How to Remain (Reasonably) Optimistic: Scientific Realism and the "Luminiferous Ether"

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1. Laudan on the "Non-Referring" but Empirically Successful "Optical Ether"

In the course of his forceful (1981) attack on "convergent realism", Larry Laudan attempted to turn an influential pro-realist consideration on its head. Scientific realists have wondered how a theory could enjoy the sort of empirical predictive success exhibited by presently accepted theories in the "mature" sciences, and yet be radically wrong in what it claims is going on "behind" the empirical phenomena. If nothing like the electrons and other particles postulated by current physics exists, how can the theories that there are such things have made the great range of successful predictions that they do about phenomena observed in particle accelerators and the like? Laudan directs such realists to the history of science and to a list of "once successful" theories which are "(by present lights) non-referring"—a list of successful theories which gave a "central" role to notions that, according to theories we accept *now*, do not exist (1981, 26). He supposes, apparently quite reasonably, that, whatever account of "approximate truth" the realist relies on (and, notoriously, there is as yet no generally accepted formal account on offer), that account will entail that a theory involving central terms with no referent cannot be "approximately true". (But see section 4.) He claims, then, that, in view of the history of science, this realist consideration is *self-defeating*. If, as the realist recommends, we hold that presently accepted theories (in the "mature" sciences) are approximately true, then it follows that these earlier theories *were* radically false, despite their predictive success. The empirical success of presently accepted theories can, then, hardly be used as an argument for their approximate truth.

Laudan's argument seems especially strong because it hits, not at particular realist arguments, but directly at the realist's *general*, underlying intuitions. Laudan himself later described his (1981) as having

> challenged the intuitions which motivate the realist enterprise by arguing .. that many (now discredited) scientific theories of earlier eras exhibited an impressive sort of empirical support, arguably no different in kind from that enjoyed by many contemporary physical theories. Yet we now believe that many of those earlier theories profoundly mischaracterized the way the world really is. More specifically, we now believe that there is nothing in the world which even

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> approximately answers to the central explanatory entities postulated by a great many successful theories of the past (1984, 157)

Laudan's argument is, however, only as strong as the quality of his historical examples of "once successful but (by present lights) nonreferring" theories. Some of the entries on his list (which he insisted could be extended "*ad nauseam*") are in fact strikingly unimpressive. He must have been working with some very loose notion of scientific "success" in order to count, for example, the vapid, truly etherial, conjectures of Hartley and LeSage even as having "enjoyed some measure of empirical success" (1981, 27). The argument needs examples of theories which were successful in the genuinely *predictive* sense. We know that *any* theory can be made to have correct empirical consequences by "writing those consequences into" it; the cases that have traditionally induced realist-inclinations in even the most hard-headed are cases of theories, designed with one set of data in mind, that have turned out to predict entirely unexpectedly some further general phenomenon. Laudan himself gives special emphasis to the one item on his list that seems unambiguously to fit this bill — Fresnel's wave theory of light and its associated elastic-solid "luminiferous ether".

It would be difficult to argue that Fresnel's theory counts as "immature" science; and *impossible* to deny that it was impressively successful predictively. Aside from the much-rehearsed case of the light spot at the centre of the shadow of a small opaque disc (for the real story of this case see my 1989), Fresnel's theory of the wave surface inside biaxial crystals turned out to predict the existence of *internal* and *external conical refraction*. Fresnel never realised that these latter predictions follow from his theory; and the phenomena themselves were not even thought of, let alone known to occur, until, after Fresnel's death, they were derived from the theory by William Rowan Hamilton (1833) and confirmed experimentally by Humphrey Lloyd (1833).

