HAROLD I. BROWN

INCOMMENSURABILITY AND REALITY

Abstract. Realists hold that science seeks to discover entities and process that exist in nature independently of whether these are accessible to our senses. This pursuit requires the development of new concepts as part of the research process, and thus requires learning new modes of thought, which is the basic source of incommensurability. I sketch a theory of conceptual content that explains how new concepts are introduced as modifications of existing concepts, and how the resulting continuities allow innovators to promote new ideas in a coherent manner. An account of evidence is proposed that explains how items under study constrain the choice of concepts, thereby limiting the scope of incommensurability and promoting pursuit of the realist goal.

My aim in this paper is to argue for a pair of theses about the role of incommensurability in the development of science. I will, however, put off formulation of these theses until the end of the paper and devote most of my space to discussing four key notions: incommensurability, concepts, scientific realism, and external evidence. The main purpose of these discussions will be to defend some substantive positions on debated issues. Thus while I will discuss various positions that have been held on these topics, I will not be concerned with detailed explication of existing views. By the time I am ready to formulate my main theses, most of the arguments on their behalf will already be in place.

1. INCOMMENSURABILITY

I want to begin with an initial sketch of incommensurability. These remarks will be preliminary since they will make use of notions that I will discuss below, so I will further clarify my account of incommensurability as we proceed.

Incommensurability can appear within a scientific field when a new approach requires ways of thinking that are significantly different from established ways of thinking. Typically the new approach has features that are incompatible with aspects of the older approach. A new approach may involve elimination of older ways of thinking, introduction of new ways of thinking, or a combination of the two. There are at least two kinds of changes in scientific thinking that generate problems of incommensurability: conceptual change and changes in scientific methodology, by which I mean changes in the standards for evaluating the relevance and acceptability of scientific claims.[1] In this paper I will focus on cases of incommensurability that derive from conceptual change.

Incommensurability occurs only when two approaches to a single subject matter are in competition. Distinct frameworks – such as the rules of basketball and the standard

P. Hoyningen-Huene and H. Sankey (eds.). Incommensurability and Related Matters, 123–142.

model in physics – regularly exist together in a culture and in an individual mind without generating the problems addressed under the incommensurability rubric because there is no reason to choose between them. The means by which we recognize a genuine dispute are highly contextual, but in practice there is little difficulty – as the examples that follow will indicate.

Notice that I am approaching incommensurability in cognitive terms: The issues in question arise when people must learn to think about a subject matter in new ways. The need for new modes of thought is a pervasive feature of the development of human knowledge. As Descartes emphasized, it is an unhappy fact about us that we are not omniscient; if we were, we would have no need for research or for a methodology for evaluating knowledge claims. Moreover, as advocates of naturalistic epistemology regularly point out, the appropriate methodology for developing human knowledge depends on the kind of cognitive equipment we have available. For example, sense perception plays a central role in epistemology because we get our information about the items around us by means of our senses.[2] If we were beings of a different sort – say, beings that get information via a form of radiation that directly modifies neural tissue, or beings that already possess all truths in a dim form and need only meditate properly to draw them out – a significant portion of our epistemology would be addressing different issues and need different approaches. By the same token, questions about the nature and consequences of conceptual change arise because reflection on individual human development and human cognitive history show pretty clearly that conceptual change occurs. The approach I am taking to the development of science is a natural extension of the traditional empiricist view that we have no direct insight into the nature of reality, and includes in this view the thesis that we have no direct insight into the appropriate concepts for thinking about any subject. The development of appropriate concepts is one of the tasks that science pursues.[3]

Since I am going to focus on conceptual change, it is important that we keep in mind how pervasive such change is in the development of science. Most discussions in the literature turn on a few important examples, but there is a much larger set of concepts in contemporary science and mathematics that was developed over the history of these subjects, and that have to be learned in the course of an individual life. Consider just a few examples of scientific and mathematical concepts whose history we can trace: ultraviolet radiation, boson, isotope, isomer, gene, telomere, logarithm, gamma function, partial derivative, and on and on. Understanding each of these concepts requires mastery of many other concepts that have been introduced in the course of our history. Nor is the development of science the only source of new concepts. Examples based on technological developments include carburetor and surrogate mother, while other fields have provided such concepts as split infinitive, separable prefix, futures option, feudalism, home run, and secretary of state.[4] I will, however, stick to scientific examples in this paper.

Conceptual change also includes cases in which concepts are dropped. It is more difficult to list concepts that no longer occur in science than to list those that do occur, so I will mention some stock examples – natural place, celestial spheres, phlogiston, and caloric – to introduce the point. However, historical research on specific fields shows that there are many other cases in which researchers introduced new concepts and then dropped them before they became part of a scientific tradition. For example, from 1901–1903 Rutherford and Soddy were studying thorium in their attempt to understand radioactivity. At one stage of this study they thought that thorium was not itself radioactive. Rather, they believed that thorium underwent a change that produced thorium X, which is the source of the radioactive radiations they detected. They succeeded in separating thorium X from thorium, but the remaining thorium retained about 25% of the radioactivity no matter how much thorium X they removed. To account for this, they postulated a second radioactive component that could not be separated from thorium by chemical procedures, and that provided the source of the residual radioactivity. A little earlier Becquerel had found similar behavior in the case of uranium. Soddy redid Becquerel's investigations and extended the account to this element. But the concepts of separable and inseparable radioactive constituents were short lived. With regard to these constituents, one historian remarks:

> Historically, such constituents belong to the same category as phlogiston and caloric. This is another example of a conclusion drawn on the basis of available evidence and within a theoretical framework ultimately found to be erroneous. But in April 1902 Rutherford and Soddy, having overcome their initial scepticism, fully embraced active constituents, both separable and inseparable, produced and made active in the process of transformation, as the general explanation of radioactivity. (Trenn, 1977, pp. 75–76)

This example is far from unique. Consider another phenomenon that was noted in early studies of radioactivity: A material placed near thorium or radium would itself become radioactive. The Curies explained this by postulating a form of radioactive induction on the model of magnetic induction. Rutherford and Soddy also noted that there were some substances that appeared only briefly in the course of radioactive change. They initially took these to be a new form of matter that they called 'metabolons'. *Radioactive induction* and *metabolons* were concepts that had rather short life-spans.[5]

It may be useful to mention a bit more of the historical context here. X-rays had been discovered in 1895 and radioactivity in 1896. By 1902 alpha rays and beta rays had been distinguished, and the latter identified as streams of electrons. It had not yet been established that alpha rays are material particles or that they carry an electrical charge – they were generally believed to be a kind of secondary X-ray caused by the beta rays.[6] Gamma radiation had been identified, but its significance was not clear and it played no role in the brief period I am discussing. Einstein's account of the relation between matter and energy, Rutherford's discovery of the nuclear structure of the atom, the concept of an isotope, Bohr's theory of the atom, and the discovery of protons and neutrons were all in the future. We are in the midst of a period of intensive research at the interface between chemistry and physics during which new concepts were being formulated, tried out, modified, and dropped. Similar examples can be found in the development of other fields.

