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A Critique of Methodological Naturalism

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Argument

Larry Laudan defends "methodological naturalism" – the position that scientific methodology can be fully empirical and be subject to radical change without sacrificing the rationality of science. This view has two main components: (a) the historical claim that 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 in science changed in response to new information about the world; and (b) the philosophical claim that all aspects of science are in principle subject to radical change and evolution in the light of new information about the world. In this paper, I argue that one main historical example used by Laudan, namely, the scientific revolution that accompanied the change from the corpuscular to the wave theory of light, does not in fact support the view that there have been radical methodological changes in the history of science.

I. Naturalism and the No-Invariant-Methodology Thesis

Until the early 1960s the belief flourished that scientific methodology[1] is not an empirical discipline. *Traditional* philosophers such as Karl Popper, Carl Hempel, and Imre Lakatos maintained that, unlike the natural sciences it studied, methodology is made up of a priori, *invariant*, and *ahistorical* epistemological principles.[2] More specifically, these traditionalists insisted that methodology was not an empirical endeavor because the validity, warrant, adequacy, and applicability of the principles and rules for the correct appraisal of scientific theories were not themselves substantive claims about the world. 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

[1] I use the term "methodology" to mean the study of the rules, methods, and standards which generally govern the evaluation and appraisal of scientific theories.

[2] I will use the term "a priori" simply as "non-empirical." That is, it does 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 – irrespective of its degree of dubitability.

> 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, 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, while 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 could 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. 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 *presuppositionless*, or at any rate not dependent upon any specific substantive scientific claim for their validity. 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, Paul Feyerabend, and other rebels, however, claim that the methods of science, the content of scientific beliefs, and scientific theories are fully intertwined with science's historical development because of the pervasive roles of paradigms (or similar networks of commitments) in scientific research. Moreover, as Kuhn would have us believe:

> 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. (Ibid., 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.[3]

Although Kuhn insists that "reports of this sort manifest total misunderstanding" (Kuhn 1977, 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 paper takes its motivation from Kuhn's view of scientific change because Laudan claims 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 substantive changes in science.[4] Agreeing with Kuhn, Laudan maintains 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" (Laudan 1990c, 46).

We need to be very precise about the claim of Laudan's naturalism. His claim is not merely that methodology is *informed by* substantive science. His 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 his claim is the strong Kuhnian one that the *validity and adequacy* of *all* methodological rules and principles rests on claims about the empirical world. Laudan's naturalism is fully committed to a complete denial of a priori assessment of methodologies. I will refer to this version of naturalism as *methodological naturalism*, an epistemological thesis which claims that there are no invariant methods and standards in science. More specifically, this position is one that upholds the following inter-related claims:

(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.

[3] In particular, Larry Laudan has accused Kuhn of relativism. Extensive charges of relativism against Kuhn can be found in Laudan 1984.

[4] 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 she *derives* her 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, Larry Laudan, the naturalist I consider in this paper, does not shy away from giving normative or prescriptive advice.

(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).

Based on a distinction he draws between *implicit* (i.e., the standards that actually and truly govern scientists' theory choice) and *explicit* (the standards scientists claim to be following) methodology, John Worrall (1988 and 1989a) has persuasively challenged the philosophical claims of Laudan by showing that Laudan is caught in a dilemma. For if all the methods and principles of science and scientific theory appraisal are subject to radical change, then competing theories or research traditions may uphold competing methodologies. When methodologies do conflict, how can choice between competing theories (or research traditions) be rationally adjudicated? Worrall's argument is that if the philosophical claim of Laudan applies to both implicit and explicit methodology, Laudan cannot defend the rationality of science. Building on John Worrall's challenge to Laudan's philosophical claim, I will offer a straight-forward refutation of one key example used by Laudan to defend his historical claim. This example is that of the nineteenth century revolution in optics.

More specifically, based on Worrall's distinction between implicit and explicit methodology, I will show that Laudan's pet example of radical methodological change does not in fact support his conclusions. Laudan's use of this example is based on his claims about the condition of independent predictive warrant. In refuting his claim, I will argue that natural philosophers, including proponents of the corpuscular theory, have long recognized the virtue of the condition of independent predictive warrant. I will discuss the methodological writings of two defenders of the corpuscular theory. 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. In the next section, I start with a characterization of the nineteenth-century revolution in optics.

