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INCOMMENSURABILITY : KINDS AND CAUSES

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The topic of incommensurability has attracted as much attention and emotion as any in *The Structure of Scientific Revolutions*, but the term appears only nine times and is never elaborated upon in the original text. Very recently Kuhn (1983) has turned his attention to the controversy over the concept as the main topic of an entire article. Nonetheless, much remains, in my opinion, to be said about the topic because his recent article is largely devoted to defenses against misunderstandings and misinterpretations and only obliquely addresses the underlying issues about the nature of incommensurability and its sources. I shall argue that there are numerous distinct kinds of incommensurability, as well as distinguishable degrees thereof. A thorough analysis must begin by reviewing the structure of scientific thought according to the Kuhnian model in his later work.

Incommensurability : From paradigms to disciplinary matrices

Any attempt to understand incommensurability confronts the major problem that incommensurability is most naturally defined as a relation between paradigms. In *The Structure of Scientific Revolutions*, the concept of a paradigm plays a central, but unclear role. The term "paradigm" appears in only one chapter heading, Chapter V. "The Priority of Paradigms," but the crucial terms in the other chapter headings are all defined in relation to paradigms. A scientific revolution is defined as a change of paradigm; normal science as a period of scientific development guided by a single paradigm; anomalies are those unsolved puzzles which lead to revolution. A scientific community is defined by the paradigm that its members share, and, the meaning of scientific vocabulary changes when a paradigm changes.

Thus, when the clarification of the concept of paradigm is presented in the postscript of the second edition one is faced with the problem of rewriting the book with suitable changes and distinctions. (It should be mentioned that the clarification in the postscript is not entirely *de novo*, a point to which we shall return shortly). The simplest alteration would be to everywhere replace the old term "paradigm" with whichever of the new terms is relevant in that context. If one were to attempt a brief characterization of which items are incommensurable, in SSR it appears that "paradigms" are the best candidate, both conceptually and in view of the fact that most of the nine occurences of "incommensurable" concern paradigms. Thus the task of sorting out kinds of incommensurabilities will not be a simple one. Let us begin by reviewing the new terminology that Kuhn deploys in place of "paradigm".

The unclarity of "paradigm" arose from the fact that the term was used both to apply to what is now to be called a disciplinary matrix and to one specific portion of a disciplinary matrix, exemplars. We will review the elements of a disciplinary matrix leaving exemplars for last because of their special character in relating the other elements.

The first component of a disciplinary matrix is the category of *symbolic generalizations*. While typical examples such as F=ma are obvious from the history of physics and those such as S→R are obvious from one branch of psychology, there remain important problems in determining whether such a class of generalizations exists in the social and biological sciences. Kuhn clearly intends to include not only those generalizations which are actually in symbolic form but also those which play the same role even if verbally formulated, such as "elements combine in constant proportion by weight" (183). Since equations and other symbolic generalizations typically serve to define technical terms as well as to express their relations, a difference in symbolic generalizations will often mean a difference in definitions, and thus in meaning. Thus the first of our types of incommensurability has to do with meanings.

The second component consists of more general and vague principles often labelled *metaphysical assumptions.*

> "I have in mind shared commitments to such beliefs as : heat is the kinetic energy of the constituent parts of the bodies; all perceptible phenomena are due to the interaction of qualitatively neutral atoms in the void... (184)"

Some other examples of metaphysical commitments would be the pre-Newtonian assumption that all forces act by contact, post-Newtonian commitment to an infinite Euclidean space, and the pre-quantum commitment to the fundamental character of continuous and deterministic processes. Metaphysical commitments are often implicit and are subjects of discussion only after they are called into question. When they are formulated prior to being questioned they have often been thought to represent a priori or necessary features of the world. The famous French, i.e., Cartesian, resistance to Newtonian mechanics is one of the famous examples of a gulf that is best described as an incommensurability of metaphysics. Debates over natural motion and the place of the earth in the universe are two others.

The third component consists of *models*. Examples are the assumption that "...the electric circuit may be regarded as a steady-state hydrodynamic system; the molecules of a gas behave like tiny billard balls in random motion" (184). There are two differences between metaphysical commitments and models : first, models are restricted in applicability to particular types of objects or processes; and, second, models are regarded as analogical, that is, they serve the function of suggesting measurements, connections with observable phenomena, mathematical techniques and the like but are not thought to provide completely accurate portrayals of the phenomena. Models are a guide in developing the explicit theory whereas the metaphysical commitments are a constraint on the type of theory which is deemed acceptable.

