Is Structural Realism the Best of Both Worlds?*

by Stathis Psillos**

(...) And God said,

$$\text{curl } H = 4\pi j/c + \varepsilon/c \; dE/dt$$
$$\text{curl } E = -1/c \; dB/dt$$
$$\text{div } D = 4\pi\rho$$
$$\text{div } B = 0$$

and there was light!

(paraphrase of Genesis, 1.1, verses 3-4)

Abstract

In a recent series of papers, John Worrall has defended and elaborated a philosophical position – traced back to Poincaré – which he calls structural realism. This view stands in between scientific realism and agnostic instrumentalism and intends to accommodate both the intuitions that underwrite the 'no miracles' argument for scientific realism and the existence of scientific revolutions which lead to radical theoretical changes. Structural realism presents itself as the best of both worlds. In this paper I critically examine the epistemic status of structural realism, and argue that it is not the best of both worlds. Yet, I stress that it reveals an insight which, properly understood, can cast new light on the debates over scientific realism.

1. Introduction

The debate over scientific realism has been overwhelmed by two important arguments which pull in distinctively contrary directions. On the one hand, there is the 'no miracles' argument which seems to make scientific real-

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ism the most plausible theory for the cognitive status of scientific theories. According to this argument, the astonishing predictive success of mature scientific theories cannot be the product of chance, or cosmic coincidence. Rather, it shows that, by and large, scientific theories are on the right track, or, approximately true. As Putnam put it: "the positive argument for realism it that it is the only philosophy that does not make the success of science a miracle." [1] (1975, 73)

On the other hand, there is the argument from the pessimistic meta-induction, which purports to establish that scientific realism is an implausible account of the cognitive status of scientific theories. According to Laudan, who put forward this argument, "(a)s even a brief glance at the history of science will show, there are many theories which have been highly successful for long periods of time (e.g., theories postulating spontaneous generation or the aether) which clearly have not been approximately true in terms of deep-structure claims they have made about the world". (1984, 91, also 1981, 230-3)

This argument, apparently, establishes that "many theories which have been strikingly successful are evidently not approximately true" (Laudan, 1984, 92). Hence, by a simple induction on past scientific theories, the argument can be used to establish that we must remain agnostic as to the approximate truth of current scientific theories, despite their stunning empirical success. In a stronger formulation, the argument intends to undercut any realist aspiration about the truth of scientific theories. For, if most past successful scientific theories turned out to be false, then, by induction on scientific theories, one must not just remain agnostic, but rather claim that current successful scientific theories are likely to be false. Mary Hesse's 'principle of no privilege' amounts to precisely this claim. Or, as she put it: "our own scientific theories are held to be as much subject to radical conceptual change as past theories are seen to be". (1976, 264)

The debate has gone on for more than a decade without resolution, its only result being that there cannot be a reconciliation between these two positions (cf. Boyd 1981, 1984, 1989, 1990, Leplin 1981, Newton-Smith 1981, Hardin & Rosenberg 1982, McMullin 1984, 1987, Devitt 1984, Fine 1986a, 1986b, Musgrave 1988, Carrier 1991). But recently John Worrall has devoted a series of papers (1989), (1990a), (1990b), to this difficult task. Specifically, he de-

[1] Some years earlier, Smart appealed to a 'no cosmic coincidences' argument to undermine the instrumentalist claim that theoretical commitments (i.e. commitments to unobservable entities) are eliminable in science. (1963, 39)

veloped a novel philosophical position which intends to accommodate both the intuition that there is something correct in the 'no miracles' argument, and the fact that there have been radical changes in our theoretical accounts of the deep-structure of the world. Worrall has dubbed this position 'structural realism' and has traced it back to Poincaré.

Worrall argues that the pessimistic meta-induction is correct in suggesting a radical discontinuity at the theoretical level of scientific theories, that is at the level of the description of unobservable entities, underlying mechanisms and causes of the phenomena. Yet, he is unwilling to admit that the only continuity that there is, as science grows, is that at the empirical level. Hence, he argues for a philosophical theory which, being the best of both worlds, could accommodate radical changes at the high or theoretical level with some more substantial continuity at a level in between empirical laws and theoretical accounts of mechanisms and causes (cf. Worrall, 1989, 111). Structural realism is supposed to be the best of both worlds. For it suggests that most of the mathematical content of preceding mature theories is retained in the succeeding theories, and that this retention marks an important non-empirical continuity in science, while high-level theoretical accounts of unobservable entities and mechanisms change radically. So, in a sense, Worrall wants to establish a philosophical position which saves as much of scientific realism as it is possible, given the existence of scientific revolutions. This position is "the only hopeful way of *both* underwriting the no miracles argument *and* accepting an accurate account of the extent of theory change in science". (Worrall, 1989, 177 – emphasis in the original)

Worrall's position rests on a number of distinctions and dichotomies. He suggests that there is a distinction between the mathematical equations that a scientific term features in and the entity or process that this terms stands for; this distinction gives rise to another one between the form and the content of a scientific theory; the latter distinction, in its turn, must be understood as revealing a dichotomy between the structure and the nature of physical phenomena, processes or entities.

In order to illustrate and vindicate his thesis, Worrall developed a particular case-study – already stated by Poincaré – that is, the retention of the mathematical form of the Fresnel laws in Maxwell's theory. He argues that from Fresnel onwards the identification of the structure of light remained unaltered, whereas there are different theories or descriptions about its nature, i.e. about what light is (cf. 1989, 119). As he put it, "(t)here was a continuity or accumulation in the shift, but the continuity is one of *form* or *structure*, not of content". (1989, 117 – emphasis in the original)

In this paper, I set out to analyze the epistemological underpining of Worrall's position. Although the paper is going to be mostly critical, I try to make some constructive suggestions in the last three sections. I shall deal with three issues: First, in sections 2-4, I shall try to show that, even if we grant a distinction between nature and structure, Worrall's 'best of both worlds' does not really answer the pessimistic meta-induction against scientific realism. Second, in section 5, I shall try to show that there is no sense in which we can establish a physical and epistemic dichotomy between structure and nature. In this connection, in section 6, I shall take on the particular case of Fresnel's laws and shall argue that, on the one hand, it does not support structural realism, and on the other hand, it can be used in defense of scientific realism and against the pessimistic meta-induction, without any recourse to the structure/nature dichotomy. In the final section 7, I shall try to show that there is an important methodological insight in Worrall's suggestion, already pointed out by James Clerk Maxwell, which can be used for a better understanding of the realism debate.

2. *Structural Realism*

Worrall does not give a detailed account of what structural realism is. But, as I mentioned already, this philosophical position was motivated by the recognizable fact that many mathematical equations which express physical laws in the context of fully interpreted past physical theories were retained, either as they were, or as fully determined limiting cases of other equations, in the passing from an old theory to a new one. Examples of this sort can be produced at will: the mathematical form of Newton's laws is a limiting case of relevant laws deduced in the special theory of relativity; the mathematical form of most of the laws of the caloric theory of heat (e.g., the law of adiabatic change, the Carnot-Clapeyron equation, Carnot's law of maximal efficiency of heat-engines *etc.*) is reproduced within thermodynamics; and similarly for other past scientific theories. This fact seems immediately to suggest a type of continuity in theory-change: a continuity at the formal-mathematical level. Yet, in many cases, the full physical interpretation of the mathematical symbols involved in the equations changed radically. Hence, in a sense, while the mathematical form of many laws remained unaltered, their content, that is the physical processes and entities whose behaviour they described, changed. Howard Stein has made this point clear when he stated that:

"What is in fact 'recognizable' is a distinct relationship, from older to newer theory, of *mathematical forms* – not a resemblance of 'entities'. This has always seemed to me the most striking and important fact about the affiliations of scientific theories. I do not suggest a philosophical 'explanation' of this fact; I cite it, on merely historical evidence, just as a fact". (1987, 393)[2]

It is arguable that this important feature of theory-change can be fully accommodated within scientific realism. In general, a scientific realist can explain the fact that mathematical equations are retained in theory-change, on the grounds that they form an integral part of well-supported and (approximately) true theories. But she would not claim that all that is retained is empirical content and (uninterpreted) mathematical equations. Nor would she claim that there is a dichotomy between the structure and the content of a physical process. On the contrary, she would argue that not only is some theoretical content also retained but that we have reasons to believe that the content of our current theories – what they predicate of the world – is likely to be true.

