INCOMMENSURABILITY — AN OVERVIEW[*]

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1. INTRODUCTION

We are here to assess the merits and current fortunes of one of the most controversial theses to emerge in the philosophy of science in the latter half of the twentieth century: the thesis of the incommensurability of scientific theories. The controversy about incommensurability dates to the year 1962, the year in which the thesis of incommensurability was first explicitly proposed by its two chief advocates, Paul Feyerabend and Thomas Kuhn.

It is convenient to treat the year 1962 as the year in which the incommensurability thesis first emerged because that is when the thesis was first asserted in print by Kuhn and Feyerabend. But this is an oversimplification. Kuhn and Feyerabend drew on earlier developments in the philosophy and history of science as well as in philosophy at large. In many respects, the incommensurability thesis is a product of the philosophical climate of the late 1950's and early 1960's. This was a time that saw the rise of the discipline of the history of science, the influence of Gestalt psychology on the philosophy of perception, the decline of Vienna Circle positivism, the influence of the later Wittgenstein and Quine's attack on the analytic/synthetic distinction.

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Apart from being a product of its time, the incommensurability thesis is also one of the characteristic claims of a new movement in the philosophy of science that began to emerge in the late 1950's and early 1960's. Together with the thesis of the theory-dependence of observation, the rejection of a fixed scientific method, and insistence on the importance of the history of science to the philosophy of science, the incommensurability thesis is one of the leading claims of what came to be known as the *post-positivist* or *historical* philosophy of science.

2. WHAT IS INCOMMENSURABILITY?

Before turning to the *thesis* of incommensurability, let me sound a note of caution regarding the *concept* of incommensurability itself. Productive discussion of the incommensurability thesis may be impeded by lack of consistent use or clear meaning of the term 'incommensurability'. The term has a standard use in mathematics, where it implies the absence of a common unit of measurement. To say that two magnitudes are incommensurable is to say that there is no common unit of measurement, whole units of which may be used to measure both magnitudes. But application of the mathematical concept to the case of alternative scientific theories is an extension of the concept that leaves considerable scope for variant interpretation.

Discussion of the incommensurability of scientific theories rarely proceeds in accord with the mathematical concept of incommensurability. Instead, discussion of incommensurability tends to be framed in terms of a range of concepts and considerations of a broadly semantic and epistemological nature. The discussion is frequently couched, for example, in terms of such factors as the incomparability of the content of scientific theories, variation in the meaning of scientific terms, translation failure between the vocabulary of theories, or absence of common standards of theory appraisal.

This raises the question of the relationship between the concept of incommensurability in the strict sense of lack of a common measure, and the various

other claims which have framed the discussion of the incommensurability thesis. Is the incommensurability of scientific theories some single, unified relation between theories, of which the various associated factors constitute mere aspects or component parts? Or is it instead the case that there are a number of different things, such as the incomparability of the content of theories, or lack of shared evaluative standards, which are each a source of incommensurability in their own right?

To answer this question one way or the other is already to take a side in the dispute. The question of how to apply the concept of incommensurability in the present context is itself one of the questions at stake. Some parties to the dispute take incommensurability to be a relation that may obtain in its own right between theories, of which such things as meaning variance and lack of shared evaluative standards are mere aspects or constitutive parts. In contrast, other parties to the dispute treat the claim of incommensurability as consisting entirely in one or another of the various claims associated with talk of incommensurability, such as the claim that the content of alternative theories is unable to be compared due to meaning variance of the terms employed by the theories. Given such potential variation in use, it is important to bear in mind that not all parties to the dispute may understand the concept of incommensurability in the same way.

3. ARE THERE DIFFERENT FORMS OF INCOMMENSURABILITY?

Let me turn now to the *thesis* of incommensurability. If one takes an overview of the critical literature concerned with the incommensurability thesis, it can hardly escape notice that this literature contains a variety of separate discussions that are conducted in quite different terms. Some authors write about the topic of meaning variance and content comparison. Some write about conceptual change and the intelligibility of alternative conceptual schemes. Others write about scientific realism and the continuity of reference of theoretical terms. And still others are concerned with the rationality of scientific theory choice, and the availability of objective standards of theory evaluation.