It is often difficult to distinguish metaphysical assumptions from models, but perhaps the existence of the aether is an example of metaphysics that became a model. One suspects that the aether begin life as a result of the abhorrence of the void, but subsequently became the quasi-mechanical medium for electromagnetic radiation. Lorentz and Einstein propounded the same equation to explain the negative outcome of the Michelson-Morley experiments, but they interpret them very differently because of their different models — a third kind of incommensurability.

The fourth component of the disciplinary matrix is the set of *values* held by scientists. "Probably the most deeply held values concern predictions : they should be accurate; quantitative predictions are preferable to qualitative ones..." (185). Further values concern the simplicity and generality of theories, their compatibility with other current theories, the degree of explicitness of formulation of a theory and the relative ease of reproducibility of results. It should be noted that the label "values" is chosen both because of the shared character of these norms and because of their lack of determinateness in cases of conflict. A commitment to precision is not a commitment to a single standard of precision in all sciences at all times — the commitment is rather to increasing the current degree of precision in a field. And the entire value of precision must be weighed against other values if one has a conflict between a less precise but more general theory and a precise but limited theory. Even scientists in the same field who share the goal of precision may well disagree as to what predictions one should be concerned about making precisely. This is well-evidenced by the early history of atomic theory in chemistry. Thus the fourth kind of incommensurability arises from differences in values.

Furthermore, cases of conflict can arise between the various elements of a disciplinary matrix. For example, Newton's gravitational theory provided fairly precise predictions and was valued on that account but was still resisted by many physicists who felt that it conflicted with fundamental assumptions about the nature of forces and hence did not provide a truly satisfactory explanation.

Another component of the disciplinary matrix which is included in the discussion of paradigms in Chapter IV (40–41) but which is neglected in the postscript is *instruments.* Frequently the kinds of radical changes in perception of the world, in the problems to be solved, in the solutions thought to be acceptable, are associated with changes in the disciplinary matrix associated with the introduction of new instruments.

The most important and novel constituent of the disciplinary matrix is *exemplars. Exemplars are "shared examples"* that illustrate and direct the work of research scientists. The most prominent exemplar from the history of science is probably Newton's derivation of the law of falling bodies and Kepler's laws from the law of universal gravitation and his other principles. Other scientists, including but not restricted to physicists, were inspired in the development of their theories by the conception that they were emulating Newton.

As Kuhn remarks, "The paradigm as shared example is the central element of what I now take to be the most novel and least understood aspect of this book". (187) I believe that considerably more analysis and illustration will be required before it is adequately understood, for there are important unclarities about the concept of an exemplar even as elaborated in the postscript. "Exemplar" is often used to refer to the particular types of solutions to problems that a scientist is exposed to in the process of training or in their experience in the field. In other cases it is used rather for those occasional high points of scientific achievement that influence scientists in many fields. Another variation is that sometimes exemplars are thought of as fully specific actual instances of laboratory investigation. However, only a small number of scientists will have access to the exemplars with any degree of concreteness. Those who learn about a piece of research from a journal will be presented with a description of a type of experiment with many details omitted, and may take away from that description some subset of properties that they believe to be the relevant ones in the description.

The distinction between the specific achievement and its descriptions is of fundamental importance and is easily overlooked. The point can be emphasized by noting that as time passes new scientists are exposed to the original research, not through observation of the experiment or even through reading of original sources, but through textbook descriptions. As Kuhn has noted, textbook presentations frequently misrepresent the historical facts.

The centrality and importance of exemplars derives from their role in binding together the other elements of the disciplinary matrix in a way that provides specific indications of how research can proceed. The exemplar as a relatively specific experiment illustrates how the symbolic generalizations and the heuristic considerations of the model can be applied to the world through particular operations and observations. In providing examples of acceptable scientific work to be emulated they also give more content to the values as they are to be applied to the field. What counts as sufficiently precise predictions, reasonable idealizations, and sufficient reproducibility of results are all projected from the exemplars. Improvement in rigor in a field results when one can improve on any of these dimensions in future work and thus the exemplars implicitly set the standards and goals of the field.