In particular, one could argue for scientific realism as encompassing the following theses:

(1) Some of the properties incorporated into the physical interpretation of a mathematical equation have been retained as parts of the new physical interpretation of the equation. For instance, as I shall argue in section 6, not only has the wave-equation which described the propagation of a light-wave in ether been retained in the electromagnetic theory of light – now describing the propagation of an electromagnetic wave in the field – but also many substantial properties that an ethereal wave was supposed to possess – for instance transversality, ability to sustain potential and kinetic energy, finite velocity of propagation and others – have been retained as properties of an electromagnetic wave.

(2) The full physical content (underlying mechanisms, processes) that current physical theories ascribe to these mathematical equations is better supported by current evidence than the physical content ascribed to these equations by past theories. For instance, it is arguable that the existence of electromagnetic fields is better supported (by means of variable and independent evidence) than the existential claims about a material substratum, known as mechanical ether.

[2] In a later article, Stein elaborated on this point and stated that the history of science has shown that "on a certain very deep question Aristotle was entirely wrong, and Plato – at least on one reading, the one I prefer – remarkably right: namely, our science comes closest to understanding 'the real', not in its account of 'substances' and their kinds, but in its account of the 'Forms' which phenomena 'imitate' (form 'Forms' read 'theoretical structures', for 'imitate', 'are represented by'" (1989, 57). He added that it is structural deepening which remains quasi-invariant in theory change, and not entities and their basic properties and relations (*op.cit.*, 58). As we shall see, these statements amount to endorsing structural realism, and hence our criticism of structural realism intends to challenge Stein's position as well.

(3) It follows from (2) that we have independent reasons to believe that the physical content given to the retained equations by our current theories is more likely to be true than false. So for instance, it is likely to be true that the wave-equation describes correctly the propagation of electromagnetic waves in the field.

As I said in the introduction, Worrall believes that this full-blown realist position is wrong. Therefore his structural realism is to be distinguished from whole-hearted realism. However, structural realism cannot be the mere recording of the fact that there is a distinctive continuity at the mathematical level. On the contrary, as a philosophical thesis it must be able to provide an explanation of this recognizable and important feature of theory-change in science. Besides, as Stein hinted and as we shall see in section 3, this feature of theory-change is compatible with instrumentalism (1987, 383). Hence, if structural realism is to be taken as an in-between position – which, however, has a realist gloss – it must take distance from instrumentalism.

I think, then, that structural realism must be understood as issuing a new epistemic constraint on what can be known and on what scientific theories can reveal. In other words, structural realism, as opposed to scientific realism, somehow restricts the cognitive content of scientific theories to their mathematical structure together with their empirical consequences.

One can argue that *structural realism* is the philosophical thesis consisting of the following positions:

(a) Scientific theories can, at best, reveal the logical form or structure of the underlying physical reality by means of their mathematical structure.

(b) Mathematical equations which are retained in theory-change express real relations between objects for which we know nothing more than that they stand in these relations to each other.

(c) Different ontologies (and hence physical contents) may satisfy the same mathematical structure but there are no independent reasons to believe in one of those as the correct one.

Although this is a strong position, I take it that it is the one that Worrall wants to endorse. For instance, he stated that the structural realist

> "insists that it is a mistake to think that we can ever 'understand' the nature of the basic furniture of the universe". (1989, 122)

Instead, what we can discover are "relationships between phenomena expressed in the mathematical equations of (the) theor(ies), the theoretical terms of which should be understood as genuine primitives" (Worrall, 1989, 122). And referring to the empirical success of Quantum Mechanics Worrall stated: "The structural realist simply asserts (that...) the structure of the universe is (probably) something like quantum mechanical". (1989, 123)

Moreover, Poincaré, who is taken by Worrall to be the precursor of structural realism, made equally strong claims about the cognitive status of scientific theories. Poincaré was well aware of the force of the argument from the past falsity of scientific theories. He stated:

"The laity are struck to see how ephemeral scientific theories are. After some years of prosperity, they see them successively abandoned; they see ruins accumulate upon ruins; they forsee that the theories fashionable to-day will shortly succumb in their turn, and hence conclude that these are absolutely idle. This is what they call the *bankruptcy of science*". (1905, 140)

Poincaré did not find this argument compelling for he thought that, after all, theories are used merely in order to co-ordinate empirical laws and predict phenomena (cf. Poincaré, *op.cit.*). Hence, if theories do not aim to describe correctly the furniture of the world, then it is no problem that their theoretical parts, the unobservable entities and mechanisms they postulate, are mere speculations which turn out to be rejected. For instance, for Poincaré, the then-abandoned "Fresnel's theory enables us to [predict optical phenomena] as well as it did before Maxwell's time". (1905, 140)

But Poincaré also thought that his view about scientific theories did not reduce "physical theories to practical recipes" (*op.cit.*), and therefore it allows for some truths to be discoverable. Apparently, his reason for this was that there is a sort of intrinsic value to the mathematical equations of successful theories, which is made specially clear when the mathematical infra-structure of an abandoned theory was taken over by a succeeding theory. For him:

"(T)hese equations express relations, and if the equations remain true, it is because the relations preserve their reality. They teach us, now as then, that there is such and such a relation between this thing and some other thing; only this something formerly we called *motion*; we now call it *electric current*. But these appellations were only images substituted for the real objects which Nature will eternally hide from us. The true relations between these real objects are the only reality we can attain to, and the only condition is that the same relations exist between these objects as between the images by which we are forced to replace". (1905, 140-141)

So, Poincaré did not find full-blown scepticism compelling because he thought that scientific theories can (at best) reveal relations between objects, and this task is well-performed by the mathematical structures of theories. In other words, his positive epistemic response to the argument from the past falsity of scientific theories was like this:

"Still things themselves are not what it [i.e. science] can reach as the naive dogmatists think, but only relations between things. Outside of these relations there is no knowable reality." (1902, 28)

And elsewhere he stated:

(...)(I)f we look more closely [i.e. at the history of abandoned scientific theories], we see that what thus succumb are the theories properly so called, those which pretend to teach us what things are. But there is in them something which usually survives. If one of them taught us a true relation, this relation is definitely acquired, and it will be found again under a new disguise in the other theories which will successively come to reign in place of the old". (1905, 351)

Or, as Worrall put it, for Poincaré "Fresnel clearly misidentified the *nature* of light, his theory nonetheless accurately described not just light's observable effects but also its *structure*". (1990a, 23 – emphasis in the original)

So, for Poincaré and Worrall structural realism is the epistemological thesis mentioned above in (a)-(c), and as such it intends to place some new epistemic constraints to what the cognitive parts of scientific theories are. As J. Giedymin has argued on independent grounds, Poincaré's philosophical stance towards scientific theories is that their cognitive status is exhausted by their observational consequences together with their mathematical structure. It follows from this position that if two theories are not only empirically indistinguishable but also share in common the same mathematical structure, they are cognitively indistinguishable theories, or equivalent theories *simpliciter*. The latter thesis is what Giedymin has called "Structuralistic Realism". (1982, 83)

3. *Structural Realism as a Species of Scepticism*

Structural realism is in itself a pessimistic epistemological theory of science. For if theoretical terms are understood as genuine primitives, without any cognitive content outside the theory in which they feature, then when theories change, the primitives that they use in their theoretical vocabulary also change. Or better, when a theory is abandoned, after, say, a scientific revolution, its theoretical vocabulary is bound to be abandoned. If the pessimistic meta-induction is correct, that is if one theory after the other is abandoned, then no retention whatsoever at the theoretical level can ever take place. Instead, a set of primitives is replaced by another set of primitives. No theory can be said to describe the world at its deeper levels better than another, since all theories are false; or, at any rate, even if one of those hits the truth, we have no way to know it. Hence, no aspiration of knowledge of the deep-structure of the physical world can be entertained.

