UNIVERSITY OF CALIFORNIA, SAN DIEGO

Beyond Structural Realism

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by

Mark Philip Newman

Committee in charge:
Professor Craig Callender, Chair
Professor Nancy Cartwright
Professor Gerald Doppelt
Professor Alfred Manaster
Professor Naomi Oreskes
Professor Kyle Stanford

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The Dissertation of Mark Philip Newman is approved, and it is acceptable in quality and form for publication on microfilm:

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Chair

University of California, San Diego

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DEDICATION

To my amazing wife Jill, who inspires me to achieve and lights up my life.

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VITA

Education:
1995-1999 California State University, Sacramento, B.A. Philosophy
1999-2004 University of California, San Diego, M.A. Philosophy
2004-2006 University of California, San Diego, Ph. D. Philosophy

Publications:
"Ramsey-Sentence Realism as an Answer to the Pessimistic Meta-Induction", *Philosophy of Science* (Supplement), vol. 72, No. 5, 2005

Teaching and Research Appointments:
2004 (Summer), 2005 (Spring, Summer, Fall) Philosophy Department Lecturer, UCSD
2001-2004 Philosophy Department Teaching Assistant, UCSD
2000-2001 Teaching Assistant, Revelle College Humanities Program, UCSD
1999-2000 Philosophy Department Teaching Assistant, UCSD

Services to the Philosophy Department:
Head Teaching Assistant, 2003-2004

Fellowships and Awards:
2003-4 Teaching Assistant of the Year Philosophy Department, UCSD
2005-6 Departmental Dissertation Fellowship, Philosophy, UCSD
1999 Academic Achievement Award, California State University, Sacramento

ABSTRACT OF THE DISSERTATION

BEYOND STRUCTURAL REALISM

By

Mark Philip Newman

Doctor of Philosophy in Philosophy

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Professor Craig Callender, Chair

How can a scientific realist answer the critic who claims we should be skeptical of scientific claims because past theories have turned out to be false? Realist arguments have so far failed in all of their responses to this problem. An imaginative answer is Structural Realism. On this approach one takes a realist stance only towards the *preserved structure* of our best scientific theories. This way we should be able to cease worrying about troublesome ontology such as the luminiferous ether, and have faith that we have captured the correct *form* of the world.

In this dissertation I tease apart four articulations of structural realism, and prove that each fails to answer the problem. Epistemic Structural Realism tells us to believe in the *equations* retained across theory transitions; we put our faith in low-level mathematical articulations of physical phenomena. The second form is Ramsey-Sentence Realism, which suggests we believe only in the *Ramsey-Sentences* of our best theories, for that is where their confirmed cognitive content lies. Third, Partial-Structures Realism, appeals to the *invariant set-theoretic structure* of our theories. Lastly, there is a view called *Semirealism*, which advises we believe only in the

structurally represented detection properties of our theories; a position relying heavily on the notion of causal interactions being reliably detectable.

In each case I precisely explain these various notions of structure. Once this is done I show that each faces a trilemma: (1) collapse into empiricism, (2) collapse into full-blown scientific realism, or (3) hold such an abstract position as to become trivial. None of these alternatives is attractive because none of them justify a realist response to the initial problem.

I conclude that securing a realist answer to the history of science requires an appreciation of the heterogeneous nature of correspondence relations between theories and that only by appeal to a pluralist interpretation of the Correspondence Principle can we hope to save the scientific realism.

Chapter 1

Scientific Realism and the Pessimistic Induction

What does science represent when its theories postulate various entities, mechanisms and laws? Is it describing the real world? If so, then should we believe what these theories say? If we answer 'yes' then we are led to think that our surroundings—tables, chairs, plants, frogs, and everything else—really are all made of molecules, and these molecules are all composed of atoms. Electrons circle atomic nuclei which are made of protons and neutrons. Inside protons and neutrons we find quarks stuck together with gluons. From these elementary particles we build ships, cell phones, and bombs. If we are fortunate the bombs go unused.

On the other hand, maybe scientific theories are just useful instruments we devise that help us to interfere with and control the natural world. It is entirely possible that we are wrong about even our best guesses regarding the unobservable entities, mechanisms and laws of this universe. Genes might not exist, nor viruses. Perhaps gravity isn't space-time curvature at all. We can even imagine that electrons are mere fictions.

These two positions take very different stands on whether our scientific theories give correct accounts of the world. The former characterizes an optimistic point of view, and the latter can only be considered a moderate form of skepticism. One fairly popular argument suggests that we ought to adopt the optimistic view of science. This argument simply looks to the remarkable success, both predictive and explanatory, that our best scientific theories have achieved, and suggests that it would have to be a miracle for all this success to come from false theories. Therefore, our best scientific theories must be at least approximately true. This particular argument is known as the 'No-Miracles Argument' (NMA) and has most famously been propounded by Putnam (1975a, 1978):

> The positive argument for realism is that it is the only philosophy that doesn't make the success of science a miracle, that terms in mature scientific theories typically refer (this formulation is due to Richard Boyd), that the theories accepted in a mature science are typically approximately true, that the same term can refer to the same thing even when it occurs in different theories—these statements are viewed by the scientific realist not as necessary truths but as part of the only scientific explanation of the success of science, and hence as part of any adequate description of science and its relations to its objects[1]

This sentiment has been echoed by many other realists, and indeed it strikes most of us as at least *prima facie* plausible. I have no interest in providing a detailed analysis of this particular form of the argument, but we will very much be concerned with one response to it which appeals to the history of science. We will get to that in a moment, but the general project throughout this dissertation will be concerned entirely with reasons both why one ought not be skeptical, but also why we ought to do our best to avoid naïve optimism about scientific theories. The position I will defend is a very moderate form of optimism—a modest 'pessimistic' scientific realism.

[1] Putnam (1975a)

1.1 What is Scientific Realism?

Our starting point is with generating a clear formulation of scientific realism. This isn't going to be easy because there are many different forms of scientific realism, each with specific commitments and principles. In an attempt to capture the core principles of the position I list the following common candidates:[2]

1. *Axiological realism*: the aim of science is to provide true (or approximately true) theories about the world. This might initially seem obvious, but it is in fact a disputed issue. Take for example those empiricists who take the aim of science only to be the development of theories that are empirically adequate. On this view, we shouldn't even be trying to provide true theories because it is in principle impossible to determine the truth or falsity of claims about the unobservable world. Contrary to this opinion, the scientific realist believes that truth is a perfectly legitimate goal for science to pursue, whether this be for the reason that he believes we do in fact sometimes achieve this aim, or for some other reason. So, the principle of axiological realism says that science does attempt to generate direct correspondence between the way the world is and our theories that try to describe it.

2. *Epistemic realism*: science provides true (or approximately true) theories about the world. This principle just says that science sometimes achieves the goal of true descriptions of reality. Importantly, this goal is accomplished for theories that talk not merely about the observable, but also the unobservable components of the world. This

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[2] This taxonomy follows the general form of that found in Niiniluoto (1999, p. 2). However, the actual content of each thesis I have changed more or less substantively, to fit what I take to be a better reflection of the realist position.

is a principle that doesn't simply claim that the correct correspondence relation between theory and world has been achieved, it also claims that we have good reasons to believe that relation holds. The distinction points to the important difference between truth-conditions for a set of claims persisting and our possession of evidence-conditions for those claims. Truth-conditions make the claims true, whereas evidence-conditions justify, or provide warrant for them. This is an important difference for our discussion because the empiricist can accept that sometimes we may stumble on the truth about unobservable reality, but he denies that we have good evidential reasons for believing it. The scientific realist, in contrast, thinks that both sets of conditions are satisfied by our best sciences.

3. *Metaphysical realism*: the world that our scientific theories describe is mind-independent. This principle is very much that of the common-sense view that the world we live in exists independently from the way in which we think about it. There is some controversy over whether this entails that if our theories are correct about the natural kind structure of the world, then this structure has nothing to do with how we came to know this fact. Some realists take the principle to apply to natural kinds in the world being fixed, whereas others are a little more suspicious of the notion 'natural kind' to begin with. However, in either form the principle's suggestion is in direct conflict with various types of traditional idealism, as well as phenomenalism, and it is in this sense that metaphysical realism suggests that although the world may be carved in different ways by our theories, the way that it is, does not depend on us.

4. *Methodological realism*: science is the best method for attaining true (or approximately true) theories about the world, and philosophers of science ought to

adopt scientific realism (or at least the remaining theses listed here) as a regulative ideal. This is really a form of naturalism, suggesting that because we ought to measure scientific success according to these other theses, we should take its methods of investigation to be the most successful of any that have been used to generate knowledge of the world in which we live.

5. *Semantic realism*: the statements in scientific theories are to be interpreted literally. This is also an important thesis because it separates out the scientific realist from some empiricists who think that what a theory says can be entirely reduced to observational vocabulary, and also from some instrumentalists who think that a scientific theory uses theoretical vocabulary merely as a calculational device, and that statements couched in these terms do not even have a truth-value.

Although none of the theses above logically conflicts with any other, they are each designed to advocate a specific answer to particular questions regarding the role and status of scientific theories. Some such questions are: What are the goals of science? Are those goals achievable? Why should we believe scientific claims? Do the theoretical entities and processes postulated in our theories really exist? Do they exist independently of us? Is science the best route to knowledge about the world? Should we accept what science tells us to be correct about the underlying nature of reality, or is it better to treat its claims as mere instruments for prediction and control?

It should be clear from the theses above how scientific realism attempts to answer these questions. One who adopts the first thesis (axiological realism) addresses how science approaches the world, but does not necessarily argue that science has attained a grasp on that world. To make the latter claim is to adopt thesis two. This pair, axiological realism and epistemic realism, are perfectly compatible. However, some philosophers of science believe that epistemic realism is too strong, although they still take their positions to be axiologically realist. This I find somewhat peculiar because it seems that we would have little reason to hold on to truth as an aim for science if we didn't have some reason to think that we were in fact capable of approaching that goal. If we don't believe that our current best theories are at least approximately true in some regards then why think that reaching the truth is ever going to happen? Perhaps this is overly skeptical, and the axiological realist will respond that there is nothing that causes tension between aiming for a particular outcome and knowing that you won't achieve it. There may be all sorts of other benefits that accrue to science along the way, and these would contribute to our adopting just the axiological assumption. More importantly, we have good reasons to think that because of our limited cognitive capacities, humans are unlikely ever to fully grasp an understanding of the universe, but, argues the axiological realist, this doesn't give us good reasons to give up trying. I don't have a response to this reaction, except to say that if the axiological realist takes this statement in its strong sense, implying we shouldn't believe we've got any good understanding of the theoretical, then he seems to be just flat-out wrong. On the other hand, if he concedes some understanding, and claims some knowledge based on it, then his argument fails.

Moving on, thesis two, epistemic realism, is obviously much stronger than thesis one; its supporters claim that we actually access and accurately represent the world in our scientific theories. However, this assertion raises the contentious question why we should think we are justified in such beliefs. Why think that we have got things right? The realist commonly replies that it seems unreasonable, given currently successful practice, to think the alternative, that we have got things wrong. As I will later explain, this argument is not going to satisfy the anti-realist.

Thesis three, metaphysical realism, raises another traditional philosophical concern; whether there even exists a mind-independent world for our sciences to access. The broader debate in philosophy is usually cast in terms of realism and skepticism, and in the philosophy of science the issues are not much different. Here, however, the anti-realist need not be thought of as a skeptic, since the apparent metaphysical independence of everyday objects is not at issue. Here the argument is over theoretical entities and their properties and relations. In the realism/anti-realism discussion there is significantly less debate regarding these metaphysical issues, and in fact recently most philosophers have focused almost exclusively on epistemic realism. One of the reasons for this situation is a prior shift in the history of the debate, from concerns over semantics and methodology, to those of justification of belief.

Philosophers of science working on scientific realism are not all committed to thesis four, which suggests that we not only take scientific methods as being the most successful at generating new knowledge, but also that as philosophers we ought to adopt a naturalist stance and use *a posteriori* means of justifying claims about science. Naturalized epistemology is really the adopted approach for these philosophers, in contrast to realists like J.J.C. Smart or Grover Maxwell who advocated different forms of scientific realism on more generally philosophical grounds, like plausibility or the high prior probability of the position in contrast to its alternatives.[3] Although non-naturalistic approaches have their merits, we are here focused on scientific realism being motivated by the NMA put forward by Putnam and Boyd. Their argument was definitely within the naturalist tradition in that they assumed it a contingent fact that science tends to deliver (approximately) true theories, as well as presuming that a realist epistemology should adopt those self-same methods as science. The scientific realist position they advocated is the most prevalent, and as such we will also include thesis four in our formulation.

Lastly, thesis five, semantic realism, is now accepted by almost everyone. It was, in the first half of this century, the primary focus for the scientific realism debate, but reductive and eliminativist programs hostile to the thesis failed to overcome severe logical and linguistic barriers. As a result, non-literal interpretive programs fell by the wayside.

So, in light of these principles and their implications, how should we formulate scientific realism? It seems prudent to adopt the relatively uncontested theses three, four, and five since these seem to be generally accepted as uncontroversial by the community of realists in the debate. Thesis one also might seem initially unproblematic. What could be wrong with aiming at truly representing the world in our scientific theories? Well if we think about it for a moment, perhaps truth is aiming too high. There are several arguments that suggest truth is a 'utopian value'[4]. This

[3] For a description of this *a priori* interpretation of Smart and Maxwell see Psillos (1999, pp. 72-77).
[4] See Laudan (1984, p.52) for this argument.

means that truth is rationally unattainable because we really have no idea of how to go about adopting methods of investigation that will ensure the realization of our goal.

As an example, imagine someone said they wanted to fly using only the power of their own flapping arms. You might laugh, or if they are about to jump from a building you might plead for them to halt, but either way, it is unlikely that you'll encourage them. It is common knowledge that there are significant physical constraints on human beings that prevent us from flying—amongst other things, we lack feathers or stretchable membranes on our arms, so we have a terrible time creating sufficient fluid resistance to generate enough lift for flight. Human flight of this kind is just not physically possible, even though not logically impossible. It seems silly to advocate an aim or a goal like this when it is unattainable. Indeed, many have argued that the definition of rationality itself entails that one be capable of fulfilling one's goals.

Similarly, we have to ask whether truth is attainable for our scientific theories. If not, then why hold onto truth as a goal for science? In fact, there are several ways in which we might brand a goal 'utopian'. First, there is 'demonstrable utopianism'. As with our flying example, we sometimes find a goal defies logic or the laws of nature. In such cases we can say that the goal is demonstrably unachievable. From the history of science one example of a demonstrably utopian goal was the search for infallible knowledge. Since such knowledge in science would presumably include universal generalizations about physical phenomena, and since empiricists use only experience for confirmation of hypotheses, confirming an infinity of instances is obviously impossible. Therefore, confirming universal laws, for the empiricist, is also impossible.

Second, there is ‘semantic utopianism’, which is where scientists adopt a goal which is actually impossible to clearly articulate and define. This might initially sound daft—when we adopt goals, surely we know what they are. Still, when we look to supposed super-empirical (theoretical) virtues like simplicity or strength, it is surprisingly hard to provide a concise and precise definition. This has also famously caused countless headaches for philosophers trying to define laws of nature.

Third, and perhaps most important for our purposes here, is ‘epistemic utopianism’. It may be perfectly clear what we mean by a goal, and it might similarly be obvious to us that the goal may be practically attainable, however in this case we are incapable of specifying a set of criteria the satisfaction of which we can say determines that the goal has been attained. We won’t know when we have succeeded. This last form of utopianism is essential in our discussion because scientific realists can be accused of setting the truth of our theories as a goal for science with no story for how we can be sure we have attained that goal.

These forms of utopianism should make clear the perils of advocating truth as a goal for our scientific theories. If realists should be taken to adopt truth as an aim for science then surely they should also be required to provide a semantics for truth and a theory of confirmation. In fact I will be arguing in chapter five that any form of realism requires at least a sketch of a theory of confirmation. But aside from these fairly deep problems, shouldn’t we at least require that if our definition of scientific realism is to include thesis one—truth as a goal—then it should also adopt thesis two—the claim that science does attain truth? After all, we see from the utopian arguments that it would be muddle-headed to adopt an aim like truth if we didn't think we had the means to achieve it. Furthermore, thinking a goal achievable seems best supported by evidence that it has already been achieved. Given this, adopting thesis two would justify our adopting thesis one.

However, many realists are hesitant to adopt thesis two in light of the fact that some of our current theories are incomplete, multiply interpretable, or even internally inconsistent. How can our theories be true if they don't even make sense? So, perhaps we should weaken thesis two and only insist that some of our theories are close to the truth (whatever that means).

Here then is my formulation of scientific realism:

Scientific Realism:

Science aims at providing us with literally interpreted approximately true theories that represent a mind-independent reality, and is not only the best means of attaining such knowledge, but also sometimes succeeds in its aim.

There are just a few comments I would like to make about this definition. First, there may be other aims for science. Just because truth is one of the aims, there is nothing wrong with scientists investigating the world for reasons other than generating true theories. They may well work for a research laboratory trying to develop an effective drug that will combat the spread of melanoma. Although developing such a drug may seem to rely upon true hypotheses and theories, even if it does there is nothing in such research that requires the origination of new theories.

Second, the notion of representation has been included in our definition to highlight the fact that a great proportion of scientific theories use more than linguistic expressions to reflect the relationship between theory and reality. As we'll see especially in chapter four, the use of models is extensive in scientific theorizing. These models vary tremendously in type. There are scale models, set-theoretic models, mathematical models, theoretical models, and even maps get used as models. Unlike sentences or propositions, in general we don't think of models as having truth values. They are objects, often abstract, that purportedly represent parts of the world via some representation relation. The definition of this relation is highly controversial, and we have no need to enter that dispute here. Still, what I want to point out is that whether one takes theories to be collections of models or merely to contain models of one form or another, these models are supposed on the definition above to reflect reality in some accurate manner.

Third, the notion of approximate truth is notoriously vague. We don't yet have a complete theory of approximate truth, and to the degree that the realist asserts our goal to include a vague and imprecise notion like this he is subject to the charge of semantic utopianism. This doesn't bother me too much since we generally think it hard to imagine our best theories about genes, molecules, bacteria, and the like as being horribly off the mark. Such vagueness is acceptable in our definition if we appreciate the difference between *optimistic* and *pessimistic* forms of scientific realism.[5] An optimistic scientific realist adopts a strong form of thesis two, arguing that our current best scientific theories are revealing the true structure and mechanisms of the unobservable as well as observable world. The pessimistic realist adopts a much weaker notion of thesis two, arguing that not only are the approximations to truth that our theories make often further from the truth than the optimist might think, but also that the frequency of our success in achieving this aim is far smaller. In other words, the pessimist takes the optimist to be too quick to pronounce victory in science's ability to unveil the true structure of reality.

But the pessimistic realist is not an anti-realist, because there are limits to how skeptical one should be. We are working with a continuum, where the optimist is at one end, the empiricist at the other, and somewhere in between sits the pessimistic realist. The pessimistic realist thinks it is still *reasonable* to hold approximate truth as an aim for scientific theories, because he thinks that sometimes we do actually achieve this aim. He does however, wish to accommodate the fact that we've very often been wrong in the history of scientific investigation and generated theories that were once thought to be true, but later turned out false. This fact doesn't entail that we should give up either our positive aim or our claim to success. It just means we should be very cautious in advocating realist claims.

Pessimistic realism is more reasonable than its optimistic cousin precisely because we have many reasons to be cautious of claiming approximate success for our current best theories in science—local underdetermination for example is sometimes

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[5] Godfrey-Smith (2003) raises this distinction.

thought to provide such reasons[6]. However, the pessimistic realist could possibly be defeated by an extreme pessimism, which can come in the form of global underdetermination[7] or historical pessimistic induction arguments. Since I am not convinced that there really are any compelling arguments of the former variety, we shall only consider the latter. In fact, we can now move on to consider the strongest recent version of historical attack on realism, which if cogent could devastate not just optimistic but also pessimistic forms of scientific realism.

1.2 The Pessimistic Meta-Induction on the History of Science

Scientific Realism has a number of plausible arguments in its favor, one of which we saw above in the form of an appeal to rationality. The NMA argues that it is irrational to believe in miracles, and it would have to be a cosmic miracle for successful scientific theories to be anything but approximately true. Therefore, if one wishes to be rational one ought to believe in the approximate truth of successful scientific theories.

Although there are several other important arguments for scientific realism, I have space here just to mention one other that, while very simple, provides similarly compelling reasons to think anti-realists are being overly pessimistic about science. The argument I am referring to is really more of a counter-argument to the constructive empiricist, who himself suggests that the observable-unobservable distinction draws an epistemic line which differentiates that which we have access to

[6] Local underdetermination suggests that we have no principled grounds for choosing one theory over an empirically adequate alternative.
[7] This claims that there are infinitely many alternative theories.

from that for which we can never have any evidence. It therefore follows, if this assumption is correct, that we should not believe theoretical claims because although they may spin a pleasing or calculationally satisfying story, they do not constitute evidence for our theories.

In response, the realist can make a number of very different arguments. Some suggest that we have good reasons to think that the very distinction between observable and unobservable is unsupported by evidence. Others take it that even if the distinction is plausible, we have very good reason to think that explanatory virtues, such as simplicity, unity, consilience, fruitfulness, or perhaps even causal mechanism somehow track truth. More direct than either of these routes would be to accept the distinction, but question why it should be epistemically probative. That is, why should we think that just because some entity is directly observable to the naked eye, it therefore attains special epistemic status? Why assume that 'observable' is coextensive with 'accessible'? This is especially problematic when we consider borderline cases such as when eyesight becomes weaker, either through age, or perhaps dim lighting—should I cease believing in that flea egg that I could see a moment ago just because the candlepower in this room has gone down slightly?

Still, the topic here is ultimately the realist response to the Pessimistic Meta-Induction (PMI), and as such we should return to its counterpart, the NMA. This latter argument can actually be extended to include, as Putnam clearly does, the notion of referential success and naturalism, as well as of cumulativity and convergence. That is, one might argue that for our successful theories to be approximately true they must have central terms that correctly refer to the entities, processes and laws posited by those theories. One might also argue that history shows scientific theories to be both retaining the remnants of successful predecessors, and that such retention can best be explained as converging on the true account of the structure of the world.

In terms of our axiomatic schema from section 1.1 we might say that these arguments supposedly establish a form of scientific realism that includes our epistemic, methodological, and semantic theses. I take it that the axiological and metaphysical theses are implicit—although this is not an uncontroversial claim, especially in lieu of Putnam's internal realist epistemology. I won't however dwell on this idiosyncratic view, since I take most scientific realists to reject Putnam's metaphysics and adopt truth as an aim for science.

Now we can move on to a detailed response to the NMA, one that takes realism at its word as a naturalistic hypothesis and looks to the history of science for counterexamples to some of the realist's assumptions. This argument was most notably made by Larry Laudan in 1981,[8]and since it plays a central role in this project, it seems appropriate to explain some of the details of Laudan's paper.

His general strategy is to show that the realist is committed to an inferential link between reference, success and the approximate truth of scientific theories. He then proceeds to show how none of these notions are either necessary or sufficient to license inference to the others, thus proving scientific realism to be founded upon inadequate premises.

Let's look at how he does this. His first move is to assume the realist is committed to the following two premises:

[8] Laudan (1981).

1. If scientific theories are approximately true, then they will typically be empirically successful.
2. If terms in scientific theories refer, then they will typically be empirically successful.

From the further assumption that scientific theories are empirically successful, the realist then suggests that it is highly likely (probable) that scientific theories are approximately true and genuinely refer. (Here we can take success to mean "the 'instrumental reliability' of theories in predicting and controlling observational phenomena"[9]). Even if we accept a fairly broad notion of success, the idea that reference secures empirical success can be faulted for excessive liberality. The problem is that Putnam wants terms like Bohr's 'electron', Newton's 'mass', Mendel's 'gene' and Dalton's 'atom' to genuinely refer, while rejecting 'phlogiston' and 'ether' as non-referring terms. Laudan points out that this leaves remarkably unsuccessful theories, like the Proutian theory of atoms and the Wegenerian theory of continental drift, as genuinely referring. Thus, premise two must be false; genuine reference is not *sufficient* for empirical success. In fact, Laudan argues, genuine reference isn't *necessary* for empirical success either—just look at the successful theories of caloric and phlogiston, or a large collection of ether theories from the 18th and 19th centuries. These were all predictively as well as explanatorily successful, yet we are now confident that their central terms failed to refer.

[9] Doppelt (forthcoming, p.7). Doppelt emphasizes that this broad notion is only part of what many realists take to constitute the notion of 'success'.

But what of being less stern, and admitting that perhaps we don't need *all* of the central terms in a successful theory to genuinely refer, but only *some* of them? Does this make life easier for the referential realist? No. Laudan argues that what separates the realist from the positivist is that evidence for a theory is evidence for *everything* the theory asserts, whereas the positivist claims confirmation only extends to the observable parts of the theory. Especially for realists like Boyd, either all parts of the theory are confirmed or none are. Realists in the holistic tradition have used low-level confirmation to filter up and confirm even very high-level parts of theories—testing only specific portions of our theories. The response Laudan gives to this approach is that it runs the risk of stripping realism of its punch by only licensing belief in those directly tested parts of our successful theories. Laudan says this "would wreak havoc with the realist's presumption [premise one] that success betokens approximate truth."[10] We will see that this strategy so disliked by Laudan is precisely how some forms of structural realism hope to save us from the PMI.

We have mostly been concerned so far with premise two, so now let's head back to premise one. Here Laudan considers, what if the realist were to appeal only to the *approximate* truth of successful theories, rather than their complete truth? Although successful theories are unlikely to be wholly true, isn't it legitimate to assume they are close to the truth, and in fact are closer to it than their predecessors were? Laudan points out that this move provokes the general problem of defining 'approximate truth'. On current accounts, it does not follow that an approximately true

[10] It seems that this is precisely what modern realists are forced to do (essential as well as structural and entity realists). Yet these realists don't think that they are turning their back on the motivation for realism, as Laudan suggests.

theory will be explanatorily successful. If we follow Popper's verisimilitude approach on this score, we end up with a case in which an approximately true theory might have entirely false consequences so far as we have tested it. Laudan rejects the realist appeal to approximate truth on the grounds that an explanatory account is not clearly available in the future, and we cannot reasonably assume the entailment between approximate truth and success until we have a clear idea that verisimilitude can do the job. As of now, we have no such clarity, and promissory notes are insufficient.

Worse still, Laudan argues, even if it could be shown that an approximately true theory will be explanatorily successful, this does not entail what the realist needs: that if a theory is explanatorily successful, then it will be approximately true. He argues that if we are going to claim a theory is approximately true, we are presumably also committed to its central theoretical terms genuinely referring. That is, genuine reference is a necessary condition of approximate truth. Laudan then gives us a list of past non-referential, yet successful theories. This includes theories of the crystalline spheres of ancient and medieval astronomy, the humoral theory of medicine, the effluvial theory of static electricity, catastrophic geology, the phlogiston theory of chemistry, the caloric theory of heat, the vibratory theory of heat, the vital force theories of physiology, the electromagnetic ether, the theory of circular inertia, and theories of spontaneous generation. Since these theories do not have genuine reference, and genuine reference is necessary for approximate truth, they cannot have been approximately true. So, we have explanatorily successful theories that were not even approximately true; a counter-example to the realist's required premise.

In response to this criticism the realist might suggest that we are supposed to be talking only of the past *mature* scientific theories, which have passed a number of rigorous tests. Of course, in this case the realist has to specify exactly what 'mature' amounts to. Here we might take a clue from Stathis Psillos, who suggests this means that the theory has passed something called a 'take-off point':

> Theories that have passed the 'take-off point'…can be characterized by the presence of a body of well-entrenched background beliefs about the domain of inquiry which, in effect, delineate the boundaries of that domain, inform theoretical research and constrain the proposal of theories and hypotheses. This corpus of beliefs gives a broad identity to the discipline by being, normally, the common ground that rival theories of the phenomena under investigation share. It is an empirical matter to find out when a discipline reaches the 'take-off point', but for most disciplines there is such a point.[11]

Yet such a distinction between mature and immature science is suspicious at the very least because it is empirically untestable, but perhaps also because even if a mature science has its success explained by the realist at the present, then there is still no guarantee that it won't fail to be successful in the future. Additionally, Laudan claims that many of the immature sciences of the past were themselves successful *by the realist's own criteria*, and so the realist would have completely failed on this distinction to have explained the success of science as a general phenomenon at all.[12] The distinction does not, therefore, seem helpful.

The above set of arguments laid out by Laudan have been taken by many to constitute a single argument form for the PMI: the history of science shows us that

[11] Psillos (1999, p. 107)
[12] See Laudan, (1984, pp. 121-2).

most of what we previously considered to be mature, successful, scientific theories were to a greater or lesser extent, false. This historical evidence should lead us to conclude that our inference methods in the sciences are not reliable, and hence that we should also conclude our current theories are most likely false. Therefore, scientific realism should be rejected. Although missing many of the details from Laudan's paper, this formulation succinctly captures the idea of an induction. Instead of inferring the falsity of a theory from some empirical evidence that it proscribes, the induction infers the likely falsity of scientific realism (a supposedly scientific hypothesis) from the historical evidence of successful, but false, theories.

However, this is not the only formulation of the argument. Some don't view the argument as an induction at all, but rather as a list of empirical data that directly contradict the scientific realist position.[13] This interpretation treats Laudan's list of historically successful but apparently false theories[14] as the basis of a simple modus tollens:

P_1: If the realist hypothesis is correct, then each successful theory will be true.

P_2: We have a list of successful theories that are not true.

C: Therefore, the realist hypothesis is false.

This formulation has the benefit of brevity, yet it is clearly giving a great deal of room to the realist to respond that premise 1 is too strong. The realist hypothesis is, by its scientific nature, defeasible, and as such it can be correct and yet not every successful theory need be true. This response will lead us back to Laudan's argument

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[13] Lyons (2002).

[14] Lyons appeals to the list from Laudan's paper. Many of these were included above.

that even if one adopts the weaker notion of approximate truth, we are not guaranteed a successful theory will get us there. This formulation also has the significant advantage that it is a deductive, rather than an inductive argument. As such, it would avoid the objection that any pessimistic induction is susceptible to; that its inductive basis is neither big enough, nor representative enough to justify the pessimistic conclusion.[15]

A further formulation of the argument has been offered, this time appealing to probabilities:

> Considering past theories, we observe that many once successful theories are now believed to be false. We sample the successful theories of the past and find that many or most of them were false. We generalize and, by induction on these cases, evaluate $Pr(\neg Tx \mid Sx)$ as being rather high for an arbitrary theory x. This holds for our present successful theories; hence we should think that they, too, will turn out to be false.[16]

On this reading of the argument we can see clearly that the anti-realist is trading on the low likelihood of a theory being true, given that it is successful. This formulation has the merit of reflecting the fact that without knowledge of the base rates of success and truth in the population of sampled theories, we cannot connect such likelihood to probabilities. This turns out to be a problem for the PMI and the NMA alike. Let me explain this briefly.

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[15] Lyons points out that Psillos makes this objection in his (1999, p. 105).
[16] Magnus and Callender (2004). Also see Lewis (2001). Here 'x' refers to some theory being scrutinized, 'T' refers to the property of truth, and 'S' refers to success.

The PMI suggests we be skeptical of today's successful theories precisely because when we look to past successful theories it turns out most of them were false. There is a response to the PMI however, which says such sampling can lead to erroneous conclusions if we don't pay attention to our classes being sampled. For example, let's just assume that very few women in some population are at any given time pregnant—let's say 1%. I have a pregnancy test, which may be very reliable—let's say 95% reliable. More specifically, it always gives a positive result for pregnant women, but mistakenly says a non-pregnant woman is pregnant 5% of the time. If I sampled1000 women with a perfectly reliable pregnancy test I would find 10 of them to be pregnant. With my fallible test I get 50 women who are not pregnant appearing to be pregnant, as well as those 10 that really are pregnant. That totals to 60/1000 women appearing pregnant; 6%. Now, my test is fairly reliable, but has told me that the likelihood of being pregnant is 6%, when it is actually only 1%. So, I'm going to think it is far more likely to be a pregnant woman in this population than it really is.

Possibly the same goes for sampling from the history of science to justify a pessimistic induction. I may have a fairly reliable rule for inferring truth (success), but if the rate of past theories actually being true (given their success) is only 1% when my test is 95% reliable, I am going to end up with an appearance of hitting on the truth a great deal more than I am justified in claming. The PMI ignores this possibility, and argues that the realist thinks he is always 95% confident that a given theory is true when the theory is successful. But, the realist responds, if the truth is hard to come by, then we should not be surprised to see many past successful theories turn out to be false. All that can be drawn from the PMI is that even when we have a 95% reliable rule one has to be cautious about the confidence one puts in to a theory. This certainly shouldn't be taken as a license to throw realism out the window though.

I think this is a serious problem for the anti-realist argument only if we can make good sense of how to characterize what counts as both 'mature' and 'successful' when it comes to scientific theories. We have just seen some of the problems with the concept of a theory being 'mature'—and the notion of 'success' can be claimed to be similarly vague. But just as with differentiating theories, I do think the anti-realist has to concede it does make sense to say of some theories that they were both mature and successful. Admitting this much we might adopt Magnus and Callender's pessimism regarding the debate. We also might agree that the focus should really be on explaining the success of any single theory, not science in general.[17] That is, for some successful theory, say Newtonian mechanics, we really ought not worry about what the proportions of true to false theories are for a population of theories sampled. Here what we care about are the reasons for thinking Newtonian mechanics itself is true. Just because there may be a high base-rate of false theories in our population, that doesn't effect the truth or falsity of this classical theory in the least. The pessimist here actually insists that the NMA and PMI are both problematic because we cannot get the base rates that are necessary to get their arguments going. In response, we might simply insist that the realist still has to say *something* about why some theory is successful, and for many realists this is going to be especially difficult without appealing to notions that the anti-realist refuses to acknowledge as truth indicators (like novel predictive success, or simplicity, or unity perhaps).

[17] The following point has been emphasized by Psillos (private communication).

However, if as I think we should, we treat the PMI as a set of counterexamples to the realist's 'success-to-truth rule'—a rule he claims is mostly justified—then we can still address the NMA argument, regardless of whether it should be treated as a statistical argument or not. We see this above in the deductive form of the PMI. When we refuse to treat the PMI as an induction, but rather as a deduction, we see that anti-realists challenge the realist directly by appealing to cases that they can both agree to be scientific theories, under any construal given above. So, no matter whether one adopts the received or the semantic view of theories, the anti-realist is able to meet the realist on his own ground, using realist criteria for judging how to delineate a scientific theory, and still point to cases of past science that satisfied the success criteria yet failed to be true. This then is how I think we ought to treat the PMI—not as an induction at all, but as a set of counterexamples to an inference rule, cherished by realists.