One of the deep and controversial issues surrounding Kuhn's work is whether the implicit guidance of exemplars can or should be rationally reconstructed in terms of rules. I will address this question in the next section although until we know more about the nature and role of exemplars any such discussion must remain somewhat tentative.

The Scientific Group

One of the basic functions of the exemplar is to define a direction of research and thus exemplars play the central role in characterizing normal science and the scientific group. The term "scientific group" is used to cover a range of different sizes and types of groups and thus is more readily understood in our new framework, though the variability was already recognized in SSR, (45—50). If we take seriously the point that a scientific community is a group that shares (part of) a disciplinary matrix, then we find communities of different kinds depending on how much we demand that a group share.

To begin with, the values enumerated earlier would be shared by all (or nearly all) contemporary scientists, so the entire scientific community in the broadest sense is the group that shares this portion of the disciplinary matrix. If one considers further the groups that share symbolic generalizations and metaphysical assumptions, we would isolate groups roughly the size of disciplines. That is, physicists share many more metaphysical assumptions and symbolic generalizations with each other than with chemists as a group, and there is far more similarity among psychologists, or perhaps among schools of psychologists, than they share with other disciplines. When we press further and identify groups which share a large number of symbolic generalizations and instruments and exemplars, as well as the more general values and metaphysical assumptions, we begin to approach the level of specificity of scientific research areas. As we identify increasingly smaller sub-groups within disciplines, we should find that they share more and more of the elements of the disciplinary matrix and that they have been socialized into their craft by being exposed to an increasingly large number of common exemplars. Superficial reading of Kuhn (1970) often leads to the interpretation that exemplars are only those instances of scientific work which have a direct impact on a large portion of research, but this is incorrect. Each subgroup will have its own particular exemplars in addition to those that they share with larger groups.

The recognition that scientific communities can be discerned at many different levels has important implications for our understanding of many of the other key terms. A revolution was defined as a change of paradigm, but this must now be replaced by a definition in terms of a change of all or part of a disciplinary matrix. One immediate question is whether the change requires a total or only a partial change in the matrix. The answer might be that a change *is revolutionary for group G* if it involves a change in the *shared elements* of the disciplinary matrix. Thus a change which is revolutionary for a small group, e.g., solid state physicists, will not generally be a revolutionary change for the community of scientists. Thus whenever we speak of a change being revolutionary we must relativize the claim to a particular level of analysis of scientific groups. It is only when a change involves the most general and widely shared elements of the disciplinary matrix one can speak of a revolution *tout court*. In fact, I would suggest that the terminology be changed and that "revolution" be reserved for very large scale changes. Another term such as "reconceptualization" would be more appropriate for the types of change under discussion. Thus the main contrast would be between what was a reconceptualizing change as opposed to a normal change for a scientific community. Revolutions would be large scale reconceptualizations.

A consequence of this point is that many of the claims about the incommensurability and change of world view that attend revolutionary changes are drastically overstated when applied to smaller reconceptualizations. Changes that involve simply adopting a new instrument or giving up a previously accepted exemplar do not always produce conceptual changes that are unsettling to the general scientific community or which provoke difficulties of communication outside a narrow subfield. But when metaphysical assumptions are questioned or basic values criticized, then one finds the rhetoric of revolutions appropriate. This should not blind us to the fact, however, that the kind of change we are labelling a *reconceptualization* is distinct in kind from the continuous changes which represent normal science at that level.

I believe that Kuhn saw clearly the importance of the phenomena of "changes of vision" for science, including the relatively smaller changes of perspective that would be considered aspects of normal science for larger groups. But the most effective way to make the point that such non-volitional changes in understanding are important is to make the point with regard to the larger and more dramatic changes such as the Copernican revolution. Thus the focus of the book is on the large scale revolutionary changes. One unfortunate consequence of this strategy is that most readers of the book have become fascinated with these issues and have ignored the equally important but less dramatic changes that frequently occur on a smaller scale and which constitute most of scientific development.

The most significant aspect of revolutions is that they involve the kind of change that Kuhn has likened to Gestalt switches. Just as the same line drawing can be seen as a duck or rabbit, as ascending or descending stairs, so a particular experimental outcome can be seen as demonstrating that the earth is stationary (given impetus theory) or the law of inertia (given that the earth is moving). Some revolutions are tied to large scale controversies that are difficult to resolve, that cause social upheaval that have social and religious implications, that are resisted by eminent scientists to their death. This results from a change in conceptualization if the field involved is fundamental or the phenomena in question are basic to general views of the world. But the more extravagant social consequences of revolutions occur only in rare cases. The same kinds of intellectual difficulties in evaluating the changes and in assimilating it after the fact are also found in smaller scale cases, but they fail to make headlines or to attract general attention from scientists let alone from non-scientists. For example, the discovery of the double helical structure of DNA required conceptual reorientation for those making or using the discovery, but not for the larger field.