My general point, then, is that conceptual development is a central feature of the ongoing human attempt to describe and understand the world we live in – and that we pursue this attempt without any *a priori* guidance. A brief consideration of some of the literature on incommensurability will help clarify why I want to invoke this concept here. Recall, first, that when Feyerabend and Kuhn introduced this notion there was a prevailing view of the nature of concepts that had been derived, with modifications, from the empiricist tradition. On this view, our concepts divide into two classes: basic concepts which get their content by direct association with observables, and auxiliary concepts that, somehow, get their content from the basic concepts. On the oldest version of this view, auxiliary concepts are constructs out of basic concepts, and are completely replaceable by combinations of these concepts. Thus auxiliary concepts are genuine 'auxiliaries': they are introduced for convenience, and are eliminable for purposes of epistemic analysis. However, the development of science led to the pervasive introduction of theoretical concepts, and these proved to be resistant to analysis in empiricist terms. Early in the twentieth century, philosophers attempted to resolve these concepts into basic concepts on the traditional model. This approach ran into serious difficulties, and was followed by a series of alternative accounts of the relation between theoretical concepts and observation concepts (cf. Brown, 1979, chapter 3). By the late 1950s there was a prevailing view which held that the empirical content of theoretical concepts derived from a combination of implicational relations among axioms plus correspondence rules that related these axioms to a set of observation concepts. The key point for present purposes is the central role that observation concepts played in empiricist philosophy of science. In particular, since this view held that the content of any concept is wholly determined by a combination of observation concepts and logical relations, the introduction of a new concept into science required only the reorganization of already available materials. In addition, logic and observation provided a smooth path by which one could learn new concepts.

Feyerabend and Kuhn challenged this entire approach when they challenged the existence of a special set of observation concepts. (See Khalidi, 2000 for a recent account.) Without the common medium provided by observation concepts, the psychological process of learning new concepts becomes rather more difficult than it seemed according to the prevailing view. Kuhn emphasized the central role that this lack of a common medium plays in his thought when he explained that he had taken the term 'incommensurability' from mathematics where it is used to express the lack of a common unit for measuring, for example, the side and diagonal of a square:

> Applied to the conceptual vocabulary deployed in and around a scientific theory, the term 'incommensurability' functions metaphorically. The phrase 'no common measure' becomes 'no common language'. The claim that two theories are incommensurable is then the claim that there is no language, neutral or otherwise, into which both theories, conceived as sets of sentences, can be translated without residue or loss. (Kuhn, 1983, p. 670; cf. Kuhn, 1977b, p. 301)

Kuhn went on to emphasize that this lack of a common measure does not imply that no comparison is possible, but that comparisons are of a different sort than comparing two different items expressed in a common language. The cognitive form of incommensurability that concerns me in this paper comes to the fore when we consider learning the new concepts. However this is done, it requires a different – and presumably more difficult – transition than would be involved if the new concept could be completely formulated in terms of already available concepts.

A linguistic example may help underline the psychological point. The possessive adjectives in English have an odd characteristic: In the third person singular (his, her, its) – and only in the third person singular – these adjectives carry information about the gender of the possessor. French (to take but one example) is more systematic; none of the possessive adjectives carry information about the gender of the possessor. To be sure, all French adjectives come in both masculine and feminine forms, but the gender of an adjective is determined by the gender of the noun being modified. The choice of a possessive adjective has nothing to do with the gender of the person whose possessions are being discussed. Everyone, whether male or female, says '*ma mère*' and '*mon père*', and there are no French words that play the same linguistic roles that 'his' and 'her' (in this use of this ambiguous word) play in English. Thus an English speaker cannot learn the French forms by simply translating between individual words in the two languages. In French it would be fairly easy to write a story that introduces a character by describing objects that the character possesses without revealing the character's gender. Translation of the story into English would not be straightforward. (See Kuhn, 1983, pp. 679–681 for further examples.) In psychological terms, this situation requires an English speaker learning French to learn some new patterns of thought without making a smooth transition via a simple mapping of one term onto another, or mapping both onto some third language. It is this psychological gap that I am focusing on in my use of the term 'incommensurability' and I suggest that this psychological gap is the fundamental phenomenon. Other issues that are commonly discussed under the incommensurability rubric, such as change of meaning and change of reference, are important but derivative from this more basic source. For example, one important form of reference change in the development of science involves reorganization of reference classes – such as when Copernicans included the earth in the same class as the planets, and the sun in the same class as the stars. But these reclassifications result from changes in the concepts being used to think about a particular domain.[7] I will have more to say about such cases shortly, but first I must explain how I am using the notion of a concept.

2. CONCEPTS

I want to approach the subject by recalling that when Putnam argues that meanings are not in the head, he distinguishes meanings from concepts which are in the head (Putnam, 1975, pp. 217–218, 226, 245, 248). I am concerned with concepts in just this sense: Concepts are mental entities that represent some items of interest. This use of the term 'concept' is common in cognitive science.[8] There is no settled account of how concepts are implemented in the brain, and conceptual systems are regularly studied as abstract structures with the understanding that an adequate account must eventually be cashed out in terms of neural structures (cf. Thagard, 1992, pp. 28–29). This is the approach that I will follow here.

Once we focus our attention on the development of knowledge, the need for conceptual change as science proceeds leaps to the mind. Consider one common mode of scientific research: scientists pick out items that they consider to be natural kinds and attempt to develop concepts that will describe their properties. As research continues, drastic changes may occur both in our understanding of which items are members of a single kind, and in the ways in which we think about these kinds. For example, at an early stage in the development of chemistry, long before modern notions of chemical structure appeared, some theorists picked out earth, water, air, and fire as basic kinds into which the physical world is divided, and introduced two pairs of contrary properties – hot/cold and wet/dry – as a basis for understanding the characteristic features of these elements. Water, for example, was described as the cold, wet element, and earth as the cold, dry element.[9] From a contemporary perspective these researchers made mistakes both in their selection of the natural kinds and in the concepts they used to describe them. When we select a set of items as members of a single kind we fix the referents of the concepts we seek to develop, but to select items as constituting a single kind is to propose a revisable hypothesis. The reorganization of reference classes is a common outcome of research on classes we have selected. Still, once such classes have been selected, researchers can formulate hypotheses as to their distinguishing characteristics. When they become confident that these hypotheses are correct, they build them into the concepts they use to describe the items in these classes. For the moment I want to emphasize that the early chemists' concept of, say, water as the cold, wet element was a genuine concept, even though we no longer consider the concept to be an adequate description of its subject matter. The same point holds for the concept of phlogiston, which was introduced at a later stage in the development of chemistry. Those educated in the appropriate traditions could have provided analyses of these concepts, and historians can now recapitulate those analyses. I am going to concentrate on concepts for most of this paper, but referents will eventually force their way into the discussion.[10]

In order to understand conceptual change and the way in which incommensurability is generated, we need an account of how conceptual content is determined. I have already sketched the prevailing view in philosophy of science when Kuhn and Feyerabend began talking about incommensurability, and I noted how logic and an observation language were thought to provide a medium in which the content of all concepts could be formulated. As a result, on this view incommensurability is always avoidable in principle.