II. The Methodology of Light

By the end of the third decade of the nineteenth 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.[5] The central assumptions of the corpuscular theory can be regarded as the following: (a) Light is made up of tiny particles (called corpuscles) emitted from luminous objects; and, (b) These particles obey the usual (Newtonian) laws of particle physics. The central assumptions of the wave theory can be taken to be: (a) Light is a kind of disturbance in an all-pervading elastic medium called the ether; and, (b) Differences in the color of light depend on the frequency of vibrations excited by luminous objects in the ether. The scientific revolution that occurred during the nineteenth 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 this revolution was accompanied by an underlying and deeper change in the methodological and epistemological requirements of science:

> 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, 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. "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

[5] 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 nineteenth-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 eighteenth century; nor is there any doubt that some version or other of the Young-Fresnel wave theory became dominant by the end of the 1830s.

entities that could be observed or measured" (ibid., 158). But proponents of the wave theory were committed to the method of hypothesis:

> The epistemology prevalent in the second half of the eighteenth 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 nineteenth century prompted a . . . radical critique of classical epistemology, a critique that produced some highly innovative and historically influential methodological ideas. (Ibid., 157–158; my emphasis)

Concerning the initial opposition to the wave theory by Scottish natural philosophers, Laudan 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" (ibid., 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, 111), "supporters of the wave theory, unlike its objectors, championed the method of hypothesis" (ibid., 114).[6]

> 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 favor 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. (Ibid., 109)

There is, of course, an old distinction between *implicit* and explicit *methodology*.[7] A scientist's *explicit* methodology is what she *actually* says and writes in her reflections on her scientific 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

[6] One significant difference between Laudan's and Cantor's treatments of the optical revolution is that while Laudan compares the methodology of nineteenth-century wave theorists to that of eighteenth-century Corpuscularians, Cantor (1975 and 1983) compares the debate between nineteenth-century wave and corpuscular theorists.

[7] See John Worrall (Worrall 1988) for further details.

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.[8]

Unfortunately, as John Worrall has persuasively shown, Laudan is not very clear on whether his view of radical methodological claim applies only to *explicit* methodology, or to both *explicit and implicit* methodology. Is the change Laudan describes in his account of the revolution in optics 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?

A good deal of what Laudan has to say about this historical episode 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, 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 Leibniz) 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 ability was a sine qua non for an adequate theory. (Laudan 1981b, 185, n.92)

[8] 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" (Newton 1966, 547; my emphasis). It should also be noted that Laudan does not claim that Newton himself was a strict inductivist who rejected observation transcendent hypotheses.

Surely if the methodological change that occurred in the 1820s was merely in the ability of scientists to adequately 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.

I hesitate to attribute 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. (Ibid., 181)

Because of this, I shall take Laudan's *full* view to be that there were changes in *both* implicit and explicit methodology.

Laudan divides his analysis of this historical episode into two phases: 1740–1810 as the first phase, and 1820–1840 as the second phase. He 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: 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. He 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 realized 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" (ibid., 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. (Ibid.)

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 favor by this means.* (Quoted in ibid.)

Laudan insists that Hartley's version of the method of hypothesis is just as stated above. That is, the method justifies the acceptance of *any* theory in as much as an assumption of that theory is consistent with a 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 favor, . . ." 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. (Ibid., 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 that are not *induced* from experimental observations" (ibid., 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. (Ibid., 163)

Although the eighteenth-century defense of the method of hypothesis by Hartley was unsuccessful, Laudan insists that the development of the wave theory of light in the early nineteenth 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 eighteenth-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 eighteenth-century proponents of the method of hypothesis regarded *all* consequences of theories as evidence for theories. Nineteenth-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 eighteenth-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. (Ibid., 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 nineteenth century.

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

(1) It is clear that Laudan's reconstruction of the methodology of light supports the view that in their *explicit* methodology, eighteenth-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: Eighteenth-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, eighteenth-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 eighteenth-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), nineteenth-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 eighteenth 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 nineteenth century prompted a more radical critique of classical epistemology, a critique which produced some innovative historically influential methodological ideas" (ibid., 157–158). *The most significant of these innovative methodological ideas was the requirement of independent predictive warrant.* Indeed, "no epistemologist in the eighteenth 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 nineteenth century" (ibid., 175). How accurate is Laudan's reconstruction of this historical episode? I think that Laudan is mistaken in claiming that the condition of independent predictive warrant is a methodological requirement that was peculiar to defenders of the wave theory. I will illustrate this with the examples of David Brewster and Thomas Reid.

III. David Brewster[9]

Brewster was a nineteenth-century defender of the particulate theory of light who did not *accept* the wave theory of light: "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, 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*. This is because he rejected the substitution of *truth* for *possible* in inferences to the best possible explanation; that is, the best possible explanation may nonetheless be false, and Brewster maintained that the wave theory was false. 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" (ibid., 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: "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" (ibid., 360). 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 ..." (ibid., 360). His main objection, however, was that since the theory could not give any viable explanation of the phenomena of dispersion and selective absorption, it was better to continue developing the corpuscular theory which was already very successful at explaining and predicting a wide variety of phenomena.