As I stated already, Worrall conceded this point to the advocates of the pessimistic meta-induction. For him the lack of independent reasons for believing in a particular ontology, for example that there are electromagnetic fields, is due to the alleged, inductively established, fact that our present favourite ontological commitment is likely to be false; that is, our current commitment that light is an electromagnetic wave of specific frequencies is likely to take the route to the 'museum of scientific ideas' together the with the ether, the caloric, and other now abandoned putative entities. In other words, structural realism is happy with the claim that at the theoretical level, where our theories try to penetrate the deeper levels of reality, we are likely to go wrong; or, again, even if we happen to be right, we have no means to know it.

But, if we recall Worrall's claim that "it is a mistake to think that we can ever 'understand' the nature of the basic furniture of the universe", we may wonder why we should bother about what the furniture of the world is. Why, after all, should we develop theories and explanations of the world in terms of unobservable entities and causal mechanisms? Why do we not go straight for an instrumentalist view of scientific theories according to which a scientific theory is a useful instrument for classification and prediction of observable phenomena? (Duhem, 1906) Or, for a neo-instrumentalist view according to which theoretical mechanisms are useful fictions and theoretical claims are fairy tales without cognitive content? (Van Fraassen, 1980)

Structural realism is not outright instrumentalism. For it is not committed to the view that scientific theories are merely instruments for predictions and coordination of phenomena. According to structural realism there is something realistic in the interpretation of scientific theories, and this is the claim that theories can reveal the structure of the world by means of their mathematical structure; they can reveal the relationship between unobservable entities or between causal mechanisms and observable phenomena but not their nature. So, the attitude that structural realism suggests towards scientific theories is not instrumentalist of the traditional sort.

But neither is it neo-instrumentalist à la Van Fraassen. For it states that scientific theories can represent correctly the non-empirical structure of reality. So, scientific theories are not accepted as merely empirically adequate descriptions of the world. Since they may reveal the structure of the world, structural realism recommends a cautious belief in the correctness of the mathematical structure of the theories and not just an acceptance of theories as empirically adequate.

Nonetheless, structural realism is a species of scepticism about the possibility of scientific knowledge. It admits that scientific theories are either true or false in virtue of a reality that exists independently of our theories and is not logically determined by them; yet it insists that we cannot know whether a particular theory is true in the sense that it correctly represents this reality. As I said, this view alone would not be radically different from modern versions of agnostic instrumentalism. Yet Worrall tries to give a realist twist to neo-instrumentalism by arguing that we can at best know that a scientific theory may be correct in representing the structure of reality (Worrall, 1990a, 23-24). So, the epistemic status of structural realism is scepticism about knowing the content of scientific theories, but realism about knowing their structure. Epistemically, therefore, the structural realist attitude towards scientific theories is a compromise: belief in the structure of the theory, yet agnosticism concerning its content.

I want to stress the latter claim, namely that the Poincaré-Worrall attitude towards scientific theories is agnostic instrumentalist about their content. For, if there is no aspiration for the knowledge of the physical content of a scientific theory, then the only *working role* that can be ascribed to this content is useful fiction. It might be objected here that our ascriptions of physical content to a scientific theory may be seen as our best guess about the furniture of the world. In fact, I gather that even Van Fraassen would be willing to argue for this position. But how realist is a guess about the furniture of the world? If it is not accompanied by good reasons to believe that this guess is likely to be true, then it is not realist at all. Besides, since Worrall accepts the conclusions of the pessimistic meta-induction, he must think that our best guess about the furniture of the world is likely to be false. I think that a likely-to-be-false-best-guess of the furniture of the world is, if anything, a fig-leaf realism.

4. *Is There a Structural Realist Response to the Pessimistic Meta-induction?*

The plausibility of structural realism as an alternative to scientific realism rests heavily on the possibility of drawing a viable epistemic distinction between the structure and the content of a scientific theory in the sense of justifying why the first is knowable whereas the other is unknowable; or, at any rate, why structure can be warrantedly known whereas content cannot. Hence, it is quite important to examine whether such a distinction is physically possible and epistemically imperative. I am going to argue that no such distinction is viable. Hence, I shall try to show that there is no point in distinguishing between belief in the structure and belief in the content of a scientific theory. I shall leave this task for section 5. For the time being, I shall assume that such a distinction is viable and shall try to show that even so, structural realism fails to give new reasons for countering the pessimistic meta-induction and, at any rate, it does not score better than more full-blown realist responses to this argument.

First of all, in order for Worrall to establish his halfway house between scientific realism and agnostic instrumentalism about the non-empirical knowledge in science, he must give an argument against Van Fraassen's generalized agnosticism. I take it that the essence of Van Fraassen's argument against scientific realism was that anything in a physical theory that goes beyond the level of observable phenomena, that is all non-empirical parts of a scientific theory, has no cognitive value at all. This argument is, in effect, an argument against structural realism as well. For, the generalized agnosticism about the non-observable parts of scientific theories that Van Fraassen's (1980) wanted to establish would sweep away belief as the right attitude towards both the structure and the content of the theoretical part of a theory, since both exceed the level of observable phenomena. In order to see this, let us suppose for the sake of the argument, that there is a well-defined divide between the mathematical content (or form) of, say, Maxwell's equations and the physical quantities designated by the magnetic and electric vectors. If, as I think plausible, both sides of this divide go beyond the level of observable phenomena, Van Fraassen's position would entail an equally agnostic attitude towards both of them. If, on the contrary, Worrall denies that both sides of the divide go beyond the level of observable phenomena, that is, if he thinks that the mathematical form of Maxwell's equations is either observable or about observables, then I fail to see how he can speak of mathematical structure belonging to the non-empirical part of a theory.[3]

So, I think that Worrall is in need of an argument to sustain his thesis that we can know at least some non-empirical content of our theories as it is described by their mathematical structure. The argument must be such that it establishes in virtue of what, and how we can know that the mathematical structure of a mature theory represents correctly the structure of reality. For instance, in order for Worrall to sustain the thesis that Maxwell's equation correctly represent the structure of the field, he has to give an answer to the question 'in virtue of what do Maxwell's equations correctly represent the structure of the field?' Short of a response to this question, Worrall is not able to perform the demanded compromise: realism about the structure of the field – agnosticism about its nature. And generally, short of a general response to ques-

[3] In his review of Van Fraassen's (1980), Worrall does not raise the issue of the distinction between observables and unobservables, which seems to feature centrally in Van Fraassen's constructive empiricism (Worrall, 1984). So, I am not sure what stance he takes on this issue. Surely, his review shows very elegantly that one can produce important arguments against Van Fraassen, regardless of the aforementioned distinction. But, I think, that if he is to provide structural realism as an alternative to Van Fraassen's agnostic intrumentalism he must show in virtue of what belief in the structural correctness of a theory is justified. As he admitted in his (1984), theories "can, at best, be regarded as 'approximately' true, or as 'reflecting *to some extend* the real structure of the universe'" (1984, 69-70 – emphasis in the original). But he recognized that *both* of "these notions have notoriously proved resistant to precise analysis". (1984, 70)

tions like this, Worrall's structural realism cannot be a viable response to Van Fraassen's generalized agnosticism.

It turns out that Worrall needs the same argument in order to defend his position as a genuine response to Laudan's pessimistic meta-induction. As I said already, Worrall argues against Laudan, that at least some non-empirical content of a scientific theory is retained while theories proper change. The argument he provides, as we saw, is the fact that some mathematical equations are retained in theory-change. But Laudan has already taken care of such a response. He has anticipated that one "might be content with capturing only the formal mathematical relations" of the superseded theory T_1 within the successor theory T_2 (1981, 237). In other words, Laudan has anticipated Worrall's argument against the pessimistic meta-induction. However, he rejected this argument as a viable realist-minded response since, he contended, it amounts to the response of "'closet' positivists". (1981, 237)

Laudan is right in stating, in essence, that without further arguments the appeal to mathematical continuity would not be a natural realist response to the pessimistic meta-induction. So, in order for Worrall to utilize this response and dress it in a realist gown, he must give an extra argument to the effect that mathematical equations represent the structure of the world, and hence that their retention in theory-change marks a sense in which the superseded theory was right about the world.