So there is at least one theory on Laudan's list that was unambiguously successful. And, so Laudan asserts, that theory "centrally" involved a notion denied any real referent by theories *subsequently* accepted in science. Within Fresnel's theory "the optical ether functioned centrally in explanations of reflection, refraction, interference, double refraction, diffraction, and polarization"(1981,27); but Maxwell's theory and then the General Theory of Relativity entail that there is just no such thing as Fresnel's elastic optical ether. According to these later theories, light "in fact" consists of vibrations of the electromagnetic field, a field which is "*sui generis*", explicitly *not* a manifestation of the contortions of some underlying material medium. (As is well known, Maxwell himself tried hard to produce a "mechanical model" for the field—that is, to explain the field in terms of some underlying material medium. But his failure in this attempt, and the failure of his contemporaries and successors, led to the acceptance of what might be called the "mature" version of Maxwell's theory— a theory that sees the field as a "primitive" part of the furniture of the universe.)

The case of Fresnel's theory of the elastic ether poses, then, *via* Laudan's argument, a sharp challenge for the realist—a *prima facie* strong historical reason to be "pessimistic" about the likely fate of *currently* accepted theories. Two realist responses to the challenge are possible: (i) deny that the ether played a "central" role in Fresnel's theorising, and (ii) deny that the ether has in fact been rejected by later science. Both of these strategies have found interesting particular instantiations in the recent literature—in Hardin and Rosenberg (1982) and in Kitcher (1992). Both accounts see the two, apparently quite different, strategies as in fact closely interrelated.

I argue (in sections 2 and 3 respectively) that neither realist strategy succeeds. Each does, however, point to important features of the logical relationship between

Fresnel's theory and the theories that later replaced it. In section 4 I argue that, when that logical relationship is properly described and its consequences fully drawn out, although this case of a theory-shift *is* inconsistent with scientific realism as normally construed, it "confirms" a view that has been called *structural realism.* This was first developed by Poincaré (and is very different from the sort of anti-realism that is usually attributed to him). In fact, Poincaré—who fully anticipated the "pessimistic induction" argument—used exactly this historical case of the shift from Fresnel's elastic ether to Maxwell's electromagnetic field theory as the chief illustration of his general view of the aim and status of theories. Structural realism encourages an *optimistic* induction from the history of theory-change in science, but an optimistic induction concerning the discovery of *mathematical structure* rather than individual ontology.

2. Did the "Luminiferous Ether" Really Play a Central Role in the Success of Fresnel's Theory?

It can't, I think, sensibly be denied that Fresnel *believed* in the the ether as a real, material medium. He refers to such a material medium explicitly (and in explicitly "realist" terms) at various points in his scientific work. For example, in his famous (1818) "Prize Memoir" on diffraction, he characterised the general problem of diffraction as follows : "Given the intensities and the relative positions of any number of systems of light waves of the same wavelength, propagating in the same direction, to determine the intensity of the vibrations resulting from the concourse of these different systems, that is to say, the velocity of the oscillations of the molecules of the ether" (248). In his (1822, 136) he stated that the properties of polarised light are simply explained on his theory "by supposing that, in light waves, the oscillations of the molecules of the ether are executed at right angles to the rays".

It doesn't however follow that the ether played a "central role" in his theory. Indeed the standard view in history of science—following Whittaker's influential treatment—is that Fresnel's account of ether-dynamics, was pure window-dressing: whatever Fresnel's own beliefs, the heuristic impetus for his theoretical work came from mathematical considerations—the mechanical-dynamical considerations attached themselves only later, only unsuccessfully and certainly without any independent empirical success. Charting Fresnel's route to his theory of the wave-surface in birefringent crystals, Whittaker remarked (1951,119):

> Having ... arrived at his result by reasoning of a purely geometrical character, [Fresnel] now devised a dynamical scheme to suit it.

(A similar claim that Fresnel's ether played no "generative" role in the development of his theory of polarisation is made in Buchwald 1989.)

No realist should advocate a "realist attitude" towards *all* theoretical claims—even theoretical claims within successful theories. Some play no effective role and are, in Kitcher's (1992) terminology, "presuppositional". (Newton's assumption that the centre of mass of the universe is at absolute rest is surely a case in point.) If the standard story were correct, then the realist would be justified in claiming that, since the material ether was a "merely presuppositional posit", and since he holds no brief for such notions, the eventual rejection of that material ether represents no threat to his position.

