

INCOMMENSURABILITY NATURALIZED

Abstract

In this paper I argue that we can understand incommensurability in a naturalistic, psychological manner. Cognitive habits can be acquired and so differ between individuals. Drawing on psychological work concerning analogical thinking and thinking with schemata, I argue that incommensurability arises between individuals with different cognitive habits and between groups with different shared cognitive habits.

1 Introduction—incommensurability and quasi-intuitive cognitive capacities

Incommensurability was one of the primary topics in the philosophy of science of the later twentieth century, having been introduced principally by Thomas Kuhn while also vigorously promoted by Paul Feyerabend. In *The Structure of Scientific Revolutions* (1962) Kuhn discussed two kinds of incommensurability—incommensurability of standards and incommensurability of meaning. He spent much of the rest of his career developing the latter using various ideas from the philosophy of language. Interest in such ‘classical’, Kuhnian accounts of incommensurability has declined from its peak, largely because most philosophers hold that other developments in the philosophy of language have shown how incommensurability may be defeated or at least deflated (as is amply demonstrated in the work of Howard Sankey (1994, 1997)).¹

Kuhn’s later work on incommensurability contrasts with the tenor of much of *The Structure of Scientific Revolutions*. The latter I regard as having a strong naturalistic streak that was lost in Kuhn’s subsequent discussion of incommensurability, which was much more standardly philosophical and aprioristic in character (Bird 2002, 2005). While I agree with Sankey’s criticisms of meaning incommensurability, I believe that a return to Kuhn’s earlier work will provide us with the starting point for a rather different approach to the phenomenon of incommensurability. Firstly, we may revisit the issue of incommensurability of standards that Kuhn did not develop in much detail. And secondly we may explore how Kuhn’s early naturalistic account of the functioning of paradigms (as exemplars) may furnish a naturalistic, primarily psychological conception of incommensurability.

The key idea in what follows is that we all use in thinking various cognitive capacities and structures that have the following features: (i) they cannot be reduced

¹Declined but very far from evaporated. See Hoyningen-Huene and Sankey (2001) and this volume as examples of considerable continued interest in the general issue of incommensurability and allied issues.

to general, formal rules of reasoning (e.g. formal logic); (ii) their existence and the mechanism of their employment are typically unconscious, so that they are deployed in a manner that is akin to intuition—what I call a *semi-intuitive* manner; (iii) they are often acquired as a matter of practice and repeated exposure and practice, so that they have the character of skills. The sorts of skill or capacity I am referring to here include: mental schemata, analogical thinking, pattern recognition, quasi-intuitive inference. As I shall describe below, these are related, and together I shall refer to them as an individual’s quasi-intuitive cognitive capacities (QICCs).

The proposal of this paper is that as a result of social induction with a set of paradigms a scientist in a given field acquires a set of QICCs specific to that field. Incommensurability arises when scientists operate with different QICCs; this incommensurability is an incommensurability of standards and an incommensurability of understanding (which is not quite the same as an incommensurability of meaning, unless the latter is understood in the relatively loose sense corresponding to the intended message of a communication). Such incommensurability may arise in communication between radical and conservative scientists during a scientific revolution, in the understanding of the work of scientists from another era, and also between scientists working on similar problems but from differing background fields.

2 Exemplars in science—an example

Much cognition is habitual. These habits are acquired. This occurs in scientific thinking no less than in informal, everyday cognition. Consider the following example of a test question that might be set for physics students:

A thin rectangular plate, O , of mass m is free to rotate about a hinge at one edge. The length of the plate from the hinge to the other edge is l . It is placed within a stream of fluid which exerts a pressure P in the direction of flow. Let θ be the angle between the plate and the direction of fluid flow. Write down an equation describing the motion of O in terms of the change of θ over time, assuming θ is small.

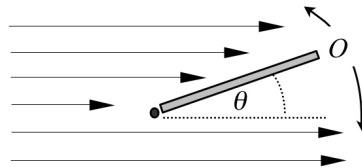


Figure 1:

Many will be able to see immediately that the motion will be simple harmonic and will be able to write down the equation of motion straight away as having the form:

$\theta = \theta_{max}\sin(kt)$. In some cases this will be assisted by seeing an analogy between the oscillating plate and a rigid pendulum (see fig. 2). The motion of the pendulum is the first and best known instance of simple harmonic motion that any student of physics gets to meet. Seeing the analogy will thus enable the student to know the correct equation of motion, or at least its form, without further ado. The analogy in this instance will be easy to spot, since the standard diagram for a pendulum is very similar to that employed in the question given, rotated through a right angle.

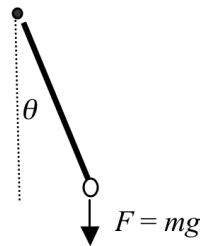


Figure 2:

This example illustrates important aspects of cognition in science:

- (i) The employment of paradigms. Kuhn (1962) defines paradigms as standard exemplars of problems and their solutions. In this example, the motion of the pendulum and its standard solution are a paradigm.
- (ii) Analogical thinking. The use of a paradigm is essentially analogical. The analogy between the new problem of the oscillating plate and the well-known case of the pendulum shows or suggests to the student that the equation for the former will have the same form as that for the latter.
- (iii) Scientific cognition is not a matter of following very general rules of rationality (where ‘rule’ is understood in a strong way, implying that it can be followed mechanically). (This was a common background assumption among many logical empiricists, and the significance of paradigms in Kuhn’s work is that they provide a refutation of the logical empiricist and positivist view of scientific cognition.)
- (iv) Learned, quasi-intuitive inference patterns and associations: the ability to answer immediately or via the pendulum analogy is quasi-intuitive. It is like intuition in that it is non-reflective or only partially reflective, being a more-or-less direct inference (rather than one mediated by the sort of rules mentioned in (iii)). But it is unlike intuition in that it is learned, acquired by training with paradigms/exemplars (e.g. many student exercises are of just this sort).
- (v) Cognitive processes can be acquired habits. The quasi-intuitive inferences mentioned in (iv) are made as a matter of habit. The habits are acquired as a result of repetitive exposure and practice. For example, what

may start out as a conscious, sequential activity of reasoning, eventually becomes a one-step quasi-intuitive inference. This does *not* mean that the same process of sequential reasoning takes place unconsciously. Rather the habit replaces the conscious reasoning process.