However, with hindsight we can see that there is considerable scope for conceptual change even on the most traditional empiricist approach. These changes will occur on the level of the auxiliary concepts, but most of the concepts we use in everyday life and in science are auxiliary concepts. Human beings create these concepts in response to experience, and we transmit them to our children as part of the process of teaching them how to deal with their physical and social environments. On the usual empiricist epistemologies, auxiliary concepts embody beliefs about phenomena that tend to occur together in some regular fashion, and research can lead us to introduce new auxiliary concepts and eliminate others from our active conceptual repertoire. We can, for example, conclude that Hesperus and Phosphorous are two manifestations of a single entity, and replace the two distinct concepts with a single concept. We can find that jade or earth is not a single natural kind after all, and introduce new concepts to capture the relevant distinctions. Learning new auxiliary concepts will often involve learning new ways of thinking about and responding to experience, and may be quite as challenging psychologically as it is on alternative theories of concepts since people do not learn new concepts by building them up from observation concepts. In other words, even if the empiricist account of concepts were correct, the point remains that human thinking takes place mainly in terms of auxiliary concepts, where the challenges of adapting to new modes of thinking do arise.

Kuhn and Feyerabend rejected this view of scientific concepts, opting instead for a view that had long been in the literature, although of only limited popularity in the empiricist tradition. On their view, concepts occur only as members of systems of concepts, and conceptual content is determined by relations among these concepts. No concepts are intrinsically basic, and experience plays no role in determining conceptual content. Rather, when concepts are related to experience the concepts provide an interpretation of that experience.[11] One consequence of this approach is that changes in relations among concepts generate changes in all the concepts in a set. Since there is no universal set of basic concepts to which all other concepts are reducible, when a new set of concepts is introduced we often end up with two conceptual systems that are not mutually translatable. On this approach, then, it is no longer possible to avoid incommensurability even in principle.

It is important to be clear that the conceptual holism involved in the Kuhn-Feyerabend account is local: a system of concepts is peculiar to a specific scientific subject. We are not talking here about a single massive system that includes all of an individual's concepts, or all of the concepts prevalent in a society, or even all the concepts of physics. A full account of how local holism works is too complex to be developed here, but I want to note some key points. Research fields overlap in a variety of ways, but there is also a considerable range in which fields and sub-fields are conceptually independent. Evolutionary biologists need not attend to the relativistic concepts of space and time; considerable change can take place in high-energy physics without a direct impact on some areas of physics, or on biology, geology, or psychology – although as consequences are recognized, a change in any one of these fields may have impacts on any of the others. Changes at the most fundamental levels of a hierarchical structure will have immediate impacts on many fields. So, a change in the conceptual structure of special relativity or basic quantum mechanics will directly impact much of physics and many related fields. Still, the point remains that there is a significant degree of conceptual isolation between various disciplines and sub-disciplines. That Kuhn was aware of this point is indicated by various remarks about the importance of mini-revolutions that affect only a small number of practitioners in a sub-discipline (cf. Kuhn, 1970, pp. 6–7, 180–181; see also Kuhn, 1983, pp. 670–671). Indeed, I think that both Kuhn and Feyerabend were clear that they were discussing only concepts in specific areas of science, not global conceptual systems.[12] People typically wield several independent conceptual systems. It is not unusual to find a single individual who has mastered concepts involved in relativity physics, gardening, antique collecting, and chess. In addition, a single individual may have two incompatible conceptual systems available, and shift between them as the need arises – a point that should be familiar to anyone who has studied both relativity and classical mechanics (cf. Kuhn, 1983).

The holistic approach to concepts opens up possibilities for conceptual innovation that are not available in standard empiricist theories of concepts exactly because holism does not recognize a set of intrinsically basic concepts. Still, I think that the Kuhn-Feyerabend version of holism is not sufficiently rich to account for the varieties of concepts that we use or for the fine structure of the process of conceptual change. I want, then, to sketch a more complex form of local holism which I will draw on in the remainder of this paper.[13]

The content of scientific concepts is determined, I propose, along three dimensions. Kuhn and Feyerabend emphasized the *first* of these dimensions: implications among the concepts in a system. The need for a *second* dimension derives from the point that central scientific concepts are introduced to describe extra-conceptual items – that is, items that exist (if they exist at all) independently of the concepts we use to represent them.[14] It is, however, a familiar point that a set of concepts specified only by mutual relations can be given many different extra-systemic interpretations. A full-blown system of descriptive concepts must include some additional element that relates it to a specific subject matter. This additional element plays a role in determining the content of those concepts and requires some further discussion.[15]

I suggest that this second feature is a set of criteria for assessing whether a concept is instantiated. In many cases this role is played by criteria for picking out instances of the concept in question; this is typically the case for items that are easily observable or have easily observable properties. But there are cases in which this approach does not apply. Consider, for example, the top quark. The evidence for the existence of these entities consists of a body of recorded interactions plus an argument for the conclusion that it is extremely improbable that none of these interactions involved a top quark. In this case physicists never pick out a particular interaction as one in which the particle occurred, nor a specific phenomenon that involved a top quark. Still, they have a well-developed concept of a top quark, and they had this concept before they designed experiments to determine whether these particles exist. My proposal is that their criteria for what counts as evidence for or against the existence of top quarks is part of the content of that concept.

I have avoided talk of 'reference' in discussing this second dimension because in the course of research we sometimes discover that a concept does not refer – that nothing in the relevant domain instantiates that concept. Now the point I want to stress is that we can have a genuine descriptive concept that lacks instances but that includes, among its constitutive features, an account of the kind of evidence that is relevant for deciding if it has instances. We can, for example, describe what Aristotelian physicists would count as evidence for the occurrence of natural and violent motion even though we now hold that no such phenomena – as understood in Aristotelian physics – exist in the physical world. In general, we must distinguish between formulating the content of a descriptive concept and asking whether that concept is instantiated. We must understand the content of a concept before we can undertake to determine if it has instances. I will argue in section 3 that the putative referents of our concepts do play a vital role in research, but my present point is that conceptual content is determined independently of the actual existence of referents and independently of any decision as to whether such referents exist.

The *third* dimension is the function that a concept plays in our thinking.[16] The guiding idea here is that we introduce concepts into a descriptive system in order to achieve a specific cognitive end, such as describing a newly encountered phenomenon, or making a distinction that was not previously made. For example, when Newton introduced the concept *mass* as a physical property that is distinct from, but proportional to, *weight* he invented a conceptual function that did not exist in previous frameworks. The new conceptual role was motivated by several features of his new dynamics. These include his treatment of the common phenomenon of weight as a force between two bodies that depends on the distance between them and an intrinsic property of each. It is this new intrinsic property that Newton sought to capture in the concept of mass.[17] In a similar way, the Aristotelian concepts of natural and violent motion served to distinguish two fundamentally distinct classes of terrestrial motions. One result of the seventeenth-century revolution in physics was the elimination of those concepts from the conceptual framework of physics.

