obtain solutions to their problems by direct modelling on their paradigms, comparing incommensurable theories could not serve that same function. This is because noting the similarities and differences in the ways distinct paradigms determine what is true, is not in itself a way of determining what is true (see *ibid.*, 122-3); for no paradigms mediate *that process*. Nevertheless, that external comparative process can aid in determining which (if any) of two competing theories is more accurate or promising by displaying each one's relative merits *in its own terms*. "That exhibit can be immensely persuasive, even compellingly so" (Kuhn 1970a, 94).

In summary, Kuhn's notion of a scientific paradigm performs many of the same functions as the more traditional notion of a universal generalization. Thus, just as on the traditional model where scientists deduce particular empirical consequences from their theories (generalizations), so too on Kuhn's approach scientists obtain "concrete achievements" by "direct modeling" on their existing theories (paradigms). Where Kuhn's approach differs from the more traditional one, however, is by making the determination of theoretical content an historical process and by permitting incommensurability. His account also differs in significantly reducing the sensitivity of theory to experiment. Still, as we shall see, because it does not completely eliminate experimental corrigibility, in the end Kuhn's view also makes experience the final court of appeal for theories.

2.2 Felt Anomaly

As noted above, Kuhn's account of how a theory's concepts and generalizations get their content from exemplary achievements (paradigms) by articulation is based on historical evidence of the empirical and formal indeterminacy of scientific concepts. Since articulation is (roughly) Kuhn's counterpart for the traditional notion of logical derivation, and since the usual concept of theory falsification depends on that of logical derivation, one may expect that Kuhn will also employ in his approach an alternative notion to that of falsification. That alternative notion is anomaly or anomalous experience (Kuhn 1970a, 77, 80, 146).

Anomalous experiences are similar to and different from falsifying experiences in the following ways. First, just as on the traditional logical model where a (genuinely) falsifying experience results in the rejection of a theory, anomalous experiences also (often) result in theory rejection (*ibid.*, 146).[12] Moreover, just as falsifying experiences are of situations that are different from that implied by a theory, so too anomalous experiences are of circumstances which fail to fit "paradigm-induced expectations" (ibid., 52-3). By contrast, however, while (genuinely) falsifying experiences *always* demand the rejection of the theory which entails them, this is *seldom* the case for anomalous experiences. An anomaly is, for Kuhn, only a failure of nature to fit a theory *in its current state of articulation*; in other words, it is a failure *pro tem*. In support of his own concept, Kuhn points out that historical and contemporary evidence show that *right from their very inception*, all theories fail to fit nature in many areas, and even in those places where they are notably successful, the theory-nature match is often far from perfect. Hence, if one accepted the traditional view that "any and every failure to fit were ground for theory rejection, [this would result in the paradoxical view that] all theories ought to be rejected at all times" (*ibid.*, 146).[13]

Usually, even the "most stubborn" anomalies a theory is confronted with, prove to be resolvable by paradigm articulation, the method of normal science (Kuhn 1970a, 81; see also Kuhn 1977b, 262). While such circumstances are recognizably incongruous from the beginning, because their import for the paradigm cannot yet be known with any precision, Kuhn sometimes describes them as "felt anomalies" (Kuhn 1977, 264). And just because of that current unclarity of import, scientists often shelve such problems to be readdressed by "a future generation with more developed tools" (Kuhn 1970a, 84). Some historical examples of felt anomalies that later proved to be resolvable by normal science were the initial failures of Newtonian mechanics to get a good value for the speed of sound in air; to give the actual orbit of the moon; and to find a law for the forces between electrical charges (Kuhn 1970a, 33, 81; Kuhn 1977, 196-200). The first problem was largely solved by Laplace (about 1815), the second by Clairault (1750), and the third by Coulomb (1785).

2.3 Critical Anomaly and Crisis

While most (felt) anomalies prove to be resolvable by the paradigm's (growing) resources, occasionally some prove to resist the greatest efforts at articulation by that paradigm's best practitioners (Kuhn 1970a, 5). When that happens, Kuhn says, (in the area of the stubborn anomaly) the practice of normal science becomes impossible and the "extraordinary science" which takes its place, indicates a "crisis state." Because it is the stubborn anomaly's resistance to being "assimilable by paradigm articulation" (*ibid.*, 35) that provokes the crisis, I shall call such anomalies "critical." Historically, such "transition[s] from normal to extraordinary research" share the following characteristics: "the proliferation of competing articulations, the willingness to try anything, the expression of explicit discontent, *the recourse to philosophy and the debate over fundamentals*..." (Kuhn 1970a, 91. Italics mine).[14]

The process of normal science often generates "competing articulations"--that is to say, alternative ways of extending established precedent. For example, Kuhn points out that the extension of the caloric theory of heat, from situations involving mixing and changes of state, to those involving chemical reactions, friction and gas compression and absorption could have taken place in different ways. And "many experiments were undertaken to elaborate these various possibilities and to distinguish between them" (*ibid.*, 29).[15] In a state of crisis, however, the number of competing articulations "proliferate" because no one of them proves sufficiently successful to provide the new basis of research in the problem area. Scientists become "willing to try anything" because their previous exemplars no longer serve to guide research.

That new openness to alternative procedures even turns scientists "to philosophical analysis as a device for unlocking the riddles of their field" (*ibid.*, 88). Philosophical analysis can serve this purpose, Kuhn says, for two reasons: (1) the very process of searching for *assumptions* can weaken the belief that only those procedures sanctioned by the current paradigm(s) can resolve the issues; and, more deeply, (2) it can "expose the paradigm to existing knowledge in ways that isolate the root of crisis with a clarity unattainable in the laboratory" (*ibid.*, 88). Since, for Kuhn, "existing knowledge" is embodied in all the articulations a paradigm has undergone since its inception, what philosophical analysis does on his model is to uncover which articulations ultimately come into conflict with their paradigm. Analysis achieves that result, Kuhn says, "by transforming felt anomaly into concrete contradiction" (Kuhn 1977, 264). As examples of just this notion of crisis-inspired philosophical analysis, he cites the classical scientific "thought experiments" of Galileo, Einstein and Bohr, among others. As we shall see, one intriguing outcome of Kuhn's approach is that thought experiments are not purely conceptual in import, but also have empirical consequences. But that should not be surprising if one recalls Kuhn's arguments, discussed in chapter one, that choice of concepts is not theoretically, and thus also not empirically, neutral.

2.4 Thought Experiment: Isolating the Root of Crisis

Kuhn says that, on the traditional philosophical understanding of the scientific role of thought experiments, that "function...is to assist in the elimination of prior confusion by forcing the scientist to recognize contradictions that had been inherent in his way of thinking *from the start*" (Kuhn 1977, 242. Italics mine). In other words, on that traditional understanding, scientists who originally formulate concepts that are later shown to be problematic, initially make conceptual errors that were, even at their formulation, eminently avoidable. The traditional view leads, Kuhn argues, to such implausible suggestions as that Aristotle, one of the most brilliant logicians, made extremely elementary conceptual errors (*ibid.*, 253).

