# Waves, Particles, Independent Tests and the Limits of Inductivism

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In this paper, I shall be taking exception to a few of the ideas in Peter Achinstein's recent (1991). In some of these cases, I think he is flat wrong. But that does not diminish in the least my admiration for his book which is, in my judgment, the best extended piece of work we have on the epistemological problems posed by 19th-century physics.

## 1. Introduction

More than a decade ago, in Laudan (1981), I told a story about the development of 19th-century optics and empiricist epistemology . It went roughly as follows: the inductivist epistemology that became popular in the philosophical aftermath to Isaac Newton made it methodologically precarious to postulate theoretical entities, especially if those entities had properties unlike those of observed objects. This, in turn, meant that a variety of theories—including the wave theory of light—were to receive a hostile reception at the hands of many 18th-century empiricists and those natural philosophers heavily influenced by them. After all, theories which postulated the existence of elastic, imperceptible and imponderable fluids were not the sorts of beliefs that an 18th-century empiricist could happily countenance. It was the moral of my story that, before such theories could begin to enjoy a wide acceptance, changes had to occur in prevailing methodological standards. Specifically, as I put it then, a shift was needed away from a narrow inductivism and towards a recognition of the merits of the method of hypothesis. Such a shift would enable fluid theorists to argue for their theories by pointing to their explanatory and predictive resources, even if the entities they postulated were quite beyond the reach of ordinary observation and inductive generalization from the observable.

As I showed, this H-D sort of inference from a confirmation of consequences to the probability of the theory itself carried no weight among traditional empiricists such as Bacon, Newton, Hume or Reid. When Newton said 'hypotheses non fingo', it was this sort of inference he was repudiating. I suggested that it was thus no accident that the revivified wave theory of light and the method of hypothetico-deduction gained ascendancy at about the same point in the 19th century. The wave theorists needed the method of hypothesis to justify their approach and the successes of the

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wave theory managed, in turn, to constitute vivid examples of the scientific fruits of hypothetico-deduction. I claimed that this linkage between the advocacy of etherial fluids and anti-inductivism explains how, for instance, that principal advocate of the method of hypothesis in the first half of the 19th century, William Whewell, became one of the leading spokesmen for the wave theory.

At about the same time that I was doing this research, a British historian of physics—Geoffroy Cantor—was coming to a complementary conclusion from a different direction (1983). He had been studying the writings of such corpuscularians as Brougham and was struck by how heavily their criticism of the wave theory was imbued with inductivist language. Like me, he came to the conclusion that there was a close connection in the Enlightenment and early 19th-century science between where one stood on the wave/particle question and what theory of scientific method one espoused.

Such a happy consilience of perspectives convinced Cantor and me that we were right, of course, especially as there were few demurrals to be heard from other scholars for more than a decade. Until last year, that is, when Peter Achinstein published his extremely interesting book, *Particles and Waves*.[1] In that book, as in his paper here, he has a different story to tell about this episode. According to Achinstein, there was no major methodological divide separating the corpuscularians from the undulationists. Methodological consensus happily prevailed among the physical scientists of the period. Still worse, at least as far as Cantor and I were concerned, the consensus that Achinstein detects was an agreement that induction, not the method of hypothesis, is the appropriate epistemology for science. Achinstein devotes two lengthy chapters of his important book to developing a probabilistic, quasi-Bayesian analysis of the episode, purporting to show that optical theorists in the early 19th century were all, at least implicitly, Bayesian conditionalizers.

Now, I cannot speak for Cantor but I want to say for my part that I think that Achinstein's analysis has—on this particular point— got both the philosophy and the history wrong. It will be the purpose of my remarks today to motivate that reaction. For those of you who are saying to yourselves, "Who cares how the 19th-century light debates went?" I will try in passing, although this obviously cannot be my main concern today, to draw out some lessons from this episode for debates in contemporary philosophy of science.