The need to address a variety of issues under the heading of incommensurability owes much to the original discussion by Kuhn and Feyerabend. In 'Explanation, Reduction and Empiricism' (1962), Feyerabend took incommensurability to consist in absence of logical relations due to semantic variance of the terms used by theories, resulting in the inability to directly compare the content of theories. By contrast, in *The Structure of Scientific Revolutions*, Kuhn treats incommensurability as a multi-dimensional relationship between paradigms, which involves methodological, semantic and perceptual components. According to Kuhn, paradigms employ diverse standards of theory appraisal, and address different sets of scientific problems. The vocabulary employed by scientists changes meaning in the transition between paradigms. Scientists in rival paradigms perceive the world differently. Perhaps they even inhabit different worlds.

With so many themes already present in Kuhn's and Feyerabend's original discussion, it is no wonder that a host of issues emerged when philosophers turned to the topic. To impose order on the discussion, I will distinguish between two versions of the incommensurability thesis. The first version, which I call the *semantic incommensurability thesis*, is the thesis that alternative scientific theories may be incommensurable due to semantic variance of the terms employed by theories. The

second version, which I call the *methodological incommensurability thesis*, is the thesis that alternative scientific theories may be incommensurable due to absence of common standards of theory appraisal.

I will now sketch the main developments that have taken place in connection with each of these two versions of the incommensurability thesis. I will conclude by briefly mentioning a number of significant directions being taken by current research on the topic.

4. SEMANTIC INCOMMENSURABILITY

The thesis of semantic incommensurability derives from the claim of Kuhn and Feyerabend that the meaning of the terms employed by theories varies with theoretical context. Both authors reject the empiricist idea of an independently meaningful, theory-neutral observation language. Instead, they claim that the meaning of the terms employed by scientific theories depends on the theoretical context in which the vocabulary is employed. Given the contextual nature of meaning, the meaning of scientific terms is subject to variation with the theory in which they occur.

The thesis of meaning variance gives rise to the thesis of semantic incommensurability in the following way. Because the meaning of the terms employed by scientific theories varies with theoretical context, the vocabulary of such theories may fail to share common meaning. But if theories are unable to be expressed by means of a common vocabulary, the content of such theories cannot be directly compared. For in the absence of a shared, semantically neutral vocabulary, it is impossible for statements about the world asserted by one theory to either assert or deny the same thing as any statement made by the other theory. Theories which are unable in this way either to agree or disagree with respect to any claim about the world are incommensurable in the sense that their content is unable to be directly compared due to semantic variance.

For simplicity, I have formulated the thesis of semantic incommensurability in terms of radical meaning variance. However, a more limited version of the thesis may also be formulated in terms of partial meaning variance restricted to a limited portion of the vocabulary employed by theories.

Response to the semantic incommensurability thesis divides into two main lines of criticism. On the one hand, advocates of what I call the *referential response* argue that there are relations of co-reference between the terms of meaning variant theories which suffice for content comparison. On the other hand, advocates of what I call the *translational response* argue that the idea of an untranslatable language, to which the meaning variance thesis gives rise, is an idea of which no coherent sense can be made. I will first discuss the referential response to semantic incommensurability.

The referential response was presented by Israel Scheffler in his 1967 book, *Science and Subjectivity*. Employing a Fregean distinction between sense and reference, Scheffler pointed out that, even if the sense of a scientific term varies with theory, it does not follow that the reference of the term also varies with theory. Terms may co-refer but differ in sense. But if terms employed by theories preserve reference through variation of sense, it remains possible to compare the theories with respect to content. For statements which theories make about the world may agree or disagree with respect to common states of affairs, provided only that their constituent terms refer to the same things, despite variation of sense.

But Scheffler's point that reference need not co-vary with sense was not enough to settle the issue. On the one hand, there are a number of historical cases in which different theories seem to employ the same terms to refer to different things. On the other hand, the existence of radical conceptual change in science suggests that there has been widespread discontinuity of reference in the history of science, since radical conceptual change seems to imply variation of reference. Thus, while common reference may well

suffice for the comparability of content, it remains to be shown how continuity of reference may be sustained in the transition between meaning variant theories.

Rather than settle the issue, therefore, Scheffler's appeal to reference serves merely to shift the focus to the issue of reference. For it raises the question of how terms which occur in different theories may preserve reference through variation in the conceptual content which theories associate with them. More specifically, it raises the question of the determination of the reference of the terms that are employed by scientific theories.

