"Scientific Rationality and Methodological Change:

A Critical Examination of Some Recent Attempts to Naturalize Methodology"

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Abstract

Following the work of Popper and especially of Kuhn in the 1960s, the attention of philosophers of science has been very much concentrated on *change* in science. Popper's picture was of constant change ("revolution in permanence") at the level of scientific *theories*, but constant change in accordance with *fixed* methodological standards of evaluation. Drawing on Kuhn's work, however, many recent philosophers of science have held that the phenomenon of scientific change is much more radical and far-reaching than anything allowed by Popper: specifically, that there have been major changes in methodological standards during the history of science alongside changes in accepted fundamental theory.

The chief problem facing this *no-invariant-methodology* thesis is that it seems to inevitably entail relativism. If the methods and principles of scientific theory appraisal are subject to radical change, then competing theories or research traditions may uphold competing (or conflicting) methodologies. When methodologies do conflict, how can choice between competing theories or research traditions be rationally adjudicated? How can the methods and principles for the correct appraisal of scientific theories themselves evolve rationally?

Two major attempts have been made in the recent literature to construct positions which accommodate change in methodological standards while nonetheless avoiding relativism. These are the versions of *methodological naturalism* developed by Larry Laudan and Dudley Shapere, respectively. This dissertation examines these two positions in detail and argues that they fail: in so far as they really incorporate the *no-invariant-methodology* thesis they inevitably embrace relativism. I argue that the way to resolve this

difficulty is to reject the *no-invariant-methodology* thesis. Moreover, methodological naturalists (like Laudan and Shapere) have not succeeded in giving any genuine and convincing illustration of *radical* methodological change.

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CHAPTER 1

Methodological Naturalism

1. INTRODUCTORY REMARKS

In this dissertation, I critically assess one version of the move to make methodology more informed by empirical considerations and the workings of science-- namely the naturalist approach to the study of scientific methodology.

Like most *...isms*, there are so many different versions of *naturalism* that it is essential to state precisely the version of it I examine. My subject is a variety of naturalism concerned with the status and validity of the methods that are used (or that ought to be used[1]) for the adjudication of scientific theories. This version of naturalism is primarily epistemological (not metaphysical). Philip Kitcher describes the general thesis of epistemological naturalism as follows:

Naturalistic epistemology confronts a range of traditional questions: What is

[1]Philosophers have often debated the issue of whether a naturalist philosophy can give normative advice on which methods scientists should adopt. I will not consider this issue for two reasons. First, the issue has often been confused with that of whether an *ought* (normative or prescriptive advice) can be *derived* from an *is* (a mere description of the methods actually employed in scientific practice). But, of course, a naturalist need not claim that he *derives* his methodological postulates from descriptions of scientific practice. Rather, the claim could be that from a description of the actual methods used by scientists, we can *construct* a philosophical thesis which also gives normative advice on which methods scientists ought to employ. So even if an ought cannot be derived from an is, the naturalist need not abstain from giving normative advice. Moreover, the naturalists I consider in this dissertation do not shy away from giving normative or prescriptive advice. I will not, therefore, examine the naturalism of philosophers like R.N. Giere and W.V.O. Quine.

knowledge? What kinds of knowledge (if any) are possible? What methods should we use for attaining knowledge, or at least, for improving the epistemic qualities of our beliefs? Because the sciences appear to be shining exemplars of human knowledge, the pursuit of these questions leads easily into the philosophy of science.

> Naturalistic philosophy of science emerges from the attempt to understand the growth of scientific knowledge. Epistemological naturalism can be characterized negatively by its rejection of post-Fregean approaches to these investigations. ... [Post-Fregean approaches] have two important presuppositions: first, following both Frege and the Wittgenstein of the *Tractatus*, they pursue epistemological questions in an apsychologistic way-- logic, not psychology, is the proper idiom for epistemological discussion; second, they conceive of the products of philosophical reflection as *a priori*-- knowledge is to be given a logical analysis, ... the improvement of methodology consists in formulating the logic of science. ... [N]aturalistic epistemology ... is committed to rejecting both [presuppositions of post-Fregean approaches]. (Kitcher, 1992, pp.56-58)

Larry Laudan also characterizes the epistemological variety of naturalism as follows:

> Epistemic naturalism ... is a theory about philosophic knowledge: in very brief compass, it holds that the claims of philosophy are to be adjudicated in the same ways that we adjudicate claims in other walks of life, such as science, common sense and law. More specifically, epistemic naturalism is a meta-epistemological thesis: it holds that the theory of knowledge is continuous with other sorts of theories about how the natural world is constituted. It claims that philosophy is neither logically prior to these other forms of inquiry nor superior to them as a mode of knowing. Naturalism thereby denies that the theory of knowledge is synthetic a priori (as Chisholm would have it), a set of "useful conventions" (as Popper insisted), "proto-scientific investigations" (in the Lorenzen sense) or the lackluster alternative to "edifying conversation" (in Rorty's phrase). The naturalistic epistemologist takes to heart the claim that his discipline is the theory of knowledge. He construes *epistemic claims as theories or hypotheses about inquiry, subject to precisely the same strategies of adjudication that we bring to bear on the assessment of theories within science and common sense*. (Laudan, 1990c, pp.44-45, my emphasis)

One main concern of proponents of this sort of naturalism is therefore the repudiation of the idea of *a priori*, invariant (ahistorical), epistemological principles.[2] More specifically, proponents of this version of naturalism insist that methodology is an *empirical* endeavour just like the natural sciences. It is empirical because the validity, warrant, and applicability of the principles and rules of scientific theory appraisal are to be assessed by considering substantive claims about the world.

Perhaps the chief source of the particular version of naturalism I consider in this dissertation is Thomas Kuhn's view of scientific change. In the remainder of this chapter, I delineate a connection between this version of naturalism and Kuhn's view of scientific change.[3]

[2]Unless otherwise indicated, I will use the term *a priori* simply as *non-empirical*. That is, it would not connote the Kantian view of an *a priori* which is indubitable or absolutely certain. Hence, in my usage of the term, a claim which is accepted (or known) on *a priori* grounds is not necessarily known to be true with absolute certainty; it is just a claim which is accepted on the basis of *reason* alone irrespective of its degree of dubitability.

[3]There are (at least) two main routes to epistemological naturalism in contemporary philosophy of science. One takes its upshot from Quine in the sense that it is primarily concerned with the introduction of psychology into epistemology and methodology (or rather, the reduction of epistemology to psychology); while the other is mainly concerned with making methodology more informed by the history of science (and hence the repudiation of ahistorical, a priori, methodological principles). In this dissertation, I will be concerned only with the historical version of naturalism outlined in this chapter.

2. METHODOLOGY AND SCIENTIFIC CHANGE

I shall, until further notice, use the term "methodology" to mean the study of the rules and standards which generally govern the evaluation and appraisal of scientific theories.

Until the early 1960s the belief flourished that scientific methodology is not an empirical discipline. If we substitute *methodology* for *philosophy* in the following claims of Wittgenstein's *Tractatus*, we have a very good depiction of this traditional (orthodox) attitude to methodology:

4.111	*Philosophy* is not one of the natural sciences.
4.112	*Philosophy* aims at the logical clarification of thoughts ...
4.1121	Psychology is no more closely related to *philosophy* than any other natural science. ...
4.1122	Darwin's theory has no more to do with *philosophy* than other hypothesis in natural science.

Thomas Kuhn's *Structure of Scientific Revolutions* however ushered in a revolutionary reaction to this traditional stance. Kuhn's *Structure* opens with the well known claim that:

> History, if viewed as a repository for more than anecdote or chronology, could provide a decisive transformation in the image of science by which we are now possessed. ... This essay attempts to show that we have been misled by [the old image] in fundamental ways. Its aim is a sketch of a quite different concept of science that can emerge from the historical record of the research activity itself. (Kuhn, 1962, p.1)

What exactly is "the image of science by which we [were then] possessed"? Kuhn is surprisingly unclear. Nevertheless, we can identify various counts on which Kuhn's view of science differs from the traditional view. For instance, the old image held that there is a sharp distinction between observation and theory, Kuhn denies this. Proponents of the old image held that observation and experiment provide the foundations for the rational acceptance of theories over their competitors; but Kuhn seems to claim that theory-choice is not a rational (or at least not a fully rational) affair. Proponents of the old image held that science can sharply be demarcated from non-science; Kuhn seems to deny this as well.

But perhaps the most fundamental contrast between the *old image* and the *new revolutionary image* is in their different approaches to the relationship between scientific method, scientific beliefs, and history.[4] According to the older image, scientific beliefs and theories may come and go, but the principles for the objective ranking of such beliefs and theories are eternal. The old image is therefore that of an ahistorical methodology in which the correct rules and standards of theory evaluation have remained stable and invariant throughout science's development. Methodology was regarded as invariant because the principles, rules and standards of theory appraisal were taken to be

[4]I do not wish to claim that *only* two approaches to methodology are identifiable in the history of philosophy of science! My identification of the traditional and revolutionary approaches is a simplification adopted for exegetical purposes. By concentrating on these two approaches, I aim to spell out one route contemporary philosophers have taken to naturalism. This route to naturalism takes its motivation from the Kuhnian historically oriented approach to philosophy of science because Kuhn maintains that: "... writing on philosophy of science would be improved if history played a larger background role in its preparation. When speaking here of the history of science, I refer to that central part of the field that is concerned with the evolution of scientific ideas, methods, and techniques ... when I speak of the philosophy of science, ... I am thinking of that central area that concerns itself with the scientific in general, asking, for example, about the structure of scientific theories, the status of theoretical entities, or the conditions under which scientists may properly claim to have produced knowledge." (Kuhn, 1977, p.12) Kuhn also insists that "history [is] ... relevant to the philosopher of science and ... the epistemologist in ways that transcend its classical role as a source of examples for previously occupied positions. ... [H]istory is an explanatory enterprise ..." (Kuhn, 1977, p.4-5) The approach Kuhn and Kuhnians sought to replace is what I describe as the orthodox (traditional) approach, (and I describe the Kuhnian approach as the revolutionary approach).

presuppositionless, or at any rate not dependent upon any specific substantive scientific claim for their validity.[5] Since methodology was regarded as not being dependent upon substantive science, traditional philosophers also claimed that these rules and principles of theory appraisal served as the neutral set of criteria for judging change and progress in science. In short, methodology was the basic tool of scientific rationality, and traditionalists believed that once they had hit upon the *correct* characterization of the criteria of scientific merit, these criteria were valid for all times-- past, present, and future.

Kuhn, Feyerabend, and the rebels, however, claim that the methods of science, the content of scientific beliefs, and scientific theories are fully intertwined with science's historical development. Scientific methodology (according to the revolutionaries) is fully liable to radical change as science develops:

> Successive paradigms tell us different things about the population of the universe and about that population's behaviour. They differ, that is, about such questions as the existence of subatomic particles, the materiality of light, and the conservation of heat and energy. These are substantive differences between successive paradigms ... But paradigms differ in more than substance, for they are directed not only to nature but also back upon the science that produced them. They are the source of the methods, problem-field, and standards of solution accepted by any mature scientific community at any given time. ...[N]ew paradigms necessitates a redefinition of the corresponding science. ... And as the problems change, so ... does the standard that distinguishes a real scientific solution from a mere metaphysical speculation ... . (Kuhn, 1962, p.103)

Kuhn is not merely committed to the claim that methodology is heavily informed and influenced by substantive science. He also explicitly claims that *radical* change extends

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[5]A closer analysis of this point is given in section 2 of chapter 3 above.

to the core principles of scientific theory appraisal:

> The transition from a paradigm in crisis to a new one from which a new tradition of normal science can emerge is far from a cumulative process, one achieved by an articulation or extension of the old paradigm. Rather it is a reconstruction of the field from new fundamentals, a construction that changes some of the field's most elementary theoretical generalizations as well as many of its paradigm methods and application ... When the transition is complete, the profession will have changed its view of the field, its methods, and its goals. (Kuhn, 1962, pp.84-85)

Indeed in Kuhn's view, "the case for cumulative development of sciences's problems and standards is even harder to make than the case for cumulation of theories". (Kuhn, 1962, p.108.)

Kuhn's revolutionary image relies heavily on the role of *paradigms* in scientific change. Unfortunately, Kuhn is entirely unclear and imprecise in his use of *paradigm.* Margaret Masterman (1970), for instance, identified twenty-one different uses of the term in Kuhn's *Structure of Scientific Revolutions*. The concept is in fact so vague and imprecise that Dudley Shapere accused Kuhn of using the term "to cover anything and everything that allows the scientist to do anything." (Shapere, 1984, pp.50-51)

Nonetheless, it is possible to identify some of the term's core uses. One core use of the term (perhaps *the* central use of "paradigm") is that in which it is a model which defines the world (or a whole sweep of reality) for a community of scientists:

> [By paradigm] ... I mean to suggest that some accepted examples of actual scientific practice-- examples which include law, theory, application, and instrumentation together-- provide models from which spring particular coherent traditions of scientific research. These are the traditions which the historian

> describes under such rubrics as 'Ptolemaic astronomy' (or 'Copernican'), 'Aristotelian dynamics' (or 'Newtonian'), 'corpuscular optics' (or 'wave optics'), and so on ... Men whose research is based on shared paradigms are committed to the same rules and standards of scientific practice. ... *[A paradigm is therefore] a fundamental unit that cannot be fully reduced to logically atomic components which might function in its stead* ... . (Kuhn, 1962, pp.10-11, my emphasis)

In this "world view" use of the term, a paradigm is the constellation of a group's commitments. Although it is almost impossible to specify all the elements of these constellations of commitments, proponents of the same paradigm must (at least) share ontological and methodological commitments. As Kuhn puts it:

> [A paradigm is a] strong network of commitments-- conceptual, theoretical, instrumental, and methodological ... it provides rules which tell the practitioner of a mature speciality what both the world and his science are like ... .(Kuhn, 1962, p.42)

One major reason why Kuhn has been charged with relativism is the pervasive role given to *paradigms* in his account of scientific change.[6] According to Kuhn:

> .. paradigms provide scientists not only with a map but also with some directions essential for map-making. *In learning a paradigm the scientist acquires theory, method, and standards together ... in an inextricable mixture.* (Kuhn, 1962, p.109)

In fact, Kuhn further claims that:

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[6]There are various other sources of relativism in Kuhn's *Structure of Scientific Revolutions*. For example his views concerning incommensurability, his attack on the notion of theory-independent facts, and his claim that theory changes are "conversion experiences", have all led various philosophers to accuse him of relativism.

> ... the proponent of competing paradigms practice their trades in different worlds. ... [Even though] both are looking at the same world, and what they look at has not changed ... in some areas they see different things, and they see them in different relations one to another. ... [This] is why, before they can hope to communicate, one group or the other must experience the conversion we have been calling a paradigm shift. Just because it is a transition between incommensurables, the transition between competing paradigms cannot be made a step at a time, forced by logic and neutral experience. (kuhn, 1962, p.150)

If facts underdetermine theory; if the change from one paradigm to another requires the reconstruction of *all* the fundamentals (ontologies, methods, aims, instruments, etc,) of a field anew; and if scientists adhering to different paradigms live in different incommensurable worlds; then some high degree of relativism must loom in theory choice. For Kuhn seems to have rejected all the bases on which rational choice can be made. More particularly, *if each paradigm contains within itself its own set of rules of theory appraisal, and there are no trans-paradigmatic methods, then the validity and rational acceptability of scientific theories are relative to each paradigm.*

Kuhn's own explicit claims about theory choice seem to clinch the relativistic interpretation of his view of theory choice. He claims, for instance, that:

> [Paradigm choice is] about techniques of persuasion, or about argument and counter-argument in a situation in which there can be no proof ... when asked about persuasion rather than proof, the question of the nature of scientific argument has no single or uniform answer. Individual scientists embrace a new paradigm for all sorts of reasons ... some of these reasons ... lie outside the apparent sphere of science entirely. Others must depend on idiosyncrasies of autobiography and personality. Even nationality or prior reputation of the innovator and his teachers can sometimes play a significant role ... Our concern will not then be with arguments that in fact convert one or another individual, but rather with the sort of community that always sooner or later re-forms a single group. (Kuhn, 1962, pp.152-153)

Kuhn, in fact, specifically claims that when a "revolution" occurs there are no universal standards according to which the revolutionary theory is objectively superior to its older rival, and, therefore, according to which those who reject the new theory are objectively mistaken. Not only are there always scientists who "hold out" for the older theory, their actions and beliefs do *not* violate any general standards:

> The transfer of allegiance from paradigm to paradigm is a *conversion experience* that cannot be forced. Lifelong resistance [to a new paradigm] ... is not a violation of scientific standards but an index to the nature of scientific research itself ... the historian ... will not find a point at which resistance becomes illogical or unscientific. (Kuhn, 1962, p.151)

Because of these claims, various philosophers have argued that theory choice, as explained by Kuhn in the *Structure of Scientific Revolutions* (and in some later explications of the claims made in *Structure*) leads to relativism and irrationalism.[7]

Although Kuhn insists that "reports of this sort manifest total misunderstanding" (1977, p.321) of his view, it seems to be Kuhn himself who underestimated the import and implications of his account of theory choice.

The version of naturalism considered in this dissertation takes its motivation from Kuhn's view of scientific change because proponents of this version of naturalism claim that Kuhn is (at least) correct in upholding the view that *all* epistemological principles of scientific theory appraisal are vulnerable to revision and radical change in the light of

[7] In particular, both Shapere and Laudan have accused Kuhn of relativism. Extensive charges of relativism against Kuhn can be found in Laudan's *Science and Values* [1984], and Shapere's *Reason and the Search for Knowledge* [1984].

substantive changes in science.[8] Agreeing with Kuhn, contemporary naturalists claim that radical changes in the development of science have not been restricted to substantive theories alone. All aspects of science, including its methods, are said to be subject to possible and actual radical change.

Alan Chalmers captures the full spirit of this version of naturalism in claiming that:

> We can expect methodologies to alter in the light of new discoveries, including practical discoveries, and for this reason the notion of a universal, ahistorical account of method that can serve as the standard, not only for present but for all future knowledge, is an absurdity ... There is no universal method. There are no universal standards. (Chalmers, 1986, pp. 25-26)

Dudley Shapere expresses similar sentiments in claiming that:

> There are no brute facts which confront us and force our theory choices in certain obligatory directions; there is no "given" which does not involve observation. Nor is there a single scientific method which is applied unambiguously across all science, past present or future. The extraction or testing of theories and hypothesis is far more complicated than can be captured by rules of any formal logic. ... *According to the view of science which I shall present, ... considerations guiding the development of science, far from being an a priori and essential characteristic present in science from its inception, has itself been a product of that development.* (Shapere, 1987, p.2, my emphasis)

[8]The delineation of the lineage of *naturalism* need not take its upshot from Kuhn. Undoubtedly, some version or the other of this sort of naturalism had been espoused in philosophy before the writings of Kuhn. Hence various sources of the contemporary naturalistic turn in philosophy of science could be identified. For instance, Philip Kitcher in his "The Naturalist Return" [1992] traces another lineage of naturalism as far back as Ernst Haeckel of the late nineteenth century. I emphasize Kuhn because the two philosophers I examine in detail in this thesis, Laudan and Shapere, developed their views as alternatives to Kuhn's radical views of scientific change.

And Larry Laudan describes his own position as follows:

* by way of underscoring th[e] parallel between epistemic rules and scientific theories, I have argued that *the rules guiding theory choice in the natural sciences have changed and evolved in response to new information in the same ways in which scientific theories have shifted in the face of new evidence;*

* ...the historicists are right [in claiming] that the aims (and methods) of science have changed through time, although some of their claims about how these changes occur (especially Kuhn's) are wide of the mark.

* the naturalist, if true to his conviction that science and philosophy are cut from the identical cloth, holds that the same mechanisms which guide the change of aims among scientists can guide the epistemologist's selection of epistemic virtues. (Laudan, 1990c, pp.46-47, my emphasis.)

Shapere and Laudan, therefore, both explicitly uphold the view that methodological principles for the appraisal and validation of scientific theories are subject to radical revision and rejection in the light of changes in substantive science.

We need to be very precise about the claim of the naturalists. Their claim is not merely that methodology is *informed by* substantive science. Their claim is not merely that there has been *methodological progress* (i.e. that we have come to acquire, discover, or invent new rules and principles of evaluation as a result of changes in our substantive beliefs about the world). Rather their claim is the strong Kuhnian one that the *validity and adequacy* of *all* methodological rules and principles rests on claims about the empirical world. Contemporary naturalism is fully committed to a complete denial of a priori assessment of methodologies. Moreover the naturalists I consider claim to overcome the problem of relativism with which Kuhn is saddled. The full claim is therefore that methodology can be fully empirical, and be subject to radical change, without sacrificing the rationality of science. I will refer to their version of naturalism as **methodological naturalism**.

3. THE NO-INVARIANT-METHODOLOGY THESIS

Methodological naturalism has been characterized as an epistemological thesis which claims that the validity and credibility of scientific methods ultimately depend upon science's substantive claims about the world. More specifically, anyone who upholds the following inter-related claims will be regarded as a **methodological naturalist:**

(a) The Historical Claim: As a matter of historical fact, radical changes in science have not been confined to the level of accepted general theories. Just as substantive science has changed and developed in response to new information and evidence, so have the basic rules and methods which guide theory appraisal changed in response to new information about the world.

(b) The Philosophical Claim: Even if (at least) some methodological principles have remained (relatively) stable throughout the history of science, in principle, there is no good ground for upholding an ahistorical, (invariant) attitude to scientific method. *All aspects of science are in principle subject to radical change and evolution in the light of new information about the world.* Methodological rules are subject to possible radical change because they are judged and evaluated in the light of substantive scientific beliefs (beliefs which are themselves subject to change and modification).

The philosophical claim is the most important aspect of the no-invariant-methodology thesis. This is because even if we can identify some methodological principles that have been present throughout the history of science, the naturalist would still insist that, as a purely philosophical point, no methodological rule *need* be ahistorical; there are no rules that are *in principle* immune to change as science develops. *The historical claim therefore provides collateral support to the naturalist's philosophical claim.*

One main stimulus of the no-invariant-methodology thesis is the naturalist's conviction that there is a striking mismatch between the methods postulated by philosophers (e.g falsificationism, inductivism, predictivism, etc.) and scientists' actual theory choices. Scientists, (the naturalist claims) have often accepted theories which are not sanctioned by philosophical views of scientific methodology. Thus both Shapere and Laudan criticize positivistic philosophers on the ground that although their accounts of scientific change are formally objective and rational (in a way that the Kuhnian view is not), these accounts do not fit science as historically practised.

This is why Shapere claims that one of "the most important weaknesses of the Logical Empiricist program" is that:

... in its concentration on technical problems of logic, the logical empiricist

> tradition has tended to lose contact with science, and [its] discussions have often been accused of irrelevancy to real science. ... for in their involvement with logical details (often without more than cursory discussion of any application to science at all), in their claim to be talking only about thoroughly developed scientific theories (if there are any such), and in their failure (or refusal) to attend at all to questions about the historical development of actual science, logical empiricists have certainly laid themselves open to the criticism of being, despite their professed empiricism, too rationalistic in failing to keep an attentive eye on the facts which constitute the subject matter of the philosophy of science. (Shapere, 1984, p.61)

In the same spirit, Laudan also claims that:

> The historicists are surely right in thinking that existing methodologies often fail to pick out the theories which the scientific elite have chosen. Thus, Newton's physics was accepted long before it was known to have made any successful surprising prediction, thereby violating the rules of Popperian methodology. Galilean physics was accepted in preference to Aristotle's, despite the fact that Aristotle's physics was much more general than Galileo's, thereby violating Popper's and Lakatos' injunction that successor theories should be more general than their predecessors. (Laudan, 1987a, p.21.)

Having convinced themselves that all attempts at formulating ahistorical methodological principles have failed in the sense that such ahistorical methodologies do not square with scientific practice, the naturalist goes on to advocate that we abandon all claims concerning the *apriority* and invariance of methodological principles. Instead we should adopt a fully empirical approach to methodology in which the actual rules and principles that present (and past) scientists use are taken as guides to the formulation of our scientific methodologies. These empirically constructed methodologies would also be tested and evaluated by empirical evidence.

The naturalist does not take methodology to be *empirical* in the sense of "reducible to the phenomena" or "immediately apprehended matters of fact". The claim is merely that methodologies should be as empirical as the natural sciences in the sense that their adequacy should be intricately bound to substantive science.

Laudan in particular advocates an inductivist approach in which the methodologist gathers empirical evidence about which rules and principles scientists have successfully applied in the past. The methodologist would then recommend only those methods which have been most effective (in the past) for advancing scientists' cognitive goals.

Naturalists also claim to overcome the pitfalls of both traditionalists and revolutionaries. They claim to break the impasse between: (i) the formally objective and rational, but historically inadequate, views of the traditionalist, and, (ii) the historically relevant, but relativistic, accounts of the Kuhnians. Methodological naturalism is thus taken to be the best of both worlds: a view of scientific methodology in which all epistemological rules of theory appraisal are in principle subject to radical revision and abandonment, but in which relativism and irrationalism of theory-choice (and of method-choice) are avoided.

But is methodological naturalism a viable alternative theory of scientific method? Can the ahistorical and the invariant be cast out of methodology without thereby embracing relativism-- or without ending with an irrational (or non-rational) account of scientific change? In chapters 2 and 3, I subject the philosophical claims of the Shapere and Laudan respectively to critical assessment. In assessing these two influential account of methodological naturalism, I indicate how naturalism is correct in emphasizing the relevance of substantive science to methodology. There has been progress in scientific methodology, and naturalists are right in emphasizing the importance of substantive science for theory appraisal. Nevertheless, I will argue that naturalist's are mistaken in claiming that *all* aspects of scientific methodology are subject to possible radical change. One can appreciate and accept the point that methodology must be informed by substantive science without denying that methodology is invariant. My main contention will be that if naturalism is so strong that methodology is itself an empirical discipline which can only be justified by the substantive claims of the science it validates, we end up with relativism.[9]

In chapter 4, I examine in detail one of Laudan's main examples of radical methodological change-- namely the alleged change in early 19th century from Newtonian inductivism to the method of hypothesis. I argue in this chapter that a proper analysis of this historical episode does not support Laudan's claim of radical methodological change.

[9]It is important to state very clearly that I am not interested in those versions of naturalism that give up the rationality and objectivity of science. I am only interested in evaluating the adequacy of two versions of methodological naturalism that also claim to be objectively rational. My main question therefore is: can the rationality of science be upheld if the no-invariant-methodology thesis is fully accepted?

CHAPTER 2

Shapere's Bootstrappism

1. THE CHARACTER OF SCIENTIFIC CHANGE

In this chapter, I examine Dudley Shapere's version of the no-invariant-methodology thesis. In particular, I critically evaluate his arguments in support of the claim that methodological change can be allowed without thereby embracing relativism (or without ending with an irrational account of scientific change).

Shapere's view of scientific change starts with the basic idea that *"science builds on what it has learned"* (Shapere, 1982, p.485) in the sense that its established *theories, laws and assertions* (i.e. established science) guide the articulation and construction of new theories; they guide practical action, and also constrain possible conjectures. According to Shapere, the "process of building consists not only in adding to our substantive knowledge, but also in increasing our ability to learn about nature ...". (1982, p.485)

Shapere quite explicitly claims that 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. ... A cycle of mutual adjustment of beliefs and methods has thus become a characteristic feature of the scientific enterprise. ... Nor is the process ... limited to the gradual integration of methodology into the rest of science. What counts as a reason, too, has become a function of scientific belief-- belief which itself has been attained by a process of reasoning. (Shapere, 1987a, p.5)

Shapere's claim is not merely that scientific methodology *evolves* or is *modifiable* in the process of learning more about the world. On Shapere's view there is *radical* methodological change just as there is *radical* substantive and theoretical change:

> ... it is important to realize that *radical* changes in the fabric of science have not been restricted to alterations of our substantive beliefs about how things are. They have also extended to the *methods and rules of reasoning* by which we arrive at those beliefs, and the aims we have in seeking them. (Shapere, 1987a, p.4, my emphasis)

The full claim is thus that there is *nothing* unalterable or sacrosanct in science.