The claim that the ether was "central" figures as a bald, unsubstantiated assertion in Laudan's paper unaccompanied by either analysis or argument. Nonetheless it is, I believe, correct or at any rate correct enough to block this escape route for the realist. I cannot argue the case fully here, but detailed analysis (forthcoming) shows that the

suggestion that the material ether was an idle component in Fresnel's system is significantly misleading. Whittaker claims, for example, that "geometrical reasoning" led the way in Fresnel's development of the wave surface in birefringent crystals. However, this "geometrical reasoning" itself did not spring from nowhere, but was based on Hooke's law, Huygens's principle, the principle of superposition ("coexistence of small movements") and other assumptions of a *general* mechanical kind. Moreover, although Fresnel's extension to cover all crystals of Huygens's famous sphere/spheroid construction for the two refracted beams can be characterised mathematically as a process of putting two equations together (by introducing three parameters for Huygens's two), that process was in turn undoubtedly guided by Fresnel's "realist" belief that there could only be one light-carrying medium and the "natural" assumption that, in the general case, the coefficients of elasticity of that medium in the three orthogonal directions in space will be different. Fresnel *did* get some important heuristic mileage out of certain general mechanical-dynamical ideas concerning some sort of mechanical medium with some sort of vibrating parts.

Whittaker and others are, I believe, wrong: Fresnel did *not* first operate geometrically and only later "interpret" his theory dynamically. Rather than two separate stages, Fresnel was, I think, working all the time with a mix of basic mechanical ideas, mathematics and known experimental results. (This is surely generally true in theoretical physics.) He arrived at an account of light as a wave motion in a mechanical medium possessing certain *general* properties in accord with the known principles of dynamics. This account was dramatically successful empirically. He (and his successors) made various attempts to *strengthen* the account into a fully-fledged dynamical theory in which everything would follow from a "natural", "unified" account of the forces operating on the ether particles. These attempts were, however, immediately refuted (or if you prefer, immediately ran into some notable and stubborn anomalies).

The problems with the attempts to strengthen the theory of the wave surface into a fully-fledged dynamical theory of the ether are well-known: the most famous being the problem of the longitudinal wave. Although Fresnel continued to talk about the etherial "fluid" even after he became convinced that the oscillations of the medium are *transverse* to the direction of propagation of the wave, he was fully aware that transversality meant that the medium had to have resistance to *shear* (and this makes it, at least according to later terminology, unambiguously an elastic *solid*). "Ordinary" elastic solids are resistant *both* to shear *and* to compression, and hence transmit *both* transverse *and* longitudinal waves. Fresnel took it that his and Arago's experimental results on the interference of polarised light establish that no longitudinal component plays any role in optical effects. What happens to the longitudinal wave in the elastic solid ether? Fresnel simply hypothesised it away. He assumed that the ether must be infinitely (or "near infinitely") resistant to compression so that the longitudinal wave travels at infinite or "near infinite" velocity and so can somehow be ignored. Unfortunately hypothesising the longitudinal wave away once is not enough: even when you have got, or rather have given yourself, a purely transverse wave, it ought on mechanical principles to develop a longitudinal component again whenever it meets the boundary of a medium of a different optical density. So you have to *keep on* hypothesising away such longitudinal components. That is, you have to keep on violating the laws of mechanics.

The intractablity of this and other problems and their role in leading eventually to a replacement theory is what lies behind Whittaker's treatment (and those of Hardin and Rosenberg and of Kitcher). But this represents illicit use of hindsight: there was, of course, no reason to believe back in 1821, say, that those problems would prove intractable. There certainly was reason in 1821 (when Fresnel completed his paper on the

form of the wave-surface in biaxial crystals) to regard the ether as problematic. But—as is now generally recognised following the work of Kuhn, Lakatos and others—the highest-level, most interesting parts of theoretical science are invariably problematic, when examined closely. If the realist is in the business of advocating a realist attitude only toward entirely unproblematic theories, then realism is indeed restricted in scope.

