

# RATIONALITY AND METHODOLOGICAL CHANGE: DUDLEY SHAPERE'S CONCEPTION OF SCIENTIFIC DEVELOPMENT

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## Abstract

*Over the last 4 or so decades, Dudley Shapere has developed a rich and interesting alternative to the Kuhnian “relativist” account of science and its development. This paper is a review of this alternative viewpoint. It is a critical evaluation of Shapere’s arguments in support of the claim that radical methodological change can be allowed in science without thereby embracing relativism (and without ending with an irrational account of scientific change).*

## 1. The Character of Scientific Change

Shapere’s view of scientific change starts with the basic idea that “science builds on what it has learned” in the sense that its established theories, laws and assertions guide the articulation and construction of new theories; they guide practical action, and they also constrain possible conjectures. According to him, the process of building on what we have learnt indicates how all aspects of science, including its methods and rules of reasoning, are subject to possible radical change:

*It is truly all aspects of science, not only what are considered its substantive beliefs about nature, but also its methods and aims, that are subject to change in ways that have continued to surprise us. The problems we face in our inquiry about nature, and the methods with which we attempt to deal with those problems, co-evolve with our beliefs about nature. (Shapere 1987a, p.5.)*

The claim is not merely that scientific methodology evolves or is modifiable in the process of learning more about the world. The full claim is that there is nothing unalterable or sacrosanct in science, and as such, Shapere’s view implies what may be described as the no-invariant methodology thesis.

The problem confronting any view such as Shapere’s is that it threatens inevitably to entail relativism. For although there are various forms of relativism, the central claim of all relativists is that there are no independently valid criteria

for determining rational choice (or for supplying justification) over and above those specified by a given view-point (or culture, or paradigm). If the methods, standards, rules of reasoning, and, indeed everything else in science, is subject to (possible) radical change, then in two competing theories (or paradigms), the principles for the correct appraisal of theories may differ radically. When they do differ, how can choice between them be rationally made? If competing theories differ in their methods and rules of reasoning, in virtue of what do we compare them?

Shapere is fully aware of the threat of relativism. He in fact charges Kuhn and Kuhnians with the espousal of relativistic views, and he himself explicitly rejects relativism. As Shapere sees it, the problem with the Kuhnian model of scientific change is not due to the fact that it allows change in science to go deeper than change of theory. Rather the problem stems from the manner in which scientific research is said to be governed by some “broader” and more fundamental “interpretative frameworks” called *paradigms*. Shapere’s task, therefore, is to show that a view of scientific change can be developed in which *nothing* is *sacrosanct* or *inviolable*, but which, unlike Kuhn’s view, fails to entail relativism.

## 2. Science and Its Development

According to Shapere, there are two main lessons to be learnt from the historical development of science—especially its development within the last 150 years. These lessons are stated in the form of two principles: the “Principle of Rejection of Anticipations of Nature” and the “Principle of Scientific Internalization”. Shapere states the first principle as follows:

The results of scientific investigation could not have been anticipated by common sense, by the suggestions of everyday experience, or by pure reason. (Shapere 1987a, p. 1.)

The significance of this principle is in the point that our contemporary image of science departs very radically from our common sense everyday beliefs. On the basis of common sense everyday beliefs (or of pure reason alone), no one could have anticipated complex theories such as the quantum theory, the general theory of relativity, and evolutionary Darwinism. Consider the contemporary views of evolution and genetics. These views involve very complex claims about fundamental similarities (and differences) between various species of organism;

assumptions about some tacit non-cognitive form of co-operation amongst individual organisms in their struggle for survival; claims about sexual selection and heredity; etc. — which depart very radically from the dictates of everyday common sense beliefs.

Although the Principle of Rejection of Anticipations of Nature emphasizes the point that science departs radically from our everyday common sense beliefs (and pure reason), it does not tell us how science has managed to go beyond the confines and dictates of common sense. Furthermore, the principle does not tell us *why* the departure of science from the confines of common sense is justified; nor does it tell us whether those current views of science which depart so radically from common sense imagination can be regarded as *true* (or adequate) depictions of nature and reality.

Because of the limitations and negative message of the principle of rejection of anticipations of nature, Shapere supplements this principle with another principle which “furnishes profound insight into . . . the *knowledge-acquiring* aspect of scientific enterprise”. (Shapere 1987a, p. 3, my emphasis) Shapere states the second principle as follows:

Every aspect of our beliefs ought, whenever possible, to be formulated, and to be brought into relation to well-founded beliefs, in such a way that it will be possible to test that aspect. (Shapere 1987a, pp. 3–4.)

Shapere calls this the *Principle of Scientific Internalization*. The principle of internalization complements the principle of rejection of anticipations of nature because while the latter principle rejects certain modes of knowledge-acquiring (i.e. it urges us not to anticipate the nature of the world on the basis of pure reason or common sense) the former principle outlines the process by which the range of ideas within science ought to be expanded. Specifically, the principle entails that:

The sorts of considerations that have led us [and that should always lead us] to alter our beliefs about nature, at least when those considerations are ones we call 'rational' or 'based on evidence', have themselves been scientific ones. (Shapere 1987a, pp. 3–4.)

The historical development of science plays a crucial role in identifying and lending detail to this second principle. A close look at the history of science indicates that scientific research is always conducted on the basis of some presumed facts, laws, and theories. This common body of laws, fact, and theories are presumed

because the scientist conducts her research by taking their *truth*, validity, or adequacy for granted. The solar physicist, for instance, carries out research on stars or nuclear fusion by taking for granted things such as: Einstein's equation  $E = mc^2$ ; that natural phenomena is governed by four main types of forces or interactions known as the strong force, the electromagnetic force, the weak force, and, the gravitational force; the theory of stellar evolution; and various other laws and theories. Shapere refers to the presumed set of facts, theories, and beliefs that guide research in any field of inquiry as the *background knowledge* of that field of inquiry.

The fact that the principle of internalization now governs scientific activity and the fact that this principle had to be learned can be (according to Shapere) illustrated by a comparison of the Milesian science of the 6th century B.C. with the science of 17th century Europe. Shapere describes the Milesian approach to the study of nature as "holistic" and that of 17th century Europe as "piecemeal".