3 Schemata and analogies in scientific thinking

I take the above to be ubiquitous features of scientific cognition (and indeed of cognition in general). But the illustration given by one rather low-level example is not sufficient to show this. Nor does it show how quasi-intuitive inferences dovetail with the undeniable existence of sequential, conscious ratiocination in science. Even so, other work confirms the claim that these features are indeed widespread in science.

First of all, it is clear that cognitive and inferential habits are a general feature of human psychology. This has been recognized since Hume and before, and has recently been the subject of more systematic study. For example, educational psychologists, employing an idea originating with Piaget have developed schema theory, according to which thinkers employ *schemata* which link certain patterns of stimuli with responses in such a way that does not require the conscious attention of the subject (Schallert 1982; Anderson 1984; Stein and Trabasso 1982; D'Andrade 1995). (*Scripts* are also often discussed, which are schemata that apply to behaviour.) A schema encapsulates an individual's knowledge or experience, and is typically acquired through repeated use of a certain pattern of inference. Although schemata have not been discussed especially in relation to science, if schemata, or something like them (e.g. mental models, which can be regarded as a more general kind than schemata) are ubiquitous in thinking outside science, it is reasonable to suppose that they play a role in scientific thinking also. Schemata would correspond to the quasi-intuitive inferences I have mentioned above. One may then hypothesize that the function of Kuhnian exemplars is the inculcation of scientific schemata. It is notable that while many schemata are individual (and some may be innate), many are also shared and culturally acquired, as are Kuhnian exemplars.

Exemplars also function analogically. Historical work, most notably that of Mary Hesse (1966), has shown how prevalent analogy is in the history of science, while contemporary psychological work by Kevin Dunbar (1996, 1999) confirms that analogical thinking is an important feature of modern scientific thinking.² Rutherford's analogy between the solar system and the structure of the atom is a well known example. Dunbar studied scientists at work in four microbiological laboratories. He found that use of analogical reasoning was very common. Distant analogies, such as Rutherford's were unusual, but close analogies were frequently used in the design of experiments (very close—e.g. one gene on the HIV virus to another on that virus) and the formulation of hypotheses (moderately close—e.g. the HIV virus and the Ebola virus). Analogy is particularly helpful in explaining unexpected results: again, local analogies are employed first in coming to a methodological explanation; if a series of unex-

²See also Holyoak and Thagard (1995) on analogical reasoning, particularly in science. But note that Gentner and Jeziorski (1993) also argue that the degree to which analogy is used differs through history, and that medieval scientists used metaphor to a greater extent than true analogy, relative to more recent scientists.

pected results survives methodological changes, then scientists will use more distant analogies in the construction of models to explain their findings. Analogical reasoning has been modeled successfully with computer simulations; of particular interest is the modeling of the analogical use of old problem solutions in solving new problems, known as *case-based reasoning* (see Leake 1998), which neatly describes the particular use of analogy described in the example of the hinged plate and the pendulum. While case-based reasoning is most conspicuous in medicine and most especially in psychoanalysis³, Dunbar's work as well as that of Hesse and of the historians of science shows how it is a ubiquitous feature of scientific cognition.⁴

Kuhn stresses the role of exemplars in forging relations of similarity among problems. Research (Chi et al. 1981) confirms that training with scientific problems induces new similarity spaces on problems and their solutions. Thus novices (undergraduates) categorize problems differently from experts (graduate students). The former employ superficial criteria (e.g. similarity of diagrams and keywords—i.e. categorizing problems as rotating disk versus block and pulley problems) whereas the latter categorize the problems according to the underlying physical principles involved (conservation of energy versus force problems). What role does the acquired similarity space play in scientific problem solving? Clearly, if one is able to categorize problems according to their underlying physical principles then one is in a good position to start focussing on the equations and laws that may be used in solving the problem. In the light of the forgoing discussion of analogies and schemata, one key function for a similarity space is the selection of an appropriate schema or the bringing to mind of an appropriate analogy. More generally we can see the possession of a similarity space of this kind as an instance of a pattern-recognitional capacity. What are seen as similar are certain structures or patterns. When we use a schema or analogy we see that the target has the structure appropriate for the application of the schema or one that it shares with the source analogue. It is possible to see most or even all of scientific thinking as involving *au fond* pattern-recognitional capacities—Howard Margolis's (1987) account of scientific cognition is just that.⁵

Analogical thinking and thinking in schemata are not *prima facie* the same. Schemata are abstract structures with place holders for various possible inputs and outputs. Analogies are more concrete. The source analogue (e.g. the solar system in the Rutherford example) is complete rather than possessing place-holders. Nonetheless there are clear similarities and relations between the analogs and schemata. Any interesting use of analogy involves abstracting salient features of the source analogue. Thus the structure abstracted from an analogue will have the character of a schema. The nodes in such an abstracted structure are now place-holders. If we take Napoleon's invasion of Russia in 1812 as an analogy for Hitler's invasion of the Soviet Union in 1941, then

³See Forrester (1996) on thinking in cases in and beyond psychoanalysis, linking it to Kuhnian themes.

⁴Nickles (2003a) also relates Leake's case-based reasoning to science and to Kuhnian exemplars. Nersessian (2001, 2003) discusses (in)commensurability in relation to model-based reasoning. My approach is sympathetic to Nersessian's 'cognitive-historical' approach. Unlike Nersessian, I am keen to explore, as far as possible, the contribution of cognitive science in a manner that is independent of issues of concept formation and conceptual change (cf. footnote 12). That said, Nersessian (2001: 275) does distance herself from the view that these matters are to be understood in purely linguistic terms.