Shapere indeed refers to any thesis in which certain characteristics of science (be they methods; or rules of logic; or even claims about the nature of the world, e.g. principle of uniformity of nature) are immune to abandonment (or change) as upholding an "Inviolability" or "Presuppositionist" thesis. He identifies four main versions of this thesis-- each of which he rejects:

> (1) The view that there are certain claims about the way the world is which must be accepted before any empirical inquiry is possible, or before further beliefs (well-founded beliefs) can be acquired, which claims, being presuppositions of the knowledge acquiring process, cannot be revised or

rejected in the light of any result or process.

(2) The view that there is a method, 'the scientific method', by application of which knowledge or well-grounded belief about the world is obtained, but which, once discovered (by whatever means), is in principle not subject to alteration in the light of any belief arrived at by its means.

(3) The view that there are rules of reasoning-- rules, for example, of deductive or inductive logic-- which are applied in scientific reasoning, but which can never be changed because of any scientific results.

(4) The view that certain concepts are employed in or in talking about science which are not open to abandonment, modification, or replacement in the light of new knowledge or (well-founded) beliefs.

> I will refer to all such views indiscriminately as 'presuppositionist' views of science, though the four sorts of views I have mentioned tend to hold further that the alleged presuppositions are *a priori* (or at least formal), and that they constitute invariant characteristics of science .... (Shapere, 1984, pp. 205-206)

Agreeing with Kuhn, Feyerabend, and others, he maintains that it is the study of the historical development of science that shows how change has gone deeper than mere changes of theory:

> A torrent of historical studies have indicated more and more convincingly that changes over the development of science have gone deeper than mere change of theory. The changes seemed to extend also to what was counted as evidence, as observation, as factual; to criteria of adequacy of explanations of that evidence, or those observations or facts, and even to what counted as explanation; and to method, which seemed not to be a single thing after all, but a multiplicity varying from period to period and subject to subject. (Shapere, 1987b, p.1)

As already indicated, 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). Thus, a particular system of belief X is *better* than another system of belief Y only in the sense that X *judges itself better from within its own included criteria.* In issues of justification, according to the relativist, there is no external or independent set of criteria to which we can appeal in deciding between alternative systems.[1] Is Shapere not committed to some version or the other of relativism? 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 more general contexts of scientific research, such as research traditions 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? If each scientific tradition includes within itself its own standards of evaluation, in what sense can theory choice, especially in situations of competing theories (such as Darwinism vs. Lamarck's alternative) or in situations of competing research traditions (such as theories of evolution vs. Creationism), be regarded as objective? In particular, how can the principles and standards for the correct appraisal of scientific theories themselves evolve rationally?

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

[1]A detailed analysis of the problem of relativism is given in section 4 above.

governed by some "broader" and more fundamental "interpretative frameworks" called *paradigms*. The acknowledgement of the existence of (corrigible) fundamental interpretative frameworks in science in itself need not lead to relativism. But Kuhn's particular account of paradigms as fundamental interpretative frameworks for analysing change does lead to relativism.[2] In Kuhn's view, *paradigms* guide the construction of evidence, facts, observation, and theory; they also determine methodological rules, and they lay down the principles of scientific theory appraisal in each field of science. But as these paradigms differ fundamentally from tradition to tradition, or from group to group, (competing paradigms are in fact regarded as incommensurable), and proponents of different (incommensurable) paradigms employ different methods and adjudicating principles, Kuhn is unable to give any rational account of scientific change:

> In emphasizing the determinative role of background paradigms, and [in] ... attacking the notion of ... any ... independent factors or standards whatever, Kuhn appears to have denied the possibility of reasonable judgement, on objective grounds, in paradigm choice; there can be no good reason for accepting a new paradigm, for the very notion of a *good reason* has been made paradigm-dependent. ... Objectivity and progress, the pride of traditional interpretations of science, have both been abandoned. (Shapere, 1984, p.51)

Shapere thus recognizes the point that if facts, methods, and the correct standards of scientific theory appraisal all depend on paradigms, (or on any such fundamental interpretative framework-- e.g. research programmes, or research traditions), and

[2] I am not suggesting that this is the only count on which Shapere charges Kuhn of relativism. In *Reason and the Search for Knowledge* [1984] (and elsewhere) Shapere discusses at length other sources of relativism in Kuhn's view of scientific change. However, the central charge of relativism stems from the role of paradigms in scientific change; and this form of Kuhnian relativism is the one most closely related to the role and status of methodology in scientific change.

paradigms vary radically from epoch to epoch, then there can be no independent constraints standing above individual paradigms on the basis of which rational choices can be made between conflicting paradigms. Nevertheless, Shapere still insists that:

> Science ... develops through a give-and-take interaction between the methods with which it approaches nature and what it learns about nature ... . Included in that interactive development are ... the subject-matter, the problem-structure, the standards, and the goals of science: in all these aspects, science is subject to change. (Shapere, 1984, p.xxxiii)

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. Shapere is quite explicit about this. He asks:

> ... can an account of the knowledge-seeking and knowledge-acquiring enterprise be given which, while not relying on any form of Inviolability thesis, will not also collapse into relativism? ... Is it possible to understand science ... as able to proceed rationally without presupposing criteria of what counts as "rational", criteria which could be arrived at in the course of seeking knowledge, but which must be assumed in order to engage in that enterprise at all, or at least successfully? (Shapere, 1984, p. xxi)

Shapere answers these questions in the affirmative. His answer to these questions lies in his conception of *scientific reason*. This account of scientific reason (and of scientific rationality) in turn relies on the role of *background knowledge* in the development of science.

To clarify Shapere's view of scientific reasoning and his view that background knowledge plays an important role in the further development of science, I start by *describing* his view of the nature of scientific change in the next section. Particular attention will be given to Shapere's emphasis on the role of the history of science. In section 3 I go on to assess the adequacy of Shapere's claim that the important role of background knowledge indicates how radical change has extended to *all* aspects of science. Finally, in sections 4 and 5 I examine the issue of whether Shapere's version of the no-invariant-methodology thesis truly succeeds in avoiding 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 which he calls: (a) the "Principle of Rejection of Anticipations of Nature"; and (b), 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), Shapere insists that no one could have anticipated complex theories such as the quantum theory, the general theory of relativity, and evolutionary Darwinism. Consider specifically 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.

Contemporary conceptions of space-time and matter provide another good illustration of the principle of rejection of anticipations of nature. The fusion of the concepts of space and time into that of space-time contradicts our common sense beliefs (and earlier physics). And with the advent of the general theory of relativity, matter and space-time became causally connected in ways that were not conceived even at the time of Newton.

We need to be very precise about Shapere's emphasis on this principle. For there are two readings one could give to the "principle of rejection of anticipations of nature". On the first reading, since the principle states that the results of scientific investigation could not have been anticipated by common sense, by everyday experience, or by "pure reason", the principle can be read in such a way that it is merely a principle for ruling out certain types of ideas in science. This interpretation commits anyone who upholds the principle to the claim that no new idea in science can be counted as a revolutionary innovative development. The principle would thus amount to the position that all developments in science have recognizable precedents.

Some of the acknowledged important revolutions in the history of science can (naively) be interpreted along the suggestion of this interpretation. The central thesis of the Copernican revolution is the idea that the earth is a planet-- like Mars and Mercury--

which revolves round the sun. But this central thesis itself was not radically new in the sense that it had no recognizable antecedent in the history of ideas. The Greek philosopher, Aristarchus, for instance, upheld this basic thesis long before Copernicus. Also the central thesis of Darwin's theory of evolution-- the thesis that species evolve from simpler organisms into complex ones-- had long been around before the Darwinian revolution. Interpreting the principle of rejection of anticipations of nature in this manner would commit Shapere to the view that all revolutions in science are of the Copernican and Darwinian type. That is, according to this interpretation, Shapere would be committed to the view that all important theories and ideas in the development of science have recognizable antecedents in history.

There is however no justification whatsoever for foisting this interpretation on Shapere.[3] Because while this interpretation limits the horizon of science, (i.e. on this naive interpretation, the principle becomes a principle for ruling out those ideas which lack precursors), Shapere in fact advances the principle to stress the point that science transcends common sense everyday experience, and pure reason, in previously unimagined ways. (And there is no reason to suppose that it will not continue to do so.) The correct interpretation of the principle is that in which it emphasizes the point that, on the bases of common sense or pure reason, we should not try to anticipate the complexities to be found within nature. For instance, the principle urges that we should not anticipate whether science will, or will not, have a grand unified theory of the four main forces in

[3]This first interpretation of the principle is untenable. For it is only by an incredible stretching of the imagination that one can identify precursors of all innovative ideas in science. I draw attention to this reading of the principle simply to ensure that we have a good grasp of why Shapere attaches some importance to the principle of rejection of anticipations of nature.

nature (the strong, weak, electromagnetic, and gravitational forces) on the basis of pure reason. Rather, questions such as this must be resolved by a deeper understanding of nature. The best interpretation of this principle is thus that in which it emphasizes the development of science as a give-and-take procedure in which cognitive beings interact with nature, and in which we learn how to learn in the process of learning: "The principle [is not] a means of ruling out certain types of ideas, rather [it is] a principle [for] opening the door to unforeseen possibilities." (Shapere, 1987b, p.1)

Although this principle 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 insists that we need to supplement 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, p.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)

And in contrast with the negative message of the principle of rejection of anticipations of nature, Shapere insists that:

> It is a *normative* principle; and its value, its necessity, as a policy, a guiding principle, of science is something that has itself been learned through the scientific process, through a record of achievement that led to its adoption". (Shapere, 1987a, p.4)

Again, 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.[4]

The fact that the principle of internalization now governs scientific activity and the fact that this principle had to be learned can, according to Shapere, be 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 endeavour. 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. Consequently, all aspects of nature were regarded as relevant and important to their explanations of phenomena. 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

[4]Of course, Shapere does not claim that *any* sort of belief or assertion can operate as *background knowledge!* In Shapere's view, only those beliefs that are *successful, free from specific and compelling doubts,* and which are *relevant* to a piece of inquiry can function as *background knowledge* in a scientific *domain.* I give a detailed analysis of these requirements in section 4.

understand nature and the universe as a whole, (as the Milesians did), various aspects of nature (such as bodies, salts, stars, and gases), were investigated in isolation from each other. The *newer* approach "replaced [the] older holistic approach, stemming from Milesian philosophy of trying to explain the nature of change or of substance *in general.* The specific subject-matters for study in this new approach may be referred to as *domains of investigation*". (Shapere, 1987a, p.3)

But what are domains of investigation or 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 is not claiming that any association of items of information can be regarded as a domain of inquiry! Only those associations which exhibit the following characteristics are acceptable:

(1) The association is based on some [genuine, unitary] relationship between the items.
(2) There is something problematic about the body so related.
(3) The problem is an important one.
(4) Science is "ready" to deal with the problem. (Shapere, 1984, p.279)

Shapere also adds the following caveats:

> ... we will find that, in science, such bodies of information have other characteristics besides these four. [And] we will find that, although it is generally desirable for (4) to be satisfied (to an appropriate degree) in order that an area count as fully scientific ..., nevertheless areas which satisfy conditions having to do with (1), and (2), and (3) are often counted as "scientific" ... even if they fail to satisfy (4). Similar qualifications will be found necessary in regard to (3). (Shapere, 1984, p.280)

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 unification of the phenomena of electricity and magnetism into that of electromagnetism provides one clear example of how changes occur in the boundaries of domains of inquiry. In the early history of electricity and magnetism, the two sets of phenomena were regarded as different. William Gilbert, for instance, having discovered that when various metals were rubbed together they attract light bodies, identified various differences between electricity and magnetism on the basis of sensory qualities:

> Between the magnetic and electric forces Gilbert remarked many distinctions. The loadstone requires no stimulus of friction such as is needed to stir glass and sulphur into activity. The loadstone attracts only magnetisable substances, whereas electrified bodies attract everything. The magnetic attraction between two bodies is not affected by interposing a sheet of paper or linen cloth, or by immersing the bodies in water; whereas the electric attraction is readily destroyed by screens. Lastly, the magnetic force tends to arrange bodies in definite orientations; while the electric force merely tends to heap them together in shapeless clusters. (E.T. Whittaker, 1951, quoted in Shapere 1984, p.274)

But by the 19th century, various questions about the phenomena of electricity were raised. How is electricity conducted? Is it produced in inanimate objects alone? Answers to such questions suggested fundamental similarities between electricity and magnetism. Various scientists including Franklin and Faraday investigated the similarities between electricity and magnetism. And with the advent of Maxwell's theory of electromagnetism, the two subject-matters became unified as one domain of inquiry. Whittaker describes an occasion in which similarities between the two phenomena is observed as follows:

> The suspicion [that there were fundamental similarities between the two phenomena] was based in part on some curious effects produced by lightning, of a kind which may be illustrated by a paper published in the *Philosophical Transactions* in 1735. A tradesman of Wakefield, we are told, "having put up a great number of knifes and forks in a large box, and having placed the box in the corner of a large room, there happen'd in July, 1731, a sudden storm of thunder, lightning, etc., by which the corner of the room was damaged, the box split, and a good many knifes and forks melted, the sheaths being untouched. The owner emptying the box upon a counter where some Nails lay, the Persons who took up the knifes, that lay upon the Nails, observed that the Knifes took up the Nails. (Whittaker, 1951, quoted in Shapere 1984, p.274-275)

The contrast between 17th century natural philosophy and 20th century science provides another 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 (i.e. research on the basis of distinction domains) 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)

The foregoing is the second main lesson from the history of science -- the lesson emphasized by Shapere's *Principle of Scientific Internalization.* According to this principle *all* aspects of science are altered and shaped by changes in substantive beliefs.

To fully appreciate the full content of Shapere's principle of scientific internalization, I will examine in some detail two of Shapere's main examples of internalization -- the solar neutrino experiment, and the 18th century revolution in chemistry.

In the solar neutrino experiment, astrophysicists claim to "directly observe" the production of neutrinos in the central region of the sun:[5]

> ... neutrinos originate in the very hot stellar core, in a volume less than a millionth of the total solar volume. This core region is so well shielded by the surrounding layers that neutrinos present the only way of directly observing it. (Weekes 1969, quoted in Shapere, 1982, p.489)

> There is no way known other than by neutrinos to see into a stellar interior. (Clayton, 1968, quoted in Shapere, 1982, p.486)

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

[5]So far I have used various examples from the history of science (a) to explicate Shapere's emphasis on the role of background knowledge in scientific change; and, (b) to explain the concept of *scientific domains*. In the remainder of this section, I will focus on two further examples of Shapere (the solar neutrino experiment, and the theories of material substances in chemistry). In my consideration of these two further examples, I shift the focus of attention from the concept of *domains* to *the process of internalization* itself.

hydrogen into helium. This is the so-called proton-proton sequence of reactions. This main sequence of reactions leads to another chain of reactions[6] which culminates in the production of the radioactive isotope Boron 8 (^8B). When this radioactive isotope decays, it releases neutrinos which are highly energetic. Travelling at the speed of light, neutrinos are believed to bombard every square centimetre 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. That is, the nature of neutrinos are not altered in the process of getting to the surface of the earth.

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 of a chain of reactions in the tank of perchloroethylene. The expected reaction was the

[6]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.

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?:

> Is the scientist using the term "observation" and its cognates in ways which are at best only tenuously related to the philosophers usage, and perhaps to ordinary usage as well, so that the scientist's way of speaking is misleading, at least to the non-scientist and perhaps even to the scientist himself? Or are the philosopher and the astrophysicist interested in entirely different and unrelated problems, which are reflected in their different usages of the term, so that they are talking completely past one another even though their usages are, from their respective points of view, equally legitimate? (Shapere, 1982, p.486)

One obvious response seems to be that the astrophysicist *infer* 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)

Shapere claims that 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.490-491)

In his contrast between 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 in astrophysics 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. For the key to the astrophysicist's claim lies in the claim that neutrinos hardly interact with other particles. More specifically, because of the weak interaction theory, *information about the stellar core received via neutrinos* becomes analogous to *information about the stellar surface received via photons*. 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). This explains Shapere claims that the experiment shows:

> ... three major ways in which the concept of observation has become integrated into the fabric of scientific beliefs. First, the status of observation as the primary means of testing beliefs and obtaining knowledge ... . Second, the interpretation of what counts as an observation has come more and more to depend on the content of the very scientific beliefs to which it itself has led. ... And third, the exact ways in which observation plays its evidential roles-- of confirmation and disconfirmation, verification and falsification have been shaped by the content of beliefs about the world whose warrant have themselves come from observation. ... The problems we face in our inquiries about nature, and the methods with which we attempt to deal with those problems, co-evolve with our beliefs about nature. ... The methods we employ lead us to new beliefs, which in turn lead us to modify those very methods, sometimes replacing them with new ones. The result is a growing integration of method with belief. ... it is a process of the *internalization* of methodology into the rest of science. (Shapere 1987a, p.5)

The role of background information is crucial. For, 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:

> [A] generalization of the notion of a "receptor" is made in the light of the existence of these further sorts of interaction: an "appropriate receptor" can now be understood in terms of the instrument which is able to detect the presence of such an interaction ... . Thus the extension of knowledge has led to a natural extension of what counts as observational: the very fact that information received by the eye becomes subsumed under a more general type of information leads to the treatment of the eye as a particular type of receptor of that information. Further discovery that that type of information (electromagnetic) is only one of four types of information leads to a further generalization. (Shapere, 1982, pp.505-506)

Of course Shapere is not claiming that sense perception has no significant role to play in this generalized conception of observation! After all, whatever is *observed* or *recorded* by any non-human receptor (i.e. those that are not recorded by the human senses) still has to be transformed into humanly accessible form. For instance, before the information recorded in the solar neutrino experiment can be of any use, it must be transformed into audible clicks, photographs, or computer print-outs. Human perception still has an important role to play in this *naturalized* view of observation. But this point in no way undermines the significance of Shapere's central thesis. Because what Shapere's thesis amount to is the claim that: as a result of developments in our scientific beliefs about the nature of the world, a distinction has been made between the *perceptual* and the *epistemological* aspects of *observation.* From an epistemological point of view, an *observation* need not be *perceptual* or directly accessible to the human senses. Humans need not be present when useful information is recorded by adequate receptors. And as the human senses are in fact incapable of directly receiving these sort of information, *observation* is no longer equated with *perception*. Hence although the astrophysicist does not *perceive* the central region of the sun, she is nonetheless justified in her claim to be *observing* processes occurring in the sun. She is also justified in claiming that the observation is *direct*. This is because the measures of *directness* are no longer the senses. The *directness* of observation is determined by the sorts of interference that can affect information in-between the *source* of that information and its *reception*.

Another example of the Principle of Internalization Shapere considers is associated with the 18th century revolution in chemistry-- the revolution in which Lavoisier and others brought about profound changes in the understanding of chemical substances. According to Shapere, a distinction can be drawn between two general approaches to the study of material substances; the *Perfectionist* approach, and the *Compositionalist* approach. The former approach was embedded in the pre-modern idea that material substances are like plants and animals which grow to maturity. This view is essentially the one adopted by alchemists in their research. For the alchemist, there is only one kind of element, "earth". All substances exist in various degrees of *actualization* or *perfection* of the element "earth". A substance was regarded as *imperfect* not because it was an admixture of other substances (i.e. not because it was a compound). Rather, substances are *imperfect* because they exist in a *potential* form of the most *perfect* stage of matter. The most perfect stage of matter (i.e. the highest degree of "actualization" of the element "earth"), was taken to be gold.

Within the perfectionist tradition, substances were usually described and named on the basis of their obvious sensory properties. (Naming was, however, sometimes based on considerations such as place of discovery, and the uses of the substance in question.) The naming of substances on the basis of their apparent sensory properties had profound implications on alchemical research. For instance, as colours were also ordered in a hierarchy from gold as the highest to blue as the lowest (or most imperfect) colour, bringing a substance to perfection involved processes such as: bringing the substance in question to a state of "formlessness" (i.e. the process of bleaching, which was also described as the process of "killing" the form of substances), and the process of "tinting" the now "formless" substance to a more perfect colour.

But by the latter part of the 18th century, various substantive developments led to modifications in the study of matter. For instance, previously unknown substances (and types of substances) were discovered. A distinction between two types of alkalis (sodium and potassium) was also recognized. And as a result of these developments and discoveries, the study of material substances became more sophisticated. In particular, researchers began to realize that a classification of substances on the basis of their obvious sensory properties was inadequate. The chemist, Guyton de Morveau, for instance, remarked that:

> No doubt it was still possible to remember the improper names of some thirty salts and to retain them in the memory by re-reading them and hearing them; but today chemistry is familiar with eighteen acids ... it has newly discovered two earths and several semi-metals; if we are to examine with care the action of so many substances on each other ... it becomes essential to adopt a nomenclature to indicate the result without confusion. (Quoted in Shapere, 1977, p.289)

The inadequacies of the perfectionist approach in the naming of material substances, the discovery of various hitherto unknown substances, and the recognition of (deep) chemical similarities between substances (as opposed to sensory ones such as colour) soon led to the belief that a better knowledge of substances can be gained through the study of their constituents. In short, there was a gradual change from the *perfectionist* approach to the *compositionalist* one. The *compositionalist* approach is based on the idea that "material substances can be understood in terms of their constituents, the arrangement of those constituents, and that ... such understanding is what we should aim at in our laboratory dealing with material substances". (Shapere, 1984, p.327)

The change from the perfectionist aims to compositionalist ones exhibit *internalization* in the sense that the shift is a result of developments and discoveries in substantive claims about the nature of material substances.[7] Just as the substantive discoveries in chemistry supplied the basis for the adoption of the compositionalist aims, so did the general Greek philosophical tradition supply the basis for the perfectionist approach.

Shapere adds the further point that the transition from the perfectionist approach to the compositionalist one was very gradual. Various episodes in the history of chemistry contributed to the overthrow of the perfectionist approach. For instance, Stahl's theory of acidity, which had very close ties with the alchemic idea of phlogiston and negative

[7]Indeed the rejected perfectionist approach had itself developed within the general context of the Greek philosophical tradition. Early Greek philosophers had recognized and distinguished three states of matter, viz., the solid, liquid, and the gaseous. Corresponding to these three states of matter were the three ***elements*** earth, water, and air. And with fire as a fourth element, the existence of all forms of terrestrial matter was explained by the Greeks. Thus Shapere insists that: "as many alchemical theorists were quick to see, the idea of "perfecting" ... metals was readily conceivable within this "framework", and specific processes for bringing about such perfection could be given a theoretical basis in its terms." (Shapere, 1977, p.286)

weight, contributed to the development of the compositionalist approach. This is because Stahl's account of acidity and combustion laid the grounds for Lavoisier's theory of chemistry. It was in his rejection of Stahl's theory that Lavoisier made very significant contributions to the understanding of substances in terms of their constituents (or compositions).

On Shapere's view therefore, although the change involved a fundamental shift in the aims of the study of material substances, "a continuity can be discerned between the earlier and the new nomenclature, [and] it is misleading to say *simply* that what was involved was *either* a new way of talking about "the same things", or a "radically new" way of talking which completely reconceived what the things are" (Shapere, 1977, p.290). This is why questions such as: *how could compositionalist theories, procedures, or explanations, have been accepted if the alchemic goals were truly accepted?*, do not arise in Shapere's explanation of the change. The transition, according to Shapere's account, was too gradual for questions such as this to be of any significance. It is only when we look at the change from its two "ends" (i.e the fully blown perfectionist approach and the fully blown compositionalist approach) that the change appears so radical. If we are interested in giving a rational explanation of the change, Shapere would insist that we concentrate on the gradual developmental process of the change; and not on the two end products.

Nonetheless, this process -- no matter how gradual -- *does* eventually produce a *radical change*. And radical change not just in substantive claims, but also in the criteria of theory appraisal and evaluation:

> In the light of this process of internalization, it is easy to see why and how scientific change is so pervasive. In the course of inquiry, domains come more and more to be formulated in the light of background beliefs ... . But the background beliefs which lead to such changes in domain structure and conception also lead to alterations ... in other parts of the fabric of science. ... The problems associated with particular domains become altered, as do the lines between recognized "scientific" problems and questions that are classed as "non-scientific". ... New background information, or old information formerly considered irrelevant, is found to be relevant to a particular domain. Old methods are rejected or reinterpreted, new ones introduced; new standards of possibility and acceptability arise. (Shapere, 1987b, p.7)

We need to be very precise about what the Principle of Scientific Internalization entails. Shapere's view of *internalization* is not restricted to the integration of observation and methodology:

> ... the process of internalization [is not] limited to the integration of methodology [and observation] into the rest of science. What counts as a reason, too, has become a function of scientific belief ... the distinction between the scientifically relevant and the scientifically irrelevant is [also] one that has had to evolve ... . The problems that count as scientific, too, alter with the development of science. Questions once considered scientifically legitimate - how to bring matter to perfection; the final causes of things - have been abandoned .... Questions earlier dismissed as being improper subjects for scientific investigation - the origin of life, the origin of stars, the chemical composition of stars, ... have become scientifically tractable problems. They have, in other words, become internalized into the scientific process. Even such lofty concepts as "truth" and "existence" have been refashioned, or are being refashioned, in the light of what we have come to believe in science. ... The ... achievement [of internalization thus] serves an important function in the knowledge-seeking enterprise. By bringing methodology, reasoning-patterns, goals and so forth into intimate connection with more overtly substantive claims, it makes it possible to subject all aspects of science to test. But the need to bring all aspects of scientific inquiry, even the methods, goals, reasoning-patterns, and standards of explanation, to test, is precisely what I describe as the second lesson of modern science. That is why I have called that lesson the Principle of Internalization. (Shapere, 1987a, pp.5-6)

On Shapere's view, therefore, it is because scientific inquiry is always conducted on the basis of well-founded claims-- i.e established laws, theories, facts and assertions which have acquired the status of *background knowledge*-- that *all* aspects of science are (in principle as well as in practice) subject to radical change. Background knowledge provides guidance in deciding what is relevant to investigation, on how to carry out investigation in science, and on how to understand the results of scientific investigation. Just like Kuhn, Shapere maintains that the development of science is governed by some assumed web of substantive claims and beliefs about the world. This background of beliefs, because they are taken for granted, provide the framework within which all aspects of science are interpreted and evaluated. On the basis of new beliefs, theories, equations, and laws (or modifications to old ones), we arrive at new methods and rules of theory appraisal:

> The methods we employ lead us to new beliefs which in turn lead us to modify those very methods, sometimes replacing them with new ones. The result is a growing integration of method with belief. A cycle of mutual adjustment of beliefs and methods has thus become a characteristic feature of the scientific enterprise. (Shapere, 1987a, p.5)

3. TWO SENSES OF METHODOLOGY

A good deal of confusion has been wrought in recent philosophy of science, and especially in discussions of naturalized methodology, 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.[8] 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 judgement 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

[8]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.

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 further development of science. They perform a heuristic role 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.

As we have seen, the solar neutrino experiment illustrates how science is always conducted from within a background of theoretical and factual assumptions. Without this 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.

One could describe these heuristic roles of scientific assumptions as *methodological.* Indeed, this is one main sense in which Kuhn and his followers use the term. Kuhn and the Kuhnians did not regarded 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.) Methodology for them includes, highly substantive principles and, positive and negative heuristic principles which guide scientific research (and the construction of new theories).