Given these three dimensions, a complete account of a descriptive concept will include accounts of its implicational relations to other concepts in a system, criteria for determining whether it is instantiated, and an account of the function that this concept serves. These dimensions are not totally independent of each other; changes in one dimension can force changes in the others. Thus Newton's introduction of the mass concept involved new implications and required new techniques for determining mass. Still, in specific cases conceptual change can be more drastic on one dimension than on others, and each dimension can provide a significant degree of continuity through that change. This approach thus allows us to understand how relatively small changes at key points in a conceptual system can yield major innovations in the way scientists think about some aspect of nature, while maintaining continuity with an older view. For example, introduction of the concept of an isotope involved changes in chemical thinking on each of our three dimensions. *a*) It introduced a new conceptual function – distinguishing varieties of a single chemical element with different atomic weights. Such a function was incompatible with the prevailing view which held that each element is characterized by a unique atomic weight. *b*) It eliminated the implications between elements and weights that was central to the older conceptual framework. *c*) It eliminated the key criterion that had been used to assess the purity of a sample of an element. In the older framework a non-integral atomic weight was sufficient evidence that the sample was not pure. After the discovery of isotopes such evidence became irrelevant to chemical purity because the researcher might be dealing with a sample consisting of a mixture of isotopes.

These changes had important consequences for the practice of chemistry. One immediate consequence was the elimination as pointless of a central experimental research project of nineteenth-century chemistry: determination of the precise weight associated with each element. In addition, a new project was brought into existence: the search for a new view of the defining characteristics of a chemical element. Still, these changes left much chemical knowledge and practice intact. The periodic table was not changed, although its basis had to be completely rethought. Standard chemical and spectrographic criteria for identifying elements endured, but those that licensed inferences from a weight determination to an element had to be dropped. Many procedures of physical chemistry remained unchanged, although here too procedures that depend on weight relations had to be reworked to some degree.

Introduction of the concept of an isotope involves incommensurability because the older framework does not have the conceptual resources needed to express the new ideas. In the present example this is an understatement since if we were to attempt to introduce isotopes into the older framework without making any other changes, we would generate an inconsistency. Indeed, from the perspective of the older framework, the concept of an isotope is just a contradiction in terms. At the same time, the new framework *by itself* cannot be used to express accurately the concepts of the older framework. The new framework eliminates the implications between a specific weight and a specific element that were constitutive in the older framework. In the new framework, not only can instances of a given element have different weights, but instances of different elements can have the same weight. The transition from the older framework to the new one involves new ways of thinking and important changes in chemical practice. Nevertheless, we are not dealing with a case in which one conceptual system is dropped and another invented *ex nihilo*. The change takes place by selective additions to and deletions from the older system, along with reorganization of some of the older links among concepts.

3. SCIENTIFIC REALISM AND EXTERNAL EVIDENCE

Eventually I am going to defend a pair of theses about the relation between incommensurability and scientific realism, so I must next explain what I understand by the latter doctrine. The form of scientific realism I want to defend amounts to two claims.

(SR1) One aim of scientific research is to learn about the nature of various domains; doing so amounts to seeking a conceptual system that correctly describes items in a domain.

I want to emphasize several points about this thesis. *a*) I do not take this to be the only aim of science. The aim of finding ways of predicting the unobserved is compatible with this realistic aim, and may even be the best that can be done in a given field at a particular stage of its development. *b*) "Knowing the nature of reality" is not a single all-or-nothing accomplishment. Scientific research takes place in a number of disciplines, each with its own domains, sub-domains, and associated conceptual systems. Many of these domains can be studied independently of each other, and we can learn a great deal about specific aspects of nature without coming close to knowing everything. *c*) Progress towards knowledge in a domain need not occur by means of successively closer approximations to the correct account. We may have a well developed but radically incorrect view of a domain, and this incorrect view may provide a basis for continuing research in that domain – even for research that will lead to its own rejection. *d*) Elsewhere (Brown, 1990) I considered the major attempts to show that the realistic aim is misconceived in principle, and argued that these attempts fail. For present purposes I want to highlight just one point. Many contemporary anti-realists hold that the view of concepts as human creations implies that the realist aim is an impossible dream.[18] I think this is a fundamental mistake. One function of concepts is to allow us to think about items that are not themselves concepts – much as language allows us to speak about items other than language, and photographs allow us to study items that are not themselves photographs. The thesis that our concepts are created, not found, is quite compatible with the possibility that a conceptual system may accurately describe its domain – and that we may have good reasons for believing this to be the case. This last remark brings me to the second thesis of the version of scientific realism I am proposing:

(SR2) Our ability to pursue the realist aim has been improving primarily because of the development of improved means of gathering evidence by which to evaluate the adequacy of specific conceptual systems.

I want to elaborate on this claim, which requires that we consider the nature of the evidence mentioned.

The key point about such evidence is that it is derived from attempted interactions with the part of nature we are seeking to describe. This process is 'theory-dependent' in many ways, but this theory-dependence does not determine the outcome when we test a theory. I have avoided the term 'theory' until now, and a full account of theories is not possible here. However, for present purposes a theory can be viewed as the assertion that a particular conceptual system, or cluster of conceptual systems, provides the correct description of its domain.[19] It is a basic working hypothesis of this approach that nature exists independently of our attempts to describe it, and that the outcome of an interaction with some aspect of nature need not match the expectations generated by our theories. Unexpected outcomes are commonplace in science. Without the thesis that nature is independent of human concepts and beliefs, Kuhn's discussions of the role of empirical anomalies in scientific research – including his crucial argument that anomalies are not always counter-instances – make no sense.[20]

I can now explain why I hold that our ability to pursue the realist aim has been improving. Through the development of scientific instrumentation we have learned to interact with a much wider portion of nature than we could when we were limited to the senses we evolved on the surface of this planet. We gather evidence throughout the electromagnetic spectrum, we study radioactive emissions, we produce new interactions among items in particle accelerators, and so on. These developments vastly extend the range of evidence by which we test our theories. But this is only part of the story. We have also learned to get much more precise results than our predecessors could, and – through the development of both mathematics and computing power – to subject these results to more sophisticated analysis. All of these developments have increased the constraints that an acceptable scientific theory must accommodate.

Before proceeding I want to respond to two common objections that will occur to many. *First*, anti-realists may object to my use of current scientific concepts, such as *electromagnetic spectrum*, to describe the items what we study; but such descriptions were used only for ease of communication. Even if such concepts are abandoned in the future, one point will remain: Each time we develop a new kind of instrument we generate a new kind of interaction with nature whose outcome must be dealt with by subsequent theories. *Second*, those who object that scientific instruments generate 'artificial' phenomena are, in a sense, correct but are missing the point. Use of these instruments provides information about how nature responds under various circumstances. The more varied these circumstances, the greater the body of evidential constraints on our theories.