With this conclusion in mind, we can look to the current debate for examples of particularly successful, mature, scientific theories, which even realists will have to accept as instances of apparent counterexamples to the No Miracles Argument. Three cases are now generally agreed to be problematic: caloric theory, phlogiston theory, and luminous ether theories. I will use the last of these as a running example through the dissertation, but will postpone introducing specific instances until the next chapter. There we will see the philosophical rubber hit the scientific road, and be far better able to evaluate the degree to which one specific form of scientific realism (structural realism) can wriggle its way out of this tricky spot.

Before heading into that new territory however, we need to look briefly at the various responses to the PMI, some of which we will consider at greater length and others we will leave behind. This will provide a short background that should aid in understanding the material that follows.

1.3 Realism Retreats

For traditional scientific realists the PMI, in whichever form one takes it, is devastating. Not only does it provide counterexamples to the NMA canvassed by Putnam and Boyd, it also reveals the unreliability of a key inferential rule used by realists: Inference to the Best Explanation (IBE). This rule simply claims we are justified in inferring that the best explanation for some observed phenomena is true. Laudan has cut this success to truth link by detailing how in the history of science these two concepts fail to entail one another.

Realists have several responses, and here we'll just look at the most prominent: entity realism, structural realism, and essential realism. Each of these positions adopts the method of singling-out particular parts of past and current science in a principled way, such that we can claim to be justified in believing scientific claims only when they are based on the relevant principled method. These accounts try to draw epistemic lines, where what lies on one side of the line is beyond our epistemic access, and what lies on the other side we are justified in believing to be true. Each formulates a general set of principles that is designed to answer the PMI; for without such a response, the realist picture collapses to the historicist challenge. Each account must therefore avoid the holistic approach of traditional scientific realism and find a non-ad hoc method for selecting out just those parts of scientific theories that refer to entities/properties that really seem to exist, both today, and in the past. In this way, each might reveal how and why some false theories in past science were successful, and hence show that if we use the right method, we really can account for problematic counterexamples. This sort of principled approach should then enable us to safely infer the approximate truth of our current theories when following the appropriate inference principles—if they legitimate it.

We can think about the situation in the following way. Each realist position has two tasks. First, it has to show that the principled distinction that is advocated (which draws a line between what we ought to believe in a theory and what we should not believe), has to fit with the history of science. In particular, the realist ought at least to accommodate the hard cases, like the luminiferous ether, and illustrate where those components that are required by realists really are retained across theory transitions. One could think of this task as that of generating a rule, or a set of rules, that will be capable of picking-out a thread or ribbon of entities or processes through the history of science. The things being appealed to by each account have to be there in the historical record, and they have to be retained through to the current moment. The cumulative nature of this project should be apparent, but no realist will want to argue that all of a theory is at any given time true *simpliciter*, and hence, we shouldn't expect to see that ribbon to be the entirety of any one given theory. To the degree that this entails we still don't have the full truth about any given domain, and that all of our theories are in some way incomplete, then we have a perfectly reasonable reflection of the realist's commitment to fallibilism.

The second task facing any realist approach is to show that the components being selected from various theories are adequate to provide an explanation of the phenomena for which the theory is supposed to account. This follows straight from the realist's popular strategy of using IBE as the rule for justifying belief in theoretical entities. If one agrees that this rule has been seriously undermined by the PMI, that is, if one thinks that successful explanations are included in the initial use of the 'success-to-truth' implication, then the retreating realist has to provide an account of IBE that now appeals not to the success of the whole theory, but only to those parts condoned by their account. That means that a realist now has to show how those specific parts of a theory that generate predictive success are the same parts that provide explanatory success. If they didn't satisfy this requirement, then the realist could be accused of falling back into an out-dated positivist notion of 'explanation-as-prediction'. That is, early accounts of scientific explanation took prediction to itself be explanatory. If you could use Newton's laws to predict the path of a comet then that was good reason not only to think the theory empirically adequate, but actually true, because explanatory. We now know however, that there is very little explanatory content to a mere derivation, unless perhaps we are explaining one law in terms of another. Since realists take explanation to be the indicator of truth-tracking, they need to avoid such deductivist accounts, and reveal how the traditional, more typically causal, notions of explanation can still work even with less theoretical constituents in the reconstruction of past science.

Entity Realism

We do not have space here for an extended treatment of the very interesting position known as ‘Entity Realism’, which is a great shame because it holds a great deal of promise. I will therefore just mention the main themes of the position and consider a couple of arguments that have often been made against the view. It should be made clear however that these are in no way to be considered ‘knock-down’ arguments against entity realism, and in fact I think the position has a great deal more going for it than is typically thought. There is a little irony that in Chapter five I will end up advocating for a position with very strong ties to entity realism, and it will be left to future work to unravel the similarities and differences between the two approaches. For now, though, a minimal sketch must suffice.

Entity realists do not infer from empirical success to the truth of an *entire* theory, only to the *entities* postulated by the theory, and as such are committed to most of the entities of past science. For those entities obviously incorrectly described by past science, like the luminiferous ether, or phlogiston, the entity realist will have to generate some plausible story for why we should not take that specific case to count against their view on current science. These realists are all united in limiting epistemic realism by insisting that although science is able to provide us with access to the correct entities, truth claims regarding theories themselves are unjustified. For example, it is legitimate for us to accept the existence of electrons, but not the truth of theories that posit them. Despite this consensus, entity realists espouse a variety of theses in regard to IBE. Some accept the rule, some are only somewhat critical of it, others accept it only in those cases where there is a *unique* explanation, and others reject it outright. Even without IBE, surrogates are available which apply only to entities. Hence, these entity realists can all advocate inferring to the existence of theoretical entities, but not to the truth of scientific theories in general. In this way the entity realist avoids being lumbered with the problems associated with IBE, while not falling into the skeptical position.

The entity realist therefore diverges from the traditional realist's commitments by limiting the epistemic thesis. Instead of claiming that science provides true theories about the world (epistemic realism), they claim that it is correct only about the entities that arise in those theories. We might call this the 'epistemic entity realist' thesis.

There are a couple of apparently major problems for entity realism. First, the position just seems on the surface to be incoherent. How can we claim to be realists about something, say electrons, if we don't think there is justification for believing in properties that they might have? After all, it is the theory itself that describes the electron's properties, but the entity realist rejects the theory. How are we to believe in electrons (just their existence), if we are not willing to believe something else about them? We are presumably restricted in just this way because it is only electron theory that tells us anything about electrons. Other theories that refer to these entities presumably do so contingent on a prior understanding of that part of quantum mechanics which deals with these entities. To the degree that other theories might inform us of further properties of electrons, they modify this prior theory about these entities.

Alan Musgrave provides a forceful analogy to this problem:

> To believe in an entity, while believing nothing further about that entity, is to believe nothing. I tell you that I believe in hobgoblins (believe that the term 'hobgoblin' is a referring term). So, you reply, you think that there are little people who creep into houses at night and do the housework. Oh no, say I, I do not believe that hobgoblins do that. Actually, I have no beliefs at all about what hobgoblins do or what they are like. I just believe in them[18]

This example brings out the point nicely. It is just wrong headed to think that we can assert the reality of something without also asserting that it has some properties.

A second supposed problem for entity realism is that it is explanatorily vacuous. For the sake of argument, let's assume that entity realism really is coherent and that it can be distinguished from other forms of scientific realism. The position provides a response to the PMI by suggesting that for the most part it is the entities that are preserved through theory transitions over the history of science. Even if we accepted this claim, then it is apparently up to entity realists to provide us with a plausible argument that establishes that the entities being posited are representing the correct objects of the world, rather than being useful theoretical posits that our scientists choose to retain through theory transition. In order to make such an argument one could appeal to IBE, claiming simply that these entities provide the best explanation of the observable world. However, if this argument is going to lend support to entity realism it has to maintain that it is the existence of these theoretical entities *alone* that is exclusively responsible for the predictive and explanatory success of our theories that involve them. And while it may be true that the existence of these

[18] Musgrave (1996). It is only fair to acknowledge that Musgrave himself admits that what he is giving here is a rather gross characterization of the position, but the point, I think still stands.

entities is supported by such an argument, it is not true that the existence of these entities alone can provide us with any predictions at all. Predictions require more than mere entity existence claims; they require auxiliary assumptions, and especially, theoretical laws. To derive a prediction one often requires mathematical equations representing the relations that hold between theoretical entities, but at the very least we need to attribute specific properties to the entities such that we can determine what causal interactions they will undergo, and hence what empirical outcomes are likely to result. Because of this need to load down the entity with at least detection properties, the credit we apply to hypotheses in light of their confirmation cannot be entirely given to the entity itself, we have to give some of it to the auxiliary assumptions too. In this way the entity realist is committed to substantial theoretical content underlying his realism, which reflects a commitment to a substantial epistemic claim about the properties of the objects posited.

If a theory's being empirically successful lends any credit to the idea that parts of our theory have correctly grasped parts of the world, those elements are not merely uninterpreted entities residing within the theory, but must include some significant theoretical assertions about those entities. A scientific realist ought instead to claim that the best explanation for observable behavior is that some theoretical elements that go beyond mere existence claims, (such as theoretical causal mechanisms, substantial properties, and laws that capture regular causal interactions), are all approximately correct.

The two arguments above against entity realism have their merits, and have indeed been used against entity realists over the years. However, it is important to appreciate that entity realism comes in a variety of packages, some more detailed and intricate than others. There are versions which may be able to avoid the above difficulties, depending on their epistemic and metaphysical commitments and which inference rules they endorse. We unfortunately don't have room to consider these variations on the view here, and will have to rest content with the conclusion that as it stands the position needs more research to establish its longevity.

Stuctural Realism

A similar attempt to find some middle ground that retreats from full-blown classical scientific realism, yet doesn't at the same time fall into instrumentalism or empiricism, is that of structural realism. This comes in at least two forms; epistemic and ontic. The former, like entity realism, adopts an alternative epistemic thesis. Structural realists of this kind claim that science is able to provide us with the correct mathematical representation of the structure of the world, but not the correct set of entities, or true theories in general. This structure is frequently cast in terms of a mathematical isomorphism between a theory's equations and the structural relationships between entities or processes in the world. It is legitimate for us to accept the existence of these relations, but we must be agnostic concerning the nature of the theoretical objects these relations connect. For example, we can believe in the mathematical description of an electron interacting with a magnetic field, but we should not believe in the electron itself. We might call this the 'epistemic structural realist thesis'.

The ontic form of structural realism goes further, claiming that not only are we exclusively restricted to accessing the structure of the world, but also that structure is all that really exists in the world. This view modifies both the epistemic and the metaphysical principles of realism. The epistemic is adjusted in the same way as it is for the epistemic structural realist, but the metaphysical thesis is transformed into the claim that there are no objects in reality, only relations. This doesn't effect the mind-independence of the metaphysical thesis, but it does mean that further refinements of that thesis need to be made. Where the traditional realist assumes that metaphysical realism covers objects, their properties, their relations, and properties of relations and properties, the ontic structural realist asserts that it covers only relations, because that's all there is.

This view typically draws on indistinguishabilty arguments from quantum mechanics to support its case, where it is claimed to be impossible to establish identity conditions for particular objects. Like entity realism and its epistemic alternative, ontic structural realism is designed to avoid and explain the historical cases where science went wrong by advocating a partial realism. This approach, therefore, takes seriously the above challenge to realism, and recommends agnosticism regarding all entities and their causal relations that cannot be directly observed.

For the structural realist, past errors in science are primarily traceable to such entity commitments, and to the degree that they concede structural errors in the history of science they also have to provide some plausible story for why their recommendations for current scientific belief are not undermined. They do, therefore, share with both the traditional and the entity realist, a commitment to the following theses: axiological realism, metaphysical realism, methodological realism, and semantic realism.

Essential Realism

Some realists explicitly take only the epistemic, metaphysical and semantic theses as constitutive of scientific realism; although, I think they tacitly assume the other two traditional theses as well (axiologial and methodological realism). The difference between this approach and traditional scientific realism again revolves around epistemic realism. Essential realists, like Philip Kitcher and Stathis Psillos, have tried to overcome the PMI by cutting down the breadth of their epistemic claims, so that they typically have a preferred theory of reference, or some such mechanism, which allows them to cope with failed past scientific theories that were once thought successful. In these cases, it is common to use this additional theory of reference to reduce the number of past successful scientific theories by raising the standards of acceptance. We can call their refinement of the epistemic thesis the 'essential epistemic realist' thesis.

Proponents of this view still typically accept IBE, which, as we saw, is rejected by some entity realists. It is in this regard, then, that I take them to be classical or traditional realists; they accept explanation as a legitimate form of belief-inducing reasoning. The essential realist approaches the challenge from a semantic direction, arguing that much of what was incorrect in past science was due to the failure of theoretical terms to correctly refer. In their view, proponents of past false theories did not actually refer to the superfluous entities, processes, or theoretical posits of their theories—that is, to those elements that were *not necessary* to that theory's being capable of generating correct predictions. The theory *did* succeed, however, just to the extent that it correctly referred to those elements that were *essential* in the derivation of its predictions. Here I will take a brief detour to describe the most fully developed and plausible version of essential realism—that advocated by Stathis Psillos.[19] The problems that arise for Psillos' view greatly motivate the analysis of structural realism in this dissertation, since the latter seems to be the most plausible alternative to essential realism.

First of all, Psillos adopts the strategy mentioned above of narrowing down the empirical basis for both the NMA and PMI to those scientific theories which can be considered 'mature' where this is cashed-out in terms of having passed the 'take-off point'. Recall that this point refers to when a theory coheres nicely with the principles of those in other disciplines, and also when the theory itself has a well-established set of basic principles which demarcate its domain of applicability and methods of solving problems in that domain. He also takes the notion of 'success' to refer only to *novel* predictive success—the successful prediction of previously unexpected phenomena. Actually, the notion of novelty is much more precise than this. It would be somewhat arbitrary if *novelty* was merely a matter of *when* a piece of evidence arose for one's theory. Why should it really make any confirmatory difference, for example, that the scientific community *accepted* that starlight is deflected by gravity in 1919, rather than if they had come to know this fact back in 1910, long before the development of General Relativity? Why, that is, should Einstein's theory derive confirmatory support

[19] For details see Psillos (1999).

from this observation any less if it were made ten years earlier than it in fact was? Psillos recognizes the contingency of historical discoveries, and separates this *temporal* novelty from the more relevant *epistemic* novelty, which he refers to as 'use novelty'. This takes novelty not to be temporal, but importantly focuses on the *construction* of the respective theory: a theory makes use-novel predictions if the predictive result is not used in the construction of the theory.

Aside from these two notions, which are used to narrow down the number of counterexamples to the NMA on the PMI list, Psillos also adopts both of two common realist strategies for answering the PMI directly. First, he adopts a causal theory of reference that aims at showing how abandoned terms like 'caloric' or 'phlogiston' really did have referential content after all. Second, he argues that it is only the theoretical terms (from past refuted theories) which were *essentially* involved in the derivation of novel predictive successes that are those which we should consider as truly referring. In other words, we selectively pick from Laudan's list of counterexamples those parts of successful theories which genuinely contributed to their theory's success. Now the question is, of course, how do we do that?

Although Psillos adopts a refined theory of reference, we still don't have a sufficiently well developed theory to avoid the following problem: causal theories of reference border on being trivial because they define reference for a term to be whatever causes the phenomenon which is itself responsible for the introduction of that term. For example, we once thought that 'ether' referred to the medium through which light propagates, but now since a causal theory of reference picks-out transverse oscillations as responsible for reflection/refraction phenomena, we can happily argue that we were really referring to the electromagnetic field all along.

Additionally, when it comes to distinguishing the *essential* from the *non-essential* components of a theory, as we'll see in later chapters, these categories are extremely hard to establish for any given theory, as well as being historically contingent. First of all, in order to make this distinction we have to look at the derivation of some novel phenomenon. Then we have to evaluate which components were essential to the scientist in his derivation. But notice that this is an interpretive issue. Whether we take the same presuppositions as the scientist did to be necessary for his work is an open question for many cases. It may appear to some that Maxwell could never have derived his equations for the electric and magnetic fields if he had not developed his concept of the displacement current, and that without his mechanical ether models this may not have occurred at all. Similarly, although some scientists of the time may have developed such a notion without models, perhaps it was essential for Maxwell, because of his psychology alone, that these models were required. Therefore, even if we could decide that some one individual required a component for his derivation, why should this contingent psychological fact influence whether we should believe in that component? Appeal to the specific causal role of that component alone will not help either, since it is unlikely that when scientists used to refer to 'ether' they really thought they were referring to an entity which shared causal roles with all future theoretical entities possibly responsible for light behavior (perhaps superstrings or some component in a theory of quantum gravity).

These three views, (entity, structural, and essential realism) are in direct conflict with one another. Entity realism and structural realism adopt opposing epistemic claims. The entity realist counsels us to limit our beliefs to statements regarding theoretical entities, the structural realist does the same, but for theoretical structure. On the surface these two pictures of science could not be more diametrically opposed, since once the laws and entities are stripped from a theory there is very little left. One view appears to be the contrary of the other.

Similarly, there appears to be a polar opposition between the essentialist approach and that of both the entity realist and the structural realist. Unlike the latter two, Psillos thinks it is impossible for us to clearly distinguish between the laws of a theory that describe theoretical relations, and the nature of the entities which are being related. For him the distinction between an object like an electron, and our descriptions of the structural relations in which it stands to other objects in the world, is far from clear—one informs the other. On the other hand, both the entity realist and the structural realist rely upon just such a distinction. Without some principled means by which to separate the electron from the relational properties it possesses, neither of these views can justify their privileging a specific part of a theory.

What these three approaches have in common is that they are trying to pick-out that kind of thing in our successful theories which is responsible for their success. This is what I referred to earlier as finding the thread, or ribbon. In order to answer the PMI, an account must, as we have seen, pick-out the kind of thing we see retained across the history of science, and it must also be the sort of thing that provides explanations—hence permitting the use of the NMA to infer to realism. Whether one takes this kind of thing to be entities, structures or the essential components required for predictions, the task is daunting. In the chapters that follow we'll see just how problematic proposed answers to the PMI which follow this path can be. A really strong answer, one that would put to rest the realist argument, would find a nice 'thick' ribbon that weaves its way through the history of science in different theories and different disciplines. But most responses that claim this prize fall short. On the other hand, it will not do to produce too 'thin' a response, because the slimmer the ribbon, the less capable we find it of revealing components of theories that can do the required explanatory work. For example, if one were to point to a particularly vague notion, then although it is likely to be satisfied by many relevant, retained parts of science, it is also very unlikely to do much explanatory work. If one points instead to a rather robust and explanatory component, it may work nicely for some theories, but not well for others—or it might be that it works well for a single theory, but not its ancestors. That is, we are going to see that finding a long as well as thick ribbon to answer that PMI is going to be very, very difficult.

Of course not all philosophers of science fall into one of the above camps. For our purposes there are at least two interesting cases that in response to issues like the PMI reject the realist position altogether.

Constructive Empiricism

Van Fraassen's constructive empiricism is the most sophisticated 'instrumentalist/empiricist' alternative to realism. It is interesting because, amongst other things, like those strategies so far reviewed it also attempts to draw an epistemic line and suffers from the PMI. His position is that science only needs to provide empirically adequate theories, not theories that are true beyond the everyday observable level. He therefore adopts an epistemic realism about observation sentences in science, but agnosticism about all theoretical statements. He thinks that theoretical sentences do have truth-values, but we are unable to determine what these values are. In fact, his realism is not merely about what we have so far observed, nor about what we will observe in the future, he thinks we are justified in all claims that are *in principle* observationally confirmable. Advocates of this view think it is better to draw the epistemic line at the observable/unobservable position (or something like it), because we are less likely to make mistakes in our knowledge claims this way.[20] On this account, then, one privileges the observable as epistemically justified. Theories that go beyond what is observable are to be treated as useful fictions; we remain agnostic as to whether their unobservable claims are true or false. Here the special status of what is observable derives from a concern over the nature of scientific inference to the unobservable. We need not postulate our theories as true in order to explain their success, we can settle for their empirical adequacy.

It should be clear however, that drawing the line at the observable and claiming anything unobservable is unknowable does not evade the PMI. The reason for this is simply that past theories that have been superseded were left behind precisely because they were empirically inadequate.[21] That is, there are plenty of those that were at one time empirically adequate and yet turned out to make some

[20] For a convincing argument how the observable/unobservable distinction can be made respectable see Vollmer's (2000).
[21] Blackburn (2002)

predictions which turned out to be false. For example, Bode's law is a law that related the location of a planet from the sun relative to other planets to the particular orbit that it followed—the distance from the sun in particular. The law can even be read to have made the prediction of an asteroid belt between Mars and Jupiter. Still, the law was abandoned shortly after its predictions for the position of the newly discovered Neptune turned out to be false. The law was empirically adequate for a while, but lost its ability to account for the radius of the orbits of the planets as new members of the solar system were discovered. Therefore, although the successful observable phenomena are of course retained through the history of science on this account, there is little benefit to being a constructive empiricist—just by being more conservative about what one is willing to license as epistemically justified you do not gain security against future falsifying evidence. The constructive empiricist's claim that the aim of science is empirical adequacy rather than generating true theories does not help him when it comes to Laudan's critique.

Quietism

Alternatively one might opt for pessimism about the whole debate. Such 'quietism' comes in a variety of flavors, all of which reject the project of drawing any epistemic line at all. Simon Blackburn argues that once we clarify the nature of the realism/anti-realism issue, the differences between those who accept a given theory as empirically adequate, and those who believe it to be true, evaporate. Any claim to modesty that constructive empiricism advocates is therefore little more than rhetorical subterfuge. From this vantage point it is not surprising that Blackburn sees the merits of the pragmatist position, which suggests that theoretical belief just is theoretical immersion combined with explaining the utility and inevitability of a theory in terms of predictive organization, control, systematization and other virtues.[22] Under such an interpretation, Blackburn appeals to William James:

> James is aligned with Kuhn and Ellis with whom we began: he sees realism as idle, as nothing more than the renunciation of all articulate theory. So far as this recent episode in the philosophy of these things goes, it appears that he may have been right.[23]

Others refuse to draw lines for different reasons (e.g. Fine's Natural Ontological Attitude (NOA), Laudan's normative naturalism, Maddy's pragmatism). It is Blackburn's view that what distinguishing him from Fine's NOA is that the latter approach *does* recognize a line to exist. Indeed Blackburn thinks it is because of NOA's rejection of IBE that the position doesn't collapse into realism itself—if it didn't recognize a difference between acceptance and belief, then NOA would advocate realism because it says that we are justified when inferring to empirical adequacy. If empirical adequacy legitimizes acceptance, and if acceptance is equivalent to belief in truth, then empirical adequacy legitimizes realism. This is why Fine has to reject Blackburn's deconstruction.

All of these are interesting positions, but since this is a dissertation about realism as a response to the PMI, I shall ignore entirely the last two alternatives. I have

[22] *ibid.* p.132
[23] *ibid.* p.133

also chosen to focus on Structural Realism primarily because it seems on the surface to provide possibly the most convincing response.

The structural realist position should seem at least somewhat plausible, since we can easily see mathematical continuities across the history of science: take for example the early phenomenological theory of thermodynamics. Here we see that the Ideal Gas Law, which relates pressure, volume, and temperature of a gas, has a structure retained even in modern quantum statistical mechanics. Other examples might include the similarity of mathematical structure between classical mechanics and modern quantum mechanics, as well as the similarity between classical dynamics and special relativity. There are also many, many more examples where appealing to the structure of our past successful scientific theories seems to save realism against the pessimistic meta-induction.

The motivation for selecting structural realism then comes from its at least initial success at not only answering the PMI but doing so in a manner that also respects the force behind the realist's NMA. By pointing to the structural components of theories and showing that they are retained across revolutions the structural realist can answer one half of the PMI. To answer the other half of the problem, he then just has to show how that structural content can legitimately be used to explain the novel predictive successes of the respective theories. Having this answer to hand also accommodates the realist's intuitive sympathy for the fact that such remarkable successes as we see science provide cannot be mere coincidence.

In what follows we'll look at a number of formulations of Structural Realism, and consider how one might formulate a version which is able to overcome some rather drastic problems faced by the others.

As an example, consider the problem of establishing exactly what it is structural realists are committed to when they appeal to 'structure'. On some accounts structure is to be equated with the mathematical equations of a theory, on others it is the abstract structural relations between the entities of the theory under discussion. We will see in a later section how the former looks overly simplistic and accounts for very little successful science, while the latter leaves the structural realist with a commitment to little more than the cardinality of the theory's domain.

One might also be concerned that because of structural realism's reliance on scientific laws, it mischaracterizes most of science. Despite a few select paradigm cases drawn from physics it is hard to maintain the structural realist's appeal to mathematical equations alone as worthy of our belief. Even in cases where they appear to be less controversial, such as in classical physics, it is still hotly debated whether these equations are accurately capturing laws of nature, or indeed whether it is even appropriate to talk of laws as traditionally conceived. Even if these issues were not problematic, most of science does not operate with such clear cut mathematical representations of its laws. This leaves the structural realist having to modify his account to cover all the special sciences it currently ignores—no small task.

Chapter 2

A Short History of Structural Realism and the Optical Ether

Previously I defined scientific realism as a view that claims science aims at providing approximately true theories that represent a mind-independent reality, and that science often succeeds in this aim. Now we are going to start our investigation into a particular form of the position that tries to accommodate both the No Miracles Argument and the Pessimistic Meta-Induction. The view is Structural Realism, and to understand its current manifestation, we will first describe a classic case study from the history of science, the optical ether theory of Fresnel and its overthrow by Maxwell's theory. We will then use this study to better unpack the history of structural realism—how it developed and the major problems it has faced. The recent resurgence in both the position and the use of this case study comes from John Worrall's work, which we'll see more of in the next chapter, but he takes his cue from the work of Henri Poincaré, Bertrand Russell, and Pierre Duhem. These are the characters we'll meet in this brief historical survey.

2.1 Fresnel's Optical Ether and Maxwell's Electro-Magnetic Field

In this section I merely wish to sketch the relevant parts of two theories; Fresnel's theory of light refraction and reflection, and Maxwell's electromagnetic interpretation of the same phenomena.[1] These descriptions are not in any way attempts to explain these theories in their entirety, nor to resolve any questions currently debated by historians. They are supposed merely to provide enough detail to facilitate an appreciation of the structural realist attempt to use this case in support of his position.

By the turn of the nineteenth century, corpuscular optics, the view that light rays consist of streams of particles, was dominant both in England and Europe. Generally these ‘molecules’ were taken to be atomic in nature, have mass, occupy zero volume, and exert forces on one another at a distance; they were thus treated as point masses undergoing centrally directed forces between one another. About a decade later, Laplace and Malus provided corpuscular accounts of both atmospheric refraction and double refraction; achievements surpassing competing wave theories of light. However, no mathematically rigorous account of these phenomena was forthcoming, and explanations were limited to experimentally established laws.

There was a competing view of optics gaining ground in England at the time, pushed by Thomas Young, who thought that light was more like sound, consisting of waves propagating through a medium of some kind. Like its competitor, this theory lacked a complete mathematical representation, and consequently it couldn’t provide compelling reasons for those on the continent to consider replacing the corpuscular approach and its use of ray ‘orientation’ to account for phenomena like polarization. Besides, the corpuscular view also provided at least a superficial physical story of how

[1] Much of the relevant history for this case study comes from the following excellent works: Buchwald (1985, 1989), Cantor and Hodge (1981), Darrigol (2003), Hendry (1986), Laudan (1981), Seigel (1991), Simpson (1997), Shaffner (1972), Whittaker (1951).

rays operate, appealing to material points applying forces over a distance. The wave theory on the other hand had no account of the medium through which light waves propagated.

Augustin Fresnel almost single-handedly changed this sad situation for wave optics. He explained various experimentally observed diffraction patterns via wave interference, where the corpuscular theory could provide no story. By the end of 1818 he had provided a general calculation of interference patterns, and it was for this that he famously won the Paris Académie des Sciences prize. However, his work initially gained little support because he still failed to account for polarization, and the medium through which waves propagated was still very much superficial; he had not in fact derived the force dynamics for ether particles from mechanical principles, but had rather simply assumed them. Consequently, without much of a physical interpretation for his mathematical theory, nor a story for polarized light, few felt the urge to join the ranks of wave theorists.

Polarization was a significant problem for Fresnel because he was aware that light rays orthogonally polarized do not mutually interfere, whereas those polarized parallel to one another do. This could not be explained on the view that light consisted of waves that were longitudinal in nature, primarily because such interference requires rays to have some kind of asymmetry along their axes, and these two properties are incompatible. The only way for Fresnel to account for such interference patterns would be to show that unpolarized rays have both longitudinal as well as transverse vibrational components and those that are polarized lose the longitudinal element. He had no mechanical means of showing this however.

By 1821 Fresnel had made significant progress on this problem, and by adopting the view that the light medium consisted of Laplacean point masses, he argued that transverse molecular displacements of the vibrating waves were much slower than their longitudinal counterparts. The latter oscillations, under these conditions, actually vanished. In this way the two components are entirely separate in both polarized and unpolarized rays, and light becomes a purely transverse vibration. Furthermore, molecular displacements of the medium generate restoring forces, and as a consequence the velocity of propagation of a transverse wave now depended upon the intensity of the reactive force. Now, since the intensity of this restoring force depended on the direction of the wave's propagation, so too did its velocity. This was essential in explaining the phenomenon of polarization in double refraction because the rays were vibrating along paths of different orientation.

As interesting as this last episode of the story is, we are more interested with Fresnel's decomposition of reflected and refracted light and his derivation of the equations for the intensity of transverse vibration components. That is, Fresnel, in 1819 derived the equations which represent the amplitudes of oscillations that are normal to a wave front. This is before he determined optical waves to be entirely transverse. It is these equations that will play an important role for structural realists.

Fresnel's derivations of the reflection and refraction formulas begins with two rays which are in the incident wave front, the front interacting with a flat surface (like that on the side of a glass prism). These rays partially reflect from and refract through the material, just as when a light beam is split, some reflecting at an angle and some passing through the glass. The rays make contact with the surface at an angle (the angle of incidence), reflect at an equal angle (angle of reflection) and some pass through the glass at an altogether different angle (angle of refraction), which is determined partly by the index of refraction for the material. (See the figure below).

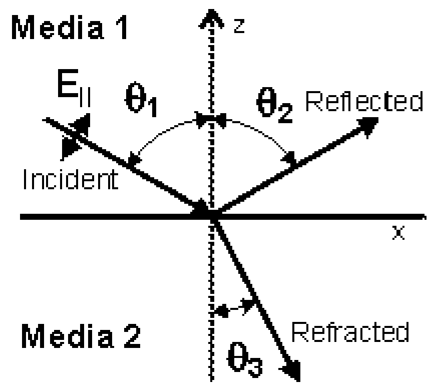

Figure 1: Reflection and Refraction of Rays

Fresnel wanted to use the principle of conservation of momentum to determine the reflection and refraction formulas, and check his derivation with the results generated by using the principle of the conservation of energy. He had to work through this procedure a couple of times before getting it quite right, but along the way he determined that light must have a transverse component, rather than oscillating in a longitudinal manner.

In the first derivation (1819) Fresnel had to assign momentum, and hence mass (m), to the wave front and to its refracted ray (μ). Through geometric analysis, which took into account the angle of incidence (i) and refraction (e), he determined that the reflected (v) and refracted (u) amplitude of the oscillations were related to the amplitude of the incident (V) in the following way:

$v = \mathrm{V}\sin(i - e)\ /\sin(i + e)$ $\qquad$ $u = \mathrm{V}\cos(i)/r\cos(e) * \sin(2i)/\sin(i + e)$

These equations represent the amplitudes of oscillation of the light that are orthogonal to the direction of propagation of the wave front. They were then checked against a derivation using the conservation of energy, rather than momentum. However, unless the index of refraction is equal to unity, the equations don't hold (unless the angle of incidence is zero). So, Fresnel concluded that perhaps the oscillations are not entirely longitudinal. In doing so, he had to include energy conservation in his derivation because momentum derivations alone only work for longitudinal deductions.

In 1821 Fresnel had a new approach which decomposed the wave front rays into two components; the incident and that normal to the incident. The parallel component provided a challenge since it now would undergo no change of orientation, despite the change of direction of propagation for the refracted and reflected rays. The interaction at the interface was treated like two colliding bodies in an elastic collision, where the 'masses' had velocity ratios, representing oscillation amplitudes, related in the following way:

$v\ /\ \mathrm{V} = m - \mu\ /\ m + \mu$ $\qquad$ $u\ /\ \mathrm{V} = 2m\ /\ m + \mu$

Assuming that the density of the refracting medium is proportional to the square of its index of refraction, these relations (because the mass ratio remains $n[\cos(e)/\cos(i)]$) provide the following equation for the *oscillations normal to the plane of incidence* [for the square of the amplitude ratio (intensity ratio)]:

Reflected / incident $= [\sin(i - e)\ /\sin(i + e)\ ]^2 = [[\tan(i) - \tan(e)]\ /\ [\tan(i) + \tan(e)\ ]]^2$

This is known as ‘Fresnel’s Sine Law”. It was only a couple of days later that he derived his ‘Tangent Law’ for oscillations in the plane of incidence:

Reflected / incident = $[[\sin(2i) - \sin(2e)]/ \sin(2i) + \sin(2e)\,]^2 = [\tan(i - e)/ \tan(i + e)\,]^2$

By 1823 Fresnel had developed a derivation that used no elastic collisions whatsoever, and instead combined energy conservation with a continuity constraint on the medium at the interface; a boundary condition. The boundary condition was that the *components of oscillation that are parallel to the interface must be continuous with it*. Using a derived energy equation, $[\sin(e)\sin(i)(1- v^2) = \sin(i)\cos(e)u^2\,]$, Fresnel derived both his Sine and Tangent Laws in a new form:

$v = -\,[\,\sin(i - e)\;\;/ \sin(i + e)]$

$v = -\,[\sin(i)\cos(i) - \sin(e)\cos(e) / \sin(i)\cos(i) + \sin(e)\cos(e)]$

It is precisely these equations that Lorentz later showed (1875) could be derived from Maxwell’s electromagnetic principles. We won’t look at the details of this derivation, but in what follows can see clearly how Maxwell’s theory provides a very different (non-geometric) route to the same equations.