The discussion in section 2.1 of scientific generalizations suggests that one reason the traditional philosophical paradigm is driven to such implausible claims is that it presupposes that concepts have fully determinate meanings at inception. But on Kuhn's view scientific concepts acquire meaning over time by articulation to novel, but relevant circumstances. Thus depending on how the world is, that articulation could introduce applications that later come to conflict with earlier ones. The falsification of the empirical content embodied in the earlier usage would then also reveal a conceptual conflict between the earlier and the later articulations. But if the world to which the scientific concepts were articulated had turned out to be different, then the empirical commitments they came to embody might not have led to conflict. So for Kuhn the problematic concepts have no "intrinsic defect"; they just fail "to fit the full fine structure of the [actual] world" (Kuhn 1977b, 258).

Another, more subtle source of the traditional view's construal of thought experiments is its presumption of commensurability: its belief that all possible empirical theories are formulable within a common system of strictly logical and non-logical concepts. Hence, on that received paradigm, the logical contradictions revealed by thought experiments will be solely attributable to a misapplication of the common logical apparatus at the time when the now problematic concepts were first formulated. Which means, of course, that no *empirical* conclusions can be drawn from the resolution of those contradictions.

Nevertheless, in opposition to the traditional view, much philosophical, historical, and psychological evidence suggests both that concepts are seldom, if ever, completely determinate, and that scientific theories are often incommensurable. Such indeterminacy and incommensurability imply therefore that the contradictions which often surface in periods of crisis need not also have been present at the paradigm's inception. They also suggest that those articulation-generated contradictions can reveal the ways in which the system of concepts is (now) malformed, and thus provide a hint to the resolution of those contradictions (c.f. Kuhn 1977b, 264). That resolution will clearly involve a reorientation of the previous paradigm's conceptual relations, and so will create a completely new, hence incommensurable, conceptual system whose articulation will embody previously inaccessible empirical knowledge.

Kuhn's conception of thought experiments has a distinctly "dialectical" character. Marx Wartofsky defines dialectic as "the destruction of an argument [thesis], or better yet, a universal proposition by an argument--by the discovery of a contradiction [antithesis] in the views of those who propound it" (Wartofsky 1977, 13). That destruction however often has a "constructive consequence" or "synthesis"; it often pinpoints the problematic commitments of the thesis thus showing how to construct "alternatives to the failed premises" (*ibid.*, 14). Similarly, Kuhn says that the famous (dialectical) thought experiments of "Galileo, Einstein, Bohr and others" (Kuhn 1970a, 88) not only "transformed felt anomaly into concrete contradiction"; they simultaneously "provided the clues to set the situation right" (Kuhn 1977b, 264). As we shall see in chapter three, thought experiments also prove that the new "synthesis" must be incommensurable with the previous "thesis."

In order both to illustrate the concept of a thought experiment and to explicate Kuhn's account of them, I shall now give a brief analysis of two typical members of this class: Piaget's volume conservation and Galileo's velocity thought experiments.[16]

2.4.1 A Piagetian Thought Experiment: Volume Conservation

In the Piagetian experiment, typically a young child (four to five years old)[17] will claim that there is more water in a tall, narrow glass than in a short, wide glass--even though the child knows that both glasses have been filled with water poured from two, identical receptacles with equal levels of water in them, and whose volumes he originally pronounced equal. What is even more surprising than the child's initial response is that at this stage it is normally impossible to convince him that he has made a mistake. Thus, when one pours the water back into the original vessels, where the water levels are once again apparently equal, the child then readily agrees that both vessels *now* have the same volume. Clearly, at this stage, the child's criterion of comparative volume for liquids is something like relative height.[18]

A short time (about a year) later, however, while our typical child will, when confronted by the same situation, once again claim that the tall, narrow glass has more water in it, he now dimly recognizes that his reply is anomalous. And when he is still older, he immediately claims that both the tall, narrow and the short, wide glasses have equal volumes of water in them. Obviously, something has happened to the child in the interim between the first and later trials to change his perception of the experimental situation. That is, it is clear that somewhere along the way, the child has acquired the concept that liquid volumes do not change merely through transference from one vessel to another. Piaget's thought-experimental situation is perfectly designed to force a conflict between the child's original notion and that later development. Here is one plausible Kuhnian reconstruction of the child's progress.

Initially, our typical child probably learned the concept of relative volume in simple situations where the vessels were of quite similar size and shape. In other words, the child's original "paradigms" of relative volume likely only took into account circumstances in which vessel shape and size were roughly identical. It is quite easy to see how those paradigms could then have been "articulated" to develop the related concept of volume conservation through liquid transference. For the child would almost certainly also have acquired that extension of his original paradigm in familiar situations where the vessels were of quite similar size and shape. Notice that, on Kuhn's view, the articulated paradigm will not yet embody a contradiction; for the articulation has not yet itself been extended to situations where volume transference takes place between vessels of different sizes and shapes. Moreover, Kuhn wants to argue that even after such a further extension of the original paradigm, the child's "conceptual apparatus" still does not yet contain a contradiction. That is because life *need never* present a child with the more tricky situation contrived by Piaget, and because it also seems unreasonable to demand that when his concepts were formulated he must have had such situations in mind (c.f. Kuhn 1977b, 254). In other words, it seems utterly "Pickwickian" (Kuhn 1977d, 288) to imagine that, when forming concepts, they be designed to "be applicable to any and every situation that might conceivably arise in any possible world" (Kuhn 1977b, 254).

Nevertheless, *this world* apparently does often present children with Piaget's sorts of anomalous circumstances, for they invariably go on to form the more sophisticated adult notion of relative volume. In fact, at a certain age the Piagetian experimental situation can itself be used to achieve that end.[19] Once the child recognizes that transferring liquids in such circumstances doesn't change their volume, but that his paradigm of relative height says that the comparative volumes have changed, he is forced to re-evaluate the relative importance of his original paradigm and its later articulations. Often now the conservation criterion takes pride of place, and that of relative height becomes subordinate.[20] This conceptual reorganization is therefore a revolution; for even those situations adequately handled by the previous paradigm are now construed quite differently. Thus because the thought experiment transforms the ways in which "existing [empirical] knowledge" about such situations is seen, it therefore also gives new empirical information.

2.4.2 Galileo's Thought Experiment: Total Motion and Acceleration

In the following thought experimental situation, Galileo shows that an articulated form of Aristotle's concept of velocity leads to contradictions. Aristotle treated the notion of change of place, to which the term "motion" is now usually restricted, on analogy with qualitative changes of state like healing or burning (*Physics*, Bk. V): thus he says that "*every* change is *from* something to something--as the word itself *metabole* indicates" (*Physics*, Bk V, 224b35-225a1; Kuhn 1977b, 246). That choice of definition leads Aristotle to make an empirical commitment: he makes essential use of the beginning and terminal points of a change of place to determine the quantity and speed of that motion. For example, he claims that "The quicker of two things traverses a greater magnitude in an equal time, an equal magnitude in less time, and a greater magnitude in less time" (*ibid.*, 232a25-7).