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:

> ... the problems we face in our inquires about nature, and the methods with which we attempt to deal with these problems, co-evolve with our beliefs about nature ... The methods we employ lead us to new beliefs, which in turn lead us to modify those very methods, sometimes replacing them with new ones. *The result has been a growing integration of method with belief. A cycle of mutual adjustment of beliefs and methods has thus become a characteristic of the scientific enterprise; it is a process of internalization of methodology into the rest of science.* (Shapere, 1987b, p.5, my emphasis)

Shapere, in fact, also claims that:

> ... shifts of aims, problems, methods, and vocabulary are linked to substantive beliefs about the world; aims, methods, and so forth in science are as much subject to discovery and evolution as the facts and theories with which science deals. (Shapere, 1984, p. 214)

As already indicated, I agree with Shapere that the important role of background knowledge was largely overlooked by pre-Kuhnian philosophers. 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 questions can be asked: does the role of background knowledge in scientific change genuinely support the no-invariant-methodology thesis? Do the heuristic roles of substantive beliefs genuinely support the view that *all* methodological rules are subject to possible radical change? To provide an answer to these questions, we need to take a closer look at the nature and function of *background knowledge* in the development of science.

As Shapere's example of the solar neutrino experiment shows, only those assumptions which are taken for granted by scientists can operate as the (perhaps temporarily) "unquestioned truths" referred to as *background knowledge*.[9] But we need to distinguish between various types of these unquestioned assumptions.

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*:[10]

(a) Specific Theories.

(b) General Theories.

(c) Highly General Metaphysical Principles.

(d) 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

[9]Of course Shapere does not claim that these assumptions can never be questioned! Rather, as I will explain fully in the next section, his claim is that these assumptions are not questioned as long as there is *no specific reason to doubt them*. Once specific doubts have been levied against them, they cease to function as background knowledge.

[10]The distinction between the 'parts' of background knowledge modifies and builds on that developed by Worrall (1985). And, I am not claiming that these four "parts" of background knowledge exhaust the range of items within background knowledge! I concentrate on these four merely because they suffice for my analysis.

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.

Of course there are various sorts of differences in the kinds of specific theories (and of general theories) that operate as "background knowledge" in science. For instance, some specific theories (and some general theories) which form the background knowledge of a domain X will be theories which have been "imported" from related domains; while others will be theories from within the domain in question itself. For instance, the claim that the sun is made up of three layers-- the photosphere (i.e. the core), the chromosphere (i.e. the "sphere of colour"), and the corona (i.e. the outermost, gaseous layer)-- is a specific claim which functions as background knowledge within astrophysics. This entrenched claim is from within the domain of astrophysics itself. The equation of Einstein's theory of relativity ($E=mc^2$) is taken for granted not merely in kinematics, but in various aspects of modern science-- e.g in chemistry and astrophysics. In these sciences, the equation functions as a well entrenched aspect of background knowledge which is imported from kinematics.

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 (temporarily) 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 favour 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 still 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) 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 favour 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 favour 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.

Shapere's analysis of the solar neutrino experiment of course commits him to the view that radical discontinuity extends right down to the observational and empirical level of science. And he in fact explicitly rejects the "continuity at the empirical level" view:

> There can by now be no reasonable doubt of the pervasive role of presupposition, of interpretation, in science and scientific change. There are no brute facts which confront us and force our theory choices in certain obligatory directions, there is no "given" which does not include interpretation. (Shapere, 1987b, p.2)

But it seems to me that Shapere's view of radical empirical discontinuity rests on an ambiguity between a wider and a narrower sense of the term "fact". "Facts" are usually taken to be the bases for the testing of scientific theories. But 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.[11]

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. (Indeed, as I shall argue shortly, a methodological principle that is invariant in scientific change is associated

[11]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 lose usage of "theory" claims such as "the computer screen before me is green and black" would employ "theoretical assumptions" about myself, 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 are those which I regard as expressing scientific facts, and those in the second set express crude facts.

with the empirical aspect of scientific knowledge.)

Indeed there are some facts which are (strictly speaking) scientific facts, but which Shapere's radical discontinuity thesis do not touch. For instance, we regard Newton's theory of gravity as being tested by factual descriptions of planetary positions. But obviously, descriptions of planetary positions are not crude facts. The real crude facts are expressed in statements such as "a characteristic spot of light is spotted in the sky at locations uvw when the telescope is inclined at angle xyz." "Facts" such as those of planetary positions are clearly interpreted facts. But there is no doubt that because these sort of interpreted facts involve very low-level theoretical assumptions (unlike the claim "scientist observe the production of neutrinos in the sun"), they are also not subject to the sorts of radical changes Shapere envisages.

My general point then is this. There are 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 broad 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.[12] 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

[12]Shapere need not claim that as a matter of historical fact, there has been no stable methodological principle over the developments of science. Indeed, Shapere makes no such claim; and my point is quite different from the mere factual claim that some non-theoretical methodological principles have remained stable over the historical development of science. Rather my point is simply that we should resist the move from *substantive* to *seriously corrigible*. Even if we accept (just for the sake of argument) that all methodological rules are inextricably linked to background knowledge, we should not be misled into thinking that this is a sure mark of corrigibility.

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.

All the cases that Shapere point to as ones in which "methods" were radically altered can, at least as revealingly, (indeed, I believe, more revealingly), be analyzed as cases *in which substantive ideas were modified because of new evidence in accordance* *with fixed and underlying methodological principles*. Take the solar neutrino experiment. According to theory, the source of the heat and light of stars (such as the sun) is the conversion of its mass into energy according to the formula $E=mc^2$ of Einstein's theory of relativity. This conversion takes place at the core of stars. Theory also asserts that some of the total energy produced takes the form of neutrinos; and that any square centimetre of the earth exposed to the sun is bombarded by as many as 70 billion neutrinos per second. But because neutrinos are believed to lack electric charge, are either massless or almost without mass, and they (according to theory) hardly interact with any other particle, they should pass unimpeded between the sun's core, the earth and beyond.

This is an interesting and possibly testable prediction of theory. However, in order to test it in *practice*, we need some "auxiliary" theories that tell us what it takes to capture a neutrino. On the basis of these auxiliary theories, the experiment was set up to confirm (or disconfirm) this theory. If scientists succeed in trapping neutrinos, and the rate of capture is consistent with the predictions of theory, then theoretical claims about the source of stellar radiation would be predictively successful. Although the wide range of experimental techniques and heuristics of the experiments wouldn't have been conceivable without background knowledge, the role of background knowledge in the experiment satisfies the more fundamental principle that a theory is acceptable only if it is predictively and empirically successful.

Shapere can therefore be criticized for overlooking the point that associated with the observational and empirical aspects of science is a more foundational, more invariant, set of methodological standards.

4. METHODOLOGICAL RELATIVISM

In the previous section, I argued that a distinction can be made between a *narrow* and a *broad* sense of scientific methodology. I also argued that Shapere adopts the broader usage of methodology and that he has not successfully defended his version of the no-invariant-methodology thesis with respect to narrow methodology. For the traditionalist could still successfully maintain that methodology, narrowly conceived, has remained invariant. Furthermore, the traditionalist can plausibly maintain that all the changes that have undoubtedly occurred in broad methodology have all been constrained by the principles of narrow methodology. In this section, I go on the assess Shapere's attempt to overcome the problem of relativism. My claim will be that in so far as Shapere avoids relativism, it is because he implicitly relies on *some* invariants.

But first, we need a precise characterization of the relativist's position. Ian Jarvie characterizes the general relativist position as follows:

> Relativism is the position that all assessments are assessments relative to some standard or other, and standards derive from cultures. The attempt to assess without regard to cultural context and, particularly, the attempt to assess cognitive statements on some transcendental scale of truth, is futile. No assessment can escape the web of culture and hence all assessment is culturally relative. (Jarvie, 1983, p.44)

Although this characterization refers to *cultures* and *cultural contexts*, for our purposes in this thesis, we can substitute *theories or historical epoch* for *cultures*. Hence, the version of relativism we will be concerned with is that in which *no assessment can escape* *the web of theory (or history), and hence all assessment is relative to theory (or history).*

This version of relativism is a thesis about the cognitive status of justifications. It is not a thesis which merely aims to document the fact that there are substantive variations, disagreements and differences in what different theories (or research programmes) claim about the world. 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).

As we are particularly interested in the rational assessment of methodological rules and principle, let us describe this version of relativism as *methodological relativism* (MR for short). Hence MR will be the thesis that the rational adequacy of any methodological rule (or principle) depends only upon the standards of its corresponding theoretical unit (or research programme).

I shall argue that Shapere is in a dilemma. My argument will be that if there are no invariant characteristics of scientific reasoning, Shapere's view collapses into methodological relativism. However, if Shapere avoids relativism as he explicitly claims, he avoids it only because he is implicitly committed to some invariant characteristics of scientific reasoning. Moreover, I shall identify those aspects of Shapere's theory of scientific rationality which seem to be invariant. If Shapere would allow change in those aspects of his theory I shall identify, he would be a methodological relativist. I begin by analysing Shapere's theory of scientific rationality.

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:

> Insofar as science is able to proceed in the light of its best beliefs, its arguments and alterations are rational ... [and] the relativism into which Kuhn's view collapsed is ... escaped, even while all aspects of science are left open, in principle, to revision or rejection. (Shapere, 1984, p. xxv)

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 below, 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:

> Whatever else might be required of an explanation of a particular body of presumed information (domain), the 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)

But before "successful" theories can function as a rationale of development (i.e. as a basis for developing "new hypotheses, new problems, new methods, new standards, and even new goals for [science]", (Shapere, 1984, p.xxv), 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 sceptical 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) But surely the point is also that the experiment, strictly speaking, *disconfirmed* astrophysical theory. The prediction of theory is that about 70 billion neutrinos per second should bombard each square centimetre of the earth exposed to the sun. But the experiment was able to detect *just one neutrino about every three days*!

As was to be expected, various hypotheses have been put forward to account for the missing neutrinos. In 1969, for instance, Soviet scientists explained the deficit as due to particle metamorphosis. The claim of these scientists is that electron neutrinos could be transformed into muon neutrinos, and vice versa, before the electron neutrinos produced in the sun reach the earth. But, as the Homestake neutrino detector was devised to trap electron neutrinos, it merely succeeded in detecting those electron neutrinos that were not transformed into muon neutrinos. And in 1988, scientists in the Soviet Union completed another neutrino detector for the capture of muon neutrinos. The new detector is located at a place named "Neutrino Village" somewhere in the Caucasus mountain range of Georgia. But even if this new detector succeeds in trapping muon neutrinos, independent evidence would still be needed to support the theory that electron neutrinos can be transformed into muon neutrinos en route from the sun. One such independent support comes from Hans Bethe's calculations in 1986. These calculations support the claim that two-third of electron neutrinos produced in the sun's photosphere could be transformed into muon neutrinos within half-a-second (or less) of their production in the sun.

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 experience" 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 judgement 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 favour 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 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.

Shapere does emphasize the point that the use of the chain-of-reasoning approach to scientific reasoning, and the conditions of relevance, success, and freedom from doubt, were learnt from the actual practice of science itself. They are not *a priori* stipulations he foists upon science. This is why he claims to adopt an *empiricist* and *naturalist* view of scientific reasoning:

> [Methodology] does not exist and function on a level above and independent of the substantive content of scientific beliefs; it is integrally linked to that content, and its ... conclusions must rest on the results of the very science with which it is concerned. This view of the philosophy of science and its relations to science has certain affinities with what is called a "naturalistic" approach to the theory of knowledge, as advocated by Quine and others. [It agrees with Quine and others] that *an understanding of our knowledge-seeking and knowledge-acquiring processes, and of the validation of the results of those processes, must rest on the*

results of science itself .... (Shapere, 1987a, p.24, my emphasis)[13]

But there are two sorts of issues which needs to be carefully distinguished: those concerning the *source* and *origins* of methodological principles and standards, and, those concerning the *validity (justification)* and *warrant* of these rules. Someone who adopts a *traditionalist* approach to scientific rationality only needs to uphold an invariant attitude in issues of *validation.* A traditionalist could therefore claim that methodological principles and standards may be contingent in the sense that they are not products of innate introspection or *transcendental deductions* (hence, a traditionalist may be empiricist in issues of source or origin). But in issues of *validation and justification,* the traditionalist would insist that irrespective of how principles are arrived at, they need to be invariantly valid if relativism is to be avoided. One can be *empiricist* in questions concerning how we come to *acquire* methodological rules (i.e. the source of methodological rules), while being non-naturalist about the *validation and justification* of methodological strictures.

For example, a philosopher who studies the history of science may become aware of the fact that scientists have tended to accept theories that are predictively successful. The philosopher may then proceed to provide philosophical arguments which exhibit the rationality of this tendency by incorporating it into a principle he calls the predictivist rule of theory appraisal. In such a situation, the *source* of the rule (as far as this philosopher

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[13]The similarity of Shapere's view to Quine's is merely in the claim that the testing of methodologies must rest on the substantive content and results of science; hence, Shapere's naturalism is not *psychologistic*; i.e. it is not an attempt at the reduction of epistemology (or methodology) into psychology. This is why Shapere claims that "... psychological considerations play no role *as reasons* for raising problems, employing specific methods, or accepting or rejecting specific beliefs in actual science ...". (Shapere, 1987a, p.25)

is concerned) is history (and consequently the philosopher did not develop the principle on the basis of "pure reason"). But this does not imply that the principle is *contingently* valid. The validity and adequacy of the principle would depend upon the sort of argument the philosopher advances in support of the predictivist thesis, and not merely on the fact the principle was adopted in practice by historical figures.

Shapere explicitly commits himself to the naturalist view in both issues of source and validation. Concerning the source of methodological rules, he says:

> It is important to realize that [the] results [of this view] are contingent: there is no way in which their development could have been shown in advance, by *a priori* or transcendental arguments, to be a (or the) necessary outcome of inquiry. (Shapere, 1987a, p.15)

And concerning the *validation* of methodological standards he claims that "the *validation* of the results of those processes, must rest on the results of science itself". (Shapere, 1987a, p.24)

But it is the naturalistic approach to the *validation* of methodological standards that poses the difficulty for Shapere's view. For in his explication of how radical change can be rational, Shapere claims that change is rational *if and only if* we can identify an objective process of reasoning connections from old methods (and theories) to the new ones. (Of course, not any sort of reasoning connections will do; only those connections that are traceable in light of relevance considerations, considerations of successful and compelling doubt, etc, are admissible if change is to be rational). But can change in science (radical and non-radical change) occur rationally if we cannot identify a chain-of-connections between *old methods, new methods* and the *considerations* (or grounds) for choice? Can scientific change be rational if we cannot identify reasoning connections between background information and choices? Can the basic idea of science developing in terms of reasonable connections between beliefs ever be (rationally) abandoned in science? Can scientists ever (rationally) reject the basic idea of chain-of-reasoning connections? If one answers *any* of these questions in the negative, one would in fact be claiming that there are no logical connections between scientific choices and the basis of these choices. Hence, if Shapere makes any such claim, he would end up with an extreme form of relativism.

The problem then is that, if we concentrate on issues of *validation and justification* (rather than those of *source or origin*), Shapere avoids MR *if and only if the principles underlying the chain-of-reasoning connections approach (and those governing the conditions of freedom, relevance, and success) remain sacrosanct, inviolable or invariant characteristics of scientific reasoning.* For if we do not operate with a principle like: "accept only those methodological choices that are the product of chains-of-reasoning connections", (and, consequently, "accept only those theories and rules that are brought about by parameters such as 'relevance' and 'coherence'"), scientific change would lack any rationale or rational justification.

Shapere has been smuggling all along! He cannot deliver the judgement that the acceptance of a new theory or method is *objectively better* than its rival *unless* he implicitly takes these core characteristics of scientific method to be invariant. Consequently, he has not succeeded in fulfilling his self-assigned task of giving "... an account of the knowledge seeking and knowledge acquiring enterprise ... which, while not relying on any form of Inviolability thesis, will not also collapse into relativism ... ."

(Shapere, 1984, p. xxi)

5. THE WEAKNESS OF BOOTSTRAPPISM

In Shapere's view, a domain of investigation is a unified body of information which provides a basis for scientific research. This characterization of domains allows for situations in which rival theories belong to different domains because theories (even if the belong to different domains) can be regarded as rivals as long as they advance inconsistent (or competing) explanations of the same phenomenon. In this section, I will argue that Shapere's theory of rationality runs into serious problems when applied to situations in which competing theories belong to radically different domains of inquiry. I use one such example to bring out my point. Specifically, I will concentrate on the debate between evolutionist and creationist accounts of the origin of the world.[14]

Not all creationists refuse to make use of *evidence*, or deny that explanations which are to count as *good reasons* for choice must be *relevant* to the domain of inquiry in question. Nor do they reject the claim that *good reasons* must be successful, relevant, and free from specific and compelling doubts. In short, *scientific creationists* neither deny the use of background beliefs, nor deny that a chain-of-reasoning connection must always be traceable between choices. However, *scientific creationists* are not *scientific* in the

[14]There are various types of creationisms. But for present purposes, we can simply divide them into two broad types: *simple creationism* (e.g. Jehovah witnesses) and *scientific creationism*. The main difference is that scientific creationists are willing to examine the claims and arguments of evolutionists in the light of scientific evidence (e.g. fossil records); hence they do not simply dismiss scientific theories of evolution. Indeed some scientific creationists claim to be evolutionists as well! As long as we bear in mind that no single scientific creationist would accept all the claims I attribute to scientific creationism, these nuances need not detain us here. This is legitimate as my point can equally well be made with any hypothetical situation in which all the conditions laid down by Shapere are met, but in which rational choice cannot be delivered between competing theories which belong to different domains.

sense that they adhere to the basic methods of the natural scientists. Rather they explicitly denounce the methods used throughout the natural sciences as applicable for use in *creation science:*

> Far from simply debating the scientific evidence, it appears that creationist and evolutionist groups structure their perceptions of reality in very different ways, based on very different cognitive principles and on different assumptions about the rules of knowing. (Eve, 1991, p.6)

The problem is that *scientific creationists* do not want the *validity or justification* of their theories to be evaluated in light of methods and principles which bear any similarity to those of the natural sciences. Their own preferred *method* is described as the method of "Common Sense Realism". Common sense realism is said to be an "epistemological philosophy ... [which claims] that our ordinary common sense perceptions do provide a direct and reliable guide to how the world works." (Eve, 1991, p.14) This *method* makes use of the "notion of divine or other supernatural involvement in the origins of the universe or humanity" (Eve, 1991, p.3); and the *method* also validates the drawing of conclusions "on the basis of inferences from the *internal evidence* in the Bible". (Eve, 1991, p.15, my emphasis)

The disagreement between *scientific creationists* and Darwinists cannot therefore be settled by identifying chains-of-reasoning connection, nor by explaining change in terms of beliefs which are successful, relevant to each domain of inquiry, and which are free from specific doubts. This is because the disagreement is a much more fundamental one. It is a disagreement about what is to be regarded as the background beliefs, facts, evidence, and, the rules of reasoning for use in inquiry about the origin of life. This is why creationists insist that: "For scientific creationists, the *correct* interpretation of *scientific* evidence is actually consistent with Genesis." (Eve, 1991, p.50, my emphasis)

Given these differences, we can now ask the question: Isn't Shapere's bootstrappism committed to the (highly contentious) conclusion that Darwinism and Creationism *are equally valid views* of the origin of the world? For if Creationism is fully able to satisfy the requirements of its internal set of criteria, we can not rationally adjudicate the dispute between these rival theories. Unless there are some characteristics of scientific methodology which are invariant, and the validity of such methods can be established irrespective of a specific domain of inquiry, we would not be able to deliver the judgement that Darwinism gives an objectively better account of the origin of the world. We would therefore end with **MR** whenever we are concerned with rival theories such as Creationism and Darwinism. Identifying reasons of the sort mentioned by Shapere cannot dictate the choice of theory. For each theory satisfies its own included criteria of merit and success.

Identifying a chain-of-reasoning connection between choices and grounds for choice would also not settle the dispute. For both Creationists and Darwinists would give reasons in the straight-forward sense of arguments which support conclusions for their respective choices. Each would point to explanations which are *successful, relevant to their respective domains, and free from specific and compelling doubts* (again, all according to their respective internal measures).

Of course Shapere, (or any naturalist), could reply that a methodology need not deliver the verdict that Darwinism is better that Creationism. He could claim that a theory of scientific methodology and rationality need not deliver the judgement that Darwinism is objectively better than Creationism. But this would lead to an extreme form of relativism! Scientific Creationism and Darwinism are conflicting and competing accounts of the origin of species. If Shapere were to claim that a methodology need not deliver the judgement that Darwinism is objectively better than Creationism, we would be left with no methodological guidance on which of these two rival theories to accept in our explanations of the origin of species. The naturalist would in effect be committed to a position similar to that of Feyerabend's in which *anything goes!*

If it were possible to identify some further essential and invariant characteristics of scientific explanations (e.g. genuine predictive success) such that these characteristics could be applied in situations such as this, then it would be possible to claim that the theories, beliefs, and explanations of the creationists are not *scientifically cogent.* But Shapere's explicit claims prevents such a move. On Shapere's view, there are no characteristics of scientific rationality and methodology over and above the tracing of a line of descent from theory to theory-- or from method to method--, according to the *internal* criteria of the domain in question. The employment of the background beliefs of a domain, (as long as they are subject to the constraints of relevance, freedom from specific doubt, etc), is all that counts:

> ... what better standards or criteria could we employ ... than those beliefs ... that have proved successful and have not been confronted with specific doubts? ... *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, my emphasis)

But as we have just seen, this account of scientific reasoning is not strong enough to dictate (rational) preference *whenever* competing theories belong to genuinely radically different domains of inquiry.

5. CONCLUDING REMARKS

1. A traditionalist would easily explain *all* the changes in substantive method that Shapere points to as being dictated by some more fundamental and invariant core of methodological principles. Hence, *Shapere has not truly succeeded in naturalizing method when we consider narrow methodology.*

2. Shapere has not succeeded in showing that the no-invariant-methodology thesis (or the no-inviolability thesis, as he prefers to call it), can be accepted without relativism. On the contrary, he seems to have succeeded in showing that there are some invariant and inviolable attributes of science. This is because Shapere's view is implicitly committed to an invariant principle of scientific reasoning. Namely, that which underlies the chain-of-reasoning approach to scientific reasoning. Moreover, Shapere's naturalized account of rational change is committed to some sacrosanct components (i.e. the conditions of "relevance", "success", and "freedom from specific and compelling doubts"). The only alternative to making these aspects of Shapere's view invariant is an extreme form of relativism.

3. Irrespective of whether these specified aspects of Shapere's view are invariant or not, any view of scientific methodology which relies only on these characteristics (without tacitly assuming any invariant characteristic) would be exceedingly weak. Such a view

would not be able to dictate choice in situations where competing theories belong to domains of inquiry that are genuinely radically different. In such situations, a very high degree of irrationality and relativism is inevitable.

CHAPTER 3

Laudan's Normative Naturalism

1. INTRODUCTION

Larry Laudan has developed in recent publications another version of the view that change in science extends beyond the factual-theoretical level to the level of accepted methods and rules of appraisal. Like Shapere, Laudan characterizes his view as a version of "naturalism" in epistemology. And like Shapere, he explicitly holds that rules of scientific theory appraisal may change without surrendering to relativism. My aims in this chapter will be: (i) to outline carefully and in detail Laudan's account of scientific change and to explain why he describes it as a version of "naturalism"; and (ii) to argue that Laudan's account of naturalism ultimately fails to give an adequate account of scientific rationality.

2. THE RETICULATED MODEL OF SCIENTIFIC RATIONALITY

Laudan makes a distinction between three interrelated levels of scientific commitment, viz.; the factual-theoretical level, the methodological level, and the axiological level (i.e. the level of the ends, aims and goals of science). Theories of scientific change are characterized as "hierarchical", "holist", or "reticulated" on the basis of how these three levels of commitment function in scientific change. Laudan's preferred model is the reticulated model. In this section, I will examine Laudan's arguments in favour of the reticulated model, and in the next section, I delineate the connections between *reticulation* and *normative naturalism.*

According to Laudan, the hierarchical model arranges these three levels of scientific commitment in a hierarchy which moves from the factual-theoretical level, through that of methodology to that of axiology. The assumption is that methodology governs factual issues and that axiology in turn governs methodology. Rationality is achieved by settling factual disputes by reference to methodological rules and principles. (*Factual issues* are, of course, taken in the broad sense to include theoretical claims which form the basic ontology of scientific theories.) This is because when there is factual disagreement, (or disagreement on which theory is the best), agreement is achieved by reference to shared methodological commitments. Disputes at the methodological level, if they arise, are resolved by reference to the axiological level where shared goals and aims adjudicate methodological disagreement. This is possible because methodological rules are regarded as instruments or techniques for realizing cognitive goals or ends.[1] Philosophers like Carnap, Hempel, and Popper are Laudan's chief examples of hierarchical modellers.

The hierarchical modeller (according to Laudan) further assumes either that axiological disagreements do not exist, (because scientists share the same cognitive goals), or that axiological disagreements are irresolvable, (when and if they do arise). Laudan's view is not merely that hierarchical modellers *overlooked* or played down the fact that there are significant axiological disagreement. His full claims are: (1) that because hierarchical modellers genuinely believed that scientists share more or less the same set of cognitive goals, there are little or no axiological disagreements to resolve. However,

[1]A detailed exposition of Laudan's instrumental construal of methodological rules is given in section 3 above.

(2) if such disagreements were to arise, proponents of the hierarchical model also believed that such disagreements are irresolvable:

> Influential voices within the philosophy of science have argued that differences in goals, particularly cognitive goals, are simply not open to rational resolution. Both Karl Popper and Hans Reichenbach, for instance, have said that the adoption (or change) of basic cognitive goal is a subjective and emotive matter which cannot be rationally negotiated. ... the hierarchical model of rationality ... leaves basic questions of values perched precariously at the top of the justificatory ladder. (Laudan, 1984, p.47)

Laudan insists that the hierarchical model breaks down when we consider the issue of the resolution of axiological disagreements. Because contrary to the claims of the hierarchical modeller, not only do axiological disagreements often arise in science, there are mechanisms for adjudicating such disputes-- axiological disagreements are resolvable:

> The history of science is rife with controversies between, for instance, realists and instrumentalists, reductionists and anti-reductionists, advocates and critics of simplicity, proponents of teleology and advocates of purely efficient causality. At the bottom, all these debates have turned on divergent views about the attributes our theories should possess (and thus about the aim of scientific theorizing). The existence of such controversies, along with the fact that they often eventually issue in consensus, exposes the core weakness in the hierarchical model, for that model gives us no reason to anticipate the emergence of consensus in such circumstances, nor can it explain that consensus once it does materialize. (Laudan, 1984, p. 42)

Laudan suggests a mechanism for the rational adjudication of axiological disagreenent by placing the following constraints on scientific goals: (i) aims and goals must be internally consistent (i.e. they must be free of contradictions); (ii) aims must not be *'utopian or unrealizable"*; and (iii), aims must accord with *"the values implicit in the* *communal practices and judgements we endorse".* (Laudan, 1984, p. 50)

There are various problems with Laudan's characterization of the hierarchical model. One concerns its historical accuracy. Did any of those philosophers mentioned by Laudan truly subscribe to the hierarchical model described by Laudan? As Worrall rightly observed, (Worrall, 1988), philosophers like Popper, Carnap and Hempel whom Laudan cite as proponents of the hierarchical model did not hold that there are any genuine methodological disagreements which can be resolved by appeal to the aims and goals of science. For these philosophers, if two or more scientists (or philosophers) disagree on which method to adopt, or if they disagree on whether a scientific inference is valid, one of them must be wrong. (Of course both *might* be wrong.) *These philosophers never assumed, as Laudan claims, that methodology is governed by axiology.* The only type of lower-level disagreement they regarded as resolvable by a higher-level agreement was that of factual-theoretical disputes.