Several points about the historical record are uncontested. Let me begin with a summary of those: through much of the 18th century, Huygens' wave theory of light was eclipsed by Newton's corpuscular theory, not least because it seemed that Huygens could not explain the rectilinear propagation of light. At the turn of the 19th century, Thomas Young attempted to revive the wave theory using it to explain phenomena of diffraction and optical interference such as the colors of thin films and diffraction. Young's theory in turn failed to be able to account for polarization. Then Fresnel came up with a kinematic model which conceived light as a transverse vibration transmitted in an elastic etherial fluid. This enabled him to explain polarization and double refraction and to predict a number of surprising phenomena, including the famous bright spot at the center of a shadow cast by a disk. During the early 1830s, Cauchy developed a dynamical wave theoretic model that explained dispersion as well. After intense debate among physicists in the 1820s and early 1830s, most scientists had come to accept the superiority of the wave theory by the late 1830s, although a few hold-outs persisted for another generation. So much for the common ground.

What is in dispute here, to put it in its most general terms, is this: what sorts of epistemic virtues led to the triumph of the wave theory? In very brief compass, the

Laudan reply was this: the wave theory made a series of surprising predictions that turned out to be right and for which there were no counterparts in the corpuscular theory. It also explained a broader range of phenomena of diverse types without resorting to ad hoc adaptations.[2] In sum, it solved more empirical problems than its corpuscularian rival and did so with less ad hocery. These, however, are virtues from an H-D perspective, not from an inductivist one. Achinstein's answer—to which I shall turn in a moment—is, in brief : the wave theorists managed to show that the corpuscular theory had a vanishingly small probability and this created, by a kind of method of exclusion, a presumption that the probability of the wave theory was close to 1. Such positive confirmations and surprising successful predictions as the wave theory enjoyed merely reinforced this conclusion; they were, Achinstein insists, insufficient to motivate it. In other words, Achinstein denies that the ability of the wave theory to explain and predict a broad range of empirical phenomena, many of them surprising, did, or even in principle could have done, much to enhance its credibility. In what follows, I shall sketch out the two stories in more detail and indicate why I remain skeptical about Achinstein's version.

## 2. The Achinstein Account

It is important to note at the outset that Achinstein's analysis is simultaneously working at two levels, the normative *and* the descriptive. What drives his normative analysis is a conviction that a Bayesian theory of evidence and testing is the only sound one. Descriptively, Achinstein is concerned to show that the participants in the light debates of the early 19th century were in fact making appraisals in accordance with Bayesian recipes. My principal concern here will be with the *descriptive* adequacy of Achinstein's rational reconstruction rather than with its normative underpinnings. But if, as I expect to show, the Bayesian story falls short of being able to capture the reasoning of the agents involved, then it will be appropriate to ask, time allowing towards the end of my comments (or perhaps in the discussion to follow), whether Bayesianism could possibly capture the character of the scientific use of evidence in cases like this one.

Achinstein's rational reconstruction of the episode goes as follows. The wave theorists, he says, adopted a four-step strategy:

1: Start with the assumption that light is either a wave phenomenon or a stream of particles.

2: Show how each theory explains various optical phenomena.

3: Show that the particle theory, in explaining one or more of the observed phenomena, introduces improbable hypotheses while the wave theory does not.

4: Conclude that the wave theory is (very probably) true, *because* the particle theory is (very probably) false.

Achinstein then proceeds to offer slightly more formal characterizations of these four steps. Step 1, he says, is tantamount to asserting that, relative to certain observations O and background knowledge b,

(1) $p(T_1 \text{ or } T_2/O\&b) \approx 1$ ($\approx$ means "is close to"),

where $T_1$ is the wave theory and $T_2$ is the particle theory. Step 3 above amounts to the claim that the particle theorists had recourse to certain auxiliary assumptions, h, such that although the auxiliaries are very plausible given the corpuscular theory, viz., although

(3a) $p(h/T_2\&O\&b) \approx 1$,

the fact is that there is strong evidence against the truth of those auxiliaries, viz.,

(3b) $p(h/O\&b) \approx 0$.

A quick application of Bayes' theorem to (3a) and (3b) yields the result that

(3c) $p(T_2/O\&b) \approx 0$.

Combining (3c) with (1), Achinstein infers that the wave theory is very probably true, viz.,

(4) $p(T_1/O\&b) \approx 1$.