Aristotle's notion of velocity is thus very much like the modern concept of *average* velocity; however, it is not identical with the modern concept because it embodies no distinction between average and instantaneous velocity. The latter concept, since Galileo, has been seen to be necessary to deal with continuously-varying motions or accelerations. In fact, because he doesn't recognize the modern distinctions, Aristotle formulates physical laws like the following: "If, then, A the movent have moved B [the thing moved] a [total] distance C in a [total] time D, then in the same time the same force A will move 1/2 B twice the distance C, and in 1/2 D it will move 1/2 B the whole distance C: for thus the rules of proportion will be preserved" (*ibid.*, 249b30-250a4; Kuhn 1977, 256). Such a law, while it seems initially peculiar from the standpoint of Newtonian physics, is in fact perfectly adequate for those situations that do not involve acceleration.[21]

While Aristotle was *largely* unaware of situations involving accelerations, he occasionally dimly recognized the presence of changing motions. For example, when he distinguishes "natural" or unforced from "violent" or forced motions, Aristotle makes no mention of their endpoints: "But whereas the velocity of that which comes to a standstill seems always to increase, the velocity of that which is carried violently seems always to decrease" (*Physics*, 230b23-5; Kuhn 1977, 247). In other words, Aristotle seems able to compare velocities without his standard criteria. Nevertheless, as the need for the Galilean thought experiment later showed, he probably presumed that eventually such motions could be assimilated by his *motion-as-change-of-state* paradigm.

Before Galileo could convict the Aristotelian concept of inadequacy, however, that concept first had to be further articulated so that it could begin to formulate more precisely situations involving continuously-changing motions. That articulation was achieved by the impetus theorists of the middle ages through their notion of *latitude of forms*. That notion allowed a distinction between the "total" velocity of a motion and the "intensity" of a velocity at any point during that motion (Kuhn 1977, 247). While this neo-Aristotelian distinction is clearly a lot closer to the post-Galilean one, as Kuhn points out, it still requires that the distances and times ("extensions") of the compared motions are equal. It is just this articulation of the original Aristotelian paradigm that Galileo shows leads to contradictions in the thought-experimental situation he contrives in his *Dialogue Concerning the Two Chief World Systems*.

In that fictitious dialogue, Galileo asks two Aristotelians to imagine a situation in which two bodies slide without friction down two planes (CB and CA) from a common point (C). (Note that CA is significantly longer than CA so that the extensions of the two motions are not equal.)

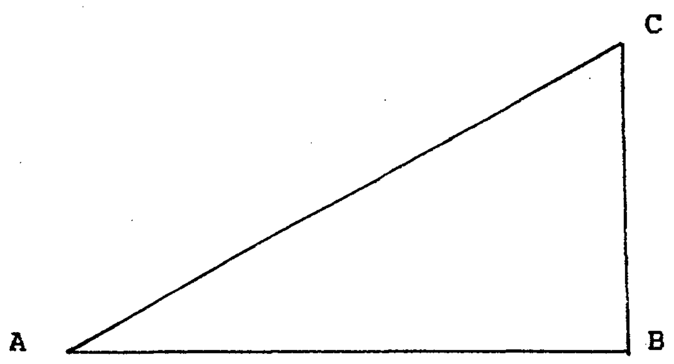

Fig. 2

Galileo first asks his Aristotelian opponents to agree that when the objects slide down CB and CA and reach their respective termini, they will have the same impetus or intensity of motion: in other words, they will have acquired sufficient velocity to return them to their common starting point, C. He then asks them which of the two bodies is the faster. Their first (natural) response is to say that the object sliding down CB is faster: it gets to its terminus in less time and has greater apparent velocity along the way. Nevertheless, that response seems incompatible with the articulated criterion of the impetus theorists; for that criterion says that since both bodies started with the same initial velocity (at rest) and ended with the same final velocity, their mean speed must be the same.

Galileo then suggests that the problem arises from trying to compare two motions with different spatial extensions. So the solution to the Aristotelian dilemma might be to choose equal extensions, say, by only requiring the body on the inclined plane to travel a distance CB. Unfortunately, this only worsens the problem. Thus, it turns out that, depending on where one lays out the length CB on the plane CA, the Aristotelian criterion permits the velocity on the incline to be at once slower, faster, and the same. For example, if the length CB is measured on CA from C, the vertically moving body will reach its terminus in less time; if that length is measured up from the point A, then the body on CA will reach its terminus first; and if that length begins at some well-chosen point along CA between C and A, both bodies will reach their termini at the same time. Thus the application of the articulated Aristotelian concept to a comparison of the velocity of accelerating bodies results in contradiction.

Galileo used this thought experiment to bifurcate the older Aristotelian notion into the modern kinematic concepts of instantaneous and average velocity. In Galileo's reformulation, the concept of instantaneous velocity is the more basic since it can compare both constant and changing speeds. Thus after Galileo's conceptual revolution, the concept of instantaneous velocity becomes the paradigm, and Aristotle's "qualitative change" notion is transformed into the modern concept of average velocity.

2.5 Morals

In this chapter I have tried to show that Kuhn's account does no great violence to traditional scientific values. Thus his version of how paradigms give meaning to scientific concepts does not make theories completely immune to empirical evidence; nor, therefore, does it make them rationally incomparable. For when an experimental anomaly becomes critical, the new theory which "renders the anomaly lawlike" (Kuhn 1970a, 97) by conceptual revolution can be seen to be a better theory than the one it supplants--even though the two theories employ different concepts. For example, even though Dalton's new chemical theory employed different concepts from Berthollet's affinity theory, no common concepts were required to show that Dalton's view was better. Chemists were able to see *directly* that Dalton's concepts dealt with chemical phenomena in a better manner than Berthollet's because, among other things, those concepts avoided Berthollet's anomalous conflation of seemingly-distinct substances.

It is often difficult to convince those who strongly associate particular formulations of some subject with that subject itself that when one challenges those formulations, he is not thereby necessarily challenging the basic integrity of that subject. For example, because ethics is, for many, inextricably associated with the idea of God, denials of God's existence often lead those people to exclaim that "if God is dead, everything is permitted" (Dostoyevsky, *The Brothers Karamazov*). Similarly, in the philosophy of science, some philosophers conclude, whether in alarm (e.g. Scheffler) or glee (e.g. Feyerabend), that if traditional, formal canons of rationality don't apply to science, then "anything goes" (Feyerabend 1975, 23-8). But that conclusion doesn't follow anymore than the previous one. Thus just as Socrates showed (in the *Euthyphro*) that the concepts of ethics and God must be dissociated to avoid contradiction, so too, I have argued in this chapter, must the concepts of empirical rationality be defeased from formal accounts of them to avoid making out as irrational our epitome of rationality, science.

Finally, incommensurability should not be seen as "methodologically undesirable" but as a fact of life. Anyone who has taken part in "pointless" political, scientific or religious debates knows that incommensurability is not a problem to be wished away but one needing serious philosophical attention. But any methodology that suggests such antagonists really share a common viewpoint cannot help but make an opponent who disagrees about *everything* seem just plain stupid. It follows that traditional epistemology is inadequate to the problem of incommensurability; for it naturally conflates (Carnap: "external") questions about which standards should govern a subject with ("internal") questions about that subject under a particular standard. This failing suggests that if instead "participants in incommensurable points of view" are alert to the "fundamental" nature of their debate, that both comprehension and rational resolution of their conflict may be possible.

Notes

1. This does not mean no pair of theories could be formalized--i.e. written out in logical notation, but only that such formalization, if achieved, would be misleading because implying that certain of the theories' concepts were absolutely nonlogical.

2. A generalization would be (largely) formal in content, for example, if it functioned (more) as a *definition* or *rule* determining relations among its constituent concepts than as an empirically-testable law. (I have placed "largely" and "more" in parentheses to accommodate Kuhn's belief, discussed in chapter one (see §1.1.1), that even definitions are not fully either logically or empirically neutral.)