⁵This we may in turn think of as being realized by connectionist systems in the brain. For how this might work in the scientific case, see Bird (2005); cf. Nersessian (2003: 185)

we are abstracting ‘ x invaded Russia/Soviet Union’ with x as a placeholder that may be taken by either Napoleon or Hitler. Thus we can see that use of an analogy may lead to the creation of a schema.⁶ We start by thinking analogically, but repeated use of a certain analogue will in due course allow us to think directly with the schema it gives rise to. Alternatively, we may regard the source analogue as a concrete vehicle for the schema. The analogue is concrete, but its use is by reference only to the abstract, schematic structure it possesses.

A further difference between schema and analogy is that the possession and operation of the former is typically unconscious while the use of analogy is conscious. Nonetheless, the similarities are more significant. First, there are unconscious elements in the operation of an analogy: the selection of an appropriate analogy may come immediately as may the forging of the structural links and the drawing of conclusions from them. In the example given above, the problem solver may immediately think of the pendulum as a good analogy and having done so immediately see what implications that has for the equation of motion of the hinged plate. Secondly, in both cases the reasoning and inferential processes involved are not algorithmic—for someone with an appropriate schema or who is adept at using analogies in the way described, the deployment of the schema/analogy is intuitive. Of course, the acquisition of a schema or facility with analogy is acquired, a matter of learning or training, and will often involve repetition and practice. So the ‘intuition’ is learned. That may sound like an oxymoron (so prompting my use of ‘*quasi-intuitive*’), but nevertheless captures the entirely coherent idea that cognitive habits can be acquired. It is the same phenomenon for which we use the term ‘second nature’. First nature we are born with, second nature is acquired. But both feel natural and intuitive, rather than forced or the product of consciously following a rule or explicit ratiocination. Second nature covers a wide range of acquired habits and skills such as the fingering technique of an accomplished pianist; for our purposes the habits of relevance are cognitive ones, and in particular the quasi-intuitive inferences encapsulated in schemata and analogies.

Altogether, I shall refer to schemata, analogical thinking, quasi-intuitive inferences, similarity spaces, and pattern recognition as *quasi-intuitive cognitive capacities* (QICCs). In what follows I shall argue that if as claimed QICCs, acquired as a result of training with exemplars, are a common feature of scientific thinking, then we should expect science to be subject to a certain kind of incommensurability. I shall then suggest that it is this phenomenon to which Kuhn was referring in *The Structure of Scientific Revolutions*. This would permit a naturalistic understanding of incommensurability. It is naturalistic because the features described above are not *a priori* characteristics of scientific cognition; to appreciate their existence and to understand their nature requires empirical investigation of science. To that extent Kuhn’s earlier historically founded account of scientific change also counts as naturalistic. And it is noteworthy that Kuhn remarked in 1991 “. . . many of the most central conclusions we drew from the historical record can be derived instead from first principles,” (1992: 10) thereby insisting that even his own earlier historical naturalism was unnecessary. However, the naturalism of Kuhn’s interest in psychology and of the approach taken here goes further, in that it

⁶C.f. Gick and Holyoak (1983) and Holyoak and Thagard (1997: 35) on analogy inducing a schema. Gick and Holyoak note that problem solving with analogies is more effective when achieved by reasoning via an induced schema as against reasoning directly from an analogue.

draws upon the *scientific* investigation of science and scientific cognition. It is true that when it comes to psychology, the boundaries between the *a priori* and the empirical and between the empirical-but-not-scientific and the scientific may be quite unclear. For it may be that some proportion of folk psychological knowledge is innate, and it is certainly true that much psychological insight may be acquired without science simply through reflective acquaintance with the variety of persons about us and through reflection on our own cases. While Kuhn's basic insight may not itself be a product of scientific research, he was certainly willing in *The Structure of Scientific Revolutions* to draw upon the results of scientific experimental psychology, be it Gestalt psychology or the playing card experiments of Bruner and Postman. Furthermore, there are few insights of subjective or folk psychology that cannot be improved and put on a firmer epistemic footing by scientific psychology. The proposals as regards incommensurability I put forward here do not presume to constitute a scientific investigation of that phenomenon. I do believe that they have some plausibility by appeal to our low level psychological knowledge. But they do also draw upon the scientific investigations referred to above and, more importantly, they are programmatic in that they suggest an approach that stands open to scientific development and confirmation or refutation.

According to my proposal the features of science that permit incommensurability exist in virtue of the nature of human psychology (principally the QICCs). The nature of incommensurability must be regarded as fundamentally psychological. This contrasts with more explicitly *a priori* (principally semantic) accounts of incommensurability given by many philosophers, including Kuhn himself in later life, which draw upon the philosophy of language broadly construed. Insofar as incommensurability is to be found between groups of scientists, there is also a sociological dimension. But the basic component is still psychological. It is the fact that groups of scientists are all trained with the same or similar examples and the fact that familiarity with those examples and a grasp of the quasi-intuitive inference patterns they engender are preconditions of membership of the group that explains why incommensurability emerges as a social phenomenon. (In a similar way language is a social phenomenon. But what makes it possible are features of individual psychology.)

4 Incommensurability of standards

Incommensurability, Kuhn tells us, is a matter of a lack of common standards. It is easy to see how incommensurability may arise from the psychological features of scientific cognition outlined above. Let us assume for a moment that the QICCs discussed also encompass the capacity for making judgments of correctness. For example, a judgment of correctness may be made on the basis of the use of some schema induced by experience with a particular set of exemplars, or on the basis of perceived similarity to such exemplars. If that is so, then scientist A and scientist B who have been trained with different sets of exemplars, and so have different schemata or similarity spaces may not be able to come to the same judgment over the correctness of some putative scientific problem-solution. What goes for individual scientists goes also for groups of scientists, where the two groups differ in their exposure to exemplars.