Thus, the fundamental point about revolutions is the *kind* of change involved, and I suspect that a better understanding of this kind of change is more likely to be forthcoming from the study of small scale reconceptualizations rather than large since the analyses of the former are less likely to be entangled in larger issues. In closing this section, it would be appropriate to point out that the possibility of the type of change under discussion was recognized by earlier philosophers of science. Carnap, for example, on several occasions put forward the question whether probability assignments should be changed only by conditionalization, or whether in some cases a rational agent may thoroughly reassign probabilities (equivalent to reassigning prior probabilities or to changing language). Kuhn has not given arguments that such changes are matters of rational decision, but he can be seen as arguing that, rational or not, such changes frequently take place in the history of science.

Since, in the terminology of SSR, incommensurability accompanies revolutions, which are changes of paradigms, we should expect that there are extremely various degrees of incommensurability just as there are degrees of revolution. Once we have distinguished the separate elements of a disciplinary matrix, we can see that there are distinct kinds of incommensurability potentially associated with each element. The greater the number of components that change in the disciplinary matrix, the greater the number of kinds of incommensurability that arise. We will consider each type separately below in assessing their impact on the objectivity of science and its progress.

Incommensurability's sources : Exemplars versus rules

We have seen in the last section that the pegs that hold a disciplinary matrix together are the shared exemplars. I believe the most significant (and controversial) elements of Kuhn's work all flow from this insight that it is exemplars rather than rules that form the basis of the scientific subcommunities. Thus a pivotal and controversial point in discussions of Kuhn's view of science is the question whether exemplars are fundamental to the nature of scientific development or whether they only play a minor indirect role. Kuhn's view is that the interpretation that symbolic generalizations receive is largely via the exemplars, scientists do not learn explicit principles. The traditional view is that only fully specified languages can be systematically studied. The distinction being invoked here is that epistemology is the study of changes of belief given a fixed language, whereas the case being discussed would be an example where either it was indeterminate exactly which fully specified language was being learned or else where the language was changing.

But this is not an objection to the model of natural language acquisition that was being presented but instead is a refusal to recognize that there is a subject here to be studied. The distinction between change of belief and change of language is one that serves us ill in the study of the development of science. Scientific language is fully articulated and precise only at the end point where the theory and its application and assumptions have all been thoroughly worked out. The day to day work of the scientist is as much a matter of forging a suitable new language and set of concepts to describe the world as it is a matter of making adjustments of belief in a given language. It is true that we have no good models of language within which we can currently approach these problems — all semantic theories assume fixity of language that is incompatible with the phenomena that are at the center of our investigation. But this is a reason for being dissatisfied with our semantic theories rather than for abandoning the investigation.

The positivistic conception of science was sharply molded by the positivist conception of language and Kuhn's rejection of the former always implies a rejection of the latter. But since the shaping is not always visible and explicit the objection of the linguistic conceptions is not always explicit or even conscious or deliberate. Suppe, for example, misses this point when he presents Kuhn with a dilemma concerning the resemblance relations learned through exposure to exemplars. With regard to the similarity relationships learned via exemplars he asks :

> "...are they important relationships because the community picked them out or does the community pick them out because there is some good reason to pick them out ?" (507, SST)

Suppe argues that if one chooses the first alternative then "although you may have escaped a private-individual theory of science nevertheless you have got a private-group view on your hand... (507)". There is an unstated premise here that I find difficult to state in any plausible way : I doubt that English is an important language for any reasons intrinsic to its syntax or semantics, but rather it is important because there are many English speakers. But what follows from this ? Do we have a private-group view of language as a consequence ?

Be this as it may the other alternative that Suppe mentions is the more significant :

> "On the other hand, if you take the other alternative — namely that the community licenses them because there are good reasons for considering them to be exemplars — then although your reference to community may be sociologically interesting, it nevertheless is not illuminating as far as understanding the rationale behind the use or role of exemplars in science. For then no reference to the community is necessary because although the community teaches them, nevertheless the reasons for using them are things that are independent of the community." (507)

The dilemma is a false one, for its claim to exhaustiveness depends on a dichotomy between matters of fact and matters of convention.