Incidentally, one could argue against Worrall, that the retention at the level of equations signifies only a pragmatic consideration, namely that the scientific community finds it just convenient and labour-saving to build upon the mathematical work of their predecessors. This favouritism towards mathematical equations, the argument would go on, signifies just the conservativeness of the scientific community rather than any discovery of facts about, and real relations in, the world. Here again, short of an argument, Worrall cannot sustain that the retention at the level of mathematical equations signifies some grasp of the real structure of the world.[4]

I conclude that in order for Worrall to make his case for structural realism, as a realist position which is both different from Van Fraassen's agnosticism and effective in blocking Laudan's pessimism, he has to provide an argument to the effect that mathematical equations represent real relations in the world, which are knowable independently of their *relata*. In particular, Worrall is in

[4] Actually, modifying Van Fraassen's Darwinian argument for the success of sientific theories, (1980, 40) one could produce a like-minded 'Darwinian' argument in order to explain the retention at the mathematical level: mathematical equations featuring in empirically successful theories have survived a severe competition with a good deal of alternative theories, and this is enough to explain their retention in successor theories.

need of a general and independent argument which could take him from the fact that some mathematical equations are retained in theory-change to the substantial claim that they signify real relations between physical objects otherwise unknown, (or, worse, unknowable). I am not aware of such an argument in Worrall's (and Poincaré's) writings.[5] I do not think that it is impossible to give such an argument, but I think, prior to its being given no case for structural realism can be sustained, even if we grant that there is a viable divide between the form and the content of scientific theories.

4.1. What is the Missing Argument?

Worrall could still argue that the missing argument is nothing but the argument from the predictive success of scientific theories.[6] Or, more concisely, a weak version of the 'no miracles' argument. This argument suggests, as I mentioned in section 1, that the predictive success of mature scientific theories could not be explained unless one accepted that these theories are approximately true of the world.

Worrall is not pleased with the 'no miracles' argument in its full strength. Referring to the status of the wave theory of light compared to our current theories of light he stated that "the classical wave theory is (...) 'to a large degree empirically adequate' – yes; 'to some degree *structurally* accurate' no doubt; but 'approximately true' – no". (1990b, 343 – emphasis in the original)

I conjecture that the structural realist version of the 'no miracles' argument could be like this:

(W) Predictive success is cumulative; that is, subsequent theories predict at least as much as preceding theories predicted. But mathematical structure is also cumulative; that is, subsequent theories incorporate the mathematical structure of preceding theories. Therefore, there is a correlation between the accumulation of mathematical structure and the accumulation of predictions. Successful predictions suggest that the theory is on the right track. Then, the

[5] E. Zahar has suggested that for Poincaré "convenience, and convenience alone, operates like an index of verisimilitude" (1989, 161). He then goes on to claim that convenience, for Poincaré, is "a purely syntactical notion based on the mathematical structure of a given proposition, not on any semantic relation between its descriptive terms and some external reality" (1989, 161). For Zahar, Poincaré was a neo-Kantian who subscribed to the thesis that we can "simulate the physical world, but not to refer to it directly" (161). This is an interesting contention and explains a possible philosophical motivation for structural realism, but I still think that it is in need of support by some arguments.

[6] In fact, Worrall argued like this in a private communication.

claim that the 'carried over' mathematical structure of the theory correctly represents the structure of the world explains this predictive success.

In general, it is an important argument that some salient features of science, notably its impressive predictive success, cannot be explained unless one admits that theories have got the world right in some way. Poincaré, the precursor of structural realism, used such an argument frequently. For instance, he stated that "It will be said that science is only a classification and that a classification cannot be true, but convenient. But it is true that it is convenient; (...) it is true finally that this cannot be by chance". (1905, 352)[7]

But there are at least three problems with the structural realist's appeal to this argument. Let us take them in turn. First, if this argument is to be available to a structural realist, then she must argue that the mathematical structure of a theory is somehow exclusively responsible for the predictive success of the theory.[8] In other words, a structural realist would have to admit that the predictive success of a theory lends credence only to the structure of a theory. Worrall seems to defend this strong view when he says:

"It is true – and importantly true – that many of the mathematical equations supplied by the wave theory of light still live on in science; and it is true – and importantly (if rather obviously true) – that repeatable (and repeated) experiments do not change their results, so that all the correct empirical consequences of the wave theory are still, of course, correct. Nonetheless, at the theoretical level there has been radical, ineliminable change. (1990b, 342)

[7] On another occasion, comparing Ptolemaic and Copernican astronomy, he pointed out that the two theories are kinematically equivalent. Yet, he also observed that Copernican astronomy gives a better dynamical explanation of some phenomena, which under the Ptolemaic framework appeared coincidental. He then asked: "Is it by chance (...)?" The answer was that Copernican astronomy provided "a bond between the (...) phenomena" and that "[this bond] is true". (cf. 190 5, 354)

[8] In fact, Carnap once believed something similar to this. He contended that in order for a theory to yield predictions, its mathematical structure need not be interpreted (1939, 66-67). What was needed, according to Carnap, was a set of singular statements, describing observations, which fed into an otherwise uninterpreted calculus, yield another set of singular sentences, testable by observations. So, he endorsed the view that "for the application of a physical calculus we need an interpretation only for singular sentences" (*op.cit.,* 66). Of course, he thought that uninterpreted sentences which occur in the derivation of a prediction are, in fact, indirectly interpreted in virtue of their connections with observations; "but", he added, "we need not make their interpretation explicit in order to be able to construct the derivation [of a prediction] and to apply it" (*op.cit.*, 66). Yet, he soon realized that this view, being akin to an instrumentalist conception of theories as 'calculating devices', gives an inadequate and incorrect account of scientific theories. In his [1956], he endorsed the view that "for an observer X to 'accept' the postulates of T, means here not to take T as an uninterpreted calculus, but to use T together with specified rules of correspondence C for guiding expectations by deriving predictions about future observable events from observed events with the help of T and C. (1956, 45)

But it is not true that there is any sense in which mathematical equations alone, devoid of their physical content, can give rise to any predictions whatsoever. Moreover, for any particular prediction to get off the ground, theoretical assumptions, and auxiliaries, are used over and above the ones already included in the interpretation of the mathematical equations used for the derivation of a prediction. Therefore, as I shall illustrate in section 6.2 using the case of Fresnel's laws if the predictive success of a theory can at all support the claim that a theory has got it right about how the world is, it also supports the claim that *some* of the physical content of the theory is correct. So, I contend that if Worrall were to use the foregone argument (W) in order to claim that retained mathematical equations reveal real relations in the world, he would have to admit that some physical content, not necessarily empirical and low-level, is also retained. But such an admission would undercut his claim that the predictive success supports only the mathematical structure of a theory, and similarly, it would undercut the epistemic dichotomy between the structure and the content of a physical theory.

Second, this version of the 'no miracles' argument cannot be available only to a structural realist. A scientific realist who claims that we can know the furniture of the world can argue along the same lines to the effect that some theoretical mechanisms – as well as substantial properties and law-like behaviour ascribed to entities – are also retained while theories change. So, a scientific realist can argue along the lines of this argument too, avoiding reification of relations independently of their *relata*.

Moreover, any formulation of the 'no miracles' argument in defence of structural realism would involve no less epistemological risk than using this argument to defend full-blown scientific realism. The only difference would be one of degree, relating to how much excess non-empirical content one is willing to justify belief in. But neither is it the case that a scientific realist is committed to the view that all non-empirical claims that theory makes are equally justified; nor is it the case that a different kind of belief is involved when we assert that we know the structure of a process than when we claim that we know its nature.

Third, even if we grant that Worrall can appeal to a weak version of the 'no miracles' argument to vindicate structural realism, we must add that he rests on an implicit 'inference to the best explanation'. For his argument is, in effect, that there is certainly a justifiable preference for one of the following two accounts of the retention of mathematical equations in theory-change.

(a) (Explanation) Accidental feature of theory-construction.

(Fact) Mathematical equations are retained in theory-change.

(b) (Explanation) Retained mathematical equations reveal objective relations in nature.

(Fact) Mathematical equations are retained in theory-change.