By the middle of the nineteenth century there was a flourishing industry within the physics community where theoreticians were busy building models of the optical ether. Fresnel’s equations were clearly empirically adequate to the task of calculating light refraction and reflection at the boundary of a transmitting surface, but the medium by which this was accomplished was still uncertain. James McCullugh, George Green, and George Stokes, all had incomplete models of the ether, none yet satisfactory for explaining how light propagated. Surprisingly it was not from any attempts to model the optical ether that light's apparently true nature would be revealed.

Let's turn now to a completely different area of physics research, that of electricity and magnetism. By the end of the eighteenth century many of the basic properties of electrostatics and magnetostatics were experimentally well established. Charles-Augustin Coulomb had performed extensive experiments on these phenomena in the 1770's and 1780's, determining the inverse square laws of electrostatics and magnetostatics. By 1826 Siméon-Denis Poisson had formulated in vector notation the equations for the electrostatic and magnetostatic potentials (V_e and V_m), from which one can derive the electric field strength ($\boldsymbol{E} = -\text{grad } V_e$) and the magnetic flux density ($\boldsymbol{B} = -\mu\text{grad } V_m$).[2] The work of Luigi Galvani and Alessandro Volta in the closing decade of the eighteenth century revealed the phenomenon of current electricity, which with the development of the voltaic pile and then Volta's 'crown of cups', provided a controllable source of electric current (precursors to today's batteries). By 1820 Hans-Christian Ørsted demonstrated that with every electric current there was an associated magnetic field, and shortly thereafter Jean-Baptiste Biôt and Félix Savart discovered the law describing the dependence of the strength of a magnetic field at some position on an element of that electric current. André-Marie Ampère in 1826 took the Biôt-Savart law, and developed it into a relation between the current flowing through a closed loop and the integral of the component of magnetic flux density around that loop. This relation became known as 'Ampère's circuital law', and he was also able to show how it related to the relation describing the force between two

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[2] μ is the permeability of the medium.

current-carrying elements. By 1827 Georg Simon Ohm had developed his law ($V = RI$) which relates the potential difference and current via resistance of the material through which current flows. So, by 1830 much of the story of static electricity was very well developed.

In the meantime, Michael Faraday had been performing experiments investigating the relationships between currents and magnets since 1820. From observing the circular patterns made by iron filings around magnets, he developed the concept of a 'magnetic line of force'—a field line that represents the direction in which a force acts on a magnetic pole when placed in a magnetic field. The more lines of force in any given vicinity near a magnet, the stronger is the force. In fact Faraday was able to show how all the lines of force of a magnet can be simulated by a current-carrying wire that was bent into a loop, and this breakthrough revealed that all the laws mentioned above for the forces between electrostatics and magnetostatics could be derived from the equivalence of magnetic dipoles and current loops.

Faraday's discovery of electromagnetic induction in 1831 revealed that we can induce in a current loop an electromotive force, and this current is in the opposite direction from, and directly related to the rate at which the magnetic field lines are cut. He further developed his experimental investigations, and in 1845 discovered that the polarization of light is influenced by the presence of a strong magnetic field— the plane of linearly polarized light is rotated when passed through a transparent dielectric in the direction of magnetic field lines. This phenomenon came to be known as 'Faraday Rotation'.

Although unable to mathematically formulate these relationships, Faraday's ideas were essential to the development of a rigorous analysis of electric and magnetic effects by his follower, James Clerk Maxwell. By 1856 Maxwell had spent a great deal of time studying the works of both Faraday, and another leading electro-magnetic physicist, William Thomson (Lord Kelvin). He had found in Faraday's work the use of analogy particularly helpful in trying to conceptualize the nature of both electric and magnetic effects. Faraday's notion of 'lines of force' provided a useful analogy by which Maxwell hoped to mathematicize much of Faraday's achievement, and the young Cambridge graduate produced the first in a series of groundbreaking papers entitled 'On Faraday's Lines of Force'. His approach was broadly to develop as far as he could the ideas of electric and magnetic phenomena in terms of Faraday's lines of force based upon mathematical analogies that he saw in other physical systems. In particular, Maxwell took mathematical accounts of phenomena from mechanics and hydrodynamics and extended them to the field of electrodynamics. It was Thomson's work on the mathematical description of various electrical and magnetic phenomena that permitted Maxwell to proceed with particular speed.

In the paper Maxwell appealed to the notion of 'incompressible fluid flow' to model magnetic lines of force, just as Faraday had before him. He showed that much of the mathematics of hydrodynamics might well apply to electromagnetic phenomena. For example, if we think of an incompressible fluid in a volume v, that is bounded by a closed surface S, the mass flow per unit of time through a surface element d**S** is $\rho \boldsymbol{u}$.[3] Here $\boldsymbol{u}$ is the fluid velocity and ρ is the density distribution of that fluid. The total mass flux through the closed surface is

$$\int_s \rho \boldsymbol{u} \cdot d\mathbf{S}$$

This is equal to the rate of loss of mass from v:

$$-d/dt \int_v \rho dv$$

From here we can apply the divergence theorem to the last equation:

$$\int_s \rho \boldsymbol{u} \cdot d\mathbf{S} = \int_v \mathrm{div}(\rho \boldsymbol{u}) dv = -\int_v \partial\rho/\partial t dv$$

Since the fluid is incompressible, ρ does not depend upon time or space coordinates, and hence div $\boldsymbol{u} = 0$.

Maxwell drew an analogy between this fluid flow derivation and magnetic field lines, and took $\boldsymbol{u}$ to be analogous to the magnetic flux density $\boldsymbol{B}$. Hence, just as when lines of force diverge, the field's strength and the fluid's velocity decreases. In this case it would imply that the magnetic field has zero divergence: div $\boldsymbol{B} = 0$.

Maxwell also highlighted the difference between $\boldsymbol{B}$ and $\boldsymbol{H}$, where the former is the magnetic flux, which he called 'magnetic induction', and the latter is the 'magnetic force', or intensity, on a unit magnetic pole. As a consequence he was able to capture all the then known electro-magnetic phenomena in terms of several laws:

1. Faraday's law of electromagnetic induction: curl $\boldsymbol{E} = -\partial \boldsymbol{B}/\partial t$. This he derived from Neumann's 1845 form, (which took the electromotive force induced in a closed circuit to be proportional to the rate of change of the magnetic flux).
2. Ampère's law was rewritten for an elementary surface area: curl $\boldsymbol{H} = \boldsymbol{J}$. Here he started with the original law, took the path integral for an enclosed surface

[3] Here I use vector notation, even though it was not introduced by Maxwell until 1870.

and applied Stokes' theorem. $\boldsymbol{J}$ is the current density and $\boldsymbol{H}$ is the magnetic force.

3. div $\boldsymbol{B} = 0$ (which was derived as shown above).
4. div $\boldsymbol{E} = 0$ (which was derived in the same way as (3) for free space in the absence of electric charges).
5. $\boldsymbol{B} = \text{curl}\, \boldsymbol{A}$ where $\boldsymbol{A}$ is the 'vector potential'
6. $\boldsymbol{E} = -\partial \boldsymbol{A}/\partial t$.

With this formal framework in place Maxwell had developed the beginnings of a rigorous analysis of electromagnetic phenomena. Still, much more work remained, and in 1861-2 he wrote a series of four papers under the title 'On Physical Lines of Force' in which he developed a physical model for his theory.

In his new papers Maxwell wanted to extend his analogy between fluid flow and magnetic field lines to a deeper model in which the rotational nature of magnetism was captured in a mechanical fashion. He accomplished this by appealing to the then common notion of an all pervading ethereal substance, but in his picture there were vortices of rotating tubular magnetic flux, analogous to the vortices produced by rotating tubes of fluid. The medium was similar to the optical ether, through which light was thought to propagate, although at first this electromagnetic medium was made of rigid vortices, which all revolved in the same direction. The idea depended upon the consideration that when left alone, magnetic field lines expand apart from one another, just as would a rotating vortex of fluid due to centrifugal forces. That is, for a rotating tube of fluid, its kinetic energy can be written:

$$\int_{\mathrm{v}} \rho \boldsymbol{u}^2 \cdot \mathrm{d}v$$

where ρ is the fluid density, and $\boldsymbol{u}$ is the rotational velocity. Where $\boldsymbol{u}$ is analogous to $\boldsymbol{B}$ Maxwell thought we could take this equation to be analogous to the energy of a magnetic field distribution. As such, we might be able to capture the image of a magnetic field with the picture of rotating vortices.

The mechanical image was inadequate as it stands, because collections of rigid vortices, all in contact with one another, are incapable of rotating in the same direction unless they have no friction with one another. This however would run contrary to the stresses between magnetic lines of force discovered by Faraday. Maxwell therefore had to suppose that between these rotating magnetic vortices rode layers of 'idle wheels' which acted mechanically like ball bearings—these bearings would sit between vortices rotating in the opposite direction and would prevent fervent disruption of the entire system due to friction. The ether with vortices and idle wheel particles is pictured below.

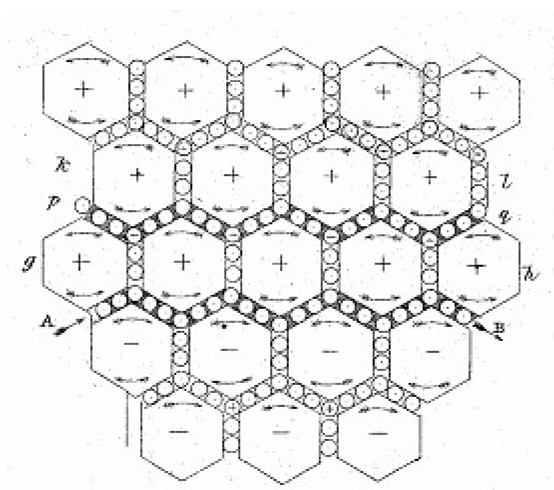

Figure 2: Maxwell's Rotating Vortices

Here we can see the vortices represented as hexagons, and the idle wheels sitting between these vortices. It turns out that Maxwell needed to interpret these wheels as constituting the electric current (A to B) when they undergo sheer stress and slide between the vortices in the direction of magnetic angular velocity. In free space these wheels were not permitted to move linearly, although they could still rotate. Thus, by identifying the wheels as electric particles he could account for the currents produced in conductors, as well as their absence in insulators. In fact, this rather simple mechanical model was capable of accounting for the entirety of then known electromagnetic phenomena. It could account for the circular magnetic field generated around a wire that carries a current by showing how vortices of circular rotations accumulate at ever increasing radii around the wire with electric particles spinning between them. The model could picture how an electric current is generated and flows between two parts of a magnetic field which have different strengths. In the stronger field the vortices are rotating with greater angular velocity, and impart this additional force to the electric particles, which since they cannot pass it on to the other, weaker, portion of the field, release the additional energy as motion in the direction of the current. Besides these, the ether model was capable of accounting for induction as well as the propagation of electro-magnetic waves through a vacuum. The latter of these is essential to our story, so we'll take a minute or two here to sketch this remarkable achievement.

Maxwell knew that insulators, like the vacuum, can store electrical energy. He assumed that in his model when this storing of energy occurred (say by the application of a changing electric field) it was the displacement of electric particles from their usual positions of equilibrium. The model therefore had to be interpreted as one of an elastic ether in which displacements of particles generate restoring forces associated with their elastic potential energy. When the particles shift position in this way they generate small electric currents—a *displacement current*. The shifting of particles also causes wave disturbances in the ether, which propagate through the medium—even through a vacuum. The displacement of these particles is directly proportional to the strength of the electric field and so Maxwell argued that the displacement current density should be included in the calculation of magnetic field strength: curl $\boldsymbol{H} = \boldsymbol{J} + \boldsymbol{J}_d$. From here Maxwell could proceed to determine the wave equation for the propagation of the wave generated by the changing electric field in the medium. This wave has no solution for its wave vector $\boldsymbol{k}$, parallel to $\boldsymbol{H}$, but does have solutions for when $\boldsymbol{k} \cdot \boldsymbol{H} = 0$, that is, when the waves are transverse. In fact this represents vectors perpendicular to one another and to the direction of propagation. Maxwell found this velocity to be almost identical to Kohlrausch and Weber's experimental values for the speed of light.

This extraordinary discovery is of course very well known. However, since our concern is to illustrate how Maxwell's new theory retained Fresnel's Sine and Tangent laws, it is best here to give a brief derivation of how they follow from Maxwell's equations. Let's start with a beam of light incident on a boundary between two dielectric media.[4] This beam will be split in two, one beam reflected and one refracted (transmitted) into the second medium, depending on the angel of incidence and the permeability and permittivity of the media. Maxwell's equations tell us the angles of reflection and refraction, and the relative intensities of the two resulting beams. If we

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[4] The relevant physics is sketched here, but detailed accounts can be found in Feynman, Leighton, Sands (1964), Jackson (1999), and Westgard (1997).

assume a uniform medium then each of the fields can be represented by a plane wave function, for example:

$$\boldsymbol{E} = \boldsymbol{E}_0 \exp[\mathrm{i}(\omega t - \boldsymbol{k}\cdot\boldsymbol{r})]$$

where $\boldsymbol{E}_0$ represents the amplitude at point $\boldsymbol{r}$ at time t. Here, $\boldsymbol{k}$ is the wave vector, pointing in the direction of motion, and the wave has a phase velocity $v_{ph} = \omega k$. The magnitude of the vector is given by $k = \omega n/c$.

We take the beam to be approaching an interface of the media which is oriented in the x-z plane as in the figure below.

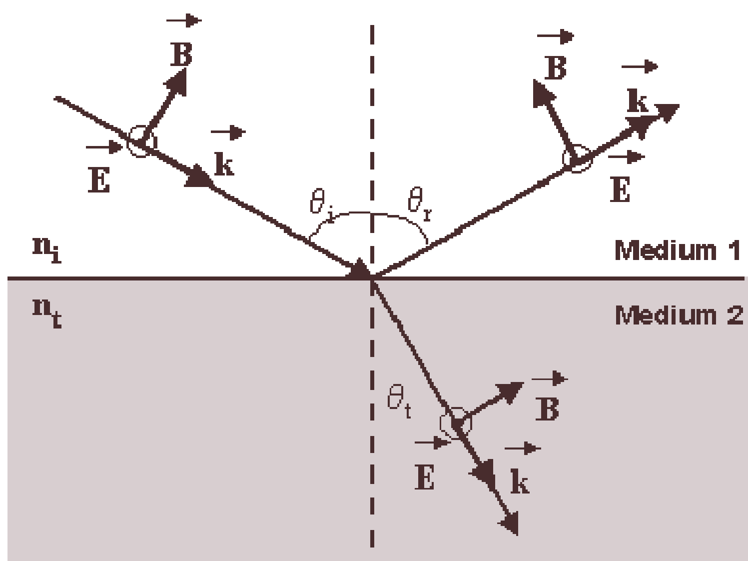

Figure 3: Maxwell's Analysis of Reflection and Refraction

Maxwell's rewritten version of Faraday's law says

$$\nabla \times \boldsymbol{E} = -\partial \boldsymbol{B}/\partial t$$

which for a wave becomes

$$-\mathrm{i}\boldsymbol{k} \times \boldsymbol{E} = -\mathrm{i}\omega \boldsymbol{B}$$

This then gives us

$$\boldsymbol{B} = (\boldsymbol{k} \times \boldsymbol{E})/\omega$$

which tells us that both $\boldsymbol{E}$ and $\boldsymbol{B}$ are at right angles to each other as well as the direction of motion. We can now write our electric vector for the *incident* wave in this form

$$\boldsymbol{E}_i = \boldsymbol{E}_0 \exp[i(\omega t - \boldsymbol{k} \cdot \boldsymbol{r})]$$

Similary, the *reflected* wave's electric vector can now be written as

$$\boldsymbol{E}_r = \boldsymbol{E}'_0 \exp[i(\omega' t - \boldsymbol{k}' \cdot \boldsymbol{r})]$$

and the *transmitted* wave:

$$\boldsymbol{E}_t = \boldsymbol{E}''_0 \exp[i(\omega'' t - \boldsymbol{k}'' \cdot \boldsymbol{r})]$$

We have to consider boundary conditions for these three waves, and Faraday's law requires that the electric field must be the same on both sides of the boundary, which entails that $\boldsymbol{E}_i + \boldsymbol{E}_r = \boldsymbol{E}_t$. But since this says that two oscillating waves are equal to a third, and this can only happen if all the oscillations have the same frequency, then we know that $\omega + \omega' = \omega''$, and since $k^2 = n^2\, \omega^2/c^2$, this entails that $k = k' = k''$, where n is the index of the medium. When we combine these equations with the wave expression at the boundary, which reduces to:

$$k_x \boldsymbol{E}_0 + k_x' \boldsymbol{E}_0' = k_x'' \boldsymbol{E}_0''$$

then we get the following equation for electric field amplitudes:

$$\boldsymbol{E}_0' = (k_x - k_x'')/\, (k_x + k_x'')\boldsymbol{E}_0$$

and since $k_x = k \cos \theta_i$ we can use Snell's law ($n_2 \sin\theta_t = n_1 \sin\theta_i$) to substitute in and derive:

$$\boldsymbol{E}_0'/\, \boldsymbol{E}_0 = \sin(\theta_i - \theta_t)/\, \sin(\theta_i + \theta_t)$$

Which is equivalent to Fresnel's Sine law found above. The same form of derivation can be applied to derive the Tangent law.

In summary, Fresnel derived the Sine and Tangent laws for the amplitudes of reflected and refracted light because he recognized that light was composed of transverse oscillations in what he thought was an elastic solid ether. He took the ether to be a mechanical medium, applied mechanical principles like the conservation of momentum to it, (as well as wave principles), to geometrically generate a solution that used boundary conditions for the system. Maxwell was tackling a different problem, but his theory generates the same solutions. By addressing the interface of dialectric media, and using continuity conditions, in combination with Maxwell's equations, one can derive the Sine and Tangent law without appealing to the notion of an ether.

The move from rotating vortices to wave disturbances of the electric and magnetic fields is of course a significant shift in ontology for theories of light propagation, and in fact by the time Maxwell had written his final form of the theory in his *Treatise on Electricity and Magnatism* (1873) his commitments to any underlying substance were non-existent. No longer did he believe in an all-pervading elastic-solid ether whose vortices were magnetic lines of force and whose displacement currents were electrical discharges. It is to exactly this sort of agnosticism that the structuralist appeals in his philosophical interpretation of physical theories, and so now let's turn to look at the relevant philosophical positions.

2.2 Duhem

Pierre Duhem (1861-1916) was an excellent theoretical physicist who committed himself to developing ideas in the philosophy of science through a good working knowledge of the history of science. It should not be surprising therefore that he perceived as clearly as anyone before him the roots of what we are calling the Pessimistic Meta-Induction. By working on the history of mechanics and astronomy he could clearly see the radical transitions evident across successful theoretical approaches to specific problems in various domains of science. Consistent with these episodes in the history of science he generated a philosophy of science that drew several important distinctions.

The first of these, the one that perhaps performs the most work in his account, is that between *representing* a set of relationships between observable properties, and of trying to *explain* those relationships by appeal to unobservable relationships between unobservable entities. Perhaps because of his commitment to a particularly formal approach to physics, where theories are first axiomatized and predictions derived from principles, he took the aim of science to be that of generating representations of phenomenal relationships only. That is, relationships between observable properties. About this he suggests physical theory should be seen as providing "an abstract system whose aim is to *summarize* and *logically classify* a set of experimental laws without pretending to explain these laws."[5] Such claims reveal Duhem's aversion to scientists moving beyond the phenomenal level of experience and trying to reveal to us the unknowable reality beneath observable experimental

[5] Duhem (1903, p.3)

outcomes. He says of explanation (as explication in his terms) "To explain, *explicare*, is to divest *reality* from the *appearances* which enfold it like veils, in order to see that reality face to face."[6] Such flights of fancy he relegates to the realm of metaphysics and religion, and sees no need for them in science. Although to many it may seem Duhem entertains scientific explanations as legitimate, this is a misunderstanding. He does concede that sometimes with regard to an explanation:

> The more complete it becomes, the more we apprehend that the logical order in which theory orders experimental laws is the reflection of an ontological order, the more we suspect that the relations it establishes among the data of observation correspond to real relations among things, and the more we feel that theory tends to be a natural classification.[7]

However, this appreciation for what we now might classify as a No Miracles type argument, is not to be confused with a move towards realism. Perceptive philosophers have pointed out that psychological terms like "apprehend", "suspect", and "we feel" are more likely meant by Duhem to be mere reflections of our compulsions towards realism, not strong arguments in favor of the position.[8] We should not see plausible explanations as providing good reasons to believe in the theoretical entities or theoretical laws they posit:

> What is lasting and fruitful is the logical work through which they have succeeded in classifying naturally a great number of laws by deducing them from a few principles; what is perishable and sterile is

[6] *ibid.* p.3-4
[7] *ibid* pp. 26-27
[8] See McMullin (1990), Psillos (1999)

> the labor undertaken to explain these principles in order to attach them to assumptions concerning the realities hiding underneath sensible appearances.[9]

Here it seems Duhem observes what might be classified as a Pessimistic Induction type argument. His loyalty is to the anti-realist camp, stemming in no small part, no doubt, from his familiarity with historical cases of theory transition. He carries his anti-realism into his definition of the aims of physical theory:

> A physical theory is not an explanation. It is a system of mathematical propositions, derived from a small number of principles, whose purpose is to represent a set of experimental laws as simply, as completely and as exactly as possible.[10]

From this definition we can see that Duhem takes physical theory to be mathematical in nature, and importantly economical—it must appeal to the simplest and most complete representation available. As such, derivations of concrete experimental outcomes stem from more abstract principles or laws, and this represents an appreciation for the scientist's ability to abstract away from a multitude of experimental data to determine a set of mathematical relations common to all the phenomena. Here we also see another of Duhem's important distinctions; *experimental* versus *theoretical* laws. The former are justifiable canonizations of observable relations, the latter are unjustified stipulations about relations between entities we can never know; those that are unobservable to us. Unsurprisingly he accepts the former, while rejecting the latter.

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[9] Duhem, (1903, p. 38)
[10] *ibid*. p 24

This last distinction is relevant to the modern debates over structural realism, and we will see later that it plays a role in distinguishing the constructive empiricist from the epistemic Ramsey-Sentence Realist. For now, however, it seems apparent that in considering our case study, Duhem is explicit about several issues. He would without doubt reject the model-making impulse found in the work of the British; he says as much throughout *The Aim and Structure of Physical Theory*, but nowhere more so than in his chapter titled 'Abstract Theories and Mechanical Models'. Still, even though he thinks reliance on model building a poor means of investigating the world, his rejection of methodology hides his deeper rejection for explanations. Since theories are merely representational tools, we ought not take such explanatory models seriously, and as such, those like McCullugh, Green, and Stokes who attempted to model the optical ether, as well as Maxwell in his initial attempts to model the electro-magnetic ether, are all generating images that although useful as heuristic devices, should by no means be taken to reflect unobservable reality. The ethers then, would be cast aside by Duhem.

But what of the mathematical formulations we've seen Fresnel and Maxwell develop and which they each used in their attempt to explain the phenomena of reflection and refraction? Well, here we can look to Duhem's distinction between experimental and theoretical laws to inform our evaluation. When it comes to laws the experimental are those that we can confirm with direct observation, and as such are those that systematize relations between observable predicates. For example, Fresnel's Sine and Tangent Laws would seem to fall into this category. They each contain only the geometric sine or tangent function applied to apparent angles of incidence, reflection and refraction. Perhaps one has hesitations that these notions can provide us with the amplitude of the respective waves, but once we accept that this is stipulated as equivalent to the intensity of the wave, then we can accept the latter concept as primary, because observable, while treating the latter as instrumental. So, on Duhem's account, Fresnel's laws are experimental rather than theoretical. Maxwell's equations are another story entirely. On his picture when we talk about the intensity of incident, reflected, and refracted light rays, what we are really referring to are electric and magnetic field amplitudes, and these certainly cannot be observed directly. The question as to whether the relations that in Maxwell's theory are equivalent to Fresnel's Sine and Tangent Laws should be taken as observable suddenly becomes tricky. We can obviously see that there are light rays which stand in these relations, but have no idea, according to Duhem, if these rays are constituted in the way described by Maxwell—self inducing orthogonal oscillations in the electro-magnetic field. Duhem does commit to the phenomenological laws of reflection and refraction, and one can infer that he is committed likewise to the laws that indicate Fresnel's relations because these, although referring to amplitudes, are taken to be experimentally measured via the intensity of the respective rays. How this is specifically achieved is part of the measurement theory of photometry of the time. This may appeal to visual or thermometric, but more likely to Bunsen photometers. However, beyond the Fresnel Laws, Duhem was skeptical of the referents of Maxwell's equations (especially the notion of 'displacement current'). Therefore, Duhem rejects much of Maxwell's theory on empiricist grounds, while not denying that it provides, in its equations, strong psychological force, and appears as much as any theoretical set of laws to be what he called a natural classification—that is, a systematization which *appears* to reflect the natural kind relations of the world. It is still contested amongst historians what Duhem really meant by a natural classification, but this is not a debate we have room for here, so let's move on to another practicing scientist of the period who had a significant impact on philosophy of science.

2.3 Poincaré

Very much like Duhem, Henri Poincaré (1854-1912) was a specialist in science (particularly mathematics) who developed a novel and accessible philosophy towards physical theory. He is perhaps best known in philosophy of science for his views on the conventional adoption of Euclidean geometry in theories of space. The idea here simply being that although we have chosen the axioms of Euclid's geometry to express the structure of space in classical theories (such as Newtonian space-time), we could just as well have chosen some other, non-Euclidean axioms for our geometry. It is only convenience and familiarity that dictated the choices previously made—we could have adopted Riemann's geometry, or even Lobatschewsky/s.

To be sure, our decisions are informed by experiment and observation of the empirical world, but we are limited in our ability to determine the structure of the unobservable, and hence the apparent structure is really just that which we have found fits best given our preferences for calculational ease, or some other non-objective consideration. In this way Poincaré 's views here also significantly overlap with Duhem's, agreeing that the aim of science should be to provide economical, simple, and unified mathematical theories of observable phenomena:

> Now what is science? I have explained…it is before all a classification, a manner of bringing together facts which appearances separate, though they were bound together by some natural and hidden kinship. Science, in other words, is a system of relations. Now we have just said, it is in the relations alone that objectivity must be sought; it would be vain to seek it in beings considered as isolated from one another. [11]

Poincaré, like Duhem, also saw the need to address PMI type arguments:

> The ephemeral nature of scientific theories takes by surprise the man of the world. Their brief period of prosperity ended, he sees them abandoned one after another; he sees ruins piled upon ruins; he predicts that the theories in fashion today will in a short time succumb in their turn, and he concludes that they are absolutely in vain. This is what he calls the *bankruptcy of science*.[12]

Like Duhem he also saw the draw of providing physical explanations for observable phenomena that appealed to unobservable entities, and regarded such theories as making assertions beyond legitimate scientific constraints:

> Does science teach us the true nature of things?...no one would hesitate to say, no; but I think we may go further; not only science cannot teach us the nature of things; but nothing is capable of teaching it to us and if any god knew it, he could not find words to express it. Not only can we not divine the response, but if it were given to us, we could understand nothing of it; I ask myself even whether we really understand the question.[13]

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[11] Poincaré (1913, p.347)
[12] *ibid*, p. 122
[13] *ibid*, p. 347

Here we see perhaps indications of Kant's influence on the author, where the notion of an object's nature is equated with something like the *ding an sich* of the *Critique of Pure Reason*. This skepticism is revealed in a series of distinctions Poincaré makes which frame his philosophy of science. Perhaps the most important for us is the distinction between different types of hypotheses made in science. Broadly speaking, he saw scientists using hypotheses in three very different ways. First, 'real generalizations' are simply generalizations from observable instances such as that of generalizing a relation from experiments on some specific mass on a spring to all masses on springs. These are low level generalizations, but must always be empirically falsifiable (in the most charitable interpretation of that term). Second, there are 'necessary hypotheses', which are those that abstract away particular parts of a problem, or even idealize the experimental situation for ease of computation. For instance, one might hypothesize that visible light reflecting off billiard balls plays little relevant role in the momentum of a collision. The last kind of hypothesis is one that is used as an aid to understanding the phenomenon at hand. This can have harmful consequences if one doesn't take heed of Poincaré's warning above; that the true nature of unobservable entities must lie forever beyond our purview.

On the other hand, unlike Duhem, Poincaré did believe that science could legitimately assert knowledge of something unobservable; not the entities that inhabit the observationally inaccessible world, but the *relations* in which they stand to one another:

> Can science teach us the true relations of things? …At first blush it seems to us that the theories last only a day and that ruins upon ruins accumulate. …But if we look more closely, 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 has taught us a true relation, this relation is definitively acquired, and it will be found again under a new disguise in the other theories which will successfully come to reign in place of the old.[14]

In fact, it seems that Poincaré saw early on that successful science needs explaining, and rather than taking Duhem's anti-realist approach, adopted the view that one way of explaining such success was via appeal to the continuity of structure across theory transition. This would seem to be Poincaré's argument for realism about unobservable relations. In one form, it goes like this:

> And for these [relations], then, what is the measure of their objectivity? Well, it is precisely the same as for our belief in external objects. These latter are real in this, that the sensations they make us feel appear to us as united to each other by I know not what indestructible cement and not by the hazard of a day. In the same way science reveals to us between phenomena other bonds finer but not less solid; these are threads so slender that they long remained unperceived, but once noticed there remains no way of not seeing them; they are therefore not less real than those which give their reality to external objects; small matter that they are more recently known since neither can perish before the other.[15]

And in a last appeal, reminiscent of the NMA he says,

> 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

[14] *ibid.* p.348-9
[15] *ibid.* p.349

> convenient, it is true that it is so not only for me, but for all men; it is true that it will remain convenient for our descendents; it is true finally that this cannot be by chance.[16]

These quotes illustrate a number of important issues about Poincaré's views. Let's just focus on those that speak to the issue of structural realism. First, we can see that Poincaré is offering at least two kinds of arguments for belief in retained relations, which we can call 'retained structure'. One argument is a form of No Miracles Argument, in which he first appeals to the objectivity of convenient, or pragmatic, classifications of relations; he then suggests that these relations enjoy a continued objectivity over time and space and infers that the likelihood of this being the case if these relations were not true is very, very low. Therefore, one ought to believe that retained structure is genuine structure. This is so for observable and unobservable structure alike. The other argument tries to justify this last claim. Observable and unobservable structures are of a similar kind, and our inference to their existence is likewise of the same kind. He suggests that the sensations we experience of the observable world around us are all unified in some mysterious way, and that whatever the cause of such unity, the appearance is not random, but well ordered. We experience the relations between objects, and even parts of objects, in what appear to be non-haphazard ways. The same, he argues, goes for the structures we cannot directly observe, which are revealed by science. So, the observable relations between, for example, observable properties of a gas, like its volume, pressure and temperature, are revealed in the same way as those unobservable relations between molecules of

[16] *ibid.* p.350

that gas. Therefore, he reasons, we are justified in believing the unobservable *retained* structure of our theories has genuinely hooked onto the world, even though we are still prevented from knowing the intrinsic properties of entities inhabiting the unobservable realm. Notice the role played by the notion of structure being retained. In the last quote he appeals not merely to the structure being objective because it is the same for each of us, but also for our descendents, indicating his view that part of why this cannot be chance is especially because of structural preservation over theory transitions. Because of this last clause we ought to be somewhat careful when differentiating Poincaré's position regarding our example of the transition from Fresnel to Maxwell from that of Duhem. Where Duhem was willing to believe in the relations between observables, a kind of structural empiricism you might say, Poincaré goes further, arguing that we are also justified in believing the relations posited to hold between theoretical entities which are retained across theory change. But what is the difference in our example?

Obviously we can start by adopting Fresnel's Sine and Tangent Laws, since these are between observable relations. Now, what about the amplitudes of the electric field component for our three waves at a boundary? Initially it might seem that Poincaré would reject the electric field vector for each wave because these were clearly not present in Fresnel's earlier theory. But now we come to a problem of interpretation, namely, when Poincaré talked of theory change and component retention, was he concerned only with cases where the retained structure comes from a single theory, or could he mean that retention can apply to multiple theories that find their next generation to be a unifying theory? It seems that in our example we have just such a case. Maxwell produced a theory that unified electrical and magnetic phenomena, and at the same time, by a reduction of light to electromagnetic waves, also performed the added feat of unifying electromagnetic and optical phenomena. Thus, we could feel justified, even though Poincaré is quiet on the subject, in thinking that he would condone belief in the electric and magnetic vector relations. These vector components can be found in the work of Thomson and Faraday, and hence in a sense can be seen to be retained structure.

Although our example has provided some cause for concern, the important issue to focus on is that for Poincaré belief in theoretical relations that are retained in subsequent theories is on the same footing as is our belief in external observable objects. This argument, the one that links observable and unobservable relations, noticeably addresses the nature of our experiences. The relations between everyday middle-sized observable objects such as chairs and tables are given in experience in a similar manner, Poincaré suggests, to those of unobservable entities. But this relationship has not yet been clearly explained. How are we justified, for example, in believing we have hit on the correct relations? Poincaré suggests a No Miracles response, but it is well known that given any set of data points, finding a single function to fit them doesn't guarantee you've hit on the function that represents the phenomena. Just because the same curve works from theory to theory over time, why not think this interpretation underdetermined? There are an infinite number of ways to capture points on a curve, why think that choosing the most economical, or practical, is going to reveal the true fit? Furthermore, since Poincaré appeals to our experience of everyday objects, what of the phenomenalist's objection that we have no reason to believe the world is as it appears in anything other than the first person case? Well, in fact another philosopher of the period was addressing just these issues, and by considering his views on our knowledge of the structure of the unobservable world we will see how answering these problems leads to other, more challenging, problems. His name is Bertrand Russell.

2.4 Russell

Recall that structural realism in both its epistemic and its ontic forms accepts the axiological, metaphysical, methodological, and semantic theses of realism. Just as with entity realism, concerns arise over the epistemic thesis, and in particular, where one is to draw what we have been calling the epistemic line.

Structural realism has not always taken precisely this path. The first systematic formal account was given by Russell in *The Analysis of Matter*. On this account the primary concern was to establish the metaphysical thesis via a particular epistemic realism. That is, Russell's enemy was not Laudan's PMI (unsurprisingly), but rather the idealist and phenomenalist accounts of knowledge. On the idealist account, all that could justifiably be claimed were propositions limited to our own subjective mental states regarding the world; our percepts. The phenomenalist was willing to concede that we have knowledge of not only our own percepts, but also those of others (although not the reality of any percept not actually experienced by someone). Neither of these views accepted metaphysical realism as we have formulated the idea, and they certainly both rejected epistemic realism since they drew the epistemic line at percepts, either those belonging to oneself (idealism) or those belonging to both oneself and others (phenomenalism).

