Waves, Philosophers and Historians

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An odd sense of dissonance comes over me as I listen to the eloquent and eminently reasonable arguments presented by Professors Achinstein and Laudan. Nothing similar to either of their remarks would likely be heard today at a history of science meeting, much less at a convention, say, of the Modern Languages Association. Historians would be much more likely to ask what interests were served by the particular rhetorical characters of Whewell's or Brougham's or Mill's or even Young's or Fresnel's arguments. They would be deeply skeptical that arguments per se had anything much to do with the contagious expansion of wave methods during and after the 1830s. In their respective ways, and despite the obvious differences between them, Professors Achinstein and Laudan both feel that argumentation was a central aspect of the historical events involved in the establishment of wave optics. They differ primarily over whether there was a change in the nature of argument. Laudan asserts there was; Achinstein insists there was not. What I want to discuss is whether argumentation did much historical work at all — whether, that is, anyone ever actually persuaded anyone else to change a belief. I will however turn in my concluding remarks to the points that are at issue between Laudan and Achinstein.

It is certainly immensely refreshing to be confronted with the clarity and certainty of philosophical assertion. Historians these days love words like rhetoric, problematic, symmetry, interests and so on and revel in the essential uncertainty that these words inevitably convey. The hard brightness that confidence in the efficacy of argument shines on soft, murky history is attractive. But, in the case of wave optics at least, it seems to me to be a deceptive illumination. To put my point rather too crudely and bluntly, I do not think that anyone at the time was substantially swayed by the arguments deployed by wave propagandists like Whewell or Lloyd, whatever the precise nature of these arguments might have been. These public, overt, highly self-conscious arguments skimmed only the surface of the many differences between wave partisans and their predecessors; they did not reach to the core of what was at issue between the contending factions. The battle concerned subtle, complex issues that were rarely addressed overtly. Indeed, to the extent that the word "battle" suggests something whose main outlines are well delineated and obvious to all participants it is a misnomer.[1] For to become a wave partisan required a great deal more than accepting the wave theory; it required the understanding and adopting of wave methods on

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paper and in the laboratory, methods that differed utterly from the ones associated with ray methods.

This is not the place thoroughly to rehearse a distinction that has elsewhere been drawn at some length (Buchwald, 1989) but the essence of it can be conveyed reasonably simply. My use of the phrase pairs "wave methods" and "ray methods", rather than "wave theory" and "ray theory", suggests the distinction. Wave partisans deployed as basic resources the concept of a wave front with the associated notion of phase. In this scheme optical rays had to be constructed in a rather complex fashion as physically-secondary objects that had the same kind of identity as a geometrical line, i.e. such things could not be counted. As a result of the signal importance of phase and front, the mathematics deployed by the wave partisan involved intricate trigonometric decompositions and recompositions, as well as integrations over surfaces. Ray partisans, in marked contrast, deployed as basic resources the concept of an individually existent ray, something that formed a bundle of light by grouping with other rays. In this scheme the ray was the central physical object, and appropriate mathematics involved ray-counting, which in practice often required elaborate differential methods since the rays collected naturally in densely populated sets.

Deployers of wave methods for the most part simply did not understand ray methods, and vice versa, by which I mean that each group did not know how to solve the other group's problems using the opposing group's methods. There is a great deal of evidence available to substantiate this claim, and if it holds up then the question of persuasion takes on a rather different character. Unless x and y, though partisans of different methods, are capable of using the other's scheme with full understanding, or are even willing to try doing so, then what precisely could each possibly persuade the other to accept? Unless x can show y by using y's own methods that y's scheme does not work to solve a particular problem, or is in some way that y feels compelled to accept inadequate to the task, then y will continue to feel reasonably secure. Possibly y might simply collapse in silence, snowed under as it were by an opponent's vituperative and unrelenting attacks, unable to mount an equally vicious counter-offensive, convinced nevertheless that the opponent simply did not understand y's claims. This actually did happen in the case of a controversy between Biot on the one hand and Fresnel and Arago on the other. But this is hardly persuasion; it is more like conquest. Even it did not happen to any great extent.