3. As we shall see in section 2.2, that scientists strive to make their generalizations come out true does not make their theories empirically incorrigible or tautologically true (see Lodynski 1982, 98). For occasionally "a normal problem...resists the reiterated onslaught of the ablest members of the group within whose competence it falls" (Kuhn 1970a, 5). Such failures of expectation then "lead the profession at last to a new set of commitments" (*ibid.*, 6).

4. For example, two hundred years after Newton published the *Principia*, the physicist Ernst Mach voiced a common (if historically inaccurate) sentiment when he claimed that Newton "completed the formal enunciation of the mechanical principles now generally accepted. Since his time no essentially new principle has been stated" (*Encyclopedia of Phil.* s.v. "Newtonian Mechanics and Mechanical Explanation").

5. Kuhn does not believe, however, that because Newton's *Principia* did not in fact always give a clear picture of every situation that later seemed relevant, that the *Principia* was poorly thought out or that its concepts were, in any realistic sense, imprecise. Just the opposite. He says that "no other work known to the history of science has simultaneously permitted so large an increase in both the scope and precision of research" (Kuhn 1970a, 30).

6. In another paper, "Second Thoughts on Paradigms" (Kuhn 1977c), Kuhn describes Bernoulli's achievement in more detail.

> Determine the descent of the center of gravity of the water in tank and jet during an infinitesimal interval of time. Next imagine that each particle of water afterwards moves separately upward to the maximum height obtainable with the velocity it possessed at the end of the interval of descent. The ascent of the center of gravity of the separate particles must then equal the descent of the center of gravity of the water in tank and jet. From that view of the problem, the long-sought speed of efflux followed at once. (Kuhn 1977a, 306).

While the resemblance of Bernoulli's application to Huyghen's can be immediately recognized--*once Bernoulli has drawn our attention to them as a possible construal*--there seem no obvious "more basic" rules he could have used to get his result; moreover, any of the other attempts he made or could have made before this one in fact succeeded, could also have been compatible with Huyghen's model and could also have proved successful.

7. This is not Kuhn's complete view. Thus he considers paradigms to be only a part of what he calls the total "disciplinary matrix" which includes such other elements as: symbolic generalizations, beliefs in particular models, and shared values (Kuhn 1970a, 181-7). Nevertheless, Kuhn claims that the idea of achieving *concrete problem solutions* by direct resemblance is "the most novel" and "philosophically...deepest" aspect of his view (*ibid.*, 175, 187).

8. Kuhn says here that "...the concrete scientific achievement [is] a fundamental unit...a unit that cannot be fully reduced to logically atomic components which might function in its stead."

9. Also, since certain "professional subspecialties" make different uses of the same paradigms--like physicists and chemists in quantum mechanics--the paradigm "is not the same paradigm for them all" (Kuhn 1970a, 50). Hence, at the point where their applications diverge, those subspecialties could also be said to be incommensurable, though not usually also incompatible.

10. Recall that Kuhn says: "From the viewpoint of this essay [Newtonian and Einsteinian dynamics] are *fundamentally incompatible* (i.e. logically incompatible and incommensurable) in the sense illustrated by the relation of [Bertholletian to Proustian chemistry]: Einstein's theory can be accepted only with the recognition that Newton's was wrong" (Kuhn 1970a, 98).

11. Kuhn's critics often complain that his examples don't show that Einstein and Newton had different *concepts* of mass; instead those examples show merely that they had different *theories* of mass (e.g. Musgrave 1978, p339). As we saw in chapter one, however, Kuhn believes that for such cases one cannot draw a sharp distinction between theoretical and conceptual differences.
here would be a purely theoretical difference only if both theories were: agreed on which things had mass and how to measure that mass; but disagreed on properties of that mass that both recognized as conceptually possible.

12. I include "genuinely" in parentheses to exclude the idea of falsification of "auxilliary hypotheses." This latter idea, motivated by the work of Pierre Duhem, is similar to Kuhn's notions of anomaly in articulation. It differs from (my reconstruction of) Kuhn's position in implying that a theory's auxilliary hypotheses are independent of the theory. But for Kuhn, choosing a theory's articulations help to interpret it.

13. It would be mistaken here to conclude that Kuhn's account confuses the mere *fact* that scientific theories are often very resistant to falsification with *normative*, logical criteria that those theories *should* attempt to meet (e.g. Lakatos 1970, 177; c.f. Kuhn, 1970b, 233, 235-7). For example, it would be a misunderstanding of Kuhn's position to suggest that scientists who behave in the way he describes, *dishonestly* endulge in "conventionalist strategems" (Popper 1968, 37) or "*ad hoc* rescues" (Giere 1984, 163-4) to protect their theories from their *real* consequences. Those rejoinders to Kuhn would be moot only if scientific concepts were (or could be) actually determinate prior to their application to unforseen, but relevant, circumstances. For only thus could logical criteria be identified with norms of scientific reasoning. Since, as we have seen, Kuhn argues that scientific concepts are seldom, if ever, determinate in advance, he rejects the view that logical criteria can serve *that* normative role. Hence, for Kuhn, the response of scientists to anomalous situations often shows that in such cases the application of their paradigms are *not yet determinate*; it does not imply that their application is all too clear, and that, therefore, the scientists in question are lacking in intellectual courage or honesty.

14. Incidentally, just because, as is obvious, the states of crisis Kuhn describes have an irredeemably-*psychological* aspect to them, it should not be inferred that the concept of *crisis* can have no *epistemological* import. Recall that for Kuhn the *process* by which paradigms are articulated and, therefore, by which knowledge is acquired, is ultimately based on the unmediated *judgements* of those paradigm's practitioners. Hence, a breakdown in

that process, is a breakdown in the normal *epistemological* process: hence, such crises are *epistemological crises*.

15. Another possible example of the need to choose among competing articulations of the same paradigm is Newton's particulate and Huyghen's wave theories of light. Thus while the light-particle version of the original Principian-mechanical paradigm to the behaviour of light was perhaps more natural, the more accurate wave theory was also seen as an articulation of the *Principia*. Thomas Young's twin-slit experiments were instrumental in choosing the wave theory over the particle model (Mason 1962, 468-70). Of course, as Kuhn notes, from the more restricted point of view of the predecessor particle-optics (sub) paradigm, wave theory was quite revolutionary (Kuhn 1970a, 86).

16. By "Piaget," I am referring to the famous Swiss child psychologist and "genetic epistemologist," Jean Piaget. I came across this Piagetian thought experiment through my wife and have tried it out several times with young children, always with Piaget's results. My discussion of the Galilean thought experiment is based on that in Kuhn's paper "A Function for Thought Experiments" (Kuhn 1977b, 240-65). There Kuhn cautions that not all those things termed "thought experiments" in the history of science have the same features as the ones to be discussed; still, they are "typical of an important class" (*ibid.*, 241).

17. Piaget's stages of conceptual development are most commonly--though not necessarily--associated with certain age groups.

18. This formulation of the child's criterion is perhaps misleading because it implies a distinction between volume and height that the child himself apparently cannot yet recognize.

19. When my niece was about four years old, she did not yet even *dimly* recognize an anomaly when my wife confronted her with the Piagetian situation. My wife then made several interesting permutations on Piaget's original format: she switched the water for cream soda, put significantly more cream soda in the first of the two similar vessels, and then poured them into two other vessels that had greatly different diameters. In that situation, my puzzled niece claimed both that the narrower vessel had more cream soda in it than the wider one, and that nevertheless she would rather have the cream soda in the wider vessel.