Obviously, although (a) provides a potential explanation of the *explanandum*, Worrall's argument can be construed as suggesting that this 'fortunate coincidence' is not the best explanation available. Instead, (b) provides a more plausible, and in fact the best available, explanation. Hence, one could argue, we should go from the *explanandum* to the *explanans* by means of 'inference' (b). I think that there is nothing wrong with this inference and that, as Boyd has taught us, the employment of abductive inferential strategies is an indispensable component of a realist philosophical package (Boyd, 1989, 1990). But I also think that the employment of such an inference for the vindication of a realist explanation of the retention at the mathematical level is incomplete insofar as it is used to justify only cautious belief in the structure of a theory.

However, Laudan would argue that this is an illegitimate move, as he has already argued against the use of this inference for the defence of the approximate truth of empirically successful mature theories (1981, 242-3). Worrall could respond that he does not mean to employ such an *inference* to justify his position. Instead, he could say that his version of the 'no miracles' argument is a philosophical consideration, rather than a 'rigourous' employment of a rather dubious *inference* to the best explanation of the continuity at the mathematical level. I cannot see any merit that this move may have in establishing structural realism as a tenable position over and above the merits of inference to the best explanation. Besides, even if one grants that inference to the best explanation is dubious, in that it cannot establish the truth of the explanation offered, it is in no sense more dubious than an inference using enumerative induction.[9] The argument for the cognitive viability of such inferences is neither that they are logically sound (for they are not) nor that they are indefeasible (for it is a consequence of their not being logically sound inferences that they

[9] I understand that this is a contentious issue. But it is not the purpose of this paper to work on this. I shall only state, in a vague way, some of the common features of these inferences. Both inferences are defeasible; both are ampliative in the sense that make claims far beyond what it is really entailed by their premisses; both require some prior ordering of the hypotheses they support, in terms of some methodological desiderata, notwithstanding Reichenbach's descriptive vs. inductive simplicity; both are justifiable, if at all, by means of rule-circular arguments. I think that the adversaries of inference to the best explanation, who nonetheless employ inductive arguments, consider the former inference dubious because it goes from observable behaviour to unobservable mechanisms. Hence, a philosopher of science who does not accept that there are special problems in our knowledge of the unobservable world, e.g. an in principle unknowability, and is happy with inductive arguments, ought to accept inferences to the best explanation as a cognitive tool. For more on this issue cf. Psillos (1994b), Chapter 4.

are defeasible) (cf. McMullin 1987, Musgrave, 1988). The argument in their favour is that, within the scope of defeasible and uncertain scientific practice, it is an indispensable tool for going beyond the phenomena, and for supporting cautious beliefs about what it is going on beyond them. But, at any rate, I cannot see why Laudan would be moved by weak and unsupported considerations if he is not already moved by a more rigorous formulation of the 'no miracles' argument.

So, I may conclude that structural realism is not a successful response to the pessimistic meta-induction, even if we grant that there is a distinction between the structure and the content of a scientific theory, and that, in any case, it is no more successful that full-blown realists responses.

5. *Structure vs. Nature?*

I said in section 4 that the tenability of structural realism rests on the epistemic viability of a distinction between structure and nature. Here I shall enunciate two theses: First, that the nature and the structure of a physical entity form a continuum; and second, we can come to know the nature of an entity, process, or physical mechanism.

According to the alleged dichotomy between structure and nature, it is as if the nature of a scientific entity is something over and above its structure. Or, equivalently, it is as if the physical content of a mathematical symbol, (that is, the physical entity or process it stands for) is somehow on top of the totality of the mathematical equations in which it features, (that is, the totality of laws which describe its behaviour). But when scientists talk about the nature of an entity, what it is normally understood is a bunch of basic properties and a set of equations, expressing laws, which describe the behaviour of this entity. That is, they rather speak of the way in which this entity is structured. I think that talk of 'nature' over and above this structural description (physical and mathematical) is reminiscent of the medieval 'forms' and 'substances'. This talk was overthrown by the scientific revolution of the 17th century.

Let me try to make this point concrete by considering the case of classical mass. The traditional idea of mass is the 'quantity of substance' possessed by a body. If this idea is retained, then the nature of mass has something to do with the substance of a material body. Then one could argue that the structure of mass may be something different from its nature. But after the scientific revolution this idea was slowly replaced by the concept of inertial mass, which was described as the property in virtue of which the body resists acceleration, and its description was given by Newton's second law, viz., the equation

$$m_i = F/a. \qquad (1)$$

Hence, mass was understood as having, loosely speaking, a structural property instantiated when a body is exerted some force and resists acceleration. 'Structural property' may not be a nice term, but it seems to me that it can put across the point that by discovering more about the structure of mass we discovered more about its nature, that is about what mass is. Likewise, the gravitational mass of a body was described by the law of Universal Gravitation as the property of the body in virtue of which it is accelerated in a gravitational field of another massive body M. This property was express by the equation

$$m_g=F\, r^2/G.\, M. \qquad (2)$$

Moreover, it was an empirically established discovery that these two properties are identical; i.e. the property in virtue of which the body resists acceleration is the same as the property in virtue of which it is accelerated in a gravitational field. That is,

$$m_g=m_i \qquad (3)$$

By equating these two properties, more structure, so to speak, was added to mass, and more knowledge about what mass is was gained. So, I think there is nothing more in the classical scientific concept of mass than what is expressed by equations (1), (2) and (3). Or, equivalently, knowing what mass is involves knowing what laws it obeys, and in particular, what equations it satisfies within a scientific theory.

It is arguable that knowing what laws an entity obeys does not exhaust the knowledge of what this entity is. But the idea behind this contention seems to be that the causal role that this entity plays, and some of its properties in virtue of which it plays this role, are over and above a quantitative description in terms of mathematically specifiable laws and mechanisms. It is certainly possible that some of the properties in virtue of which a certain entity plays a causal role may not be specifiable in terms of mathematically formalizable laws and descriptions. It is also true that at any given point we do not know all of the properties that an entity possess, or we may be wrong about some of those. But these are empirical claims to be discovered and established by natural science itself. They do not guarantee that there is always a, so to speak, 'excess nature' in every entity which cannot be captured by further investigation into the laws that this entity obeys. On the contrary, the actual scientific practice urges that improvements in our knowledge of what an entity is involve further knowledge of laws that this entity obeys. I then conclude that the 'nature' of an entity is not over and above its 'structure' and that knowing the one involves and entails knowing the other. It is in this sense that I claimed that nature and structure form a continuum in science.

6. *The Case of Light*

Let me now consider the case of light, which Worrall and Poincaré took as illustrative of their thesis that there is a dichotomy between nature and structure. For instance, after citing the 'structural similarity' between Fresnel's laws and Maxwell's laws, Worrall appealed again to Poincaré and argued that the discovery that light consists in vibrations of the electromagnetic field must be understood not as suggesting something about the nature of light but rather as saying that

> "Maxwell built on the relations revealed by Fresnel and showed that further relations existed between phenomena hitherto regarded as purely optical on the one hand and electric and magnetic phenomena on the other". (1989, 120)

This quote may be taken to suggest that Maxwell discovered more about what light is and how it is propagated. Then I would entirely agree, since it would just express the continuity between structure and nature that I already mentioned. But I think that the aforementioned quote is meant to suggest that only relationships, i.e. structures in Worrall's terminology, are discoverable, whereas natures are not. As I said, this is not only a physical opposition of structure to nature; it is also an epistemological thesis concerning what aspects of the physical world are knowable. Worrall could argue here that he does not intend to raise a new epistemic barrier in the knowledge of the physical nature of a quantity involved in a physical process. We may hit on the truth, he could say, but the question is 'what is it legitimate to claim we know?'. His answer to this question would be that it is legitimate to claim that we know the structure of light but illegitimate to claim that we know its nature. As he put it:

> "From the vantage point of Maxwell's later theory, the picture on which Fresnel's equations are based is, as before, quite mistaken. But nonetheless, Fresnel's theory has, again when judged from this later vantage point, exactly the right structure – it correctly makes the effects here entirely dependent on something or other that undergoes periodic changes at right angles to the direction of propagation, it's 'just' that it completely misidentifies what changes". (1990a, 25)

And elsewhere he stated "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, altough the periodic changes which the two theories postulate are ontologically of radically different sorts – in one material particles change position, in the other field vectors change their strength – there is nonetheless a structural, mathematical continuity between the two theories. Something importantly more than merely correct empirical content, there is a carry over at the theoretical level too, but one of *structure* rather than content". (1990a, 21 – emphasis in the original)

Yet, it is not correct that it was only 'structure' (i.e. mathematical equations) that was carried over in the passing from Fresnel to Maxwell. I shall try to show that fundamentally correct theoretical principles about the propagation of light and some features of the carrier of light waves were also carried over. Moreover, I shall try to sketch a realist argument against the pessimistic meta-induction, without appealing to the expedient dichotomy between the structure and the nature of light.