Laudan also insists that the hierarchical model lacks a mechanism for adjudicating axiological disputes. He proposes one such mechanism of his own on top of the internal consistency requirement, (which he concedes the hierarchical modeller has), he proposes "that one may argue against a goal on the grounds: (1) that it is utopian or unrealizable; or, (2) that it fails to accord with the values implicit in the communal practices and judgements we endorse." (Laudan, 1984, p. 50)

There are various problems with these requirements of Laudan's. First, it is simply untrue that hierarchical modellers did not adopt one version or the other of the *utopian or unrealizability* criterion. It can easily be shown that this constraint has long being used by philosophers of science. For instance, the aim of science during the time of Francis

Bacon and Rene Descartes was that of establishing and accumulating indubitable truths.[2] The *accumulation of indubitable truths* view of science was however criticized by various philosophers on the grounds that: (1) the claims of science cannot be conclusively established, hence they cannot be absolutely certain; and, (2) the earlier claims of science are not always preserved in the current developments of science, nor will they all be preserved in the future developments of science (Watkins, 1987). But these two criticisms amount to saying that the Bacon-Descartes ideal of science be rejected because it is un-realizable or too utopian! This was why philosophers like C.S. Peirce rejected the Bacon-Descartes ideal of science and adopted the approach-to-truth view of scientific progress. Hans Reichenbach also rejected the Bacon-Descartes ideal and opted for an inductivist view of science.

Moreover, Popper-- one of the hierarchical modellers who, according to Laudan lacks a mechanism for adjudicating axiological disputes-- rejected the inductivist aims and goals of science and opted for a deductivist view on the ground that the truth of our scientific theories can never be demonstrated. But this also amounts to the claim that the inductivist view of science is utopian and unrealizable! The point then is that Laudan's claim that philosophers like Popper overlooked the virtues of adopting goals that are non-utopian or realizability is untrue.[3]

[2]In his "A New View of Scientific Rationality" [1987], John Watkins describes this as the "Bacon-Descartes Ideal": "Francis Bacon and Rene Descartes ... believed that human understanding, properly regulated, can get to the very bottom of things, unlock Nature's deepest secrets, grasp her ultimate essences. And they both believed that the knowledge to be acquired at this ultimate level could be certain or infallible" (p.64).

[3]I am not suggesting that Popper's anti-inductivism is acceptable! I merely use this to illustrate the point that there is nothing innovative about Laudan's non-utopian or unrealizability criterion. Popper and various philosophers adopted some version or the other of this criterion. Furthermore, as I will argue shortly, this criterion in fact does not have the sort of import Laudan believes it to have.

But when Laudan criticizes an aim as utopian ("that is, we do not have the foggiest notion how to take any actions or adopt any strategies which would be apt to bring about the realization of the goal state in question", 1984, p.51), is he criticizing the aims scientists *really* have and exhibit in their work (whatever they may themselves think), or is he criticizing the aims they explicitly endorse?

Popper's claim, for example, is that whatever they may have thought and said, scientists could not *really* have been aiming at inductively proved general truths. In other words, Popper's claim is *not* that scientist 1 might have applied an inductivist methodology (because she had aim 1 -- inductively proved truths) while scientist 2 applied a falsificationist methodology, and scientist 2 then argued for his own aims by showing the unrealizability of aim 1. Popper's claim is that because the aim is utopian, scientist 1 never in fact applied inductivist methodology (because she couldn't, there is no such thing as an inductive proof on Popper's view) whatever scientist 1 might explicitly have believed.

Laudan, however, creates some confusion in his own specific criticism of utopian aims. He, for instance, distinguishes "semantic utopianism" from "epistemic utopianism". At first, Laudan's characterization of semantic utopianism seems to be about explicit aims:

> Many scientists espouse values or goals that, under critical challenge, they cannot characterize in a succinct and cogent way. They may be imprecise, ambiguous, or both. (Laudan, 1984, p.52)

And concerning epistemic utopianism, he claims that:

> It sometimes happens that an agent can give a perfectly clear definition of his goal

> state ... but that nonetheless its advocates cannot specify (and seem to be working with no implicit form of) a criterion for determining when the value is present or satisfied and when it is not. (Laudan, 1984, p.53)

But in criticizing aims which are semantically utopian, Laudan claims that:

> It should be clear why the charge of semantic utopianism ... is a serious criticism of a goal, cognitive or otherwise. If someone purports to subscribe to an aim, but can neither describe it in the abstract nor identify it in concrete example, *there is no objective way to ascertain when that aim has been realized and when it has not. ... (Indeed, it is difficult to see how radically ill-defined goals could play a genuine role in any theory of action, whether rational or irrational, objective or subjective.)* (Laudan, 1984, p.52, my emphasis)

But surely this criticism applies only to epistemic utopianism. Indeed, I would argue that semantic utopianism is not utopianism at all. An aim cannot be unrealizable simply because scientists cannot give any precise characterization of that aim. An aim should be characterized as unrealizable only if no set of actions can result in the achievement of such an aim. The problem of unrealizabilty is not that of whether the aim in question can be "characterize[d] in a cogent and succinct way". Rather, the problem is that no rational procedure can bring to fruition the stated objectives of such goals. If we concentrate on unrealizability in this epistemic sense, (and ignore the ambiguity semantic unrealizability creates), surely philosophers like Popper did not overlook the import of unrealizability criticisms (as Laudan would have us believe).

Laudan is of course fully aware of the implicit/explicit distinction. But unlike Popper who makes use of this distinction in criticising utopian aims, Laudan turns this distinction into a criticism in its own right by demanding that a scientist's explicit aims be consistent with her implicit aims:

> Often a scientist will find himself explicitly advocating certain cognitive aims, yet seemingly running counter to those aims in terms of the actual theory choices he makes in his daily work. Still worse, ... it sometimes happens that the dominant goals o[f] an entire community of scientists, as voiced in the explicit accounts they give of these matters, are discovered to be at odds with the goals that actually seem to inform that community's choices and actions as scientists. Whenever a case can be made that a group of scientists is not practising what it preaches, there are prima facie grounds for a change of either explicit or implicit values. The change may come, of course, in either area, or in both. (Laudan, 1984, p.55)

Laudan is of course correct to observe that a scientist (or a community of scientists) can be criticized if their implicit and explicit aims are at odds. The problem however is that, unlike Popper, Laudan is very unclear on the question of whether a scientist could implicitly employ an unrealizable aim. If a science is progressive and rational, Popper would insist that those scientists who propound the progressive theories within that science couldn't *really* have adopted an aim which is unrealizable.

There is a further objection to Laudan's characterization of the hierarchical model. Contrary to the impression Laudan gives, philosophers and scientists with very different accounts of the aims and goals of science generally agree on which theory is currently the best in a particular domain of science. We have both instrumentalists and realists of various sorts agreeing that Einstein's theory is better, in many respects, than Newton's; that Darwin's theory of evolution is currently the best evolutionary theory around. This seems to suggest that we can resolve theoretical and methodological disagreements (if they arise) even when there is little or no (explicit) axiological consensus. If this is so, the lack of a mechanism for the resolution of axiological disputes may not be so problematic.

Indeed the objection can be put more strongly: if rational consensus at the factual-theoretical and methodological levels can be achieved when there is sharp axiological dispute, then axiology plays little (if any) role in the resolution of theoretical and methodological disputes. And this is surely as it should be. Axiological disputes are more philosophical. And it is a commonly accepted feature of philosophical problems that they can hardly ever be definitely resolved. So why should we give any special weight to the fact that a model of scientific change lacks a mechanism for the *resolution* of axiological disputes?

Having rejected the hierarchical model, Laudan goes on to reject the holist alternative of Kuhn and Feyerabend. According to Laudan's characterization of the holist's position, there are disagreements at the level of axiology just as there are at the level of methodology and theory. The holist further maintains that radical change occurs at all levels of scientific commitment. The change occurs, however, simultaneously at all the three levels; it entails concurrent changes in factual-theoretical commitments, methodological appraisal principles, and in the aims and goals of science. A scientist never gives up a theory for another independently of changes in methodology and axiology. Change can only occur if she accepts one triad in favour of another.

Indeed the holist accommodates the idea of widespread disagreement and change in science. But s/he does so at the expense of rationality. For in the explanation of change, the holist leaves no arbiter to judge the transition between two successive sets of commitments (i.e. research traditions). Since the decision to abandon a set of three levels of commitment is at the same time the decision to accept a different triad, the holist model merely accommodates the idea of widespread disagreement; it cannot explain how scientific disagreements are rationally resolved. The holist account, therefore, collapses into relativism.[4]

Feyerabend, for instance, who is a holist promotes *epistemological anarchism.* The *epistemological anarchist* maintains that all knowledge claims are epistemologically on a par; and that because they are on a par, one can defend and "correctly" uphold any theory of knowledge. Kuhnians, on the other hand, claim that scientific paradigm-choice cannot be adequately justified because all previous rules of evaluation and justification are given up in paradigm change. Hence, for Kuhnians, irrational or non-rational factors play important roles in scientific paradigm acceptance. This is precisely why Kuhn describes the acceptance of new paradigms as "conversion experiences".

Laudan puts his criticism of the holist model as follows:

> ... Kuhn can readily explain why many scientific debates are protracted and inconclusive affairs. If both sides are indeed "talking past one another", if they are judging their theories against different yardsticks, then it is no surprise that they continue to disagree. ... Kuhn's model correctly predicts that dissensus should be a common feature of scientific life. What [the holist model] cannot explain ... is how - short of sheer exhaustion or political manipulation - scientific disagreements are ever brought to closure. If rival scientists cannot understand one another's point of view, if they have fundamentally different expectations about what counts as a "good" scientific theory, it seems utterly mysterious that those same scientists ever (let alone often) reach a point where they eventually agree about which paradigm is acceptable. (Laudan, 1984, pp. 16-17)

It is against this background that Laudan proposes his *reticulated model.* This model (according to Laudan) incorporates the virtues of both the hierarchical and holist

[4]Laudan discusses this and related criticisms of the holist view in his *Science and Values,* 1984, p.68-87.

models; but it overcomes their defects. Just like the holist model, it allows for widespread disagreement at all levels of scientific commitment, but unlike the holist model, it rejects the simultaneity of change. Like the hierarchical model, the reticulated model claims that methodological rules can justify theory-choice, and that axiology constrains methodology. But it rejects the one-way justificatory process of the hierarchical model. Rather, justification in the reticulated model is "unitraditional"; that is, accepted theories may judge changes in aims and methodology just as methods could bring about changes in factual-theoretical claims and in the aims and goals of science. Change in Laudan's "unitraditional change" is:

> ... a complex process of mutual justification ... among all three levels of scientific commitment. Justification flows upward as well as downward in the hierarchy linking aims, methods and factual claims. No longer should we regard any one of these levels as privileged or primary or more fundamental than others. Axiology, methodology and factual claims are inevitably intertwined in relations of mutual dependency. (Laudan, 1984, pp. 62-63)

To be more precise, where the holist envisages a revolutionary change in world-view from a set$_1$ of theory-methodology-axiology $\{T_1 \& M_1 \& A_1\}$ to another set$_2$ $\{T_2 \& M_2 \& A_2\}$, the reticulated modeller claims to avoid relativism by allowing for the modification of only one element at once. Hence, the change from:

$\{T_1 \& M_1 \& A_1\}$ may become a change to

$\{T_2 \& M_1 \& A_1\}$ or to

$\{T_1 \& M_2 \& A_1\}$ or to

$\{T_1 \& M_1 \& A_2\}$, ...

The holist model falls into relativism because it leaves no arbiter to effect the change from one research tradition to another, but Laudan claims that relativism can be avoided if only one element changes at once. Those levels of commitment that are left unchanged thereby provide the necessary arbiter for the changing element. But those elements that are left unchanged are only temporarily so; they may also change at a latter date.

The distinguishing features of Laudan's reticulated model can, therefore, be summarized as follows:

(1) No aspect of scientific commitment need remain fixed over time.

(2) Change in science is a piecemeal - not simultaneous - process among the three levels of commitment.

(3) Justification in science is a relation of mutual dependency; methods may justify theories or *vice versa*; axiology may justify methods or *vice versa*; and axiology may justify theory or *vice versa*.

Before delineating the connection between reticulation and naturalism, it is important to point out that in spite of the concentration of Laudan's model on *cognitive values*, or *cognitive ends*, or *cognitive goals* (as he interchangeably calls them), Laudan fails to give any adequate explanation of what these so-called values are. Laudan does give short examples of these values; for instance, he refers to them as:

> ... such familiar cognitive goals as truth, simplicity and predictive fertility ... . (Laudan, 1984, p. 35)

and, in connection with scientific realism, he also says:

> At the core, realism is a normative doctrine about what the aims or values of science ought to be. Specifically, the realist maintains that the goal of science is to find ever truer theories about the natural world. (Laudan, 1984, p.106)

But a footnote (in Laudan's *Science and Values*) in which he distinguishes cognitive values from non-cognitive values gives the pretence away. In that footnote, we have Laudan's most precise characterization of cognitive values. Unfortunately, the definition of cognitive values he gives in that footnote also fits a description of methodological standards:

> The question of precisely how one distinguishes cognitive values or aims from noncognitive ones is quite complex. For purposes of my analysis here, we can adopt this rough-and-ready characterization: *an attribute will count as a cognitive value or aim if that attribute represents a property of theories which we deem to be constitutive of good science.* (Laudan, 1984, pp. xi-xii, fn.2, my emphasis)

The problem is that Laudan's *rough-and-ready characterization* turns all methodological rules, principles and standards into cognitive values! If I advocate the falsifiability criterion as a methodological rule, or the principle that good theories must not be *ad hoc*, (or any methodological rule whatsoever), I must of necessity regard the quality stipulated by that methodological rule as a virtue which good scientific theories must exhibit or comply with. But by Laudan's own *rough-and-ready characterization*, such virtues are cognitive values! Hence, Laudan's *rough-and-ready characterization* does not adequately distinguish cognitive ends from methodological standards.

But if methodological rules are indistinguishable from cognitive values, the tale of the three levels of scientific commitment becomes implausible. It would seem more legitimate to postulate two levels of scientific commitment (the factual-theoretical level and the level of principles of appraisal) and not three levels as Laudan maintains.

3. FROM RETICULATION TO NORMATIVE NATURALISM

Laudan's normative naturalism is a view about the status and justification of methodology in philosophy of science; it is a meta-methodology. Meta-methodology, according to Laudan, is made up of two interdependent aspects: a theory of methodology, and a theory of axiology. The theory of methodology is concerned with the justification of methodological rules and standards as more or less likely to lead to the achievement of given cognitive ends, while axiology is concerned with the appraisal of cognitive ends themselves.

A methodology, for Laudan, is a set of rules. Examples of methodological rules, according to Laudan, are:

(1) Propound only falsifiable theories.
(2) Avoid ad hoc modifications.
(3) Prefer theories which make successful predictions over theories which merely explain what is already known.
(4) When experimenting on human subjects, use double-blinded experimental techniques; etc. (Laudan, 1987a, p.23)

Although these rules are stated in the form of commands, or categorical imperatives, Laudan claims that since they are *means* for achieving specific *cognitive ends,* they are really hypothetical imperatives:

> ... methodological rules, when freed from the elliptical form in which they are often formulated, take the form of hypothetical imperatives whose antecedent is a statement about aims and goals, and whose consequent is the elliptical expression of the mandated action. (Laudan, 1987a, p. 24)

Hence, according to Laudan, the Popperian rule: "Avoid ad hoc modifications", is more properly formulated as the rule:

> If one wants to develop theories which are very risky, then one ought to avoid ad hoc hypotheses. (Laudan, 1987a, p.24)

Laudan's naturalism is a consequence of his hypothetical interpretation of methodological rules. Since Laudan claims that methodological rules are best construed as hypothetical imperatives of the means/ends type, he infers that they are contingent claims about optimal ways to realize our ends, and that whichever method is the optimal way to realize our ends depends on the way the world is (and, what we believe is the optimal way itself depends on what we believe about the constitution of the world):

> Whether our methods, conceived as means, promote our cognitive aims, conceived as ends, is largely a contingent question. What strategy of inquiry will be successful depends entirely on what the world is like, and what we as prospective knowers are like. One cannot settle a priori whether certain methods of investigation will be successful instruments for exploring the world, since whether a certain method will be successful depends on what the world is like ... I do hold that the theory of methodology can be and should be as empirical as the natural sciences whose results it draws on. (That is precisely what I mean by a "reticulated" view of scientific rationality.) (Laudan, 1987b, p. 231)

According to Laudan, rational behaviour has to do with establishing the efficacy of action in relation to some cognitive aim. Establishing an action as the most effective way of bringing to realization a cognitive end, however, is an empirical affair. It is empirical in the sense that claiming that "*Y* is an effective way for realizing *Z*" is a conditional statement which asserts a contingent relationship between two "observable properties"; 'doing *Y*' and 'realizing *Z*'. Whether *Y* will indeed be a successful instrument for establishing *Z* depends on what the world is like. This is why Laudan claims that meta-methodology involves contingent linkages between cognitive ends and means; it is instrumental and empirical (natural):

> Crediting or discrediting a methodological rule requires us to ask ourselves whether the universe we inhabit is one in which our cognitive ends can in fact be furthered by following this rule rather than that. Such questions cannot be answered a priori; they are empirical matters. It follows that scientific methodology is itself an empirical discipline which cannot dispense with the very methods of inquiry whose validity it validates. Armchair methodology is as ill-founded as armchair chemistry or physics. (Laudan, 1984, pp. 39-40)

Unlike most versions of naturalism, Laudan's claim to uphold philosophy of science's traditional normative character; it is prescriptive. It advocates that one should adopt only those methods that best promote one's ends, and it prescribes that only those ends that are non-utopian are admissible in science. (Moreover, as we shall see, Laudan prescribes a sort of straight "meta-level inductive" principle that says that the rational thing to do is to assume that those methods that have been successful in achieving a certain cognitive goal in the past will continue to do so in the future.) This is why he claims that his naturalism "... can both discharge [methodology's] traditional normative role and nonetheless claim to be sensitive to empirical evidence." (Laudan, 1990c, p.44) We can identify the components of Laudan's normative naturalism as follows:

* an instrumental conception of rationality;
* a construal of methodological rules as hypothetical imperatives of the means/ends type;
* a naturalist (empirical) approach to the testing of methodologies in philosophy of science; and
* a prescriptivist (and inductivist) philosophy in which we are urged to continue adopting those means which have hitherto been successful in bringing to fruition their associated ends.

I shall consider three problems confronting Laudan's normative naturalism. I shall start by outlining two surface problems, the narrowness problem, and the credibility problem, and then move on to the major problem, which is the problem of relativism.

1. The narrowness problem: Gerald Doppelt [1990] criticizes Laudan of espousing a very narrow naturalism. Doppelt describes this as "a self-imposed limitation" of Laudan's naturalism, and he claims that:

> [Laudan's] naturalistic approach to methodological choice ignores the central role of logical and conceptual anomalies in determining which methodological standards scientists accept. (Doppelt, 1990, p.15)

But surely, Laudan's naturalism is not narrow in the sense that it concentrates on empirical considerations alone to the detriment of conceptual ones. In one of his books, *Progress and its Problems*, Laudan actually distinguishes conceptual problems from empirical problems, and he explicitly claims that conceptual problems are as important as empirical ones.

In fact, in one of his early expositions of normative naturalism, Laudan writes that:

> I am not claiming that the theory of methodology is a wholly empirical activity, any more than I would claim that theoretical physics was a wholly empirical activity. Both make extensive use of conceptual analysis as well as empirical results. But I do claim that methodology can be and should be as empirical as the natural sciences whose results it draws on. (Laudan, 1987b, p.231)

In my evaluation of Laudan's naturalism, I shall criticize the specific ways in which he makes use of empirical evidence. My criticisms should, however not be confused with that of Doppelt. Unlike Doppelt who argues that normative naturalism overlooks the important role of the conceptual, my examination of Laudan's naturalism shall concern the extent and usefulness of evidence of the means/ends type.

2. The credibility problem: Laudan's naturalist approach to methodological rules depends on the conviction that an empirical approach to a theory of methodology will reveal that there are hypothetical connections between scientific methods and cognitive ends. But no methodological means is unique to any particular aim. Instrumentalists and realists alike both accept rules like predictive success, they both reject ad hoc modification, etc. But if the same *means* can be associated with conflicting or contradictory *cognitive ends*, or conflicting (or contradictory) *means* be associated with the same *cognitive ends*, then an hypothetical construal of rules may be unable to yield a meta-methodological verdict on the adequacy of the principles of scientific theory appraisal.

This problem can be illustrated by considering two different interpretations of the predictivist rule of theory appraisal. Since Laudan maintains that all methodological rules are hypothetical imperatives of the means/ends type, we can have the following conflicting interpretations of the predictivist rule by the realist and an instrumentalist:

(a) <u>The Realist:</u> If you want theories which are true and which give genuine descriptions of reality, then, accept only theories that have successfully made surprising predictions.

(b) <u>The Instrumentalist:</u> If we want theories which are empirically adequate, which are codification of the directly observable; but which are neither true nor false descriptions of the world, then accept only theories that have successfully made surprising predictions.

Whatever evidence is regarded as an empirical justification of (a) by a realist will also serve as a corroboration of (b) for the instrumentalist. Hence, it is impossible to adjudicate between these two conflicting rules by empirical connections of the means/ends type as Laudan maintains.

The problem is that stating *the predictivist thesis* in an hypothetical form is unnecessary. The cognitive ends which serve as the antecedents of (a) and (b) add nothing whatsoever to the credibility of *the predictivist methodological rule. The predictivist rule* can stand on its own as the following methodological rule:

(c) Before any theory can be regarded as empirically well-founded and as making any positive contribution to the growth of scientific knowledge, it must make successful surprising predictions.

Rules such as (c) do not derive their credibility from associations with any specific set of cognitive ends.[5] It is a rule whose credibility can be argued for independently of any specific cognitive end or aim. Rules such as (c) are those which I regard as more foundational than those which can only be stated hypothetically in connection with specific aims. Other examples of such rules are: *reject inconsistent theories; avoid ad hoc*

[5]This is not to say that they are "analytic" or "a priori". The point is that exhibiting the validity of such rules do not require the specification of any particular claim about the natural world. Rather they rest on more general metaphysical and epistemological assumptions which stand or fall with the nature of this world.

modifications; prefer simple theories to complex ones. These rules do not owe their credibility to the specification of any particular or specific cognitive end. Evidence of means/ends connections, and stating them hypothetically is largely irrelevant to their credibility. They are rules which assert standards of scientific theory appraisal, the validity of which is independent of interpreting them hypothetically.

The objection is not that these more foundational rules cannot legitimately be added to cognitive ends to form hypothetical imperatives. On the contrary, they can be added to most, if not all, cognitive ends to form hypothetical imperatives precisely because they contain within themselves their own credentials. The objection is that adding them to specific cognitive ends in no way increases their credibility. This is why they are *foundational*; if we accept them as credible, they must serve as the basis on which any theory of scientific methodology is overlaid.

The fundamental point is that we need to distinguish between *instrumental* and *intrinsic* properties (or attributes) of methodological rules. Not all methodological requirements express instrumental virtues in the sense that we need to specify some cognitive end upon which the acceptability of their virtues depend. Some methodological requirements (such as avoid inconsistent claims) express virtues which do not require the specification of any specific empirical condition; hence, they are foundational.

There is one obvious solution to what I have described as the credibility problem. The solution is to specify an aim (or a set of aims) as the optimum aim of science which all scientists and philosophers adopt. Such an aim would have to be invariant and valid for all times. Most realists, for instance, accept truth as the optimum aim of science. Truth is therefore regarded by realists as a cognitive goal which all theories, past, present and future, must exhibit. This move is however not open to Laudan. In Laudan's view, there are no invariants, everything in science is fully subject to radical change:

> [This view] is thoroughly Heraclitean: theories change, methods change, and central cognitive values shift. ... all these ingredients are potentially in flux ... nothing can be taken as a permanent fixture on the scientific scene ... .(Laudan, 1984, p.64)

3. The problem of relativism: Irrespective of the two surface problems below, (the narrowness problem and the credibility problem), the further question can be asked: does Laudan's normative naturalism provide an adequate response to the challenge of relativism?

As explained in a previous chapter, the problem of relativism confronting any no-invariant-methodologist is this: if the basic principles of rationality are themselves subject to change, against what are we supposed to objectively evaluate our theories? If there are no invariant standards for determining the adequacy of our scientific claims, what counts as a *good reason* for holding a scientific claim or belief will vary from research tradition to research tradition as standards of rationality change. Can scientific beliefs have any plausible claim to objective correctness if each tradition legislates for itself its own standards of acceptability?

The first main point to note is that the relativist's challenge can be made at two different levels. At one level, (let's call this relativism$_1$) the challenge is merely that of whether methodological and epistemic standards have *any* plausible rationale or *any* putative reasons in their support. At this level, relativism is the claim that beliefs and choices lack any rationale or justification. Beliefs and choices are *arbitrarily* chosen and adopted without any real argument in their support.

The relativist's challenge can, however, also be posed at a more fundamental level. At this fundamental level (relativism$_2$), relativism is not merely the thesis that choices, beliefs, and methods lack any putative reasons in their support. Rather the thesis is that *there are no justifications which are valid across traditions; that the only sort of "justifications" we can give is that our present methods turn out better when judged from within specific standpoint, specific theories, or specific research traditions.* Ian Jarvie gives a very precise characterization of this sort of relativist thesis as follows:

> Relativism is the position that all assessments are assessments relative to some standard or other, and standards derive from cultures. The attempt to assess without regard to cultural context and, particularly, the attempt to assess cognitive statements on some transcendental scale of truth, is futile. No assessment can escape the web of culture and hence all assessment is culturally relative. (Jarvie, 1983, p.44)

At this level, relativism implies that the methods adopted by a theory *T* are valid (i.e. justified) for that theory only in the light of that theory's included criteria of merit, and those methods adopted by a rival theory *T* are valid (or justified) for the rival theory only when viewed from within its own internal set of criteria. The relativist, at this level, thinks that our present point of view is *right* in adjudging our present methods correct.

Does Laudan's Normative Naturalism provide an adequate response to the more fundamental relativist challenge? In the following sections of this chapter, my main aim is to show how Laudan's normative naturalism fails to overcome the more fundamental challenge of the relativist's. I also highlight some other key problems with Laudan's normative naturalism.

4. ARE METHODOLOGIES ADEQUATELY JUSTIFIED INSTRUMENTALLY ?

Laudan's instrumental justification of methodologies involves two steps:

(1) A construal of methodological rules as hypothetical imperatives which link cognitive ends to effective means for their realization.

(2) The assumption of a warranting or evidencing principle which can be invoked as a principle of empirical support when adjudicating between competing methods of science. Laudan formulates this principle (R_1) as follows:

> If actions of a particular sort, m, have consistently promoted certain cognitive ends, e, in the past, and rival actions, n, have failed to do so, then assume that future actions following the rule "if your aim is e, you ought to do m" are more likely to promote those ends than actions based on the rule "if your aim is e, you ought to do n. (Laudan, 1987a, p. 25)

The assumption in step (2) tells us the type of *evidence* Laudan has in mind; it is historical evidence about the past efficacy of the postulated means in bringing to realization the ends at issue.

The two steps in Laudan's instrumental justification are controversial. As we have seen, the claim that methodological rules are best construed as hypothetical imperatives is problematic.

The problem with the second step is the age old problem of induction. Step (2) obviously involves an inductive assumption. It requires us to use the past success-rate of means in effecting corresponding ends to justify the assumption that this will continue in the future. First, as Laudan is fully aware, not all theories of scientific method would accept this evidential principle. In some philosophies of science-- namely, Popperian and neo-Popperian philosophies-- inductive reasoning in whatever guise is not permissible. Hence, the assumption not a warranting or evidential principle which all methodologies accept.