20. Because now restricted to situations where other volumetric factors like cross-sectional area are held fixed.

21. For example, if we restrict our attention to those situations in which the body B (of mass *m*) undergoes an *impulse* (a force having a limited duration) $I = Ft$ (= $mv(t) - mv_0$), and we assume that friction plays no essential role, Newtonian mechanics will yield something like Aristotle's law. That is, in such circumstances, the momentum $p = mv$ will be a constant; so, for example, when $m = 1/2B$, the average velocity will double and B will travel twice the initial distance C in the same time, just as Aristotle claims.

Chapter 3

Proving Kuhnian Incommensurability: A Case Study

3.0 Introduction

Ian Hacking claims Kuhn's concept of incommensurable scientific theories isn't nearly so precise as the original mathematical notion of incommensurable geometrical magnitudes. "[Philosophers like Kuhn] are thinking of comparing scientific theories," he says, "but of course there could be no *exact* measure for that purpose" (Hacking 1983, 67. Italics in original). Nevertheless, in this chapter, I want to use a discussion of the historical discovery of the incommensurability of the side and diameter (or: diagonal) of the square, based on Kuhn's account of so-called "thought experiments," to obtain a more general notion of incommensurability that is quite rigorously provable for successive scientific theories.

While it turns out that proofs of incommensurability between such theories can themselves be formulated quite rigorously, because they ultimately depend upon accepting certain *plausible* articulations as definitive of the old paradigm, conceptual indeterminacy means that the exact *import* of those proofs for that paradigm often turns out to be partly ambiguous. However, since it also turns out that the precise significance of the proof of mathematical incommensurability was similarly open to interpretation, the version for theories need not suffer by comparison.

My strategy in this chapter is to use the analytic concepts discussed in chapter two to portray the Pythagorean discovery of incommensurability as a Kuhnian process of normal and revolutionary science. In particular, I describe the original proof of incommensurability as a thought experiment that directly compares the Pythagorean notion of *arithmos* (roughly: *natural number*) with an articulated notion of geometrical magnitude. It turns out that the successor Euclidean mathematical tradition took this concept of geometrical magnitude to be the more basic notion and treated that of arithmos as its "special case." Hence, because the Pythagorean and Euclidean traditions in effect employed different concepts as their standards, in my articulation of the Kuhnian notion, those traditions are incommensurable.

3.2 The Pythagorean Paradigms

The exact origins of the Pythagorean school of mathematicians are hard to determine with any precision. Pythagoras (ca. 580-500 B.C.), himself, is a somewhat legendary figure who likely learned his first mathematics in visits to Egypt and Babylonia (Boyer 1968, 50). Probably, as is recorded by later authors, when he returned to the Greek dominions, Pythagoras established, in the southeast of Italy, the school of mysticism and mathematics named after him (*ibid.*, 52). Some central beliefs of his school, which we no longer strongly associate with mathematics, were political conservatism; transmigration of souls; vegetarianism; and the shunning of beans or lentils (*ibid.*, 53).

Much later (ca. 384-322 B.C.), Aristotle was to express the fundamental tenet of the Pythagorean school with the generalization, "All things are (or partake of) number" (*Metaphysics* I.5, 985b23ff in Knorr 1975, 22). Of course, as I argued in chapter two, such formulations should often be understood as programmatic paradigms, rather than as already-completed generalizations (sec. 2.1.1). Thus to understand the content of that Pythagorean law, it is necessary to see in which way it was originally applied, and how those paradigms came to be further articulated, a process that eventually culminated in the thought experiment which pointed the way to the new, Euclidean method of doing mathematics.[1]

To grasp the full significance of that thought experiment, however, it is important to realize that the Pythagoreans meant entirely by "number" (Greek: *arithmos*) what we should now consider to be merely natural numbers or positive integers; in particular, it is critical to note that the Pythagoreans had no notion of rational number, only that of ratios or pairs of integers. In fact, modern distinctions like rational, algebraic, irrational and real number only arose as a (*much* later) consequence of the proof of incommensurability (Knorr 1975, 9).[2] In essence, what the Pythagorean paradigm expressed was the belief that the true natures of all things--whether practical, theoretical or geometrical--could be captured entirely in terms of the properties of natural numbers or their ratios (Boyer 1968, 79). It seems that the Pythagoreans were originally greatly impressed by such facts as that the apparently *qualitive* musical harmonies can be completely expressed with sequences of pairs of integers; and that the relative lengths of the sides of many right triangles can be captured entirely by such triples of integers as 3 : 4 : 5 and 5 : 12 : 13 (Popper 1968, 76; see also von Fritz 1945, 245-6).

One can get a perhaps deeper insight into the Pythagoreans' world view by noting that, for them, an even more fundamental category than number was the distinction between odd and even. Thus Aristotle says that the Pythagoreans took the "elements of number to be the elements of all things...and the elements of number to be the odd and the even" (Knorr 1975, 134; Aristotle, *Metaphysics* 986a 17). In fact, one group of Pythagoreans evinced that basic distinction in the following so-called "Table of Contraries":

ONE	MANY
ODD	EVEN
MALE	FEMALE
REST (BEING)	CHANGE (BECOMING)
SQUARE	OBLONG
STRAIGHT	CROOKED
RIGHT	LEFT
LIGHT	DARKNESS
GOOD	BAD

(from Popper 1968, 78)

The Pythagorean belief that number is the ground of all individuation resulted in their describing numerically even what we should now regard as nonmetrical intangibles: masculinity, femininity, marriage, harmony, justice, creation, etc. For example, they claimed that

> ...the number one is the generator of numbers and [thus] the number of reason [Greek: *ratio*]; the number two is the first even or female number, the number of opinion; three is the first true male number, the number of harmony, being composed of unity and diversity; four is the number of justice or retribution, indicating the squaring of accounts; five is the number of marriage, the union of the first true male and female numbers; and six is the number of creation (Boyer 1968, 57).

As I noted in chapter two, Kuhn claims that often at the beginning of a new paradigm programme, its adherents are aware of certain situations that are apparently anomalous for that paradigm. In other words, they *dimly feel* that such situations do not have quite the characteristics their paradigm would lead them to expect. Many times, it is true, in the course of normal science, such anomalies prove to be resolvable with expanding resources. But with the benefit of historical hindsight, we can say that, for the Pythagoreans, the felt anomaly that eventually proved critical was "the computational anomaly posed by such quantities as $\sqrt{2}$" (Knorr 1975, 4). In fact, many previous mathematical traditions had also recognized that such quantities could *seemingly* only be represented as a successive series of approximating ratios, and not, for example, by a single pair of integers (*ibid.*, 4).[3]

3.3 Dot-Diagramme Articulations of the Pythagorean Paradigm

While the Pythagoreans believed that the essence of all things were numbers and their ratios, this did not mean that all they studied directly was arithmetic. In fact, they did much geometry as well. But always, geometrical problems were studied as "analytic instrument[s]" for their more fundamental aim of extending the application of their numerical paradigm (Knorr 1975, 132). The Pythagoreans were able to hook up their number theory with geometry using a pebble (Greek: *psephos*) or dot-diagramme method of representation (*ibid.*, 135ff; Popper 1968, 76-9).