6.1. The Transversal Character of Light-Waves

For the wave-theory of light, the most important discovery concerning the properties of light was that the propagation of light is a transversal process, that is, that light is a disturbance (vibration) which takes place at right angles to the direction of propagation. This was basically a discovery induced from the empirical fact that light admits polarization. The discovery of polarization effects cast doubt on the long entertained view that light consists of longitudinal waves, pretty much like sound. The effects of polarization rendered clear that light must have at least a transversal component.[10]

It was in 1816 when Fresnel and Arago observed that two light beams polarized at right angles to each other never interfere, whereas when they are polarized parallel to each other display interference patterns (cf. Fresnel & Arago, 1819). As Fresnel stated:

> "We [i.e. Fresnel and Arago] both felt that these fact would be explained very simply, if the vibrations (oscillatory movements) of the polarized waves took place in the plane itself of these waves [i.e. if they are transversal]. But, what became of the longitudinal oscillations along the light beams? How were these oscillations destroyed by the polarization phenomenon and why did not they reappear when the polarized light was reflected or refracted obliquely on a glass plate?" (1866, vol.1, 629 – appeared also in Swindell, 1975, 80)

In fact, the laws of mechanics demanded that, when a polarized light-ray strikes the interface of two media, the longitudinal component of a light-ray

[10] As T. Young put it, in 1817: "I have been reflecting on the possibility of giving an imperfect explanation of the affection of light which constitutes polarization without departing from the genuine doctrine of undulations. It is a principle in this theory, that all undulations are simply propagated through homogeneous mediums in concentric spherical surfaces like the undulations of sound, consisting simply in the direct and retrograde motions of the particles in the direction of the radius, with their concomitant condensations and rarefactions. And yet it is possible to explain in this theory a transverse vibration, propagated also in the direction of the radius, and with equal velocity, the motions of the particles being in a certain constant direction with respect to the radius; and this is a *polarization*". (quoted by Whittaker, 1951, 114)

was expected to re-emerge. No such a re-emergence of the longitudinal component was observed. Fresnel toyed with the idea that there is such a longitudinal component, "admitting however at the same time the presence of transverse movements, without which it was impossible for me to conceive the polarization and the mutual non-influence of the beams of polarized light at right angles" (1986, Vol. 1, 630). But he soon accepted what he called "the fundamental hypothesis", namely that the propagation of light is a uniquely and exclusively transverse process. In more technical terms, Fresnel's fundamental hypothesis asserts that "the luminous vibrations are executed on the surface of the wave itself perpendicularly to the ray [i.e. perpendicularly to the wave-front]" (1986, Vol. 1, 786), which amounts to an elimination of the component of the vibrations normal to the wave-front.

The elimination of the longitudinal component was one of the central problems and anomalies faced by Fresnel and all the scientists who tried to investigate the dynamics of light-propagations by means of an 'elastic solid' model. Yet, Fresnel was right in suggesting that light is a uniquely transversal process. From this fundamental discovery, Fresnel was able to discover important laws concerning the behaviour of light in several media. In particular, he was able to derive the set of laws which Worrall cites as a prime example of structural retention in science, namely the laws of the relationships between the amplitudes and intensities of the incident, refracted and reflected rays in the interface of two media with different refractive indices. (Worrall, 1989, 119)

6.2. *The Fresnel Laws*

The discovery of these laws by Fresnel was, by and large, independent of any particular mechanical representation of ether. Instead, it was based on the theoretical claim that light is a transversal process, on sound physical principles such as the principle of conservation of energy, and on several geometrically derived assumptions concerning the boundary conditions in the interface of two media. The only mechanical assumption explicitly used was the minimal one that the amplitude of the light-wave is physically represented by the displacement of the molecules of ether. I shall now try to show in more detail how Fresnel derived the laws that bear his name.

His problem was the calculation of the amplitude and intensity of the reflected and refracted (i.e. transmitted) rays with respect to the amplitude, intensity and state of polarization of the incident ray, when a light ray strikes the interface of two media.

Fresnel discovered that the amplitudes of the reflected and transmitted rays depend on the polarization of the incident ray, and in particular on whether the incident light is polarized perpendicularly or parallel to the plane of incidence. Fresnel took it that, generally, the vibrations constituting light are propagated perpendicularly to the plane of polarization. His well-known laws have the following form: (Fresnel 1823/1866, Vol. 1, 733-774)

(A) The incident ray is polarized along the plane of incidence, i.e. the vibrations are executed perpendicularly to this plane. Then,

$R_{par} = -\sin(i-i')/\sin(i+i')\ I_{par}$ (4)

where, R_{par} is the amplitude of the reflected ray polarized as mentioned above, I_{par} is the amplitude of the incident ray, i is the angle of incidence/reflection and i′ is the angle of refraction (transmission).

(B) The incident ray is polarized perpendicularly to the plane of incidence, that is to say, the vibrations are executed parallel to this plane. Then,

$R_{per} = -\tan(i-i')/\tan(i+i')\ I_{per}$. (5)

He was also able to derive that for normal incidence, i.e. i=90°, expression (5) gives that

$R_{per} = r-1/r+1\ I_{per}$ (6)

where r is the refractive index between the two media.

Although Fresnel's proof is very nice I shall skip the mathematical details. I shall only state that he makes use of the following:

(i) The *minimal mechanical assumption* that the absolute velocity of the displacement of the molecules of ether is proportional to the amplitude of the light-wave. To be precise, we have to add here that for Fresnel the velocity of the propagation of light in an optical medium was inversely proportional to the square root of the density of the medium. That is,

$v_1 = \sqrt{d_1}$ (7)

and hence, the refractive index of two media was

$v_2/v_1 = \sin i/\sin i' = \sqrt{d_2}/\sqrt{d_1}$. (8)

(ii) The *principle of conservation of energy* ("*forces vives*") during the propagation of light in the two media. According to this principle,

$m_I v_I^2 = m_R v_R^2 + m_T v_T^2$ (9)

where m_i (i=I,R,T) is the mass of the incident, reflected and transmitted rays respectively, and v_i (i=I,R,T) is the velocity of the rays. In particular, Fresnel took the velocity coefficient (amplitude) of the incident ray to be unity and the velocity coefficients of the reflected and transmitted rays to be R and T respectively. Applying the principle of the conservation of energy to the effective components of light in the interface of the two media, he arrived at a general relation of the form

$$\sin i' \cos i(1-R^2) = \sin i \cos i' T^2. \quad (10)$$

As he said:

"This is the equation that results from the principle of conservation of *vis viva* and it must be satisfied in all cases, irrespective of whether the incident ray had been polarized parallel or perpendicularly to the incident plane" (1823/1866, Vol. 1, 772 – my translation).

Then, he distinguished the cases (A) and (B) already mentioned.