In this section I shall consider the question, can the account of QICCs be extended to include judgments of correctness? In the next section I shall extend the account to encompass incommensurability of *understanding*, by which I mean the phenomenon of incomprehension that Kuhn identifies between adherents of different scientific schools or sides in a revolutionary dispute.

Even if cognition often involves QICCs, it does not immediately follow that judgments of correctness involve a similar mechanism. One might invoke the familiar context of discovery versus context of justification distinction. Employing this one could claim that QICCs may lead one to an appropriate answer to a problem, but that one need not use that same mechanism in showing that the answer is a good one. For example, in formal logical systems, such as first-order Peano arithmetic, there is no algorithm for generating a proof of any given proposition. But given any putative proof there is an algorithm for checking whether or not it is a proof. Mathematician A and mathematician B may come at their proofs in different ways, but they will be able agree on the correctness of a proposed proof. Similarly two scientists may produce their theories in different ways as a result of differences in training with paradigms, but that does not preclude their being able to make similar judgments of the correctness of some given theory.

Such a response underestimates the role in cognition of exemplars and the QICCs they induce. Let us consider the mathematical case. It is in fact both rare and very difficult in mathematics to reduce the proofs that mathematicians do offer to a logical form that may be checked algorithmically. For a start, the possibility exists, even in principle, only when the mathematical field in question has been formally axiomatized, and that is a relatively modern development and is still not ubiquitous. It is worth noting that Euclid's axiomatization of geometry fails in this respect, not only because it is not formalized within a logical system, but also because it too depends for its operation on quasi-intuitive inferences. Indeed, much of the impetus behind the programme of rigor, formalization, and the development of logic in the nineteenth century arose from the realization that even Euclid's proofs rested upon intuitive assumptions, in many cases generated by a visualization of the geometrical problem in question. Thus what standardly passes as proof, even today, is not something that can be algorithmically checked. If it were, then the checking of proofs would not be the difficult and laborious process that it is. Moreover, mathematicians are not interested in proof merely as a means of validating belief in theorems. Rather the proofs themselves furnish understanding and insight—at least a good proof does—which is why mathematicians may seek new proofs of old theorems. But that understanding cannot be gained by looking at the proof at the level of formalized steps, but only by having an overview of the proofs, and that will involve seeing how the proof works at a level such that when 'A follows from B' the relationship between A and B is of the quasi-intuitive inference kind rather than the formal logical step kind.

Thus even in the paradigm case of a discipline where contexts of discovery and justification can come apart, it seems that QICCs play a part in the context of justification as well as that of discovery. If that is true of mathematics, how much more true is it of empirical science. What has been said about mathematics is true also of the mathematical components of a scientific paper. Furthermore, the relationship of confirmation is one that cannot be formalized. More generally, and of particular importance, is the fact

that any scientific paper rests on a mountain of unstated assumptions and background knowledge. These are assumptions concerning the mathematics employed, theoretical background knowledge common to all working in the field, and practical background knowledge concerning the experimental apparatus and other aspects of the experiment. These are all encoded in the schemata that a trained scientist (quasi-)intuitively employs.

Thus a difference in schemata, standard analogies, similarity spaces, and hence the sets of quasi-intuitive inferences scientists will possess and employ, may be expected to lead scientists from different traditions to make different judgments of correctness.

5 Incommensurability of understanding

Although Kuhn makes clear that incommensurability means the lack of common standards of evaluation, he nonetheless emphasizes the connection between incommensurability and matters of meaning, and indeed the search for an account of incommensurability in terms of meaning was the focus of much of his philosophical writing in the later part of his career.

In the last section I mentioned the importance of background knowledge and tacit assumptions. These are encoded in schemata, and since background beliefs influence our judgments, we may expect that those with different sets of background beliefs may make different judgments. However the influence of tacit assumptions is not limited to the making of judgments but also extends to communication. For example, if subject A asserts “P therefore Q” where the inference from P to Q is a quasi-intuitive one, mediated by a schema that subject B does not possess, then B may not be in a position to understand A’s reasoning. In this case the literal meaning of ‘P therefore Q’ is comprehensible. But communication goes beyond mere literal meaning.

Almost all communication depends on shared tacit assumptions. To employ a homely example, a cook’s recipe may state “break two eggs into a bowl; stir in 200g sugar and 100g butter . . .”. The recipe does not tell one to exclude the egg shells from the bowl; that’s just obvious, at least to a cook, as is the fact that the sugar should not be in cubes or the butter frozen. We all make tacit assumptions of these kinds all the time in our conversations with one another. And this is no less true in science. Indeed it is more true in science because there is so much background knowledge that it simply would not be possible to refer to it all explicitly. The function of schemata is to encode such information. Thus a cook might have an EGG-IN-COOKING schema that tacitly encodes the instruction to keep the yolk and egg but discard the shell (unless explicitly instructed otherwise). Similarly scientists will have schemata that encode information about the use and purpose of standard pieces of equipment, schemata for various common kinds of inference patterns, and likewise schemata for encoding all sorts of other knowledge. The function of a scientific education is, in good part, to furnish young scientists with the schemata required to participate in their speciality. Since all scientists in the given field share the same schemata communication between them is schemata-dependent. Even a schoolboy can say and understand, “the litmus paper turned red, therefore the solution is acidic” without having to enunciate or hear the major premise, “acids turn litmus paper red”, since the latter is part of the LITMUS

schema, one of the earliest acquired in a science education. That inference exemplifies a simple quasi-intuitive inference. A slightly more sophisticated instance would be the familiar pattern found in reasoning concerning simple harmonic motion, as may be applied to the problem with which this paper starts: (i) $d^2\theta/dt^2 = -k^2 \sin \theta$, therefore (ii) $d^2\theta/dt^2 \approx -k^2\theta$ for small θ , therefore (iii) $\theta \approx a \sin kt + c$ for small θ . Those inferences will not be comprehensible as rational inferences by someone who lacks the training and hence schemata common to mathematicians, physicists and engineers.