For the Pythagoreans and other ancients, the notion of the unit was "point without position" (Knorr 1975, 136). It follows on analogy with that picture, therefore, that one *might* introduce numbers into figures by giving them a "position." Now the Greeks had long used pebbles (*psephoi*, Latin: *calculi*) for such various activities as playing board games and counting votes. In fact, Plato often drew analogies between playing those games and doing mathematics (*ibid.*, 137; Plato *Gorgias* 450CD; *Laws* 819D-820D). And the best evidence is that the Pythagoreans also worked out their arithmetic theorems with the help of pebbles to give their units position. Knorr argues, based on Euclidean definitions, that the Pythagoreans represented even numbers by a row of pebbles separated by a larger space; and odd numbers as even numbers with a single pebble in that space.

For example

o--o--o--o--o--o / o--o--o--o--o--o

Fig. 3

represents twelve (where the "/" marks the larger space between the pebbles, "o"). The odd number thirteen is therefore represented in this notation as follows (Knorr 1975, 140).

o--o--o--o--o--o / o / o--o--o--o--o--o

Fig. 4

Knorr illustrates how it is possible with this primitive notation to prove many arithmetical theorems; for example, that the sum of any number of even integers is also even; and that the square of an even integer is even (*ibid.*, 140-2).[4] The Pythagoreans also found that they could represent geometrical shapes and figures with pebble-numbers. These are the so-called "figured numbers," the most basic of which are the *triangular*, *square*, and *oblong* or *heteromecic numbers*. The following figure illustrates triangular numbers.

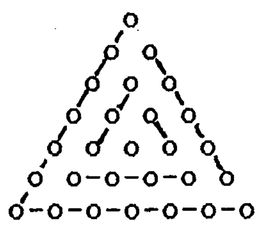

Fig. 5
The seventh triangular number, 28, as represented by the neo-Pythagorean, Iamblichus (*ibid.*, 146)

In Iamblichus' way of ordering this figure's constituent numbers, we begin with a single stone at the centre, and then build up the figure by adding around that stone, first two stones, then three, then four, and so on until we reach the seventh step, and add seven stones. Thus the triangular numbers are 3, 6, 10, 15, 21, 28, and so on. The figured number that one must add in each case to get the next triangular number is called its *gnomon* (Knorr 1975, 143).[5] For the triangle, these gnomons are therefore 2, 3, 4, 5, 6, 7, ...

The following figure is a representation of the fifth square number, twenty-five.

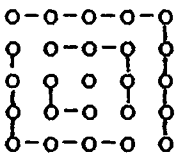

Fig. 6

The series of square numbers are thus (as we might expect): 4, 9, 16, 25, ... and the gnomons are 3, 5, 7, 9, ... The third main type of figured number is the *oblong* or *heteromecic* number whose representation is indicated by the following diagramme of the fourth such number, thirty.

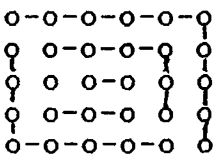

Fig. 7

The series of oblong numbers are thus: 6, 12, 20, 30, ... and the gnomons are 6, 8, 10, ...

By comparing the triangular with the square and oblong numbers, one can see (allowing the unit to be a number) that square numbers are the sum of consecutive triangular numbers, and that oblong numbers are the double of triangular numbers. One can also portray these results in pebble notation (Knorr 1975, 150. Note, this is not Iamblichus' pattern, but an alternative one given by Nichomachus).

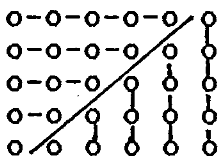

Fig. 8

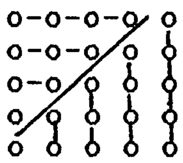

Fig. 9

There is a certain symmetry to the representation of geometrical figures with numbers. Thus, if numbers can be used to represent geometrical magnitudes, then, symmetrically, those magnitudes may also be used to represent numbers. So, for example, one can postulate minimal, indivisible line segments, squares, or cubes to correspond to the integral unit. Now, if we look at figure 6, we notice that the rectangular figure delineated by those dots also divides into two equal triangles, when a diameter line is drawn. On the other hand, as is revealed by figure 7, while the pebble-methodology does show that the square is composed of two triangles, because they are successive, their triangular *numbers* are different. By contrast, the *geometric* square divides along the diameter into two *equal* (isoceles) triangular *figures*. Interestingly, with respect to the discussion in chapter two of articulation and family resemblance, the Pythagoreans were aware of this "disparit[y], but [sought] in their presentations *to exploit the likenesses, rather than the differences*" (Knorr 1975, 174. Italics mine).

3.4 Rules for Finding Pythagorean Triples

Knorr also shows how it is possible to capture many other aspects of geometrical figures in this pebble-methodology, including a formula for generating an infinite number of the so-called *Pythagorean triples*. As mentioned above (p4), the fact that triples of integers could completely represent the relative lengths of the parts of some right triangles had deeply impressed the early Pythagoreans. Knorr uses their pebble-methodology to characterize the properties of such triples in terms of the fundamental categories odd and even (*ibid.*, 158-60). The following are the most relevant theorems. Note here that "C" names the largest number, hence that of the hypotenuse, and "A" and "B" name leg numbers.

> I. Given the Pythagorean triple A, B, C, if two of the terms are even, the third is also even (Knorr 1975, 158).
>
> II. If in a given Pythagorean triple...C is even, then all three terms are even (*ibid.*, 158).
>
> III. Given a Pythagorean triple *A*, *B*, *C*, if one of the terms is odd, then...*C* is odd, and of the numbers A and *B*, one is odd and the other even (*ibid.*, 159).

As mentioned above (p9), the fact that integral numbers and their ratios could represent geometrical figures, meant that those represented figures could also stand for numbers. Of course, the Pythagoreans believed that this symmetry was only partial because numbers could also be used to represent other things than geometrical figures. Nevertheless, since these geometrical figures could go proxy for numbers in geometrical problems, cases in which a general geometrical pattern was discovered could be used to set a problem for the arithmetic paradigm.

3.5 An Arithmetic and Geometric Version of Pythagoras' Theorem

Knorr shows how an articulation of the pebble-methodology to square and oblong figures could lead first to a general arithmetic formula relating Pythagorean triples of integers, and then to a still more general geometric formula relating the sides of right triangles to their hypotenuses (Knorr 1975, 175-7). These formulas express, arithmetically and geometrically, the famous Pythagorean theorem on right triangles.

Here is the arithmetic form of the theorem Knorr reconstructs.

> Theorem 1. Given a right triangle of which the sides about the right angle represent the numbers A and B, and the hypotenuse represents the number C, then the square of C equals the sum of the squares of A and of B. That is, the three numbers, A, B, C form a Pythagorean number triple.

The following is the geometric form of Theorem 1.

> Theorem [2]. Given a right triangle, the square on the hypotenuse equals the sum of the square on the sides about the right angle.