(iii) Analysing *geometrically* the configuration of the light-rays, he showed that the displacements are continuous in the interface of the two media. Hence he took it that the velocity coefficients of the three rays must satisfy the boundary condition

$$1 + v = u \quad (11)$$

where 1 is the velocity coefficient of the incident ray, and v,u are the velocity coefficients of the reflected and refracted rays respectively. In other words, according to this consideration, the amplitudes of the three rays in the interface of the two media must satisfy the boundary condition

$$A_i + A_r = A_{tr}. \quad (12)$$

He then analysed the components of the rays in each case, and by instantiating the principle of the conservation of energy for the components of the rays active in each case, he derived the two laws (4) and (5). Finally, by taking the intensity of the light-wave to be a function of the square of its amplitude, he was ready to derive similar laws for the intensities of the light-waves. (1823/1866, 775ff)

In his proof, Fresnel did not appeal to any specific mechanical model of the ether in order to derive his laws. The minimal mechanical assumption that the amplitude of the light-wave is proportional to the absolute velocity of the displacement of the ether was a subsidiary assumption in the proof, its sole purpose being to help formulating the principle of conservation of energy. One can take energy as a function of the square of the amplitude of vibrations, without being committed to any specific material realization of this amplitude. The latter claim becomes clear when one looks at Fresnel's general way of demonstrating "the exclusive existence of transversal vibrations in light rays" (1822/1866, Vol. 2, 490). There again, he represented physically the amplitude of vibrations as "proportional to the amplitude of the oscillations of the molecules of ether" (1822/1866, Vol. 2, 491). Yet, the demonstration was clearly non-mechanical in the following sense. He took it that the absolute velocity of the ethereal molecules can be represented as a vectorial quantity and he analyzed it in three components – along the Cartesian co-ordinates. For instance, the first components is normal to the wave and the other two perpendicular to the first, e.g. the second parallel and the third perpendicular to the plane of polarization. He then made clear the redundancy of the minimal mechanical assumption, when he stressed that "whatever the nature of the oscillations executed by the molecules of the ether, we can regard then as resulting from the combination of three series of rectilinear oscillations, the directions of which follow these three rectangular axes" (1822/1866, Vol. 2, 492 – my translation). In other words, it was enough for the correct formulation of the principle of conservation of energy to take energy as a function of the square of the amplitude of the light waves, irrespective of the material significance of this amplitude. (Fresnel, 1822/1866, Vol. 2, 493)

Hence, as regards the use of this case-study by Worrall, it seems to me that my account of the discovery of Fresnel's laws can make clear that there is no sense in which Fresnel distinguished between the structure and the nature of light-propagation; and, there is no sense in which Fresnel was 'just' right about the structure of light-propagation and wrong about the nature of light, even if such a distinction were to be made. Unless of course one understand 'structure' so broadly as to include the principle of conservation of energy and the theoretical mechanism of light-propagation. But I do not think that the issue is terminological. On the contrary, I think that the theoretical mechanism of (exclusively) transversal propagation is as structural, and as natural, as rectilinear propagation, diffraction, interference, finiteness of the velocity of propagation, and satisfaction of the principle of conservation of energy are. And all of these properties of light-propagation were carried over in Maxwell's theory.

I also think that we can tell where Fresnel was right and where wrong without appealing to any distinction between structure and nature. For, even if we grant that Fresnel believed that light is an ultimately mechanical process, we can clearly say that he was right about *some* of the properties of the light-waves and patently wrong about some others, especially about the alleged mechanical character of the propagation of light. He was right that light is a process which needs a carrier, but wrong about the alleged molecular constitution of this carrier. He was right about the transversal character of the oscillations that constitute light but wrong about their mechanical underpinning. He was right in stating that the propagation of light satisfies the principle of conservation of energy, but wrong in reducing the amplitude of the light-wave to the velocity of molecular displacements. He was right that the velocity of propagation was finite and hence that he was in need of medium-wise propagation, but wrong in thinking that this medium must be ultimately mechanical. He was right in suggesting that light is a vectorial physical quantity but wrong in identifying the vectorial components with ether displacements. Therefore, it is not correct that Fresnel discovered the structure and 'just' misidentified the nature of light. (cf. Worrall, 1990a, 25)

6.3. Fresnel and the Pessimistic Meta-Induction

It may be interesting, at this point, to sketch how the aforementioned case-study can show that the pessimistic meta-induction is wrong, at least when applied to Fresnel's case. In order to do so let us divert into Laudan's account of Fresnel's theory. Laudan has argued that the optical ether was central in Fresnel's theory of light. As he put it:

"Within the theory of light, the optical ether functioned centrally in explanations of reflection, refraction, interference, double refraction, diffraction and polarization. (...) (O)ptical ether theories had also made some very startling predictions, e.g., A. Fresnel's prediction of a bright spot at the centre of the shadow of a circular disk; a surprising prediction which, when tested, was proved correct. If that does not count as empirical success, nothing does!". (1981, 225)

Since no optical ether exists, Laudan's claim, conjoined with the assumption that a theory cannot be approximately true unless its central assumptions are correct, (or equivalently, unless its central terms refer) (*op.cit.*, 230), gives rise to the contention that there is no sense in which Fresnel's theory is approximately true. (*op.cit.*, 232-3)

It is a well-established philosophical fact that we lack a formal theory of approximate truth, or equivalently, that all attempts to capture formally our intuitions about approximate truth have failed. But, I think, despite this failure, there are clear-cut ways in which past theories, when examined individually, can be shown to be approximately true; and this can be established by particular case-studies in the history of past scientific theories.[11] Be that as it may, Laudan's pessimism would follow only if one was to grant that a theory cannot be approximately true unless *all* that it says about the central entities

[11] I have attempted to substantiate this claim in my (1994a); (1994b).

that postulates are correct. There are independent grounds for arguing against this presupposition.[12] But, for the current purposes it would be enough to examine whether Fresnel's 'optical ether' was as central as Laudan has argued.

Laudan is, in a sense, right. One must certainly grant that Fresnel was committed to the existence of a carrier of light waves. Hence, if we take 'optical ether' to refer to the carrier of light waves, then the assumption that light needs a carrier was really central in Fresnel's theory, and in all subsequent theories of light propagation. Moreover, it is arguable that this assumption was a correct account of light-propagation. But, the interesting issue is not this. It is rather whether Fresnel's optical ether, that is the carrier of light-waves, was necessarily mechanical and, even if he thought it was, whether as such it played a central role in Fresnel's theory. Both issues are in need of further examination, but it is not outright true that a mechanical ether played a central and working role in Fresnel's theory.

What is generally true is that Fresnel never ceased to interpret physically the amplitude of oscillations as representing the velocities of the displacements of the ethereal molecules. But this is a quite general assumption about the carrier of the light waves, rather than a specific assumption about the constitution of this carrier. And it is not clear how committed Fresnel was to any specific mechanical model of ether. For, as I said, when the chips were down, that is when he was to demonstrate laws, his concern was to find an appropriate formulation of the principle of conservation of energy. He then interpreted the amplitudes of oscillations as the velocities of the displacements of ethereal molecules in order to be able to derive, use, and give a specific physical significance to the principle of the conservation of energy for the propagation of light-waves. From this it does not follow that he was committed to a specific account of a mechanical realization of this energy. (cf. Fresnel, 1822/1866, 493)

In this connection, it is also important to notice that Fresnel introduced a model of the carrier of the light-waves based on the dynamics of an elastic solid because he correctly understood that a model based on the dynamics of a fluid, like the one underlying the propagation of sound, was inadequate for an understanding of the propagation of light. In fact, he had a heated debate with Poisson over this issue (cf. Rosmorduc, 1975, 18-20). In this debate, Fresnel stressed that the constitution of the carrier of light-waves was yet unknown. He protested against Poisson's attempts to capture some properties of light-propagation in terms of a fluid-model which shared some statical

[12] cf. previous footnote, and also McMullin (1987) and Boyd, (1990), 186-187.

properties in common with the light-propagation, yet it was different dynamically (cf. Rosmorduc, 1975, 20). He correctly observed that his fundamental theoretical mechanism of light, that is its transversality, can be modelled dynamically, and hence have any chance to be further understood, only by means of a medium possessing elastic properties, that is, properties in virtue of which it resists deformation. Nonetheless, it is quite a step to argue that Fresnel was committed to an identity between the elastic solid model and the physical system underlying the light-propagation.[13]

In fact, my case study showed that Fresnel's important discovery about the mechanism of the propagation of light, i.e. transversal propagation, did not depend on any specific mechanical assumption of the constitution of the carrier of light-waves. Nor did the discovery of the Fresnel laws depend on any such model. Instead, it was dependent upon sound general principles, already known fundamental properties of light, such as its transversal propagation, and the correct mathematical formulation of the laws of propagation of light in terms of vectorial quantities.