But it was our knowledge of the *external world* that concerned Russell. He clearly wanted to show that we really could have some genuine knowledge of reality beyond what goes on in our heads, and hence, he wanted to adopt some form of epistemic realism. He knew that it would be impossible to *logically prove* his opponents wrong regarding epistemic realism, but thought that by adopting what he considered *plausible scientific assumptions* he could make a good case for knowledge of a mind-independent world. He appealed to a causal theory of perception to provide this argument, which contained the crucial assumption that similar percepts are caused by similar stimuli. This is just to say that when we have different experiences in our perceptual fields, they have different causes.[17]

From this assumption Russell argued that although our percepts were not guaranteed to provide an accurate reflection of the external world, it is possible to infer from them the causal *structure* of the world. This is done according to laws of perspective.[18] He explains that when we map-out the percepts individuals have of a single object from different perspectives, we can extract laws of perception which determine what events are occurring at locations where there are no individuals to observe the object from, and hence, even where no percepts are formed. In this way, we infer knowledge of events that are not experienced, and thus of an external world beyond mere percepts (epistemic realism).

[17] I leave aside for the moment the issue of whether this was taken by Russell to be a 1-1 causal relation.
[18] Russell, (1927, p.216)

M.H.A. Newman (1928) gives a nice analogy of this process: imagine a large white screen that has, through something similar to a photographic developing process, revealed a partially complete picture of what appears to be concentric circles. If we assume the partially developed image is developed at random, we are justified in claiming the circles would emerge if we developed the entire screen. In this analogy, the partial image represents the collection of percepts, and the undeveloped but remaining parts of the circles represent the unperceived events whose existence Russell assumes, and knowledge of which he wishes to infer, in line with what he takes to be the usual canons of scientific inference.

Russell claims that when we combine the above-mentioned principle (that different percepts imply different stimuli) with the assumption that spatio-temporal continuity holds in the world, we can know the *structure* of our stimuli.[19] This knowledge is of the external world, which causes our percepts by stimulating our sensory systems. However, it extends only to the structural relations between such causes, we can have no knowledge of any of their intrinsic qualities (first-order properties and relations). Thus, he draws the epistemic line at a very abstract notion of structure.

But what is this structure supposed to be? For Russell it is the *form*, the abstract set of relations, which hold between entities within domains. This notion is not that of any particular relation between any particular set of objects, since that would entail knowledge of the relations and properties themselves, which cannot be inferred from mere percepts. More precisely, two sets of objects, A and B, with

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[19] *ibid*, p.226-7

respective relations, R and S, between their respective objects, are of the same structure when there is a 1-1 correlation between members of A and B, such that when two members of A have relation R, their correlates in B have relation S, and vice versa. Thus, a structure is the form of the relations between the objects in a given domain, and can be instantiated by any variety of sets with sufficiently many members. A structure is therefore something over and above the relations between objects in any specific domain. In fact, we might say that on this view structure is abstract rather than concrete in that it is the set of all those isomorphic forms of relations between objects in different domains, no matter what their specific instantiations may be. Thus, as Newman stresses, "The important feature of the definition, brought out by the example, is that it is not at all necessary for the objects composing A and B, nor the relations R and S, to be qualitatively similar. In fact to discuss the structure of the system A it is only necessary to know the incidence of R; its intrinsic qualities are quite irrelevant."[20]

From this it seems clear that Russell's epistemic realism is really quite limited. He is unable to take for granted, as we typically do in the modern debate, the existence of a mind-independent world (metaphysical realism), and as such is forced into a position even more conservative than the modern empiricist. That is, he can't even claim knowledge of the observable world, which today is taken for granted by empiricists and realists alike. Russell's epistemic realism, it could be said, is thin as a thread, tethered to reality only by his assumption that our sense experiences, our percepts, are caused by something, and that when that something changes, so do our

[20] Newman, (1928, p.139)

percepts. Structural realism in Russell's hands says that the only things we can claim to know are our percepts and the structure that lies behind them.

At this point, it should be objected that Russell's position really isn't as bad as all that. He is afterall fighting a much tougher battle than today's scientific realists; he is trying to defeat skepticism in its various forms, and that is a task neither the modern scientific realist, nor the constructive empiricist have addressed. As such, to claim that his position is weaker than empiricism is a bit cheeky. It seems that if one is willing to grant knowledge of everyday observable objects to the constructive empiricist, then one should do likewise for Russell. Fair enough. If we take this to be the case we might take his position to be just slightly stronger than that of the empiricist, claiming knowledge not just of observables, but also of the structure that underlies their behavior. However, in making this move one might claim that Russell's causal theory of perception is being extended beyond its legitimate scope to infer the causal structure of everyday objects, rather than its proper domain, that of percepts. I fail to see the difference here. If the theory is accepted as justifying knowledge claims to what we observe, how is this any different if we concede that these observations really do, rather than only might, accurately reflect reality?

And so, we can think of Russell's picture in a modified sense; it might be reformulated to first accept metaphysical realism, and then infer a structural epistemic thesis that goes beyond mere empiricism. In the next chapter we will see this reformulation attempted in the work of Elie Zahar and John Worrall. However, there does exist an apparently devastating criticism of Russell's original position as outlined above, and it will serve us well to look at this objection because if it holds up, then all subsequent structural realist positions will have to be careful to avoid it. This important consideration will also help us to sort through some ambiguities that arise in later positions.

The objection I refer to came from Newman in 1928, just a year after Russell published *The Analysis of Matter*. It is a simple charge of triviality. The idea is that to talk about the structure of a set of objects when you have no idea what the relations are that connect those objects is just meaningless. The only information that you really have is the number of objects you are dealing with, because from any collection of objects, which have some structure W, it is always possible to organize some other set of objects to be isomorphic with W, provided you have the right number of them. Since we have no information about the particular relation R that holds between these objects, apart from its existence, there is nothing but the number of objects to constrain how the new structure is constructed. This is trivial precisely because we can logically deduce W simply from the information that there exist the right number of objects in the world.

Newman's argument can be reconstructed as follows:

First, assume the following constraint: The truth of at least some propositions in physics should turn out to be non-trivial; that is, true by empirical investigation (discovery) not definition (stipulation).[21]

Claim: Russell's theory fails to satisfy this constraint.

1. According to Russell *only* structure is knowable: "the world consists of objects, forming an aggregate whose structure with regard to a certain relation

[21] *ibid*, p.143

R is known, say W; but of the relation R nothing is known (or nothing need be assumed to be known) but its existence; that is, all we can say is, '*There is* a relation R such that the structure of the external world with reference to R is W.'" [22]

2. However, "Any collection of things can be organized so as to have the structure W, provided there are the right number of them." [23]

3. Therefore, a consequence of Russell's theory is that only the cardinality of the domain of objects is an empirical question. (All other knowledge claims are derived as logical consequences of the existence of this number of objects).

4. However, it is false that our knowledge claims in physics are of the purely logical kind; mere consequences of the cardinality of the domain of a model, as suggested in 3 above. They are empirical claims, to be judged on evidence drawn from the world around us.

5. Therefore, it is false that our knowledge of the unperceived parts of the world is purely structural.

Notice that the problem for Russell arises *after* the domain of objects has been fixed, and is not therefore a problem of how to interpret the objects in the models of our theories. The problem is that even if a set of objects is specified, we still have to "distinguish between systems of relations that hold among the members of a given

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[22] *ibid*, p.144
[23] *ibid*, p.144

aggregate".[24] This is problematic because as a matter of logic there is always a relation between these objects with structure W, and thus knowledge of structure appears trivial. Russell needs to be able to distinguish the important (empirical) from the unimportant (logical) relations that generate structure W. He cannot do this because his theory claims we can have no knowledge of relations, and as such is incapable of comparing one system of relations with another.

So, for example, if Russell tells us that there exists a structure S, with some set of three objects O, and a relation between those objects R, that structure might be represented by the following map:

S: O—O—O

This map gives us a representation of the system of relation R. But the problem is that this tells us very little. For S we can construct a number of models:

S' = (U, R') where U is the domain of objects which are women, and R' is the mother-of relation.

S'' = (V, R'') where V is the domain of objects which are men, and R'' is the father-of relation.

S''' = (W, R''') where W is the domain of objects which are the real numbers and R'''is the successor relation.

And so on.

More importantly, even if we fix a single domain of objects, it is always possible to generate a further relation which instantiates the system. So if we take S' where U is the set of women, then we could use R', the mother-of relation for our

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[24] *ibid*, p.147

model, or any number of other relations. All that is required for our model is that the relation be transitive, non-reflexive, anti-symmetric, and apply to women. For example, other candidate relations include the taller-than relation, the heavier-than relation, the happier-than relation, and the more-intelligent-than relation. However, we would not be permitted to use relations, which although satisfying the logical properties of transitivity, non-reflexivity and anti-symmetry, fail to apply to women. That is, we cannot form a model for S' which uses the father-of relation, or the successor relation. It is, however, impossible for Russell to distinguish between just such legitimate and illegitimate relations for a specified domain because his theory of knowledge forbids anything but knowledge of second order properties of relations. Such properties are purely logical, and hence don't provide enough information to secure against the importation of illicit relations.

It would be illegitimate for Russell to argue that we know more than mere cardinality of the structure's domain because that would require some knowledge of either the properties of the objects or of the relations that hold between them. Given Russell's starting point, which is to overcome phenomenalism, this would be an illicit move (since phenomenalism permits knowledge only of percepts, not objects themselves). So he is stuck in the position of either maintaining his structural realism at the price of having only trivial knowledge of the cardinality of the domain, or of abandoning the position altogether. We will deal later with the modern formulation of Russell's position, which starts from the empiricist position and argues that structure really does give us non-trivial knowledge. For now it will suffice to recognize that Newman's objection pushes the epistemic line back for Russell to the phenomenalist position, which it seems is a realism only about our subjective mental states. In its modern formulation, it looks like Russell's position becomes empiricism, drawing the line at what is observable.

So, given what we have said about Russell's structural realism, what would he make of the preserved structure between Fresnel and Maxwell? He clearly opts for a notion of structure that applies only to second-order properties and relations, and so we need to ask what these are for the two roads to the amplitude relations. That is, his position restricts our knowledge to that of the logical properties of unobservable relations, and not any of the properties of the relata between which these relations hold. This is importantly different from Poincaré's view, where what we can know are the first-order relations between relata (about which we remain agnostic). As such it becomes very tricky to determine exactly where Russell could draw his epistemic line for this example. Russell was clear that what we may claim to know about the unobservable may be its purely mathematical structure, without any inference as to what this structure might actually physically denote. My intuition is to say he would be very happy to accept Fresnel's laws, but also Maxwell's mathematical description of the world. The latter however will be purely structural because its subject is the unobservable structure of the world. In regard to such mathematical descriptions he says, "When we are dealing with inferred entities, as to which…we know nothing beyond structure, we may be said to know the equations, but not what they mean: so long as they lead to the same results as regards percepts, all interpretations are equally legitimate."[25] We are then permitted to adopt Maxwell's theory, but must keep the mathematical relations free of properties that would tell us anything about specific relata. The problem with this move is that Fresnel was not working with the same mathematics as Maxwell, and so any structure that one finds preserved between the two theories will be part of a necessary mathematical reconstruction of these theories. Although this doesn't appear to be particularly problematic for the structural realist's case, since he can say that no matter how one goes about representing structure, it is the same stuff that gets retained, it requires significant reconstruction that Russell certainly never generated. The reconstruction is however appropriate to the more recent accounts of structural realism that we will look at next. Consequently, let's postpone such an excursion until the following chapter, where we consider new formulations of both Poincaré's and Russell's views, and delineate exactly what these new versions consider to be the defining characteristics of preserved structure.

[25] Russell, B. (1927) p. 287

Chapter 3

Structural Realism and Ramsey Sentences

We have up to this point been looking at the historical development of structural realism as a response to the PMI. In this chapter I want to turn to the current debate over structural realism and offer several criticisms of the most promising versions now in fashion. We will look first at how the philosophers John Worrall and Eli Zahar have adopted Poincaré's position, appealed to Russell's work to make the position more rigorous, and used Ramsey-Sentences to try and avoid the Newman objection. I claim that this move fails, and further attempts to save the position are currently looking hopeless. Then we will look at an alternative approach to the use of Ramsey-Sentences, one that relativizes the distinction between what we consider theoretical and what we consider observational, and find that it makes significant progress over the previous view. This theory is advanced by Pierre Cruse and David Papineau. There are still serious problems with their approach, which I argue are not special to Ramsey-Sentence strategies. I close the chapter by considering the relationship between structural realism and Ramsey-Sentences more generally, arguing that the former position hinges on epistemic considerations irrelevant to the latter's semantic orientation. In fact there are good reasons for using Ramsey-Sentences, but they have nothing to do with being a structural realist. The two positions do not entail one another, and in fact the most attractive forms of structural realism and Ramsey-Sentence Realism do not even coincide.

3.1 Poincaré Again—Worrall's Early Structural Realism

In a couple of very important papers John Worrall[1] has re-introduced into the scientific realism debate the position known as 'structural realism'.[2] On his view, which very closely follows Poincaré, the history of science shows dramatic discontinuities at the theoretical level, and hence we are justified in a skeptical attitude toward theoretical entities posited by past and current science. However, these interpretive blunders are offset by remarkable continuities in mathematical structure; the equations of our theories. It is in virtue of such mathematical continuity, which goes beyond the merely empirical level, that the optimistic No Miracles Argument is justified. Since scientific theories seem to exhibit significant continuity not just empirically, but also structurally, we should not be surprised that science has in general been a very successful endeavor.

Structural realism not only points out the structural continuity apparent in theoretical transitions, it also provides an explanation for these continuities. The claim is, as we saw in the last chapter, that structural realism provides an *epistemic constraint* on what it is possible for us to know about the world. This idea relies upon a clear distinction between the notions of structure and content in our theories, and it is this distinction that separates out those parts of a theory which we are justified in believing as true from those that are mere conjecture. Worrall views the dichotomy as that between a theory's mathematical equations and the theoretical interpretation of its ontology. Where there exists mathematical continuity across theory transitions and

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[1] Worrall (1989), (1994)

[2] Grover Maxwell was the first to use the phrase in a paper (1970) in which he appealed to Russell's version of the doctrine, but his interpretation fell into worse difficulties than those we are about to consider, so I won't further dwell on his contribution to the debate.

revolutions, we are justified in believing we have accurately hooked onto the world. The claim is that it would be an error to believe in theoretically interpreted ontology because it is just this kind of thing we find suffering radical discontinuity across theory transitions. Thus, structural realism adopts a realist position to the degree that it believes in structure, which is beyond the empirical. It rejects traditional realism by drawing an epistemic line at structure and discarding all theoretical interpretation. On the other hand, structural realism avoids instrumentalism because it views the mathematical structure in our theories to be a true representation of relations between unobservable entities, not merely a calculational device for generating predictions.

Worrall's aim then is to overcome the pessimistic induction with an account of successful science that marks out what is true, and at the same time explain radical theoretical discontinuity. To illustrate what he means he uses our example of the transition from Fresnel's to Maxwell's theory of light:

> 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 (probably but nonetheless importantly) involved; but there is an electro-magnetic field.[3]

The structural realist claims that Fresnel's theory made correct predictions because it accurately identified certain relations between optical phenomena, and

[3] Worrall (1994, p. 340)

especially because these phenomena depend upon something or other undergoing periodic change at right angles to the light—even though he was utterly wrong about the theoretical mechanisms involved. The point Worrall wants to emphasize is that Fresnel's theory didn't just accidentally make some correct predictions, it made them because it had accurately identified certain relations between optical phenomena.

However, one might ask, is this example idiosyncratic? Will structural realism be able to account for other revolutionary changes in science? Well, no I don't think so. This case is peculiar in that the equations were transmitted entirely in tact, which we saw in chapter two, but such identities between the mathematical structures of theories is rather uncommon in the history of science. Worrall thinks that in other cases the equations will be *limiting cases* of new equations, and hence, strictly speaking inconsistent. This won't be problematic if one can show that the limiting case scenario still provides reason to believe in some retained structure. To defend this idea he appeals to what is known as the 'Correspondence Principle': *mathematical equations of the old theory are limiting cases of those in the new*. This principle actually acts as a heuristic device in developing new theories, although applicable purely to mathematics, not to theoretical terms that might be used when interpreting mathematics. It is also a rule that seems to be at play in the history of physics, and is one that *seems* to legitimize structural realism over traditional realism. We will return to the merits and drawbacks of this principle below.

Worrall also rejects the requirement that entire theories make the world comprehensible, claiming that it is a mistake to think we can ever 'understand' the *nature* of the basic furniture of the universe. The structural realist embraces instances where a theory is so successful that we are required to adopt a problematic concept (like action at a distance) as a primitive part of our ontology, suggesting that our desire to explain is merely a symptom of our antecedent metaphysical prejudices. Structural realism therefore rejects the metaphysics of theoretical interpretation while embracing a formal realism. This is important because making this move would seem to exempt the structural realist from having to depend upon explanatory coherence of a theory's structure in order to infer to the reality of that structure—something standard forms of realism have traditionally relied upon.

It seems to me that the account given by Worrall is an advance over traditional realism in some respects, but suffers from at least five serious problems:

1. There is ambiguity in use of the term 'structure'. If we take 'structure' to refer to the *abstract* form of a set of relations that hold between entities, then the view is not sufficient to pick-out a unique set of relations in the world. This is because to single out a unique referent for a relation, we would have to stipulate what the intended relation is, which is to go beyond the *purely* abstract structural description.[4] This is a familiar point from chapter one, but to appreciate what is really meant by the notion of an abstract structure, we need to spend just a little time distinguishing it from its alternative, the notion of a *concrete* structure. Here we can use the familiar notion of a concrete structure as a system of related elements, such as the legs, seat and back of a chair, or the wood, plaster, metal joints and struts, clay tiles, etc. that are assembled in

[4] In chapter two we saw that this point was originally made by M.H.A. Newman in response to Russell's causal theory of perception, although it has now been revisited in Demopolous and Friedman (1985).

a set of relations to make a house. These relations between specific objects are a concrete structure. Now we can define an *abstract* structure in the following way:

> To define an *abstract* structure we can imagine collecting structures into *isomorphism classes*, where two concrete structures in the same isomorphism class are related by a bijective correspondence which preserves its system of relations in the sense that if in the one structure the elements $x_1, x_2, \ldots x_n$ satisfy the n-ary relation R then in the second structure the corresponding elements $y_1, y_2, \ldots y_n$ satisfy $R'(y_1 \ldots y_n)$ if and only if $R(x_1 \ldots x_n)$, where R' is the n-ary relation in the second structure which corresponds to R in the first structure.[5]

This notion of an abstract structure can therefore be thought of as a second-order form that is shared by the concrete relational structures which comprise a particular isomorphism class. For example, we can capture the abstract structure of a collection of particles, say a gas distributed throughout a room, by representing their relations with real numbers in phase-space. The concrete structure is composed of the actual relations between the particles, whereas the abstract structure is the second-order form of these relations which hold for any collection of objects falling under the same isomorphism class as the concrete structure. Our criticism of Worrall's position therefore amounts to the Newman objection, if he takes the notion of structure to be abstract, as defined here.

2. On the other hand, if the structural realist is using 'structure' in its *concrete* form, where instead one is referring to the specific relations between entities, then structural realism cannot be distinguished from traditional scientific realism without a

[5] Redhead (2001, p. 74-5)

dubious distinction between *structure* and *nature*.[6] Hence, structural realism in this form fails to make a legitimate distinction between the parts of theories we should or shouldn't believe, and therefore makes no progress over traditional scientific realism. The idea here is that the nature and structure of an entity are not separable, in fact they form a continuum. Structure and nature are both equally knowable; knowing one component entails knowing the other. This point has been illustrated by Stathis Psillos who makes this case through the example of 'mass': The property of resistance to acceleration can be captured by $m_i = \boldsymbol{F/a}$ and hence can be understood as a structural property. The property of a body being accelerated in a gravitational field can be captured by $m_g = \boldsymbol{Fr^2/GM}$. These two properties are of course identical, and as such "by equating these properties, more structure, so to speak, was added to *mass*, and knowledge about what mass *is* was increased…to show what an entity *is* is to show *how this entity is structured.*"[7] Thus, one cannot defend the dichotomy between nature and structure that Worrall relies upon.

3. Structural realism hinges on the observation that mathematical structure is preserved across theory transitions. However, as Psillos[8] has argued, mathematical continuity alone is not sufficient to answer the pessimistic meta-induction, we need a positive argument that identifies the mathematics *alone* as responsible for a theory's empirical success. The structural realist needs a separate argument to show that the mathematical equations *represent* the structure of the world; retention through theory change is not sufficient. To defend against this criticism the structural realist would

[6] This point is also argued for by Psillos (1995, 1999), Papineau (1996), and Ladyman (1998).
[7] Psillos (1999, p. 152)
[8] *ibid*, p.156

need to adopt an argument that would appeal to the correlation between the empirical success of our theories and their retained mathematical content, which aims to show that the equations have somehow represented the underlying structure of the world. Yet such an argument would also have to commit to the view that it is the mathematical content *alone* which is responsible for the empirical success of our theories. More specifically, Worrall would have to use an argument for structural representation akin to the No Miracles Argument itself. That is, both empirical success and mathematical structure are cumulative through scientific revolutions, and because empirical success suggests that the theory has somehow hooked-on to the structure of the world, one might plausibly infer that the mathematical structure has also hooked on to the structure of the world. However, this argument is incapable of providing justification for the reality of relations between phenomena without *first order properties* being attributed to those entities for which the relations hold. Psillos says,

> If structural realists were to use a version of (W) [the NMA] in order to claim that retained mathematical equations reveal real relations among unobservable entities, they would also have to admit that some theoretical content, not necessarily empirical and low-level, is well-supported by the evidence.[9]

The argument Worrall would have to make requires predictive success, which requires the kind of substantive properties for theoretical entities that the structural realist wants to remain agnostic about. This is problematic because any prediction requires auxiliary assumptions and theoretical hypotheses. As such the structural

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[9] *ibid*, p.154

realist is incapable of deriving empirical confirmation for his structural relations, and he has to concede some confirmation also the that which he wishes to ignore; theoretical elements of the theory.

4. To the extent that scientific realism is a view that is supposed to apply to *all* sciences across the board, structural realism is limited to only the mathematical sciences. That Worrall intends the position to apply more broadly is clear from his claim that:

> To give the argument from scientific revolutions its full weight and yet still adopt some sort of realist attitude towards presently accepted theories in physics *and elsewhere*, I argue that there is such a way-through *structural* realism. [10]

It should strike one immediately that the kind of examples used by structural realists are limited to cases where, aside from empirical phenomena, mathematical structure alone is preserved across theory transitions. But, one ought to ask, why does structure have to be mathematical in nature? What of all of those non-mathematical theories that clearly seem to be a part of the traditional conception of science and which have undergone theoretical transformation through scientific revolutions? Surely the biological sciences contain examples where retention of elements in a series of theories warrant the same realist claims as do those cases from physics to which the structural realist appeals. Take for example Darwin's theory of evolution, which was perhaps initially correct about the structure of the process of natural selection, although completely wrong about the nature of the mechanism by which traits are

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[10] Worrall (1989, p. 99) [My emphasis]

passed between generations. Another case might be the early 19th century theories of atomism, which seem to have been correct about the structure of matter, although completely wrong about the nature of the atom itself. If these cases are good examples, then either the structural realist needs to show us how these retained elements from the biological and chemical sciences can be construed on a structural interpretation, or he needs to accept the peculiar limitation of his view as only applicable to the mathematical sciences. If the latter alternative is embraced, we are left with a rather restrictive realism, one that fails to answer the pessimistic meta-induction in general. On the other hand, the former approach, that of applying the structuralist approach to the non-mathematical sciences, is going to have a very difficult time preserving the structure/nature distinction while maintaining a commitment to purely abstract structure.

5. The last problem I wish to raise derives from the structural realist's need to isolate similar structures across theory change, and is that of specifying exactly what 'similar structure' is supposed to mean. In his paper, Worrall points out that in the history of science we don't in general see mathematical structure retained entirely intact from one theory to the next, as was the case with Fresnel's equations. More commonly we find that the old equations reappear as limiting cases of the new. However, this account is not sufficiently clear. It is far from obvious that we can successfully compare the equations of quantum mechanics with those of classical dynamics. In the former case we are dealing with operators operating on rays in Hilbert space, in the latter we are talking of continuous real valued functions. In what ways and to what degree can these equations be said to be similar? There are obvious

similarities in the symbolic representation, but are these enough to secure the kind of continuity a structural realist needs? Although appeal to an interpretive metaphysics would be inappropriate to settle the issue, the structural realist needs to show that what the equations represent is retained through theory transitions. They cannot just settle for a similarity between the symbols in the equations, for doing so would reduce Worrall's position to a trivial symbolic realism. This would certainly not answer the pessimistic meta-induction because symbols alone generate no predictions.

Given these problems, it seems that the initial impulse to save scientific realism by adopting a Poincaré-like approach to even the Fresnel-Maxwell case is thwarted. Can this position wriggle out of these difficulties?

There have been several responses to the problems raised for Worrall's account that in one way or another advocate a variety of structural realism. One response to at least the first couple of problems listed above comes from Eli Zahar (in collaboration with Worrall himself), who has turned to a modified version of Russell's position to make structural realism much more rigorous than on the prior account.

3.2 Russell Reformed?—Zahar and Worrall and Ramsey-Sentences

Eli Zahar and John Worrall[11] have provided a more developed version of Worrall's original structural realism, one that now includes a response to Newman's objection to Russell. We'll start by analyzing their answer to Newman, then look at further problems for their position.

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[11] Zahar and Worrall (2001), Zahar (2004)

Zahar and Worrall argue that Russell made a fatal mistake when he excluded observation terms from his structural account of the external world. We have already seen how Russell argued for knowledge beyond the mental world of mere percepts via his causal theory of perception. In doing so, however, he had to treat all observable terms as theoretical, and hence on a par with those that one could claim to observe directly. The triviality of structural realism can therefore be quite easily overcome by appealing to an empirico-structuralist approach which includes observation terms. One can best capture this rigorously with the notion of a 'Ramsey-Sentence'. If we take the axioms of a theory and conjoin them in a long sentence, a Ramsey-sentence is simply what we get if we replace all the theoretical constants in this sentence with distinct variables, and then quantify over those variables. Since these constants will be predicates, the Ramsey-Sentence is a second-order existential generalization. This way, all the principles and laws and equations of a theory are transformed from being composed of observable and theoretical terms, to being either observable or treated as unknown quantified variables. A theory might be originally represented as $T(t_1, t_2, \ldots t_n: o_1, o_2, \ldots o_m)$, where the t's represent theoretical terms and the o's represent observation terms. The Ramsey-sentence of this theory would look like this: $\exists x_1, \exists x_2, \ldots \exists x_n\, T(x_1, x_2, \ldots x_n: o_1, o_2, \ldots o_m)$.[12] Instead of dropping all of the terns in one's theory and then quantifying, one now just drops the theoretical terms. This generates mixed propositions of theoretical and observational terms, which themselves are now empirically falsifiable.

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[12] It strikes me that altering Russell's position in this way takes for granted reference to observable objects, something Russell himself was clearly unable to assume in his argument against idealism and phenomenalism. It is, however, clearly permissible within the current debate.

The move to a mixed expression like this takes for granted a clear distinction between terms classified as observational and theoretical. Zahar and Worrall argue that although theoretical entities are knowable only by description, the observables are also knowable by ostension. The distinction between observable and unobservable is of course problematic, but they are willing to bite the bullet on this and draw their epistemic line at what is phenomenologically perceptible. It might appear Zahar and Worrall are walking a fine line. This is, they think, unavoidable:

> However the distinction is understood, no serious version of structural realism can get going without some such distinction. If *all* the predicates of a scientific theory are taken to be interpreted only within the context of the claims made by the theory, if, that is, none is taken to be firmly anchored in experience independently of our attempted descriptions of the universe, then the constraints imposed by the Ramsey-sentence (or indeed by the original theory itself) would be hopelessly weak.[13]

Once a distinction between the observable and unobservable is made, the Newman objection supposedly fails, since it works only for treating structure as *entirely* abstract. The Zahar and Worrall approach now claims we can have knowledge of the structure of the unobservable world via the structural descriptions of theoretical entities. These Ramsey-sentence descriptions do not treat theoretical entities as entirely abstract; they detail the existence of entities that have causal capacities and which exist in both space and time. This is not, however, to believe in those particular entities. Structural realism is therefore now operating as an epistemic constraint on theoretical knowledge. On this view theories are physically interpretable, but such

[13] *ibid*, p.239

interpretations cannot be claimed as knowledge. All that is licensed are structural claims. So, for Zahar and Worrall a theory and its Ramsey-sentence have the same observational consequences, and therefore there is no experimental or theoretical reason to prefer a theoretically interpreted theory; it is better to adopt its logically weaker Ramsey-sentence. This way one can avoid commitment to all those metaphysically problematic entities over the history of science, and still preserve a minimal form of scientific realism. I will from now on call the Zahar and Worrall view 'Strong Structural Realism' because it appeals to a strong version of the theory/observation distinction.

One may of course object that structural realism in this form still makes only trivial claims beyond the observable because what the Ramsey-sentence says about unobservables is nothing but a consequence of logic, provided the initial domain has the right cardinality.[14] This amounts to the claim that structural realism achieves no more in its epistemic assertions than a consistency and cardinality constraint. There appears to be nothing 'over and above' the observable content that can't be reduced to logic or mathematics.

Zahar and Worrall respond that it is wrong to think the Ramsey-sentence follows only from its empirical basis, even if this basis consists of all true observation reports entailed by the sentence. There is more to their position, they claim, than mere empiricism. On the structural realist's account the compactness theorem provides us with a formula that can be falsified, and this is significant. For example, imagine we have a simple theory that says 'All ravens R, have a G gene, and all objects with a G

[14] William Demopolous and Michael Friedman have made just such a claim in their (1985).

gene are black, B, in color'. That is, $(\forall x)(Rx \rightarrow Gx)$ & $(\forall y)(Gy \rightarrow By)$. Now the Ramsey-sentence for this theory looks like this: $(\exists Z)[(\forall x)(Rx \rightarrow Z(x))$ & $(\forall y)(Z(y) \rightarrow By)]$. Because 'G gene' is a theoretical term, it gets expelled for the variable predicate Z. This sentence can then be reduced by transitivity to $(\forall x)(Rx \rightarrow Bx)$.

However, this Ramsey-sentence says nothing more than can be derived logically from the infinite set of empirical observation statements of the form Rx & Bx. That is, from observations of ravens and black objects one can logically infer for the specified domain all that is captured by $(\forall x)(Rx \rightarrow Bx)$. Thus, the Ramsey-sentence says nothing more than the empiricist claims for Rx & Bx.[15] Zahar and Worrall counter that $(\forall x)(Rx \rightarrow Bx)$ cannot be derived from a finite set of true observation statements; even if Rx & Bx have been verified within a domain with n members, $(\forall x)(Rx \rightarrow Bx)$ can be falsified if the domain of objects satisfying the variables has more than n elements, where n exhausts the number of those elements that are observable.

But where does this argument get the strong structural realist? Does this position really commit to anything beyond empiricism? Surely their claim is yes, but what is this extra stuff that they get? Presumably what is being claimed as knowable structure lies in the universal generalization. The move from Rx & Bx to $(\forall x)(Rx \rightarrow Bx)$ provides us with more than we are strictly licensed to infer from the empirical basis alone. This basis consists of a finite number of observations, yet the

[15] *ibid,* p.635

Ramsey-sentence, which is equivalent to $(\forall x)(Rx \rightarrow Bx)$, goes further. For those who question this claim, and who think that empiricism is justified in making just these kinds of generalizations, Zahar and Worrall respond, "This would go against the canons of even the most liberal version of empiricism; for $(\forall x)(F(x) \rightarrow K(x))$ fails to be fully empirically decidable. This is precisely why Schlick and other members of the Vienna Circle decided to regard all synthetic universal statements as expressing inference rules rather than propositions.[16]

I think that the strong structural realist position we are considering raises at least two questions: (1) Does this response really answer the Newman objection? (2) Are Zahar and Worrall justified in thinking the strong structural realist is really a realist, as indicated by their response to Demopolous and Freidman, or are they just some kind of optimistic empiricists?

Zahar and Worrall clearly think this approach adequate to answer the Newman objection. By leaving observable terms in the Ramsey-sentence they think we can have non-trivial knowledge of the external world. This move modifies Russell's approach in a straightforward way, which I alluded to previously. By including in our knowledge claims the existence not merely of the structure of the external world, but also the properties and relations of directly perceivable objects, Zahar and Worrall take the step that Russell was not permitted. They would be seen to beg the question against phenomenalism, and hence against the original Newman objection. However, knowledge of observable entities is no longer in dispute, and we need to ask whether their response doesn't suffer from the equivalent of a Newman triviality objection at

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[16] Zahar and Worrall (2001, p. 241)

the level beyond the observable. That is, why are Zahar and Worrall justified in thinking we can have knowledge of the structure of the unobservable?

We saw above that they claim we can have knowledge of theoretical structure, which is to say, of the properties and relations of theoretical entities, without making a claim to know the entities themselves. The pre-reduced Ramsey-sentence of our toy theory above looks like this: $(\exists Z)(\forall x)(Rx \rightarrow Z(x))$ & $(\forall y)(Z(y) \rightarrow By)$. The knowledge constraint is that we cannot claim to know what the Z's represent—they are theoretical entities that have been replaced by predicate variables. They could be single genes, multiple genes, or a host of other unspecified entities. However, these Z's do have to satisfy certain constraints. They are the objects that have to stand in certain specified relations to observable entities, dependent upon the descriptions that they are given. The question that needs answering is whether these variables, (Z's), although abstract themselves, entail something concrete about the unobservable. But in answering this question, the structural realist is left in a rather unpleasant dilemma: If the properties and relations by which one specifies unobservable entities in the Ramsey-sentence are concrete, then the structural realist has evaded the Newman objection only by *stipulating* such properties and relations as belonging to the Z's. Claims of this sort can hardly be considered knowledge. If the properties and relations of the Z's are, on the other hand, purely structural, and hence, abstract in the sense that we know nothing of their nature, then the Newman objection has arisen again, and the knowledge they offer us is trivial. So, it is still unclear how Zahar and Worrall's move towards concrete observable objects rather than abstract objects solves anything.