The major contrast between these two approaches lies in the fact that the Milesians did not focus on problems generated by specific fields of endeavor. Indeed, it seems that the Milesians did not conceive of inquiry about nature (and the universe) in terms of distinct *subject-matters* (such as gasses; the physical composition of plants and animals; chemical reactions; magnetism; etc). The Milesians simply regarded *all* aspects of existence, *all* forms of change, and *all* aspects of nature, as their subject of inquiry. But by the 16th and 17th centuries, a different approach to the study of nature had gradually become predominant. This is the approach of examining specific and individual subject-matters in isolation from others. Rather than trying to understand nature and the universe as a whole (as the Milesians did), various subject-matters were investigated in isolation from each other. These subject matters are what Shapere describes as *domains of inquiry*.

In Shapere's view, *domains* are characterized: (a) by certain "items of information" (i.e. facts, accepted theories, and laws) which, (b) are associated in such a way that there is some deep unity between them, and (c) these unified associations generate problems that scientists try to solve in their research activities. That is, a domain of research is a unified body of information which forms an object of scientific investigation. For example, astrophysics is a domain of scientific inquiry because it is made up of a *body of information* (information such as Einstein's equation  $E = mc^2$ ; that there are four main forces in nature; that there was a big bang; that there are elementary particles; that there is stellar evolution, etc.) which generate the problems scientist try to solve in their research.

Shapere's characterization of *domains* highlights the point that scientific research does not proceed merely in terms of theories. In the actual practice of science, research is always conducted on the basis of some assumed sets of beliefs, facts, laws and theories which form the sort of unity we imply when we identify contexts of scientific investigations like "chemistry", "astrophysics", "evolution science", "optics", etc. Although these sets of beliefs cannot be regarded as theories (theories are merely some of the items of information that make up domains), the items of information within these units constitute a coherent field of study. Shapere's concept of "domains" is an attempt to characterize such units. But not any old unit of *items of information* will count as a *scientific domain*. Only those associations of background knowledge that are unified in the sense that they yield genuine problems for scientific research are *domains*.

Shapere regards the classification of science into various domains of inquiry as a result of the process of learning from nature. We had to learn how to classify science into distinct domains, and any current classification is always subject to change and modification as we learn more about the world. Early classifications, for instance, were based on considerations such as sensory similarities, pragmatic functions and use, a substance's place of discovery, etc. For example, metals were classified on the basis of their obvious sensory appearances, as were salts and crystals. But as we learned more about nature, these initial classifications were rejected; *domains* which were previously regarded as distinct were unified, and new domains identified. This is because previously accepted bases of classifications were rejected, perceived similarities (and differences) between items classified as members of the same domain were seen to be superficial; hence new basis for the classification and separation of subject-matters into domains were laid.

The contrast between 17th century natural philosophy and 20th century science provides a good example of Shapere's claim that domains of inquiry alter and change as a result of the growth of knowledge. During the 17th century, there was no clear cut distinction between philosophy, theology, physics, astronomy, and mysticism. All these fields of inquiry fell within the scope of natural philosophy. Thus, Kepler who is well known for his explanation of nature in terms of precise and fundamental mathematical laws also inquired into the relationships between "harmonies" in planetary motions and musical harmony. He also delved into questions such as the effects of the angle of two planets during a person's birth on that person's future. Newton also regarded theological considerations as part and parcel of scientific inquiry. Indeed, it is often claimed that

Newton devoted at least as much of his time and energy to inquiry in alchemy and mysticism as to science as now understood.

Shapere further insists that the classification of science into distinct domains of inquiry lays important requirements on theory-choice and explanation. For in emphasizing the point that the boundaries of domains alter as science develops, he also claims that the sorts of constraints that are imposed on the questions we ask, what is relevant to inquiry, the character of an adequate explanation, etc, also change:

... the very adoption of the piecemeal approach to inquiry — the laying-out of the boundaries of specific areas of investigation — automatically produced a standard against which theories could be assessed. Whatever else might be required of an explanation of a particular body of presumed information (domain), that explanation or theory could be successful only to the extent that it took account of the characteristics of the items of that domain. (Shapere 1987b, p. 3.)

Shapere's point is that the development of the piecemeal approach to inquiry has given rise to the requirement that scientific theories and explanations be regarded as good or bad (successful, adequate, or inadequate), on the basis of how well they can account for the problems of their domain: "the methods we consider appropriate for arriving at well-grounded beliefs about the world have come more and more to be shaped by those very beliefs, and have evolved with the evolution of knowledge" (Shapere 1982, p. 178). Hence this "viewpoint maintains that method not only determines the course of science, but is itself shaped by the knowledge attained in that enterprise" (Shapere 1982, p. 181).

To fully appreciate the full content of Shapere's principle of scientific internalization, I will examine one of Shapere's main examples of internalization — the solar neutrino experiment. In the solar neutrino experiment, astrophysicists claim to "directly observe" the production of neutrinos in the central region of the sun.

How is the astrophysicist's supposed to *directly observe* (or *see*) the central region of the sun? According to established theory, the centre of the sun lies at the core of 400,000 miles of dense matter. Theoretical astrophysics further maintains that deep in the core of stars like the sun is a thermonuclear furnace, whose exceedingly high temperatures of at least one million degrees Kelvin, force the nuclei of hydrogen atoms to fuse into helium. The main initial nuclear reaction (according to theory) is the conversion of hydrogen into helium. This is the

so-called proton-proton sequence of reactions. This main sequence of reactions leads to another chain of reactions which culminates in the production of the radioactive isotope Boron 8 ( $^8\text{B}$ ).<sup>1</sup> When this radioactive isotope decays, it releases neutrinos which are highly energetic. Traveling at the speed of light, neutrinos are believed to bombard every square centimeter of the earth at the rate of 70 billion per second. The solar neutrino experiment was set up in an attempt to detect the neutrinos that accepted theory entails are produced and transmitted into space.

Two of the most important items of information within background knowledge to the solar neutrino experiment concern neutrinos themselves: (1) Neutrinos are believed to be massless (or almost so). But according to modern particle physics, a massless particle cannot change its form; that is, it cannot *interact* with any other particle. All it can do is to absorb or emit energy. And because of this lack of interaction, neutrinos are also believed, (2) to obey the “weak interaction theory”. This theory entails that neutrinos can pass unimpeded through almost everything they encounter *en route* from the sun’s core.