Where Scheffler formulated the referential response in terms of a Fregean theory of reference, the subsequent emergence of the causal theory of reference seemed to offer a promising resolution of the issue. It was suggested by such authors as Saul Kripke and Hilary Putnam that reference is determined in a direct manner by means of causal relations between speaker and object, rather than by the descriptive content which speakers associate with the terms they employ. On such a view, reference is fixed at the initial introduction of a term, and the reference of later use of the term traces back by a historical chain to its original use. Given that reference is determined independently of descriptive content, the reference of terms employed by scientific theories may be preserved despite variation in the concepts associated with such terms. But if reference remains stable through variation of conceptual content, no problem of theory comparison arises, since reference is preserved even though terms may be associated with divergent conceptual content in the context of alternative theories.

So simple a resolution of the issue was, of course, too good to be true. There are several difficulties facing the causal theory which prevent employing it in unmodified form to sustain the referential response. First, it is implausible to suppose that reference is permanently fixed at the initial introduction of a term, since this excludes in principle the possibility of change of reference in the history of science. Second, if the reference of natural kind terms is fixed by entirely non-descriptive means — e.g., by ostension of

samples of a kind — then it is impossible to secure unambiguous reference to a specific natural kind as opposed to the numerous other kinds instantiated by the sample set. Third, if the reference of theoretical terms is determined by means of a causal relation between observed phenomena and the entities responsible for the phenomena, it would be impossible ever for theoretical terms not to refer. Yet failure of reference would appear to be a routine occurrence in the history of science.

Such problems suggest that the causal theory of reference must be modified to allow variation in reference subsequent to initial term-introduction, as well as to include a role for descriptive content in the determination of reference. This has led to the development of various modified versions of the causal theory, such as the causal descriptive theory of reference. But such modified versions fail to deliver results as unequivocally contrary to the incommensurability thesis as was initially promised by the causal theory of reference in its original form.

I turn now to the translational response to semantic incommensurability. This response is directed against an implication of the meaning variance thesis that there may be translation failure between the vocabulary employed by incommensurable theories. The thesis of radical meaning variance suggests that the terms employed to express a theory might be unable to be translated by means of any of the terms employed by an alternative theory with which it is incommensurable. Taken to the extreme, meaning variance suggests that there might even be entire languages that fail to be intertranslatable.

The idea of an untranslatable language has seemed paradoxical to many philosophers. For if one cannot translate a foreign language at all, then what evidence can there be that what fails to be translated is in fact a language? Failure to translate is indeterminate between being evidence that a language is untranslatable and that it is not a language at all. If one provides, as evidence of untranslatability, examples of expressions of the untranslatable language, then the very act of providing such examples undermines

the claim of untranslatability, since to present the examples *within* one's own language presupposes translation. Still worse, even to profess to understand what is said in an untranslatable language seems to imply the *tran*slatability of the language, since understanding a foreign language seems to presuppose translation into a language that one understands.

Such thoughts as these lie at the heart of Donald Davidson's famous article, 'On the Very Idea of a Conceptual Scheme' (1973/4). The thrust of Davidson's critique, however, is not restricted to semantic incommensurability. He was "after larger game". According to Davidson, to make sense of the idea of a language independent of translation requires a distinction between conceptual schemes and the content organized by such schemes. But, Davidson argues, no coherent sense can be made of the idea of a conceptual scheme. So no sense may be attached to the idea of an untranslatable language.

Some of the deep issues raised by Davidson may be avoided by taking into account two points Kuhn and Feyerabend make with respect to translation. First, in later work Kuhn developed a local version of incommensurability, which restricts translation failure to narrow clusters of interdefined terms from rival theories. Restricting untranslatability to such local clusters of terms, or even to the entire vocabulary of theories, removes the need to make coherent sense of either a totally untranslatable language or the scheme/content dualism. Second, both Kuhn and Feyerabend sought to distinguish between translating a language and understanding it. While one might fail to *translate* from a foreign language into one's own, it need not follow that one must fail to *understand* the other language. The combination of these two points yields a refined version of semantic incommensurability, on which translation failure is restricted to specialized vocabularies within a language, which are capable of being understood by rival theorists. Such a refined version seems an unlikely target for Davidson's attack.

5. METHODOLOGICAL INCOMMENSURABILITY

I will now discuss the thesis of *methodological incommensurability*. I will focus primarily on the development of Kuhn's views on this topic. According to the methodological incommensurability thesis, there are no shared, objective methodological standards of scientific theory appraisal. Hence, alternative scientific theories may be incommensurable due to absence of common methodological standards capable of adjudicating the choice between them.