There should be no controversy over Brewster's recognition of the virtues of the method of hypothesis. For Brewster recognized the difference between the eighteenth-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

[9] The treatment of Brewster given in this section builds on that of John Worrall (Worrall 1990).

ad hoc manner), and the nineteenth-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, 272; my emphasis). The first major historical 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.

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. (Ibid., 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" (ibid., 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. (Ibid., 305–306; my emphasis)

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 group 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. (Ibid., 306; my emphasis)

Most devastatingly 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. (Ibid., 306)

Brewster's full recognition and acceptance of the condition of independent predictive warrant hints at one fundamental problem with Laudan's analysis of this nineteenth-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 eighteenth 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, on whom Laudan has written extensively as a *bona fide* Newtonian inductivist (hence, according to Laudan, a strict empiricist), is also not as hard-headed as Laudan makes him.[10]

IV. 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, 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. (Ibid., 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

[10] Laudan compares the methodology of eighteenth-century Corpuscularians with that of nineteenth-century wave theorists, and he also claims that the condition of independent predictive warrant "is a product of the early nineteenth century" (Laudan 1981, 130). So Laudan could object to my treatment of any nineteenth-century Corpuscularian like Brewster by claiming that, just like their wave theory counterparts, they accepted the condition of independent predictive warrant. That is, Laudan could insist that only eighteenth-century Corpuscularians, but not nineteenth-century Corpuscularians, adopted Newtonian inductivism. I do not think this move is open to Laudan. First, Laudan himself seems to imply that nineteenth-century Newtonianism was incompatible with nineteenth-century versions of the method of hypothesis. Moreover, 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 nineteenth-century Corpuscularians with the methodology of nineteenth-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 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, 184, n. 64). In short, in Laudan's view, nineteenth-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 the method of nineteenth-century Corpuscularians was incompatible with the methodology of nineteenth-century wave theorists. This is because eighteenth-century Newtonians such as Reid provide ample difficulties for Laudan's claim of radical methodological change.

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-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" (ibid., 89).

But is Laudan's interpretation of Reid's methodological commitments correct? There can be no doubt 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 16 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 judgment remains in suspense, and all I can say is, it may be so, and it may be otherwise.* (Reid 1872, 56–57; my emphasis)

Indeed, the italicized portions of this quotation are 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*. (Ibid., 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. (Ibid., 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. (Ibid.)

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. (Ibid., 57)

Reid's point is that it is not enough that a theory entail the evidence, for that 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.* (Ibid.; my emphasis)

And he claims that:

> The first proposition [in the syllogism above] 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*. (Ibid.; 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. (Ibid.)

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, 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. 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 stronger 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, 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. (Ibid., 250)

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. (Ibid.)

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]" (ibid., 249).

V. The Implications of Brewster and Reid

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 (1990), for instance, argues that the disagreement between David Brewster and the wave theorists was chiefly over "the way forward." As shown above, Brewster, just like the wave theorists accepted that the predictive and empirical success of a theory counts favorably 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 were reasons to conclude that any version of the wave theory would be unable to explain these phenomena. And since the corpuscular theory was then the only viable and serious alternative to the wave theory, Brewster decided to continue working on that alternative.

Proponents of the wave theory of course accepted Brewster's point that their current versions of the theory could not account for the phenomena of dispersion and selective absorption. 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 program would eventually stage a comeback.

Similar points apply to Reid. For as I have shown above, the condition of independent support, which Laudan claims is exclusive to the wave theorists, was in fact accepted by Reid. As Reid puts it:

> . . . 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 . . . any new evidence has been found of its existence. (Reid 1872, 250)

Moreover:

> . . . while we have no proof of [the existence of waves and the aether], 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. (Ibid.)

From the example of Brewster and Reid, we certainly cannot conclude that most or many Corpuscularians appreciated the virtues of the condition of independent warrant! All we can say is that the debate between some Corpuscularians and the wave theorists was about the way forward. Nonetheless, 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. If at least some Corpuscularians appreciated the values of independent warrant, then Laudan's claim that "the epistemology prevalent in the second half of the eighteenth century was altogether incompatible with the various ether theories that emerged in the natural philosophy of that period" (Laudan 1981b, 158) simply must be false.

VI. Concluding Remarks

In this paper I examined one historical episode advanced by Laudan as an example of radical change in scientific 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" (ibid., 181), my examination of the episode establishes the following:

1. Laudan's claim is that predictive success was not part of eighteenth-century methodology and that scientists only started to invoke this in the early nineteenth century as a result of the triumph of the wave theory. In fact, however, historical records show that the methodological requirement of predictive success is by no means exclusive to wave theorists. Proponents of the particulate theory also accepted the virtues of this methodological requirement. Significantly, Thomas Reid, Laudan's own main example of a Newtonian inductivist, accepted the virtues of these methodological requirements.
2. 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 viable challenge to the traditional approach to methodology, which is exclusively concerned with the invariance of implicit methodological commitments.

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