The neutrino detector used in the experiment is a 400,000-litre tank of the cleaning fluid *perchloroethylene*. The tank is buried 4,850 feet into a mine to prevent particles that can produce effects similar to those of neutrinos from interfering with the results of the experiment. Scientists calculated that neutrinos should have enough energy to trigger off a chain of reactions in the tank of perchloroethylene. The expected reaction was the changing of chlorine atoms in the tank into isotopes of argon. The atoms of the argon were then to be counted on a proportional counter.

But surely, the questions must be asked: in what sense can the astrophysicist legitimately claim to *observe (directly)* the central region of the sun?

One obvious response seems to be that the astrophysicist *infers* her claims about the internal constitution of the sun on the basis of her currently best theories! For she, in fact, does not, and cannot, *see* (in the normal usage of *perceiving*) the events and processes occurring at the centre of the sun. At best (one might insist), what the astrophysicists actually *see* is the occurrence of certain reactions in the tank. Or perhaps, she is merely observing clicks that are registered on the proportional counter that counts atoms in the tank. Whatever else the astrophysicist might be *seeing* (we might insist) it is not the core region of the sun. For the claims are made on the basis of a study of the processes occurring in tanks.

But the objections to the astrophysicist’s claims do not end here. Because even if we concede (just for the sake of argument) that the astrophysicist “sees”

(in a very loose sense) the core region of the sun, surely, the “seeing” cannot be *direct*. For the detection of neutrinos in the experiment is based on very complex *inferences*. After all, the claim can be made *only if* theories such as that of stellar evolution, an assumption of the age of the sun, etc, are made. Any conclusion arrived at on the basis of these assumptions must be *inferential*.

Shapere warns that we should not be too hasty in charging the astrophysicist of using the terms “observation”, “direct”, and “seeing” loosely. This is because there is an important contrast to be drawn between the information carried by neutrinos and the electromagnetic information we receive via light-photons. Unlike neutrinos, light-photons do not obey the weak interaction theory. Although neutrinos and photons are believed to be produced by the same nuclear fusion process, unlike neutrinos which pass unimpeded through almost everything they encounter, photons take a very long circuitous path to the stellar surface. En route from the core, photons collide with the atoms of hydrogen and helium gas that populate the radiative zone (the zone in which nuclear fusion takes place) of the sun. Energy is lost with every collision, and photons also change direction randomly with every collision. Hence, photons (the carriers of electromagnetic information) take something within the range of 100,000 to 1,000,000 years to reach the sun’s surface. During this very long period, they would have been absorbed, scattered and re-radiated so drastically that although they were initially produced as high-frequency, short-wave gamma rays, they are received as low-frequency, long wave visible light. Because neutrinos do not undergo any such drastic alteration en route from the sun they “are at one and the same time the most reliable and the most reluctant of messengers” (Fowler 1967, quoted in Shapere 1982, p. 491).

It is this contrast that provides the key to a proper understanding of the astrophysicist claim:

The key to understanding the astrophysicist’s use of ‘direct observation’ and related terms in his talk about neutrinos coming from the center of the sun is to be found in the contrast between the information so received and that based on the alternative available source of information about the solar core, the reception of electromagnetic information (light photons). (Shapere 1982, pp. 490–1.)

In contrasting the information received via neutrinos from that received via photons, Shapere identifies three aspects of the “observation situation” in the solar neutrino experiment, viz; the release of neutrinos by the source; the transmission

of neutrinos; and the reception of neutrinos by the detector. (Shapere calls these three aspects of the observational situation the theory of the source, the theory of transmission, and the theory of the receptor, respectively.)

Consider first the release of neutrinos from the sun, i.e., the theory of the source. Without background information such as the general theory of relativity; the equation  $E = mc^2$ ; the claim of modern physics that the universe is governed by four main forces — the strong, weak, electromagnetic, and gravitational forces; the theory of stellar evolution; etc., the experiment would have been inconceivable. It is because all these theories, laws, and equations, function as claims which are taken for granted that astrophysicists are able to conjecture the emission of neutrinos from the sun.

In the theory of transmission, background knowledge plays a crucial role as well. More specifically, because of the weak interaction theory, *information about the stellar surface received via photons* becomes analogous to *information about the stellar core received via neutrinos*. This is because the journey of photons to receptors on earth can be divided into two parts. The first is the long circuitous one from the core to the surface. This is the journey that can take up to 1,000,000 years. But once photons break onto the surface, the journey to receptors on earth take just about 8 minutes. Also, between the sun's surface and the earth, photons do not (except very infrequently) undergo any collisions which alter their character. Consequently, information about the surface of the sun brought via photons (information which is captured or detected by receptors such as telescopes, cameras, etc.) are regarded as authentic. Information recorded by telescopes, etc., are regarded as reliable because physics tells us that there is no significant interference with light-photons between the sun's surface and the recording of that information. In the same manner, since current theory specifies that there is hardly any interference with the information carried by neutrinos *en route* to the receptor from the core, information so received is as reliable as information about the stellar *surface* carried via photons.

Background knowledge also plays a crucial role in the theory of the receptor. Without the theoretical background of general relativity, the chemistry of chemical composition, etc., it would have been impossible to specify the sort of detector to construct; where to locate the detector; and how to interpret the information received.

The important point therefore is that the considerations which generate, guide and determine the results of the experiment involve a great deal of background knowledge. This background knowledge includes high level theories,

laws, equations, and practical know-how such as how to clean out the chlorine tank. Without this body of background knowledge, no one would have thought of doing this particular experiment; and no one, having thought of it, could do it perfectly. Hence, the theoretical claims operate as substantive parts of scientific knowledge in the sense that they make specific claims about the nature and constitution of stars. But these theoretical claims also perform *methodological* (i.e. heuristic) functions in the sense that they dictate the sorts of experiments that astrophysicists ought to perform, they constrain the sorts of conjectures that are allowable in the further development of astrophysics, and they also lay down constraints on the sorts of instruments to construct in solar physics.

More particularly, it also seems that, what counts as an observation, and the rules for interpreting observations are dependent on substantive scientific claims (which are subject to possible radical change).

Given that modern science entails the occurrence of processes and events to which the human senses have no access, science has built on what it knows by extending our ability to “observe” in previously unimagined ways. Consider the electromagnetic spectrum. According to modern science, the electromagnetic spectrum ranges from very short-wave high-frequency gamma rays, to very long-wave low-frequency radio waves. The total range of wavelengths between the two ends of the spectrum is about  $10^{22}$ . But the human eye is capable of receiving only a negligible sector of this very wide spectrum. Because of this background of assumptions, “the eye . . . comes to be regarded as a particular sort of electromagnetic receptor, capable of “detecting” electromagnetic waves of the “blue” to “red” wavelengths, there being other sorts of receptors capable of detecting other ranges of the spectrum. This *generalized notion of a receptor or detector* thus includes the eye as one type”. (Shapere 1982, p. 505) And with the advancement of science, various *detectors* which are capable of receiving other wavelengths within the spectrum were constructed.