Some authors do employ the term 'incommensurability' in such a methodological sense. However, this aspect of the issue is more commonly addressed under the rubric of rational scientific theory choice and relativism due to variation in methodological standards. Still, the point of departure for discussion of relativism and rational theory choice is often Kuhn's claim that standards of theory appraisal vary with paradigm, which was treated by Kuhn in *The Structure of Scientific Revolutions* as one of the constitutive aspects of the incommensurability of paradigms. Feyerabend's own critique of a fixed scientific method, which he did not present under the rubric of incommensurability, marks another key reference point in this aspect of the discussion.

The idea that scientific theories may be incommensurable in a methodological sense arises out of rejection of the traditional view that there is a uniform, invariant scientific method, employed throughout science, which is the distinguishing feature of science. In apparent opposition to this traditional view, Kuhn claimed in *The Structure of Scientific Revolutions* that standards of theory appraisal depend on and vary with the currently dominant scientific paradigm. "There is," he wrote, "no standard higher than the assent of the relevant community" (1970a, p.94). This created the impression that, for Kuhn, the rationality of scientific theory acceptance is relative to prior choice of scientific paradigm, and the choice of paradigm is incapable of being governed by shared objective standards of theory appraisal. For his part, Feyerabend argued both that the methods employed in science vary historically, and that all rules of scientific method have been justifiably violated at some stage in the history of science.

On the picture of scientific theory choice that emerges in Kuhn and Feyerabend, there is no fixed set of objective scientific standards to which appeal may be made to adjudicate the dispute between conflicting scientific theories. Consequently, the choice between alternative scientific theories is not a decision that may be made on the basis of common methodological standards accepted by all parties to the dispute. In short, the methodological views of Kuhn and Feyerabend appear to lead to a thorough-going epistemological relativism, on which scientists may rationally accept conflicting theories on the basis of alternative sets of methodological standards.

While Feyerabend did little to dispel the impression of relativism, Kuhn sought to distance himself from the relativistic implications of his original position. Kuhn had not meant to deny that the choice between alternative theories may be a rational process. He meant, rather, to insist that:

> There is no neutral algorithm for theory-choice, no systematic decision procedure which, properly applied, must lead each individual in the group to the same decision. (1970a, p.200)

Lacking such an algorithm, rational theory choice involves ineliminable elements of judgement and deliberation. Nor had Kuhn meant to deny the existence of fixed standards of theory appraisal. Indeed, he lists a number of such standards: e.g., accuracy, consistency, simplicity, scope and fruitfulness (1977b, p.322). Kuhn's point, rather, was that such standards "function not as rules, which determine choice, but as values, which influence it" (1977b, p.331). Moreover, he says, scientists "may legitimately differ about their application to concrete cases", and "when deployed together, they repeatedly prove to conflict with one another" (1977b, p.322). The result is that, despite adherence to a common set of standards, there may be rational disagreement between scientists who embrace opposing theories because they interpret or weight the same standards differently.

Kuhn's conception of rational disagreement governed by non-algorithmic standards offers a promising account of rational scientific theory choice. However, it leaves open a question which Kuhn was never able to resolve satisfactorily. This is the *meta*methodological question of the normative ground of standards of theory appraisal. Kuhn originally seemed to ground epistemic normativity in social consensus (cf. 1970a, p.94). However, at a later stage he sought to naturalize such normativity by grounding it in successful scientific practice (1970b, p.237). Still later he offered a conceptual grounding for scientific norms whose rationality he took to be analytically insured by the very concept of science (1983).

The issue of the metamethodological justification of epistemic norms has in recent years come into sharper focus as a result of critical discussion of the views of Larry Laudan. Laudan distinguishes between intuitionist, conventionalist and naturalist metamethodological stances, and argues that meeting the relativist challenge requires a naturalist metamethodology that grounds normative methodology in empirical facts about means to epistemic ends. Whatever the fate of Laudan's own normative naturalist alternative, resolution of the issues surrounding methodological incommensurability will require development of an adequate metamethodological theory of the warrant of methodological norms.

6. NEW DIRECTIONS IN THE DEBATE

I first began work on the topic of incommensurability in 1982. At that time, the key issues appeared to be those raised by the causal theory of reference and Davidson's attack on the idea of a conceptual scheme. I adopted a causal descriptive account of reference that grants a partial reference-determining role to descriptions and allows reference-fixing apart from original term-introductions (1991). On the basis of such an account, I argued for the comparability of the content of semantically variant theories and for translation failure between theories due to limits on the means of reference determination within the

special vocabularies of theories (1994). If I were starting work on the problem today, I would find myself faced with a very different situation. Let me conclude by briefly noting some significant directions currently being pursued in relation to the topic.