Such then, in schematic form, is Achinstein's proposed reconstruction of the arguments of the wave theorists. But what about step 2 and the comparison of the wave theory with optical phenomena? One might have thought that the single most important bit of evidence in appraising the wave theory was an examination of how it fared against the phenomena. But this process of checking the empirical consequences of the wave theory, according to Achinstein, can—even if the hypothesis stands up successfully to these tests— do little to enhance the probability of the wave theory. As he sees it, Bayesian inference insists that predictions and "explanations (no matter how numerous or varied) do not suffice to give an hypothesis high probability" (1991, p.135). All such successes can do is to "ensure that the hypothesis retains whatever probability it has on other data" (p. 135). This is quite a remarkable claim. It would certainly have come as a shock to many of the wave theorists who were impressed by the ability of the wave theory to make surprising predictions successfully and to explain many puzzling features of light. To someone like Whewell, who made a point of underscoring such successes of the wave theory, Achinstein's rebuff is quick:

> An explanatory strategy of the sort advocated by Whewell and other supporters of the method of hypothesis will not be enough to guarantee high probability for *h*, no matter how many phenomena *h* explains, even if consilience and coherence...are satisfied (p. 137).

On Achinstein's view, the only thing that can give the wave theory, or any other theory, a high probability is the demonstration that the probability of the disjunction of all its rivals is vanishingly small. Discrediting rivals is the only significant way of enhancing the probability of one's pet hypothesis. A good offense it seems is not only the best defense; it is the only defense. The eliminative refutation of rivals is, for Achinstein, the only significant way of enhancing the credibility of a theory.

I think that his analysis is flawed both conceptually and contextually. I will now try to show why. First, and more briefly, philosophically:

a) The Conceptual Problem

There is a crucial equivocation at the beginning of Achinstein's characterization of the problematic facing theorists of light in the early 19th century. The wave theorists, on his reconstruction, began with the assumption that light is either a wave or a particle. That was itself fairly controversial since, even if we ignore possible theories and limit ourselves to then extant theories, there were theories which saw light as a fluid—very like heat was conceived in the 1820s—a hydrodynamic conception that is, strictly speaking, neither particulate nor undular. But leave that reservation to one side. Let us suppose that they and Achinstein were right in thinking that light almost certainly was either a wave or a particle. The equivocation I have in mind comes in the move from the claim that

(0) p(light is a wave or light is a particle)≈ 1

to the claim that Achinstein needs for his reconstruction, viz., that the probability of a specific theory of light is close to 1 (viz., thesis (4)). The fact is that, even if it could be settled with certainty that light was not a corpuscle, and even if it could be inferred therefrom that light was almost certainly a wave-like phenomenon, it manifestly would not follow that we could *thereby* assign any particular probability—let alone a probability close to 1—to any *specific* wave theory of light.To establish by disjunctive elimination the high probability of a particular theory of light, one must not only discredit corpuscular approaches but, equally and obviously, one must show that rival wave conceptions to the one in question have a vanishingly small aggregated probability. For that reason, even if it is virtually certain that light is a wave, it does not follow without further ado that any particular theory of light is highly probable.

This is more than an quibble since there were several different versions of wave theory on offer in the first half of the 19th century.[3] To remind you of but two of them, recall that Young's wave theory of light did not involve transverse vibrations, while Fresnel's theory did. Other alternatives involved translational motion of the aether while some supposed only vibratory motion. For which of these many alternative wave theories is Achinstein claiming a probability close to 1? And how can he possibly get that claim from the refutation of a particular version of the particle theory, or even from the refutation of every known version of the corpuscular theory? Insofar as the undulationists were generally arguing for one specific version or other of the wave theory, Achinstein's machinery is of no avail. Take the case of Fresnel during the 1820s. He did not see himself as addressing the ontological question "Is light a wave?" so much as he was attempting to ascertain the credibility of specific wave models or theories of light. Refutations of the corpuscularian hypothesis were powerless to guide him with respect to choices among rival wave conceptions.