Moreover Shapere insists that from an epistemological point of view, there is no justification for regarding the eye (or the human senses) as more reliable than these other sorts of *receptors* or *detectors*. First, the human senses are not infallible. Indeed, one of the traditional problems of epistemology is the problem of perception. And although everyone agrees that the human senses are *sometimes* unreliable, some philosophers have been hasty to argue that the human senses are not trustworthy. (Some have in fact argued that the possibility of perceptual error make our senses completely unreliable!) But more importantly, it makes no sense whatsoever to regard the human senses as alternatives to these other

receptors. For as the human senses are incapable of detecting those wavelengths received by these receptors, how can the senses be better receptors of information they are unable to detect?

What is *observable*, what counts as an *observation*, and what is *directly observed* are *not* established on the basis of *sense perception*. Rather, observability is established if there are *adequate receptors* which are capable of receiving certain kinds of information. And the human senses which constitute just one type of receptors are not as efficient and reliable as other types of receptors. The concept of observation in modern physical science has been extended and generalized on the basis of science's well-founded beliefs.

### 3. Two Senses of Methodology

A good deal of confusion has been wrought in recent philosophy of science by a failure to distinguish between two importantly different senses of the term "methodology", viz: a *narrow* (i.e. formal) sense of methodology and a *broad* (i.e. substantive) sense. (See, e.g., Worrall [1988] and Doppelt [1990].) These two senses of the term correspond to the *uses* of traditional philosophers like Carnap, Hempel, Popper and Reichenbach, on the one hand, and that of Kuhn, Toulmin and Feyerabend, on the other hand.<sup>2</sup> In the use of traditional philosophers, methodology is made up of those (more or less) *formal principles* which (they supposed) invariably govern theory appraisal in science. These principles are those which enabled traditionalists to deliver the judgment that one theory is, in view of the available *empirical evidence*, verified to a certain degree or at least better supported than its rivals. Furthermore, for these traditional philosophers, there was no question of there being different sets of methodological principles which are *correct* for different scientists (and philosophers) at different periods in history. Copernicanism was better than its Ptolemaic alternative for exactly the same sort of reasons that Newtonian mechanics is better than Aristotelian mechanics; and it is in turn because of the same sort of reason that Einsteinian mechanics is better than Newtonian mechanics.

Thus when traditional philosophers discussed methodology, their concern was with the *logic of scientific inquiry* in the sense that they were concerned with the basic principles and standards for the correct evaluation, comparison, and justification of scientific theories. It is because methodology in this narrow sense was concerned with principles and standards that were taken to be applicable to

*all* aspects of scientific inquiry that the validity or credibility of such principles did not depend on any substantive claims about the world.

Of course, philosophers have hotly disagreed about how correctly and exactly to characterize these formal principles. But when it comes to questions of how well specific theories stand up to empirical evidence (especially for theories that have been around for a while), despite their differences, traditionalists more or less arrive at the same ranking of theories: Copernicanism over Ptolemaic astronomy; quantum mechanics over Newtonian mechanics; Darwin's account of natural selection over Lamarck's alternative, etc.

Indeed, agreement goes beyond the ranking of theories in terms of how well they stand up to empirical test; traditional philosophers were also agreed on intuitive points such as the importance of subjecting scientific theories to rigorous testing, and that predictive success is an important criterion of scientific merit. Disagreement comes in at the more abstract level of giving a precise characterization of the principles which make up the logic of science.

But as Kuhn, Shapere, and others have argued, traditional philosophers did not assign due importance to one very important aspect of scientific research. This is that scientific research is always conducted from within a background of theoretical, metaphysical and factual assumptions. Whenever these *assumptions* are made in scientific research, they perform a dual function: on the one hand, they function as substantive claims which make specific assertions about the nature of the world (e.g. light is a wave-like disturbance in a medium; phlogiston is emitted into air in combustion; events in nature are deterministic). On the other hand, these assumptions also perform heuristic roles in the sense that: (i) they lay down certain requirements about what sorts of explanations, conjectures and theories are admissible within a domain of inquiry (e.g. any new theory of light must explain the wave-like properties of light if it is to be accepted); and (ii) they also specify the kinds of modification that are acceptable within their domain of inquiry (e.g. as long as the principle of determinism is accepted, any explanation in fluid mechanics, say, must not rely on indeterministic assumptions). Theoretical, metaphysical, and factual assumptions, therefore, also function in a natural way as *positive and negative heuristic principles* which guide the further development of science.

The solar neutrino experiment illustrates the heuristics meaning of methodology. Without a certain body of background knowledge, no one would have thought of doing this particular experiment. Consequently, although these theoretical claims operate as substantive parts of scientific knowledge in the sense

that they make specific claims about the nature and constitution of stars, they also perform heuristic functions in the sense that they guide the further development of astrophysical research. In particular, they constrain the sorts of conjectures that are allowable in the furtherance of astrophysics, and they lay down constraints on the sorts of instruments to construct.

The heuristic roles of scientific assumptions is one main usage of methodology adopted by Kuhn and Kuhnians for they do not regard methodology as simply the *logic of science*. (Indeed, some Kuhnians seem to imply that there is no narrow sense of methodology — that there is no logic of science.)

Shapere's view is similar to that of the Kuhnians in this respect. For, not only does Shapere deny the inviolability and invariance of any methodological rule, he also claims that *all* methodological rules are informed by the theoretical, metaphysical, and substantive beliefs of science.

There is no doubt that we need an analysis of the development of science which pays due attention to the important heuristic roles played by substantive scientific beliefs. Nevertheless the question can be asked: do the heuristic roles of substantive beliefs genuinely support the view that *all* methodological rules are subject to possible radical change?

Suppose we start by accepting a claim as “part” of *background knowledge* if that claim operates at any rate for some time as an unquestioned assumption within a context of scientific research. Then, we can identify (at least) the following four types (or “parts”) *background knowledge*: specific theories; general theories; highly general metaphysical principles; well-established empirical facts and observational laws.