The alternatives Suppe allows are that the community chooses a similarity relation for no good reason or that they choose it for reasons that would be good reasons for any group. But the obvious other alternative is that they may choose for reasons that are good *for that group.* This would assume a relativism of good reasons, but I see no cause to shrink from the idea that good reasons can be relative to the current state of knowledge, problems solved and problems being considered, fundamental assumptions about the world and so on. Thus one reply to Suppe is that there may be good reasons for picking out a relation that can only be understood in relation to the group.

A second reply is that even in the case where a relation is one that would or should be important for any group the only way that it can be picked out is via exemplars. There is, I believe, considerable confusion (perhaps even more in Kuhn's explanations) about whether the similarity relations define the scientific group or vice versa. The answer is that the distribution of *individual* similarity relations determines the group and that *group's* similarity relations. The group similarity relations will be messy composite relations formed from those of the members of the group in ways that require (further ?) study. There is a clear sense in which the characterization of such groups is relative, but this does not mean that there are not objective reasons for the classification into groups.

The point might be clearer if we illustrate it with a simple example. Imagine a department divided on the question of how many courses to require of its students — suppose that there are nine department members and that they respectively believe that the following numbers of courses should be required zero, zero, one, two, three, fifteen, sixteen, nineteen, twenty. Graphically it is clear that there are two groups. In terms of one place predicates we could characterize the groups by saying that all members of the first group believe that at most $3 + n$ courses should be required while those of the second believe that at least $15-m$ should be required for any choice of n and m such that $n + m < 12$. There are other ways of characterizing the group; each member of the first believes that fewer courses should be required than any member of the second. Or, what captures the fact that there are groups most perspicuously, any member of either group resembles the other members of that group more than they resemble any member of the other group. That this is the fundamental characterization can be seen from the fact that if the distribution were : zero, two, five, seven, nine, eleven, thirteen, sixteen, twenty, we could still divide the overall group into two subgroups (in several ways) so that the earlier characterizations above still apply. Thus in this case what defines a group in opposition to another group is that its members pairwise resemble each other more than any member of another group. The defining relationship requires a three place relation and thus is relative in that sense which contrasts with definability in terms of a one place predicate, but there is no significant ontological sense in which the groups are not objectively defined. The further point of the illustration is that even in Case 1 where clear groups exist on the basis of course requirement attitudes, there is considerable difficulty in saying what the group attitudes are. Does the first group want the median number of courses, one, or the mean number 1.2 ?

Returning to the main point, the scientific community will be defined as those scientists whose similarity judgments are more alike than they are like members of other groups. And the "group similarity relation" is a useful fiction that provides a brief way of speaking about the diverse though rather similar relations belonging to the individual members of the group. Kuhn's point is that the group need not be defined by perfect agreement in their judgments, but only in terms of their relative ease of agreement in contrast with the difficulties of communicating with members of other groups.

Incommensurability : How disastrous is it ?

One of the most disputed and misunderstood of the claims in SSR is that scientific theories from different disciplinary matrices are incommensurable. In mathematics the term "incommensurable" has a clear and precise meaning that is being used very straightforwardly in the analogy involving scientific theories. Two magnitudes are incommensurable if there is no pair of integers n and m such that the ratio of the first magnitude to the second is represented by n/m. The most famous historical example of incommensurable magnitudes are the side and diagonal of a unit square Since $\sqrt{2}$ is an irrational number no pair of integers represent the ratio in question. This does not imply however that we cannot compare the magnitudes in question with as great a degree of precision as is desired; for any pair m and n we can determine whether m/n is greater or less than the ratio, all that the incommensurability proof shows is that *m/n* is never equal to the ratio.

The claim that scientific theories from different disciplinary matrices are incommensurable is intended to make an equally strong claim, *but no stronger*. Philosophy of science in the earlier part of this century was based on the assumptions that there was a theory neutral observation language and that conceptual analysis would reveal the appropriate system of inductive logic. Given the neutral observation language and a fixed inductive method, two scientists could disagree only if the two of them had been in a position to make different observations. Any two scientists who had been in the same circumstances would have formed the same beliefs with regard to observation sentences, and given the shared basis in observation sentences the agreed upon method of theory evaluation would confer the same probabilities or degree of confirmation on any theories considered.