Returning to the main line of argument, note that I have attributed no special role to our senses in this sketch. I have even avoided the term 'empirical' in my account of evidence because of the historical association of this term with sensory experience. Our senses do have a role to play: they are the conduit by which information makes its way into our brains. But this is a pragmatic constraint on the design of instruments, not the foundation of our epistemology or our semantics. The important feature of the theory-testing process is that these tests are attempts to interact with the items about which a theory makes claims – items that exist or fail to exist independently of the theory being tested. In other words, theory testing is a process of attempting to interact with the presumed referents of our concepts. It is at this point that these referents, if they exist, constrain our theories and thus constrain the appropriate concepts for describing them. Moreover, even if the concepts embodied in a theory do not refer, we are still interacting with a part of nature that the theory picks out as relevant. As a result, the testing process can provide the evidence that leads us to revise or reject these concepts.

Nothing in this account guarantees that we will eventually achieve a correct description in any given field – which is why I formulated the realistic thesis as an aim. But nothing precludes this outcome, and we may be more successful in some fields than in others. Given that the aim is pursuable, no more is needed to justify its pursuit than the fact that some people are curious about what nature is like.

4. INCOMMENSURABILITY AND REALITY

I am now ready to state the first major thesis of this paper:

(IR1) Pursuit of the realist goal requires incommensurability because this pursuit requires the introduction of new concepts and the elimination of old concepts.

The basis for this claim is a set of fairly mundane observations about human history and cognition; many of these observations have already been introduced. Concepts developed early in our cognitive history often turn out to be mistaken: sometimes they describe items that do not exist; sometimes they embody incorrect, or partially incorrect, accounts of items that do exist; and sometimes they group items on the basis of features that, by later lights, fail to capture their important properties. In addition, earlier stages in the development of knowledge typically lack concepts for describing items that have not yet been encountered. Consider again the old distinctions between natural and violent motions and between the terrestrial and celestial realms, as well as the relation between the identity of an element and its weight. The five elements that the ancient Greeks believed to constitute the entire universe provide an example well worth pondering. One of these, ether, presumably does not exist.[21] None of the remaining four 'elements' are elements according to the contemporary concept that is associated with that term (which does not require that elements be *elementary* in a particularly deep sense); only water is now considered a chemically distinct substance. For most scientific purposes earth, water, air, and fire would not now appear as members of the same class.

For obvious reasons, early science focused on easily observable properties that often turned out to be superficial in more ways than one. Studies of radioactive decay, for example, have taught us much more about the nature of physical objects than studies of their colors. As a result, key steps in the development of a science have required the invention of new concepts that embody ways of thinking which cannot be captured using the older concepts. Someone who is thinking in terms of the older concepts will often have to learn new ways of thinking in order to understand and evaluate a new theory. And once the new concepts have taken hold, it may require considerable effort to learn to think in ways that once were commonplace.

We have now reached a point at which questions of theory evaluation come to the fore. But it is important to distinguish two different questions about evaluating theories: whether the comparative evaluation of two theories is *rational*, and whether this evaluation gives us any reason for believing that the successful theory is a better candidate for a *correct description of its domain.* I want to develop this distinction and each of its sides.

One reason for making this distinction is that the question of the rationality of theory change applies to anti-realists as well as to realists. Kuhn and Feyerabend, whose work moved the question of rational scientific change to the center of current philosophy of science, were both anti-realists (although not initially in Feyerabend's case). There are many varieties of anti-realism, but I will discuss only instrumentalism. For instrumentalists, incommensurability and the associated problems of comparing alternatives arise because we must learn which phenomena are reliable indicators of which other phenomena – and our views on this matter can undergo radical change. Consider what was involved in learning to predict the likely occurrence of disease from the presence of mosquitoes, or from the outcome of chemical tests on local drinking water, or from a failure of physicians to wash their hands. The problems of rationally justifying radical new proposals for predicting phenomena are quite as difficult from an instrumentalist perspective as on a realist view. Nor can instrumentalists simply fall back on predictive success as a universal test. Predictions made under real-life conditions are rarely perfect, and this provides wide scope for debating predictive adequacy. In addition, criteria for what counts as predictive success also change. A particularly striking example occurred when scientists concluded that only probabilistic predictions are available in some domains.

Two points provide the basis for an account of rational theory change in the face of incommensurability. These points are familiar, but they underline the value of a cognitive perspective on these issues. *First*, those who create new concepts in a field, such as Galileo and Einstein, are typically educated in terms of, and fully understand, the framework they would supersede. *Second*, it is the task of the innovators to provide their colleagues with a motivation for undertaking the work of learning the new concepts. I will highlight two ways in which this motivation can develop. Some may become interested in a new approach because they are aware of serious problems in an existing framework, and hope that the new framework will not have parallel problems. In this case, one may have to learn the new framework before even its promise can be evaluated. But another approach is sometimes available that does not require mastering a new framework before one can recognize that it has merits. This occurs when the innovators display new results that can be described in terms of concepts already available in the older framework, but that cannot be accounted for using the resources of that older framework. New results come in two varieties: results that directly contradict expectations generated by the older view, and results for which the older view generates no expectations at all. Galileo was pursuing the first tack when he predicted that a stone dropped from the mast of a moving ship would land at the foot of the mast. The prediction of a gravitational red shift from general relativity illustrates the second case.

Realists, however, face an additional problem because we have an additional goal: We want to describe nature as it is and adjust our beliefs accordingly. So we must ask what reasons we have for believing that a description may be on track. One consequence of the way that the problem of rational conceptual change arises for realist and anti-realist alike is that rational change, by itself, does nothing to assure that a newly adopted conceptual system is a better candidate for a correct description of its domain. I suggest that the only reason we have for thinking that a conceptual system may correctly describe items in its domain derives from the evidence we acquire as a result of attempts to interact with those items. But here too we must be careful. With the exception of extreme social constructivists, anti-realists also recognize that science must accommodate evidential constraints derived from interactions with nature. As Kuhn emphasized,[22] it is the attempt to solve problems that arise out of interactions with nature that distinguishes science from many other endeavors. To motivate realism I must, then, defend the thesis that accommodating external evidence provides positive grounds for thinking that a theory may embody an accurate description of its domain. I want to approach this question by exploring the nature of these constraints more closely, beginning with an argument due to Greenwood (1990).

Greenwood was concerned with the way in which Quine's sweeping holism has been used to argue that *any* scientific hypothesis can be protected against an evidential challenge by making adjustments elsewhere in the system. On the contrary, Greenwood argued, it is difficult to make these adjustments exactly because of the way the members of a system are linked together. Changes aimed at accommodating one set of troublesome observations will often have consequences that bring other parts of the system into conflict with other observations. "The 'web of belief' tends to reduce rather than increase the room for intellectual maneuver when faced with recalcitrant observations" (Greenwood, 1990, p. 566). The larger the system, the harder it is going to be to find a set of adjustments that protects the preferred members and still accommodates all the evidence. Consider, for example, the hypothesis that planets move in square orbits. It is not plausible that our current belief system can be adjusted to include this hypothesis and still be brought into conformity with all currently available evidence. Moreover, this example immediately suggests an infinite set of other orbital shapes that cannot be taken seriously.