Examples of *specific theories* would include Newton's three laws; the version of the wave theory of light that held that light waves are longitudinal; the alternative transverse wave account of double reflection, etc. Theories of this kind are *specific* in the sense that there are some higher level *theories* (or more general frameworks) within which they are developed. For instance, the general theory that light is some sort of disturbance which spreads out in a wave-like fashion in an all-pervading medium is one under which various specific theories, such as Fresnel's transverse wave account of light, and the earlier longitudinal version of the wave theory of light, are subsumed. The general corpuscular theory of light, or the general theory of evolution are also examples of general theories.

When specific and general theories are accepted in science, they perform *methodological* roles in the sense that they constrain the sorts of explanations and conjectures that are allowable in scientific research. It is, however, also a fact that

the history of science is littered with the ruins of such theories; radical changes in science have occurred at the level of both specific and general theories. It follows from this that *methodological* constraints attached to such theories are subject to radical historical change. This is because the heuristic (*methodological*) functions of substantive beliefs cease with the rejection of their corresponding beliefs. For instance, when the corpuscular theory of light was finally rejected and firmly replaced by the wave theory, scientists obviously stopped explaining optical phenomena in terms of light corpuscles.

Changes in substantive claims (and the corresponding changes in *methodological* heuristics) occur, however, in a rather definite, if complex, manner. For when scientists are confronted with *refutations* of specific theories that had previously been successful, they generally look for a different specific theory of the same general kind. Refutation of his initial longitudinal wave theory led Fresnel, for instance, to reject that specific theory in favor of another specific theory of the same general kind namely, the transverse wave theory. It is only after various attempts to produce specific theories of the same general kind have failed that scientists tend to challenge their more general theories.

Another aspect of background knowledge which is still more general than what I have called *general theories* is made up of *metaphysical assumptions and principles*. Examples include principles such as those of determinism and mechanism; the perfectionist and compositionalist theories of material substances, and various conservation and symmetry assumptions. These principles are more general in the sense that they are assumptions which cut across different general theories, “paradigms” or “research programmes”. For instance, the assumption that optics involved only mechanistic and deterministic processes is an assumption that formed part of the background knowledge of both corpuscularians and wave theoreticians.

As metaphysical assumptions are normally more firmly established in background knowledge than specific theories and general theories, they often provide justification for the acceptance (or rejection) of less general theories. When empirical difficulties arise in a domain of inquiry, scientists normally hold on to the general metaphysical principles as frameworks from which alternative general theories are to be found. (Of course, this need not be a *conscious* process.) Highly general assumptions tend to be replaced only after repeated failures to find general theories of the same metaphysical kind. This suggests that the more specific or less general the theory, the higher its intuitive likelihood of being replaced in situations of “crises”.

Consider, for instance, the changes that occurred in general theories about the nature of light. Although there have been very radical changes in optical theory from the corpuscular theory, through the wave theory to the electromagnetic theory (but with the exception of the photon theory), the general metaphysical assumptions that optics involved mechanical and deterministic processes remained constant. And as long as these assumptions were made, they provided part of the justification for change in theory.

The three aspects of background knowledge discussed so far all show that methodology (broadly conceived) can, and has, changed along with the substantive developments of science. But one aspect of scientific knowledge which has been essentially cumulative is its empirical aspect. A cursory look at the history of modern science will reveal that our empirical knowledge has grown enormously as science develops. Consider again the history of modern optics. Although there have been very radical changes at the purely theoretical level, there has been no such change at the empirical level. The corpuscular theory held that light consists of tiny particles, and the theory led to some important empirical consequences in optics. For instance, the theory's accounts of simple reflection and refraction were correct. The theory was, however, later rejected in favor of the wave theory which held, not that light is made up of material particles, but rather of periodic wave-like motions through a medium called the *luminiferous aether*. There was thus a very radical change at the theoretical level. Fresnel's *luminiferous aether* was later rejected in favor of Maxwell's electromagnetic field. And Maxwell's theory itself was still later replaced by the photon theory.

But the story is quite different at the empirical level. The corpuscular theory was able to give correct empirical accounts of simple reflection and refraction, and the wave theory was able to account for these and more by giving adequate explanations of diffraction, interference, and polarization. The electromagnetic and photon theories were also able to add to the empirical successes of their predecessors.

The contents of science's empirical knowledge can be described as "facts". These facts are usually taken to be the bases for the testing of scientific theories. We, however, need to distinguish between a narrow and a broad sense of the *fact*. For in the testing of their latest theories, scientists generally take for granted other theories which they have already regarded as true or certain. In the solar neutrino experiment, the astrophysicist takes for granted the theory of the big bang and the theory of stellar evolution. And since these theories function as part of the material against which the claim that neutrinos exist is tested, they

are taken for granted as “facts” (if “facts” are taken in the wider sense). But these “facts” are obviously different from facts like “the dial in the proportional counter is pointing at the mark ‘2’”.

The distinction between the narrow and the broad usages of fact have been described as “scientific” and “crude” facts by Poincaré [1958]. Scientific facts are statements which are taken to express true descriptions of reality, but which involve the use of other theoretical assumptions. But statements which do not depend upon the assumption of any high level theoretical assumptions express crude facts.<sup>3</sup>

If the term “fact” is used in its wide and rather attenuated sense, then obviously, radical discontinuities extend right down to the levels of “facts”. Various scientific facts which were once regarded as true descriptions of reality (e.g. phlogiston, ether, caloric) are now regarded as false. But if facts are taken to be low-level descriptions of reality (crude facts), then we have one part of background knowledge to which the sort of radical change Shapere envisages do not extend. In turn, the methodological rules that are informed by these aspects of science are more resistant to change.

My general point then is this. Just as we can identify two senses of *methodology* and two senses of *facts*, so can be distinguish between two broad classes of *background knowledge*: the *theoretical* class which is made up of specific theories, general theories, and highly general metaphysical principles; and the *factual* class. Included within the factual class are crude facts, and descriptive statements which require very low-level “theoretical” assumptions. If we take methodology in its narrow sense, it does not follow that *all* methodological rules are up for grabs in scientific change. For although those methodological stipulations which are informed by the theoretical parts of background knowledge will cease to perform their heuristic functions once their associated theoretical considerations are overthrown, those methodological constraints that are informed by the non-theoretical aspects of background knowledge would be more resistant to radical change. From the alleged fact that background beliefs play an important role in scientific methodology, it does not follow that *all* methodological rules and principles are subject to possible radical change. This is because there is an important difference between those methodological principles that are informed by the theoretical aspects of background knowledge (e.g. “look for mechanistic and deterministic optical theories”) and those that are related to the empirical and observational aspects of background knowledge (e.g. “any new theory of light must successfully explain phenomena such as polarization, diffraction, etc.,

which are some of the empirical successes of the photon theory of light). The rules which are informed by the factual aspects of background knowledge will, to say the least, be more resistant to change than those that are upshot of the theoretical aspects of background knowledge.