First, over the last decade of his career Kuhn elaborated the philosophical aspects of his theory of science with increasing sophistication and refinement. Of special relevance is the version of semantic incommensurability that I call *taxonomic incommensurability* (1998). According to the taxonomic version of the thesis, incommensurable theories employ different systems of taxonomic categories; the narrow set of terms which refer to these categories is holistically interdefined; and there is localized translation failure between these sets of interdefined terms. Taxonomic incommensurability marks an improvement over Kuhn's original treatment of semantic aspects of incommensurability. It seems to be immune to such telling objections against the original doctrine as Davidson's critique of conceptual schemes. It also raises interesting challenges, including some intricate issues about change of reference of scientific kind terms.

The second notable development is Ian Hacking's approach to what he calls Kuhn's "new-world" problem. Hacking sets Kuhn's talk of "world-change" and taxonomic change within the framework of nominalist metaphysics. Rather than say that the world itself changes, Hacking proposes a revolutionary nominalism, according to which no deep-level kinds exist in nature, so that what changes with theory are the classificatory systems that different theories impose on the world. Kuhn rejected Hacking's nominalist interpretation because it is not simply the kinds to which objects belong, but the objects themselves, that vary with classificatory scheme (1993, p.315). But Hacking's nominalism remains of interest in its own right, since it preserves the semantic aspects of taxonomic incommensurability within a metaphysical framework not committed to "world-change" in any robust sense. It also raises interesting questions about the status of natural kinds and their relation to systems of classification.

Speaking of "world-change" brings me to the third significant development, that of our host here in Hanover. In his book, *Reconstructing Scientific Revolutions*, Paul Hoyningen-Huene proposes an interpretation of Kuhn's metaphysics as a sort of dynamic Kantianism. What changes in the transition between scientific theories are "phenomenal worlds", rather than the "world-in-itself". Phenomenal worlds are jointly co-constituted out of "genetically object-sided moments" — input from the "world-in-itself" — and "genetically subject-sided moments"— the contribution of the human subject. Hoyningen-Huene's interpretation draws various aspects of Kuhn's position together into a coherent package, making insightful use of some neglected remarks about language-acquisition made by Kuhn in his 'Second Thoughts on Paradigms' (1977a). At the same time, Hoyningen-Huene's neo-Kantian interpretation of Kuhn shifts the discussion into a deep dispute between realist and Kantian metaphysics.

Fourth, work on semantic aspects of incommensurability is increasingly informed by cognitive science. A growing body of research employs the tools of cognitive psychology to analyze the processes of conceptual change in science. Following the work of such psychologists as Eleanor Rosch, the classical view of concepts as defined by necessary and sufficient conditions has been rejected in favour of the view that concepts are represented by prototypical examples and have a graded structure. In addition, Lawrence Barsalou's *dynamic frame* model of concept representation has been fruitfully applied to scientific conceptual change. Analysis of scientific concepts in terms of frames of hierarchically structured features has led to fine-grained analysis of a variety of historical cases of conceptual change. The frame-based approach to conceptual change fits well with Kuhn's later, taxonomic conception of incommensurability, as well as with Hoyningen-Huene's interpretation of Kuhn's account of concept-acquisition.

The final development to mention is a historical one. Recent research on the history of 20th century philosophy of science has led to re-evaluation of the relationship between positivist and post-positivist philosophy of science. Some studies suggest that

logical positivism had more in common than previously supposed with Kant than with British empiricism. Other studies show that the logical empiricist double-language model contained the seeds of the thesis of meaning variance. Still others make much of Carnap's sympathetic reception of *The Structure of Scientific Revolutions* in his capacity as editor of the *Encyclopedia of Unifed Science*. Indeed, striking parallels have been drawn between the views of Carnap and Kuhn in seeking to explain this reception. At the very least, these studies suggest that the incommensurability thesis can no longer be regarded as a thesis which crystallizes the differences between diametrically opposed positivist and post-positivist positions.

7. FINALE

In these opening remarks, I have sought to chart the paths that have led to the current state of discussion of incommensurability, as well as to indicate a number of the directions that the debate is currently taking. Participants at this conference represent virtually every branch of the discussion. It is both my hope and belief that where the discussion goes from here will in large part be decided by what takes place here in Hanover over the next four days.

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