If my claims about Fresnel's theoretical work are at all correct, then the realist still faces a threat from this case, and cannot convincingly hide behind the problems that Fresnel's basic theory of the ether undoubtedly faced. The elastic-solid ether played a problematic, but nonetheless somewhat positive role, so the realist had better have *something* positive to say about it.

3. Was Fresnel Talking about Electromagnetic Waves all Along?

Hardin and Rosenberg (1982) make the, at first glance audacious, claim that the "something positive" the realist can say about the ether is that, contrary to Laudan, we still believe it to exist: the realist can justifiably view Fresnel as talking about the electromagnetic field all along when he used the term "ether". They concede (p.611) that this account of reference "severs it from the detailed beliefs of" Fresnel. Indeed, this is hardly a question of detail: Fresnel could scarcely have believed that he was referring to an entity which was first thought of only some decades after his death! Hardin and Rosenberg point out—citing the problems faced by the elastic ether —that various features of Fresnel's accounts of optical phenomena undoubtedly sit more easily with Maxwell's field theory than with his own notion of an elastic solid. And they argue (613-4) that, given this and given the "continuity of causal role" stretching from Fresnel's ether to Maxwell's field and beyond, a realist can reasonably regard Fresnel as referring to the electromagnetic field when he used the term "ether":

> Looking back across the range of theories from Fresnel to Einstein, we see a constant causal role being played in all of them; that causal role we now ascribe to the electromagnetic field. One permissible strategy of the realist is to let reference follow causal role. It seems not unreasonable, then, for realists to say that 'ether' referred to the electromagnetic field all along.

But in fact, the causal role is only "constant" if we ignore certain inconvenient features of the earlier theory: for example, the elastic solid ether *ought* to have had the effect of slowing down the planets as they moved through it. If we are allowed to be similarly selective in other cases, there seems equally to be a "constant causal role" between, say, Aristotle's notion of a body's desire to be in its natural place and Einstein's notion of a body moving along a geodesic in curved space-time; or between the seventeenth century notion of a witch and twentieth century notions of sufferers from mental illnesses of various kinds. If it is "not unreasonable ... for realists to say that 'ether' referred to the electromagnetic field all along", it seems equally to fail to be unreasonable to say that "desire for its natural place" refers to "necessity to move along a geodesic" or to say that "witch" (sometimes) refers to "sufferer from certain kinds of mental illness". The causal role in explaining certain kinds of "odd" behaviour that was attributed in the seventeenth century to a person's being a witch is certainly now attributed to mental afflictions of various kinds. Laudan makes much the same point in his (1984)—taking it to be a severe embarrassment for Hardin and Rosenberg's account (whether or not it really is an embarrassment will be considered in section 4).

Philip Kitcher's (1993) treatment of this issue has a similar theme, though with interesting variations. Kitcher concedes that Fresnel's term "ether" fails to refer, but suggests (1993, 147) that Fresnel's tokens of the term "light wave" nonetheless *do*

refer. Fresnel undoubtedly believed that such waves were waves in his elastic medium but, so Kitcher claims, since Fresnel's "dominant intention" was "that of talking about the wavelike features of light, *however they happen to be realized*", it is reasonable, in view of what science now tells us, to take tokens of that term (or perhaps most of them) as "genuinely refer[ring] to electromagnetic waves of high frequency" (146).

So far as the ether itself is concerned, Kitcher asks if "the schemata employed by Fresnel and other wave theorists of the early and middle nineteenth century contain *ineliminable* commitments to the ether?"; and, concluding that they do not, infers that "[t]he ether is a prime example of a presuppositional posit" (1993, 149). I have suggested that the ether was in fact a problematic, but certainly *non*-idle notion in Fresnel's approach. Fresnel got real heuristic assistance from ideas of a *general* mechanical kind. But are those ideas nonetheless "eliminable"? Can't the principle of superposition, Hooke's law and the other assumptions that Fresnel used be cut off from their original mechanical bearings and be re-clothed in the terms of some other theoretical framework?