During the 1830s and 1840s wave methods in optics did substantially replace ray methods, by which I mean that by the end of the decade in Britain and France, and to a lesser extent in Germany (where however optical activity remained comparatively small during this period), the majority of printed papers, archival sources, and laboratory work deployed fronts and phases rather than rays and groups. Did this happen because wave partisans persuaded ray partisans to change their minds, or because they persuaded a new generation to adhere to their scheme? I have found no evidence for such a thing. I do find wave methods during this period beginning higgledy-piggledy to creep into optics texts, with the notion of phase cropping up here and there, often in a confused way, and in the context of instrumentalities that are designed about phase in the first place (such, e.g., as the Fresnel rhomb). By the 1840s the characteristic methods of ray optics have begun to vanish, almost as though they sneaked away sometime during the night.[2] And yet nowhere did anyone in effect say "Yes, Whewell et al. are correct; I used to think otherwise, but I am now convinced that ray methods must give way to wave methods because on the preponderance of evidence the wave theory of light is better than the emission theory of light".

I have used the phrase "emission theory of light" for the first time. Yet most wave partisan attacks against the alternative optics looked directly to the latter's putative foundation in particles and forces. Putative not because I have any doubts at all about the degree to which ray opticians had traditionally bound their practical behavior to this way of thinking, but because the fact is that particles and forces had little work to do beyond providing a secure (and utterly essential) framework for image-building - which was precisely the same work performed for wave partisans by the ether. In both cases the physical models deployed were indubitably essential components of the scheme, but for the most part the models equally indubitably did not have active, working functions to perform, in these senses: they did not structure mathematics or laboratory work; they did not suggest new instrumental methods or kinds of optical things.

It is worthwhile briefly examining one of Humphrey Lloyd's remarks in his long, comparative brief for wave optics in 1834 to see just how wave partisans did argue. He wrote:

> I would observe that any well-imagined theory may be accommodated to phenomena, and seem to explain them, if we only increase the number of its postulates, so as still to embrace each new class of phenomena as it arises. In a certain sense, and to a certain extent, such a theory may be said to be true, so far as it is the mere expression of known laws. But it is no longer a physical theory, whose very essence is to connect these laws together, and to demonstrate their dependence on some higher principle: it is an aggregate of separate principles, whose mutual relations are unknown. Thus the cycles and epicycles of the Ptolemaic system represented with fidelity the more obvious movements of the planetary bodies; but when the refinements of astronomical research laid bare new laws, new epicycles were added to the system, until at length its complication rendered it useless as a guide. Such appears to be the present state of the theory of emission. (1834, p. 349)

This is pretty stiff. The "emission theory", Lloyd asserts, is essentially useless because it cannot act as a "guide" to new physics; this is because "it is an aggregate of separate principles". These principles all directly concern the behavior of optical particles.

I will skip over the Whewellian echoes about guidance here to follow Lloyd a bit further in his comparative argument. According to him wave optics is nothing like the unamalgamated aggregate of emission optics, and this is why it can generate new physics. Among the new physics that Lloyd had in mind was partial reflection and refraction, in which some light striking a transparent body passes through it, whereas other light is reflected from it. Here, he explained, Biot's physics for optical particles, which Lloyd developed in some detail, simply could not yield formulae - nothing useful emerged from it at all. Fresnel was much better. He could obtain formulae, and he could, Lloyd asserted, link them nicely to ether physics.

Let's follow Lloyd here a bit more closely. He remarked:

> In the development of his theory [of partial reflection] the character of Fresnel's genius is strongly marked. Our imperfect knowledge of the precise physical conditions of the question is supplied by bold, but highly probable assumptions: the meaning of analysis is, as it were, intuitively discerned, where its language has failed to guide; and the conclusions thus sagaciously reached are finally confirmed by experiments chosen in such a manner as to force Nature to bear testimony to the truth or falsehood of the theory. (1834, p. 363)

This is a remarkable argument for two reasons. First of all like Fresnel before him Lloyd was apparently completely unaware that ray optics could and in fact did generate formulae for partial reflection; or, to be more accurate, Lloyd (like Fresnel) probably did know that such formulae existed, but he simply could not see where they came from. Such formulae (and others as well) came from ray statistics, as Biot thoroughly understood and (in a different context) kept on objecting by distinguishing between ray optics and the emission theory proper. Wave partisans like Lloyd did not look beyond particles and forces to the tacit apparatus of ray statistics deployed by Biot, Malus, Brewster and others. If proponents of the alternate scheme had formulae, particularly in areas that wave optics claimed for its own, then these formulae had to be a sort of empirical guess, whatever that might be, and as such had no bearing on the debate concerning theory-choice.