More significantly for our present discussion, two theories could be compared by deducing from the theories (together with suitable auxiliary hypotheses) incompatible observation sentences. If two theories did not lead to different observation sentences, then the theories were regarded as empirically equivalent. Thus a fundamental assumption of this standard picture of theory testing is that although theories may be framed in different language they share a common sublanguage, the observation language.

To contrast the various ways that theories can be shown to be incompatible it will be useful to introduce some more precise terminology. We can define theories T_1 and T_2 to be syntactically incompatible if they are in a common language with a set of inferences rules such that for some sentence S, S is deducible from T_1 (and accepted auxiliary hypotheses) while the negation of S is deducible from T_2 and the same auxiliary hypotheses. This is the strongest form of incompatibility though it is relative to a set of inference rules and, in any serious applications, to a set of auxiliary hypotheses as well.

However, there are other senses of incompatibility — "Schnee ist weiss" and "La neige est noire" are intuitively incompatible sentences, although they are not in the same language. Thus let us define theories T_1 in L_1 and T_2 in L_2 to be *truth theoretically incompatible* just in case there is a language M which is a meta-language of both L_1 and L_2 such that there are correct truth theories for L_1 and L_2 in M, and it follows from the truth theories and the logic of M that T_1 and T_2 cannot both be true. In effect, this amounts to the syntactic incompatibility in M of the theories :

$$D_1 \,\&\, \mathrm{True}_{L1}(\text{“}T_1\text{”}) \qquad D_2 \,\&\, \mathrm{True}_{L2}(\text{“}T_2\text{”})$$

where D_1 and D_2 are the respective truth theories.

Although I have formulated this alternative without any evident mention of analyticity, we have had to rely on the conception of a correct truth theory for one language in another. I do not know how to settle, for example, questions about whether a correct truth theory of French in English requires the specification that "noir" is incompatible with "blanc" or whether one simply specifies enough relations to obtain some truth condition in English for each French sentence. Thus it is a difficult question whether "Schnee ist weiss" is truth-theoretically incompatible with "La neige est noire".

Even if these issues were clear, there would be reason to look at a further concept of incompatibility. I think that on no theory of analyticity would the sentence "Mr. Carter was in Paris in 1978" and "The President of the U.S.A. was not in the capital of France in 1978" be analytically incompatible. Yet it seems clear that two English speakers who utter those respective sentences are contradicting each other. Thus we are led to formulate the broadest sense of incompatibility, *de facto* incompatibility. Since theories are presumably formulated without token reflective terms, we can avoid the complications and relativizations that a full specification of the relation for all sentences would require. T_1 in L_1 and T_2 in L_2 are *de facto* incompatible if there is a language M containing correct (possibly partial) truth theories D_1 and D_2 for L_1 and L_2 respectively such that for some set of true sentences K of M,

$$K, D_1, D_2 \vdash_M \sim (\mathrm{True}_{L1}(\text{“}T_1\text{”}) \,\&\, \mathrm{True}_{L2}(\text{“}T_2\text{”}))$$

but K, D_1 and D_2 do not entail the falsity of either T_1 or T_2. Since this definition allows any set of true sentences of M to enter, we need not be concerned about issues involving what is or is not a semantic rule, analytic truth or meaning postulate of any language. It is true that we need the notion of a correct truth theory, but we now need not be concerned about whether a correct truth theory contains any extra information. The previous definition depended on the notion of what followed from a truth theory alone, this one allows any further amount of factual information. Furthermore, we do not require that there exist full truth theories that give truth conditions in M for each sentence of L_1 and L_2; it suffices if we know enough about the truth conditions to know that in fact the relevant pair cannot both be satisfied.

This last definition of incompatibility is the one that is required to understand how incommensurable scientific theories can be compared. We will not require that two theories be within the same laguage, that they be related to a common neutral observation language, or that there is any common language into which they can both be translated. If, using all of the information at our disposal about both the theories, and the world we can show that not both theories can be true, then we can take the further step of attempting to ascertain which, if either, is compatible with the facts. Some readers may believe it objectionable that so semantic a concept as incompatibility is treated as depending on factual matters. Instead of giving the lengthy general defense of this situation that could be adduced, let me note simply that for our purposes the important question about a pair of theories is not a semantic question but the factual question whether they are incompatible as descriptions of the world to the best of our knowledge of the theories and the world.