But those rules which are informed by the empirical and factual levels of science are in fact instances of a more general, and truly narrow, methodological rule. For instance, the rule that any new theory of light must successfully explain optical phenomena such as polarization and diffraction is in fact, a particular instance of a more general rule. This more general methodological rule has ultimately to do with the empirical and observational aspects of science, and it can be formulated as follows: *any new scientific theory must (eventually) explain all the empirical successes of its extant rival*. Another narrow methodological rule which is related to the empirical and observational aspects of background knowledge is the stipulation that: *genuine predictive success is a special mark of merit for a scientific theory*. The difference between these sort of rules and those related to the more theoretical claims is that the validity and justification of the rules of empirical support do not depend on the specification (and acceptance) of any specific substantive claim about the world.

The main result of my analysis of the different parts of background knowledge is therefore the following: those methodological rules which are informed by the theoretical and metaphysical aspects of background knowledge correspond to the principles of *broad methodology*. While those rules that are related to the empirical and observational aspects of scientific knowledge correspond to the principles of *narrow methodology*.

Moreover, *all* the changes that have occurred in broad methodology can be shown to have occurred in an effort to meet the requirements of the more formal and genuinely invariant standards of narrow methodology. Accepted beliefs (i.e. metaphysical assumptions, specific and general theories, and the empirical/observational claims) that operate as background knowledge at any stage in the development of science form a hierarchical structure in the sense that when confronted with difficulties, the more general claims provide the rationale for change in the less general claims. But just as these beliefs form a hierarchy, so do their associated heuristic principles. The more general a theoretical claim, the more resistant to change its associated methodological rule. And underlying all the changes that have occurred in broad methodology (the traditionalist would claim) is a set of some more restricted, more formal, methodological principles. Hence (the traditionalist would argue), changes that have occurred in

broad methodology have *all* occurred in light of these more formal methods. Scientists change their more substantive methods in an attempt to satisfy their more formal methodological requirements. If a (broad) methodological principle lays down the requirement that physical theories should be mechanistic, but a new theory, which is more predictively successful than the accepted theory flouts this principle, then, since the assumption of mechanism is highly theoretical anyway, the new theory can be accepted because it satisfies the more basic requirement of predictive success.

The traditionalist would, therefore, give an at least equally adequate account of all the (broad) methodological changes Shapere cites by responding that those heuristic principles which are tied to substantive scientific beliefs have the force they seem to have because they are themselves constrained by the more formal, invariant, standards of appraisal — namely fixed (or narrow) methodology. Methodological rules and principles which are deemed more formal and invariant would be regarded as providing the arbiter and rationale for changes in those more substantive rules.

Of course, these more restricted methodological norms are also linked with substantive science in the sense that they are the principles which rank theories in the light of empirical and predictive success. Hence in applying the norms of fixed (i.e. narrow) methodology, we have to examine substantive science to find out which theory is best supported by the evidence. But we should not confuse the fact that the *application* of a principle requires examining substantive science with the question of whether the *rationale* or *adequacy* of these principles themselves rely on substantive science.

#### 4. Methodological Relativism

In this section, I will assess Shapere's attempt to overcome cognitive relativism. This version of relativism makes the following claim: if a theory  $T_1$  (or a research programme  $R_1$ ), upholds  $M$  (where  $M$  is a set of methodological rules), and another theory  $T_2$  (or research programme  $R_2$ ) upholds  $M'$  (where  $M'$  is a rival set of methodological rules that is inconsistent with  $M$ ), and these rival rules are all *correct* according to the internal criteria of these rival theories (or research programmes), then there is no question of pronouncing the rules of any of these theories (or research programmes) wrong. There are no overarching criteria of rational assessment. There are no possible evaluations beyond those from within a specific theoretical unit (or research programme).

According to Shapere, although all aspects of science are in principle subject to revision and alteration, relativism is avoided insofar as change and alteration is effected by the *best* background beliefs of the domain in which change occurs. But which beliefs are to count as science's *best beliefs*? In Shapere's view, the *best beliefs* of any domain are a subset of that domain's *background knowledge*. Specifically, they are those background beliefs which are "successful" and "free from specific and compelling doubts":

... science need not appeal to a transcendent and irrevocable principle of rationality in order to account for the occurrence of rationality and progress within scientific change. For what better standards or criteria could we employ — at least when we are able — than those beliefs... that have proved successful and have not been confronted with specific doubt; or at least specific doubt which has either not been removed, or else which has been shown to be not compelling enough to worry about? In the attempt to find some basis for considering certain things to be observable, or for distinguishing between those hypotheses to consider and those not to consider, and so forth, what else should one expect to use and build on, whenever possible, if not such beliefs? No further sorts of reasons are available to us, and none further are required, in order to account for the rationality and progress of the scientific enterprise. (Shapere 1984, p. 270.)

In short, rationality depends on using "successful" beliefs that are "free from specific and compelling doubts" as the source of reasons for holding other (theoretical) beliefs. So, for example, part of the reason for believing that the solar neutrino experiment yields a direct observation of the solar core is the *successful* theory of the big bang. There was no "specific reason to doubt" that this is true of neutrinos at the time of the experiment concerned. But what does Shapere mean by "success" and "freedom from specific and compelling doubts"? The idea of success is intricately bound to the concept of "domain". As explained in section 1 above, a "domain" of inquiry is a body of related information, facts, beliefs, and theories, concerning which there are problems for scientific research. Examples of domains would include astrophysics, organic chemistry, fluid mechanics, etc. A theory (or belief) is "successful" if it accounts for the facts of its domains, or if it provides adequate solutions to the problems of its domain.

But before "successful" theories can function as a rationale of development, the conditions of "relevance" and "freedom from specific doubts" must also be satisfied. That is, only those claims within background knowledge that are: (i)

“successful”, (ii) “relevant” to a domain of inquiry in question, and (iii) are “free from specific and compelling doubts” can function as standards of scientific admissibility.