The strong structural realist may reply, the step towards universal generalizations regarding all possible observation reports is indeed a form of non-empirical, structural knowledge we can have, as shown above in the reduction of the Ramsey-sentence. This response brings us to the second of our questions, is the strong structural realist anything more than an empiricist? They clearly adopt the view that non-theoretical entities are synonymous with observables, and as such they have to defend the epistemic authority of the same observable/unobservable distinction van Fraassen has spent so long protecting. They have at least this much in common with empiricism, and most would argue that is quite enough. If the difference between the two camps is only that an empiricist refuses to endorse universal generalizations, then we might ask why Zahar and Worrall feel justified in endorsing such generalizations themselves.

One plausible answer is that only by appeal to the theoretical structure of a theory can we save ourselves from the PMI. If we look to Zahar's earlier chapters on Poincaré[17], and to Worrall's ([1989], [1994]), we see in both a strong commitment to the no miracles argument. Both cite Poincaré as being on the right track when appealing to structure as picking out the parts of our most successful theories and as genuinely hooking onto the world. It is the structure that saves the realist, this is what gets preserved through theory transition. Our theoretical knowledge is only of structure. As we have seen, it cannot be abstract structure, else it would fall to the Newman objection. But neither can this knowledge be of anything theoretically interpreted, because that would commit us to some form of theoretical ontology,

[17] Zahar (2001, especially p.56)

which is exactly what the strong structural realist is trying to avoid in his response to the PMI.

To judge whether this position is much different from empiricism, we can contrast it with some of van Fraassen's comments on structural realism[18]. In fact, van Fraassen thinks an empiricist structuralism is perfectly defensible. He argues that there is indeed a steady accumulation of knowledge through scientific revolutions, that it is structural, and that it is progressive. However, unlike the abstract structure studied in mathematics, this structure is of the concrete, observable world. It is simply the empirical knowledge that has been tested and retained through the triumphs of past science. This knowledge provides an answer to the no miracles argument because new *empirical* successes are a credential for acceptance. We have successful scientific theories, van Fraassen thinks, because they are the only ones we accept. However, new theories are not successful because of, nor are they selected for, continuity of *theoretical* relations, as Zahar and Worrall suppose. Van Fraassen sees any apparent structural cumulativity as a representation of nature all right, but only in the sense that they made possible models of empirical, observable phenomena. There was nothing special about the structure, nothing that meant it correctly hooked onto the world. Rather, mathematical structures are a partially accurate way of establishing a model for the phenomena that we wanted represented at some particular level of discernment. The empirical descriptions of phenomena are the parts that accumulate, and require explanation by successor theories. The theoretical substructures are retained only to the degree that they successfully model such phenomena.

[18] van Fraassen (2006)

So it *might* appear that the strong structural realist's commitment to theoretical structure in the form of unreduced Ramsey-sentences is more than an empiricist of van Fraassen's stripe is willing to take onboard. However, as we saw above, the reduced Ramsey-sentence seems to go beyond the *strictly* empirical only by advancing universal generalizations ranging over variables with observable predicates. If, as the strong structural realist claims, this is not empiricism in virtue of the falsifiability of such generalizations, then it is 'realism' only in virtue of being a liberal empiricism. The constructive empiricist, like van Fraassen, who is willing to treat statements regarding all possible observables as legitimate knowledge claims, rather than merely those events so far observed, would also permit universal generalizations of observable phenomena. This means for Zahar and Worrall that the constructive empiricist is no longer empiricist, and van Fraassen turns out to be a realist! It rather looks like their own account appears realist only in contrast to an outdated, positivist notion of empiricism. The story they tell refuses to council belief in those theoretical entities that are replaced by Z's in our Ramsey-sentence above. As such, Zahar and Worrall refuse to believe our theories are correctly representing the underlying nature of unobservable reality, and therefore they should not be considered realists at all. They draw their epistemic line just beyond the observable, but not far enough beyond it to license belief in theoretical entities. To them, the line between empiricism and realism is far closer to the observable/unobservable distinction than most would concede, and it is only this that allows them to reject the existence of things like electrons and yet still consider themselves realists.

But even if we were to concede the label 'realist' to their position, does the account Zahar and Worrall advocate really answer the PMI? Unfortunately, as we have seen, Worrall's argument hinges on a rather unusual case where the mathematical structure of the original theory (Fresnel's ether theory of light) was entirely preserved in its successor (Maxwell's theory of light). This is extremely rare in the history of science, and as such cannot possibly hope to answer the PMI. Using the transition between these two theories seems to me to provide a nice case by which we can delineate exactly what different versions of structural realism commit to, and on top of that, it seems a minimal requirement that any form of realism be capable of accounting for this example. However, satisfying the Fresnel-Maxwell case study is only a necessary, but not a sufficient condition for structural realism to answer the PMI. This point is recognized by Worrall, and he appeals to the vague notion of the correspondence principle, and its 'limiting cases' as being the appropriate way to treat the original/successor theory relation. It remains to be seen, however, how one can interpret the formal structure of Phlogiston theory as a limiting case of Oxygen theory. We don't get much more in the way of explanation from Zahar. Perhaps it will help us to look at an example he provides in *Poincaré's Philosophy* of structure preservation through scientific revolutions.

Zahar's tactic is still to draw attention to the constancy of relations that remain across revolutions, while permitting the referents of the relations' arguments to change. His first example appeals to similarity between the equations in Fresnel's, Maxwell's and Lorentz's hypotheses regarding the mechanical ether, the electromagnetic ether, and a disembodied electromagnetic field respectively. He doesn't unpack the details of this case however. A second example is the form of the laws retained from Newtonian to quantum mechanics. The Newtonian equations, $[p_x = m*dx/dt, dp_x/dt = -\partial V/\partial x$, etc.] connect the acceleration of a particle with the force acting upon it. Those of the corresponding quantum mechanical equations, $[<p_x> = m*d(<x>)/dt, d(<p_x>)/dt = -<\partial V/\partial x>$, etc.] are relations between expectation values.[19] He focuses on this case explaining that one of the primary difficulties in defending structural realism lies in its lack of a semantics that can interpret relations without going through their relata. If he can appeal to prima facie plausible cases like those above, then perhaps the position can be made more attractive.

It is in virtue of the Correspondence Principle that the transition from Newtonian to quantum mechanics preserved such a high degree of form, or similarity. The relations between referents are left almost in tact, even though there is no commitment at all to what the arguments denote. However, the structural realist is subject to the criticism that these similarities are remarkably thin; although there may be some formal similarities between the two, quantum and classical states and observables have utterly different mathematical classifications.

To see this, notice that classical states are represented by a point in a real valued phase space—the state of the system at any given time can be represented by a point in this space. The point is given by a set of real-valued coordinates, so if this space were two-dimensional then we'd by using $\mathbf{R}^2$, the plane of the reals. On the other hand, pure quantum states are represented as rays (or subspaces) of Hilbert space. This space is a complete complex vector space, and is therefore defined using

[19] Zahar (2001, p.39)

complex numbers. A two-dimensional complex space uses $\mathbf{C}^2$, the plane of the complex numbers, and a point in complex space is given by two complex numbers. Furthermore, because classical observables are real valued functions of that point in phase space they commute, whereas the observables in quantum mechanics use non-commuting Hermitian operators ($\hat{O}$).

Surely in this case, where the strong structural realist claims we can maintain some continuity between the classical and quantum formalism, it is not so much that we have to give up our ontology, (which picks out the referents), but that we can't even claim to have latched onto the right logical form of the relata themselves.

Zahar recognizes this problem, claiming that although it seems premature to think we can make claims about relations when we can know absolutely nothing about their relata, there is still hope because, "Mathematics provides examples where the focus shifts from the study of elements subsumed under certain predicates to higher-order relations between predicates themselves." In this case, "Quantum theorists extract non-classical logics from the lattice of subspaces of a Hilbert space, *without* mentioning the vectors constituting these subspaces. Lattice theory can thus be viewed as a study of universals without direct reference to any individuals."[20] This means that the form of the equations need not any longer be taken to express the relations between specific objects, but rather, in some way it captures the mathematical structure of high-level regularities in the world.

Now what should immediately strike us is that this move away from concrete relations between individuals looks a great deal like the move to abstract structure

[20] *ibid*, p.40

Russell initially argued for. However, the approach advocated by Zahar and Worrall includes observables, and as such is not subject to Newman's triviality objection at the empirical level. Besides, right now we are concerned with whether this view provides a response to the PMI, so let's take such abstraction to be permissible. Nevertheless, high-level theory is still problematic as a response to the PMI. Although the structural realist may be happy to give up the full meaning of the symbols in the equations, they should still be reluctant to say that the equations could represent *absolutely anything*. If this were the case, the equations would become empty and trivial. It would become impossible to understand what the structure that is being preserved is minimally supposed to be. We can see how the symbols apparently compare, but there is nothing to suggest that we need remain attached to these particular symbols. What do splotches of ink on paper have to do with the relationships between theoretical phenomena?

Perhaps the strong structural realist ought to appeal to something like a thin definition that these mathematical objects might get from their relationship to other equations. Perhaps their status remains somewhat secured by their relation to other equations, and the objects that comprise them. Perhaps we can make some kind of sense of what an equation is supposed to mean in virtue of its position in a mathematical network. But this probably wouldn't work either. Structural realists would presumably want to reject any holistic account of even thin meaning. They'd reject it because they need the flexibility to be able to drop from the history of science other equations that were not successfully retained through theory transition. On the other hand, they don't want to say that just because a similar structure was used in two or more disparate theories, perhaps in entirely different disciplines, that this means they were formally identical. If we were committed to this, then two sets of equations, which intuitively do not support the same successful predictions, would both get confirmation from one another's disciplines. Colin Howson[21] suggests that if this were the case, we'd have to credit physics with predictions in population genetics, since they use the same diffusion equations.

So, it looks like the Zahar-Worrall structural realist is in a tight spot with regard to both establishing his position as anything more than empiricism, as well as answering the PMI. After all, the more they retreat from the realist position, and hence the stronger their answer to the PMI superficially appears, the less they have to be realist about, and the less they have to be realist about , the closer they come to empiricism.

3.3 Relativising Ramsey Sentences

The problems raised above for strong structural realism seem to hinge primarily on the position being committed to a strong observation-theory distinction. This distinction has so far been treated as falling at the line between what we can directly perceive with unaided senses, and that which requires theory-driven interpretation. The strong version of the dichotomy tells us that observation terms are permitted in a Ramsey-Sentence of any axiomatized theory, whereas non-observation terms must be expelled. But need we stick with this dichotomy if we wish to support *some* form of structural realism? It seems that if we want to stick with the Ramsey-

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[21] Howson (2000, p. 40)

Sentence approach we need to work with some version of the theoretical/non-theoretical dichotomy, but this need not necessarily be equated with the strong theory-observation distinction we've been working with so far.

The idea for the structural realist is clearly to pick out that formal component of historical scientific theories that can be tracked over revolutions as the correct part of the theories responsible for their empirical success. Appealing to pure, abstract structure runs us into the Newman objection, so perhaps a weaker notion of structure can be used in combination with the formal representation of the Ramsey-Sentence approach to provide us with that elusive thread through science which reflects the truth in our best theories.

Although they themselves do not directly offer a response to the pessimistic meta-induction, Pierre Cruse and David Papineau[22] defend a form of structural realism by claiming that on one interpretation of the realist thesis, the referential status of theoretical terms is actually irrelevant. In particular, to address Laudan's original PMI they suggest:

> Laudan concurs with the thought that a realist explanation of empirical success in terms of approximate truth requires that the relevant theory refers…our intention is to propose an alternative realist hypothesis which removes theories of reference from their alleged role in the realist's explanatory scheme.[23]

This is going to provide an answer to Laudan, they believe, because they are taking reference right out of the picture—reference failure may indeed occur for terms

[22] Cruse and Papineau (2002)
[23] *ibid*, p.176-7

like ‘phlogiston’ or ‘caloric’, but it doesn’t pose a PMI problem for their position because they don’t rely upon reference. So, although they do not guarantee against the possibility of a pessimistic induction that doesn’t depend on successful reference, their new account is supposed to avoid the PMI with which we have been concerned.

Like Zahar and Worrall’s view their interpretation claims that the cognitive content of a scientific theory lies in its Ramsey-sentence. However, this new version of structural realism, which from now on I will call ‘Ramsey-Sentence Realism’, goes further than its predecessor by suggesting that when we quantify away theoretical terms, scientific realism is no longer hostage to any theory of reference—we don’t even have to provide a story of how a theory refers to the correct structure of the world. This is important because it is the rule which tells us we can infer from success to correct reference that plays a necessary role in the No Miracles Argument. That is, success legitimates claims to correct reference, and correct reference is necessary for a theory to be even approximately true. If one can show that accepting this link between reference and approximate truth is not necessary for scientific realism, then it might be possible to overcome the pessimistic meta-induction.

Ramsey-Sentence Realism may start off looking similar to the Strong Structural Realism account but it makes no commitment to the observable/unobservable distinction as being epistemically privileged. On the new approach, “The Ramsey-sentence realist says that we should believe in the approximate truth of a successful theory’s Ramsey-sentence, on the grounds that it would be a miracle that the theory were successful, were its Ramsey-sentence not true."[24] Here then lies a crucial difference: we are counseled to believe not in the (limiting case) mathematical relations retained through theory transitions, but rather we ought to believe in the entire Ramsey-Sentence itself. The difference is important because on this new account we are asked to make an *existential commitment* to all of the entities whose theoretical properties we have stripped away with the process of quantification, rather than merely to the relations which we see retained from one theory's axioms (and its Ramsey-Sentence) to the next's.

All that Ramsey-Sentence realists take to be necessary to answer the PMI is to show that 'approximate truth' can still be used to explain the success of science, and this can be established even if a theory's terms fail to refer. The differences between the two views on Ramsey-Sentences starts to emerge more clearly if we consider how Cruse and Papineau's account attempts to answer some of the problems raised for Strong Structural Realism:

1. Ramsey-Sentence Realism is not ambiguous on the term 'structure'. The view holds that we can claim to know the non-theoretical terms in our Ramsey-sentence for a theory, and that we can know the properties of the theoretical terms as they are used in that theory. Knowledge of such properties entails knowledge of the relations between the variables used to replace theoretical constants, and this means we can know the structure of concrete relations that hold in the world. This is a concrete, not an abstract, notion of structure.

2. Because Cruse and Papineau's use of 'structure' is concrete, the view they advocate may appear subject to the charge of making an arbitrary distinction between

[24] Cruse and Papineau (2002, p. 179)

structure and nature. This accusation depends upon what one considers theoretical in one's theory. Ramsey-Sentence Realism says that some terms are to be considered theoretical and since it does not commit to the strong observation-theory distinction it is unclear why one should take their notion of structure to be arbitrary. We will soon see in fact why their distinction is neither arbitrary nor universal, and it is this latter property that makes it impossible to specify here.

3. Although Strong Structural Realism hinges on the observation that mathematical structure is preserved across theory transition, Ramsey-Sentence Realism doesn't. What gets preserved across such transitions is the Ramsey-sentence, not just mathematical entities. As such they do not face the problem of showing how mathematics alone is responsible for a theory's empirical success.

4. Similarly, because Cruse and Papineau's proposal does not treat structure as singularly mathematical, they evade the criticism that structure is incapable of capturing theoretical continuity in the non-mathematical sciences. It seems quite reasonable on their approach to think of theoretical changes in, for example, chemistry or geology to be capable of characterization in terms of Ramsey-sentences.

5. The last problem was Strong Structural Realism's need to isolate similar structures across theory change; this theory needs to have specified exactly what 'similar structure' is supposed to mean. It should be clear that 'similar structure' for Ramsey-Sentence Realism appeals to embeddability of one Ramsey-sentence into another. If the objects of a predecessor theory have all the same properties as specified in the successor, then the original Ramsey-sentence will be preserved, provided the

theoretical/non-theoretical line is drawn in the same position on both accounts. ‘Similar structure’ is therefore perfectly well specified on their account.

Now that the position has been introduced, let’s consider how Cruse and Papineau refine Ramsey-Sentence Realism in light of certain problems, although I think that each of their refining moves is ultimately inadequate.

First, one may ask how is it that one decides what is and what is not ‘theoretical’? The distinction may itself be flexible. Cruse and Papineau ensure that their position does not collapse into empiricism by drawing the ‘theoretical’ line, not at the observable/unobservable position, but in a manner first expounded by David Lewis[25]. On Lewis’ account we treat as theoretical only that which is not ‘antecedently understood’. That is, for some theory T, what is antecedently understood are terms that receive their meaning from *outside* the theory in question. The division between theoretical and non-theoretical terms is now really that between old and new terms in a theory. The old terms are defined through other theories, the new are those whose meaning is given only by the theory at hand. Since the meaning of terms is derivative on prior established theories, this approach advocates a *theory-relative* account of how to define theoretical terms. Accompanying this relativity, there would seem to be the threat that the meaning of all terms suffers from a regress through theories. Where are we to ground our terms if they always rely upon some that are previously understood?

Cruse and Papineau avoid this problem by appealing to the notion of a primitive language, consisting of terms that themselves are not defined in any theory. They say.

[25] Lewis (1970)

> Without prior empiricist prejudices, why not allow that a term could fail to be defined in a theory, and yet be neither observational nor logical? Antecedently understood terms could thus refer to such substantial non-logical relations as causation or correlation, or indeed to many kinds of unobservable things.[26]

However, I see several serious problems with this attempt to ground the Ramsey-Sentence Realist's indispensable distinction between theoretical and non-theoretical terms:

1. Lewis' account builds upon an assumption of a previously defined language, whereas Cruse and Papineau have no such previously defined language upon which to build-up new terms. Surely they can only rely upon this Lewis-type approach if the primitive language is made plausible. They've made no attempt to argue for such plausibility. Where is such a primitive language supposed to arise, and on what grounds does it avoid begging the question against the anti-realist? Besides, a primitive vocabulary is supposed to be theory independent, but this is arguably not possible. It has been a commonly accepted thesis since the '60's and '70's that theory inherently infects observation statements, and as such that there is no clear distinction between theory and such a primitive language. In response to this argument it might be possible for someone advocating Cruse and Papineau's line to respond using something like Fodor's account of theory independence of observation. Although this would be an interesting approach , I do not have room to address such a response here.

2. What does 'antecedently understood' really mean for Cruse and Papineau? Does it mean that some prior theory introduced a term and that theory was successful?

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[26] Cruse and Papineau (2002)

If so, what are their criteria for success? This is a notoriously ambiguous notion; does the success have to be one of explanatory depth, novel prediction, empirical adequacy, or what? Perhaps the theory need not even be entirely successful, maybe it suffers from some serious anomalies, yet is still considered a legitimate forum for the introduction of new theoretical terms that later come to be taken as old. On the other hand, perhaps 'success' is not the defining characteristic of a theory that legitimately introduces new terms.

Here we arrive at the crux of the issue, how is the Ramsey-Sentence Realist to distinguish those theories from which we can adopt a term, once theoretical and now (in a new theory) non-theoretical, from those theories in which a new term is introduced, but which we now consider it illegitimate to introduce such a term? For example, what distinguishes the legitimacy of oxygen theory and not that of caloric? The distinction has to pick-out such legitimacy in a non-*post hoc* manner, and given the Ramsey-Sentence Realist's approach, must be capable of signifying why caloric theory's Ramsey-Sentence has a theoretical term in it that is not to be converted into an old term in a new scientific theory that wishes to use it. Similarly, this account must indicate why 'oxygen' can legitimately be converted from theoretical to old term in a new theory.

We see then that although for Cruse and Papineau correct reference to theoretical terms is not required for the approximate truth of a theory's Ramsey-Sentence, correct reference to old terms definitely is necessary. However, without some account of how to pick between legitimate and illegitimate cases there's no reason to accept a new term in theory A as an old term in theory B. We need some notion of what makes a theory legitimate such that its theoretical terms can then be used in subsequent theories as non-theoretical. Cruse and Papineau could in fact be said to define the problem away; they assume which properties in the Ramsey-Sentence are legitimate ones, but how are we to tell which are and aren't legitimate properties ahead of time? As must by now be obvious, this project just is that of the preservative scientific realist; to select those parts of past false but successful theories that were truly referential. From these considerations it is tempting to conclude that for the Ramsey-Sentence Realist, reference is smuggled-in through the notion of 'antecedent understanding', and that this is all that differentiates the position from traditional realism. On such an interpretation Cruse and Papineau's Ramsey-Sentence Realism just collapses into full-blown traditional scientific realism, the very position to which it was a response.

3. There is a final objection to Ramsey-Sentence Realism I want to raise. Cruse and Papineau face a problem akin to that of Worrall's need to select similar structure across theory transitions. Remember that in objection 5 to Worrall's account I argued that structural realists are incapable of specifying the required continuity across theory transitions because they need a notion of similarity of structure that is more than merely symbolic. In the case of Cruse and Papineau it is going to be similarity of concepts, or meaning of terms, that causes the problem. That is, where we have Ramsey-Sentences for at least two different theories, continuity lies in the descriptions of their theoretical terms (e.g. 'mass' in classical and relativistic physics). But the meaning of these terms is going to differ from one Ramsey-Sentence to another, since they will have different properties. For example, in classical physics we can define

'mass' via $F = ma$ and $F = Gmm/r^2$. In relativity the notion of mass occurring in Einstein's equations has a far more complicated, and arguably, different meaning. How can Cruse and Papineau maintain even simple concepts like 'mass' across Ramsey-Sentences? The terms have different meanings in these sentences, even though they may not be considered theoretical at all. Therefore, this is a simple similarity issue that afflicts not merely new, but also old terms in their account.

So, given the preceding objections, I think it fair to say that this revised, relativized form of Ramsey-Sentence Realism is not adequate to the task of solving the structural realist's problems.

Before dismissing the view entirely however, it might be wise to consider if it was even fair to expect Ramsey-Sentence Realism to answer the PMI. In closing this chapter I now want to ask what the relation is between Ramsey-Sentence Realism and Structural Realism. In particular, we need to consider if one approach entails the other, or whether they are two separate theses altogether. I will argue the latter claim, proposing that how we represent our theories is irrelevant to their epistemic status, and since Ramsey-Sentences are merely a form of representation they are uninformative when it comes to determining the warrant our theories enjoy. Of course it is a separate question whether if we cannot use Ramsey-Sentences to represent a theory, then we ought to reject them as a means of expressing our knowledge of the world.

3.4 Does the Structural Realist Need Ramsey Sentences?

I take it from Worrall's initial account of structural realism that Ramsey-Sentences were never assumed to be required for expressing the structural realist's position. We have seen that the problems which led to Strong Structural Realism's appeal to Ramsey-Sentences were all focused on the notion of structure—is it abstract or concrete? Is it genuinely retained across theories? Etc. But the clarity and rigor that comes with formally representing in a Ramsey-Sentence the distinction between what one takes to be theoretical versus non-theoretical in the axioms of a theory has not helped us to delineate a successful form of structural realism. Why might that be?

Well, when you think about what would provide an adequate response to the PMI by a structural realist, there are several places we might start when looking for an answer. We know we're looking for that thread through science, but how should we go about finding it? Perhaps the thing to do, like Worrall, is to start with an empiricist position and work towards a realist view by adding to the empirical basis. In his case this started with formal structure beyond the observable, which was justified on the strong account by appeals to continuity, correspondence and unity. Another approach is to adopt a relativized view to the terms we use in our theories, like Cruse and Papineau, and argue that relativizing our theories based on some primitive notion of understanding will enable us to retain a realist view of the world.

But notice how different these two approaches are. In the former we look to the world to provide us with grounds for making an *epistemic* distinction between what we are and are not justified in believing. In the latter we are making a *semantic* distinction between the terms that we use in our theories. No wonder these views fall wide of the mark. What the structural realist needs is an epistemic distinction, and if he wants to apply that to Ramsey-Sentences then that distinction must be cashed-out in semantic terms. The Ramsey-Sentence approach licenses us to expel certain kinds of *terms* based on prior theory, and although we can imagine a Structural Realism that is similarly relativized, permitting inferences only to previously epistemically acceptable *properties*, it seems the two will be difficult to reconcile since epistemic justifications are very much more demanding than semantic definitions. To bring the two approaches into line would require we show that our notion of what is 'antecedently understood' is coextensive with all that we are justified in believing, not merely defining. Can this actually be done in a way that will answer the PMI?

I think we have good reason to be suspicious of this required cooperation because I don't believe the epistemic justifications we're looking for will support a single distinction between theoretical and non-theoretical terms across the board for all cases of Ramsification. Let me explain this.

Let's start with the structuralist who looks to the world and sees observable objects as entirely unproblematic for his ontology. So far he is in the empiricist's position, and hence there is no problem with his point of departure. Now he looks to the unobservable world and says there is structure there that describes the relational properties of objects, but he has no desire to posit these objects as real. The relations are real enough, but he wishes to go no further. Here though is the rub. The structural realist needs to believe in *observable* predicates that are true of *unobservable* entities, but he can't commit to those entities. The Ramsey-Sentence Realist only has to take these unproblematic observation predicates and apply them to problematic theoretical entities, so defining a new property that is now going to be taken as non-theoretical in a successor theory.

Take for example the unproblematic observable predicates like ‘wave’, ‘oscillation’, ‘vibration’, ‘orthogonal’, ‘direction of propagation’. These seem perfectly harmless since they are indisputably observable terms that we apply to the world around us all the time. Now with Fresnel’s theory the Ramsey-Sentence Realist is able to keep all of these observation terms as they are when quantifying over theoretical terms in the axioms of the theory. Hence, the Ramsey-Sentence will appeal to light being a ‘wave-like vibration in a substance that oscillates orthogonally to its direction of propagation’. For this approach the problem lies in whether we can treat ethereal substances as elastic solids. If the Ramsey-Sentence of the successor theory, and in this case it is Maxwell’s, was to posit a similar elastic-solid substance, then the ether would have become a non-theoretical notion. Of course it didn’t, so the preserved terms were coincident with our modern view of the phenomena. However, things could have been different, and the ether may have been retained. Nothing would have been illegitimate about the Ramsey-Sentence Realist committing to such new terminology, because he shouldn’t think he can answer an epistemic question with a theory of how to define theoretical terms.

More importantly, the Ramsey-Sentence Realist strips away a certain *type* of term when he Ramsifies a theory, and which terms he treats as theoretical is dictated by prior theory. Consequently, whether it is legitimate for one to treat terms of the type ‘optical ether’ as old rather than new, (i.e. as non-theoretical), is a universal dictate across all of our theories at any given point in time. More precisely, when the Ramsey-Sentence Realist finds a term has made the transition from new to old, that term becomes old across all Ramsification processes regardless of the theory being addressed. This means the relativized position takes old terms to be old throughout science, not just relative to some particular theory or experimental procedure. Optical ether, if it were to have made the transition from new to old, would have been considered old for all theories in science. This is what happened to the modern theory of the atom. The initial theories of the atom were inadequate, but once an acceptable definition came along we could treat the notion as non-theoretical. This doesn't mean the concept 'atom' becomes static—refinements to the definition in other theories are acceptable, but the theory from which the term originated has not changed how it treats the term itself. So, once a term becomes non-theoretical, it has fallen prey to the sweeping arm of the progress of science.

This is far from the case with structural realism, for which the license to treat a term as non-theoretical varies from theory to theory and between experimental situations, depending on standards of evidence and available observational data. From the structural realist's perspective the problem is not one of specifying the definition for a theoretical term, it is figuring out if those terms denote entities that we are justified in believing exist. This is not a process that often results in a single term becoming *uniformly* treated as non-theoretical across all science.

To unpack the difference between the positions I would suggest that since the Strong version of structural realism fell victim to numerous difficulties above, we should move on to consider a slightly weaker version. Imagine we take a step beyond accepting only abstract structure, and we say that 'Weak Structural Realism' is the position that accepts not just the purely mathematical relations we find retained across theory transitions, but also those *minimal interpretations of the relations which are* *necessary to generate successful predictions for our theories*. That is, instead of restricting our notion of structure to purely abstract mathematical relations, we extend our position to accept the very least in metaphysical assumptions we can get away with in order to generate observable predictions. For example, instead of treating structure in terms of Fresnel's equations, we take these equations to represent something *physical* that has a wave-like motion which oscillates perpendicular to the direction of motion of light propagation. We don't commit to this physical stuff being anything in particular, least of all an ethereal medium, but we do commit to some minimal physical *properties* necessary to say what we should expect to see in an experimental result.

This position gives critics of Structural Realism an opportunity to burden us with the PMI of course, and I will consider whether this weaker version of structure can escape this criticism in chapter five. For now, let's be charitable, since our task is to distinguish structural realism from Ramsey-Sentence Realism, and assume that at least something along these lines might work.

Weak Structural Realism then is committed to some minimal physical interpretation of the retained structure across theory transitions for mature sciences, and this commitment is to particular properties that objects may have. Although it is perhaps debatable in each case exactly what physical assumptions are minimally required to derive an observable experimental outcome, we should regardless be able to see that these minimal physical commitments will vary across scientific disciplines, and even sub-disciplines. There will not always be some uniformly accepted evidence that justifies our acceptance of some set of properties that define an entity. Take for example the difficulties in defining the ‘nature’ of light. We have been dealing with the Fresnel-Maxwell transition, which treats light as a wave phenomenon. There were in the 19th century plenty of evidential reasons to think of light as being wave-like, including of course Fresnel’s prediction of the Poisson white spot, which relies upon the interference of light waves. On the other hand, there were also plenty of reasons to believe light was corpuscular in nature, and there still are, such as the photoelectric effect.

In each case it is not that we can’t decide whether light is constituted by waves or by particles, and hence that we shouldn’t yet treat it as a non-theoretical entity. It is rather that light has a dual nature, and how one treats light very much depends upon the experiment one is performing. The same is true of electrons, which behave as particles in a cathode ray tube, but interfere when fired through a double-slit apparatus. In these cases, it is not legitimate to take a term like ‘photon’ or ‘electron’ and treat it as having a consistent set of properties. That is not to say, however, that we can’t treat these entities as non-theoretical for some particular experimental set-up, because in some circumstances we have good reason to believe that we’ve hit on the right properties for an entity.

This extends beyond dual natured entities to include for example the vortex theory of light. As we saw in chapter two, the use of vortices was taken very seriously by Fresnel, Green, McCullugh, Stokes, and many others; even Maxwell remained throughout his career committed to the idea of some mechanical means by which to explain the electromagnetic field. We have seen these physicists treating vortices as rotations in an ethereal elastic-solid medium of some kind. For both the Ramsey-

Sentence Realist and the Weak Structural Realist the notion of a vortex poses no problem, however, where the former can treat the notion as non-theoretical and hence retain it in a Ramsey-Sentence for a vortex theory for electromagnetic phenomena, the weak structural realist will be unwilling to include it in his theory. Properties like angular momentum, or magnetic moment, might be acceptable to the structuralist, but just because an everyday notion like 'vortex' is easily defined, this does not permit us to suggest that it exists in some ethereal elastic-solid medium.

Similarly, we don't want to treat the medium itself as a retained component in the history of science, but we most certainly do want to retain the idea that there is an elastic restoring force present. It was entirely in virtue of this notion of a restoring force acting on electric particles that led Maxwell to see the displacement current as an electric current. So, when interpreting Maxwell's equations, the weak structural realist will want to retain the notion of some kind of restoring force acting in space and over time. Importantly, he will have to commit to the notion of something having electric charge (not wanting to commit to electric particles of an ether), perhaps a field, that is caused to release a build-up of potential kinetic energy when a changing electric current is present. On the weak structural realist account therefore, the equations Maxwell produced will have to include the notions of electric charge, potential energy, and displacement. None of these are problematic for the Ramsey-Sentence realist because they are defined in prior theories, but the weak structural realist has to see that they are absolutely necessary to interpreting the equations of Maxwell's theory before he can go treating them as fictions.

These examples should make clear that not only do the Ramsey-Sentence Realist and the Weak Structural Realist have very different criteria for when we can accept or reject a certain kind of term in our theories, it is not even clear that they will be capable of matching their approaches when using Ramsey-Sentences for different theories that use the same terms. This consequence certainly seems to follow from the simple fact that when Ramsifying either Fresnel's or Maxwell's theories the Ramsey-Sentence Realist will be justified in treating all seemingly non-theoretical predicates like 'force' and 'perpendicular oscillation' indiscriminately, whereas the weak structural realist will be willing to apply them only to the equations as is absolutely necessary. The weak structural realist would certainly not wish to apply these terms to some ethereal medium, which would appear to be an easily definable term (and hence available to the Ramsey-Sentence Realist) and yet totally unjustified for the structuralist.

I would suggest as a consequence of these arguments that the structural realist may want to be careful when appealing to the Ramsey-Sentence formulation of a theory as a means to clarify his position. It seems to me that this can be a useful task perhaps when one has already a very clear idea, and supporting arguments, for where to draw the line between what we can treat as theoretical (not yet worthy of belief) and that which we think is epistemically well grounded. Of course, if the structuralist is able to do this while avoiding the PMI then he has no need for appealing to Ramsey-Sentences in the first place—they are just a useful means of presenting a theory in a particularly clear and rigorous way. On the other hand, we have seen little in the way of success in achieving this goal by either form of structural realism or by Ramsey-Sentence Realism.

3.5 Conclusion

It should now be clear that Ramsey-Sentences are merely a means of representing the axiomatized versions of our theories. If there are epistemically problematic theoretical notions buried in these axioms, then the process of building their Ramsey-Sentence may be very useful for uncovering them. It is not, however, going to prove a useful means by which we can answer the PMI. Such an achievement must be won on epistemic grounds, not semantic grounds. In this chapter we have seen how the strong and weak views on structural realism diverge from the position known as Ramsey-Sentence Realism. Although the Newman problem may have been avoided by appealing to observable properties as non-theoretical, this did not remove the challenge of triviality for the strong view. The weak form of structural realism clearly will avoid the triviality objection, but it remains to be seen whether this can be done while simultaneously avoiding the PMI.