But perhaps, when Achinstein tells us that the probability of the wave theory is close to 1, he has in mind no particular, full-blown version of the wave theory but rather some generic Ur-wave theory which contains only those assumptions held in common between the various wave theories. Let us give him the benefit of the doubt here and suppose that it is that body of assumptions that he means by the term 'wave theory'. What would such a theory look like? Absolutely essential to any attempt to characterize the common or generic elements of early 19th-century wave theories is the idea of a *luminiferous aether*. Although the student of 20th-century physics, in contemplating a wave theory of light, has been trained to resist asking the question: "In what medium are the waves propagated?" no early 19th-century physicist could be so ontologically blase. According to both its opponents and its detractors, the wave theory—in *all* its known versions—was committed to the existence of an all-pervading, highly elastic, uniformly dense aether whose constituent parts were imponderable, i.e., without weight. To accept the wave theory (in any sense stronger than as a useful predictive instrument) was, in this epoch, to claim to have a warrant for postulating such a medium. Much of the debate between the wave theory and its critics—a side of the debate that Achinstein largely ignores— is about the appropriateness of this commitment to a highly theoretical entity.

Indeed, once one realizes that every version of wave theory in the period is committed to this principle, we can recast Achinstein's earlier prima facie eliminative disjunction into this form:

(1') p(light is a particle or light is propagated through an elastic, homogeneous, imponderable fluid/O&b)≈ 1

When thus recast, the core premise of the Achinstein reconstruction suddenly becomes, I submit, a great deal less plausible. What might have looked initially as a virtually exhaustive disjunction now comes to seem much less so. We can readily imagine that both disjuncts may be false and therefore that the initial assignment of a probability close to 1 to their disjunction is no longer compelling. And in that case, wave theorists are going to have to do a great deal more than argue negatively for a low probability that light is a corpuscle. Of course, Achinstein is right that the wave theorists tried to show that light is not corpuscular, but if that was all that they had done, or the principal thing they had done, then they would have had no license whatever for supposing themselves to have provided a warrant for accepting the wave theory, not even in its generic version, given the implausibility of (1'). And argue they did. Over and again, the wave theorists claimed that the strongest bit of their case rested on the ability of wave theories to solve a large range of empirical problems, including many which had been anomalous both for corpuscularian and for earlier undular theories.[4]

The philosophical point here about the precariousness of eliminative induction is a familiar one but it continues to be often ignored by Bayesians in our time, as it was ignored by Mill and his followers in the 19th century. Any account of evidence and theory evaluation which requires the enumeration of all the possible hypotheses for explaining some phenomenon, or in more modern probabilistic parlance, any approach which requires the enunciation of a set of hypotheses which are mutually exhaustive and pair-wise exclusive is, when applied to real scientific choices, almost guaranteed to fail. Scientists are rarely in a position to assert that they have canvassed all the relevant possibilities for explaining any body of phenomena. Indeed, properly viewed, the history of science is a record of refutations of such claims, whenever scientists have been so cheeky as to imagine that their extant theoretical options exhaust the available conceptual space. If Achinstein is right in claiming that hypotheses can acquire high credibility only by the exhaustive elimination of rivals, then we have to conclude that few if any scientific theories ever become credible. The alternative, of course, is to suggest that it is a reductio of the Bayesian position *if* it insists that credibility can be achieved by a theory only when all possible rivals to that theory have been both enumerated and vanquished.

Before I move on to discuss what I earlier called the contextual problem, there is another conceptual problem that I want to mention. Full treatment of it would require another essay, but I can briefly summarize my worries in this fashion: as should already be clear, Achinstein believes that the accumulation of positive instances of a theory or hypothesis, however numerous, makes, at best, only a marginal difference to its probability. With that in mind, let us review Achinstein's reconstruction of the wave theorists' argument. Recall that the crucial step 3 in Achinstein's reconstruction of the wave theorists argument involves their showing that auxiliaries introduced by the corpuscularians are highly improbable. In particular, their argument goes as follows: the corpuscular theory requires that the deflecting force is independent of the mass and shape of the deflecting aperture. This, say the wave theorists, is very unlikely, given that in the other cases of forces acting at a distance of which we are aware, the force in question is related to both the mass and the shape of the acting body. Now, how do the wave theorists know this? Well, one has to suppose they know it by virtue of an enumeration of known cases of bodies acting at a distance, combined with the knowledge that the force in question has thus far always depended on the mass and shape of the body exerting the force.[5]