The *relevancy* condition states that “in any argument concerning a subject-matter, those considerations will be relevant as reasons that have to do with that subject-matter” (Shapere 1984, p. 263).

The condition of “freedom from doubt” is this; unless there is a particular reason to doubt a theory (or to reject a line of action), the mere general skeptical doubt that that theory might be wrong, (or that that line of action might be inappropriate), should not be the sole reason for rejecting that theory (or for inaction).

Shapere distinguishes between “universal doubts” and “specific doubts”. He claims that: “in the knowledge-seeking enterprise, universal doubt, doubt that applies indiscriminately to any belief whatever, is irrelevant; only doubts specific to a particular belief constitute reasons for doubting that belief.” (Shapere 1984, p. 237)

Shapere is surely correct in maintaining that mere universal doubt plays no significant role in the development of science. For if we have learnt anything from the history of science, we surely have learnt that even our currently best theories may turn out to be, strictly speaking, false. Scientific theories are never rejected because of the *mere possibility* of doubt.

But specific doubts are raised against particular beliefs. They are not doubts which arise because of the mere possibility that a belief might be wrong. They are doubts which arise because there is something specifically problematic about a belief or theory. For example, the results of the solar neutrino experiment have provided specific reasons to doubt current astrophysical theory. This is because the experiment in fact did *not* confirm the predictions of theory. Shapere puts the “failure” of the experiment as follows: “... there are subtleties about the notion of “observation” in this case because the expected neutrinos from the sun have *not* been observed. (The actual capture rate is consistent with *no* neutrino having been received from the source.)” (Shapere 1982, p. 513, fn. 14)

The neutrino deficit clearly illustrates Shapere’s distinction between specific and universal doubts. The deficit raises specific and compelling doubts against astrophysical claims. As John Bacall, one of the two major physicists who devised the experiment, puts it, the deficit indicates that “there is something wrong either with the sun or with the neutrino — or with what we think we know about them”.

The use of successful, relevant, and doubt free beliefs in effecting change also

illustrates a procedure Shapere describes as the “chain-of-reasoning connections” approach to scientific reasoning:

Methods, rules of reasoning, criteria (e.g., of what can count as an explanation) go hand-in-hand with the beliefs arrived at by their employment, and are on occasion altered in the light of the knowledge or beliefs arrived at by their means. Constraints on scientific reasoning develop, being sometimes tightened and sometimes broadened, as science proceeds. And thus, although at one stage of science, what (for example) counts as a legitimate scientific theory or problem or explanation or consideration might differ, even radically, from what counts as such at another stage, there is often a chain of developments connecting the two different set of criteria, a chain through which a “rational evolution” can be traced between the two. We can then recognize that, given the knowledge and criteria available at a particular time, certain beliefs about possibilities and truth were reasonable, even though alteration and improvement were later possible, with the emergence of new knowledge and new criteria. (Shapere 1984, p. 212.)

Shapere’s idea of chains of development connecting radically different sets of standards relies on a special use of *presuppositions* because a domain’s *best beliefs* also function as “presuppositions”. Those aspects of background knowledge that are successful and free from doubt also function as *presuppositions* on the basis of which scientific theories can be evaluated.

The presuppositions of traditionalists are founded upon the idea of an invariant method or logic (and on the reliance of science on observational facts) whose truth or validity is accepted, and on the basis of which science can be explained and evaluated as rational. But unlike the traditionalist, Shapere claims that his *presuppositions* are subject to (possible) radical change:

... the objectivity and rationality of science, far from demanding freedom from any “presuppositions” whatever, actually depends... on the employment in science of “presuppositions”, though only on ones which satisfy certain constraints. The employment of presuppositions is not only consistent with the rationality and objectivity of science; if (but only if) the presuppositions are of the right sort, their employment is necessary in order for science to be rational and objective. ... (Shapere 1985, p. 639.)

We should then carefully distinguish between two types of presuppositions; the *absolute* presuppositions of the traditionalist, and Shapere’s *relative* presuppositions. The presuppositions of the traditionalists are unalterable and ahistorical.

The results and the contents of scientific inquiry could not lead to modifications in these presuppositions as they are themselves constitutive of the criteria for assessing substantive science; they are the unjudged judges which supply science its rationality and objectivity. But the types of *presuppositions* Shapere (explicitly) allows into his model are part and parcel of the substantive content of science. They are those parts of background knowledge which (a) have proved successful, (b) concerning which there is no specific reason for doubt, and (c) which are relevant to the specific domain in which they are to function as *presuppositions*.

What guarantees the rationality of science, despite change in presuppositions and methods, is therefore the manner in which such changes are brought about. Changes in criteria of merit are not “conversion experiences” like the *gestalt switches* of Kuhn. Changes are brought about when there are specific reasons for doubting the adequacy of rules or methods. For example, when a new set of criteria is better able to account for the success of the theories of a domain. Moreover, the judgment that a rule is “adequate” and that a theory has “greater success” than another is made only in the light of criteria within the domain in question. On Shapere’s view, there is *no* criterion which is valid across domains.

The problem of how the basic principles (rules, and standards) of scientific reasoning can themselves evolve rationally is therefore (allegedly) solved by the following procedure. First, we find out whether there is some developmental connection between the different criteria of scientific appraisal such that one can be said to be a rational descendant of the other. When there is a developmental connection and change occurs because the old set of criteria is no longer acceptable (e.g. when there is specific reason to doubt the applicability of a rule or method) then, there is a chain-of-reasoning connection between radically different sets of standards; and, according to Shapere, rationality is preserved.

For instance, changes in the goals of inquiry may alter the nature of the beliefs and explanations that are required in a domain of inquiry. And as criteria of merit are inseparable from the content of science, there will also be change in the rules of merit. One example of such change given by Shapere is the following:

The chemical revolution of the eighteenth century... carried with it a change in conception of the goal of matter-study, from... the idea of bringing matter to perfection to the idea of understanding matter in terms of its constituents. That change of goal brought with it changes in conceptions of what it is for a view of matter to be “successful”. Standards of success are among our beliefs, and there are a variety of ways in

which they can change without the assumption of a transcendent, unchanging criterion of success. (Shapere 1984, pp. 269-279.)