Chapter 4

Ontic Structural Realism, Semirealism, and Eclectic Realism

We have seen that structural realism in its Ramsey-Sentence form has failed to pick-out that thread of retained structure across the history of science which is required to answer the PMI. In this chapter we consider other forms of structural realism, called Ontic Structural Realism (OSR), Semirealism, and Eclectic Realism, all of which are attempts to circumvent some of the problems of the approaches we examined in chapter three. The ontic structural realism we'll look at has been advocated by James Ladyman and Stephen French in a number of recent papers, and is supposed to provide a tangible alternative to the epistemic structural realism that has been our focus so far. Semirealism, advocated by Anjan Chakravarty, is a compromise between OSR and the weak version of Epistemic Realism, and Eclectic Realism is Juha Saatsi's adaptation of Semirealism. I start by analyzing the strong form of OSR, and argue both that it is incapable of representing the required retained structure across theory transitions, and that it fails to satisfy the required explanatory component which licenses the realist's appeal to truth. I will then consider a form of structural realism closely related to OSR in its adoption of the semantic view of theories, but which goes beyond the purely structural approach. This position is an improvement, but seems unnecessarily tied to the semantic view of theories, and it fails to adequately motivate the drastic revision in our usual fundamental ontology that is required for the view to work. I move on to consider Semirealism, and its close relative, Eclectic realism, and argue that although these accounts are by far the best attempts at justifying the adoption of structural realism, they too ultimately fall short. By again looking to the history of science to evaluate our philosophical accounts, I argue that none of these positions is capable of adequately answering the PMI.

4.1 Ontic Structural Realism

The versions of structural realism examined in chapter three were epistemic in nature; they were attempts to draw an epistemic line which dictates where we can justifiably claim to have knowledge of the unobservable structure of the world. However, when Worrall first advocated his structural realism, it was met by James Ladyman with a response one might not initially expect. Ladyman saw an ambiguity in Worrall's position, asking, "There is a fundamental question about the nature of structural realism that should be answered: is it metaphysics or epistemology?"[1] He was highlighting the fact that when we say structural realism ought to be concerned only with relations and properties we could abandon not merely the possibility of knowing theoretical entities, but we have open to us the option that they don't even exist! That is, perhaps structural realism is better interpreted as a position which rejects all talk of entities in anything but a pragmatic, heuristic sense, and holds that at bottom all there is are structural relations without any relata.

One's initial response to this suggestion might question the coherence of this position. After all, on the one hand it seems obvious, if only by definition, that relations hold between relata, so how can we have sets of relations without any objects between which they stand? On the other hand, why would dissolving objects into

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[1] Ladyman, (1998, p. 410)

structures answer the PMI, which seems to require that we avoid the positing of erroneous (false) theoretical properties, not merely the objects on which they hang?

Well the motivations are several-fold. First of all, we have seen how epistemic versions of structural realism seem to have fallen afoul of the Newman objection, so perhaps by appealing to relations as ontologically primitive we can avoid the difficulty of generating trivial isomorphic structures by simply populating a theory's domain with the correct number of objects. Second, there has been no satisfactory resolution to the problem of distinguishing between the nature and the structure of an entity. By dissolving the existence of entities into nothing more than relations it might seem possible to answer this distinction by rejecting the dichotomy in the first place. Third, the history of science may appear to retain limiting-case correspondence relations between precursor and successor theories, but we've seen that this often involves a transition of logical form and as such is far more problematic than first thought.[2] The differences in logical structure for the similar components of successive theories may be explained by the similarity of specific relations, rather than having to commit to objects which fall into one logical category or another. Fourth, OSR is motivated by problems of individuality in quantum mechanics and the need for a representational method that can accommodate models in modern physics. That is, not only does OSR provide a response to the PMI, but it also supports an answer to a fundamental underdetermination argument from the philosophy of physics. That Ladyman intends his approach to answer the PMI can be drawn from his comments:

[2] For a reminder, see chapter three.

> According to Zahar (1994 p.14) the continuity in science is in the *intension* not the *extension* of its concepts. Perhaps, if we are to believe that the mathematical structure of theories is what is important, then as Zahar suggests, we need a different semantics for theories: one that addresses the representative role of mathematics directly. The advantage of adopting such a view is that we would then be content with the continuity of mathematical structure that is found even between theories that differ radically if taken realistically, and so would not be confounded by theory change. This would seem to entail a corresponding shift from a metaphysics of objects, properties, and relations, to one that takes structure as primitive.[3]

Here Ladyman is concerned with the third motivation listed above, the problem we ran into in the last chapter for the syntactic approach where appeal was made to the Correspondence Principle: there does seem to be significant formal correspondence between the mathematical structure of theories in the history of science, but frequently the logical type of relata are divergent. As we saw in chapter three, one strong example of this difficulty is that, although Ehrenfest's theorem is clearly quite similar to the classical force law **F** = m**a**, the former has expectation values for Hermitian operators as its arguments, whereas the classical case has continuous real variables. If we were to move away from traditional semantics and towards a picture that cares only about continuity of mathematical structures then, Ladyman argues, we could avoid this problem. The picture here would put the mathematics in an ontologically privileged position, over and above that of objects, properties and relations. This peculiar move has further implications, in particular in a move away from the syntactic approach to theories, with which we have so far been concerned:

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[3] Ladyman (1998, p. 418)

> Hence, the debate about how to characterize theories and their structure is of central concern for the structural realist…The alternative 'semantic' or 'model-theoretic' approach to theories, which is to be preferred on independent grounds, is particularly appropriate for the structural realist. That is because the semantic approach itself contains an emphasis on *structures*.[4]

By appealing to the semantic view, the ontic structural realist is taking advantage of a way of characterizing scientific theories that is not hostage to theories of reference. This is because on at least some forms of the approach theories consist of collections of models, and models cannot be true or false. They can mimic reality to one degree or another, but they are not truth-valuable. As such, using models rather than statements to represent the world is supposed to avoid certain semantic difficulties of empty reference faced by the traditional scientific realist. We will take a closer look at Ladyman's use of the semantic approach, and its problems, very shortly, but before doing so let's briefly consider the fourth motivation—the argument in favor of ontic structural realist from underdetermination in quantum mechanics.

The basic idea here is that there exists a sort of metaphysical underdetermination problem in quantum mechanics; quantum particles can be interpreted as either individuals or non-individuals. The issue originates with the fact that in quantum statistics the permutation of indistinguishable particles does not give rise to a new state. For example, take two particles 1 and 2, and two sections of space A and B, and apply the condition that each particle has to be in either A or B. In classical statistics there are four possibilities: 1 and 2 are in A, 1 and 2 are in B, 1 is in

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[4] *ibid.* p. 416

A and 2 is in B, 1 is in B and 2 is in A. With each of these four options being equally probable we can see clearly that each outcome has a probability of ¼. However, in quantum (Fermi-Dirac) statistics for fermions the last two states are counted as the same state, and hence there are only three possible outcomes, each with a probability of ⅓. In fact, problems are compounded by combining this situation with the Principle of the Identity of Indiscernibles: if two substances resemble one another in all respects then they are the same individual. This principle can be taken in a strong or weak form depending on whether one includes the property of space-time location, but from either reading we should be able to appreciate the following underdetermination problem: Assuming quantum mechanics is complete, either the Principle of the Identity of Indiscernibles is false because quantum particles are individuals and yet can share all of the same properties, or quantum particles are not individuals, and the Principle is irrelevant. The trouble is that we have no way of distinguishing between these two alternative metaphysical pictures.

In response to this problem Ladyman suggests the following remedy:

> We need to recognize the failure of our best theories to determine even the most fundamental ontological characteristic of the purported entities they feature. It is an *ersatz* form of realism that recommends belief in the existence of entities that have such ambiguous metaphysical status. What is required is a shift to a different ontological basis altogether, one for which questions of individuality simply do not arise. Perhaps we should view the individuals and nonindividuals packages, like particle and field pictures, as different *representations* of the same structure.[5]

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[5] Ladyman (1998, p. 420)

He goes on to say that, “This means taking structure to be primitive and ontologically subsistent” and that “Objects are to be picked out by individuating invariants with respect to the transformations relevant to the context. Thus, on this view, elementary particles are just sets of quantities that are invariant under the symmetry groups of particles physics.”

By adopting this solution to both this problem and to the PMI, Ladyman concludes:

> So we should seek to elaborate structural realism in such a way that it can diffuse the problems of traditional realism, with respect to both theory change and underdetermination. This means taking structure to be primitive and ontologically subsistent.[6]

We see in OSR therefore, an attempt to transform the debate. By appealing to a change in the primitive nature of the world’s ontology, and by moving to a semantic view of theories, OSR is supposed to answer at least two specific problems, quantum underdetermination and the PMI. I will avoid discussion of OSR as an answer to the former issue, focusing instead on how structure is supposed to be retained across theory transitions on this view. This is supposed to be achieved by illustrating how structure is preserved through models of successive theories, and hence depends on looking to how theories are represented on the semantic view. Consequently, to analyze this position we’ll now have to look briefly at how Ladyman has adopted Stephen French’s Partial Structures account of the semantic view.

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[6] *ibid.* p. 420

In several recent papers French and Ladyman have argued that we ought to adopt a variety of the semantic view of scientific theories originally inspired by the work of Patrick Suppes. On this account, scientific theories are best represented as partial structures, which are set-theoretic models. These structures contain partial relations which themselves represent our current epistemic state—some relations we may claim to know hold in the world, others we may be ignorant of, and yet others we may claim do not hold in the world at all. Let's flesh this out a little.

On their account, each model is characterized as a set-theoretic structure with two elements, $\mathbf{S} = \langle D, R \rangle$, where D represents a collection of individuals in the model, and R is a collection of relations that hold between members of D. Each R is actually only a partial relation because $R = \{R_1, R_2, R_3\}$ where R_1 is the set of n-tuples for which the relation R holds, R_2 is the set for which it does not hold, and R_3 is the set for which it is not specified whether R holds or not. It is because R_3 is a partial relation that this is called a partial structure.

There are likely to be many relations that hold for the members of D, and so we represent each kind of relation as R_i. Thus, we represent the relation R_i with the value 'true' for some object in D as R_{i1}. These relations are specified extensionally, just as is the domain, so the relation R_{i1} will actually consist of an n-tuple of objects. For example, if R_{i1} is the 'successor' relation and the positive integers are the domain D, then the extension of this relation would be the set of ordered pairs: $\{\langle 1,2 \rangle, \langle 2,3 \rangle, \langle 3,4 \rangle, \ldots\}$. Similarly, R_{i2} will consist of all those objects for which we specify R_i does not hold. Those objects in D for whom we do not specify if R_i holds or not constitute the extension of R_{i3}. This group may reflect our ignorance, or we may want to place objects here for other reasons (perhaps we idealize or abstract away certain objects in our domain pragmatically). If R_{i3} is empty, then we know whether R_i holds or does not hold for every member of D. In a case where all of the relations have the empty set as the extension for R_3 we no longer have a partial structure because we no longer have any partial relations. This scenario reduces to the more traditional notion of a 'full structure'. From this we can see that for a partial structure, **S**, each R_i is actually an ordered triple $R_i=\langle R_{i1,} R_{i2}, R_{i3}\rangle$, where $R_{i1,} R_{i2}, R_{i3}$, are mutually disjoint sets, with $R_{i1} \cup R_{i2} \cup R_{i3} = D^n$.

The set of partial structures representing a theory are ranked in a hierarchy. This hierarchy can be analogized to something like a ladder. The bottom rung can be thought of as the lowliest partial structure, which is usually going to be a representation of a data model of some phenomenon in the world. From there we move upward using partial structures for each rung until we reach the model of our theory that represents the most fundamental level of partial structures at the top of our ladder. These 'high-level' structures are very general and are usually schematic in form.

The relations between structures on our ladder are generated in two ways. From our lowly data model structure, we can raise ourselves up to a more 'abstract' structure by a process of either *idealization* or *abstraction*. In the former case we are acknowledging that our model of the real world system of interest is using a false value for some variable. For example, in a classical particle system resembling a gas we might assume our objects to be perfectly elastic solid spheres even though we know quite well they are not. This might be for ease of computation. On the other hand, we might use abstraction as a process of eliminating parts of the system we don't think are relevant to our interests, such as dust particles.

French and Ladyman claim that these interrelationships between models can be captured logically within the framework of partial structures by using partial isomorphisms. They suggest that a function (*f*) between two partial structures, **S** and **S'**, is a partial isomorphism iff:

1. *f* is bijective and
2. For all x and y in D, (R_{i1} xy) iff ($R'_{i1}f(x)f(y)$), and (R_{i2} xy) iff ($R'_{i2}f(x)f(y)$) where R_{i1} is the *i*'th relation.[7]

Now each transition up or down the hierarchy for any given theory can be represented in a formal and rigorous manner. Furthermore, partial isomorphism doesn't just connect partial structures within a given theory, it can also capture the correspondence between models in different theories, and hence accommodate theory change diachronically. That is, we can supposedly capture the retained structure across theory transitions by tracking the partial isomorphisms between structures in the hierarchies of the respective theories. In this way even the radical theoretical discontinuities raised by the PMI, such as that from classical to quantum mechanics, or from our example of Fresnel's ether to Maxwell's electromagnetic theory, can be captured under this account by partial isomorphisms between the structures that represent models of our theories. That is to say, for every partial model M_1 of a predecessor theory T_1 we should be able to find an isomorphism with a substructure in a partial model M_2 of our successor theory T_2. This should apply to all problematic

[7] Beuno, French, Ladyman (2002, p.499)

theory changes that the realist must accommodate. Some examples from French and Ladyman include the preservation of structure between quantum electrodynamics and quantum chromodynamics[8], between Bose-Einstein statistics and superfluidity[9] and between the Block-Landau and the London-London models for superconductivity[10].

It seems less helpful for our purposes to address directly any of the above mentioned cases since we are worried about the PMI, and as such should first and foremost deal with those tricky cases for the realist rather than historical cases from modern physics where it is perhaps debatable whether we are dealing with a mature and successful theory. That's why we'll try to reconstruct through the partial structures account the example we've been working with from chapter two; Fresnel's optical theory and its retained structure in Maxwell's electrodynamics. This account is going to be fairly brief, giving what I take to be the essential outline of the transition in partial structure terms, and furthermore, it is an approximation to the French and Ladyman account because we are constructing it here for ourselves—it is not to be found in their own writings.

If we begin with what might go into each level or structure in the hierarchy when we reconstruct Fresnel's theory, one useful place to start is with Suppes' characterization of what each level might include—that is after all where French and Ladyman derive their picture of theories. Suppes takes the hierarchy of structures to include at the bottom a theory of experimental design and a theory of the models of data. Note that actually the experimental design is not formally represented because it

[8] DaCosta and French (2003, p. 123)
[9] Beuno, French, Ladyman (2002)
[10] French and Ladyman (1997)

does not have a collection of models. Also important at this first level, the data models do not represent direct observation reports of empirical phenomena. That is because these models are informed by our models of the experiments we consider. Next up the hierarchy come the models of the experiments. The next level is composed of models of the mathematical structure of the theory itself, and at the final level (or higher) the theory is represented in uninterpreted structural terms. This seems to fit the pattern above.

In the Fresnel case we might expect to find the experimental design and models of the data. What this might mean could be related to the specific case described in chapter two. Take a beam of light traveling through free space and incident on a flat surface of a glass prism. Use a method by which one can determine with accuracy the precise angles of reflection and refraction of the resultant beams. Also use a precise method by which one can determine the intensity of the light of these resultant beams—a variety of photometric device. The concerns about experimental design here are many, but even simple impediments to success (inaccuracy in the measuring devices, minor defects in the smoothness of the glass, polarization of the light beam, significant fluctuations in air density, or perhaps imperfections in the prism) can play an important role at this level. Most other issues over experimental design can be formally represented (like randomization of spatial orientation, or of time of day/month/year). These seem to be rather minor in this case.

The models of the data, just as with the models of the experiment, are somewhat peripheral to our purposes here, and because of their complex nature I shall simply move on to consider the models of the theory as a mathematico-physical structure. That is, the lower levels of structure require detailed theories of fit between data accumulated from experimental trials, and the phenomena itself. We neither have such models for Fresnel's theory, nor is it clear that constructing them here will help our investigation.

Let's just consider then the way one might characterize Fresnel's main achievement, the derivation of his Sine and Tangent laws. In chapter two we saw that in its final form of 1823 Fresnel's derivation proceeded without reliance on elastic collisions of ether molecules, but instead appealed to both energy conservation and a continuity constraint on the medium at the interface—a boundary condition which said that the components of the oscillation that are parallel to the interface must be continuous with it. What this amounts to is actually quite complicated, but we can take this continuity equation to be for some quantity E, which is proportional to the product of an incident light beam's intensity, I, the area it sweeps out in a given period of time, a, and the refractive index squared, N^2, of the medium it is passing through:

$$E \propto I*a*N^2$$

The continuity equation itself is just:

$$E_0 = E_1 + E_2$$

Together these can be used to derive the energy equation :

$$\sin(e)\sin(i)(1- v^2) = \sin(i)\cos(e)u^2$$

Where, recall, e is the angle of refraction, i is the angle of incidence, $v = \mathrm{V}\sin(i - e) / \sin(i + e)$, $u = \mathrm{V}\cos(i)/r\cos(e) * \sin(2i)/\sin(i + e)$, and V is the amplitude of the incident ray.

Fresnel derived his final laws by combining the above relations with another continuity equation, which he took to hold at the boundary between the media. This equation was for the vector components of the amplitudes of light in the parallel or perpendicular orientation relative to the plane of incidence—a 'no slip' assumption for the components which said the velocity of the wave incident to the boundary should be summed with that reflected from the boundary to provide the value of the refracted component:

$A_{incident} + A_{reflected} = A_{refracted}$

Now without the details of how Fresnel himself thought of representing his theory, the partial structures account would take all of the above relations to be capable of being characterized by a structure, $S = \langle D, R_i, f_j, a_k \rangle_{i \in I, j \in J, k \in K}$ where the domain, D, is a set of individuals. The relations and functions are captured extensionally as being n-tuples of members of D, and because they are partial relations, capture an important element of science—that of heuristics and analogies. That is, where Fresnel may have assumed his theory to have been about ethereal molecules with mass that oscillate in a manner which indicates they are gaining and losing kinetic and potential energy, (and consequently momentum), these relations are captured by assigning members to the relevant relations' R_1 set. If one were not assuming the ether to actually consist of such entities, instead treating such mechanical notions heuristically, the relations reflecting their existence would all be R_3. If the molecules were taken in the theory to definitely be non-existent, then these properties would fall under R_2.

In Fresnel's theory the structure would include in R_1 or F_1 sets, at least the following assumptions:

1. The optical ether is comprised of molecules.
2. The ether molecules have mass.
3. In oscillations the molecules gain and lose kinetic energy, potential energy, and momentum.
4. Velocity of oscillation of the molecules is directly proportional to the amplitude of light waves.
5. Amplitude of light waves is proportional to the square root of their intensity.
6. Collisions between molecules is elastic.
7. Energy and momentum are conserved quantities.
8. Ether oscillations are transverse in nature.
9. Continuity of quantity holds for components of oscillation parallel and perpendicular to an interface between media.
10. Snell's law.
11. The density ratio of two media are proportional to the square of their refractive indices.
12. There is no slippage between media—parallel oscillation components are continuous across the boundary.

So, given that these principles are captured in the relations and functions of Fresnel's theory, how do we determine the partial isomorphism between this structure and that of Maxwell's derivation of the Sine and Tangent laws? Well, now we have to look to those relevant relations and functions in Maxwell's theory that are used to derive the equations and look for isomorphisms between partial relations. This isn't so difficult as it may at first seem because we already saw all the relevant relations in chapter two.[11] Take Maxwell't theory to be captured structurally via his equations, but also in this theory for light reflection and refraction amplitudes we incorporate his respective equation for electric wave amplitudes:

$$\boldsymbol{E_0}' = (k_x - k_x'')/ (k_x + k_x'')\boldsymbol{E_0}$$

Which as we saw in chapter two, generates the Sine law.

Of course, with Maxwell many of the assumptions listed above for Fresnel's theory are gone. Use is still made of energy conservation, as well as Snell's law, but aside from that, the modern approach is wave-theoretic rather than a geometric analysis of rays, and rejects most of the problematic ontology of the predecessor theory.

What corresponds structurally between the two theories? Well, of course there is the obvious Sine and Tangent laws themselves. Without them there would be little point in using this example in the first place. But accepting a correspondence of these laws, one which I take to be clearly isomorphic, is there anything else? Well that very much depends on the notion of partial isomorphism, which is supposed to provide us with the appropriate links between theories. According to the definition of partial isomorphism provided by French and Ladyman in early work, which is that given above, the correspondence between these theories should be that given by the partial relations in each structure which stand in full isomorphism to those in the other. But this is overly restrictive because although we have a full isomorphism between Sine

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[11] See specifically pp. 9-10.

and Tangent law for these theories, this is not the case in other theory transitions. At best, in most cases of theory transition all one can hope for is a limiting case correspondence, and as we have seen above, this brings with it a difference in logical form of the arguments—a problem the semantic approach was supposed to avoid, but which full isomorphism fails to solve. French and Ladyman recognize that their early definition would yield just this kind of overly restrictive account for most theory change, and hence weakened the notion to that of partial homomorphism:

> Let $S = \langle D, R_i \rangle_{i \in I}$, and $S' = \langle D', R'_i \rangle_{i \in I}$ be partial structures. So, each R_i is of the form $\langle R_1, R_2, R_3, \rangle$, and each R'_i is of the form $\langle R'_1, R'_2, R'_3, \rangle$. We say that f: $D \rightarrow D'$ is a partial homomorphism from S to S' if for every x and every y in D,
>
> (i) $R_1xy \rightarrow R'_1 f(x)f(y)$,
>
> (ii) $R_2xy \rightarrow R'_2 f(x)f(y)$[12]

With a partial homomorphism the cardinality of the domains of the predecessor and successor theories do not have to correspond, and this makes for a much more plausible means of showing formal relationships between non-isomorphic theories than did the requirement of full isomorphism for partial relations.

There are however some problems to be overcome with the partial structures account despite this remedy. Perhaps the most obvious is that we don't yet have a means of distinguishing theoretical from non-theoretical relations for our theories. This was the most significant problem for the Ramsey-Sentence approach considered in chapter three, and without some principled reason to call a relation one or the other, then the partial structures approach seems also to suffer from all the traditional

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[12] Beuno, French, Ladyman (2002, p.503).

problems we've seen advanced against realism. But notice that on the partial structures account we really don't have to worry about what we call theoretical and non-theoretical, since this dichotomy is not being used to differentiate an epistemic line of any significance. On this approach what we care about are retained relations, and for the ontic position relations are all that exist in the world, and consequently, the relevant question to ask is really: What picks-out the *correct* relations that are retained across theoretical transitions? We can see the Sine and Tangent laws of reflection and refraction of light are preserved for our example, but this doesn't provide a *principled* means with which to determine what structure to retain as reflecting the objective nature of reality, rather than perhaps what to discard as 'excess structure'. That this is an important problem to avoid can be seen from the mere possibility that we can find not merely partial homomorphisms but even partial isomorphisms all over the place in historical transitions from one theory to the next. These morphisms can capture unimportant, or trivial relations between structures that we certainly wouldn't consider sufficient to answer the PMI. For example, if we merely require partial homomorphisms then it looks like Aristotle's 'natural place', Descartes' 'gravitational vortices', and Newton's 'gravitational force', all retained some small *structural* element when each is represented structurally as having relations that indicate the motions of objects toward each other. But this is just the problem that Laudan raised with causal theories of reference which are far too tolerant for the realist's purposes.[13]

In fact things are worse than this, for without additional epistemic constraints all we require in order to satisfy the request for partial homomorphism between

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[13] Laudan (1984, p.160)

structures is a single ‘object’ in the domain, D of structure S and a single object in D’ of S’ to share a single property (monadic relation). Now, this is not going to answer the PMI, and French and Ladyman would never come close to suggesting such an absurdity, but it goes to show that on this account we need further epistemic constraints in order to pull-out enough structure to avoid trivial explanations for the success of science. How else are we to determine minimal epistemic conditions for asserting the existence of real relations?

Ladyman himself recommends that we treat as real, or objective, those sets of relations which are invariant under group transformations:

> The idea then is that we have various representations which may be transformed or translated into one another, and then we have an invariant state under such transformations which represents the objective state of affairs. Representations are extraneous to physical states but they allow our empirical knowledge of them. Objects are picked out by individuating invariants with respect to the transformations relevant to the context. Thus, on this view, elementary particles are just sets of quantities that are invariant under the symmetry groups of particle physics.[14]

Here we have a strong constraint on partial homomorphisms, and one certainly supported by, for example, Weyl’s and von Neumann’s unifications of the Heisenberg/Schrödinger/Dirac early formulations of quantum mechanics, and especially so for the prediction of the positron. However, despite its obvious usefulness in modern physics, this principle seems somewhat restrictive when it comes to science as a whole, and if it is taken to provide the *general* kind of constraint we are looking for, it surely fails.

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[14] Ladyman (1998, p. 421)

Another significant problem for Ladyman's account is that it suffers from explanatory difficulties. The game here remember is to find a *principled* means of selecting-out a thread which runs through the history of science that can explain the success of mature theories, while avoiding their failures. If we take parital homomorphisms between invariant group structures as our constraining principle, it seems difficult to imagine how we can explain the success of any particular theory. The reason for this is that we have not yet seen an argument which justifies the inferential move from the retention of structure to the claim that homomorphic structure is accurately representing the structure of the objective world.

One might wonder why explanatory difficulties should trouble the structuralist, but there has typically been only one successful strategy for inferring from inductive premises to the truth of a theory: explanatory success. By treating successful scientific inferences as prime examples of inference to the best explanation (IBE) reasoning, we have been treating the PMI as providing good reasons to be suspicious of IBE. It might be acceptable to infer to the empirical adequacy of our best explanations, so the argument goes, but it is not so clear we can infer to their truth. By appealing to the preserved structure the structuralist has made a fair attempt at answering the first task in front of him, that of finding a thread through the history of science. But even if that task is accomplished, the second task is to show why it is reasonable to believe that structure is real, and this seems achievable only by showing that structure to be genuinely explanatory.

This puts the structuralist in the position of appealing to the explanatory power of structure. But how can uninterpreted logico-mathematical structure *explain* a theory's success? The trouble for even the partial structures realist is that once one is committed merely to structure, the semantic version of a Newman problem arises: purely structural models are supposed to represent the world, but so long as we have the correct cardinality in the model, we can generate a set of isomorphic, and certainly homomophic, relations which match the world. The ability to generate such models trivializes the partial structures account without a means of showing why we are justified in thinking we have the right relations, and this is why the structuralist relies on the explanatory power of the structure: the right relations are supposed to be those that maximally explain the success of the theory.

How then, can pure structure explain? Perhaps the structuralist could argue that explanations really aim at understanding, and so if we can show how structural explanations generate understanding we can justify the appropriate realist inferences. Such an approach has been advocated by R.I.G. Hughes, who suggest that we might derive understanding from merely displaying the elements of our models and illustrating how they all fit together. He draws an example from special relativity:

> Suppose we were asked to explain why, according to the Special Theory of Relativity (STR), there is one velocity which is invariant across all inertial reference frames…A structural explanation of the invariance would display the models of space-time that STR uses, and the admissible coordinate systems for space-time that STR allows; it would then show that there were pairs of events, e_1, e_2, such that, under all admissible transformations of coordinates, their spatial separation X bore a constant ratio to their temporal separation T, and hence that the velocity X/T of anything moving from e_1 to e_2 would be the same in all coordinate systems. It would also show that only when this ratio has a particular value (call is "*c*") was it invariant under these transformations.[15]

[15] Hughes (1989, p. 256-7)

Now although this example may certainly provide some sense of understanding, we should note a problem with such an account of explanation, for the structuralist at least. The example points out how *c* gets incorporated into the models of STR, but it does not explain *why* it has been so incorporated. The models of space-time being used are mathematical structures that attempt to represent the physical world's space-time, and the transformation operations applied to these models reveal an invariant ratio. This may well explain why STR appeals to that constant, but it doesn't explain why the ratio is what it is, nor how it came to be. For those explanations we'd require a far deeper explanation appealing to the nature of space-time itself. Admittedly, this might be an option to some accounts, but it is not open to the pure structural realist like Ladyman, because he has to avoid interpretations since they posit ontology, which is where the PMI strikes. The nature of space-time is not given in the models, because revealing the nature of space-time itself requires us to interpret the mathematical representation of its models, and doing that is to go beyond providing a structural explanation.

This response is likely to provoke the objection that asking for an account of the nature of space-time, and hence an interpretation of its models, is unnecessary to many who garner what they consider to be 'understanding' from mathematical models. After all, why should we be surprised that a mathematical physicist will claim a deeper 'understanding' of phenomena when given purely mathematical explanations than will someone without extensive mathematical training? The former has well developed mathematical intuitions, honed by years of study in his discipline. The latter lacks such intuitions, and as a consequence is unlikely to claim a sense of understanding from mathematical proofs.

Here we might benefit from Wesley Salmon's work on explanation to clarify the situation. Salmon[16] has very usefully identified three kinds of theories of scientific explanation: Ontic, Epistemic, and Modal. I will here focus on the first type, which attempts to answer what sort or kind of nomic relations have to hold in the world in order to provide an explanation. I draw attention to this category precisely because the structural explanation given by Hughes appeals to the relations between events in models of a theory. His account is of the ontic variety, which as a kind appeals to the relations that hold in the world so as to facilitate understanding of why things happen and why they are the way they are. Hughes thinks the relations we need appeal to can be given merely structurally. Salmon, (as well as many others), on the other hand, selects *causal relations* as crucial. An adequate scientific explanation will provide a series of causal relations, which constitute *causal processes* when connecting events. A sequence of such processes which runs from initial to final state can be considered a *causal mechanism*. Such mechanisms can be used to provide understanding when given in the form of an explanation that connects cause events and effect events. The mechanism fills in any explanatory gaps between events by showing how these events are causally connected.

Now this is just a minimal sketch, aimed at giving a sense of what Salmon saw as important to an explanation, which previous accounts had failed to consider. In particular, this approach goes beyond the D-N covering law account of scientific

[16] See Salmon (1984)

explanation by filling-in all explanatory gaps between initial conditions for a system and the laws it follows. But not all explanations have to be causal-mechanical according to the structural approach. We can perfectly well appeal to the relations between events to capture the sequence of a process without reliance on causal mechanisms. When Hughes talks of ideologies as metaphysical presuppositions he's recommending we can come to understand a phenomenon by seeing how its initial event state is connected to its final event state. In doing so, we are not falling back to the D-N picture, where blind appeal to a law of nature is supposed to provide understanding. Here, we are looking to our models and tracking a process through their inner workings. In this way we can explain not only why there is one velocity invariant across all reference frames for STR, we can also explain such conundrums as the Bell correlations in quantum mechanics.

I disagree. It seems obvious that the very reason so many have a problem with the Bell inequalities is precisely because they appear to force a rejection of causality (assuming locality). If your theory of explanation has already given up on causation, then there's little surprise that you'll be happy to accept explanations of these correlations by merely appealing to the structure of our models for quantum theory. However, aversion to such a theory of explanation presumably stems from an ideology chained to a classical metaphysics, and as such is part of the pragmatics of explanation.

This indicates that structuralists should perhaps for the time being leave aside pragmatics when considering the move from explanation to realism. If the success of an explanation is relative to its audience, which would be a pragmatic issue, we might be better off looking to past cases of supposed successful explanations and inferring some structurally successful principles by which we can reliably explain phenomena in the future.

Another alternative for the structuralist is to give up on the project of retaining a purely structural account, arguing that we do need explanations that are not merely structural, but also not subject to the PMI because they are minimally structural. We have seen in chapter three that going beyond pure structure was a difficult move to defend, but will the partial structures approach provide a more amenable means of achieving the structural realist's compromise? In the next section we look at a softer version of the partial structures view which hopes to accomplish this task.

4.2 Extending Partial-Structures Realism

We have seen above that Ladyman's ontic structural realism, with its adoption of the partial structures framework developed by French, suffers from significant difficulties. First, although partial homomorphisms appear necessary for the required retention of structure, and satisfy our case study of the transition from Fresnel to Maxwell, it is not a sufficient condition to guarantee anything more than a trivial thread across theory transition. This is somewhat akin to the Newman problem for syntactic formulations of structural realism which we ran into in chapter three. Second, attempting to justify the link between structure and the world is a necessary component of the realist account, but so far we have yet to see an unequivocally successful notion of explanation that works for purely structural accounts. In fact this second problem is compounded by the additional concern that if we take a theory to be identical to a collection of models, then it is unclear how we can legitimate realist inferences: belief in a theory is sentential and truth valuable, but models are not.

In an attempt to answer these problems one might try both to reject the idea that theories are identical to collections of models, and soften the structuralist account by appealing to more than mere structure. This is a tack adopted by French and Juha Saatsi in recent work.[17] The first move they make is to argue that it is only a popular misconception that the semantic approach identifies theories with collections of models:

> There is more to the semantic approach than pure logico-mathematical structures; after all, we speak of particular models representing the unobservable world behind *particular* phenomena, we *interpret* theories by *describing* the properties and relations the state variables in a model stand for, etc. As a matter of fact the advocates of the semantic approach have never taken theories to be (with the 'is' of identity) *just* structures, and representation to be *just* a structural relation, in the logico-mathematical sense of structure being determined only up to isomorphism.[18]

This is a considerable concession, for it tells us that whatever theories are (and that is left open on this account) the models of structural realism, be they partial or otherwise, are not equivalent to the theories we are considering, they are representations of these theories. This entails that although we may characterize for example, the Fresnel-Maxwell theory transition in set-theoretic terms, there may be more to it than purely structural relationships like partial isomorphism or homomorphism can capture. This move points to an important distinction being made

[17] French and Saatsi (forthcoming)
[18] *ibid.* p.8

on this extension of partial-structures realism: when we look at the structuralist account of a theory we can view it from two importantly different perspectives; one epistemic, the other representational. When we look to the structural representation of a theory we are observing it from the 'Extrinsic' point of view, and as such we can legitimately ask only after the structure of the theory, how its levels are related, how it stands in relation to other structurally represented theories, etc. The extrinsic perspective is concerned only with characterizing a theory, and does so from outside any particular logico-linguistic framework. Here models are purely representational and we should avoid epistemic questions because they are reserved for the 'Intrinsic' point of view, which reflects our epistemic attitudes about theories and not their representation via models. When we inquire about one another's epistemic view of theories we shift over to the intrinsic characterization, and here we may inquire into the truth value of our descriptions of models, but not the truth or falsity of the models themselves.

But does this really make any progress over the syntactic view of theories that we dealt with in chapter three? What do we gain on this approach if the intrinsic view still commits us to a correspondence view of the relation between world and theory? French replies:

> It is not the case that nothing is gained by such a move, since the realist is not only concerned with truth and correspondence. She is also concerned with theory change and inter-theoretical relations in general and those aspects of scientific practice are better captured by the semantic approach.[19]

[19] *ibid.* p.10-11

We have seen above that it is plausible to think the partial-structures realist can exhibit in his representational framework the minimal relations between set-theoretically characterized theories, but justifying the realist inference and avoiding trivial homomorphisms requires a further epistemic constraint. To fit this bill the account we are considering here suggests something new: underpinning realist inferences with 'localized evidential support' in the form of *abstract principles* that are used to generate successful predictive derivations. It is via such principles that we will be able to pick-out the success-generating theoretical constituents of our theories which explain the respective partial homomorphisms between structures (i.e. retention of structure).