We have seen that we can sidestep (in a metalanguage) the problem caused by so-called "meaning incommensurability". We cannot guarantee, of course, that the appropriate truth theories can be found or uncontroversially agreed upon. The old assumption that there is a class of observation sentences provided a particularly simple solution to the problem of how theories attach to the world and to how theories are to be evaluated.

To assume that such guarantees exist leads to misunderstanding of the nature of science. If we believe the myth of observation sentences then we are blinded to the importance of the quest for experiments that will produce intersubjective and intertheoretic conviction. To attack the myth of observation sentences is to attack a certain conception of the essence of the objectivity of science, but it is not to attack its objectivity. It is only if we carefully scrutinize the significant features of actual science as it develops intead of the crystalline rational reconstruction that emerges at the end that we will find the features characteristic of its objectivity. Unlike Reichenbach, we can see objectivity in the development of scientific theories and concepts — the field for epistemology is richer not poorer for this redirection of attention.

Incommensurability : As the world changes

In the previous sections I argued that incommensurability derives from many sources, from differences in symbolic generalizations, in models, in metaphysical assumptions, and in values. I also argued that the degrees of incommensurability vary greatly but that these differences between disciplinary matrices need not imply incomparability. Nor the impossibility of incompatibility. Such incompatibility at a predictive level, and indeed the actuality of progress at that level is recognized by Kuhn :

> "It must already be clear that my view of scientific development is fundamentally evolutionary. Imagine, therefore, an evolutionary tree representing the development of the scientific specialties from their common origin in, say, primitive natural philosophy. Imagine, in addition, a line drawn up that tree from the base of the trunk to the tip of some limb without doubling back on itself. Any two theories found along this line are related to each other by descent. Now consider two such theories, each chosen from a point not too near its origin. I believe it would be to design a set of criteria — including maximum accuracy of predictions, degree of specialization, number (but not scope) of concrete problem solutions — which would enable any observer involved with neither theory to tell which was the older, which the descendant. For me, therefore, scientific development is, like biological evolution, unidirectional and irreversible. One scientific theory is not as good as another for doing what scientists normally do." (Kuhn, 1970, 264)

To return to the task of interpreting SSR, we can note with satisfaction that almost all of the occurrences of "incommensurability" can be interpreted as incommensurability of disciplinary matrices. The actual terms are often slightly different, "traditions" (103, 148), "paradigms" (150, 157), "viewpoints" (175, 200), "standards" (149), "solutions" (165) and "ways of seeing the world" (4). Almost but not all. The recalcitrant passage (112)

> "... at times of revolution, when the normal-scientific tradition changes, the scientist's perception of his environment must be re-educated — in some familiar situations he must learn to see a new gestalt. After he has done so the world of his

> research will seem, here and there, incommensurable with the one he had inhabited before."

can be interpreted away by emphasizing the epistemic verb "seem".

The difficulties of communication across theoretical divides is well documented in the history of science, the forcelessness of "rational argument" to convert scientists to new viewpoints is legendary. But these are the mere epistemic manifestations of a change of disciplinary matrix and do not threaten a more sophisticated approach to questions of objectivity as outlined above. However, the analysis above assumes that a sufficiently neutral metalaguage can be found, partly because the two theories in question are attempting to describe a single world. If, however, one takes seriously the suggestion hinted at in the passage quoted and more explicit thirty-eight pages later, one sees a much deeper sense of incommensurability lurking in Kuhn's text :

> "In a sense I am unable to explicate further, the proponents of competing paradigms practice their trade in different worlds." (150)

An ontological chasm yawns here. If distinct disciplinary matrices are describing different worlds then none of the earlier comparisons via truth theories make sense.

The positivist conception of science presupposed that we were given boxes into which we sorted the objects of nature. Kuhn (and others who have attended to the history of science) have argued, as discussed above, that we must first manufacture the boxes, deciding on their size, shape and number. And sometimes in mid-sorting we must redesign the boxes. But the metaphysical doctrine darkly hinted at in the last section suggests that scientists in different disciplinary matrices are not only using different boxes, but are using different objects too. Clearly this is not a thesis that admits of historical defense but requires further philosophical scrutiny.

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All references not further specified are to Kuhn 1962.

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