As Laudan is of course fully aware of this fact, he merely claims that "it seems plausibly to hold that a broad consensus could be struck among philosophers of science about the appropriateness of (R_1)". (Laudan, 1987a, p.26)

Suppose we waive these objections against the two steps; does Laudan's instrumental approach to justification provide an adequate justification of methodological rules in science? A close look at the actual justifications Laudan give suggests that an instrumental justification of methodological rules is not adequate for scientific rationality. For instance, one of Laudan's favourite examples of how methodological rules are justified instrumentally (and of methodological change in science) is the switch from single-blind techniques to double-blind ones in clinical trials. Until relatively recently, double-blind trials were not part of the "methodology" of clinical trials. This switch could be stated as the following prescriptive rule:

(i): Prefer double-blind clinical trials to single-blind ones.

The first step in Laudan's instrumental approach to justification requires that we reformulate this rule in its hypothetical form. Hence the rule becomes:

(i'): If you want to determine whether a drug genuinely has specified physiological effects, prefer double-blind clinical trials to single-blinded ones.

Laudan maintains that there are empirical considerations which show that double-blind clinical trials are more efficacious in bringing to realization the stated cognitive end of (i'). These empirical considerations are: Scientists have come to realise that the reassuring act of receiving medications and medical attention often has curative effects on patients-- even when they have been given pharmacologically inert drugs. This is the placebo effect. To control the placebo effect in the testing of drugs, controlled experiments are performed on a group of patients. The group of patients on which the clinical trial is to be performed is sub-divided into two groups: the test group, and the control group. Patients in the test group are administered the drug under test, while patients in the control group receive a pharmacologically inert drug which looks like the true drug. But as patients in either group will not know to which group they belong (i.e. patients will not know whether they are receiving the real drug or the dummy drug) the problem of the placebo effect was regarded as eliminated. This is the single-blind test.

But as we learnt more about therapeutic effects, we came to realize that in single-blind tests, researchers can, and often do, convey their own therapeutic expectations to the test patients; hence, the placebo effect could still recur in single-blind tests. Moreover, doctors' expectations might affect their judgement as to whether a patient had benefited from a certain treatment. So it became preferable to perform clinical trials double-blind. In double-blind trials, neither the patients nor those who conduct the experiment know which patient receives the genuine drug, and which the dummy drug. As double-blind trials eliminate a possible source of error which single-blind tests do not eliminate, double-blind tests are more effective means for determining whether drugs genuinely have the therapeutic effects they are said to have.

Of course Laudan is correct in claiming that double-blind methods are better than single-blind ones. The question is whether it is mere instrumental efficacy that provides the rationale for adopting double-blind tests over single-blind tests in clinical trials. As Siegel points out (Siegel, 1990), if Laudan were right in claiming that it is *merely* instrumental efficacy (i.e. the efficacy of double-blind methods in finding out genuinely therapeutic effects) that justifies double-blind tests, then if cognitive ends changed, double-blind methodology might seem less appropriate. Siegel gives a very good hypothetical example in which we are to imagine:

(i) That science has advanced to the stage that we know the extent to which placebo effects which are due to experimenters' expectations are affecting clinical trials; and that this rate of effect is minimal.

(ii) Suppose further that the cost of performing double-blind trials is so high that it substantially reduces the number of subjects to which the drug would have been administered if the test had been single-blind.

In this kind of situation, the choice between single and double-blind methods becomes a choice between:

a) Wasting resources on experiments which control for a real but very small error-- namely, placebo effects due to the transmission of the experimenter's expectations to the subjects - and,

(b) Conducting single-blind experiments which do not eliminate this possible but minimal (and unlikely) source of error, but which have the added advantage of a greater number of tests.

Scientists who favour the use of single-blind experiments in this hypothetical situation could choose to adopt a completely different methodological rule in which single-blind tests are favoured over double-blind ones. Siegel states this alternative rule as follows:

> If one wants to learn, to an acceptable degree of approximation, whether a drug or therapy is genuinely effective, and one wants to learn this of the largest number of drug/therapies one can, prefer single-blind to double-blind experiments. (Siegel, 1990, p. 300)

The problem is that the two alternative rules-- i.e rule (i') and Siegel's hypothetical rule below-- are both equally justified by Laudan's instrumentalist account of justification. For in both cases, empirical considerations can be presented to show that the proposed means are the most efficient ways to the realization of these different ends. Hence, if it were merely the instrumental efficacy of proposed means in bringing about adopted cognitive ends that counts in the justification of methodological rules, it would be impossible to choose between these two alternative rules. This is because given the different sets of goals the two rules adopt (one that of determining whether drugs genuinely have their supposed therapeutic effects; the other that of determining to a degree of approximation whether drugs are effective) and the different means they adopt (double-blind trials against single-blind trial), the two rules would be justified by Laudan's approach. They would both be justified in the sense that scientists who adopt one set, and those who adopt the other set would be able to provide empirical evidence to show that the means they postulate is the best for bringing to realization their corresponding ends. Instrumental efficacy would be completely redundant in choosing between the two methods.

Of course, we all agree that double-blind experimental techniques are better than single-blind ones in clinical trials. The point, however, is that double-blind experimental techniques are better for purely epistemic, non-instrumental, reasons. This is because they eliminate a possible source of error which single-blind tests are unable to eliminate.

Even if we choose to adopt single-blind trials for pragmatic reasons, (as in Siegel's hypothetical case) the epistemic justification of double-blind trials over single-blind ones would still hold. An adoption of single-blind trials over double-blind trials in this hypothetical situation does not show that single-blind techniques are categorically *better* than double-blind ones. Rather, it shows that in science, as well as in everyday life, there are times when the best line of action is not the most practical option to choose. But if methodologies are merely hypothetical imperatives, and if we adopt a purely instrumental conception of justification, it would be impossible to choose between these two rules if goals of inquiry differ.

The point is not that methodological rules *cannot* be "justified" instrumentally as Laudan maintains. Laudan is right in claiming that *evidence* which show the effectiveness of means in bringing ends to realization can, and often do, count in favour of methodological rules. My disagreement is about whether an instrumental justification of methodological rules is strong enough. More specifically, Laudan's naturalist approach to the justification of methodologies will often lead to relativism of choice. In situations such as Siegel's where different cognitive ends are favoured, Laudan's naturalism does not, and cannot, yield the verdict that one method (double-blind trials) is better than another (single-blind trials). All it can say is that given their respective cognitive ends, scientists who adopt the different methods have a rationale in favour of their choices. But this lands naturalism into the sort of relativism it was devised to overcome. Because if cognitive goals are different, all naturalism can say is that a methodological means is justified only in light of its associated goals.

5. HOW INSTRUMENTAL IS RATIONALITY?

Laudan's instrumental approach to the justification of methodological rules is based on his conviction that rationality is about goal-directed action. It is because he maintains that instrumental efficacy is a necessary condition for rationality in general that he also claims that scientific rationality is to be measured by the effectiveness of means in bringing ends to realization:

> Whatever else rationality is, it is agent- and context- specific. When we say that an agent acted rationally, we are asserting minimally that he acted in ways which he believed would promote his ends. Determining that an agent acted in a manner that he believed would promote his ends may or may not be sufficient to show the rationality of his actions; philosophers will quarrel about that matter. But few would deny that it is a *necessary* condition for ascribing rationality to an agent's action that he believed it would promote his ends. (Laudan, 1987, p. 21, my emphasis)

Laudan's view is however not merely that instrumental efficacy is a necessary condition for rationality. He is also committed to the view that actions and choices cannot be justified unless we can exhibit instrumental connections between means and ends:

> ... whenever we judge an agent's rationality ... we must consider: what actions were taken; what the agent's ends or aims were; the background beliefs which informed his judgments about the likely consequences of his possible actions. There is no viable conception of rationality which does not make these ingredients *essential to*, even exhaustive of, the assessment of an agent's rationality. (Laudan, 1987, p. 21, my emphasis)

> ... beyond demanding that our goals must reflect our beliefs about what is and is not possible, that our methods must stand in appropriate relations to our goals, and that our implicit and explicit values must be synchronized, there is *little more* that the theory of rationality can demand. (Laudan, 1984, p. 64, my emphasis).

We need to be clear about what Laudan's full view is because the italicized phrases in these two quotations might suggest that Laudan is not fully committed to making instrumental efficacy exhaustive of rationality. In a reply to Harvey Siegel's [1990] Laudan is very precise and clear about his full commitments:

> ... the only meta-epistemic illustration selection criterion endorsed in Siegel's paper

> is a straightforward illustration of how ends/means analysis provides the framework for the ... analysis of methodological rules. And how could it be otherwise? Justification is itself a *relational* notion. To say that 'x is justified in doing y' is always enthymatic for 'x is justified relative to end(s) in doing y'. There is no coherent sense of justification ... in the absence of the specification of the ends with respect to which an action is deemed justified or rational. That is the central premise of instrumental rationality and of normative naturalism. (Laudan, 1990b, p.317)

Laudan's full view therefore is that explaining an action or belief as rational does not simply depend on whether that action/belief is true or false; nor does rationality consist of purely epistemic considerations about the reasonableness or unreasonableness of proposed lines of action. *What matters most in rationality is the relationship between means and ends (goals)*

The foregoing suggest that, for Laudan, there is a special sense in which instrumentalism is sufficient for rationality. On this view, instrumentalism is not sufficient for rationality in the sense that *all* cases of means/ends connections are rational! Rather, instrumentalism is sufficient for rationality in the sense that we only need to examine the effectiveness of means in bringing ends to fruition to exhibit rationality:

> ... the giving of evidence, no more than the proffering of justifications, does not occur in a vacuum; it is always modulo some aim or other. ... Good reasons are instrumental reasons; there is no other sort. (Laudan, 1990b, p.320)

Laudan is of course correct in claiming that, in science and everyday life, there is an important sense in which to explain actions or beliefs as rational or irrational is to show the instrumental efficacy of adopted means in relation to stipulated ends. Newton-Smith (1981) has described this sense of *rational* as *mini-rationality* (or *minirat* for short).

We can construct *minirat* explanations of scientists' actions or beliefs in abandoning one scientific theory or research programme for another. Such *rational* explanations of scientific change would simply consist in evaluating particular scientist's theory-choice in relation to their implicit goals of inquiry. But does *minirat* provide an adequate explanation of rationality in general, and of the rationality of scientific change in particular? Scientific rationality surely requires a lot more than *minirat* explanations.

First, we can have situations in which a purely instrumental approach to justification cannot dictate choice. Consider again the change from single-blind trials to double-blind ones. If scientists adopt different goals, instrumentalism cannot objectively rank double-blind techniques over single-blind ones. Hence, to overcome relativism of choice in such situations, we need to show that we rank double-blind trials over single-blind trials, not because of instrumental efficacy, but because double-blind trials provide better evidence about the therapeutic effects of drugs, irrespective of individual goals.

Laudan could of course reply that in the example of single-blind/double-blind methodology below, I am at least committed to the aim of having genuine evidence, or the aim of knowledge. I agree that "knowledge" or "genuine evidence" can be characterized as aims of goals. We should note however that these sorts of aims are more *general* or *primary* to science than aims like "control for placebo effect". "Knowledge", if it is to be characterized as an aim, is an aim which is constitutive of the scientific enterprise in general. Hence it is more *primary* to science than aims like "control for placebo" effect.

The problem however is that aims which are constitutive of the scientific enterprise in general are not the sorts of aims Laudan has in mind. In his criticism of Laudan's naturalism, Jarrett Leplin [1990], for instance, takes Laudan to task on the issue of whether cognitive aims are subject to change. Leplin distinguishes the *central* or *primary* aims of science from its *subordinate* or *secondary* aims. And he argues that:

> Despite the many examples readily adducible of what appear major changes in methods and goals, I believe it is easy to show that there must be relative stability at some level. For it must be possible for scientists of periods separated by axiological change to recognize one another as engaged in a common enterprise. ... It is no doubt plausible in some contexts to regard different scientists as operating with different conceptions of knowledge, but at a deep enough level we must regard the concept of knowledge as stable to make sense of the reasoning by which epistemic disagreement is adjudicated in scientific debate. (Leplin, 1990, pp.25-26)

In his reply to Leplin, Laudan of course agrees that aims as general as "knowledge" have remained unchanged in science. Laudan, however, thinks that an aim such as this is too general, and that in any case, he is concerned with the more specific sorts of aims:

> I would be last to dispute that scientists and natural philosophers through the ages would probably all have assented to the claim that the aim of science is "knowledge"; but ... [i]s the knowledge science aspires to a knowledge of causes? In that case we see no agreement among either scientists or philosophers (Laudan, 1990, p.49).

When Laudan claims that: "the giving of evidence, no more than the proffering of justifications, does not occur in a vacuum; it is always modulo some aim or other ... good reasons are instrumental reasons; there is no other sort" (Laudan, 1990, p.320), he is in effect restricting himself to secondary aims. This is precisely why he can claim that his view "is thoroughly Heraclitean: theories change, methods change, and ... cognitive values shift. ... all these ingredients are potentially in flux ... nothing can be taken as a permeant fixture on the scientific scene" (Laudan, 1984, p.64).

My argument so far has been that if the claim that "instrumental efficacy is exhaustive of rationality" is taken to be the: "exhibiting the effectiveness of means must always be in relation to *secondary* aims" (and nothing more), then Laudan's normative naturalism will sometimes lead to relativism of choice. Instrumental efficacy (of the type Laudan wants) cannot be *exhaustive* of rationality. But is instrumental efficacy in fact *necessary* for rationality?

Siegel [1990] raises the same question, and in a reply to Siegel, Laudan's response clarifies a great deal of confusion. Siegel raises the question as follows:

> It is true that 'rationality' is regularly used to denote instrumental efficacy. Still, is it clear that the last mentioned belief is a necessary condition for rational action? It can plausibly be thought not ... . (Siegel, 1990, p.303)

Siegel cites two kind of cases against Laudan's claim that instrumental efficacy is necessary for rationality. In the case 1, an agent mistakenly believes that her actions will promote her ends - even though all the evidence indicate that they do not. (Siegel gives one such example as follows: "a mother's belief that frequent insistence upon her son's regular attendance at Sunday dinner will promote her end, namely his regular attendance, in the face of massive evidence that her frequent insistence has the effect of making his attendance less frequent", [Siegel, 1990, p.303])

Case 2: In the second case, an agent justifiably believes that her actions will promote her ends, but the adequacy of the ends themselves are questionable. (Siegel's example of this second case is one in which a father who wants his daughter to become a professional pianist justifiable believes that getting her devoted completely to the piano will make her a good pianist. Siegel, however, questions the reasonableness or adequacy of the father's goals on the grounds: (a) that they violate the daughter's autonomy, and (b) that "there is ample psychological evidence that the life of a professional pianist is troubling in ways that other lifestyles are not". [Siegel 1990, p.303])

The two cases Siegel cites are problematic. Indeed they do not challenge Laudan's view that instrumental efficacy is a necessary condition for rationality. Consider the first case in which Siegel shows that people sometimes believe, unjustifiably, that their actions promote their ends (when in fact these actions do not). Surely my belief that my personal actions promote my ends has no bearing on whether means *must of necessity* be efficacious to ends before we can judge actions as rational. Laudan's simple response to Siegel is that a necessary condition might be present even when that for which the condition is necessary is absent. That is, we have one of the necessary conditions for rationality (i.e. the belief that actions promote my ends), but in which rationality does not obtain. (As I shall argue above when I consider Laudan's analysis of aims that are acceptable and those that are not, the real problem is that Laudan inevitably augments instrumental rationality with non-instrumental, epistemic, considerations to avoid making cases like this examples of rational actions.)

Siegel's second case is more problematic than the first case. To start with, one could argue that it is not immoral to subject people to psychological traumas. But even if we grant Siegel the claim that it is immoral for a parent to subject his child to unnecessary psychological traumas, surely there is the further question of whether an *immoral* action is *irrational.* But even if we set aside these issues, and accept that immoral actions are irrational, Siegel's second case would be a case in which one necessary condition for rationality (namely, an agents believe that his means promotes his ends) is present while another necessary condition (the requirement that means truly be effective for their ends) is absent. Laudan is thus justified in claiming that Siegel's:

> ... cases can have no bearing on the claim that a *necessary* condition for an action's being rational is that the agent believes his action will promote his ends. ... the logical structure of Siegel's examples makes them irrelevant to assaying a claim about the necessary conditions for rational action. To impugn my thesis that an agent's belief that his actions will promote his ends is a necessary condition for those actions being rational, Siegel needs to adduce cases in which an action is clearly rational even when the agent did *not* believe that his actions would promote his ends. (Laudan, 1990b, p.319)

Can we supply any such example? Consider again the preference for double-blind experimental techniques over single-blind ones in clinical trials. In Siegel's hypothetical situation, we are to imagine that science has advanced to the stage that we know the precise extent to which placebo effects due to experimenters' expectation can affect clinical trials. Unlike Siegel's use of this example, let us suppose that the rate of effect is significant. Suppose further that the cost of conducting double-blind trials is considerably more expensive than that of conducting single-blind ones. (For instance, suppose that the cost of running 10 double-blind tests is the same as that of running 1000 single-blind ones; and that, in any case, running 2 double-blind tests would completely usurp the funds of the research laboratory conducting the tests.)

In such a situation, a scientist who *genuinely* adopts truth as her goal of inquiry (i.e. who, in general, *genuinely* believes in determining whether drugs *actually* have their supposed pharmacological effects, and who, for example, *genuinely* wants to determine whether this new drug can cure cancer) could choose to adopt single-blind tests rather than double-blind ones. In such a situation, the scientist, given her aim, would be adopting a means which she knows is not the most effective one to the realization of her ends. But given the specifics of the situation, the scientist's choice of single-blind trials over double-blind ones is nonetheless rational.

Would this example be a case in which an action is rational even when the agent did *not* believe that his actions would promote his ends? I am sure that Laudan would have a good rejoinder to the example below. And, indeed, to any example which purports to be a case of rational action, but in which the agent did not believe that his actions would promote his ends, Laudan would simply claim that the example merely shows that the scientist had another implicit aim to which her actions can be associated to yield instrumental efficacy. The strategy of stating implicit aims, is quite handy; this is because any example in which one shows rationality, but no association of instrumental efficacy, would easily be explained away in this fashion by Laudan.

Nevertheless, my basic point can still be made independently of examples. The point is that an account of rationality in which instrumentality is taken to be the only adequate mode of rational action or explanation ignores the important role of non-instrumental, epistemic, considerations in rationality. Doppelt (1990) identifies the following three circumstances in which instrumental efficacy is insufficient for assessing the rationality of human conduct (and choices):

(1) [An] act *A* violates powerful social standards of conduct embedded in the judgements of [a person] *P*, as well as the community or group(s) in which

P's activity is embedded.

(2) *P*'s subjective ends *E* are so bizarre, idiosyncratic, incoherent, illegitimate, or misguided by reference to powerful social norms of conduct embedded in the judgements of *P*'s community of peers, as to make *P*'s action *A* seem senseless, incoherent, mad, or otherwise inappropriate.

(3) *P*'s background of beliefs *B* is itself so inconsistent, irrational, idiosyncratic, or unstable relative to epistemic standards embedded in the judgements of *P*'s community or peers, as to make *P* an irrational agent, no matter how effective *A* is, to the realization of *E*. (Doppelt, 1990, p.9)

The point is that we can have situations of instrumental efficacy (hence, mini-rationality) which violate any of the three (especially 2 and 3) circumstances below. Irrespective of whether the action or choice was instrumentally efficacious in bringing the actors' goal to fruition, we will still pronounce such actions and choices maxi-irrational.

The underlying problem is that Laudan also wants to make instrumental efficacy exhaustive of rationality:

> ... beyond demanding that our goals must reflect our beliefs about what is not possible, that our methods must stand in appropriate relations to our goals, and that our implicit and explicit values must be synchronized, there is little more that the theory of rationality can demand (Laudan, 1984, p.64)

He also claims that:

> .. whenever we judge an agent's rationality we must consider: what actions were taken; what the agents ends or aims were; the background beliefs which informed his actions. There is no viable conception of rationality which does not make these ingredients essential to, even exhaustive of the assessment of an agent's rationality. (Laudan, 1987a, p.21)

If the consider the role and nature of axiology in science, it is, I think, clear that instrumental efficacy cannot be "exhaustive of the assessment of an agent's rationality". Consider, for instance the rule of predesignation. According to Laudan, "this rule specifies that a hypothesis is tested only by the new predictions drawn from it, not by its ability post hoc to explain what was already known" (Laudan, 1984, p.36) Popper, Whewell, and Peirce are regarded as proponents of this rule, while Mill and Keynes reject the rule.

As Laudan himself observes, "all parties to the controversy would, I believe subscribe to the same cognitive aims. They seek theories which are true, general, simple and explanatory." (Laudan, 1984, p.36) So the issue is not simply that proponents and antagonists of the rule accept different cognitive values. But if there is axiological consensus, it should be possible, on Laudan's view to perform a simple empirical test to resolve the dispute. For all we need do is to gather evidence to show whether an adoption of the rule of predesignation will yield "theories which are true, general, simple and explanatory".

Of course, no such evidence can be gathered. This is why Laudan is compelled to admit that this "150-year-old and on-going" dispute defies a means/ends solution. Laudan, however, tries to explain away the impossibility of establishing whether predesignation is an effective means to the goals of truth and simplicity by blaming it on the "complexity" of the relationship between methodological means and cognitive ends:

> ... no one has been able to show whether the rule of predesignation is the best, or even an appropriate, means for reaching those ends. That failure is entirely typical. There is no cognitive value and associated methodological rule which have been shown to stand in this one-to-one relation to each other. So far as we know, there may be equally viable methods for achieving all the cognitive goals usually associated with science. (Laudan, 1984, p.36)

I think there is a better explanation for why the dispute over predesignation defies empirical solution: the dispute is not (and never was) about how effective predesignation is in bringing cognitive ends to realization. Empirical evidence has been unable to resolve the dispute not because the relationship between cognitive ends and means is too complex (as Laudan would have us believe); but because the dispute is in fact not an empirical one to start with.

Laudan, perhaps anticipating a point like this claims that we cannot exhibit the adequacy of methodological rules in complete isolation from a discussion of what is a real cognitive aim:

> We have so far been assuming that all aims were on a par and that a methodology's task was simply to investigate, in an axiologically-neutral fashion, which means promote those aims. On this analysis, the construction of a methodology of science is the development of a set of methodological rules, conceived as hypothetical imperatives, parasitic on a given set of cognitive or epistemic ends. Yet although this is an attractive conception of methodology, it scarcely addresses the full range of epistemic concerns germane to science. I suspect that we all believe that some cognitive ends are preferable to others. Methodology, narrowly conceived, is in no position to make those judgements, since it is restricted to the study of means and ends. We thus need to supplement methodology with an investigation into the legitimate ends of inquiry. That is, a theory of scientific progress needs an axiology of inquiry, whose function is to certify or de-certify certain proposed aims as legitimate. (Laudan, 1987a, p.29)

But what sort of considerations would make up these certifications or de-certifications? Laudan proposes two considerations: (i) that we should not adopt utopian goals, and (ii) that goals which fail to accord with the values implicit in the communal practices and judgements of science, (or those which fail to reconcile theory and practice), must be rejected.

There are two sorts of problems with these considerations. First is the question of whether these considerations are strong enough to function as certification or de-certification conditions in determining the appropriate aims of inquiry. Consider, for instance, the consideration that we should accept only those aims that are in accord with the values implicit in communal practices and judgements of science. To adopt this as a certification rule is to presuppose that there is no disagreement about which values are truly implicit in science. But, of course, philosophers and scientist alike disagree about which values and aims are actually implicit in scientific practice! (They also hotly disagree about how to characterize those values that are explicitly adopted by scientists.) This is why we have realists, instrumentalists, pragmatists, etc, among both scientists and philosophers. Before this condition can function as a certification rule, we must first of all settle the issue of which aims are truly implicit in the communal practices of science (and questions about how to properly characterize explicit rules.) But this question is just as problematic as the question it is supposed to help resolve!

Furthermore, even if we are able to identify and specify what these values are, there is the further question of whether these values are the ones we *ought* to promote. The mere fact that a value is actually the one adopted in scientific practice does not, in itself, imply that it is the one we *ought* to adopt. We would need some further argument in support of the adoption of such values or aims.

There is a more fundamental objection to Laudan's no-utopians decertification condition. It is not at all obvious that there is any real problem with the adoption of utopian aims. Suppose one adopts an aim which one recognizes as utopian, but against which one rank theories in light of how best they strive to achieve this utopian aim.

Although one would need to produce a criterion (or a set of criteria) against which theories can be measured to determine how close they bring us to this aim, the procedure itself is quite reasonable. Popper's adoption of truth as the goal of science is somewhat similar to this. Truth, for Popper, is an ideally perfect property scientific theories ought to aim at. But the history of science teaches us that even our most successful theories may turn out to be, strictly speaking, false. Hence, Popper proposes *verisimilitude* as a measure against which the 'nearness' of scientific theories to truth can be measured. One can criticize the adequacy of the verisimilitude criterion, but the adoption of truth as an utopian aim of inquiry is quite rational.

The second sort of problem with Laudan's certification or de-certification conditions is about the manner in which Laudan argues for these conditions. What sorts of 'reasons' does Laudan give in support of these conditions? If Laudan is to be consistent in his claim that instrumental rationality is exhaustive of rationality, then the reasons he gives for accepting these certification conditions should be instrumental ones. But Laudan, in fact, gives epistemic, non-instrumental reasons in his advocacy for these considerations. For instance, he justifies the *non-utopian* condition as follows:

> ... it is at the very core of our conception of the rational and the reasonable that anything judged as satisfying that family of concepts must, in appropriate senses, be thought to be both possible and actionable. To adopt a goal with the feature that we can conceive of no action that would be apt to promote it, or a goal whose realization we could not recognize even if we had achieved it, is surely a mark of unreasonableness and irrationality. (Laudan, 1984, p. 51)[6]

[6]This shows that Laudan overlooks the possibility of using utopian aims as the limits towards which we strive, but which is fully recognized as unrealizable. That is, for Laudan, *ideals* as unrealizable ends have no role to play in epistemology and theory appraisal. There are, however, various examples of constructive uses of ideals in science. C S Peirce's method of truth as the limit of inquiry is one good example.

He also justifies the stipulation that scientists external and internal aims must cohere as follows:

> On the pain of being charged with inconsistency ... the rational person, confronted with a conflict between the goals he proposes and the goals that appear to inform his actions, will attempt to bring the two into line with each other. (Laudan, 1984, p. 55)

Laudan has been smuggling after all! He initially claimed that to be rational, (and to give rational justifications in science), is to provide evidence which exhibit linkages between means and goals. This he calls a theory of methodology. He then concedes that instrumental justification lacks some important ingredients of rationality. What supplies the needed component to make the whole view adequate is called the theory of axiology. But the constraints axiology puts on methodology are not justified entirely instrumentally; they are justified epistemically. This is because in his justification of his two constraints on axiology, he offers us considerations which are based on conceptual (not instrumental) analysis; considerations such as consistency (that explicit and implicit aims should cohere) are those brought into play in axiology. In short, rationality is a function of good, objective, time-independent reasons after all!