In sum, the wave theorists are supposing that we have lots of instances of the hypothesis that forces exerted are dependent on mass and shape. It is that generalization that makes the corpuscularian hypothesis unacceptable. But, by Achinstein's lights, such information can do nothing whatever to make probable the hypothesis that force depends on mass and shape. Instances of a generalization cannot—in his view—make that generalization probable. Only an eliminative argument can do that. As Achinstein himself points out, Thomas Young's argument that the shape of bodies determines the kind of force they exert is based upon the fact that this is what we observe to be the case "with other known forces acting at a distance" (1991, p. 87). But, as I have said, on Achinstein's own theory, such observations cannot possibly establish with high probability the claim that "All distance forces are dependent on the shape of the body exerting the force". Yet that latter hypothesis is precisely the one which, on Achinstein's reconstruction, the wave theorist is needful of. A similar argument could be made about the *first* premise of the wave theorists, namely, that all cases of motion involve either the transmission of a particle or of a disturbance in a medium. If you deny to the wave theorist the possibility of making an hypothesis credible by citing positive instances of it , then the wave theorist cannot begin to get (Achinstein's version of) his argument against the corpuscular theory off the ground.

b) The Contextual Problem

I want now to turn away from the eliminationist issue in order to focus on what seems to me to be the central issue at stake in the debates between early 19th-century corpuscularians and undulationists. But we do not need to move too far afield, since (1') already allows me to direct attention to what I think was the core methodological divide between the wave theorists and the corpuscularians. Ever since Newton, corpuscularians had insisted that any theory about natural phenomena must not only be sufficient to explain the appearances, it must also involve postulating only true causes, or verae causae. This requirement, sometimes called the vera causa rule, is close to the core of late 18th-century empiricism; it was generally understood to mean that any entities postulated by theory *must be ones to which we have independent access*. Independent of what? Independent of the phenomena that the theory would be used to explain.

Between the time of Newton and Whewell, there was extensive discussion and refinement of this principle. Reid, Stewart, Priestley, Lyell, and Herschel were among its most ardent proponents. By the early 19th century, the vera causa demand had generally come to mean that *any properties attributed to theoretical entities or processes must be a subset of the properties of observable bodies*. The vera causa requirement, in other words, forbade attributing properties to unseen objects which were not exhibited broadly (perhaps universally) by objects accessible to inspection. Such a methodological demand was satisfied by the corpuscular theory of light; that indeed was one reason for the popularity of the corpuscular theory in the late 18th century. It postulated particles of light which, although obviously too small to be seen, behaved very like macroscopic objects, subject to Newton's laws and to familiar forces of attraction and repulsion.

Within the wave theory, however, the requirement of independent access or vera causa was apparently violated by the luminiferous aether.[6] That aether consisted of particles that, being imponderable, had no weight. Corpuscularian critics of the wave theory like Brougham and Brewster claimed that no responsible empiricist had a license for propounding theories that, whatever their predictive or explanatory successes, involved entities whose properties were not drawn from common experience.[7] When the corpuscularians demanded that there should be independent warrant for theories, this was what they had in mind.[8]

This was not a demand in which the wave theorists could acquiesce. A ponderable aether, which might have passed the vera causa test, would not do the jobs they required of their aether. Nor could they point to imponderable bodies in ordinary experience. It is for this reason, in my view, that the wave theorists found the method of hypothesis congenial, for what it offered was a way of freeing oneself from the vera causa requirement. The method of hypothesis allowed that a theory could be made plausible simply by examining its consequences (especially if they were of a broad and surprising character), without imposing any specific constraints on the sorts of entities postulated by the theories. Reading Whewell on this matter is instructive. A keen advocate of the wave theory, he goes to considerable lengths to castigate Newton and his followers for advocating the vera causa principle. Whewell sees that rule as an impediment to discovery and innovation and a gratuitous demand to make of a theory, especially if its consequential confirmation is impressive. Whewell saw clearly that, so long as the vera causa principle of independent warrant for a theory persisted, the wave theory of light would have tough sledding.