It is just a few short steps from these theorems, together with theorems I, II and III above on the properties of Pythagorean triples (), to the *reductio* proof *cum* thought experiment called the proof of the incommensurability of the side with the diameter of a square.[6]

The purely geometrical statement of Pythagoras' Theorem was clearly obtained, on Knorr's reconstruction, by extension of the numerical paradigm--in particular, via the pebble articulations of that paradigm. Therefore, the symmetry of the situation would have suggested to a Pythagorean that it should be possible to find triples of integers for any right triangle defined by the geometrical formula. In other words, a Pythagorean's well-founded, "paradigm-induced expectations" (Kuhn 1970a, 53) would lead him to search for triples for such ratios as had previously proved elusive: for example, the ratio of the diameter to the side of a square (Knorr 1975, 179). That search would quickly be thwarted as the following proof shows.

3.6 Thought Experiment: A Proof of Incommensurability

> Let us suppose that numbers characterize both the legs (sides), A and B, and the hypotenuse (diameter), C, of the isoceles right triangle (square). Obviously, the two legs (sides) are equal so A = B; moreover, C is clearly greater than A and B, but less than their double. Is C odd or even?
>
> (i) Suppose that C is even. By II above (p10), so are A and B. But if we divide each side in half with a perpendicular line, we obtain another isoceles right triangle whose legs and hypotenuse are in the same (geometrically-apparent) ratio. Hence, the new hypotenuse, C' and the new legs, A' and B' will also be even and we have an infinite regress. However, since, by hypothesis, C is integral, it cannot be divided *ad infinitum*. Thus C must be odd.
>
> (ii) Suppose that C is odd. By III above (p10), either A is even and B is odd, or *vice versa*. But by hypothesis, A = B; so the leg numbers must be both odd and even, which is a contradiction. On the other hand, if we claim that A and B are the unit, which is sometimes thought to be both odd and even, because C is clearly greater than either A or B, but less than their double, C is not integral. In other words, C is not a number (adapted from *ibid*., 179-80).

What conclusions can be drawn from this *reductio*-thought experiment? The most obvious result is that it is impossible to assign integers simultaneously to the side and diameter of a square (or to the side and hypotenuse of a right isoceles triangle). Since, for the Pythagoreans, however, integral numbers were supposed to be the measure of all things, what the proof shows is that it is in fact impossible to measure certain geometrical relations by a ratio of integers. But since each integer has the unit as a common factor, this also means that the side and diameter of the square have no common (numerical) measure; that is, they are (numerically) *incommensurable*.

Less obviously, perhaps, what the proof shows is that we must distinguish between the concepts of integral number (*arithmos*) and geometrical magnitude (c.f. Knorr, 24). In fact, in the proof, it is the hypothesis that these concepts are coextensive that leads to the contradiction. More to the point, the proof of mathematical incommensurability shows that we must bifurcate the original concept of *arithmos* into (atomic) integer and (continuous) geometrical magnitude. Later Greek mathematicians such as Eudoxus and Euclid took the mathematical proof to show that the concept of geometrical magnitude is more fundamental than that of *arithmos* (Knorr 1975, 170-1).[7]

3.7 Conclusion: Kuhnian and Mathematical Incommensurability

I claimed in the introduction that it is possible to draw a close enough parallel between Kuhn's notion of incommensurability and the mathematical one to obtain a (more) rigorous method of applying Kuhns's concept. A rough criterion Kuhn offers for taking two (Kuhnian) incommensurable paradigms to be incompatible may help in that task. Kuhn says: "Obviously, then, there must be a conflict between the paradigm that discloses anomaly and the one that later renders the anomaly lawlike" (Kuhn 1970a, 97). Thus, on this proffered criterion, the Pythagorean arithmetical paradigm which disclosed the anomaly of (numerical) incommensurability is (Kuhnian) incommensurable with the Euclidean geometrical paradigm that "render[ed] the anomaly lawlike." But the Pythagorean paradigm disclosed that anomaly in the most explicit way--that is, transformed its "felt anomaly into concrete contradiction"--with the thought experiment that was the proof of (numerical) incommensurability. Moreover, since that anomaly was really an anomaly *relative to* the Pythagorean standard of *arithmos*, but not for the later Euclidean standard of geometrical magnitude, that proof showed, more generally, that these two paradigms were Kuhnian incommensurable.

In summary, therefore, we may say thought experiments which prove that what is anomalous in one system of concepts is lawlike in another, also *prove* those conceptual systems to be Kuhnian incommensurable. Thus my proof in chapter one (§1.7) that the concept of comparison must be bifurcated into two concepts, direct comparison and commensuration, also shows that the traditional view, which identifies those concepts, is incommensurable with Kuhn's view, which distinguishes them.

The concept of a thought experiment or proof of incommensurability connects up with the notions of direct comparison and contrast as follows. The thought experiment shows directly that the attempt to express within one system of concepts relations that are expressible (lawlike) in another is conceptually anomalous--results in concrete contradiction--in that first system. For example, the attempt to express as an integer ratio, the relative magnitudes of the side and diameter of a square--an obviously intelligible geometric notion (von Fritz 1945, 261), and one which seems directly similar to arithmetic ratios, conflicts with certain properties of the integers. Similarly, the attempt to express within the traditional view Kuhn's intuitively intelligible concept of fundamental incompatibility, a notion that seems directly similar to formal incompatibility, conflicts with certain properties of formal logical systems.

Notes

1. The thought experiment/proof of incommensurability that I shall use in this chapter is W. B. Knorr's historical reconstruction. Thus Knorr argues that the more familiar proof from Aristotle (*Prior Analytics* I.23, 41a29) and Euclid (X, 117) contains elements that show it to be a much later development. His reconstruction is based, in part, on a passage in Plato's *Meno* (82B-85B) and on the Pythagorean "theory of figured numbers." (Knorr 1975, 22-7).

2. Another point that will prove relevant to the (reconstructed) proof of incommensurability is that the Pythagoreans often did not regard, or were unsure whether to regard, the unit as itself a number; and they also often considered it to be either both odd and even or neither (e.g. Knorr 1975, 146).

3. E.g. 1 : 1; 3 : 2; 7 : 5; and so on. This sequence is generated by assigning, as a first approximation, the number one to both the side and diameter (diagonal) of the square. The side number of the next approximation is obtained from the sum of previous one's side and diameter numbers; while the next diameter number is the sum of the previous approximation's diameter number and twice its side number; and so on (Knorr 1975, 16). While this (inductive) sequence of ratios seems to approach a limit, it is also, as it stands, clearly nonterminating.

4. For instance, one can show that the sum of any number of even integers--any integer times an even integer--is also even with the pebble method as follows.

Let *A*, *B*, *C*, *D*, be even integers portrayed by the arbitrarily chosen patterns below:

```
A        o--o--o--o   o--o--o--o
B              o--o   o--o
C     o--o--o--o--o   o--o--o--o--o
D           o--o--o   o--o--o
```

The sum of these integers, $E = A + B + C + D$, can now clearly be given by adding the left and right parts of each integer, respectively. Since equals were added to equals on each side, the resultant, *E*, will be representable by a single row of pebbles separated by a space with an equal number of pebbles on each side; that is, it will be even integer (based on Knorr 1975, 141).

5. Interestingly, the word "gnomon" derives from the Greek word for the part of a sundial that casts a shadow showing the time; thus the pebble-arithmetic *gnomon* is an "indicator" of the next number in its sequence (COED s.v. "gnomon").

6. Interestingly, Aristotle illustrates reasoning by *reductio ad absurdum* with a version of the proof of incommensurability (*Prior Analytics* I.23, 41a29). As we have seen in chapter two, what Kuhn terms "thought experiments" are also often *reductios*.