The role of tacit assumptions is not limited to underpinning (enthymatically licensing) the rationality of inferences that would otherwise look incomplete or irrational. For those assumptions also play a role in what a speaker intends to communicate. In the cooking example, the writer's intention is that the eggshell is excluded. Linguists have examined the role of tacit assumptions in intended meaning, for example, in rendering unequivocal a case of potential ambiguity (cf. Carston 2002 and Sperber and Wilson 1995). But as they also note, such assumptions play a role not only in mediating between the words uttered and their literal meaning but also between literal meaning and the speaker's intended message, what the speaker means to say in a broader sense.

In many cases we can fairly easily retrieve the tacit assumptions and make them explicit, but in other cases it is far from easy.⁷ A tacit assumption—a proposition included in a schema—will not always be one that was once explicit to the subject. There are many tacit assumptions with which we are born and which constitute the basis of folk physics and folk psychology as well as what one might call folk metaphysics (e.g. assumptions about causation—experiments suggest, for example, that small children expect causation to involve contiguity in time and in space). Such assumptions are difficult to retrieve because they have never been explicit. They have been hard-wired from the beginning. They ground fully intuitive inferences. Quasi-intuitive inferences occur as a result of a habit of using an explicit inference pattern repeatedly, such that, one can reasonably assume, the relevant neural pathways are developed in a way that is analogous to those that were hard-wired from the start. In such cases the need for the explicit assumption falls away. But note that the quasi-intuitive inference form may be passed on to other subjects without the tacit assumption ever having been explicit for *them*. Hence they will not be able to use memory as a tool for retrieving those assumptions. We often accept the truth of propositions on the basis of testimony; similarly we accept the reliability of inferences on the same basis. In the former case we lack the evidence for the proposition in question; in the latter case we may be ignorant of the intermediate propositions that mediate the inference pattern. In such a case 'retrieval' will not be so much a matter of finding a tacit assumption that the subject once possessed explicitly (for she never did) but will be rather more like a rational reconstruction of the inference pattern. In other cases the inference, although not innate, is one that *feels* natural.⁸ The fact that tacit assumptions may be difficult to retrieve or reveal is

⁷By 'retrieving' I mean coming to know that such-and-such is an assumption that one makes. One common case of the difficulty of retrieval is found in communicating with small children. Parents have to retrieve many common tacit assumption that adults employ but children do not.

⁸For example many of us find the inferences made in simple calculus compelling, such as: let AB be a chord on the curve C; then $(y_B - y_A)/(x_B - x_A)$ is the slope of AB. As B approaches A, $(y_B - y_A)/(x_B - x_A)$ approaches m . Therefore the slope of the tangent to C at A is m . The justification for this inference provided by Newton implied that when A and B coincide $(y_B - y_A)/(x_B - x_A) = m$. But as Berkeley pointed out

significant for an account of incommensurability, since it explains the reticence and incomprehension many feel when faced with a revolutionary proposal. Understanding and accepting the proposal requires the jettisoning of tacit assumptions, which will not be easy if they are difficult to retrieve or reconstruct.

6 Evidence for psychological incommensurability

What ought to be the best test of this explanation of incommensurability would be to use it to account for paradigmatic cases of incommensurability, such as incommensurability in disputes between adherents of old and new ‘disciplinary matrices’ (to use the term Kuhn used to capture a broader notion of paradigm, encompassing an array of commitments of a scientist or groups of scientists). This is, however, not as straightforward as it might seem. It is not clear that there is some clear phenomenon of incommensurability with paradigmatic cases we can point to. The very phenomenon is disputed. Kuhn did point to a number of alleged instances in *The Structure of Scientific Revolutions*, such as the Proust-Berthollet and the Priestley-Lavoisier disputes. But their treatment was not sufficiently detailed that we can unequivocally identify incommensurability as opposed simply to a scientific dispute.

For we should not immediately infer incommensurability from disagreement, since disagreement, even amongst rational individuals, does not show difference in standards or in understanding. To regard all rational disagreement as betokening incommensurability would be to trivialize the latter notion. The pages of *New Scientist* and *Scientific American* are full of examples of scientific disagreements. But to classify all of these as exemplifying incommensurability would be to render the latter concept of little interest. How should we distinguish disputes that involve incommensurability from ‘mere’ scientific disagreements? In most ordinary scientific disagreements the disputants will genuinely understand each other and will typically acknowledge the relevance of one another’s arguments. What will differ between them will be the weight they attach to the various considerations. In more serious disputes, those displaying incommensurability, we should expect to find the parties differing over even what counts as a relevant consideration, and in some cases to find incomprehension between the parties. If so, then incommensurability can be explained by the current account, as we have seen above. For the relevance of potential evidence depends on background knowledge, encoded in schemata, and so a disagreement over the very relevance of evidence may be readily explained by a difference in schemata. Likewise schemata are required for comprehension, and so a lack of understanding will also be explained by a lack of shared relevant schemata.