Achinstein acknowledges that early 19th-century methodological standards required that there be independent support for theoretical entities. But, having acknowledged that, he proceeds to construe that requirement, when applied to the wave theory, as being satisfiable by evidence that the corpuscular theory is erroneous! It is via that construal that Achinstein is able to act as if methodological consensus prevailed. The corpuscularians and the undulationists, he says, all accepted the principle that there must be independent empirical warrant for theories.[9] Indeed, he characterizes the undulationists' procedures as described in his steps (1) to (3) *as* a principle of independent warrant. What Achinstein fails to note is that the wave theorists' form of independent warrant—if that is what it is—is completely unlike the traditional empiricist requirement of independent warrant.[10] How the wave theorists established independent warrant, according to Achinstein, was by showing the implausibility of the auxiliaries used by the corpuscularians. But that has nothing whatever to do with satisfying the requirement of independent warrant *as inductivists and corpuscularians then understood it*. To the 18th-century empiricists and their successors in optics like Brougham and Brewster, independent confirmation of a theory T consisted in showing that ordinary bodies exhibited all the properties that T attributes to the entities it postulates. By contrast, Achinstein's version of the independent support requirement dispenses with any constraint on the sorts of permissible entities. Rather, all it demands is evidence that the rivals to T are false or unsupported. I submit that no 18th-century empiricist, no advocate of the vera causa requirement and few if any corpuscularians would have accepted Achinstein's characterization of the independent warrant requirement as an explication of what they were about.

If the wave theorists' strategy consists of the four steps that Achinstein attributes to them, then it automatically follows that—far from being inductivists in the *then* accepted sense of that phrase—they were entirely abandoning the inductivists' project for subjecting theory to the vera causa requirement. Whewell saw clearly that the wave theory could not satisfy the traditional demand for being a vera causa; that is why he argued at length against the legitimacy of that requirement in scientific methodology.

But even if Achinstein has got the corpuscularians wrong, it remains to ask whether his analysis of the case is one that wave theorists would have found congenial or close to the spirit of their project. I have my doubts. For reasons already indicated, the discrediting of *known* rivals—and that is all Achinstein's independent confirmation requirement requires—is not sufficient grounds for asserting a theory. The wave theorists understood that and therefore spent much ink arguing that the principal virtue of the wave theory consisted in its ability to predict and explain a large range of phenomena, including many surprising phenomena. Achinstein's philosophically-motivated conviction that this particular virtue cannot confer high probabilities on theories leads him to give less than its due to the prominent role accorded to positive evidence by the wave theorists. Convinced that positive confirmation, of whatever sort, cannot confer high probability on a theory, Achinstein supposes that 19th-century wave theorists must have accepted this point and acted accordingly. But I can find no evidence whatever, either direct or circumstantial, that they believed that positive confirmation was as impotent as Achinstein thinks it is.

Let me put the challenge directly: Where is the evidence that *the wave theorists* believed, as Achinstein does, that confirmation of salient instances cannot confer high credibility?[11] Where is the evidence that they regarded the low probability of corpuscular theories as the *principal* ground of credibility for their own views? And if they did believe that, why were they so concerned with finding impressive corroborations of the wave theory?[12] Indeed, if they really believed—as Achinstein suggests—that the wave theory acquires virtual certainty simply from the discrediting of the corpuscular theory, why give pride of place, in assessments of the wave theory, to its successful positive instances?[13] John Stuart Mill, himself no friend of the wave theory, believed that the ability of theories to make surprising predictions successfully was of no epistemic moment. Such phenomena are, he said, designed only to impress 'the ignorant vulgar'. I trust that it goes without saying that Achinstein is a Millian on these matters, even if his language is less figurative than Mill's. But I see no historical basis for claiming that the wave theorists shared this dismissiveness about positive evidence in general, or about surprising instances in particular. For that reason, I doubt that eliminationism was the dominant methodological strategy of 19th-century theorists of light. Had it been so, 19th-century optics—both on its theoretical and on its experimental side—would look radically different from the way it actually does.

## Notes

[1]Except where otherwise noted, all references to Achinstein will be to his (1991).

[2]I claim no originality here. Whewell (1840) gave exactly the same analysis of the case.

[3]To mention only a few: there was Young's wave theory (without transverse vibrations), Fresnel's wave theory of 1818, Fresnel's 1821 aether (consisting of molecules acting at a distance) and Cauchy's aether of the early 1830s . There were divergences among wave theorists about such matters as: the range of the molecular force associated with a particle of the aether (does it extend only to the next particle or fall off according to a $1/r^4$ law, as Cauchy thought?); and how do aether particles and ordinary matter interact? These were not idle questions as answers to them determined what sorts of empirical consequences a wave theory of light would have. Buchwald (1981) has convincingly argued that divergences among wave theorists about the nature of matter-aether interactions were "extremely important" in debates about the wave theory.