Now I am urging that scientific conceptual systems have a considerably more restricted range than they do in Quine's account. In this respect Greenwood's point holds to a lesser degree, but its full force returns when we consider the growing mass and precision of the evidence that a serious scientific theory must accommodate. This has an effect that is quite similar to the sheer size of a Quinean system. The goal of protecting favored elements of a conceptual system – no matter what the evidence – becomes very difficult to pursue as the evidence becomes more varied and precise. Moreover, according to the account of evidence I have proposed, this evidence arises from interactions with nature. As a result, constraints provided by external evidence are *constraints provided by nature*. And so we arrive at my second main thesis:

(IR2) Evidence provided by interactions with nature significantly limits the range of incommensurable conceptual systems that can provide plausible descriptions of a domain.

Thus we have reasons for believing that well-tested theories describe items in their domain because our choice of theories is limited by those items. I now want to consider how this form of realism stands up against the two main types of anti-realist philosophy of science in the current literature that also accord a fundamental role to evidence – the Kantian and empiricist views.

Consider, first, the Kantian approach typified by Kuhn (cf. Brown, 1975; Hoyningen-Huene, 1993). The anti-realist consequences of this view are supposed to derive from the role that concepts play in all of our thinking about nature, along with the thesis that we create our concepts – which change as science develops. But this view is significantly different from Kant's approach which posits a single permanent set of fundamental concepts and forms of sense that are constitutive of both science and nature. Even in Kant there is a substantive question about why space, time, causality, existence, and so forth cannot characterize things themselves. Strictly speaking, Kant's thesis is that we cannot *justify* applying these beyond the range of possible experience – but that thesis turns on Kant's attempt to justify these as *a priori* features of experience. It is central to the Kuhnian approach that the concepts at issue are not *a priori* features of all experience. But then Kant's arguments against the application of these concepts to things themselves cease to be relevant. Leaving Kant's arguments aside, the fact that we formed a set of concepts to describe items we have never experienced does not entail that those concepts cannot correctly describe the items in question – it only entails that we initially lack positive reasons for believing that our concepts describe these items. This shifts the question to the kind of evidence that could overcome this initial situation, and I have provided an account of evidence that addresses this question. Given this account there are no reasons for thinking that nature ultimately consists of items that, in principle, elude our cognitive grasp.

At this point arguments from incommensurability – that is, arguments from alternative conceptual systems – enter the discussion. These arguments come in two quite different versions. One of these is the view I have already addressed, which holds that any thesis can be defended against any body of evidence. Notice that even the Quinean version of this approach does not claim that entire frameworks can be defended against any body of evidence – only that selected parts of a framework can be so defended – and I have provided a counter-argument to this limited thesis. But there is another, even more modest, anti-realist argument that we must consider. This argument holds that it is always possible to find *an alternative* incommensurable framework that will accommodate any body of evidence. I have not argued against this view, and I do not think there is any general argument that can address it. Rather, this is a question to be decided in various scientific domains on a case-by-case basis. It may turn out that there are some domains in which only one defensible view is available. It is not enough to claim that another view is always possible; one must make the case by constructing such an alternative. When this is done, one common outcome is to point to other evidence that will distinguish the two views. Still, the version of realism I am defending leaves it an open question whether there are domains in which we will encounter alternative frameworks among which we cannot decide. If this does occur we will have learned something about the limits of our ability to learn about nature – and we will have learned this from the development of science. From a naturalist perspective this is wholly appropriate: The nature of human knowledge is one of the domains about which we learn as science proceeds. A key feature of the form of realism I am defending is that it avoids any prejudgment about what science will teach us in any domain.

I turn now to empiricist forms of anti-realism. Hempel provided a classic statement of the empiricist view: "Scientific systematization is ultimately aimed at establishing explanatory and predictive order among the bewildering complex 'data' of our experience, the phenomena that can be 'directly observed' by us" (Hempel, 1965, p. 177). Hempel goes on to note the "remarkable fact" that success in this project has been greatly enhanced by the introduction of laws that refer to entities we cannot directly observe, and that this is a problem for empiricists. He resolves the problem by arguing that the introduction of non-observables would be unnecessary if the only aim of science were to establishes *deductive* relations among observables. "But if it is recognized that a satisfactory theory should provide possibilities for inductive explanatory and predictive use and that it should achieve systematic economy and heuristic fertility, then it is clear that theoretical formulations cannot be replaced by expressions in terms of observables only ..." (Hempel, 1965, p. 222). This view is anti-realist in the sense that it does not include the study of the substantial portion of nature that we cannot observe among the goals of science. The philosophical basis for this position lies in the combination of empiricist theories of concepts and of evidence that I discussed earlier in this paper. I have offered alternatives to both of these that undercut the anti-realist arguments. The accounts of concepts and of evidence I have proposed provide positive reasons for thinking that science teaches us about all of nature, not just about the portions we can detect with our unaided senses.[23]

5. CONCLUSION

The discovery of incommensurability raises methodological problems that were not recognized at earlier stages in the development of the philosophy of science, and indicates that the process of evaluating scientific theories is more complex and less certain than many would like it to be. But incommensurability is not just a source of problems, it is also a source of opportunities. The incommensurability thesis embodies the recognition that we can, and do, overcome limitations built into our ways of thinking at a given point in our history. The view of incommensurability as solely a source of problems arises, in part, from a failure to distinguish clearly between science and a philosophy of science. Incommensurability provides a serious challenge to the form of logical empiricism that was dominant in the 1950s. But to take this as a challenge to science is to make the dubious assumption that science must operate in accordance with logical empiricist methodological strictures if it is to be a genuine source of knowledge. Many of us who reject this assumption conclude that we are dealing with one more philosophical failure, and we are thus engaged in a process of philosophical reconstruction. I have been urging that such a reconstruction should include a genuine break with two of the foundation stones of traditional empiricism: its theory of concepts and its theory of evidence. Kuhn and Feyerabend took the first of these steps, but not the second. Together, these moves away from the empiricist tradition open the way for a view that Feyerabend, in some of his periods and some of his moods, might have found congenial: that the development of incommensurable conceptual systems is one important tool for the advancement of science. Or, as Sellars once put it: "After all, it is characteristic of modern science to produce deliberately mutant conceptual structures with which to challenge the world" (Sellars, 1953a, p. 337).

Northern Illinois University

NOTES

[1] See especially Doppelt (1978); for a recent discussion see Brown (1996).

[2] I use 'item' as a neutral term to cover entities, processes, events, or whatever else we may encounter or postulate in the course of our investigations.

[3] The same point holds for the development of methodology.

[4] I am not referring here to the U. S. Secretary of State, but to a position in Illinois, where the Secretary of State is an elected official in charge of the department that licenses drivers and automobiles.

[5] For details of this history see Pais (1986); Romer (1964; 1970); Trenn (1977). The concept of radioactive induction was abandoned when it became clear that radioactive products of previous decays were being deposited on the materials in question. Metabolons turned out to be radioactive isotopes with very short half-lives, but the concept of an isotope had not yet been formulated at the time in question.

[6] Recall that X-rays are produced when a stream of electrons hits a target.

[7] See Shapere (1974) for this use of 'domain'.