One problem is that it is natural to think of incommensurability as a symmetric concept: if A is unable to judge the science of B, thanks to incommensurability, then B

$(y_B - y_A)/(x_B - x_A)$ is just 0/0 when $A=B$, which is undefined. Mathematics had to wait until Cauchy’s rigorous definition of a limit before a satisfactory arithmetical justification was given of an inference form that everybody held to be valid. What exactly makes this inference feel acceptable is not entirely clear. It may be some general mathematical sense that we acquire in learning mathematics; it may be partly the rhetoric of the way we are taught; it may be that we just learn the technique and familiarity makes it feel natural. In any case, the justification for the inference is not something we can retrieve but is learned later as a rational reconstruction of what we already accept.

is unable to judge the science of A for the same reason. While that may be the case in certain instances, we should not expect to find it in revolutionary disputes between the adherents of the old disciplinary matrix and supporters of a revolutionary replacement. For the latter will typically have been educated within the old paradigm and will be well placed to understand and assess what it is they are trying to replace. Nonetheless, we might expect the conservatives not to regard the radicals' evidence as relevant or to fail to understand the radicals' proposals. The radicals will understand the conservatives and while they may also reject their evidence, they will at least know where it originates and why the conservatives regard it as relevant.

The classic case that does seem to exemplify incommensurability of this sort is the dispute between Galileo and the philosophers in the early seventeenth century. Here it seems that the relevance of experimental evidence versus the authority of Aristotle was in dispute, and that Galileo's mechanical theories were not understood by many of his critics. It is implausible to suppose that incommensurability arose because Galileo's assumptions were tacit, hidden in his schemata. As a radical his premises would be newly developed and would be explicit.⁹ Nor, for the reasons mentioned above, would it be the case that Galileo would not be aware of the tacit assumptions being made by his opponents. Rather, in this case, it must be that the schemata employed by the conservatives *prevents* them from accepting the new proposal, even when it and the evidence for it is presented in as transparent and assumption-free manner as possible. It may be that the conservative schemata make it impossible to see the relevance of the radicals' evidence. But more often those schemata will make acknowledged facts into evidence *against* the new proposal, in some cases even to the degree of making the new proposal seem senseless. Thus the conservatives see the relative order of things on Earth as a sign that it does not move or rotate (the Earth in motion would cause widespread chaos and destruction). In particular they argued that an object in free fall should not appear to fall directly downwards, but should seem to follow an arc as it is left behind by the rotating Earth. In another case they possessed a schema for falling bodies that includes the proposition that heavier bodies fall with a greater acceleration than lighter ones, which would be a schema that we all acquire naturally but also is reinforced by Aristotelian doctrine. Thus a major obstacle to Galileo's dialectical success was the need to dismantle his opponents' schemata that were in many cases deeply entrenched. But because schemata are entrenched, their existence and function is unknown to their possessors, and their contents thus difficult for their possessors to retrieve, they cannot be removed simply by the amassing of evidence against the propositions they encode. Thus the need for more 'psychological' methods of engagement, for example the use of rhetorical techniques (much commented upon by historians) and thought experiments. The purpose of a thought experiment, I surmise, is to assist in dislodging a schema by making explicit its internal contradictions or its inconsistency with some undisputed fact.

In the above I emphasized the role of the conservatives' schemata in preventing acceptance of a radical view, rather than the existence of a radical schema they do not possess. However the latter can play a significant role in other kinds of case of incom-

⁹This is not to deny that he would have developed schemata in his own thinking. Rather he would be able to retrieve the tacit premises easily and would have a reason to make them explicit.

mensurability, and was not absent from Galileo's case. One of the factors in Galileo's dispute was that he was a mathematician whereas his opponents were not. Consequently Galileo was inclined to use forms of argument that the latter thought inappropriate and which they probably did not fully understand. Incommensurability may arise, and may be symmetrical, when a given problem is tackled by scientists coming from different disciplinary backgrounds. The backgrounds may be perfectly consistent with one another but the nature of their differing schemata is such that participants in a debate find it difficult to comprehend one another's approaches. This may partly explain the phenomenon of revolutions brought about by outsiders. The outsider's new schemata and background knowledge, as well as lack of the constraints provided by the disciplines extant schemata may allow her to see potential solutions to a problem that the discipline has hitherto been unable to see. At the same time, it may be difficult for the newcomer to persuade the current practitioners of the discipline of the value of the new solution (quite apart from the problem of disciplinary jealousies).¹⁰ More generally incommensurability may arise when different disciplines converge on a similar set of problems. For example an emerging area of research exists at the physics-biology interface. But part of the problem in making progress is the lack of a common approach between biologists and physicists. Thus whereas biologists are likely to take a detailed biochemical approach to understanding, say, blood or the behaviour of the components of a cell, physicists will apply quite general mathematical models from soft matter theory or complex fluid theory. At a grosser level of description, physicists are theory-friendly, whereas biologists tend not to be.

As it stands evidence concerning the nature of incommensurability in actual scientific practice is largely anecdotal. Even so, what there is tends to support the view that insofar as it does exist, incommensurability may be understood using the psychological tools described above.

7 World-changes and the phenomenology of incommensurability

Kuhn's incommensurability thesis is closely related to his claim that as a result of a revolution a scientist's world changes. The latter has attracted attention because of its constructivist sounding content. But it is clear that Kuhn has no simplistic constructivist intent. However, what exactly Kuhn did have in mind is less clear. I propose that Kuhn's usage is only a minor extension, if at all, of a perfectly common English metaphor of world and world-change. We often talk about someone's world changing as a consequence of a major event, as we also talk about someone's world, or the world of the poet, professional footballer, etc. to refer to their milieu and the sorts of activities they engage in. For this to be extended satisfactorily to science as an account of Kuhn's

¹⁰A recent example may be Luis and Walter Alvarez' meteor impact theory of the K-T extinction. Neither was a palaeontologist—Luis was a physicist and Walter is a geologist. Until their proposal most palaeontologists were gradualists who tended to favour evolutionary or ecological explanations. Nonetheless, the Alvarez team were not complete outsiders—most palaeontologists have a background in geology, and the palaeontologist Dale Russell had already suggested in the 1960s that the dinosaurs might have been killed off by radiation from an exploding supernova.

notion of world, we need to show that (i) incommensurability and difference of world relate to one another, so that, roughly, scientists experience incommensurability when they are operating in different worlds; (ii) worlds have a high degree of stability and are resistant to change, but may undergo the dramatic changes associated with revolutions; (iii) that worlds have a characteristic phenomenology, in that world-change may be likened to a Gestalt switch and encountering a different world leads to a particular sense of unfamiliarity and bafflement.