Of course Laudan rebuffs the charge of being non-naturalist about axiology. In his reply to a similar charge by Doppelt (1990), Laudan claims that:

> Doppelt, having supposed that I thought methodology was purely empirical, compounds the interpretative crime by further imagining that I treat the aims of science as subject to only purely conceptual analysis. ... It is true that I stress that inconsistent and incoherent aims ought to be rejected, but so should similarly afflicted rules and theories. But, I went to some lengths to argue in *Science and Val-*

> *ues* that the discovery of the *non-realizability* of certain aims ... is a powerful instrument driving the change of aims. By the same token, the most straightforward way of exhibiting the realizability of an aim is by showing that it has been realized, and that is a pretty straightforward empirical matter. (Laudan, 1990, p.51)

But this remark of Laudan's in no way defuses the weight of the arguments I set out below. For the thrust of my point is that Laudan has being taking the adequacy and import of his realizability (or non-utopian) criterion too much for granted. Is this criterion straightforwardly administered as Laudan supposes? First, I challenged the use of the criterion itself. I argued that that an aim is utopian is not straightforwardly as bad as Laudan would have us believe. One may rationally adopt an utopian aim. Second, I argued that whether an aim has been realized or not is also not straightforwardly settled by empirical issues. Philosophers and scientists hotly disagree about whether a method has brought us to the realization of an aim. (Consider again the dispute concerning predesignation.) Even when scientists (and philosophers) do adopt the same means and goals, deciding whether their goals have been realized is not so easily resolved by empirical considerations. "Exhibiting the realizability of an aim" is not "a pretty straightforward empirical matter" as Laudan would have us believe.

This suggests that even if we grant Laudan the claim that methodological rules *can* be naturalistically criticized, (i.e that evidence about the efficacy of means in bringing about cognitive ends sometimes count in favour of methodological rules), it would not follow that philosophy of science itself has been naturalized. Even in Laudan's own analysis, rational evaluation in science is not completely natural; it is above all an epistemic affair. This is why the constraints axiology puts on methodology are themselves not naturalistic; they consist mainly of conceptual, epistemic, non-instrumental analysis.

One defect of Laudan's treatment of cognitive ends, therefore, is that it is a half-hearted naturalism. For although Laudan claims to provide a naturalistic account of the rationality of cognitive aims, in fact he does not. In his so-called "naturalistic" analysis of cognitive ends, Laudan offers considerations which are based on conceptual analysis and epistemic considerations. Hence, when it comes down to it, Laudan cannot do away with the "traditional" approach which maintains that rationality is a function of epistemic, eternal, objective considerations. The major defect of Laudan's normative naturalism however is that even this half-hearted naturalism provides no viable account of scientific rationality and progress.

I would like to distinguish between the criticism I am advancing when I claim that Laudan's naturalism is a half-hearted one, and the one Gerald Doppelt [1990] advances when he claims that Laudan's normative naturalism is *narrowly empirical.* When Doppelt criticizes Laudan's naturalism for being too narrowly empirical, his argument is that in his treatment of methodological means, Laudan is interested only in purely empirical issues. But in treating axiology, Doppelt insists that Laudan makes use of purely conceptual considerations.

Of course Doppelt's contrast between a purely empirical methodology and a purely conceptual axiology misrepresents Laudan's view. Indeed, Laudan is very clear on the point that conceptual considerations play important roles in methodology as well:

> I am not claiming that the theory of methodology is a wholly empirical activity, any more than I would that theoretical physics was a wholly empirical activity. Both make extensive use of conceptual analysis as well as empirical results. But I do hold that methodology can be and should be as empirical as the natural sciences whose results it draws on. (Laudan, 1987b, p.231)

The problem I raise is not that of whether methodology is purely empirical. *My concern is with whether axiology is as empirical as methodology in Laudan's naturalism.* What I have argued is that in his analysis of the adequacy of axiology, the only viable considerations Laudan gives are epistemic, non-instrumental ones. But Laudan himself concedes that a theory of methodology cannot do without a theory of axiology; "... the construction of a methodology of science is ... parasitic on a given set of cognitive ... ends". (Laudan, 1987b, p.29) If the constraints axiology places on methodology are all non-instrumental ones, (and I have shown below that they are) then to claim that "good reasons are instrumental reasons: there is no other sort" (Laudan, 1990, p.320) is half-hearted.

6. WHY NORMATIVE NATURALISM FAILS TO PROVIDE A RATIONAL EXPLANATION OF SCIENTIFIC PROGRESS

The question I address in this section is the following: does Laudan's reticulational model of scientific rationality (and its associated normative naturalism) provide an adequate explanation of progress (and rationality) in science?

Although just like Popper and Lakatos, Laudan (1977) initially advocated the "rationalist" view of explanation in science, he (1984-to date) has made some emendations to the type of explanations he now seeks. As he puts it:

> [Although] methodology has an important role to play in explaining some striking features about the history of science ... it has nothing to do with exhibiting or explicating the rationality of past science. What does require explanation is the fact that science has been so surprisingly successful at producing the epistemic goods. We take science seriously because it has promoted ends which we find cognitively important. More than that, it has become *progressively* more successful as time

> goes by. If you ask, "Successful according to whom?" or "Progressive according to what standards?" the answer, of course, is: successful by *our* lights; progressive according to *our* standards. Science in our time is better (by our lights of course) than it was 100 years ago, and the science of that time represented progress (again by our lights) compared with its state a century ago". (Laudan, 1987a, p. 28)

In fact, the article from which this quotation is taken is titled "Progress or Rationality? The prospects for Normative Naturalism." And in this article, there is a section titled: "Progress not Rationality".

But if the only judgement we can give is that 'our lights' adjudge themselves *better*, are we left with anything other than outright relativism? The problem is that of whether one can give any viable account of scientific *progress* without the assessment of past scientists' theory choices as rational or irrational. For even if one is not directly concerned with "exhibiting or explicating the rationality of past scientists" choices, the issue of rationality still arises. This is because the status and nature of our *explanations* of *progress* and *success* should exhibit how new theories and methods are accepted in favour of old ones on the basis of *good* reasons. An acceptance of the *correct* (or the *better*) theory (or method) for bad or irrational reasons would not constitute progress in science. That is, we need to show that explanations of what constitute progress and success are justified in the sense that they are based on *good objective reasons*. An explanation of progress which violates the dictates of reason, or which is subjective, would not be rationally acceptable; nor would it amount to scientific progress. Rational change is a prerequisite for judgements of progress. Hence, even if Laudan is not interested in explaining past choices of scientists as rational or irrational, the problem of rationality can still be raised in this sense.

Laudan evidently believes that his position does not commit him to relativism. Just like the Kuhnians, Laudan allows radical change in all aspects of scientific commitment: facts, methods and theories. And as we have seen, Laudan criticizes Kuhn and the Kuhnians for their espousal of relativistic views. Doesn't Laudan fall into the same kind of relativism if all we can say is that "science in our time is better (by our lights of course) than it was 100 years ago"? (Laudan, 1987, p.28) (A judgement which Kuhn would surely readily endorse.)

This claim of Laudan's obviously commits him to the view that there are no standards of justification which transcend the boundaries of time-dependent "lights":

> The argument is straightforward: to the extent that scientists of the past had aims and background beliefs different from ours, then the rationality of their actions cannot be appropriately determined by asking whether they adopted strategies intended to realize *our* aims. ... It would be appropriate to use our methods to assess the rationality of past scientists only if their cognitive utilities were identical to ours, *and* only if their background beliefs were substantially the same as ours. (Laudan 1987a, p.21)

But "lights" or standards of evaluation are not, descriptively speaking, uniform-- even at a given period. People nowadays believe all sorts of strange things. The creationist, for instance, believes that no theory of the origin of the universe and its present inhabitants can be true or adequate unless it is consistent with the 'literal truth' of Genesis. They have produced lots of books which assert that the natural world was created by God, and they vehemently denounce Darwinism. According to the light (or research tradition) of the creationist, nature bespeaks God's hand, and Darwinism is completely false. Given the aims of the creationist, (i.e. producing theories that are consistent with Genesis), Creationism is obviously better and preferable to Darwinism (according to the lights of the Creationist). If Laudan, just like Kuhn, claims that in theory appraisal, all we can do is evaluate in terms of standards within "our light", then surely he cannot deliver the judgement that Darwinism is objectively better than Creationism. If, on the other hand, Laudan allows us to say that "our lights" are in fact those of the scientific elite and that they are preferable to those of current pseudo-science, why does he rule out the possibility of assessing the choices of past scientists by our current standards?

The problem with Creationism vs. Darwinism can be generalized. If all we can do is to relativize evaluations and judgements of rationality to specific standards of evaluations, then when confronted with radically different standards that fully satisfy their own internal set of requirements, (irrespective of whether one set is no longer accepted by contemporary scientists), surely reticulation would be unable to assess whether one set of criteria is objectively superior to the other. Indeed, if assessment in terms of internal criteria is all we have, reticulation ought to be committed to the view that, as long as respective evaluations fully satisfy the internal requirements of their standards, then they are equally valid views of the world! Surely, this is an extreme form of relativism.

Laudan can of course avoid relativism if he can supply an answer to the question of why it would be objectively correct to prefer one of two rivai theories as the better theory. If the answer he would give is to work, it would have to make evaluations across lights. Indeed, some of Laudan's considerations function as such. For instance, his requirement that aims be realizable, and the requirement that implicit and explicit aims cohere. The problem, however, is that these requirements are not relativised to any specific epoch; they are criteria of evaluation which cut across research programmes.

Indeed it seems to me that it is precisely because these considerations are not localized to specific historical epochs (i.e. contrary to Laudan's explicit claims, they function as *invariant* aspects of his naturalist account of scientific rationality and methodology) that Laudan's normative naturalism gives the (false) impression of avoiding the sort of relativism into which Kuhn and the holists fall. In so far as he avoids relativism, it is because he is in fact committed to (despite what he says elsewhere) cross-epoch standards such as the criterion of realizability.

Suppose we were to accept Laudan's version of the no-invariant-methodology thesis at its face value. This ought to imply that the non-utopian requirement, and the requirement that implicit and explicit aims cohere are both subject to (possible) change as well. But are they? A close attention to Laudan's own criticism of the hierarchical model for instance suggests that Laudan takes the condition of realizability to be an invariant. Suppose that the hierarchical modeller were to reject precisely these requirements (e.g. she accepts unrealizable aims), and develop her own rival axiological and methodological requirements (e.g. if she adopts fully recognized utopian aims as the limit of inquiry which we ought to strive to attain, but which, in fact we can never attain). Suppose further that the hierarchical modeller was fully able to satisfy her internal set of requirements. Then unless Laudan makes these two requirements of his invariant, he would have to claim that as long as long as the hierarchical modellers choices meet her internal requirements, they are acceptable. This however lands Laudan straight into the relativism he hoped to avoid.

7. CONCLUDING REMARKS

In this chapter I examined Laudan's instrumentalist version of the no-invariant-methodology thesis. According to this view, methodological rules are hypothetical imperatives of the means/ends type. Laudan further maintains that *no* aspect of science (means, ends, and methodological rules included) is in principle immune to (possible) radical change. Laudan, therefore, adopts a very strong version of the no-invariant-methodology thesis.

First, I argued against Laudan's hypothetical construal of methodological rules on the ground that some core rules and principles of scientific theory appraisal are better formulated epistemically as categorical imperatives. Furthermore, I argued that Laudan's instrumentalist approach to the justification and validation of methodological rules is deeply flawed because, contrary to Laudan's belief, such an approach inevitably leads to relativism.

I further argued that an instrumentalist account of scientific change cannot give any viable explanation of the growth and progress of scientific knowledge. Had Laudan distinguished between foundational and substantive methodological rules, it could have been possible to overcome the problem of relativism. But this option is not open to Laudan; he explicitly claims that *all* methodological rules are hypothetical and substantive in the sense that their credibility is dependent upon empirical claims about the nature of this world.

Finally, I argued that normative naturalism initially *appears* to give a rational explanation of change in science only in so far as some of its tools of evaluation are implicitly assumed to be invariant. I specifically identified two such tools: the requirement that aims be non-utopian, and the requirement that implicit and explicit aims cohere.

Despite the impression Laudan likes to give, he has not escaped (because he cannot escape) the fundamental dilemma: *either* all methodological standards are subject to change and so there is no rational explanation of change, *or* at least some such standards are sacrosanct, "above the fray".

CHAPTER 4

The 19th Century Revolution in Optics

1. LAUDAN ON THE METHODOLOGY OF LIGHT

By the end of the third decade of the 19th century, a revolution in scientific opinion about the nature of light had (to all intents and purposes) been completed. The revolution involved a change from "the" Newtonian theory of light (also known as the particle, emission, projectile, or corpuscular theory), to "the" wave (or undulatory) theory of light.[1] The central assumptions of the corpuscular theory can be regarded as the following:

(a) that light is made up of tiny particles (called corpuscles) emitted from luminous objects; and

(b) that these particles obey the usual (Newtonian) laws of particle physics.

[1]Of course in actual fact there was no such thing as *the* corpuscular theory of light, or *the* wave theory of light. Rather there were two series of theories which had various specific theories within them that shared the respective central assumptions to be outlined. These central assumptions should therefore be regarded as the "hard cores" of the respective series of theories. I adopt a somewhat monolithic construal of both series of theories simply because I am primarily interested in the *methodological issues* raised by the 19th century revolution in optics. I should add, however, that despite the variations in the specific versions of both series of theories, there is no doubt that it was a Newtonian corpuscular theory that was widely accepted in the 18th century; nor is there any doubt that some version or the other of the Young-Fresnel wave theory became dominant by the end of the 1830s.

And the central assumptions of the wave theory can be taken to be:

(a) that light is a kind of disturbance in an all-pervading elastic medium called the ether; and

(b) that differences in the colour of light depend on the frequency of vibrations excited by luminous objects in the ether.

The scientific revolution that occurred during the 19th century at least entailed a change from the central assumptions of the corpuscular theory to those of the Young-Fresnel wave theory.

But Laudan and several philosophers and historians of science have claimed that the revolution was accompanied by an underlying and deeper change in the methodological and epistemological requirements of science:

> ... the half century following publication of the *Principia* was marked by a growing antipathy to hypotheses and speculation. ... the refrains [were] ... speculative systems and hypotheses were otiose; scientific theories had to deal exclusively with entities that could be observed or measured. (Laudan, 1981b, p.158)

> There remain many philosophers of science and theorists of scientific change who, though granting that substantive theories about the world do change, nonetheless adhere to the view that the canons of legitimate scientific inference are perennial and unchanging. (Included here are thinkers as diverse as Popper, Nagel, Carnap, Hesse, and Lakatos, among others.) The case we have before us stands as a vivid refutation of their claims that scientific standards of theory evaluation are immutable. It simply cannot be denied that, prior to the early nineteenth century, the ability of a theory to make successful, surprising predictions was no sine qua non for its acceptability; nor can it be denied that by the turn of the twentieth

> century, the requirement of predictivity was a commonplace in both scientific and philosophical circles. (Laudan, 1981b, p.181)

According to this account, not only was there *a change* from the Newtonian inductivist methodological requirements to the method of hypothesis (or the *hypothetico-deductive* methodology), the change there was, was a very *radical* one. Newtonian inductive methodology, it is claimed, banned the use of hypotheses in science, and it emphasized the requirement that the claims of science must be based on inductions from the phenomena. But proponents of the wave theory were committed to the method of hypothesis:

> The epistemology prevalent in the second half of the 18th century was *altogether incompatible* with the various ether theories that emerged in the natural philosophy of that period. ... Some of the early proponents of ethereal explanations chose to abandon or modify that prevalent epistemology so as to provide a philosophical justification for theorizing about ether. ..[T]he emergence of the optical ether in the early 19th century prompted a ... radical critique of classical epistemology, a critique that produced some highly innovative and historically influential methodological ideas. (Laudan, 1981b, p.157-158, my emphasis)

Concerning the initial opposition to the wave theory by Scottish natural philosophers, he also claims that:

> The primary reason for the opposition to the ether theories was the widespread acceptance among Scottish philosophers and scientists of a trenchant inductivism and empiricism, according to which speculative hypotheses and imperceptible entities were inconsistent with the search for reliable science. (Laudan, 1981b, p.170).

Laudan is not alone in the attribution of a change in methodology to this revolution. The historian Geoffrey Cantor also insists that while corpuscularians "followed the [eighteenth-century] common-sense philosophers in considering induction to be the proper scientific method" (Cantor, 1975, p.111), "supporters of the wave theory, unlike its objectors, championed the method of hypothesis". (Cantor, 1975, p.114)[2]:

> Although in the eighteenth century, almost every British natural philosopher accepted without question the corpuscular interpretation of Newton's writings on optics, by the 1830s most British philosophers had rejected Newton's corpuscular theory in favour of the wave theory of light. Intimately bound up with this scientific "revolution" in optical theory was a change in scientific methodology: the replacement of the method of induction by the method of hypothesis. (Cantor, 1975, p.109)

In this chapter, I shall argue that contrary to Laudan's (and Cantor's) claim, no *radical* methodological change accompanied the change in substantive theoretical assumptions. The real methodological debate between corpuscularians and wave theorists, I shall argue, concerned the point at which it became rational to abandon a theory faced with accumulating empirical and theoretical difficulties. I start by taking a closer look at Laudan's analysis of this historical episode.

There is, of course, an old distinction between *implicit* and explicit *methodology*: a distinction we called upon before and which is again vital here. A scientist's *explicit* methodology is what she actually says and writes in her reflections on her scientific

[2]One significant difference between Laudan's and Cantor's treatment of the optical revolution is that while Laudan compares the methodology of 19th century wave theorists with that of 18th century corpuscularians, Cantor (in his 1975 and 1983) compares the debate between 19th century wave and corpuscular theorists.

method, while *implicit* methodology is exhibited by her practices and choices. A scientist's implicit and explicit methodology may either cohere or diverge. That is, the methodology to which a scientist is *really* committed could be very different from what she describes as her methodology. Most philosophers and historians, for instance, see Newton as the best example of a great scientist whose explicit pronouncements about the methods he adopted are completely at odds with his implicit, real, methodology. (After all, Newton, it is often said, explicitly claimed that he had no use for hypotheses in experimental philosophy yet his works are full of them.)[3]

Unfortunately, Laudan is not very clear on whether the radical change he claims occurred was merely in *explicit* methodology, or in both *explicit and implicit* methodology. Is the change Laudan describes in his account of this episode a mere change in the way scientists were likely to *describe* their methodological requirements, or was there also a change is implicit, real, methodology? Were proponents of the corpuscular theory *genuinely* committed to a Newtonian inductive methodology, while wave theorists were *genuinely* committed to the method of hypothesis?

Of course it would hardly be surprising if a change occurred in the sorts of explicit methodological pronouncements scientists were likely to make alongside change in theory. No doubt some 18th century scientists at least had convinced themselves that the

[3]I should point out immediately that I think this popular perception of Newton is unjustified. For although there is absolutely no doubt that Newton held *hypotheses* in low esteem, Newton was also very careful in his definition of *hypothesis*: "Whatever is not deduced from the phenomena is to be called an hypothesis; and hypotheses whether metaphysical or physical, whether of occult qualities or mechanical, have *no* place in experimental philosophy. In this philosophy, particular propositions are inferred from the phenomena, and afterwards rendered general by induction. Thus it was that the impenetrability, the mobility, and the impulsive forces of bodies, and the laws of motion, and of gravitation, were discovered". (*Principia*, p.547)

It should also be noted that Laudan does not claim that Newton himself was a strict inductivist who rejected observation transcendent hypotheses.

Newtonian theory of light involved no hypothetical elements; while the all-pervading ether involved in the wave theory was more obviously hypothetical. But the issue of the rationality of science concerns *implicit* methodology-- have scientists *implicitly always judged theories by the same standards?* Is Laudan really claiming that 18th century scientists would not have preferred a truly hypothetical but predictively successful theory even if one were available? And that only in the 19th century did predictive success become a really operative criterion?

I believe that Laudan is simply confused on this issue. A good deal of what he says is addressed only to the weak, unsurprising claim of a change at the *explicit* level:

> The chief source for this shift in the explicit attitudes of philosophers and scientists towards the legitimacy of postulating unseen entities was a prior shift in the character of physical theory itself. Specifically, by the 1830s scientists found themselves working with theories that, as they eventually discovered, violated their own explicit characterizations of the aims of theorizing. Confounded by that discovery, they eventually reappraised their explicit axiology. (Laudan, 1984, p.56)

But in the very last footnote of an article in which Laudan advances his claim of radical methodological change, Laudan also seems to imply that the *only* change there was was a change in *explicit* methodology:

> A minor caveat is in order ... Several philosophers and scientists before the nineteenth century (e.g., Boyle, Huygens, and Liebniz) had claimed that the ability of a theory to make surprising predictions was an epistemic advantage. But prior to the 1820s no *systematic* arguments had been made to the effect that such an ability was a sine qua non for an adequate theory. (Laudan, 1981b, p.185, fn.92)

Surely if the methodological change that occurred in the 1820s was merely in the ability of scientists to spell out the requirement of independent predictive support, then this historical episode provides no real support to the no-invariant-methodology thesis. Evidence of change in explicit methodological requirements alone do not provide any viable challenge to the traditional approach to scientific methodology because when traditional philosophers like Popper and Lakatos spoke of a fixed set of methodological requirements, their object of concern was not what scientists actually said (or wrote down) in their reflections on methodology. Their object of concern was a much more restricted set of norms which is *revealed or exhibited* by the actual choices and practices of scientists in situations of theory choice.

If Laudan's claim is merely that there was a change in *explicit* methodology, then the traditionalist could readily concede such radical change. The traditionalist would however insist that whatever changes might have occurred in explicit methodology have all been governed by implicit methodology which is itself unchanging.

I hesitate in attributing a "change in explicit methodology view alone" to Laudan because he evidently believes that his analysis of this historical episode provides evidence against the traditional approach:

> ... many philosophers of science and theorists of scientific change ... adhere to the view that the canons of legitimate scientific inference are perennial and unchanging. (Included here are thinkers as diverse as Popper, Nagel, Carnap, Hesse, and Lakatos, among others.) The case we have before us stands as a vivid refutation of their claim that scientific standards of theory evaluation are immutable. (Laudan, 1981b, p.181)

And Laudan also claims that Newtonian inductivism "was altogether incompatible with the various ether theories" of the 18th century. Because of these claims of Laudan's (despite the suggestions of the caveat below), I shall take Laudan's *full* view to be that there were changes in *both* implicit and explicit methodology. In Laudan's treatment of the 19th century revolution in optics, therefore, proponents of the corpuscular theory did not merely claim to be strict empiricists. Strict empiricism was in fact the methodology they actually practised. Conversely, wave theorists were committed both in practice and in writing to the method of hypothesis.

Laudan divides his analysis of this historical episode into two phases; 1740-1810 as the first phase, and 1820-1840 as the second phase. Laudan claims that during the first phase several thinkers developed theories which postulated the existence of an invisible ether, thereby contradicting the strictures of Newtonian inductivism. There was a strain or incompatibility between the developing ether theories and the methodological requirements of Newtonianism which had been in force since the triumph of Newton's ideas.

According to Laudan, there were two types of responses to this tension between Newtonian inductivism and ethereal explanations. Philosophers like Thomas Reid rejected ethereal explanations by allowing their inductive methodological strictures to take priority, while others like David Hartley and George Lesage developed an alternative methodology to legitimize ethereal explanations.

Hartley, for instance, was convinced of the explanatory importance of the ether. He maintained that the ether explained a broad range of phenomena: ethereal explanations were employed in accounting for the phenomena of heat, gravity, electricity, and magnetism. But most important for Hartley was the use of the ether in explanations of psychological problems concerning perception, memory, habit, etc. Hartley assumed that the brain and the nervous system are both filled with the ether, and that psychological functions were the result of vibrations within the ether.

Hartley realised that he could not deduce the existence of the ether from the phenomena. He was also aware that there was no direct empirical evidence for the existence of the ether. Hence he had to supply an epistemological justification for his ethereal explanations by developing a new method of post hoc confirmation in which:

> ... broad explanatory scope compensated for the unobservability of its explanatory agents and mitigated its failure to exhibit a traditional inductive warrant. (Laudan, 1981b, p.161)

Hartley's suggestion was that the method of inductivism is not the only route to knowledge. He therefore advocated the *method of hypothesis* as a complementary method to Newtonian inductivism. This method, according to Laudan, has the following structure:

> Here is a phenomena *x*.
> *But if there were an ether, then x.*
> (Probably) there is an ether. (Laudan, 1981, p.161)

And Laudan quotes Hartley as follows:

> Let us suppose the existence of the aether, with these its properties, to be destitute of all direct evidence, still, *if it serves to explain a great variety of phenomena, it will have an indirect evidence in its favour by this means.* (Quoted in Laudan, 1981, p.161)

Laudan insists that Hartley's version of the method of hypothesis is just as stated below. That is, the method justifies the acceptance of *any* theory in as much as an assumption of that theory would explain a wide variety of phenomena:

> ... Hartley merely insisted that if a hypothesis is compatible with all the available evidence, then that hypothesis 'has all the same evidence in its favour, ...' In a nutshell, Hartley's method of hypothesis boils down to the claim that a hypothesis warrants belief if it has a large number of known positive instances .... (Laudan, 1981, p.163)

According to Laudan, although Hartley emphasized that his method of hypothesis does not, and cannot, guarantee the truth of the hypotheses it sanctions, the method was still rejected by Newtonian inductivists. The primary objection to Hartley's methodology was simply that it violated Newtonian inductivism by postulating unobservables.

Laudan discusses Reid's criticism of Hartley's method of hypothesis. In Laudan's view, "one of the foundation stones of Reid's philosophical system and the central attitude he adopted from Newton, was his suspicion of, bordering on contempt for, any theories, hypotheses, or conjectures which are not *induced* from experimental observations". (Laudan, 1981, p.89) Given Reid's (and inductivists') commitment to an extreme empiricist version of Newtonianism, he simply could not countenance the method of hypothesis.

But there was a further objection to the method of hypothesis. This objection was that, at best, the method can only show that a conjectured theory 'saves the phenomena'. But if suitable ad hoc modifications are made, various rival theories could all be adjusted to accommodate the phenomena:

> Not surprisingly, this epistemology carried little weight with most of Hartley's inductivist contemporaries. As they could point out, there were many rival systems of natural philosophy that - after suitable ad hoc modifications - could be reconciled with all known phenomena. ... 'saving the phenomena was an insufficient warrant for accepting a theory. (Laudan, 1981b, p.163)

Although the 18th century defence of the method of hypothesis by Hartley was unsuccessful, Laudan insists that the development of the wave theory of light in the early 19th century by people such as Young and Fresnel brought about a new version of the method of hypothesis. It was this new version of the method of hypothesis that eventually forced the radical rejection of Newtonian inductivism. As already mentioned, one major objection to the 18th century version of the method of hypothesis was that its proponents could not provide any adequate criterion for distinguishing genuine hypothesis from *ad hoc* ones. Hartley and his 18th century proponents of the method of hypothesis regarded *all* consequences of theories as evidence for theories. 19th century proponents of the wave theory, however, developed a new method of theory appraisal in which theories that go beyond the phenomena are accepted only if they have *independent predictive warrant.*

The condition of independent warrant requires a theory either to successfully predict new facts, or to explain phenomena it was not originally designed to explain. So unlike Hartley's version of the method, 'saving the phenomena' was not enough:

> In brief, this criterion, which was nowhere prominent in the late 18th-century debates about the methodological credentials of subtle fluid, amounts to the claim that an hypothesis which successfully predicts future states of affairs (particularly if those states are 'surprising' ones), or which explains phenomena it was not specifically designed to explain, acquires thereby a legitimacy which hypotheses which merely explain what is already known generally do not possess. (Laudan, 1981b, p.173)

In Laudan's view, the requirement that a hypothesis yield successful surprising predictions, and the requirement that it ought to explain facts it was not originally designed to explain, were both read off, the obvious success of the wave theory. For the requirement that a hypothesis, in order to be successful, make surprising predictions, and the requirement that it explain various previously known phenomena were precisely those features that led to the widespread acceptance of the wave theory during the 19th century.