[8] See, for example, Fodor (1998); Margolis and Laurence (1999). Concepts should not be assimilated to Fregan 'senses' – at least as Frege used this term. Frege made a three-fold distinction between "a sign, its sense, and reference" (Frege, 1960, p. 58), where a sense is a sign's mode of presentation. Thus, for Frege, senses presuppose linguistic entities, and Frege is quite clear that senses are not mental entities – he reserves the term 'ideas' for this role (Frege, 1960, pp. 59–60). I want to emphasize that I am not treating concepts as linguistic entities. One reason for avoiding this identification is that it is an open question whether non-linguistic animals possess concepts. As Sellars notes (Sellars, 1975, pp. 303–304) if animals behave in ways that are sufficiently analogous to ways we behave, then the best explanation may involve attributing concepts to them. In any case, the issue should not be decided by fiat. Still, when dealing with humans it is appropriate to take linguistic evidence as evidence about underlying concepts.

[9] The ancient Chinese distinguished five basic kinds – water, earth, fire, metal, and wood – and thought about them in terms of a quite different conceptual machinery (see Brock, 1993, chapter 1).

[10] It is worth noting that the concept of a *natural kind* is itself a concept in a theory of how the world is structured and of how research should be pursued – a theory that is subject to reconsideration. One impetus for such reconsideration might come from the proliferation of isotopes. Elements are chemically identical, and may thus be considered members of the same kind for a wide variety of purposes. But all elements have isotopes, which have different atomic weights. As a result, different isotopes of an element will have different diffusion rates and may be separated in a centrifuge. In this context, the isotopes are not the same stuff. A more dramatic example occurs when we consider nuclear processes. Some isotopes of an element may be radioactive, while others are not, and this is a significant difference in kind for some purposes. Consider, as an example, thorium, which has more than twenty-five isotopes, all radioactive, with half-lives running from microseconds to billions of years. Further complications are introduced by the existence of allotropic forms of elements – especially the recent proliferation of allotropes of carbon – and of isomers of complex molecules. The usual accounts of natural kinds involve the view that there is one correct way of dividing up nature, and this may turn out to be uninteresting and unilluminating for many scientific purposes.

[11] The core issue is whether the content of scientific concepts is wholly or partially determined by experience, or whether the experience that is relevant to scientific research always already includes conceptual elements. The latter view requires an account of the source of conceptual content that is independent of experience. C. I. Lewis, (1956; however this is a reprint of a book originally published in 1929) provides a striking example of a philosopher whose overall epistemology has many empiricist elements, but who explicitly rejects the usual empiricist view of concepts, and holds that concepts are completely determined by implicational relations to other concepts. For Lewis there are no intrinsically basic concepts, and conceptual analysis requires mapping out relations between concepts; it does not involve breaking down complex concepts into elementary parts that are derived from experience. If Lewis were writing in the 1960s or later he would be viewed as holding a radical version of the 'concept-ladenness' of perception. Sellars, who seeks to synthesize elements from traditional empiricism and traditional rationalism, also holds that all perception involves conceptual elements and that "conceptual status" is determined by relations among concepts (e.g., Sellars, 1953a; 1953b; 1956). Examples of this approach from philosophers associated with the idealist tradition include Blanshard (1939) and Collingwood (1940). Harris (1970, chapters VII and VIII) discusses overlaps between the views of Blanshard, Collingwood, and Kuhn.

[12] One of the unfortunate results of treating conceptual systems as languages is that it tends to obscure this point. See Sankey (1994, chapter 4) for a good discussion.

[13] This view is built on the work of Wilfrid Sellars. See Brown (1986; 1991) for more details and further discussion. However, work done since I wrote those papers has altered my interpretation of Sellars and my views on the adequacy of Sellars' approach in the form in which he left it. For example, my account of Sellars' notion of an *entry transition* (1991, pp. 328–331) extends this notion beyond the limited set of examples that Sellars discussed, but the notion is still not adequate for dealing with cases such as the top quark; see the discussion below in the main text. In addition, the third dimension that I discuss is not an explicit part of Sellars' theory of concepts, although he makes many suggestions that point in this direction. I cannot provide an adequate defense of this theory of concepts here. Ultimately such a defense depends on showing how the theory provides a basis for studies of conceptual development, and also provides a deeper understanding of the nature and function of conceptual analysis than is now available. This work is in progress.

[14] I avoid the familiar practice of describing these items as 'mind-independent' since minds are a subject for scientific study, and thus one of the items that we seek to represent by means of concepts. The important point is that descriptive concepts serve to represent items other than themselves, and can fail to represent those items correctly. Not all concepts play a descriptive role; some have a purely formal role, such as the various components into which a vector may be resolved. Sellars distinguishes between formal, descriptive, and prescriptive concepts which play different roles in cognition and action, but I am limiting discussion here to descriptive concepts.

[15] In some of his writings Kuhn does include a role for ostension in learning some concepts. (See for example, Kuhn, 1977a, and the discussion in Hoyningen-Huene, 1993, pp. 77–81.) There are several issues that must be sorted out here. Kuhn seems to be discussing how a child may learn to make certain classifications. But, as Hoyningen-Huene points out, the account does not apply to a completely naive child since participation in the practice of learning by ostension requires a good deal of prior knowledge. Moreover, the strong Kantian element in Kuhn's epistemology militates against such a view: Concepts have to be in place before we can pick out objects. Kuhn also suggests that ostension, or something like ostension, can play a role when conceptually sophisticated scientists are attempting to teach their way of thinking to students or to other scientists. (See especially Kuhn, 1970, chapter 10, and the discussion in Brown, 1983, pp. 19–20.) It is, however, not clear what role, if any, ostension plays, for Kuhn, in determining the conceptual content. Indeed, it is not clear that Kuhn has a well-developed theory of concepts (or of linguistic meaning), but it is clear that he stresses the role of relations among concepts in determining their content, and the need to grasp an entire system in order to understand its constituent concepts. (See for example, Kuhn, 1970, pp. 101–103; 1983; 1990.) A similar emphasis pervades Feyerabend's writings (e.g., 1962; 1975, chapter 17).

[16] Whether a concept has a descriptive function is one instance of this, but at the moment I am after another point that applies among descriptive concepts.

[17] This is only part of the story. Newton's second law introduced the thesis that force is proportional to acceleration, not to velocity. This proportionality required a new constant that Newton also identified with mass. It soon became clear, however, that Newton had introduced two new conceptual roles which were synthesized into a single role in general relativity.

[18] I use the term 'anti-realist' to refer to those who hold, for whatever reasons, that the attempt to understand the nature of the world is not a legitimate aim of science.

[19] There are obvious affinities between this approach and the semantic view of theories. On that view a theory consists of a set of models, which is to be distinguished from the hypothesis that one of those models provides a description of some scientific domain.

[20] See Brown (1995) for elaboration of this point and for a more detailed account of the view of evidence that I am summarizing here.

[21] To be sure, some scientists still use the term and debate the existence of some universal material pervading spacetime. But this new ether is conceived of in terms that would be quite unintelligible to the ancients, and plays a role in physics that they did not imagine. Its existence is debated on the basis of evidence that they would not be able to understand without undergoing a great deal of conceptual reeducation.