7. Interestingly, despite common perceptions, the Pythagorean school did not wither away and die immediately after the discovery of incommensurability. Many, such as Eurytus, continued their work with integer representations, though with certain qualifications. For example, Eurytus toned down the Pythagorean belief in number by holding only that points could represent the shapes of anything, not all continuous magnitudes (Knorr 1975, 45). Another less pragmatic Pythagorean, Lysis, claimed instead that "god is an irrational number" (*ibid.*, 47).

BIBLIOGRAPHY

Arbib, M and Hesse, M.
(1986) *The Construction of Reality*. Cambridge: Cambridge University Press.

Benacerraf, P & H. Putnam
(1983) *Philosophy of Mathematics: Selected Readings*. 2d ed. Cambridge: Cambridge University Press.

Bernays, Paul
(1983) "On platonism in mathematics." In Benacerraf and Putnam (1983): 258-71.

Boolos, G. S. & Jeffrey, R. C.
(1980) *Computability and Logic*. 2d ed. Cambridge: Cambridge University Press.

Boyer, Carl B.
(1985) *A History of Mathematics*. Princeton: Princeton University Press.

Brown, H. I.
(1976) "Reduction and Scientific Revolutions." *Erkenntnis* 10: 381-85.

Carnap, Rudolph
(1966) *An Introduction to the Philosophy of Science*. ed. Martin Gardner. New York: Basic Books.

Collier, John
(1984) "Progress in Scientific Revolutions: The Problem of Semantic Incommensurability." Ph.D Thesis. University of Western Ontario.

Courant, R. & H. Robbins
(1957) *What is Mathematics?* New York: Oxford University Press.

Davidson, Donald
(1980a) *Essays on Actions and Events*. Oxford: Oxford University Press.

(1980b) "Philosophy of Psychology: Reply to Solomon." In Davidson (1980a): 243-4.

(1984a) *Inquiries into Truth and Interpretation*. Oxford: Oxford University Press.

(1984b) "On the Very Idea of a Conceptual Scheme." In Davidson (1984a): 183-98.

Dilworth, Craig
(1986) *Scientific Progress*. 2d. Dordrecht: D. Reidl.

Einstein, Albert
(1961) *Relativity:* The Special and General Theory. 16th ed. transl. R. W. Lawson. New York: Crown Publishers.

Feyerabend, Paul
(1975) *Against Method*. London: Humanities Press.

(1987) "Putnam on Incommensurability." *Brit. J. Phil. Sci.* (March): 75-81.

Giere, R.
(1983) "Testing Theoretical Hypotheses." In *Minnesota Studies in the Philosophy of Science*, vol. X. ed. John Earman: 269-298. Minneapolis: University of Minnesota Press.

(1984) *Understanding Scientific Reasoning*. 2d ed. New York: Holt, Reinhart and Winston.

Goodman, Nelson
(1984) *Of Mind and Other Matters*. Cambridge: Harvard University Press.

Hacking, Ian
(1981) *Scientific Revolutions*. Oxford: Oxford University Press.

(1983) *Representing and Intervening*. Cambridge: Cambridge University Press.

Knorr, W. B.
(1975) *The Evolution of the Euclidean Elements:* A Study of the Theory of Incommensurable Magnitudes and Its Significance for Greek Geometry. Dordrecht: D. Reidl.

Kuhn, T.S.
(1970a) *The Structure of Scientific Revolutions*. 2d ed. Chicago: University of Chicago Press.

(1970b) "Reflections on my Critics." In Lakatos and Musgrave (1970): 231-78.

(1976) "Theory-Change as Structure-Change: Comments on the Sneed Formalism." *Erkenntnis* 10: 179-99.

(1977a) *The Essential Tension*. Chicago: The University of Chicago Press.

(1977b) "A Function for Thought Experiments." In Kuhn (1977): 240-65.

(1977c) "Second Thoughts on Paradigms." In Kuhn (1977): 293-319.

(1977d) "Logic of Discovery or Psychology of Research?" In Kuhn (1977): 266-92. Reprinted from Lakatos & Musgrave (1970): 1-23.

Kuhn, T. S.
(1983) "Rationality and Theory Choice." *Journal of Philosophy* 80: 563-70.

Lakatos, I. and Musgrave, A.
(1970) *Criticism and the Growth of Knowledge*. Cambridge: Cambridge University Press.

Lakatos, Imre
(1970) "Falsification and the Methodology of Scientific Research Programmes." In Lakatos and Musgrave: 91-196.

Lodynski, Andrzej
(1982) "Some Remarks in Defense of the Incommensurability Thesis." In *Polish Essays in the Philosophy of the Natural Sciences*. 68 (ed.) Wladyslaw Krajewski: 91-102. Dordrecht: D. Reidl.

Mates, B
(1972) *Elementary Logic*. 2d ed. New York: Oxford University Press.

Meldrum, A.N.
(1910) "The Development of the Atomic Theory: (1) Berthollet's Doctrine of Variable Proportions." in *Manchester Memoirs*, LIV: 1-16.

Moberg, Dale
(1979) "Are There Rival Incommensurable Theories?" *Philosophy of Science*. 46: 244-62.

Musgrave, Alan
(1978) "How to Avoid Incommensurability." *Acta Philosophica Fennica* 30: 336-46.

(1982) "Kuhn's Second Thoughts." In *Paradigms and Revolutions*, ed. Gary Gutting: 39-53. Notre Dame: University of Notre Dame Press.

Newton-Smith, W.H.
(1981) *The Rationality of Science*. London: Routledge & Kegan Paul.

Partington, J.R.
(1957) *A Short History of Chemistry*. 3d ed. London: McMillan and Co.

Popper, Karl
(1968) *The Logic of Scientific Discovery*. Reprint. New York: Harper & Row.

(1963) *Conjectures and Refutations*. Reprint. New York: Harper & Row.

Putnam, Hilary
(1981a) *Reason, Truth, and History*. Cambridge: Cambridge University Press.

(1981b) "The 'Corroboration' of Theories." In Hacking (1981): 60-79.

Quine, W. V. O.,
(1960) *Word and Object*. Cambridge, Mass: M.I.T. Press.

Shapere, Dudley
(1981) "Meaning and Scientific Change." In Hacking (1981): 27-59.

(1980) "The Structure of Scientific Revolutions." In *Paradigms and Revolutions*. ed. Gary Gutting: 27-38. Notre Dame: University of Notre Dame Press.

Scheffler, Israel
(1967) *Science and Subjectivity*. Indianapolis: Bobbs-Merrill.

Suppes, P
(1957) *Introduction to Logic*. New York: Van Nostrand Reinhold.

Swoyer, Chris
(1982) "True For." In *Relativism: Cognitive and Moral*. eds. J. W. Meiland and M. Krausz: 81-108. Notre Dame. University of Notre Dame Press.

Thomas, S. N.
(1986) *Practical Reasoning in Natural Language*. 3d ed. New Jersey: Prentice-Hall.

Von Fritz, Kurt
(1945) "The Discovery of Incommensurability by Hippasus of Metapontum." in *Annals of Mathematics* (2), 46: 242-64.

Wartofsky, Marx W.
(1977) *Feuerbach*. Cambridge: Cambridge University Press.

Wittgenstein, Ludwig
(1968) *Philosophical Investigations*. 3d ed. transl. G. E. M. Anscombe. New York: McMillan.