The proposal is then that a scientist's world is his or her scientist's disciplinary matrix—the constellation of professional relations and commitments, instrumental and theoretical techniques, disciplinary language, and, above all, the discipline's key exemplars. And in the light of the discussion hitherto, I shall highlight an especially important and psychologically salient component of a scientist's world. This is the set of schemata, analogies, similarity spaces, pattern-recognitional capacities, and quasi-intuitive inferences that govern a scientist's response to scientific problems. Nonetheless, I do not think that a world is necessarily limited to the latter (as I suggested in Bird 2005). I think that the key to a world is principally *entrenchment*, the difficulty with which a belief, commitment, or practice can be changed. And perfectly conscious, theoretical commitments can be among these as well as tacit assumptions. Something may be entrenched because it is unconscious and difficult to retrieve or it may be entrenched because of its central role in informing a wide range of activities. That said, the distinction is not a sharp one, not least because a belief may be, for the same scientist, a conscious theoretical assertion in one context and a tacit assumption or element of a schema in another. Thus a scientist may have an explicit commitment to, say Newton's laws of motion, or the theory of evolution through natural selection and will often mention and explain those commitments in their work. At the same time, those commitments will also be reflected in the models and analogs the scientists use and in the schemata they possess.

The proposal clearly meets the desiderata for a conception of world. First, world-change will tend to bring with it the possibility of incommensurability. The account of incommensurability I am promoting explains it by recourse to non-formal, implicit mechanisms of thought (analogical thinking, schemata, quasi-intuitive inference) that form a central part of a world, and so changes in the latter aspects of a world will thus give rise to possible incommensurability. As just mentioned a world-change may and typically will involve explicit commitments and that will require a reordering of many overt activities of the scientist. Many aspects of a scientist's professional life may change (the professional relationships engaged in, the equipment and techniques used, as well as the theories defended and employed). On their own these changes need not imply the possibility of incommensurability. But almost inevitably they will, for the reason given above, that central explicit commitments inform our tacit assumptions, schemata, and so forth.

Secondly, world-change correlates with revolutionary change. Indeed this is a trivial consequence of the definition of world in terms of the disciplinary matrix. More important, the entrenchment of central features of the disciplinary matrix explains the conservativeness of normal science and the resistance of science to change. Our unconscious commitments may be difficult to retrieve and alter and our conscious commitments may be so central to our activities that it may be difficult to make all the corollary

changes required by changes to those commitments. It is also true that the professional interests of well established scientists may give them an interest in resisting change. That point is certainly true, but is far from the only source of conservativeness. Another, in particular, is the role that our entrenched tacit commitments, as built into schemata and the like, play in making it difficult to engage in revolutionary change. Since these also give rise to incommensurability, those whose commitments are especially deeply entrenched may simply not be able to see the sense of the proposed change. Older scientists may well have a greater interest in the status quo. But they will also have their world more deeply entrenched and will be more likely to fall victim to the barrier to understanding erected by incommensurability, whereas the younger scientists may have their worlds less deeply entrenched.

Thirdly, we can attribute a particular phenomenology to world-change. Kuhn likened world-change to Gestalt shifts. As he acknowledged this analogy is misleading. But it is not altogether inappropriate. To begin with, Gestalt shifts are a matter of seeing one pattern and then another, and to the extent that scientific thinking involves pattern recognition, it is reasonable to consider changes in the patterns that one's exemplars, analogies, schemata and so forth permit one to see as either analogous to Gestalt shifts or perhaps even parallel instances of some general pattern recognitional capacity. Secondly, Gestalt shifts have a characteristic phenomenology. The picture now looks different. One of the problems for Kuhn in *The Structure of Scientific Revolutions* is that he took his psychological examples from perceptual psychology, as did his predecessors, such as Hanson. Thus the emphasis in the discussion of incommensurability gave the impression that a major component is perceptual—perception and thus observation are different as a result of a revolution. But this has significant consequences for science only if one shares the logical empiricist emphasis on observation-as-perception as an epistemological foundation. As a critique of logical empiricism that may arguably be effective. But as an understanding of an objective phenomenon of incommensurability it is very limiting, since many revolutions do not involve any change in perceptual experience. Nonetheless, I think that there is a more general analogue to the Gestalt shift, in which one's 'perception' of the world changes. In this sense 'perception' refers to one's (quasi-)intuitive responses to the world. This is an appropriate extension of the strict notion of 'perception' and one which is standard in everyday English, since the information one gleans from the world is not limited simply to the sensory experience one has but includes, at least, also what one automatically and naturally infers. Indeed to allow a fairly extended process of inference fits with Kuhn's reminder that observation is quite different from perception.

Nor is it inappropriate to attach the tag 'phenomenological' to such responses and inferences because even if the mechanics of the inference are tacit, its products—a belief, an action an emotion, etc.—are parts of one's conscious experience and understanding of the world. As the phenomenological tradition since Husserl has emphasized, how we experience the world is much richer, much more imbued with meaning, than a focus on sensory experience would suggest.¹¹ The proposal here is that this is

¹¹Phenomenology has been disadvantaged by its anti-naturalism and its anti-psychologism. Were it to take a greater interest in the psychological mechanisms whereby the world as we experience it is imbued with meaning, it might both add credibility to its claims and at the same time discover a tool for making those claims more precise and rigorous.

explained by the fact that our quasi-intuitive responses to the world add content and are, in a sense, interpretative (we see things in their theoretical and causal relations with other things).