This explains why Shapere claims to avoid the sort of relativism into which Kuhn falls. For whenever there is radical change in criteria of merit, on Shapere's view, *there are always good reasons for such change*. Moreover, not *any* sort of consideration can provide the reasons and rationale for change. Only considerations such as the failure of a previously accepted criterion (i.e. the criterion's failure at meeting its own set requirements — hence, a specific reason to doubt that criterion), supply the rationale for change. This also explains why the concept of reason is said to be that of “bootstrap conceptualization”:

... the concept of “reason” is a “bootstrap” process of finding — in effect, hypothesizing — that certain considerations can be counted as reasons, using those hypothesized reasons as bases for finding further relevance-relations in the light of which the original “reasons” can be critically evaluated, and so forth. Thus at any given stage, what counts as a reason presupposes prior “reasons”, and specifically, reasons for doubt. But such presupposition does not imply that the prior “reasons” cannot be criticized and rejected as reasons. (Shapere 1984, p. 272.)

Old methods are rejected and new ones introduced, and new standards of scientific acceptability laid down, all in the light of the body of background beliefs on which a domain of inquiry relies at any given stage in the history of science. Rationality is established, not because any rule or principle is sacrosanct, but because there are always scientific reasons for changing or rejecting any one rule or principle.

Suppose we grant Shapere the claim that no individual component of background knowledge is invariant. Would it follow that there are *absolutely no invariant characteristics of scientific rationality*? On the contrary, it seems that Shapere has actually succeeded in identifying exactly such invariant characteristics. Namely, the principle underlying his “chain-of-reasoning connections” approach, and the principles underlying the conditions of “success”, “relevance”, and “freedom from specific and compelling doubts”.

The process of chain-of-reasoning connections functions as an invariant attribute of scientific reasoning in the sense that it is a process of justification which *must* be employed *if change is to be rational*. That is, on Shapere's view, the acceptance of a new set of methodological standards in favor of an old one is rational only if we can trace a chain-of-reasoning connection between the two sets of

standards. This is precisely the aspect of Shapere's view of scientific change that is different from that of the Kuhnians. For although Shapere and the Kuhnians both maintain that all methodological constraints are subject to possible radical change, the Kuhnians claim: (i) that such changes are not rationally effected (they are like *gestalt switches* which occur all at once), and (ii) that social, non-scientific considerations must come in to augment choice. Shapere denies these two claims. On his view, scientific considerations are themselves sufficient to guide theory choice, and radical change is rational if it is governed by the process of reasoning-connections. Shapere thus seemingly avoids relativism *only* because he is also committed to the view that even in cases of apparent radical methodological change, there are connections that explains the change, as in fact a chain-of-reasoning.

Furthermore, on Shapere's account of scientific reasoning, although the considerations which form the bases for the acceptance and rejection of theories, rules, and methods are said to come from background knowledge, the *warrants* of rational change (or that which make choices rational) are not included within background knowledge. These *warrants* are those Shapere refers to as the conditions of "relevance", "success", and "freedom from specific and compelling doubts". These conditions function as invariant attributes of scientific reasoning in the sense that although background beliefs may change, before change can be regarded as rational, choice must be constrained by a process in which these conditions operate. Even on Shapere's model of scientific change, change (and in particular, change in substantive methodology), is rational *only if* we can identify a chain-of-reasoning connection which leads to newly accepted methodologies. So even if Shapere's view does not make any specific methodological rule invariant, changes in methodological commitments are nonetheless constrained by processes which are themselves not substantive background knowledge beliefs. Moreover, since we must *always* identify such a process if change is to be rational, the principle underlying the process itself functions as an invariant in Shapere's account.

## 5. Concluding Remarks

The foregoing has been a critical evaluation of Shapere's major contribution to the philosophy of science. With its emphasis on how the history of science has itself shaped the progress and growth of science, one of the main achievements

of Shapere's view is the point that scientific methodology cannot be reduced to a singular methodological postulate like "falsification". Rather, his view appreciates the fact that scientific activity operates on the basis of a set of facts, theories, and beliefs called background knowledge.

Although Shapere's view overcomes the type of relativism that plagued Kuhnian accounts of science, my argument here has been that Shapere has overcome relativism at a price. Because of the pivotal roles of the "principle of rejection of anticipations of nature", the "principle of scientific internalization", and his special usage of "success", "relevance" and "freedom from specific and compelling doubt", Shapere, I have maintained, has actually succeeded in showing that there are some invariant and inviolable attributes of science. The only alternative to making these aspects of Shapere's view invariant is precisely the type of relativism Shapere had set out to overcome.

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## Keywords

Domains of inquiry; methodological relativism; background knowledge.

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### Resumo

*Durante as últimas quatro décadas, Dudley Shapere desenvolveu uma rica e interessante alternativa à explicação kuhniana “relativista” da ciência e de seu desenvolvimento. Este artigo consiste em uma resenha desse ponto de vista alternativo. Trata-se de uma avaliação crítica do argumento de Shapere em apoio da asserção de que se pode permitir uma mudança metodológica radical na ciência sem com isso adotar o relativismo (e sem acabar em uma explicação irracionalista da mudança científica).*

### Palavras-chave

Domínios de investigação; relativismo metodológico; conhecimento de fundo.

### Notes

<sup>1</sup> Actually, the proton-proton sequence of reactions gives rise to three alternate sub-chains of reactions. The possibility of the occurrence of any one of these sub-chains is calculated by probability. Only one of sub-chain can lead to the production of neutrinos.

<sup>2</sup> It should be noted that most of these philosophers do not explicitly distinguish between the narrow and broad senses of methodology. Most traditional philosophers simply assumed the narrow sense of the term in their writings, while revolutionaries such as Kuhn created a lot of confusion by: (1) adopting the broader usage, and (2) advancing their usage as an alternative to the earlier narrower usage. But as I shall argue in the remainder of this section, the two senses of the term methodology are better regarded as complementary.

<sup>3</sup> Obviously, the term “theory” could also be used loosely. If so used, then one could claim (as Shapere does) that “there are no brute facts”. In this loose usage of “theory” claims such as “the computer screen before me is green and black” would employ “theoretical assumptions” about me, the computer, an external world in which the computer is located, etc. But surely there is a significant difference between statements like: “neutrinos exist”, “atoms exist”; and those like: “my computer screen is black and green”, “the lady in front is six feet tall”. The first set of statements express scientific facts, and those in the second set express crude facts.