If this is a question about logical possibilities rather than possibilities practically available to Fresnel and his contemporaries, then the answer is of course "yes". But if "ineliminability" requires that there be *no other* theory that explains the phenomena at issue then no theory, no theoretical notion, is ineliminable. In cases such as this one where we are considering a theory that was later replaced, no recourse need be had to abstract ideas about "underdetermination" in order to establish this: the replacing theory itself forms a constructive proof of the "eliminability" of the earlier one. Maxwell's theory of the electromagnetic field shows that the role that Fresnel believed could only be played by a material medium can also be played by the field. A realist committed to a realist attitude toward only ineliminable notions faces no problem from the history of science, but then he has no position to defend. In order to count as any sort of realism, a position must entail *something* positive about Fresnel's ether despite its "eliminability".

4. Fresnel and Maxwell: the Ether, the Field and "Structural Realism"

The above remarks notwithstanding, there is in much that I agree with in the positions of Hardin and Rosenberg and of Kitcher. Both accounts point to important features of the relationship between Fresnel's ether theory and its successors. However neither account gets this relationship quite right because each seeks to defend a stronger version of realism than is, I believe, really defensible.

It is, so the realist wants to claim, vastly improbable that a theory should score the sort of extensive empirical success scored by presently accepted theories in the "mature sciences" and yet not have *somehow* "latched on to" how things are "underneath" the empirical phenomena. To defend this intuitive claim against cases of theory-change, the realist needs to show that, from the point of view of the later theory, the fundamental claims of the earlier theory (in so far as they played integral roles in that theory's empirical success) were—though false—nonetheless in some clear sense "approximately correct". He needs to show that, from the point of view of the later theory, we can still *explain* the success enjoyed by the earlier one.

A natural assumption is that such an explanation requires a demonstration *either* that the parts of the earlier theory rejected by the later one were redundant *or* that no real "rejection" was involved (but only a "re-description"). However, in this particular historical case at least, the most straightforward and least revisionary account of the explanation of the success of the earlier theory provided by its successor fits neither of those patterns.

For convenience (and temporarily) freeze the history of science at the point where the "mature" (non-medium-based) version of Maxwell's theory had been accepted. From that vantage point, there is an easy explanation of the success of Fresnel's elastic-ether theory of light—one which requires no Whiggish "reinterpretation" of Fresnel's thought. From the later point of view, Fresnel clearly misidentified the *nature* of light, but his theory nonetheless accurately described not just light's observable effects but also its *structure*. There is no elastic-solid ether of the kind Fresnel's theory (problematically but nonetheless importantly) involved; but there is an electromagnetic field. The field is not underpinned by a mechanical ether and in no clear sense "approximates" it. Similarly there are no "light waves" in Fresnel's sense, since these were supposed to consist of motions of material ether-particles. Nonetheless disturbances in Maxwell's field do obey *formally* similar (in fact, and unusually, mathematically identical) laws to some of those obeyed by the "materially" entirely different elastic disturbances in a mechanical medium.

Unless—surely very much in the spirit of *anti*-realism—we think of these theoretical notions as characterised by their observable effects, then we have to allow that Fresnel's most basic ontological claim that the vibrations making up light are vibrations of real material ether particles subject to elastic restoring forces was entirely wrong. A displacement current in a *sui generis* electromagnetic field and a mechanical vibration transmitted from particle to particle are more like "chalk and cheese" than are real chalk and cheese. But if Fresnel was as wrong as he could have been about *what* oscillates, he was right, not just about the optical phenomena, but right also that those phenomena depend on the oscillations of something or other at right angles to the light. His theory was more than empirically adequate, but less than true; instead it was *structurally correct*. There is an important "carry-over" from Fresnel to Maxwell, one at a "higher" level than the merely empirical, but it is a carry over of *structure* rather than content. Both Fresnel's and Maxwell's theories make the passage of light consist of wave forms transmitted from place to place, forms obeying the same mathematics. Hence, although the periodic changes which the two theories postulate are ontologically of radically different sorts—in one material particles change position, in the other field strengths change—there is nonetheless a structural, mathematical continuity between the two theories.