But, we see, Lloyd was not only convinced that wave optics provided formulae where the opposing system failed to, it did so in an appropriately guided way. And what was this sagacious guide in the case of partial reflection? Was it perhaps ether physics, the counterpart to inefficacious optical forces and particles? Yes, says Lloyd, it was, or rather it almost was. Fresnel had unfortunately not been able to develop an appropriately working ether physics for partial reflection, but he did come up with some wonderful boundary conditions that produced nice formulae. And how did he come up with these conditions? Why, from his remarkable intuition. In other words wave physics could produce good formulae where particle physics could produce nothing because wave partisans had much better physical intuition about the ether than their opponents had about optical particles and forces!

There is more. Ray optics not only produced formulae for partial reflection, despite the unfortunate poverty of intuition among its partisans, but these formulae were just as good in the laboratory as were wave formulae at the time. No contemporary instrument, no cleverly-structured device of the day, could tell the difference between them. Lloyd's brief here for the power of the wave theory, and for the intuitive sagacity of its partisans, could not possibly have convinced anyone on the other side who had spent much time engaged in research, and in fact it did not do so.

Of course one can always refer back to diffraction. Here again, however, arguments directed against particles and forces carried very little weight except among the already-convinced. First of all, oddly-behaved forces were everywhere in late 18th-century physics, particularly in France, including capillarity and thermal physics. Optics was hardly alone in requiring such things. The fact that diffraction patterns did not depend on material (they certainly depend on aperture or edge shape) merely constituted a point of research potential (or might have done had the wave contagion not been so successfully spread during the 1830s and 1840s). Secondly, and of even greater importance, ray opticians simply accepted periodicity as an as-yet-unexplained property of optical rays; most did not, in other words, also adopt wave fronts. But what then of Fresnel's use of Huygens' principle, which seems to require fronts? In fact even here - and even by someone as avid, indeed vicious, a partisan of Fresnel as Arago was - rays, not fronts, were used; the principle provided a convenient locus from which to draw rays. This interpretation was greatly furthered by the fact that Huygens' principle was thought to lack meaning even for mechanical waves, in which case Fresnel's use of it could hardly be construed as a support for their optical counterparts. The formulae seemed to work well, but their foundation remained utterly obscure (and were to remain so for many years, until Huygens' principle was turned into a mathematical artifact of the scalar wave equation by Helmholtz and Kirchhoff).

It seems to me that in this context Mill's remarks on the ether, which Professor Achinstein referred to, make a great deal of historical sense, at least insofar as they concern the physical foundation of wave optics, and to the extent that we extend them symmetrically to the alternative physics of particles and forces. They make historical sense because the fact is that ether physics in Mill's time was no more, or less, successful in doing practical work than was emission physics. On the other hand it is not clear to me that Mill knew this; he seems to have thought that ether optics could in fact do a lot of successful work in the laboratory, in which case his dismissal of it seems to me to be a rather deep misconstrual of what his contemporaries were up to in physical science, since a physics that was used successfully and repeatedly to produce new, stable laboratory gadgets and kinds of optical things carried immense social prestige among scientists then, as it does today and as it has since sometime in the late seventeenth century, when the laboratory became a primary organizing locus for the kind of work that we think of as science.

Practicing scientists, whether paper workers, laboratory investigators, or a combination of the two deploy tools in order to solve problems that are firmly fixed in a social matrix. Sets of tools that give everybody lots of satisfying, nicely-rewarded and highly approved work to do without the resulting mechanisms falling apart make people happy; they satisfy critical social desiderata for the doing of science. Many other essential things enter here, but remove tool-production and deployment and you also remove anything like science-work. During the 1830s wave partisans produced practical paper tools, and some laboratory tools as well, which enabled them to establish a dynamic research tradition, within which ether physics took its place as only one element. Wave partisans, for reasons that did not have much to do with comparative abstract superiority, also controlled important journals and built a close network of like-minded people through university and (especially) professional associations. Ray partisans simply did not do anything at all like this; they did not even try to do so. Was this because of the abstract logical inferiority of their scheme? Wave partisans would certainly have said so; ray partisans would (and in fact did) have vigorously denied the claim.

By the 1840s an entire universe of wave devices was being generated, one in which the instruments themselves were increasingly built around the behavior of front and phase. This universe of devices offered no point of entry to the ray physicist, for whom front and phase had no fundamental significance. By then, which was quite some time after the large majority of work in optics had shifted to wave methods, ray physics might be said to be objectively weak in comparison to wave physics. But this is rather like saying that a screw driver is more useful than a hammer in a universe held together by screws. Can it with certainty be said that ray physics simply would not have generated (to continue the analogy) a working universe held together by nails? John Herschel for one thought that it might have done so, had it been pursued with the vigor of wave physics. We think not, but then we live in a world populated by a myriad of wave-based devices. In any case my point is not to effect an abstract comparison between the two schemes, but rather to insist that the historical events do not support the claim that the wave contagion was spread by anything like argument.