Hence, it is not at all obvious that the mechanics of an elastic solid, or any other mechanical model, was central to Fresnel's theory.[14] If I am right in my account of Fresnel's theory, then it is arguable that there is a sense in which Fresnel's theory of the propagation of light was approximately true. This theory discovered and stated explicitly many important properties, features and mechanisms of the light propagation, the formulation and demonstration of which was not dependent on any detailed assumptions about the constitution of ether, that is the carrier of light-waves. The approximate truth of Fresnel's theory is best explained by the fact that it rested on sound general principles of

[13] Worrall has argued that "no one can read Fresnel's work without becoming convinced that he believed in a real material ether" (1990a, 13). But almost immediately after, he emphasized that "it would clearly be a mistake to infer that the elastic solid ether 'functioned centrally' in Fresnel's explanations of interference, double refraction and the rest". (1990a, 14)

[14] I have argued elsewhere (cf. 1992; 1994b; forthcoming) that a distinction between the physical constitution and internal connections of the carrier of light waves and the several mechanical models that were used, in virtue of specific positive analogies, to penetrate into the nature of the carrier of light-waves, underwrote the whole research programme of optical ether from McCullagh and Green up to Maxwell's electromagnetic model. This distinction is vindicated by the use, on the one hand, of general dynamical considerations in terms of Lagrangian mechanics for the specification of the general laws of dynamical behaviour of the carrier of light waves independently of particular models, and the use, on the other hand, of specific mechanical models, whose known behaviour was examined in order to cast light to what the detailed constitution and internal character of the carrier of light-waves might be. In particular, most scientists were committed to the view that the behaviour of the carrier of light-waves must be such that it obeys the Lagrangian equations of motion and the principle of conservation of energy, but were not committed to any particular mechanical model of this carrier, let alone an elastic jelly one.

physics and a correct account of fundamental properties of light-propagation.[15]

7. *Maxwell on Structure and Nature*

I said in section 1 that if Worrall's distinction between structural and nature is understood as a methodological suggestion, then it reveals a deep and correct insight into the character of physical processes. Let me now elaborate on this point, and stress that it can cast some light on the realism debate.

It was James Clerk Maxwell who pointed out what I shall call a methodological distinction between structure and nature of a physical process, like light-propagation. He distinguished between the geometrical character of the process of light propagation and the nature of the physical quantity that constitutes light (Maxwell, 1890, Vol. 2, 763ff.). By the geometrical character of the light propagation he meant all the properties of this process which are independent of the nature of light. The latter, in turn, is the physical quantity which is involved in the process. Given his contemporary status of knowledge, Maxwell stressed that light can be "a displacement, or a rotation, or an electrical disturbance, or indeed any physical quantity which is capable of assuming negative as well as positive values" [i.e. it can be represented as a vector] (1890, Vol. 2, 766). All these different physical quantities share in common a structural or geometrical pattern: they are vibrations and are expressed by (one-dimensional) equations of the form

$$V(x,t) = A \cos (nt-px+a) \qquad (13)$$

where A is the amplitude of the oscillations, the time $2\pi/n$ is the period, and the factor $(nt-py+a)$ is the phase. A configuration of this vibration represents a wave, with specific wave-length and velocity of propagation (1890, Vol. 2, 766). He pointed out that we can also determine other features of this

[15] Whittaker has also argued that mechanical models of ether were not central in Fresnel's theory of light. For instance, he stated that "Fresnel's investigations can scarcely be called a dynamical theory in the strict sense as the qualities of the medium are not defined. His method was to work backwards from the known properties of light in the hope of arriving at a mechanism to which they could be attributed; he succeeded in accounting for the phenomena in terms of a few simple principles, but was not able to specify an ether which would in turn account for these principles. The 'displacements' of Fresnel could not be a displacement in an elastic solid of the usual type, since its normal component is not continuous across the interface between two media" (1951, 125). I think that this is generally true. Yet, Whittaker also argued that Fresnel used basically geometrical reasoning to arrive at his results, and he then devised a dynamical scheme to fit them (1951, 19). I think that this is wrong. For, as I tried to show, Fresnel utilised *physical principles* in his reasoning, and, in particular, he used dynamical principles such as the principle of conservation of energy.

process, its finite velocity of propagation, its generation of phenomena of interference and polarization, its transversality (*op.cit.*). None of these geometrical features of the process of the propagation of light can determine what physical quantity is propagated, and therefore, the upshot of Maxwell's point is that we can scientifically study and discover facts about the physical process of the propagation of light without being initially committed to any physical quantity as constituting light.

We must immediately stress that this distinction is methodological for it creates no epistemic dichotomy between the structure of a process and the entity involved in it, let alone a physical dichotomy. Before acquiring abundant evidence that light is an electromagnetic disturbance, Maxwell was stressing that the laws of propagation of light can be studied without any initial commitment to what light is. His point was that since evidence cannot currently settle the issue about what physical quantity constitutes light, and since we can study and discover many features of light-propagation without commitments unsupported by evidence, we had better do the latter and, at the same time, seek for further evidence which can determine the physical quantity that constitutes light. But, after this evidence came in, Maxwell was ready to affirm that "light itself is an electromagnetic disturbance in the form of waves propagated trough the electromagnetic field according to electromagnetic laws". (1864, 42)

Yet, even before this fundamental discovery, he didn't oppose the geometrical features of the propagation of light to its physical nature. For instance, in citing interference, transversality and the like as features of the process of propagation, Maxwell begun like that: "A further insight into the physical nature of the process is obtained from the fact that..." (1890, Vol. 2, 766). And later on he presented some evidence in favour of the electromagnetic theory of light, that is, in favour of the theory that light is 'an electric displacement and an magnetic disturbance at right angles to each other' (*op.cit.*, 766 & 772). For Maxwell, although the geometrical features of the propagation of light do not uniquely determine its nature, the latter can be found and known no less than the geometrical features can.[16]

Maxwell's insight can be elaborated as follows: it is a substantial discovery about the physical world that, on some occasions, notably the light-propaga-

[16] As James Clerk Maxwell put it: "The other analogy, between light and the vibrations of an elastic medium, extends much further [than the analogy of light as moving corpuscules], but, though its importance and usefulness cannot be over-estimated, we must recollect that it is funded only on a resemblance in form between the laws of light and those of vibrations. By stripping it of its physical dress and reducing it to a theory of 'transverse alternations' we might obtain a system of truth strictly founded on observation, but probably deficient in the vividness of its conceptions and the fertility of its method..." (1890, 155).

tion, we can isolate and study some physical phenomena without being initially committed to exactly what physical quantity is involved in them. In other words we may have abundant evidence about what Maxwell called the 'geometrical features' of a process, and yet we may be ignorant about exactly what is the physical quantity that possesses these geometrical features. There is no *a priori* reason that we shall never be able to penetrate further into the physical objects whose many geometrical features we know. On the contrary, when sufficient evidence is amassed, we can claim that we have grasped what these objects are. But, we must differentiate our commitments to them, and hence our confidence about what we can claim we know, according to what evidence we possess and how it supports our theories; or, as Stein put it, we must distinguish, as far as we can, "between what is known with some security, or held at least with some probability, and what is bare and even implausible conjecture". (1989, 62)

The significance of Maxwell's (and Worrall's) insight for the realism debate is that scientific realism is not an all-or-nothing theory, in the sense that one must believe everything that a scientific theory predicates of the world to an equal degree or, else, believe in nothing but observable phenomena. In a sense, if scientific realism is to be plausible and, as most realists would urge, in concordance with the actual scientific practice, then it must go for differentiated commitments to scientific theories and what they entail about the world, in accordance to what evidence supports them, as a whole and in parts. Similarly, if the versions of agnostic instrumentalism are to be plausible and in concordance with the acrual scientific practice, they must allow space for cautious belief in the theoretical claims that scientific theories make, in view of what evidence supports them.

Structural realism, insofar as it rests on a physical and epistemic dichotomy between 'structure' and 'nature', and insofar as it makes only structure knowable, cannot be the best of both worlds, (i.e. scientific realism and agnostic instrumentalism). But, we must acknowledge that, after Worrall's attempt to specify an in-between alternative, we are wiser about what it must be involved in the scientific realism vs. agnostic instrumentalism debate. This alone would be enough for recognizing that Worrall (and Poincaré and Stein) have made a substantial contribution to this debate.

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