What do these features look like for the Fresnel-Maxwell case? They say,

> The crucial observation is that the equations derived from Maxwell's equations using continuity principles at the diaelectric interface are formally *identical* to those derived by fresnel and his mechanical principles. It turns out that one can derive these equations on the basis of metaphysically minimal premises which assume very little about the properties concerned…the abstract continuity principles fuelling Fresnel's derivation define dispositional descriptions (of properties) that are satisfied by the properties $\boldsymbol{E}$ and $\boldsymbol{B}$ in the solutions of Maxwell's equations. It is these principles describing higher-order properties of $\boldsymbol{E}$ and $\boldsymbol{B}$ which are central to the explanatory endeavour.[20]

I take it that the explanatory continuity principles which are being referred to here are just those used at the boundary between media, and as we saw in chapter two these are respectively for Fresnel the 'no slip' assumption ($A_{\text{incident}} + A_{\text{reflected}} = A_{\text{refracted}}$) and for Maxwell the continuity equation for the electric field ($\boldsymbol{E}_i + \boldsymbol{E}_r = \boldsymbol{E}_t$).

[20] *ibid.* p.12

These explanatory continuity principles are supposed to represent relations between the unobservable properties of light, and consequently are much deeper than Worrall's appeal to mathematical structure, which captures only relations between observable phenomena.

French himself adopts the view that both of these principles provide sufficient explanatory import to justify the realist inference. I am suspicious of this claim, and in the next section, where it will be more appropriate, I argue that they are not sufficient, nor general enough to provide a solution to the problem of answering the PMI. For now it is enough for us to see that French still faces the problem of slipping from structure to nature by taking these relations to inform us about the 'nature' of theoretical posits. That is, by adopting a softer version of structure, French's position faces the problem of collapsing into full-blown realism, just as we saw was problematic for the epistemic versions of structural realism. His way of avoiding this problem is the same as Ladyman's: adopt the view that structures are all that exist, which entails we don't have to worry about commitment to non-existent theoretical *entities*. But doesn't knowledge of the relevant properties reveal the nature of theoretical entities? He puts his response this way,

> If the 'nature' of theoretical posits is cashed out in metaphysical terms—as it should be if standard realism is not to be a kind of 'ersatz' realism (Ladyman 1998)—then the conclusion doesn't follow. If it is not, then 'nature' signifies nothing more than the relevant properties and the conclusion is empty. And in that case the structuralist can agree that the relations tell us something about the relevant properties, understood as aspects of structure.[21]

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[21] *Ibid*.

That is, we can avoid the collapse into believing in theoretical entities when we don't believe in such entities in the first place. In fact this picture extends even to *causal* properties:

> Given the motivation for OSR, there should be no physical properties that cannot be captured in structural terms, since any such property worth its salt, as it were, would feature in the relevant causal-nomological relations and would thus be incorporated into the structural description.[22]

The picture then is that we can go beyond mere formal structure by taking theoretical entities to be reducible to properties (perhaps causal) described by relations, and when we accept that structural descriptions can capture the unobservable via partial structures, we are constrained in our beliefs by abstract continuity principles.

If this is an accurate interpretation of the position, we have indeed come a long way from the previous picture of a purely structural partial-structures realism. We are somewhat limited in how far we can further analyze this position because it very much relies upon a notion of 'abstract principles' to inform its use of explanatory inferences to realism, which is still to be fully worked-out. There is an account provided by Saatsi of how this works for the Fresnel-Maxwell case, which we will look at in the following section, but for now I have just one further criticism of the ontic structuralist approach.

In both of their attempts to avoid the collapse to full-blown realism, French and Ladyman make the mistake of urging upon us a metaphysics for the quantum

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[22] French (2006, p. 174)

world which also solves the realist's epistemic problem of claiming knowledge of the unobservable. This is not their primary motivation for the move to the rejection of traditional ontology—as we've seen the impetus comes from underdetermination arguments in quantum mechanics—but would turn out to have pleasant consequences for defending their realist position. That is, if one takes group invariance under transformation as the primary epistemic constraint on objectivity for a world that is entirely structural, then one has a means of showing how abstract principles can constrain one's epistemic commitments, and hence a way to draw a clear line distinguishing those relations about which we ought to be realist from those we ought to reject.

How might the structuralist motivate a radical change in ontology, not just for the quantum domain, but for all of our theoretical knowledge, such as that being proposed by French? Why think a move away from an object oriented ontology is sufficiently well motivated? Here the focus is on the notion of causation, which OSR has already claimed to accommodate, and in particular how relations and properties can have causal efficacies when treated as free-standing ontological primitives, rather than when attached to objects, as is traditionally taken to be the case. In response, French diverts us back to the quantum case, saying:

> OSR does not advocate the analysis of all macroscopic causal processes in a structuralist fashion—we have to recall the motivation above. In essence the OSR account piggybacks on the physicalist's reduction of such processes in terms of ultimately quantum processes and then insists that the latter have to be understood in structural terms.[23]

[23] French (2006, p.179)

Here then we see clearly the plausibility of the ontic structural realist's position, even this softer version, hanging on the metaphysics of quantum mechanics. If one accepts the primacy of the need to provide a solution to the underdetermination problem mention above, and sees that solution in terms of a structural description of the quantum world, then it might seem plausible to push for a reconceptualization of all physical processes in purely structural terms. However, it is first of all quite plausible to take alternative approaches to solving the quantum indistinguishability problem. One might for example take a Bohmian approach to quantum mechanics and advocate that the problem being addressed is one of our epistemic ignorance as to the state of a system, rather than the more metaphysically unusual position of taking the quantum world not to consist of individuals. This points to the possibility that the ontic structural realist is mistaking an epistemic problem for one that is metaphysical. Even outside of quantum mechanics, why take the need to represent the world in structural terms as an indicator that it must also be structural in ontology?

Additionally, why would one want to privilege the quantum domain over other underdetermination problems in modern physics? By adopting a structuralist stance here, one is forced to treat space-time itself in structural terms—either adopting space-time relationism, or arguing for the reduction of the substantivalist's position to purely structural relations. A substantivalist with an ontological ideology sympathetic to an object-oriented metaphysics would surely see this move as illegitimate and premature.

4.3 Semirealism as a Last Resort?

As we have seen already, the ontic structural realist argues that we can avoid the problem of having only a trivial realism by appealing to more than pure structure preservation between structures, and in particular, arguing that even causal-nomological properties can be captured structurally, yet don't fall to the PMI because they are ultimately reducible to sets of relations. The preserved relations therefore just are causal properties. We also saw however, that hanging one's realist argument on a highly unusual and controversial interpretation of the world's ontology (based on arguments from quantum mechanics) leaves this response requiring further argument.

Moving away from its specifically ontic formulation, we might still find some plausible reasons for making such a move that can be used to defend the structural realist from the PMI. A position of this kind has been advocated by Anjan Chakravartty, and it constitutes what I take to be both the 'weakest' form of structuralism and at the same time the most plausible.

Chakravartty[24] argues for a form of structural realism he calls 'Semirealism', where we view 'structure' as picking out the relations between first order causal properties. That is, he takes structure to be "Something tied to specific kinds of relata and their characteristic relations"[25] which he takes to be identical to the notion of 'concrete' structure we saw defined by Redhead at the beginning of chapter three. In making this move, Chakravartty immediately imposes the condition that two structures

[24] Chakravartty (1998), (2004)
[25] *ibid*, (2004, p. 155)

can only be identical if they share the same relations and first order properties. On this definition, structural realism claims to have substantial knowledge of much more than mere structure, and is therefore able to avoid the Newman objection by asserting that we have access to at least some of the properties belonging to theoretical entities. The reason he claims that we have this access is because he believes "First order properties whose relations comprise these structures are what we might call *causal properties*: those that confer dispositions for relations, and thus dispositions for behaviours on the objects that have them."[26] The reasoning here is that objects interact according to their causal properties. These causal properties confer causal capacities, which are dispositions to behave in certain ways when prompted by other objects with relevant properties. It is the ways in which these dispositions are linked to one another that produces causal activity. So, causation has to do with relations; relations are determined by dispositions, and dispositions are conferred by properties for objects to behave in certain ways.

By taking this approach Chakravartty accepts the conclusion that we cannot separate knowledge of structure from knowledge of nature. The reason for this is that when knowledge of structure contains knowledge of first order properties and their relations, this entails knowledge of specific relations and specific causal properties. As such, this form of structural knowledge entails knowledge of the nature of theoretical objects.

How is this renewed account of structural realism supposed to overcome the PMI and the criticisms leveled at previous accounts? The strategy is similar to those

[26] *ibid*, (1998, p.5)

above; select out structure as the successful part of scientific theories that ought to be retained through theory transition. This is a form of 'preservativist' strategy. The differences between the realist responses lies primarily in the means by which one achieves this preservativist goal. The proposal here is that realists should commit to those properties and relations that are justified by our causal connections to the world, and these occur through our 'detection devices'. Chakravartty says,

> The capacity to distinguish between parts of theories that do and do not warrant belief thus boils down to an ability to distinguish between parts that we have good reason to think concern genuine causal properties, and those about which we are less sure.[27]

To facilitate this process, Chakravartty introduces the distinction between *detection* properties and *auxiliary* properties. The detection properties are those causal properties we have good reason to think we have detected, while the auxiliary properties are other properties we attribute to entities for any of a number of reasons (perhaps to aid our understanding of a possible mechanism). Because our ability to detect properties will change over time auxiliary properties may eventually turn out to be detection properties as science progresses, or they may be rejected as useful fictions. Our ontological commitment with regard to them is withheld; we are realist about the detection properties and agnostic about the auxiliaries. This is clearly a new epistemic line being drawn.

This approach seems plausible, but the question is, How are we supposed to practically demarcate between the two types of properties? Chakravartty's suggestion

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[27] *ibid*, p.10

is that because detection properties are typically captured in our mathematical equations, we ought to identify them by whatever *minimal interpretation* of a theory's equations is necessary to give predictions, or retrodictions, or whatever the work is that they are supposed to be doing. Any properties that go beyond this minimal interpretation are considered auxiliary, and as such not worthy of our epistemic commitments. For example, in the Fresnel to Maxwell case, we should believe in the causal properties that generate relations represented by the equations—*intensities* and *directions of propagation*. These properties did transfer from the latter theory to the former, whereas the notions of ether and electromagnetic field are auxiliary, heuristic devices.

On this account then, it is admitted that not all structures are retained in our modern theories, some inevitably fall by the wayside. This is not a problem because it can be explained by saying that they were not causally connected in the appropriate ways to our practices of detection. We should only expect retention of the structures that are necessary to provide minimal interpretations of mathematical equations used to describe established empirical phenomena.

Still, this notion of minimal interpretations bears great epistemic weight, and in many cases its determination will be very difficult, especially since the meaning of the terms that refer to causal properties and relations will have to carry over into future theories. Chakravartty's solution here is to adopt a causal theory of meaning and reference. But we have seen how such a theory tends to trivialize referential success,[28] so how is this approach going to avoid including way too many theoretical terms that

[28] For examples see chapter one.

turn out to have no referents? The answer lies in adopting a causal-descriptivist account of reference, which will accommodate the refinement of theoretical descriptions through theory improvements. The structural realist must beware of trivializing, and in so doing look to fix reference functionally, through the *specific* causal role of the relations and processes being termed. In this way the realist can give the benefit of the doubt when specific dispositions for relations conferred by particular causal properties are preserved. On this account, successful reference does not necessarily require that the referring expression is true of its referent. Thus, we don't identify Preistley's descriptions of de-phlogisticated air with Laviosier's descriptions of oxygen because, although many of the causal roles they played were the same, the detection properties themselves were not the same.

All of this sounds quite promising. Chakravartty has shown how his proposal tries to overcome the PMI in at least outline form, and it looks far more fruitful than the purely mathematical structuralism found in Worrall (and Zahar). The first of our arguments against that position was, recall, that the retention through theory transition of mathematical structure alone was not enough to answer the PMI because it would still need to be shown that the equations are uniquely responsible for empirical predictions. However, to get such predictions we have to theoretically interpret the equations and adopt auxiliary hypotheses. This is a problem for the pure structural realist, but clearly not for the Semirealist, who takes structure to include much of the causal properties required here.

At least that is how things initially look. Perhaps on closer inspection we will find that the causal properties to which Semirealism is committed are still insufficient to generate the predictive successes that realists say we require for their support. On the account under consideration, we take as real only those properties that we minimally have to commit to in order to generate predictions. But are these minimal properties *really* enough to generate the claimed predictions in case studies?

To this question I would like to defer to a couple of recent arguments which claim that any minimal interpretation, similar to that advocated by Semirealism is insufficient to account for the history of science. The first case comes from Caloric theory, and is argued by Hasok Chang, the second concerns Lorentz's notion of the electron and is argued by Angelo Cei. We'll briefly deal with each in turn to show that mere minimal interpretation is insufficient, and follow this up by arguing that appealing to detection properties to supplement and hence answer this objection, itself is insufficient without a theory of warrant which avoids the problems we found entity realism suffering from in chapter one. In fact even with a theory of warrant the position still falls to the PMI because it asks us to retain structure on first order properties, but this is not what we find retained across the history of science.

First then, consider Laplace's views of the Caloric theory. Here's a historical case study that has caused the realist almost as great a headache as ether theories like Fresnel's optics. Laplace took caloric to be a fluid composed of molecules which repelled one another with a force which was a function of distance, and temperature was defined as the density of free caloric in space. Caloric was used to explain a number of phenomena, including the speed of sound, the ideal gas law and the adiabatic gas law. So successful was Laplace's theory of gas, it was without serious competitor between the early and late 19th century. Since we are looking here at laws, we can clearly fit this picture into a minimal interpretation of structure required by Semirealism. The caloric, as we now know of course, doesn't exist, but seemed a very important part of Laplace's theory:

> What was responsible for Laplace's success? Once again…assumptions about the material nature of caloric played a crucial role…Since Laplace did not know the precise form of the inter-caloric force function, he also needed other assumptions such as the following: the force is negligible at any sensible distances; each molecule in a gas in equilibrium contains the same amount of caloric; in equilibrium the caloric-filled molecules are spherical and stationary; and so on…Could all these premises for Laplace's derivation of the gas laws be understood in any sense as approximately true according to modern theories? If so, I would be very surprised. The Laplacian metaphysics of mutually repelling caloric particles has been completely and unhesitatingly rejected by modern science; the gas laws have now been derived from entirely different assumptions.[29]

Now although this claim doesn't irrefutably show that Semirealism is intractable, I think it reflects the problem that taking the minimal interpretation of the mathematically representable relations necessary to explain some phenomenon, like the ideal gas law, is plausibly insufficient for the case of caloric. Without realistic assumptions about now rejected entities explanations of phenomena like these gas laws would not have been forthcoming.

Another example can be taken from Angelo Cei, who argues that if we look carefully at Lorentz's theoretical derivation of the Zeeman effect, and consequently of the electric charge/mass ratio, we see it relied essentially on the particle being realistically interpreted as a *rigid body*, (because the calculation was supposed to provide an estimate of the size of the particle). Basically here's how the story goes:

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[29] Chang (2002, p. 8)

Lorentz was, at the end of the 19th century, trying to analyze electromagnetic phenomena and this required he have a model of the interaction of ordinary matter with the electric ether. To mediate the gap between electrical and material phenomena he appealed to the notion of a charged particle, which when moved alters the state of the ether. This in turn moves other charged particles. The emission of light was taken to be a result of vibrations of these particles, which can be found in all material bodies. When there is no magnetic field present, these particles behave as harmonic oscillators, oscillating around a central point under the influence of an elastic force. An applied magnetic force changes the pattern of oscillation such that the direction components of oscillation behave peculiarly: for each spatial dimension the particle emits a different light frequency. Since the equations describing this behavior incorporate the charge and mass of the particles, Zeeman was able to show how Lorentz' theory provided an estimate of the charge/mass ratio. This was a new and novel prediction, but depended not merely on the properties of charge and mass, but also on the density of electric charge of the particle. The conservation of charge density is confined to the particle, from which Cei points out, "Notice that the value of *e/m* is interpreted in strict relation with the notion of particle as a rigid body since it is taken to provide an estimate of the size of the particle."[30]

Again, if the analysis given by Cei is correct, and Lorentz' theory had to take seriously the idea that an electron is a rigid body, which we now reject, then there was some structure which even on a minimal interpretation was necessary and yet

[30] Cei (forthcoming, p. 10)

incorrect. It is at least problematic then to adopt a Semirealist structural realism as currently described.

But even if semirealism could give us enough properties to reconstruct classic derivations from the history of science, how do we know we have correctly identified the detection properties that exist in the world when we look to a minimal interpretation of our equations? That is, when a causal connection is established between a measuring device and some entity, the causal properties attributed to that entity are only as specific as the measurement allows. It is surely possible that due to a lack of properties, there will be causal properties posited with no objects upon which to hang them. We will then have to accept the existence of free-floating causal properties as the price for being realists. Surely on this view, we lose any semblance of explanatory power that is supposed to accompany realism.

Well, not quite. This sort of criticism Semirealism is willing to live with because it stipulates an instrumentalism about the entities upon which we hang our properties. If there are not enough causal properties to make sense of an entity, that is perfectly acceptable, since we can still think of there being an object, it's just that we are not warranted in claiming what it is like beyond its possession of detection properties.

On the other hand, if our minimal interpretation of the equations is itself supposed to be the very principle by which we distinguish what is and what is not a detection property, then it seems we will be positing properties just up to that point at which we can derive a prediction from our equations. To the degree that we need objects themselves to accomplish this task we are therefore guaranteed to posit a sufficient number of properties to make sense of the physical process.

The trouble with this circle of epistemic justification is that it too fails to follow the history of science. We have just seen in our examples above from Caloric theory and from Lorentz' theory of the electron that sometimes non-structural entities are required as necessary conditions for a derivation, and as such Semirealism would have only two choices. First, the Semirealist could treat caloric's properties or the rigidity of the electron as detection properties—but then it would face problems from the PMI, since they were lost in the course of scientific progress. The second option for Semirealism is to reject these properties as merely auxiliary and not genuinely necessary for the supposed derivations. Here we have to defer to the historian to inform our analysis, but I suggest that since Chang and Cei are already responding to the preservativist's strategies in these cases, the burden of proof is with the Semirealist. From this analysis I propose we take Semirealism to push *too strong* a principle in its preservativist answer to the PMI.

I also wish to point out that Semirealism is *too weak* to answer the PMI. Just recall the problem of logical structure preservation we already considered in discussing epistemic as well as ontic structural realism: although the form of preserved structure may look similar, frequently we don't even have the right logical form retained. Our example of this problem, which motivated Ladyman to adopt the semantic approach, was that of the transition from classical to quantum mechanics—real valued functions and Hermitian operators can be represented to have similar form, but have radically different logical properties.

The problem for Semirealism then is that given its current formulation it is both too strong and too weak to answer the PMI. Consequently, we might now consider a little more the refinement to structural realism advocated in section 4.2. On this account we adopt a position very much like Semirealism, but instead of appealing to the minimal interpretation of our mathematical equations interpreted as *first order* causal properties, we instead draw the preservative epistemic line at *higher order* properties. That is, instead of taking a minimal reading for causal properties we adopt only those properties that are *multiply instantiable* by first order properties. This is a stance advocated by Saatsi, and in the context of the Fresnel-Maxwell case we have seen that he is suggesting we believe only in the explanatory continuity principles such as Fresnel's 'no slip' assumption ($A_{incident} + A_{reflected} = A_{refracted}$) and Maxwell's continuity equation for the electric field ($\boldsymbol{E}_i + \boldsymbol{E}_r = \boldsymbol{E}_t$). In regards to this notion of explanatory principles he says,

> Outlining what is minimally required to explain a theory's success corresponds to extracting the minimal theoretical explanation of the phenomenon successfully predicted (or accommodated). This minimal scientific explanation is compatible with a multitude of stories about the lower-level facts. This can be understood through *multiple realisability of properties*: the explanatory ingredients are properties identified by their causal-nomological roles, and most (if not all) such properties are *higher-order* multiple realizable in the sense that these properties are instantiated by virtue of having some other lower-order property (or properties) meeting certain specifications, and the higher-order property does not uniquely fix the lower-order one(s).[31]

This appeal to the relationship between lower and higher order properties is in fact an appeal to the metaphysical notion of supervenience, where some higher order property

[31] Saatsi (2005)

is multiply realizable in the modal sense of having different possible realizations. For example, a gas contained in a room may have a temperature of 75° Farenheit, but this property is multiply instantiable because its molecules may have many possible microscopic configurations. Furthermore, this account of minimal commitment, which Saatsi calls 'Eclectic Realism' combines this metaphysical position with a reductivist account of scientific explanation: higher order properties of a system are reducible to lower order properties, but in many different possible realizations. When we explain a phenomenon it may often help us to imagine what the lower level properties might be, but we only need to give an account of the higher order properties in order to satisfy the reductivist account of explanation.

This Eclectic Realist story can be illustrated for the Fresnel case. The density property Q and the vector property A are all that we minimally require for explanation because these are all we minimally require for a derivation of the Sine and Tangent Laws of reflection and refraction of light. The actual properties of light by which these higher order properties are realized are undetermined in the derivations; they may be oscillations of mechanical ether molecules or they may be self-inducing oscillations in the electro-magnetic field. Neither lower level picture matters for the Eclectic Realist because the important thing is that we now have a different notion of explanation with which to advocate the realist inference to truth:

> Once we see this we can change tack just as well: we need not worry about 'ether' being non-referring *exactly because* it is actually *not* a central term in the right explanatory sense! If 'central' *just means* 'denoting an entity the existence of which is required for the minimal realist explanation of success' the same conclusion follows, because the existence of the ether is *not* required in that explanation. What is

> required is that there is a common core of theoretical properties appealed to in both Fresnel's and the corresponding modern day theorising.[32]

Indeed, one cannot deny the important role played in the derivations by both Q and A. But we might be concerned that appealing merely to higher order properties such as these, which have respective continuity equations, is a rather trivial form of realism. Can one really swallow the story that the derivations can be explained (even reductively) in such terms? If not, then why think that a somewhat vague meta-principle, such as 'believe only in the retained higher order properties of our theories' is going to explain not only why we shouldn't be worried about entities like the ether and caloric, but also their explanatory success? To be more specific, it seems that although we can accept the need for these higher order properties of light, they just won't be enough to actually provide a successful explanation for the phenomenon. That is, we could perhaps accept, as the Semirealist does, that we don't need a specific causal mechanism to explain the reflection and refraction properties of light, but we do need at least the notion that propagation is transverse and that there are intensities associated with these waves. Without these notions then Fresnel would never have overcome the problem described in chapter two of accounting for light as a non-longitudinal wave, which we saw was essential for the development of his theory.[33]

In fact we can put this objection into the form of a dilemma. Either we believe in the higher order properties of our theories because they are independently motivated and we have good reasons to think it is they, not their particular lower order

[32] *ibid*. p. 536
[33] See Chapter Two.

instantiations which are empirically justified, or we believe in these higher order properties for some other reason, such as explanatory success. If we believe in them for the former reason, then it is hard to imagine how our confirmation filters up from the empirical level to this higher level of multiply instantiable properties without also confirming their particular realization. That is, when an experimental result confirms a prediction, surely the prediction is made at the level of instantiated, low level properties. Even if our result confirms that we were on the right path with regards to some higher order property, like energy conservation say, we still have confirmation of that principle for the particular system with which we are dealing—perhaps some specific kind of molecules for the evolution of a gas, or maybe some particular kind of material through which light is dispersed. What motivation might we have for ignoring the particulars of a system and attending only to the higher level properties?

If on the other hand we are motivated independently by some alternative criteria, such as explanatory breadth, or perhaps even more suspiciously a desire to solve a general problem in the realism debate, then we fall victim to the problem that we cannot actually practically derive empirical consequences from our theories. It was not possible (*contra* Saatsi) for Fresnel to derive the Sine and Tangent Laws believing only in the respective continuity equation. We saw in chapter two that he began his initial derivation using momentum conservation, which he then confirmed by appealing to energy conservation. More importantly he needed to assume that light was oscillating in a transverse manner in order to eliminate the longitudinal component from his theoretical calculations, so we wouldn't want to say that higher level properties alone are going to explain the empirical success of our theories.

Besides this difficulty, Eclectic Realism still suffers from the problems associated with its less abstract predecessor, Semirealism. First, as indicated above, there are important components in both the caloric and electron case that were non-structural, yet essential to the predictive success of those theories. Second, the continuity of logical form between theory transitions such as that from classical to quantum mechanics cannot be saved on this approach. Any appeal to even more abstract common features of the equations describing the motion of particles, which appears to be where the eclectic realist directs us, would fall afoul of the Newman objection. Here therefore, we must conclude that multiple realizability is yet another inadequate response to the problem, not the solution.

4.4 Conclusion

In this chapter we have considered two forms of OSR within the semantic view of theories called the partial-structures view and two non-ontic positions, Semirealism and Eclectic realism. The strong version of OSR advocated by James Ladyman ran into difficulty constraining the continuities found across theory transitions because partial-homomorphisms generate trivial correspondence between structures. The weaker version of OSR comes closer to solving this problem, but runs into trouble by following the stronger position in advocating the reduction of all physical accounts of theoretical entities to structural relations. We also concluded that both Semirealism and Eclectic realism which reject ontological revisionism while retaining a minimal account of structure and providing a more fully developed picture of how structuralism can explain the success of science, failed to answer the PMI. In the next, and final chapter I argue that the correct response to our analysis over the past two chapters is to conclude that the PMI cannot be solved by the structural realist in any form. We have surveyed the best of the structuralist positions and they have come up short. This however, is far from the end of the story. I also argue that from these most recent attempts to solve the PMI in structural terms we can extract the materials with which to build a local version of scientific realism, which although conceding the PMI as a genuinely successful argument, still can provide us with a substantive albeit minimal form of scientific realism.

Chapter 5

Beyond Structural Realism

Over the past couple of chapters we have seen various means by which structural realists have tried to 'soften' what constitutes the appropriate notion of structure in attempts to avoid several intractable difficulties faced by the strong versions of their position. In this chapter I want to argue for an especially minimal form of realism that bears strong similarities to Semirealism, which I have characterized already as the most plausible structural realist account. The view I advocate is similar to its predecessor in that it adopts the minimalist spirit of Semirealism, endorsing belief in only a very limited number of theoretical constituents. It does this by requiring of a theoretical constituent of some theory not only that we have good reasons to believe we have detected that constituent, but also that we see it retained across some theoretical transitions in the history of science. The reason for this added constraint is that the PMI might be thought to defeat structural realism if we follow the Semirealist and require merely the presence of detection properties—after all requiring only detection of some property can lead us to believe in entities that turn out in the end not actually to have been genuinely detected even though they appeared so at the time. For example, Lavoisier may very well have thought he had good reasons to think he had actually detected caloric, and Lorentz might have similarly believed that the necessity for his assumption of rigidity as a property for the electron entails its existence. We don't want structural realism to be so dependent on local theories of warrant nor for it to be susceptible to these counterexamples. This requires that my account be more conservative in respect to how much theoretical content of our theories it is willing to license we believe.

On the other hand my view is also more liberal than Semirealism because it endorses a far more heterogeneous notion of structure. In my opinion the project of answering the PMI with a *single* notion of structure, as Semirealism attempts to do, is bound to fail because Semirealism is both overly restrictive and doesn't match the historical record. I suggest that we adopt a pluralist notion of structure where we take structure to mean something far more than merely the relations between causal detection properties as captured in our equations. My account demands that we adopt a form of structural realism that reflects more accurately the diversity of correspondence relations between one theory and the next, as manifested in the history of science. In this way we can still answer the PMI, but we do so by focusing more locally on specific theory changes and showing how in those particular cases there was some form of structural correspondence (broadly construed) at least partly responsible for the success of the theory at hand.

My realism can be thought of as being something of a local realism since the heterogeneous nature of the required correspondence relations implies that which types of correspondence we see retained is going to be relative to the particular theories at hand. Consequently, unlike Semirealism, which takes all relations to be first order and mathematically representable, my position is much more context sensitive—in line I believe with the historical record. This local realism also has strong structural content, but recognizes that our realism extends beyond even the soft interpretation of structure given by Semirealism. As such this is not really a form of structural realism at all, but rather a far more metaphysically open position. While abandoning the global position by rejecting a single notion of structure and opting to go local is an important difference between my account and those we have been considering, this local realism still accords great epistemic weight to the history of science, and in particular to the preservation of theoretical components across theory transition—preservativism. Thus, the most significant property of this account is its requirement for some form of correspondence between theories. I will argue that by looking to the history of science we can justify a local form of realism that overcomes skepticism about our best scientific theories, but to do this we must appeal to the correspondence principle (CP). We have seen this principle in use before, but now I wish to show that it only comes in useful for solving realist disputes about *specific* theories by appeal to a pluralist notion of structure. There is no single notion of structure that will do the job we need; we require a collection of notions—a disjunction of kinds of correspondence you might say. In particular, I will explain how there are very many different kinds of correspondence relations between predecessor and successor theories which should play an important role in justifying our appeals to a realist account for specific theories. These relations are divergent—sometimes in theory change we see one particular correspondence relation dominant, other times it is not.

My argument is that when we see some specific kind of correspondence relation maintained across theory change and this relation falls under what we could consider a detection relation, then it shows that there really is some thread of continuity running between theories and that thread reflects the true structure of the world, as the structural realist desires. We should adopt a minimal realist account that requires some correspondence relations and be realist only about those forms of correspondence for the relevant theory. Thus, the presence of correspondence relations between successive theories is almost a necessary condition for belief in some element of a theory (I'll explain why it isn't quite this strong in section 5.6). These relations alone are not however sufficient to justify the realist inference. To fully justify a realist stance towards any given component of a scientific theory we require not just correspondence relations for that component, but also separate epistemic warrant provided by a theory of evidence—this is just the claim that we've detected the relevant component. We will not enter the debate over what constitutes empirical evidence here, or how correspondence relations are supposed to bind with evidential reasons to believe in some particular set of theoretical constituents—it's not even clear that this would be the case. Instead, we will simply start by adopting something similar to the detection/auxiliary properties distinction already relied upon by Semirealism. The difference between my account and Semirealism in regard to such properties is that unlike my predecessor I do not think that detection properties can be determined by the minimal mathematical structure required for theoretical predictions (although like Semirealism, we will also be assuming some causal-descriptivist theory of reference). Whether we have good reasons to think we have detected some theoretical component is a matter that needs to be determined by our theories of evidence, theories of experiment, and theories of instruments for any given empirical situation. These are issues beyond the scope of this discussion and so we will have to deal with just the retention side of the arguments for realism at this juncture. Because explanatory concerns loom large for the realist, and yet mere correspondence relations show little hope of generating explanations for empirical phenomena, I will also have to take explanations to fall under the category of evidential warrant. As such, the position I am sketching in this chapter has to leave to one side the thorny issue of how explanations might play a role in generating realist justifications for belief. This might seem problematic because I have previously argued against structural realist positions on the grounds that they don't provide explanations of the success of any given theory for which they are supposed to account. However, notice that these accounts (early epistemic structural realism and early ontic structural realism) did not have the notion of auxiliary properties on which to fall back. Both Semirealism and my position are happy to appeal to auxiliary as well as detection properties with which to generate explanations for the success of some theory. I treat all such properties instrumentally except those which are both retained and for which we have good reasons to think we've detected them, and in drawing this epistemic line it doesn't commit me to believing all parts of the explanation for empirical success. I only license belief in those very few necessary, detected, and retained components.

I argue for this position, which we might call 'Local Correspondence Realism' in the following way. First I argue that we should take the failures of previous structural realist accounts (which try to pick-out a single form of structure with which to answer the PMI) to indicate a general problem with the preservative structural realist strategy: a *single* structural principle cannot be used to answer the PMI. Because any notion of structure fails to capture everything that it needs to in order to generate successful predictions in all theories, and at the same time it also fails to retain enough of the right structure across the history of science, I conclude that the strategy of looking for a single kind of structure preserved across the history of science to answer the PMI is bound to fail. Combining this fact with the argument that reasoning from case studies to general principles is a problematic affair to begin with, I argue that we should give up the project. We will then look at a number of different kinds of correspondence relations from the history of science, and by appealing to the historical record I will use these to recover a more plausible realist position. I will show how such correspondence relations provide warrant for belief in the right parts of the Fresnel-Maxwell transition. In general we find through the history of science that sometimes the retained content is very thin, and sometimes it is very short lived for some particular kind. Regardless, the strong claim that I make here is that there is always some retained form of correspondence relations for successful mature scientific theories.

The reason we should think that retention and detection are good grounds on which to justify belief in the theoretical constituents of our theories is that this content is seldom ever replaceable. It is not as if we can reasonably claim that necessary retained relations have a history of replacement, and such a situation makes for a strong case. In fact in section 5.3 I briefly consider van Fraassen's 'Darwinian' explanation for the success of scientific theories as an alternative to my conclusion that our theoretical constituents that reflect correspondence relations are real. This argument I believe fails to account for the success of any particular theory, and although I do not endorse the strategy of inference to the best explanation for all theoretical entities, I think when it comes to those that have been detected and retained we have substantial warrant to think their reality a much better explanation for success than alternative pragmatic responses like van Fraassen's.

Finally, to close out my argument for local correspondence as a condition for belief, I will distinguish this account from other popular forms of realism that try to answer the PMI. Although they fail in the general project, there are similarities between these accounts and the local account I advocate, and I want to show that although there are superficial similarities between our views, the account I advocate is importantly different in its realist strategy.

5.1 Against Singular Solutions to the PMI

The most recent formulations of structural realism—those we reviewed in chapter four called Semirealism and Eclectic realism—are, I think, quite close to getting things right. They both recognize the need to pick-out a form of structure which does not commit to mere mathematical equations, (as we found with the strong epistemic version), nor do they dissolve entities into mere structures, (as with ontic structural realism). One of the toughest historical cases for the scientific realist, that of Fresnel's equations, can almost be accommodated on the minimalist construal of our retained mathematical equations, either by appeal to first order causal properties, or higher order multiply instantiable properties. Almost, but not quite. Both accounts fall victim to the semantic difficulty of non-preserved logical form and they are simultaneously too weak and too strong in the general principles they advocate.