I want to retreat a bit from what many will construe as typical historicist relativism. As a historian I do adopt a sort of professional agnosticism, since that is the only way to be certain that you do not import irrelevant future events into the past. Yet I am in fact reasonably confident that wave methods are much superior to ray methods, by which I mean that, despite John Herschel's early opinion, I think it extremely unlikely that the latter would have generated its own working universe of paper and laboratory devices. But I do not think that this is because ether physics is somehow superior to particle physics. On the other hand I do think that wave optics proved to be more powerful than ray optics. This was however hardly obvious during the years when wave methods spread rapidly and so cannot be used to account for their initial domination.

In conclusion I would like to make a few remarks concerning the specific point at issue between Professors Achinstein and Laudan. Or, better put, I would like to examine a small part of the terrain that they cover. Did wave partisans deploy some form of hypothetical method in a way that ray partisans did not? This is not a simple question to answer, because it depends on what hypotheses you have in mind.

If we say that optical rays are somehow to be thought of as essentially unhypothetical entities, then we might also want to say that ray partisans did not reason hypothetically, that, instead, they thought that they were describing the behavior of collectivities of unhypothetical things, namely rays. This is in fact rather close to what Brougham, Biot and others did think. But wave partisans certainly did not think that rays qua individuals were unhypothetical, because they did not think that such things existed in that sense at all. Consequently for them descriptions of ray-groups immediately and necessarily translated into hypothetical assertions concerning particles and forces. Ray partisans vigorously and angrily resisted the translation, but they were unable explicitly to convey their grounds for resistance.

It seems to me that, given this, Achinstein and Laudan are in a sense both correct (though I invert here Achinstein's concentration on the inductive-basis of wave arguments). Achinstein is correct because ray partisans were every bit as hypothetical in their use of rays as were wave partisans in their use of waves. Just consider, for example, the respective ray and wave discussions of the colored rings formed when polarized light, having passed through a thin slice of crystal, is then analyzed. Both discussions are intensely hypothetical; one makes use of hypotheses concerning the redistribution of rays into groups; the other uses suppositions concerning phase differences.[3] But Laudan is also correct because wave partisans deployed ether considerations in a much denser fashion than ray partisans had ever deployed particles and forces.[4] From my admittedly historicist point of view Achinstein's stand seems to be a bit stronger than Laudan's, because I see rather a change in the detailed use of hypotheses concerning micro-structure than I see a change in the nature of the argument itself.[5] Brougham and others fervently embraced anti-hypothetical arguments as a rhetorical maneuver grounded in their belief that rays were entirely unproblematic things. The very large amount of ether-talk deployed by wave partisans (and that was itself, in origin at least, a rhetorical maneuver) provided a nice, visible target which, ray partisans felt, could not be turned back against them because they rarely spoke about particles and forces.

Notes

[1]Though military historians would certainly not agree that battles have anything like this clarity.

[2]Chen and Barker, 1992 show however that ray-partisans Brougham, Brewster and Potter continued trying to destabilize wave constructs by fabricating counter-vailing experiments. Wave partisans did respond to these attacks, but there is nowhere the slightest indication that their dominance or confidence ever diminished in the slightest on this score. Indeed, the character of their response shows just how confident they remained since in one instance at least a possible problem was dismissed by claiming that the experiment fell outside the bounds of wave computability and therefore need not be accommodated at all.

[3]One can of course go on to discuss whether the suppositions used by wave partisans were more thoroughly grounded than were the suppositions used by ray partisans. In the case of ring formation neither group had a clear advantage in this respect. Ray scientists were able directly to translate certain canonical characteristics of the rings into characteristics of the ray groups. From this they could predict, quite accurately as it turns out, other aspects of the effect (given the instruments then in use). Wave scientists had also to use parameter-determining observations, from which they could in turn calculate other aspects of ring-formation. It is on the whole otiose to engage in minute, normative comparisons here.

[4]Though particles and forces *were* used explicitly to structure aspects of ray optics during the eighteenth century (Pedersen,1980).

[5]Although the fact remains that no one working in wave optics ever generated a novel result that could be realized in the laboratory from computations founded directly on ether structure. This does not however detract in any way from the suggestive power of ether physics, since considerations derived, e.g., from such notions as variable elasticity or density did suggest new mathematical routes. The closest ether theorists ever came to laboratory novelty involved dispersion, and even here there was a great deal of contentious argument concerning just what they were able to produce (Buchwald 1979).

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