The key points involved in Laudan's reconstruction of this historical episode can be stated as follows:

(1) All the historical evidence Laudan has merely support the view that in their *explicit* methodology, 18th century defenders of the corpuscular theory upheld a version of Newtonian inductivism which rejected hypothesis. But since: (a) evidence of change in explicit methodology alone provides no viable challenge to the traditional view of an invariant set of methodological criteria; and (b) Laudan claims that this historical episode "refutes" the traditional approach to methodology, I will take it that Laudan's *full* view implies the following: 18th century defenders of the corpuscular theory of light *genuinely* accepted (and adopted in practice) an empiricist version of Newtonian inductivism which banned all theoretical entities.

(2) Because of the unobservability of the ether, 18th century proponents of ethereal theories such as Hartley were forced to develop an alternative methodology--

namely the method of hypothesis-- to legitimize their theories. But because the 18th century versions of the method of hypothesis legitimized all spurious and ad hoc hypotheses in so far as they 'save the phenomena', the method of hypothesis and its then associated ethereal theories were rejected.

(3) Because of the apparent explanatory superiority of the wave theory over the corpuscular theory (the wave theory explained all the optical phenomena the corpuscular theory could explain, and it also explained phenomena such as interference which the corpuscular theory could not explain), 19th century proponents of the wave theory were also forced to modify the method of hypothesis to which they were committed. Specifically, they added the condition of independent predictive warrant.

(4) Newtonian inductivism was not merely different from the new version of the method of hypothesis, "the epistemology prevalent in the second half of the 18th century was altogether incompatible with the various ethereal theories which emerged in the natural philosophy of that period ... [and] the emergence of the optical ether in the early 19th century prompted a more radical critique of classical epistemology, a critique which produced some innovative historically influential methodological ideas". (Laudan, 1981b, pp.157-158) *The most significant of these innovative methodological ideas was the requirement of independent predictive warrant.* Indeed, "no epistemologist in the 18th century would have been impressed [by the condition of independent predictive warrant], for the notion of

independent support ... is very much a product of the early-19th century." (Laudan, 1981, p.175)

In the remainder of this chapter, I will challenge Laudan's reconstruction of this historical episode. Specifically, I will argue that Laudan is mistaken in claiming that the condition of independent predictive warrant is "a product of the early-19th century". Natural philosophers, including proponents of the corpuscular theory, have long recognized the virtue of the condition of independent predictive warrant. I will discuss at length the methodological writings of two defenders of the corpuscular theory in support of my claim. The first is David Brewster, whom Laudan does not discuss. The second natural philosopher is however Thomas Reid, Laudan's chief example of a strict inductivist. I will argue that Laudan is very selective in his presentation of Reid's methodological commitments. Reid was not as hard-headed about hypotheses as Laudan makes him. Moreover, Reid certainly accepted *a* criterion of independent warrant which is similar in structure to the condition of independent predictive warrant.

2. DAVID BREWSTER[4]

Brewster was a 19th century defender of the particulate theory of light who did not *accept* the wave theory of light:

[4]The treatment of Brewster given in this section follows on, and significantly modifies, that given by John Worrall (1990a)

> I have not yet ventured to kneel at the new shrine [that is, the shrine of the wave theory] and I must acknowledge myself subject to the national weakness which urges me to venerate, and even to support, the falling temple in which Newton once worshipped. (Brewster 1833, p.361).

Laudan maintains that corpuscularians were all inductivists who rejected the method of hypothesis and its condition of independent predictive warrant. Since Brewster was a corpuscularian who did not accept the wave theory of light, does this imply that he was a strict empiricist who rejected the method of hypothesis as Laudan would have us believe?

The first major problem for Laudan is that Brewster did not reject the method of hypothesis *tout court.* He took a very modest attitude to the method because *he merely rejected it as a method for inferring the truth of theories.* Indeed Brewster explicitly claims that:

> Twenty theories, indeed, may all enjoy the merit of accounting for a certain class of facts, provided that they have all contrived to interweave some common principle to which these facts are actually related. (Brewster, 1833b, p.360)

Brewster was also careful not to throw the baby out with the bath water: he did not claim that because a hypothesis can be contrived to suit the evidence, *all* hypotheses must be rejected. Because of this, he claimed that:

> I have long been an admirer of the singular power of this theory [i.e. the wave theory] to explain some of the most perplexing phenomena of optics; and the recent discoveries of Professor Airy, Mr. Hamilton and Mr. Lloyd afford the finest examples of the influence in predicting new phenomena. (Brewster, 1833, p.360)

In fact Brewster went as far as conceding that the predictive success of the wave theory indicates that:

> ... it must contain amongst its assumptions (though as a physical theory it may still be false) some principle which is inherent in, and inseparable from, the real producing cause of the phenomena of light ... . (Brewster, 1838, p.306)

But Brewster went on to explain why he *rejected* the wave theory. One of his reasons for rejecting the theory was due precisely to his cautious attitude to the method of hypothesis. Brewster, just like his contemporary wave theorists, recognized the fact that a theory, like the wave theory, which explained a wide range of phenomena may be false as a physical theory: "The power of a theory, however, to explain and predict facts, is by no means a test of its truth ...". (Brewster, 1833, p.360)

Brewster justified his disbelief in the physical truth of the wave theory by pointing out that the theory fails to give any viable explanation of the phenomena of dispersion and selective absorbtion. Indeed, just like wave theorists, Brewster acknowledged the significance of predictive success. But unlike the wave theorists, he refused to infer the truth of a predictively successful theory precisely because he believed that such a theory could nonetheless be false. Truth, Brewster believed, is underdetermined by predictive success.

There should be no controversy over Brewster's recognition of the virtues of the method of hypothesis. For Brewster recognized the difference between the 18th century usage of the method (i.e. the usage of Hartley and Lesage in which all that was required of a hypothesis was the ability to 'save the phenomena'-- even if in an ad hoc manner), and the 19th century usage (in which, according to Laudan, the condition of independent predictive warrant was adopted). In his already quoted 1838 review of Comte's *Course of Positive Philosophy*, Brewster was very clear about the condition of independent predictive warrant:

> ... when he who discovers new facts, detects also their relation to other phenomena, and when he is so fortunate as to determine the laws which they follow, and *to predict from these laws phenomena or results previously unknown, he entitles himself to a high place among the aristocracy of knowledge.* (Brewster, 1838, p.272, my emphasis)

As already mentioned, Brewster accepted that the wave theory was empirically successful in the sense that it predicted previously unknown phenomena, and he recognized that the theory was able to explain numerous previously known facts. But unlike his wave theory contemporaries, he was not convinced of the physical existence of "an ether, invisible, intangible, imponderable, inseparable from all bodies, and extending from our own eye to the remotest verge of the starry heavens." (Brewster, 1838, p.306)

The first major problem for Laudan is therefore that the condition of independent predictive warrant which he claims is exclusive to wave theorists is in fact not exclusive to them. Corpuscularians like Brewster also accepted this condition, hence the radical change in methodological requirements Laudan identifies in this historical episode is non-existent.

The second major problem for Laudan is that Brewster in fact explicitly claims that unobservable hypothesis and entities perform useful roles in experimental philosophy. Again in his review of Comte, Brewster is very clear on his stance about hypotheses.

First, Brewster agrees with Comte's rejection of "unrestrained" speculative hypotheses:

> Previous to the sixteenth century the active explorers of science were few in number, and even these few had scarcely thrown off the incubus of scholastic philosophy. Speculation unrestrained and licentious threw its blighting sirocco over the green pastures of knowledge, and prejudice and mysticism involved them in their exhalations. ... Those who are thus blind to the force of physical truth, are not likely to discover the errors which their own minds create and cherish. (Brewster, 1838, pp.272-273)

But Brewster went on to claim that although "a class of speculators have no position in the lists of science, and they deserve none ... in thus denouncing their labours, we must carefully distinguish them from a higher order of theorists, whose scientific acquirements are undoubted ..." (Brewster, 1838, p.273). Indeed Brewster criticized Comte for the "grave error" of not distinguishing between the justified and unjustified uses of hypotheses:

> ... we are strongly impressed with the conviction that our author [i.e. Comte] is but imperfectly acquainted with the recent acquisitions which science has made; and this opinion is confirmed by his repeated denunciations of the undulatory theory as an assumption utterly fantastical, and calculated only to check the progress of legitimate discovery. This grave error ... appears to originate from two causes - from his excluding all hypotheses as unscientific ... and from his not being aware of the actual power of the undulatory theory in *predicting* as well as in explaining phenomena. (Brewster, 1838, pp.305-306)

And Brewster went on to criticize Comte for failing to recognize three legitimate uses of hypotheses:

> The hypotheses which our author condemns may be arranged in three classes -

> those which serve no other purpose than that of an artificial memory to groupe and recall insulated facts; those which afford an explanation of facts otherwise unintelligible without making any assumption incompatible with our positive knowledge; and those to which this condition unite the still more important one of being able to *predict* new facts, and extend by real discoveries the bounds of our positive knowledge. The first of these classes of hypotheses is a very humble one; but even in its simple *mnemonic* character we are not disposed to reject its aid. Though it can neither *explain* nor *predict* phenomena, it may direct the enquirer, and even lead to discovery. ... The same observations are applicable *a fortiori* to the *second* class of hypotheses, and still more emphatically to the *third*, which claims the transcendent merit of predicting new phenomena. (Brewster, 1838, p.306)

Most significantly of all for Laudan's treatment of this historical episode is that Brewster also explicitly claims that the wave theory belongs to his third class of justified hypotheses, and that it "is a valuable instrument of discovery":

> ... Though the undulatory theory does assume an *ether*, invisible, intangible, imponderable, inseparable from all bodies, and extending from our eye to the remotest verge of the starry heavens; yet, as the expounder of phenomena the most complex, and otherwise inexplicable; and as the predicter of highly important facts, it must contain among its assumptions (though, as a physical theory, it may still be false) some principle which is inherent in, and inseparable from, the real producing cause of the phenomena of light; and to this extent it is worthy of our adoption as a valuable instrument of discovery, and of our admiration as an ingenious and fertile philosophical conception. (Brewster, 1838, p.306)

Brewster's full recognition and acceptance of the condition of independent predictive warrant hints at one fundamental problem with Laudan's reticulational analysis of this 19th century revolution. The problem is that Laudan constantly shifts between changes in explicit and implicit methodology. The evidence of radical change Laudan has is all evidence of change in explicit methodology. But he wants to argue for the stronger claim that there was a radical change in the real, implicit, revealed methodology.

Unfortunately if methodology is taken at the implicit level, at the level of the *real* principles which governed scientists' actual choices, there is no evidence of a radical change which accompanied the change in substantive ideas.

Laudan's claim "that the epistemology prevalent in the second half of the 18th century was *altogether incompatible* with the various ethereal theories which emerged in the natural philosophy of that period" is therefore unjustified. The two most significant differences Laudan identifies in these supposedly incompatible methodologies (namely, (a) the criterion of independent predictive warrant, and (b) the role of unobservable hypotheses) are not exclusive to wave theorists.

Of course Laudan could claim that Brewster was an exception. Brewster, Laudan could claim, simply failed to recognize that his acceptance of hypotheses and predictive success threatened the corpuscular theory. To close this avenue of retreat for Laudan, I will proceed to argue that Thomas Reid, whom Laudan has written extensively on as a *bona fide* Newtonian inductivist (hence, according to Laudan, a strict empiricist), is also not as hard-headed as Laudan makes him.[5]

[5]As already mentioned (fn.2 below) Laudan compares the methodology of 18th century corpuscularians with that of 19th century wave theorists. But Laudan also claims that the condition of independent predictive warrant "is a product of the early-19th century" (Laudan, 1981, p.130). So Laudan could object to my treatment of any 19th century corpuscularian by claiming that, just like their wave theory counterparts, they accepted the condition of independent predictive warrant. That is, Laudan could insist that only 18th century corpuscularians, but not 19th century corpuscularians, adopted Newtonian inductivism. I do not think this move is open to Laudan. First, Laudan himself seems to imply that 19th century Newtonianism was incompatible with 19th century versions of the method of hypothesis. As already mentioned (fn.2 below) G.N. Cantor (who also claims that there was a radical change in methodology associated with the change from particles to waves) compares the methodology of 19th century corpuscularians with the methodology of 19th century wave theorists. Laudan refer to Cantor's discussion of this part of the historical episode approvingly in a footnote: "I shall not discuss the first methodological debate which the wave theory provoked, namely, that between Young and Brougham [the second being the debate between Mill and Whewell]. I skip over it for two reasons: (1) it has already been investigated at length by G. Cantor in his "Henry Brougham and the Scottish methodological tradition", ... (2) it represents a more vituperative but less substantive replay of the earlier ether debates I have discussed with Brougham playing Reid to Young's Hartley". (Laudan, 1981b, p.184, fn.64) In short, in Laudan's view, 19th century corpuscularians such as Brougham and Brewster were very much strict Newtonian inductivists.

But we need not dally any longer on the question of whether Laudan is also committed to the view that

3. THOMAS REID

According to Laudan, "one of the foundation stones of Reid's philosophical system and the central attitude he adopted from Newton, was his suspicion of, bordering on contempt for, any theories, hypotheses, or conjectures which are not *induced* from experiments and observations". (Laudan, 1981a, p.89) In Laudan's opinion, Reid's interpretation of Newton's methodological requirement "amounts to the claim that any putative causal explanation (a) must be sufficient to explain the relevant appearances and (b) must postulate entities and mechanisms whose existence can be *directly* ascertained. ... What this amounts to is the claim that *unobservable entities*, because we can have no *direct* evidence of their existence, *have no role to play in causal explanations*. In Reid's hands, Newton's first rule of reasoning becomes a vehicle for excluding all theoretical entities from natural philosophy." (Laudan, 1981a, p.93)

We need to be clear about Laudan's full claim. Laudan mentions a distinction between a discredited *non-empirical* use of hypotheses, and an *empirical* usage. In its *non-empirical* usage, an hypothesis is any conjecture or theory which either: (i) can not be falsified or refuted by any experiment or empirical fact, or which (ii) though falsifiable, (and perhaps has been falsified), is insulated from rejection by its proponents on ad hoc grounds. The chief examples of this sort of non-empirical hypotheses are Descartes' seven laws of motion and the vortex theory. Despite the fact that these hypotheses were incompatible with empirical evidence, Cartesians still defended the truth of these hypotheses on *a priori* grounds. Indeed, Cartesians regarded their hypotheses as non-

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the method of 19th century corpuscularians was incompatible with the methodology of 19th century wave theorists. This is because 18th century Newtonians such as Reid provide ample difficulties for Laudan's claim of radical methodological change.

empirical in the sense that they were held as untestable against any empirical evidence. Laudan's suggestion is not merely that Reid rejected non-empirical hypotheses, but allowed empirical ones as legitimate in natural philosophy. On Laudan's reading of him, Reid rejected *all* types of hypotheses. This is why Laudan insists that Reid "maintained that a patient and methodical induction coupled with a scrupulous repudiation of all things hypothetical was the panacea for most of the ills besetting philosophy and science". (Laudan, 1981a, p.89)

But is Laudan's interpretation of Reid's methodological commitments correct? There can be no doubt that Laudan is correct in his claim that Reid was an empiricist for whom observation and empirical evidence were the prime criteria of appraisal. But it is also evident that Laudan takes too extreme an interpretation of Reid's empiricism. For Reid in fact explicitly concedes that hypotheses (empirical or non-empirical) have useful roles to play in natural philosophy. In one of his letters to Lord Kames, (dated 16th of December 1780), Reid is very categorical about this:

> I would discourage no man from conjecturing, *only I wish him not to take conjectures for knowledge, or to expect that others should do so.* Conjecturing may be a useful step even in natural philosophy. Thus, attending to such a phenomenon, I conjecture that it may be owing to such a cause. This may lead me to make the experiments or observations proper for discovering whether that is really the cause or not: and if I discover, either that it is or is not, my knowledge is improved; and my conjecture was a step to that improvement. *But, while I rest in my conjecture, my judgement remains in suspense, and all I can say is, it may be so, and it may be otherwise.* (Reid, 1872, pp.56-57, my emphasis)

Indeed, the italicized portions of this quotation is the key to the correct understanding of Reid's main discontent with hypotheses. Properly interpreted, Reid's attitude towards hypotheses is founded not so much on the fact that hypotheses transcend the phenomena. Rather it is based on Reid's belief that although the only sort of justification we can have for a hypothesis is probabilistic, proponents of specific hypotheses often advance their pet conjectures as indubitable truths:

> There is such proneness in men of genius to invent hypotheses, and in others to acquiesce in them, as the utmost which the human faculties can attain in philosophy, that it is of the last consequence to the progress of real knowledge, that men should have a clear and distinct understanding of the nature of hypotheses in philosophy, and of the regard that is due to them. Although some hypotheses may have a considerable degree of probability, yet *it is evidently in the nature of conjecture to be uncertain.* In every case the assent ought to be proportioned to the evidence; for *to believe firmly what has but a small degree of probability, is a manifest abuse of our understanding.* (Reid, 1872, p.235, my emphasis)

Reid believed that inventors of hypotheses have mostly abused hypotheses in this manner (i.e. the "assent" given to specific hypotheses is never "proportioned to the evidence"):

> The world has been so long befooled by hypotheses in all parts of philosophy, that it is of the utmost consequence to every man who would make any progress in real knowledge, to treat them with just contempt, as the reveries of vain and fanciful men, whose pride makes them conceive themselves able to unfold the mysteries of nature by the force of their genius. (Reid, 1872, p.236)

And Reid was so discontented with this abuse of hypotheses that he advised us to adopt the following as heuristics:

> Let us, therefore, lay down this as a fundamental principle in our inquiries into the structure of the mind and its operations - that no regard is due to the conjectures or hypotheses of philosophers, however ancient, however generally received. Let

> us accustom ourselves to try every opinion by the touchstone of fact and experience. What can fairly be deduced from facts duly observed or sufficiently attested, is genuine and pure; it is the voice of God, and no fiction of human imagination. (Reid, 1872 p.236)

But we should not be carried away by Reid's criticism of the illegitimate use of hypotheses into thinking that he is advising that we reject *all* hypotheses. In fact, in the already mentioned letter to Lord Kames, Reid sets out clearly what he considers to be the legitimate use of hypotheses:

> A cause that is conjectured ought to be such, that, if it really does exist, it will produce the effect. If it have not this quality, it hardly deserves the name of a conjecture. Supposing it have this quality, the question remains - whether does it exist or not? And this, being a question of fact, is to be tried by positive evidence. (Reid, 1872, p.57)

Reid's point is that it is not enough that a theory entail the evidence, for the theory to be acceptable. Just like Brewster, Reid required independent support, and he sets out his requirement of independent support as follows:

> All investigation of what we call the causes of natural phenomena may be reduced to this syllogism-
>
> *If such a cause exists, it will produce such a phenomena: but that cause does exist: Therefore, &c.* (Reid, 1872, p.57, my emphasis)

And he claims that:

> The first proposition [in the syllogism below] is merely hypothetical. And a man

> in his closet, without consulting nature, may make a thousand such propositions, and connect them into a system; but this is only a system of hypotheses, conjectures, or theories; and there cannot be one conclusion in natural philosophy drawn from it, *until he consults nature, and discovers whether the causes he has conjectured do really exist.* (Reid, 1872, p.57, my emphasis)

Reid did not merely pay lip service to his demand for independent support. He specifically criticized natural philosophers like Descartes for not providing independent support for their hypotheses:

> ... Des Cartes conjectured, that the planets are carried round the sun in a vortex of subtle matter. The cause here assigned is sufficient to produce the effect. It may, therefore be entitled to the name of a conjecture. But where is the evidence of the existence of such a vortex? If there be no evidence for it, even though there were none against it, it is a conjecture only, and ought to have no admittance into the chaste natural philosophy. (Reid, 1872, p.57)

Other than being able to account for the phenomena (or as Reid would have put it, other than being sufficient to produce the effect), a conjectured cause must also "be tried by positive evidence".

Laudan is therefore mistaken to insist that "what this [requirement of Reid's] amounts to is that unobservable entities, because we can have no direct evidence of their existence, have no role to play in causal explanations". (Laudan, 1981a, p.93) Even when there is no direct evidence for the existence of a conjectured cause, Reid is willing to allow them in causal explanations *as long as we do not turn any such unsupported conjectures into indubitable truths.*

To forestall one possible objection to my interpretation of Reid's methodological commitments, I will distinguish between two different attitudes Reid might have taken towards hypothesis:

1. hypotheses are heuristically useful in the sense that although they are not worthy of acceptance, they are sometimes useful aids to scientific understanding,

2. that hypotheses may be accepted in science but *only if* they have independent support.

These two attitudes are of course perfectly consistent. Hypotheses may be heuristically fruitful *even when* they have no independent support, but only accepted in science if they do. And indeed, as I shall argue shortly, I believe that Reid clearly held that both claims are true.

The foregoing makes it clear that Reid at least adopted the first attitude, and I think Reid adopted the second and strolnger attitude. My view is supported by Reid's distinction between the roles of the ether in Newton and Hartley's writings. In Reid's opinion, Newton's conjecture of the ether as the cause of gravitation is a good example of the legitimate use of hypotheses, while Hartley's commitment to the ether is the prime example of the abuse of hypotheses. According to Reid:

> Sir Isaac Newton, in all his philosophical writings, took great care to distinguish his doctrines, which he pretended to prove by just induction, from his conjectures, which were to stand or fall as future experiments and observations should establish or refute them. His conjectures he has put in the form of queries, that they might not be regarded as truths, but be inquired into, and determined according to the evidence to be found against them. Those who mistake his queries for a part of his

> doctrine, do him great injustice, and degrade him to the rank of the common herd of philosophers, who have in all ages adulterated philosophy by mixing conjecture with truth, and their own fancies with the oracles of nature. (Reid, 1872, p.249)

Reid went on to cite Newton's conjecture of the ether as the cause of gravitation as a legitimate use of hypotheses. He also claimed that in Hartley's hands, Newton's ether was illegitimately used. Hartley's use was illegitimate not because Hartley's ether was unobservable (while Newton's ether was observable!); rather it was illegitimate because although Hartley had no justifiable empirical evidence for postulating the ether, he nonetheless took the ether to be the *true cause* of neuro-psychological phenomena:

> As to the vibrations and vibratiuncles, whether of an elastic aether, or of the infinitesimal particles of the brain and nerves, there may be such things for all we know; and men may rationally inquire whether they can find evidence of their existence; but while we have no *proof* of their existence, to apply them to the solution of phenomena, and to build a system upon them, is what I conceive we call building a castle in the air. (Reid, 1872, p.250, my emphasis)

And Reid continues:

> If ... we regard the authority of Sir Isaac Newton, we ought to hold the existence of such an aether as a matter not established by proof, but to be examined into by experiments; and I have never heard that, since his time, any new evidence has been found of its existence. "But", says Dr. Hartley, "supposing the existence of the aether, and of its properties, to be destitute of all direct evidence, still if it serves to account for a great variety of phenomena, it will have an indirect evidence in its favour by this means." There never was an hypothesis invented by an ingenious man which has not this evidence in its favour. The vortices of Des Cartes, the sylphs and gnomes of Mr. Pope, serve to account for a great variety of phenomena. (Reid, 1872, p.250)

The problem with Hartley's use of the ether is that it takes the mere fact that a hypothesis succeeds in saving the phenomena as evidence for the truth of that hypothesis. Reid however demanded more than this. In Reid's methodology, before a conjecture can be regarded as true, it must have independent empirical support. Failing this, the conjecture could still be adopted in explanations as long as it is properly recognized as a *conjecture*: a hypothetical claim which should "not be received as truth..., but be inquired into, and determined according to the evidence to be found for or against [it]". (Reid, 1872, p.249)

The cases of Brewster and Reid, when properly analyzed, support (though of course do not establish) the view that there was no *radical* change in scientific methodology which accompanied the change in substantive beliefs. Newtonian inductivism and the method of hypothesis were, at least, not as radically different as Laudan would have us believe. One main similarity between these two methodologies is that both allowed the use of hypotheses about unobservables. Secondly, both methodologies had their respective conditions of independent warrant. So, the only radical change there was accompanying change in substantive ideas was in explicit methodology.

The fundamental problem with Laudan's interpretation of this episode is that he wants to claim more than he justifiably can on the basis of the evidence he has. An alternative interpretation of the methodological dispute and differences between corpuscularians and wave theorists can be given.

Worrall [1990a], for instance, argues that the disagreement between David Brewster and the wave theorists was chiefly over "the way forward". As shown below, Brewster, just like the wave theorists accepted that the predictive and empirical success of a theory counts favourably in its support. Brewster further argued that the then available versions of the wave theory could not give any adequate account of dispersion and selective absorption. Brewster, however, felt that this was not a problem confronting only the then available versions of the wave theory. His full view was that there are reasons to think that any version of the wave theory would be unable to explain these phenomena. And as the corpuscular theory was then the only viable and serious alternative to the wave theory, Brewster decided to continue working on this alternative.

Proponents of the wave theory of course accepted Brewster's point that their current versions of the theory could not account for these phenomena. But they also pointed out that the corpuscular theory could not give any good explanation of these phenomena. Moreover, they maintained that the wave theory was empirically more successful than the corpuscular theory. So, they regarded the wave theory as providing the best way forward.

Brewster had no justified counter-argument to the wave theorists' claim that the corpuscular theory was the less successful theory. All Brewster could do was to nurse the hope that the corpuscular programme would eventually stage a comeback.

Can we generalize the disagreement between Brewster and the wave theorists? I think we can. The debate between corpuscularians (not just Brewster) and wave theorists seem to be about the way forward. As I have argued below, the condition of independent support which Laudan claims is exclusive to the wave theorists was in fact accepted by corpuscularians like Reid. Further historical research might well reveal that such a generalization is justified. But that is not the subject of this dissertation. It is sufficient for this present work to show that key "inductivists"-- amongst them Laudan's chief example, Reid-- also accepted the condition of independent empirical support.

4. Relativism and Reticulational Reconstructions

I have just shown that Laudan's reconstruction of the 19th century revolution in optics is suspect. But suppose, for the sake of argument, that we accept it. Can the reticulated view of scientific rationality provide any viable explanation of the "revolution" thus described? My view is that, even if Laudan's reticulational account is taken at its face value, it fails to exhibit the rationality and progress of science.

According to Laudan's reticulational view, scientific change, including eventually radical change, occurs piecemeal. For instance, when there is a change at the theoretical level, there is no change at the methodological and axiological levels as these two levels provide the arbiter for change at the theoretical level. Rationality is exhibited within the reticulated account only if certain aspects of scientific commitment remain temporarily fixed and so provide legitimation for a changing element.

In his reticulational reconstruction of the optical revolution, Laudan specifically identifies theoretical commitment as the first changing element.

> ... the emergence of the optical ether in the early 19th century prompted a more radical critique of classical epistemology, a critique that produced some innovative and historically influential methodological ideas. (Laudan, 1981b, pp.157-158)

But herein lies the problem for Laudan's reticulational reconstruction of this 19th century revolution. John Worrall states the problem succinctly as follows:

> ... if Newtonian inductivism really were in force at the time Fresnel developed the wave theory (and *if* any Newtonian inductivism really does ban genuinely theoretical entities) then the acceptance of that theory by the scientific community could not have been rational. Conversely, of course, if the initial acceptance of the wave theory was rational, then Newtonian inductivism (as described by Laudan) was not really in force at the time. In neither case can there have been a real shift in methodology which can be explained as the rational response to the prior acceptance of the wave theory. (Worrall, 1988, p.266)

This is because if change first occurred in theoretical commitment, the reticulational view will require the unchanging elements (i.e. methodology and axiology) to provide the justification and rational for the changing element. But the Newtonian inductivist methodology Laudan describes cannot provide the rationale for the change to an unobservable! So, no piecemeal, and at the same time *rational*, reconstruction of this change can be run along reticulational lines. Conversely, if the initial acceptance of the wave theory was rational, then surely inductivism couldn't have been the methodology that was *truly* in force.