Corresponding to the fact that a world brings with it a characteristic phenomenology is the fact that a world-change itself will have a phenomenological character. The same features of the world will now elicit different responses, a new phenomenology. What was unfamiliar, may now be familiar; what was previously significant may not be insignificant, or vice-versa. Familiar things may be seen as standing in different relations with one another, and crucially for science problems may be seen as having solutions that could not have occurred to the scientist beforehand. Kuhn mentioned religious conversion in this connection, again attracting controversy. But the analogy has two worthwhile features. First is the fact that a change in disciplinary matrix cannot always be brought about by straightforward explicit reasoning but may require indirect techniques such as thought experiments and other rhetorical manoeuvres (as discussed above). Secondly, a religious conversion will cause someone to have a different phenomenology (e.g. one sees things as the creations of God, or sees actions in terms of theological good and evil, etc.). Such a change can be sudden and can be striking, even in the scientific context where adopting a new disciplinary matrix, for example Newtonian mechanics in the early 18th century, may allow one to see a vast range of phenomena in new (Newtonian) terms. In such cases one may acquire a particularly fruitful, widely applicable set of schemata.

A superficially rather different approach to world-change is the dynamic, neo-Kantian one developed by Paul Hoyningen-Huene (1993). Kuhn (1991: 12) described himself as a Kantian with moveable categories. The idea is that in Kant the categories of thought and the nature of the forms of intuition are fixed parts of human nature. Kuhn's proposal is that they are not fixed but changeable and in particular are changeable in response to revolutionary changes in scientific commitments. Corresponding to Kant's phenomena and noumena (things-in-themselves), Hoyningen-Huene draws a distinction between the phenomenal world and the world-in-itself. What changes when there is a Kuhnian world-change is the former, while the world-in-itself stays fixed.

While I used to think that this was a very different understanding of world-change, I am now inclined to think that this neo-Kantian view and my naturalistic one may be reconciled. If the structure and form of intuition and the categories do not stay fixed, but may change as our paradigms change, we may then ask *how* they change? By what means do paradigm changes affect intuition and the categories of thought? Only psychology can answer that question, and the discussion above touches on the many contributions psychologists can be considered to have made to that problem. As Hoyningen-Huene points out, this combined naturalistic neo-Kantian Kuhn faces a tension in that a properly conducted psychology, especially if it engages with neuroscience, as Kuhn assumed it must, will be talking about entities and processes that lie more on the side of the world-in-itself than the phenomenal world. Kant and Kuhn were both sceptics about knowledge of things-in-themselves. This is the primary obstacle to developing a combined view. However, if the world-in-itself is just whatever lies beyond the phenomenal world, I do not see that we *must* be sceptical about it. That is, one can make a perfectly good distinction without declaring one half of it to be un-

knowable (nor the other half to be perfectly knowable). So, maybe, one could develop a naturalistic, neo-Kantianism without built in scepticism vis-à-vis the world-in-itself.

8 Conclusion

The proposal in this paper cannot yet claim to be a theory of incommensurability, but may reasonably hope to have indicated what some of the elements of such a theory might be. A fully worked out theory would have to have two phases. First it would have to characterize carefully the phenomenon of incommensurability. By this I mean we would need to identify certain episodes in the history of science or in communication between contemporary scientists as sharing a set of features, primarily those demonstrating some kind of failure of comparison or of understanding, such that we can reasonably attach the label ‘instances of incommensurability’ to them. Preferably one would do this without hypothesizing about the causes of such cases. Rather the point of this part of the process is principally descriptive. Secondly the theory would elaborate on the various psychological mechanisms mentioned above as possible causes of the instances of incommensurability described in the first phase. Neither phase has been worked out in sufficient detail as yet. While Kuhn and Feyerabend were happy to talk at length about incommensurability, they provided little in the way of detailed historical examination of any instances of it. Furthermore, those cases they do describe are described in terms of their own theories, so it is difficult to tell whether those instances do indeed support those theories or are perhaps better explained by some other theory. There is room also for further work on the psychology of the interaction between contemporary scientists working on a common problem but from different fields. This could yield some valuable data, but the work has not been done as far as I am aware. There is considerable work on the psychology of analogical thinking, but as we have seen, in its application to science Dunbar has focussed on the large scale rather than the micro-psychological functioning of analogies in scientific cognitive processes, and in particular we need more data on specifically scientific schemata. So while many of the pieces are available, more work needs to be done on some, and in particular on the interaction between the various pieces. What is significant is that a topic that has been treated in a largely aprioristic, philosophical fashion now needs significant input from a variety of directions and disciplines, from history and sociology of science to various subfields within psychology and cognitive science. The role of philosophy of science is no longer to answer the problem itself but to co-ordinate the contributions of disparate disciplines.

I conclude by noting another distinctive feature of this programme. The incommensurability thesis has been traditionally conceived of as a thesis concerning theories and the languages in which they are couched. For example, Kuhn’s (1983; 1991) taxonomic approach to incommensurability identified it with overlapping, non-congruent taxonomies employed by different theories, being his last (published) account of incommensurability following on from a sequence of linguistically and conceptually orientated approaches (Sankey 1993, 1998). If the psychological account above is along the right lines, then the concentration on theories, concepts, and languages is at least

in part misplaced, for it is scientists and scientific communities (or, to be more precise, their cognitive habits) that are incommensurable.¹²

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¹²This is not to say that the approach proposed is inconsistent with any elaboration of semantic incommensurability. The discussion of Section 5 itself is amenable to development in that direction. In my view Kuhn's philosophical accounts of semantic incommensurability have not been successful. Nonetheless more recently Andersen et al. (2006) (and in various articles since their (1996)) have developed a naturalistic account of semantic incommensurability informed by cognitive science that could be thought to parallel my account of psychological incommensurability. That said the two approaches are logically independent—the present approach aims in part to show that one *can* understand incommensurability without considering issues of language and meaning. I note finally that the central place of the latter in human cognition and psychology does not entail that every significant cognitive phenomenon needs a linguistic explanation; incommensurability may well turn out to be such a case.

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