[22] E.g., when he described normal science as an "attempt to force nature into the preformed and relatively inflexible box that the paradigm supplies" (Kuhn, 1970, p. 24). Similar remarks occur on pp. 112, 129, 150, *et passim*.

[23] An alternative form of empiricist anti-realism has been developed by van Fraassen. Adequate discussion of van Fraassen's views would require another paper, but two points are worth noting here. First, he shares the standard empiricist view that science is concerned only with relations among items that we can observe (van Fraassen, 1980, pp. 11–19). Second, he is much less clear than his predecessors on what counts as an observable. He holds that scientific theories determine what is observable by us (van Fraassen, 1980, pp. 57–58), but it is far from clear that this determination can avoid appeal to the non-empirical portions of those theories (cf. Fine, 1986, pp. 142–147). In addition, what is observable by us depends on who we include under the rubric "us," and van Fraassen holds that this may change as a result of "ideological or moral decisions" (van Fraassen, 1980, p. 18).

REFERENCES

Blanshard, B. (1939). *The Nature of Thought*. London: George Allen & Unwin.

Brock, W. (1993). *The Norton History of Chemistry*. New York: W. W. Norton and Company.

Brown, H. (1975). "Paradigmatic Propositions." *American Philosophical Quarterly* **12**: 85–90.

Brown, H. (1979). *Perception, Theory, and Commitment: The New Philosophy of Science*. Chicago: University of Chicago Press.

Brown, H. (1983). "Incommensurability." *Inquiry* **26**: 3–29.

Brown, H. (1986). "Sellars, Concepts and Conceptual Change." *Synthese* **68**: 275–307.

Brown, H. (1990). "Prospective Realism." *Studies in History and Philosophy of Science* **21**: 211–242.

Brown, H. (1991). "Epistemic Concepts." *Inquiry* **34**: 323–351.

Brown, H. (1995). "Empirical Testing." *Inquiry* **38**: 353–399.

Brown, H. (1996). "The Methodological Roles of Theory in Science." In B. Rhoads and C. Thorn, eds., *The Scientific Basis of Geomorphology*, pp. 3–20, Sussex: John Wiley and Sons.

Collingwood, R. (1940). *An Essay on Metaphysics*. Oxford: Clarendon Press.

Doppelt, G. (1978). "Kuhn's Epistemological Relativism: An Interpretation and Defense." *Inquiry* **27**: 33–86.

Feyerabend, P. (1962). "Explanation, Reduction and Empiricism." In H. Feigl and G. Maxwell, eds., *Minnesota Studies in the Philosophy of Science, Volume 3*, pp. 28–97, Minneapolis: University of Minnesota Press.

Feyerabend, P. (1975). *Against Method*. London: New Left Books.

Fine, A. (1986). "And not Antirealism Either." In *The Shaky Game*, pp. 136–150, Chicago: University of Chicago Press.

Fodor, J. (1998). *Concepts*. Oxford: Clarendon Press.

Frege, G. (1960). "On Sense and Reference." In P. Geach and M. Black, trans., *Translations from the Philosophical Writings of Gottlob Frege*. 2nd edition. pp. 56–78, Oxford: Basil Blackwell.

Greenwood, J. (1990). "Two Dogmas of Neo-Empiricism: The 'Theory-Informity' of Observation and the Quine-Duhem Thesis." *Philosophy of Science* **57**: 553–574.

Harris, E. (1970). *Hypothesis and Perception*. London: George Allen & Unwin.

Hempel, C. (1965). "The Theoretician's Dilemma." In C. Hempel, ed., *Aspects of Scientific Explanation*, pp. 173–226, New York: The Free Press.

Hoyningen-Huene, P. (1993). *Reconstructing Scientific Revolutions: Thomas S. Kuhn's Philosophy of Science*, trans. A. Levine. Chicago: University of Chicago Press.

Khalidi, A. (2000). "Incommensurability." In W. Newton-Smith, ed., *A Companion to the Philosophy of Science*, pp. 172–180, Oxford: Blackwell Publishers.

Kuhn, T. (1970). *The Structure of Scientific Revolutions*. 2nd edition. Chicago: University of Chicago Press.

Kuhn, T. (1977a). "Second Thoughts on Paradigms." In *The Essential Tension*, pp. 293–319, Chicago: University of Chicago Press.

Kuhn, T. (1977b). "Theory Change as Structure Change." In R. Butts and J. Hintikka, eds., *Historical and Philosophical Dimensions of Logic, Methodology and Philosophy of Science*, pp. 299–309, Dordrecht: D. Reidel.

Kuhn, T. (1983). "Commensurability, Comparability, Communicability." In P. Asquith and T. Nickles, eds., *PSA 1982, Volume 2*, pp. 669–688, East Lansing: The Philosophy of Science Association.

Kuhn, T. (1990). "Dubbing and Redubbing: The Vulnerability of Rigid Designation." In C. Savage, ed., *Minnesota Studies in the Philosophy of Science, Volume 14*, pp. 298–318, Minneapolis: University of Minnesota Press.

Lewis, C. (1956). *Mind and the World Order*. New York: Dover Books.

Margolis, E. and S. Laurence. (1999). "Concepts and Cognitive Science." In E. Margolis and S. Laurence, eds., *Concepts: Core Readings*, pp. 3–81, Cambridge, Mass.: MIT Press.

Pais, A. (1986). *Inward Bound*. New York: Oxford University Press.

Putnam, H. (1975). "The Meaning of 'Meaning'." In *Mind, Language and Reality: Philosophical Papers, Volume 2*, pp. 215–271, Cambridge: Cambridge University Press.

Romer, A., ed. (1964). *The Discovery of Radioactivity and Transmutation*. New York: Dover Publications.

Romer, A., ed. (1970). *Radiochemistry and the Discovery of Isotopes*. New York: Dover Publications.

Sankey, H., (1994). *The Incommensurability Thesis*. Aldershot: Ashgate.

Sellars, W. (1953a). "Inference and Meaning." *Mind* **62**: 313–338.

Sellars, W. (1953b). "Is There a Synthetic *A Priori*?" *Philosophy of Science* **20**: 121–138.

Sellars, W. (1956). "Empiricism and the Philosophy of Mind." In H. Feigl and M. Scriven, eds., *Minnesota Studies in the Philosophy of Science, Volume 1*, pp. 253–329, Minneapolis: University of Minnesota Press.

Sellars, W. (1975). "The Structure of Knowledge." In H. Castañeda, ed., *Action, Knowledge and Reality*, pp. 295–347, Indianapolis: Bobbs-Merrill.

Shapere, D. (1974). "Scientific Theories and Their Domains." In F. Suppe, ed., *The Structure of Scientific Theories*, pp. 518-565, Urbana: University of Illinois Press.

Thagard, P. (1992). *Conceptual Revolutions*. Princeton: Princeton University Press.

Trenn, T. (1977). *The Self-Splitting Atom: A History of the Rutherford-Soddy Collaboration*. London: Taylor and Francis.

van Fraassen, B. (1980). *The Scientific Image*. Oxford: Clarendon Press.