My claim is that a reticulational explanation of the 19th century revolution in optics surreptitiously precludes rationality. Laudan claims that there was a change from a set_1 $\{T_1\&M_1\&A_1\}$ (i.e. the corpuscular theory and its associated Newtonian methodological and axiological requirements) to another set_2 $\{T_2\&M_2\&A_2\}$ (i.e. the wave theory and its associated methodology and axiology). While the holist sees a *revolutionary change in world view* from set_1 to set_2, Laudan sees a more gradual type of change.

In Laudan's more gradual picture of step-by-step changes that eventually result in the change from set_1 $(T_1\&M_1\&A_1)$ to set_2 $(T_2\&M_2\&A_2)$, he explicitly identifies the theoretical component (i.e T_1) of set_1 as the first component to change. Hence, the first stage in the change form set_1 to set_2 was the change from (T_1&M_1&A_1) to (T_2&M_1&A_1). Proponents of the reticulational view would therefore have to claim the following:

> T_2 was preferred to T_1 while M_1 and A_1 were still in force. But as M_1 was incompatible with T_2 ("the epistemology prevalent in the second half of the 18th century was altogether incompatible with the various ethereal theories which emerged in the natural philosophy of that period" [Laudan, 1981b, p.157]), proponents of T_2 had to look for an alternative methodology-- namely, M_2. ("... proponents of ethereal explanations chose to abandon or modify that prevailing epistemology so as to provide a philosophical justification for theorizing about the ether" [Laudan, 1981, p.157]).

How can a theory which is "altogether incompatible" with the prescriptions of a methodology be adjudged better by the very methodology with which it is incompatible? This problem arises because Laudan's reticulation requires that the unchanging elements of a set (T&M&A) provide the arbiter for the changing element. Unfortunately, as far as this particular historical episode is concerned, if we accept the reticulational account, then it is impossible for M_1 to rank T_2 over T_1. M_1 is incompatible with T_2, but Laudan cannot run a reticulational reconstruction of this episode unless M_1 is able to adjudge T_2 the better theory.

But supposing we grant or overlook (again for the sake of argument) the impossible move from (T_1&M_1&A_1) to (T_2&M_1&A_1). The next step in Laudan's reticulation is equally problematic. In the second step, we have a change from (T_2&M_1&A_1) to (T_2&M_2&A_1). In this step, the change is from M_1 to M_2, while T_2 and A_1 justify this change. But how can T_2 rationally constrain the change from M_1 to M_2 in step 2 if M_1 has already ranked T_2 over T_1 in step 1? There is no way we can run a reticulational reconstruction of this historical episode as Laudan believes.

Of course, as Laudan now claims that he is not interested in explaining the past choices of scientists as rational or irrational, he could claim that his view of scientific change merely accounts for scientific progress. This, however, would leave the problem entirely untouched because such a view cannot give any genuine explanation of *progress* in science either. We all agree that progress presupposes change. Progress presupposes that we can talk of transitions from a theory T_1 to a theory T_2, and so on. What distinguishes *progress* from mere *change*, however, is the verdict that T_2 is in some objective sense *better* than theory T_1, and that the acceptance of T_2 is based on *good reasons*. For instance, if a scientist were to accept an objectively better theory for the wrong reason (e.g. if a scientist were to accept Einstein's theory of relativist simply because of the ethnic origins of Einstein, *and* for no other reason), that scientist's choice would not have been rational. And by the same token, the choice cannot properly be regarded as progressive.

But Laudan's account of scientific progress cannot deliver the judgement that the acceptance of theory T_2 (e.g the wave theory) over theory T_1 (e.g the corpuscular theory) is based on *good reasons*. And if change is not a result of good objective reasons, such change cannot constitute progress.

There is a further problem with Laudan's reticulational reconstruction of this historical episode. Laudan explicitly claims that in his reticulational view, rationality is preserved by allowing change in only one component at a time ("If a scientist's methodology fails to justify his ontology; if his methodology fails to promote his aims; if his cognitive aims prove to be utopian ... the scientist will have compelling reasons for replacing one component or other of his world view with an element that does the better job. Yet he need not modify everything else. [Laudan, 1984, p.74].") He however sometimes give the impression that the first step in the piecemeal change involved a concurrent change in both axiology and theory:

> Clearly, what confronted all these scientists [i.e David Hartley, George Lesage, and Roger Boscovich] was a manifest conflict between "official" aims and goals of science and the types of theories they were constructing. The choice was a difficult one: either abandon microtheorizing altogether ... or else develop an alternative axiology of science which would provide conceptual legitimation for theories lacking a direct observational warrant. ... But they realized that such a goal made no sense in the absence of methods warranting claims about unobservable entities. Thus to make good their proposed aims, they had to develop an alternative methodology for science. (Laudan, 1984, p.57)

If Laudan's full view is that there were radical concurrent changes in both axiology and theory, then, surely the reticulational view is very close to the Kuhnian holistic view. Indeed, I would argue that it is too close to holism for comfort.

As I argued in chapter 3 (section 2) below, Laudan does not provide us with any adequate definition or characterization of axiological values. His best characterization is that "an attribute will count as a cognitive value or aim if that attribute represents a property of theories which we deem to be constitutive of good science". (Laudan, 1984, p.xii, fn2) But as I also argued below, this definition turns all methodological rules into axiological goals as well. Indeed, this characterization of axiology seems to suggest that methodological commitments are implicit in axiological goals. Hence, a commitment to a given set of axiological goals would at the same time be a commitment to a given set of methodological standards.

But if any given set of axiological commitments has its associated set of methodological commitments, and hence the change that occurred in 19th century optics involved concurrent changes in both axiology and methodology, then the reticulational account of the 19th century revolution in optics is not significantly different from the account a holist would give. Surely we can turn Laudan's criticism of holism back on his own position:

> When scientific change is construed so globally, it is no small challenge to see how it could be other than a conversion experience. If different scientists not only espouse different theories but also subscribe to different standards of appraisal and ground those standards in different and conflicting systems of cognitive goals, then it is difficult indeed to imagine that scientific change could be other than whimsical change of style or taste. (Laudan, 1984, p.72)

5. CONCLUSIONS

In this chapter I examined one historical episode advanced by Laudan as an example of *radical* change in methodology. Contrary to Laudan's claim that this "case stands as a vivid refutation of [the] claim that scientific standards of theory evaluation are immutable" (Laudan, 1981b, p.181), my examination of the episode establishes the following:

1. Laudan's treatment of the change from particles to waves succeeds, at best, only in showing that there were changes in scientists *explicit* characterizations of their methodological commitments - not in the implicit methodology which really guided their views. But radical change in explicit methodological pronouncements alone provides no challenge to the traditional approach which is exclusively concerned with the invariance of implicit methodological commitments.

2. There is a distinction to be made between "narrow" and "broad" construals of the flexible notion of "methodology". Although these two notions are both capable of degrees such that some methodological rules are broader than (or narrower than) others, clear cut particular examples of each can be given. "Don't accept theories before testing them against their plausible rivals" is a *narrow* methodological rule, as is "other things being equal, prefer theories that have made successful predictions". But rules such as: "prefer wave explanations in optics", "look for deterministic theories", or "prefer 'compositional' explanations to 'perfectionist' ones in chemistry" are examples of *broad* methodology. Of course the sorts of theories scientists looked for in optics after 1830 tended to be wave theories and thus there is no doubt that once the particulate theory was rejected and the wave theory accepted in its stead, scientific explanations were cast in terms of the broad methodological requirements of the prevalent theory. But change in broad methodology provides no real treat to the traditional approach which is concerned with methodology narrowly conceived as *the basic principles of scientific theory appraisal.*

3. Although Laudan gives the impression that he is concerned with change in methodology narrowly conceived, his analysis fails to provide any viable support to a "no-invariant-*narrow*-methodology view". His claim is that predictive success was not part of 18th century methodology and that scientists only started to invoke this in the early 19th century as a result of the triumph of the wave theory. But in fact, the narrow methodological requirements of predictive success Laudan associates with the wave theory is by no means exclusive to wave theorists. Proponents of the particulate theory also accept the virtues of this methodological requirement. Significantly, Thomas Reid, Laudan's own main example of a Newtonian inductivist, accepts the virtues of this narrow methodological requirements. (The claim that this was a dispute about physics conducted against the background of a neutral and generally accepted set of methodological standards has been argued independently by Achienstein (1991). Achienstein argues for a particular *fixed* methodology - one involving a requirement of "independent warrant". Achienstein's methodological views raise problems of their own and, since I am only concerned to argue that Laudan has provided no evidence that narrow methodological standards changed, I have not gone into Achinstein's views.)

4. The methodological debate that actually ensued between proponents of the particulate theory and defenders of the wave theory was really about *the way forward.* Proponents of the particulate theory such as Brewster argued that the theory might, or even would, eventually stage a comeback and be more successful that its wave rival. But defenders of the wave theory maintained that the wave theory had amassed enough successes to justify pursuing it exclusively in further theoretical investigations in optics.

5. Although I showed that Laudan's reconstruction of this historical episode is suspect, I further argued that even if (for the sake of argument) we accept it, the reticulational version of methodological naturalism provides no rational explanation of the change from particles to waves. This is because Laudan's reticulation specifies that the change from particles to waves was piecemeal. In particular, Laudan's account specifies that while there was a radical change in theoretical commitments (i.e. from the particle theory to the wave theory) there was no corresponding change in methodological and axiological commitments. (These two elements were, according to Laudan, also changed in a piecemeal fashion at a later stage.) The axiology and methodology (which are the two unchanging elements in this first move) thereby provided the rationale and justification for radical change in theoretical commitment. But herein lies the problem. If Newtonian inductivism was *really* the methodology in force at the time of the acceptance of the wave theory, then the change could not have been rational. For how can a Newtonian inductivism which (according to Laudan) is "altogether incompatible" with the formulation of theoretical entities such as the lumeniferous ether adjudge the wave theory better - when on Laudan's own account that theory gives a central role to the lumeniferous ether?

The point is simply that if Newtonianism was really in force at the time of the acceptance of the wave theory, then that acceptance couldn't have been justifiable. But if Newtonian inductivism was not really the methodology in force when the wave theory was accepted, then we cannot rightly claim that there was a *radical* change in *real implicit methodology*. Scientists must have been operating with a methodology quite different for the Newtonian inductivism described by Laudan. Whichever of these options is accepted, the conclusion remains the same: the 19th century revolution in optics provides no viable support to the historical claim of the no-invariant-methodology thesis.

CHAPTER 5

General Conclusions: The Thesis Set in Context

In this thesis, I have examined in detail two versions of the naturalist view - due to Laudan and Shapere - that methodological standards of theory appraisal are components of science like any other, and as such are subject to radical change as science develops. This naturalist view challenges the traditional view (upheld by Popper, Carnap, and others) according to which the objectivity and rationality of science can only be exhibited if the principles for the objective ranking of theories are themselves ahistorical, or at any rate, not dependent upon any specific substantive scientific claim for their validity.

Traditionalists like Carnap and Popper held that there is a logic of empirical support on the basis of which theories are appraised objectively in accordance with the evidence. This tradition held that the rationality of the development of science can only be exhibited if a new theory can be shown to be better empirically supported (or, in Popperian terms, better "corroborted") than the old theory: switching to the new theory is rational only if the new theory is better supported by the evidence. Such judgments, according to the traditionalist, required that the principles of theory appraisal are themselves not subject to change. Principles of appraisal are therefore the unjudged judges which provide the basis for the objective and rational evaluation of the switch to a new theory. If methods of appraisal were not independent of the content and context of the theories they are meant to appraise (so the traditionalist insists), we would not be able to account for the progress of science. All that we would be able to say is that a given context (or theory) judges itself better than its rival.

The naturalist, however, insists that the traditional view of an ahistorical method, which, once discovered (or invented) is immutable, unchanging, and not influenced by substantive developments within science is historically and philosophically untenable. The naturalists' discontent with the traditional approach stems from the fact that the traditionalist adopts an a priorist view of methodology. The most explicit characterization of a priori knowledge is that of Kant's: "we shall understand as a priori knowledge, not knowledge which is independent of this or that experience, but knowledge absolutely independent of all experience". (Kant, 1781, B2-3) But if disputes in methodology arise (as naturalists insist they do) how could they be adjudicated if these principles of methodology are a priori?

The naturalist questions the two central assumptions of the traditionalist, viz: (i) the assumption that methodology is actually independent of substantive science; and (ii) the assumption that such independence is necessary for the rationality and objectivity of science. The main virtue of naturalism is that it shows that traditionalists have overlooked (or at any rate not laid sufficient emphasis on) one important sense in which science has learnt from, and built on, its substantive discoveries. If methodology is interpreted broadly, there is no doubt that we have acquired new rules, principles, and standards of appraisal as a result of changes in our substantive beliefs about the world. The switch to double-blind from single-blind clinical trials illustrates the naturalists' point very well. There is no doubt that the discovery of secondary placebo effects (i.e. that in single-blind trials, not only might experimenters still convey their own therapeutic expectations to patients, experimenters' expectations might also affect their own judgements of whether a patient has benefitted from treatment) informed the discovery of a new methodological standard (namely, double-blind methodology). This new methodology stands or falls with the secondary placebo effect. Its validity and adequacy is not independent of substantive developments within science. The naturalist is therefore correct in claiming that the traditional stance that methodology is ahistorical, invariant, and presuppositionless, or at any rate, not dependent upon any specific substantive scientific knowledge, is - when methodology is considered in this broad sense - insensitive to the developments within science.

Nonetheless, my thesis has been that while I agree with the naturalist that traditional a priorists' accounts have often been insensitive to the history of science, naturalists like Shapere and Laudan have over-reacted. In defending the view that methods of evaluation have changed in response to new substantive information, Laudan and Shapere have gone too far. Not *all* rules of evaluation can be up for grabs if the progress of science is to be objectively and rationally explained. For if all the methods, standards, rules of reasoning, and indeed everything in science is subject to possible radical change, then when two competing theories are *truly* radically different, it would be impossible to evaluate and choose (rationally) between them.

Shapere and Laudan are, as I explained, fully aware of the threat of relativism. Indeed they both set themselves the task of giving an account of scientific change in which nothing is invariant or ahistorical, but in which, an objective ranking of rival (and radically different) theories can be given. I have argued in this thesis that, despite their best efforts, Laudan and Shapere have failed. They have failed not because naturalism, considered as the simple thesis that philosophers and methodologists must pay close attention to the history and workings of science, is itself hopeless or wrongheaded. On the contrary, this thesis is to be applauded. Rather, they have failed because they have taken their view too far. Methodology considered broadly is no doubt highly sensitive to substantive developments within science, yet a core of principles and rules of appraisal must be invariant if the progress and rationality of science is to be preserved.

Shapere and Laudan both explicitly claim to "naturalise" the whole of methodology - *no* principle is sacrosanct, *all* are at any rate potentially open to revision in the light of developments in science. However attractive this idea might be - not least in seeming to avoid the traditional problems associated with the validation of a priori methodology - I have shown in this thesis that it is untenable: *if taken seriously it collapses into relativism, if it avoids relativism it implicity appeals to a core of a priori, non-naturalised, principles.*

In defence of my position, I noted that even in Shapere and Laudan's respective views, the rationality and progress of science is preserved if and only if certain core aspects of their views of scientific rationality are taken to be invariant. Thus in Shapere's view, the process of exhibiting chain-of-reasoning connections between radically different domains of inquiry is a process which *all* radical (but rational) changes must exhibit. Hence in Shapere's view, the acceptance of a new set of methodological standards in favour of an old one is rational only if we can trace chain-of-reasoning connections. But suppose we pose a second order question about the process of exhibiting chain-of-reasoning connections. *Would radical change remain rational if the notion of what counts (formally) as a "chain-of-reasoning connection" were itself up for grabs, subject to change as science develops and different from context to context?* Evidently not. For although Shapere maintains explicitly that all methodological constraints are subject to possible radical change, he takes great care to argue that he avoids Kuhnian relativism because of the role of this process of a "chain of reasoning" in effecting, or adjudicating radical change. Shapere avoids relativism only because he is himself committed to the view that, even in cases of apparent radical methodological change, there is a deeper level of connections that in fact explain such change as a proper chain-of-reasoning.

As I have indicated in the body of the thesis (sections 4 and 5 of chapter 3) Laudan's view also appears to give a rational explanation of change in science only in so far as some of its tools of evaluation are implicitly assumed to be invariant. (This again despite explicit claims that on his position *all* methodological prinicples are at least potentially criticisable and changeable as science "progresses".) For instance, the requirement that the cognitive goals of science be non-utopian, and the inductive requirement which urges us to continue adopting those means which have hitherto been successful in bringing certain aims of science into fruition. Laudan himself unwittingly concedes that these tools of evaluation are at the core of rationality, and are, if objectivity is to be maintained, as such invariant:

> ... it is at the very core of our conception of the rational and the reasonable that anything judged as satisfying that family of concepts must, in appropriate senses, be thought to be both possible and actionable. To adopt a goal with the feature that we can conceive of no action that would be apt to promote it, or a goal whose realization we could not recognize even if we had achieved it, is surely a mark of unreasonableness and irrationality. (Laudan, 1984, p.51)

I showed in chapter 3 that Laudan's convincing (and undoubtedly important) cases of change in methodology - such as the switch to "double-blind methodology" in the clinical trials of drugs - are all cases of change in "broad methodology". And I showed that such changes can be explained as rational only if they can be exhibited to consist of "plugging " new substantive discoveries (in this case, for example, the discovery that clinicials expectations might affect outcome) into core and fixed principles (in this case, for example, the principle that theories should always be tested against plausible rivals). The one case that Laudan cites of a radical change in methodology that cannot be dealt with in this way, the one case that seems clearly to be one of (an alleged) change in "narrow" core methodology, is his case of the change from inductivist to hypothetico-deductivist methodology which Laudan sees as an accompaniement of the early 19th Century revolution in optics. This is why I devoted chapter 4 to a detailed consideration of this particular case.

In the very broadest sense of methodology there was of course a change in optics in the early 19th century: 18th century theorists had looked for ("preferred") particulate explanations of the optical phenomena, while after the "revolution" scientists looked for ("preferred") wave theories. But Laudan argues the much more striking claim that the requirement that a theory be predictively successful in order to be accepted in science was not part of methodology in the 18th Century and came to be so only in tandem with the acceptance of the wave theory in the early part of the 19th. This looks like a change in "narrow" or "core" methodology. In chapter 4, I argued two things. First that Laudan's claim to explain the rationale for the wave "revolution" in a "reticulated" way that allows change at the same time in methodology from inductivism to hypothetico-deductivism in fact fails. And secondly I argue that, historically speaking, there is no evidence for, and some evidence against, Laudan's claim. Once we make the distinction between explicit methodology (the sort of claims scientists explicitly make about the way they conduct their science) and implicit methodology (the principles and standards of appraisal that really govern their work), then there is at best evidence of some change in explicit methodological pronouncements associated with the switch to the wave theory. I argue that 18th century theorists too accepted that theories are better if they are predictively successful. (Achinstein too in his (1990) treatment came to the same conclusion: that the particle-wave debate was an argument over physics and not over methodology.)

Let me emphasise that I do not in this thesis argue against the naturalist claim that some methodological rules (even core ones) in a way carry substantive content about the world. Even "traditionalists" when forced into a corner tended to admit this. For example the preference for "unified", "simple" theories of the world and a corresponding suspicion of theories that make "ad hoc exceptions" will only work (that is, only lead toward "truer" theories) in a world which is governed by unified simple laws. But as Imre Lakatos used to say, it is perfectly possible that God drew up a blueprint for the universe which made it Einsteinian, except for 17 exceptions. Our methodology commits us to ignoring this possibility and hence to making a very general, but nonetheless substantive, assumption about the universe. I have not challenged the idea that some methodological principles may be *substantive* ('synthetic") in this manner. What I have challenged is the idea that *all* can be considered corrigible in the light of developments within science, without thereby sacrificing rationality.

Finally, let me emphasise that my conclusions apply directly only to the positions of Shapere and of Laudan, since these are the only "naturalistic" positions that I have considered. There are of course plenty of other defences of naturalism in philosophy of science (and in epistemology generally) and I make no claim that my strictures apply directly to any of these. I selected the positions of Laudan and Shapere for special study, however, because those positions do seem to me to be both exceptionally well articulated and thought-through, and exceptionally sensitive to and well-informed about the actual procedures of science. Moreover, it does seem to me that the quite general criticisms that I bring to bear on these positions do present important obstacles for any attempt fully to naturalise philosophy of science. I do not see how the naturalist can avoid the following dilemma: either she claims that all methodological principles are subject to criticism and possible change as science changes and then fails to explain the development of science as rational, or she gives herself the chance to explain the development of science as rational by holding that at least a core of methodological principles are sacrosanct - but then she faces the "old" problems of justification with respect to this core. At any rate, my argument that these two leading attempts to naturalise philosophy of science fail to avoid this dilemma, puts the ball back in the naturalist's court.

Appendix

Relativism Defined

What exactly does the "relativism" that I have accused Laudan and Shapere of failing to avoid involve? Although the main thrust of the relativism I discuss is clear - namely a denial of any absolute or objective sense in which theories can be said to have improved over time - the charge of relativism can be understood in a variety of senses and some clarification of the precise sense of it I adopt is in order here.

A distinction can be made between at least three different senses of relativism:

Relativism$_1$: Relativism$_1$ is the thesis that there are no truths over and above those relative to a framework. Newton-Smith characterizes *relativism of truth* as follows: "something, *s* is true for Ψ and *s* is false for ϕ." (Newton-Smith, 1982, p.107)

Relativism$_2$: This is the thesis that there are no reasons for accepting or preferring a claim or belief *p* to not-*p*, but only reasons for accepting or preferring relative to a framework. Newton-Smith has also characterized the *relativism of reason* as follows: "*R* is a reason for holding that *p* is true for ψ while *R* is not a reason for holding *p* to be true for ϕ". (Newton-Smith, 1982, p.110)

Relativism$_3$: This is the version of relativism discussed in this thesis. Relativism$_3$ (which I describe as *methodological relativism*, or *MR* for short) is a specific version of what others have described as epistemological relativism. Harvey Siegel describes epistemological relativism (*ER*) as follows:

> For any knowledge-claim *p*, *p* can be evaluated (assessed, established, etc.) only according to (with reference to) one or another set of background principles and standards of evaluation $s_1,...,s_n$; and, given a different set (or sets) of background principles and standards $s_{1'},...,s_n$ there is no neutral (that is, neutral with respect to the two (or more) alternative set of principles and standards) way of choosing between the two (or more) alternative sets in evaluating *p* with respect to ... rational justification. *p*'s ... rational justifiability is relative to standards used in evaluating *p*. (Siegel, 1987, p.6)

MR is a version of *ER* because while *ER* is concerned with the justification of knowledge claims writ large, *MR* only asserts a thesis about the epistemological status of scientific theories and the rules and principles for the appraisal of these theories. More specifically, *MR* is the thesis that the justification or rational adequacy of scientific theories are assessable only in terms of a given set of background methodological principles and standards - principles and standards which are themselves adjudged in the light of their associated research traditions or historical epochs.

MR as I have used it in this dissertation makes a two-fold claim. It makes a claim about the evaluation of scientific theories on the one hand, and on the other hand, it makes a claim about the adequacy of the methodological rules for the appraisal of scientific theories. The full thesis of *MR* is therefore that the adequacy of scientific theories can only be assessed by methodological rules and principles that are unique to each historical epoch or research tradition, and that these methodological rules and principles in turn derive their acceptability or adequacy from the substantive content of the sciences within which they are to operate. That is, the methods which are the instruments for the evaluation of scientific theories are themselves assessed by the changing content and context of science.

But what exactly is the problem with relativism? Why should the adoption of a relativistic process of appraisal be a vice? One main objection - perhaps *the* main traditional objection - is that relativism is incoherent. One version of the charge, for instance maintains that the notions of truth and reason are intricately linked with a traditional account of meaning, translation and truth-conditions. On this traditional account, if two sentences *p* and *p*' have the same meaning, then *p* must be true in virtue of whatever makes *p*' true. So to claim that a sentence *p* (which has the same meaning as *p*') can be true while *p*' is false would lead to incoherence.

Of course there are various counter-arguments the relativist could give to the charge of incoherence. The relativist could for instance exempt relativism from applying to itself by claiming that the truth or justifications of all statements, claims, theories, methods, etc, (*with the sole exemption of the general thesis of relativism*) are relative to specific backgrounds of assumption. Alternatively, the relativist could protest that the traditional notions of "rightness" and "good reasons" implicitly assume the very absolutist conceptions he seeks to reject.

The main problem with *MR* is not so much that it may be incoherent. Rather, the problem is that it cannot fully account for the progress of science. The empirical and predictive success of theories in the mature sciences seem to provide a prima facie argument for the view that there has been cognitive improvement in our scientific understanding of the natural world. The relativism I am concerned with is that which claims that this prima facie argument for the cognitive progress of our scientific knowledge fails. It claims that we can only say that our present theories are better relative to the standards that scientists at present happen to adopt, and that as a result of this, we cannot claim that a theory such as the photon theory is objectively superior to the corpuscular theory.

Although the problem of relativism features a lot in my discussion, my aims did not include that of *refuting relativism.* My task was simply the following. As a result of the publication of Kuhn's *The Structure of Scientific Revolutions*, various philosophers of science (including Laudan and Shapere) have argued that any view of science in which the adequacy of theories are assessed in light of methods and standards which are themselves subject to radical change (and are justifiable only during the historical epoch in which they are produced) has the problem of relativism to overcome. More specifically, both Shapere and Laudan argue that a view such as Kuhn's is unacceptable because it can't say why new theories are objectively superior to their extant rivals.

In the first chapter of this dissertation, I outlined the arguments in support of the view that defending the no-invariant-methodology thesis inevitably leads to relativism. In particular, I showed that naturalists like Laudan and Shapere also accept that at least some versions of the no-invariant-methodology thesis have the problem of relativism to overcome. The two nonetheless claim that their respective naturalistic versions of the no-invariant-methodology thesis avoid the pitfalls of relativism.

What I have argued is that despite their best efforts, these two proponents of methodological naturalism have not delivered the best of both worlds: methodological naturalism does not succeed in avoiding relativism while at the same time it genuinely accepts "the radical change in methodology" view of the Kuhnians. Via a detailed assessment of the views of Shapere and Laudan, I argued that naturalists give the impression of overcoming relativism only because they implicitly assume some absolute, theory and context neutral, set of evaluative standards. I identified these implicitly assumed standards, and confront Laudan and Shapere with a dilemma: either they allow radical change to extend to these implicitly assumed standards, in which case the adequacy of these standards would be dependent upon the specific epoch and context within which they are formulated (and naturalists would end up with the sort of relativism they themselves accuse Kuhnians of espousing); or naturalist come out clean by accepting these standards as absolute, or at least invariant and neutral. Whichever arm of the dilemma they chose, naturalist would have failed in their self-assigned task of giving "an account of the knowledge-seeking and knowledge-acquiring enterprise ... which, while not relying on any form of the Inviolability thesis, will not also collapse into relativism". (Shapere, 1984, p.xxi)

No doubt a proponent of the no-invariant-methodology thesis who fully accepts the relativistic conclusions of his naturalism could bite the bullet and proclaim relativism virtuous. But the subject of my concern in this dissertation is not those versions of the no-invariant-methodological thesis that positively defend and endorse relativism. My target is those versions of methodological naturalism that explicitly reject, and claim to avoid, relativism. What I have argued in this thesis is that in so far as the no-invariant-methodology thesis is fully accepted, naturalism ultimately fails to overcome the problem of relativism.

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