All this is reflected in the fact that if you perform the following meta-level operation on Fresnel's theory you "turn it into" a genuine sub-theory of Maxwell's:

> Go through Fresnel's theory and, wherever he talks about a molecule of the ether's being forced away from its equilibrium position, replace that talk by talk of a forced change in the electromagnetic field strength.

(Another way to put this is that if you go through Fresnel's theory and replace the notion of a molecule's being forced from its equilibrium position by a theory-neutral term such as "optical disturbance" ; and then *reinterpret* "optical disturbance" as "forced vibration of the electromagnetic field" then what you get is a sub-theory of Maxwell.)

Nothing "Whiggish" is being perpetrated here: I do not assert—indeed I explicitly deny—that this is what Fresnel's *theory* "really" amounted to "all along". What you get as a result of this process is *not* Fresnel's theory but a structurally identical *facsimile* of it. But it's the fact that this facsimile is entailed by the later theory that explains why, from the vantage point of the later theory, the empirical predictive success of Fresnel's theory was no lucky accident.

This account, in terms of the "ontological" falsity, but structural correctness of Fresnel's theory, might appear insufficiently different from those of Hardin and Rosenberg and of Philip Kitcher to justify the fuss. But let's think through what their claims that Fresnel was "really" talking about the field or waves in it "all along" really mean. If those claims mean just that the entity that "really" plays the causal role in producing a given range of phenomena that Fresnel attributed to a highly attenuated elastic medium is—*according to the science of the later nineteenth century*— the electromagnetic field, then it is of course no more than the truth. (Just in the same sense that Aristotle was—according to twentieth century science—referring to bodies moving along geodesics in spacetime when he talked about bodies seeking their natural place.) But the conditional character of such judgments is, of course, crucial. The judgments are always theory-dependent. If we ask in turn what is the "real" referent of Maxwell's term "electromagnetic field", the question can again only be answered within the context of the theories accepted at some given time. The answer according to *currently* accepted theories is a quantum field carrying probability waves—a notion radically different from anything envisaged by Maxwell himself, perhaps even more radically different from it (if we can make sense of such comparisons) than Maxwell's own notion of field is from Fresnel's notion of the elastic ether. (It seems to be a historical accident —of no more than conventional significance—that science happens to continue to use the word "field" whereas, *on the whole*, it has dropped the term "ether".) Since the "continuity of causal role" to which Hardin and Rosenberg appeal now extends beyond Maxwell, presumably they must allow that what Fresnel was *really* "really" referring to "all along" was the quantum field carrying probability waves. *Or rather* —since this theory may of course itself eventually be replaced—that we don't know what the real "real referent" of Fresnel's notion the "ether" is. What we do know is that there was a certain structural or syntactic continuity between the *theories* of Fresnel and Maxwell (and again—though this time involving the "correspondence principle"—between Maxwell's and the quantum theory of "the" field).

It was exactly this point that Poincaré had in mind when he said that both Fresnel's notion of an elastic vibration and Maxwell's notion of displacement current are

> merely names of the images we substituted for the real objects which Nature will hide for ever from our eyes (1905, 162)

Poincaré insisted that adopting this view

> cannot be said [to amount to] reducing physical theories to simple practical recipes [i.e. to instrumentalism]; [Fresnel's] equations express relations, and if the equations remain true [better; "are preserved in the later theory"], it because the relations preserve their reality ....The true relations ....are the only reality we can attain. (*ibid*)

Poincaré did not think of himself as proposing a *restriction* of a stronger view about theories and their relation to the world, but rather as pointing out that, *in view of the fact that in science we can never "get outside" of our theories but only view reality through those theories we currently accept*, this structural version is the only view (the only version of "realism") that makes any sense.

If the switch from Fresnel to Maxwell is typical (and I have given no reason in this paper to think it is), then—against the currently fashionable "pessimistic induction"—there are (inductive) grounds for optimism, optimism that science is progressing towards a correct account of the universe, but that progress is at the structural, rather than the "ontological" level. If Poincaré is correct, then any feeling that this is

less optimism than we could reasonably expect is based on a surely mistaken (but easily adopted) view that we can somehow have direct access to the furniture of the universe, unmediated by our theories.

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