[4]Thus, William Herschel in his important monograph on light (1827, p. 538) claims that:

> Nothing stronger can be said in favour of an hypothesis, than that it enables us to anticipate the results of ... experiment, and to predict facts opposed to received notions..

He was to make a similar point three years later in his 1830 classic:

> The surest and best characteristic of a well-founded and extensive induction, however, is when verifications of it spring up, as it were, spontaneously into notice, from quarters where they might be least expected, or even among instances of the very kind which were at first considered hostile to them. Evidence of this kind is irresistible, and compels assent with a weight which scarcely any other possesses (p. 170).

These and like sentiments to be found in much of the writings of the wave theorists are scarcely the views of folks who think that the refutation of a rival hypothesis is the primary vehicle for establishing the credibility of a theory.

[5]Achinstein himself describes the reasoning of the wave theorists here as a form of 'inductive generalization' (1993). I fail to see how, by his lights, such inductive generalizations are capable of establishing the high probability of the claim that forces depend on masses and shapes.

[6]I might ask in passing how, since Achinstein believes that it establishes a low probability for the corpuscular theory that it requires that the diffracting effect of an aperture is independent of its mass and shape, he can ignore the corpuscularians' argument that the wave theory must have a low probability because of its commitment to imponderable particles of the aether? If light-deflecting apertures whose deflection depends on neither their shape nor mass is contrary to previous experience, it is surely as contrary to experience to postulate particles that have no weight. Although Achinstein claims that "no auxiliary assumption is introduced [by the wave theorists] whose probability given the theory is very high but whose probability on the phenomena alone is low" (1993) , it seems to me beyond dispute that the hypothesis of the existence of an imponderable aether—although very probable given the wave theory—is an hypothesis whose probability 'on the phenomena alone is low'.

[7] David Brewster inveighed against the luminiferous aether because it was "invisible, intangible, imponderable [and] inseparable from all bodies" (1838). On those grounds, he held that it could not be postulated as a legitimate causal agent.

[8]The widespread acceptance of the vera causa demand shows up not only in the writings of the corpuscularians. During the 1820s and 1830s, there was a sizable group of optical theorists who, while opportunistic about using the mathematical and analytic tools of the wave theory, balked at the full acceptance of the wave theory because they did not see a warrant for the postulation of the optical aether. George Airy, for instance, claimed that the positive evidence as to the composition of the aether was too incomplete to enable one to judge which of the various aether models was correct (1831, vi) Baden Powell, happy to use the principle of interference, drew the line at accepting the aether precisely because it was not a vera causa whose existence had been independently established (Powell, 1835 & 1837).

[9]More than a decade ago in (1981, p. 175), I pointed out that the wave theorists's idea of independent support "ought not be confused with the earlier empiricist requirement that theories must involve *verae causae*."

[10]Achinstein writes that "both methodologies [those of the wave theorists and the particle theorists] stress the need for independent empirical warrant". (1991, p. 108) That may be so, but the fact remains that the two camps construed the demand for independent warrant in wholly different ways.

[11]I am not here asking the *normative* question whether they were correct in believing that positive confirmation can confer high probability. It is the descriptive issue that is at stake here.

[12]Discussion of the confirmation of surprising predictions made by the wave theory was commonplace in this period. In 1833, for instance, Hamilton used Fresnel's biaxial wave surface to predict (what was previously unknown) conical refraction. Within a year, Humphrey Lloyd, another partisan of the wave theory, had confirmed this result, triumphantly announcing the confirmation to the British Association meeting in 1834. Why, on Achinstein's account, make such a fuss over results that could at best only marginally increase the credibility of the theory?

[13]The only answer that I can find Achinstein offering to this question is that "the wave theorist wants to show that his theory is probable not just given some limited selection of optical phenomena but given all known optical phenomena" (1993, p. 11). That, I think, is not how the wave theorists express themselves. They are not saying: "Look, see how our theory retains its high probability even when it is extended to new phenomena!" Rather, they are saying: "The ability of the wave theory to be successfully extended to new phenomena vastly *enhances* its credibility". Achinstein does not want them saying the latter, since his epistemic apparatus has no resources for making sense of such claims. But I think there can be no doubt but that is what they were claiming, justifiably or not.

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