What are we to learn from our previous analysis of the most compelling forms of structural realism? Should we settle with these last versions and accept the anomalies as they arise? Should we continue to pursue the realist chalice in the hopes that by further narrowing our structural commitments, or by changing the level of metaphysical discourse, we will finally reach the goal of a single general principle with which to answer the PMI? I recommend neither path. I think it is a good idea to forget the hopes we have of reaching a *single general* principle, but at the same time believe that appealing to some form of retention or correspondence across theory change is still a means to answering the PMI. Let me explain this by first arguing that the hopes for a single kind of structural correspondence are misplaced given the manner in which we have to argue for it. Second, I will point to the diversity of *kinds* of correspondence we find in scientific change, and suggest that when these arise then there is good reason to adopt a minimal realist stance towards their content. It is the presence of these kinds of various correspondence relations that can be used to answer the anti-realist's PMI argument, since it is these theoretical constituents or relations that we find partly responsible, to one degree or another, for the predictive success of the theories in which they are embedded.

Let me begin with the assumption that when trying to answer the PMI the strategy used by the various accounts of structural realism with which we have been concerned is that of the preservativist: find that which has been preserved across theory transitions and show how that element really was justified in earlier, faulty, theories. I will also assume that the structuralist suggests the history of science reveals such preserved elements to be structural in nature. This I take to entail that the structural realist thinks a principle of the sort which advocates belief only in retained structural elements of theories can be found which will point out just what that structure looks like. Furthermore, this principle (whatever it turns out to be) is supposed to provide a general prescription for answering a global problem. That is, the principle being selected is supposed to answer the PMI across all theories and all times, where the PMI is still being taken as a set of counterexamples to the standard realist No Miracles Argument. If this reading of the structural realist's position is too strong, then perhaps they will find a way around the difficulties raised in chapters three and four, although I think the previous analyses provide evidence that these positions are indeed supposed to be very generally applicable.

Now let's address the question of how it is one could reasonably expect to argue for a single general principle, like that required by the structural realist, from specific case studies. That is, how is it justified to argue from specific cases of scientific change to a general principle which is to apply across the board? This is not simply another formulation of the familiar problem of induction, because when it comes to scientific realism we have seen in the PMI some very particular cases of why we should be skeptical of realist claims. When we refine our position and consider only structural realism as a means of finding the desired general principle we similarly run into trouble—the difficulties raised in the past couple of chapters for example. Using the Fresnel-Maxwell transition as a primary case study, we've seen that sometimes structure does indeed get nicely preserved. However, problematic cases such as caloric, or theories of the electron, or even the transition from classical to quantum mechanics, have all provided counterexamples to the instances where a general principle did seem to work for our primary case. The approach of appealing to case studies, even those restricted to the domain of physics, therefore, does not seem to have so far worked for the structural realist. It appears that no matter how one refines the position, there always seem to be intractable difficulties.

Perhaps an alternative method of arguing for the structural realist's general principle might be to start with some principle and argue that it is epistemically probative on *a priori* grounds, and then show how this is manifested in the history of science. In fact, although we didn't spend any time covering this approach, it has been put to some use by Zahar in his Ramsey-Sentence realism. He appeals ultimately to the notions of 'simplicity' and 'unity', as do many full-blown realists (like Philip Kitcher and Michael Friedman). Chakravartty also makes some use of this tactic, but instead of some specific super-empirical virtue, he argues that the distinction between detection and auxiliary properties on his account is *a priori* justified. He says,

> Case studies are valuable here, but from the realist perspective it is undesirable to hold the tenability of the position hostage to an exhaustive series of studies…An explicitly structuralist approach offers…an *a priori* reason for thinking that certain structures will be retained. Descriptions of causal properties do the work…Warrant for belief thus boils down to an ability to distinguish between parts that we have good reason to think concern genuine causal properties, and those about which we are less sure.[1]

And as we saw, this approach is cashed-out in terms of the minimal interpretation of our mathematical equations needed to generate successful empirical predictions. But appealing to *a priori* principles to do work on this account makes the account hostage

[1] Chakravartty (2004, p.162)

to one's argument for these principles themselves. Now we don't have room to enter the debate over principles like unity or simplicity, and their role in tracking truth through the history of science, however I do think that Chakravartty's principle is at least somewhat plausible because it is an argument from ineliminability. That is, the Semirealist (and the Eclectic realist) advocates for a minimal interpretation of structure, and this interpretation is itself constrained by the ineliminability of the respective structure. In our key example, the derivations and predictions for light propagation and intensity would have been impossible if the theory lacked the notions of transverse propagation, or conservation of energy across a boundary. On the other hand, just because structure, (or more if you're a full blown realist), has the power to unify two phenomena, (or perhaps exhibits simplicity), this does not provide it with a property which is necessarily going to support its candidacy as a general epistemic principle. It may in fact contingently work out that simple or more unifying theories just are those parts of predecessors which track the structure of the world, but to establish this requires an historical survey and that would return us to the first approach, that of generalizing from case studies. This approach therefore has so far failed for the structural realist—perhaps the unificationist can do better.[2]

If these two alternative methods (appeals to *a priori* grounds for principles of selection or using case studies) of arguing for a single general principle don't work, then maybe there is some other. I am not familiar with any successful project along alternative lines, and as a consequence think we are left having to make the best of an

[2] I actually doubt the unification approach fares any better than the structuralist, and in fact Margaret Morrison has provided a number of convincing case studies to that effect in her (2000).

already worn path. Because the debate is by its nature a naturalistic one, I will avoid discussion of *a priori* justifications and focus just on the method of arguing from cases in the history of science. Note that even though I am also appealing to history for my evidence, there is a significant difference between what I think best works as an argument for realism and those of my predecessors: I think we should abandon the hope of ever finding some *single general* principle with which to answer the PMI. Rather, I think we ought to adopt the same fallibilist realist approach provided by Semirealism, but also accept that this approach doesn't provide us with any single type of structural correspondence across theory change that could possibly answer the PMI. We need to admit that the time has come to give up on defending singular general epistemological principles by appealing to case studies and develop a realist position, which perhaps even retains the advantages of a structural representation of our causal properties, while conceding that the kinds of correspondence we find are diverse. That means we try to answer the PMI with a contextual form of realism which accords more directly with the very many intricacies found in theoretical transitions in the history of science. This approach would accommodate not merely the miracles argument, suggesting that it is some structure-like component of our theories that is responsible for their continued empirical success, but also concede that the PMI appears to have counterexamples to any single general structural principle. That won't hurt this contextual approach because here we don't attempt to advocate a *specific* structural principle—we point to the kinds of correspondence relations found across the history of science and suggest that it is perhaps at any given transition some collection of the very many kinds which is responsible for any given theory's empirical success. These notions of correspondence will I think typically provide us with a thin thread through science, and hence we don't answer the PMI with a single kind of correspondence, but accept that the anti-realist argument forces us into a pluralism about retained 'structure'. To support my case for a contextual approach to structural realism, let's take a look at what kinds of structure we see preserved in terms of correspondence relations across theories, and why appealing to the CP might provide epistemic clout for our realist inferences.

5.2 Kinds of Correspondence

First of all, our motivation for appeals to the CP come from the positive aspects of the previous structural realist accounts we've considered. In chapter three we saw both Worrall and Zahar appeal to the CP not just because it has played such an important role in the development of our sciences (especially the development of modern physics), but also because it indicates where their notion of structure preservation seems to be strongest. Such preservation is unlikely to be a mere accident, especially because where we see it arise we frequently also see successful mature theories. Consequently, one ought to infer that the CP is no mere heuristic device, but importantly reflects where our best theories are correctly representing the structure of the world.

Although their account is ultimately unsuccessful we see in Worrall and Zahar a theme repeated in other structuralist theories of science, and in particular if Semirealism and Eclectic realism are committed to the minimal mathematical structure being preserved through theory transitions, then they too are committed to a form of the CP. Each of these attempts at answering the PMI see in the CP a principle which shows directly, and unambiguously where our mathematical representations within a theory (its equations) capture what is real in the world. We have seen that there is without a doubt something right about this motivation. The Fresnel-Maxwell transition is a perfect example of such correspondence, and these relations we can be confident will stay with us indefinitely. On the other hand we've seen this strategy fail in answering the PMI because of the inability so far to account for the change of logical structure between sets of equations in theories (classical to quantum mechanics) by appealing to the CP. So, although clearly onto something compelling, the CP so far has been unsatisfactory.

On my account however, we are not trying to answer the PMI with a single kind of correspondence, but now attempt the more modest task of providing a story of how a plurality of kinds might answer the PMI, and this it seems to me frees us up to appeal once again to the CP. More particularly, although we still combat skepticism by revealing the diverse sets of relations that all fall under the CP, we can avoid the need to argue for preservation of logical structure (as with the case from quantum mechanics), but more liberally we can suggest that so long as some kind of correspondence relations are maintained, then we have satisfied the realist's need to pick out something retained which is at least partially responsible for the empirical success of the theory (remember than non-retained detection or auxiliary properties can still play a role in the explanation of success but we don't license belief in them). Let's now take a look at the kinds of relations I am referring to when claiming the CP to be heterogeneous.

We want to know what it is that we might plausibly take to correspond between theories and provide grist for the realist mill. To establish this we will need to start with some statement of what the CP means. We can certainly start with the traditional notion of the CP, which we already encountered in chapter three: *the mathematical equations of an old theory are limiting cases of those in the new.* But there are other popular versions which might prove to be more informative (mostly relevant only to quantum mechanics, but useful nevertheless):

David Bohm[3] says, "The principle states that the laws of quantum physics must be so chosen that in the classical limit, where many quanta are involved, the quantum laws lead to the classical equations as an average."

Albert Messiah[4] says, "The correspondence Principle…consists in stating precisely to what extent the notions and the results of Classical Mechanics can serve as guides in the elaboration and interpretations of the correct theory." A little further on Messiah says of the principle, "We have here a very restrictive condition imposed upon the Quantum Theory. One often expresses it in abbreviated form by saying that: *Quantum Theory must approach Classical Theory asymptotically in the limit of large quantum numbers*." Furthermore, "In order that this condition might be fulfilled, one establishes in principle, *that there exists a formal analogy between Quantum Theory and Classical Theory*; this "correspondence" between the two theories persists down to the smallest details and must serve as a guide in the interpretations of the results of the new theory."

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[3] Bohm (1951, p.31)
[4] Messiah (1958 p.29)

In his excellent history of modern physics, Helge Kragh[5] adds, “The essence of the correspondence principle, as Bohr understood it around 1920, was the following: In the limit of large quantum numbers (n→ n-m, m<<n) transitions to stationary states not very different from the initial one will result in frequencies almost identical with those to be expected classically, that is, from Maxwellian electrodynamics.”

Roberto Toretti[6] says, “Its sole motivation (or justification) was to secure that, in contexts in which Planck’s constant h is insignificant, the quantum theory would agree with classical electrodynamics.” This is very much the traditional interpretation. Yet there are those who see an earlier instance of the principle in Poincaré.

Elie Zahar[7] states, “The Correspondence Principle…Poincaré defines and justifies as follows: should an old hypothesis H prove to have been systematically ‘convenient’ throughout some domain Δ, then it is improbable that this should be due to mere chance. H must be taken to express true relations which ought to reappear—perhaps in a slightly modified form—in some new theory T. In other words: T must tend to H whenever certain parameters tend to 0, thereby confining T to the domain Δ.”

These interpretations of the principle are useful in the sense that they show the diversity of relations implicit in the CP. First, note that Bohm appeals to *laws* as implicating the CP, and in doing so it acts as an *epistemic constraint*, rather than a heuristic for devising new laws. If our equations are illustrative of laws (of course not all are), then where a new theory posits a law, it must degenerate into one that

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[5] Kragh (1999, p.156)
[6] Torretti (1999, p.313)
[7] Zahar (2001, p.16)

corresponds to it in the prior theory. In this case, as Planck's constant goes to zero, the quantum laws must approximate those of classical physics. Here the CP is used as a tool for constraining the development of new laws. In contrast, Messiah's interpretation uses the CP as a *heuristic device* as well as an epistemic constraint, since not only must quantum theory approach classical theory asymptotically in the limit, but classical theory's notions and results serve as guides in constructing the new quantum theory. This amounts to more than a mere constraint on a newly developed theory, appealing to the old theory as providing conceptual resources and empirical results that are used in the scientist's attempts to develop a new account of the world. Kragh's definition seems to appeal to a correspondence between *numerical values* for the two theories, while Toretti's makes a more explicit appeal to the correspondence between *empirical observables*. The former here being a constraint on the values for certain entities in a theory, the latter being a constraint on the prediction of empirical phenomena.

Such divergence over the CP, even within only the domain of physics, is not surprising given that the correspondence relations between one theory and its successor are almost invariably going to occur at different levels and to differing degrees. It will be important for our purposes to elucidate such relations as best we can. Here we can take our cue from Hans Radder (1991) and Stephan Hartmann (2002).

In his essay review of a collection of papers on Heinz Post's (1971), Hartmann correctly recognizes the important questions regarding correspondence: which elements of an old theory correspond to which elements of the new? Are there general rules for constructing theories that reflect such correspondences? Are these rules justified as reliable methods for generating true beliefs? The realist of course need not be committed to such heuristic rules, but must minimally provide some account of their justification if used as guides to truth. It is on this point that we will have to appeal to our own No Miracles Argument—claiming that the preservation of correspondence relations across successful theories must be responsible for that theory's empirical success. To do this we need to see what kind of relations we find retained across theories. Hartmann himself identifies seven correspondence relations:

1. Term Correspondence: Terms from the old theory are adopted in the new theory.
2. Numerical Correspondence: Old and new theories agree on the values of some quantities.
3. Observational Correspondence: The new theory 'degenerates' into the old theory in terms of what we are expected to observe under well-defined conditions of observation.
4. Initial or Boundary Condition Correspondence: Some consequences of the new theory can be incorporated as initial or boundary conditions of the old theory.
5. Law Correspondence: Laws from the old theory appear in the new theory (at least approximately).
6. Model Correspondence: A model from the old theory appears in the new theory.
7. Structure Correspondence: The structure of the old theory appears in the new theory.

This is not supposed to be an exhaustive list, but provides a very nice starting point for understanding the CP, and by following these distinctions we can find specific examples of the diversity of correspondence relations in the history of science:

First, these relations are not in any particular order of dependency and they are flexible. That is, although it seems numerical and observational correspondence rely on term correspondence, observational correspondence does not presuppose numerical correspondence. It is perfectly imaginable that the old theory have entirely different values for some property of an entity (or none at all), and yet that the observational phenomena are identical. Additionally, the notion of correspondence implies some form of similarity or analogy between kinds of correspondence in different theories. These forms of correspondence are not identity relations, and consequently, we may see some instances of limiting case talk regarding laws, or divergence of meaning between terms, but this won't undercut the importance of these relations. The question of just how liberal one should be in determining the flexibility of the correspondence relations in question is answered by taking stricter correspondence to carry greater epistemic weight. Thus, where a correspondence between theories is very weak, so too is its contribution to the realist argument. Now let's look at each form of correspondence in a little detail, providing some examples where appropriate.

Term correspondence is particularly problematic for the general realist. Examples of this relation include the retention of the term 'mass' from classical mechanics through to special relativity, and 'electron' from early work by Thomson, through to quantum theory. Although the general structural realist who appeals to ‘pure’ structure will insist that the interpretation of terms in a theory is superfluous to his project, the more recent accounts appeal to causal structure and so might seem to be troubled by the old Kuhnian charge that meaning variance undermines term correspondence across theories. Since the latter is an essential requirement in many other correspondence relations the difficulties of conceptual continuity infect even structural relations. But this difficulty is alleviated if we appreciate that the concepts being dealt with, like ‘electron’, are interpreted only very minimally on the account advocated here. That is, there need not be any commitment to anything more than those relations we have good reason to think are detection properties—and this evidence comes locally from our best theories of evidence, experiment, and instrumentation, in combination with the empirical data. If we go beyond minimal interpretations, then that is when we run into meaning invariance, but detected properties are not lost when we have strong correspondence relations for them from prior to successor theory.

Numerical correspondence means that one finds approximately the same value for entities in both predecessor and successor theories. These values may in fact apply to differently conceived entities, although this is more likely to be due to a divergence in the meaning of terms. Examples of numerical correspondence include the mass/charge ratio on the electron, or the more commonly cited example of the agreement between the values of the quantum frequency of radiation emitted in an electron transfer from one state to another and the classical radiation frequency—both provide correct calculations for the Rydberg constant. This numerical correspondence is apparently dependent upon term correspondence, which makes sense, since without a clear idea of what objects we are measuring in one theory it would be hard to compare their numerical values in a successor theory. Again this problem is minimized on the contextual approach.

O*bservational correspondence*, as an epistemic consideration, I take to be uncontroversial. This amounts to empirical adequacy, which all participants in the discussion recognize to be a minimum requirement for acceptance of a theory.

Consequences/initial conditions relations pop-up in fields like biology, and only when one interprets theories in syntactic terms. That is, a predecessor theory may well find its empirical consequences built into the initial conditions of its successor. This is relevant for cases like theories which consider the development of life on earth, but because such continuity is rare, and perhaps harder to justify epistemically, I will leave this issue aside for now.

Law correspondence can be interpreted in general terms to be the correspondence of equations, or laws, between two theories, and is taken by structuralists in particular as an indicator of veracity. This form of correspondence is, as we have seen, perfectly exemplified in our primary case of the Fresnel-Maxwell transition for Sine and Tangent laws of reflection and refraction. However, we have also seen that this law correspondence causes semantic problems for cases such as the transition from classical mechanics to quantum mechanics. Although limiting case conditions imply that both quantum mechanics and special relativity degenerate into their classical counterparts, in the quantum case the correspondence is severely weakened because classical functions are remarkably dissimilar to quantum operators. The contextual or local correspondence realist has to accept these formal differences, and concede that although using similarly structured relations to build modern physics from its classical counterpart, there is less epistemic justification to be had by appeal to law correspondence for the quantum example. Here we have a prominent example of where the PMI strikes most forms of realism, but I think it fair to say that such discontinuity should be respected, and the correspondence realist accepts these as generating reasons not to believe law correspondence provides much epistemic weight in this case. This is similar to Heinz Post's conclusion, which was to suggest the quantum case proves to be detrimental not to the CP, but to modern physics! That is, he saw the failure of the CP in this transition as implying that quantum mechanics must be false. I see no particular reason to privilege the CP in this way since it gives the impression that the CP is justified *a priori*. I claim no such justification for the CP, and think its respectable epistemic status stems from its success in past science to indicate probably approximately true elements of theories. This is a naturalistic claim, not one from reason alone, and so my realist must accept occasions when this claim is proved to advance only a limited correspondence in the history of science. Yet, this is precisely why I think we need a pluarlist, rather then singular and general notion of realism—taking our clues from the history of science we often do see law-like continuity across theory transition, but it seldom lasts forever, and when it does, we simply have a particularly strong justification for thinking we've genuinely captured the structure of the world.

Model correspondence is similarly problematic for realists. Since epistemic constraints might compel scientists to preserve models from one theory to another, the correspondence may appear straightforward. However, again, the meaning of the model, what it refers to in the world, is going to be down to our interpretation of the theory at hand. Harmonic oscillators, for example, pop-up throughout physics, and yet represent very different systems in quantum mechanics from those in classical kinematics. However, as with all forms of correspondence, I think that when a model is used successfully in repeated theories that form a chain which has the predecessor-successor sequence, then we have license to take the correspondence as epistemically probative.

Structure correspondence is the most abstract of the correspondence relations. The nature of structure is as we have seen controversial. We have noticed that when we look to recent writings on structural realism we find definitions of structure that range from talking about the general form of the mathematical equations that appear in scientific theories[8], to the abstract framework of theories like dynamics expressed in terms of group theory and Lie algebras.[9] One example of such high-level correspondence might be that of the inhomogenous Lorentz group and the inhomogenous Galileo group in dynamics. Still, what can be generally said is that most current structural realists opt for some abstract mathematical representation of the high-level laws and equations of our best theories and that this approach has so far failed to answer the realist call.

What are we to conclude from these examples of divergent forms of correspondence? Well, I think that realists who appeal to correspondence have cause both for concern, but also for hope. The divergent nature of correspondence relations

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[8] See Worrall (1989)
[9] See Saunders (1993, p. 297)

indicates that we cannot hang a justification for realism, as a general principle, upon any single kind of correspondence between theories. As I have been indicating, it seems plausible to think that correspondence between empirically successful theories, even when it doesn't last particularly long, is often cause for epistemic confidence—for thinking we're getting things right. This is so far a claim which needs arguing, so let's spend just a little time considering why the CP should be taken in its pluralist form here as a reason for taking realism to be the best explanation for the success of science.

5.3 Why Appeal to a Pluralist CP?

The account of correspondence realism that I am developing here is, to repeat, drawing heavily from its structural predecessors in that it counsels us only to treat as real in our best theories those components which are absolutely necessary (ineliminable) in making successful predictions—the detection properties. The determination of which elements these are is given by our local theories of evidence, experiment and instrumentation. All other components are treated as auxiliaries. Local correspondence realists recognize however that this is not sufficient to protect us from the anti-realist who is suspicious of retentionist claims to begin with. There are three anti-realist concerns with which we shall be concerned in this section before moving on to show how correspondence relations arise in the Fresnel-Maxwell case. Each issue provides reasons why we should not merely settle for detection properties as our minimal requirement for realism, but why we should also tighten our epistemic constraint to include the condition of correspondence between theories. The first is that the history of science reflects the possibility of being wrong about detection properties, the second is that it is impossible to separate detection instances from our background theory so we ought also to commit to some theory or else reject detection procedures as answering the PMI, and third is a van Fraassen's alternative anti-realist Darwinian explanation for the success of science. I don't pretend to have knock-down responses to any one of these concerns, but I do think they lend plausibility to the additional requirement of correspondence across theory transition as a further epistemic constraint on detection instances as a license for realism.

To begin, we have seen that the Semirealist is open to the charge of providing an overly permissive epistemology (even in outline form) since he is stuck appealing to structure that is not preserved across theory transition. Our primary example of this problem has been the move from classical to quantum mechanics, where the logical structure of even the minimally interpreted equations of the respective theories has been lost across scientific change. The Semirealist might respond that he is agnostic about all minimal structural relations except for those which we have good epistemic reasons for thinking we have made a detection, and hence that this commits him only to the logically similar structure. This is implausible, however. Quantum mechanical predictions are stupendously accurate in some cases, and so too are some of the data retrieved from experiments. If we take the simple example of the detection of parity violation for electrons we see that there is in this case a strong commitment to the mathematical structure for scattering experiments with electrons, even though this structure is of a different logical form from classical scattering. It seems implausible for the Semirealist to deny the good local evidence for saying we have detected electrons, and their obedience to the violation of parity conservation, and hence he seems committed to the retention of structure without preserved logical form because he must commit to the success of our detection apparatus in this case. If he denies such commitment due to a lack of empirical evidence for detection of this violation, then his position seems to be denying one of our strongest and most empirically verified experimental outcomes. If this example doesn't count as a successful detection what would?

In response, I suggest that the appeal to correspondence between theories is to be taken in a less strict manner. We don't need to commit to the full mathematical structure preserved from the classical to the quantum case, we merely commit to that for which we also have good reasons to suggest we've made successful detections. If our data show that we have more left-hand polarized electrons scattering off our apparatus than right-hand polarized, then we believe in those components of our theory that have been retained from our prior theory yet which also contribute to this novel fact.

Two points need to be made here. First, there is no simple transition from classical mechanics to quantum mechanics. Radder (1991) has already clearly shown this point, illustrating the various forms of correspondence between classical mechanics and early as well as more mature quantum theory. Hence, it isn't really fair to say there was a single transition; better to see the changes as multiple—yet this still incriminates the Semirealist because there is no doubt that logical form failed to be preserved from the earliest form of matrix and wave mechnics. Local correspondence realism will be far more successful at accounting for the minimal changes we see in this case, although I don't have space to substantiate this claim here. The point however is that correspondence realism commits to the retained structure detected through several changes in theory, and therefore as quantum theory progresses and we see more structure correspond to previous incarnations of the theory we broaden our belief base. In this way the correspondence realist is not committed to all the structure required by the Semirealist in the classical-quantum transition, yet he is able to handle the strong evidential claims made for our parity violation example.

The second point is that the Semirealist is sometimes going to be unable to account for cases where scientists have been wrong about detection properties. Moving away from the parity violation example and further back in the history of science, it is not unrealistic to suppose that some scientists may think that they have made successful detections of some properties of entities or relations between them, but history shows them to be wrong. For example, return to the troublesome instances for Semirealism of caloric or the electron. It is arguable, yet not unrealistic to suppose that Laplace might have thought that the necessity and ineliminability of the caloric force function, in combination with experimental evidence, led naturally to the conclusion that he had actually detected, experimentally, the caloric. Similarly, Lorentz required the assumption of rigidity for the electron. Although perhaps an assumption, it may have been one which played a sufficiently strong role in experimental theory for him that he actually was convinced we had detected the rigid-body electron.

These examples are somewhat fanciful because I have provided no evidence that this actually was the case, however the point remains that Semirealism depends heavily on local evidential arguments for its commitments, and these arguments are always going to rely on the current theories of evidence, experimentation, data modeling, and instrumentation—all of which is just as changeable as grand theories about the caloric or the electron. The Semirealist ought to be more cautious in his epistemic commitments and restrict belief to theoretical components only after these contributing theoretical beliefs are better established, or, as I'll next argue, less relevant to local evidential claims for some preserved constituent.

This brings us to the second issue: it is impossible to separate detection instances from our background theory so we ought also to commit to some theory or else reject detection procedures as answering the PMI. This problem stems from Semirealism degenerating in some sense into entity realism. If we are licensing belief only in those entities we think we have detected, and we are still wandering in the dark as to what exactly this notion of detection amounts to, it might not be implausible to interpret the notion as 'causally interacted with some property'. But this vague notion itself generates more questions than it answers, foremost amongst which is the concern that causal interaction is being thought of in terms of experimental manipulability. If this is correct, and Semirealism has in Chakravartty (1998) been argued to provide us with a form of entity realism, then it seems likely that the position will suffer many of the problems heaped on entity realists like Ian Hacking. I won't go into an extended debate here over the various pros and cons of entity realism, but one significant difficulty which Semirealism would appear to share with it even if the latter is somewhat distinct is that we are supposed to believe in entities when we can manipulate them. The trouble is that it is just our theory, to which the entity realist is agnostic, that informs us whether or not we have succeed in making such manipulations. How can the Semirealist guarantee that the relations which are supposedly revealed through experimental interactions are in fact the right ones, even if he remains agnostic about the entire set of properties the might be attributable to an entity in the future? In fact, it is quite reasonable to take examples, such as those above, or perhaps that of quasi-particles from modern physics, as being manipulated, and hence detected, particles without them actually existing. This is a significant difficulty for the Semirealist since he would now seem to be advocating a realism about entities we know do not exist. The correspondence realist faces the same difficulty except to say that in each case he should be able to show that commitment to such erroneously 'detected' entities was withheld because we discovered that the background beliefs informing our theory of detection were themselves erroneous—or that the scientists made a mistake, or some such thing. This is not of course an argument, and it is up to the history of science to provide the evidence on this score, but the correspondence realist wagers that by holding back on epistemic commitment until entities have been retained, perhaps even preferably through change in background theory, we will be pinning down just those constituents of our best scientific theories that genuinely reflect the structure of the world.

This leads us to our third reason for adhering to a correspondence realism instead of Semirealism: van Fraassen has argued (1980) that the truth of our theories (or parts thereof responsible for empirical success) is not necessarily the best explanation of such success. Just as the mechanism of evolution selects the fittest individuals in an environment for survival, so we select the successful theories based upon our own theory of what is and is not successful. The success of a theory does not entail its truth because there is no reason to think success is intimately connected to truth. The reasoning here is that successful scientific theories survive just because they are successful, just as surviving species are still with us because they have the property of survivorhood—there is no need to posit the additional property of truth to our theories; they have survived just because they are successful (unsuccessful theories fall by the wayside), not because they are true.

The realist has to respond to this criticism because it seems to provide an epistemically less risky response to the NMA. After all, if one can explain the success of a theory without committing to its truth, then one will not have to cope with all these difficulties posed by anti-realists. However, as has been argued in one form or another by Kitcher, Stanford, and Psillos, this 'Darwinian' explanation for the success of science is much too shallow to constitute a good explanation, truth is far deeper and therefore more successful itself . Following Psillos' explication,[10] we can argue that the Darwinian account is analogous to a *phenotypical* level explanation; it provides an explanation with an internal or implicit selection mechanism: empirical success. The realist has a better, internal explanation which is more *genotypical*: the approximate truth of the theory causes it to have the phenotypical trait of empirical success. It is like explaining Sid's success at admission to a school which requires a 4.0 GPA by saying that Sid has a GPA of 4.0. The school is the selection mechanism and selects based on this one requirement. But a deeper explanation would also explain why Sid had a GPA of 4.0 by appealing to internal mechanisms like his hard work, his natural

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[10] Psillos (1999) pp. 96-7

intelligence, coming from an encouraging and academically successful environment, etc. In the case of science, the realist appeals to the truth of specific mechanisms for particular theories to explain why they are empirically successful, and this is far deeper and explanatorily satisfying. It also provides the added advantage that if a theory's success is due to its being approximately true, then it will continue to be true in the future. The best that the Darwinian account (in its naïve form) can do is argue for future empirical success based on straight induction from the past, which is a much weaker prediction that one based on internal mechanisms—although, this response is not really available to the Semirealist (as it is to the correspondence realist) if, as we saw above, the former's position doesn't always get the responsible theoretical components of a theory correct. Again, correspondence realism is to be preferred because it is more cautious about its commitments and projects their future retention only after background theory change.

This completes the defense of why a pluralist about structure should also adhere to the notion of correspondence between theories in delineating conditions for belief in theoretical entities. Now we can return to the explication of this pluralism by taking account of how it exists in the history of science.

5.4 Correspondence in the Fresnel-Maxwell Case

The description of this case has mostly been provided in chapter two, and it shouldn't be surprising that the structural realist looks to the exact correspondence between laws in this example as providing epistemic weight for their revealing the true nature of reality. But Fresnel's laws are of course not what is disputed with the traditional realist, and here because the local version of correspondence realism adopts a minimal interpretation of the theory, (just like its predecessors Semirealism and Eclectic realism), we too adopt retained minimally necessary laws. In the Semirealist account this amounted to the commitment to relations between transverse oscillatory motions and the intensity of these oscillations. These relations we have seen already:

$$v = \mathrm{V}\sin(i - e)\ /\sin(i + e) \qquad\qquad u = \mathrm{V}\cos(i)/r\cos(e) * \sin(2i)/\sin(i + e)$$

Here we see represented the amplitudes of oscillation of light which is orthogonal to the direction of propagation of the wave front. Again, (i) is the incident ray's angle, (e) is the refracted ray's angle, (v) and (u) are the reflected and refracted amplitudes of the oscillations and are related to the amplitude of the incident ray (V). We should also commit to energy conservation principles, and the boundary condition supplied under the Eclectic realist analysis:

$$\mathrm{A}_{\text{incident}} + \mathrm{A}_{\text{reflected}} = \mathrm{A}_{\text{refracted}}$$

Notice however, that although we take our A's here to refer to energy of some form, we need not commit to momentum as carried by the waves since it was not specifically preserved in the transition. There is no correspondence of momentum conservation between the two theories. We can see this clearly since Maxwell rejected many material elements of his own early electrodynamic models, but still insisted on the reality of the energy of the field[11] by arguing that he wanted, "Merely to direct the mind of the reader to mechanical phenomena which will assist him in understanding the electrical ones" but that the intrinsic energy of the field should be treated literally.[12]

What other correspondence do we find in this case? Well if we work down the list of kinds of correspondence above and start with term correspondence, then there is not much substantive terminology retained. Of course we still see terms like 'refraction' and 'reflection' in our successor theory, but these imply little about the unobservable world. 'Mass' is lost in the transition, so too is 'ether', as is the original notion of a 'ray' in Fresnel's derivation. It seems also that the notion of a 'wave front' has been transformed into talk of wave vectors and phase velocity. So although there are some terminological overlaps, these are insubstantial, and so do not pose the same kind of issues we see arise in cases like taking the notion of 'mass' as retained in relativity theories to be identical to that of its counterpart in classical dynamics or mechanics.

On the other hand, we have a strong numerical correspondence for this case when it comes to the actual values of the intensities of our respective light phenomena, which is to be expected since these could be determined accurately with the then standard instruments of photometry. Anything less than a similar numerical value for either reflection or refraction amplitudes would render the theory at hand empirically inadequate, and hence unlikely to reach the heights of maturity we require for any

[11] In particular Maxwell insisted that the potential energy was to be found in the electrostatic energy of the field and the kinetic energy was in what he called 'elektrokinetic' energy. See Maxwell (1873, chapter XI).
[12] Maxwell (1864, p. 41)

form of realism. Of course where magnitudes of vector fields, like **B** or **E** are concerned we shouldn't expect to find any numerical correspondence since we don't find even term correspondence—these are entirely new entities, and hence terms, in our successor theory. We might however suggest that successful predictions in Maxwell's theory also confirm those background assumptions which were common to both theories, like Snell's Law.

Moving on to 'observational' correspondence, we note here that strictly speaking, for the derivation at hand, there is exact observational correspondence for those measuring devices which are used to measure the results of either theory. Of course, Maxwell's theory has empirical consequences very much different from Fresnel's in all manner of experiments, but for this particular scenario, the results are observationally identical. This is not in the least bit surprising, given the requirements of empirical adequacy.

The next kind of correspondence is that of initial or boundary conditions. This form of retention occurs when the consequences of a theoretical prediction are taken as either initial or boundary conditions for the successor theory. I won't dwell on this scenario here, except to mention that in our case, the initial and boundary conditions are clearly retained. The start of the experimental set-up is identical in both cases, and the boundary conditions are interchangeable: the continuity equations require continuity in the components of the oscillations parallel or perpendicular to the interface, respectively.

I have already talked about the corresponding laws in this case, and so shall move on straightaway to the concept of model correspondence. Here there are obvious dissimilarities, and the question is whether there is anything from Fresnel's model which corresponds in some sense with Maxwell's electro-magnetic field theory. I suggest that the only retained element of the models used by these physicists was that which we have already captured under previous correspondence kinds above: the transverse oscillations, the intensity relations, and the boundary conditions. One might also be tempted to point to Maxwell's explication of very complex models of the rotating ether, but of course he gave up on the attempt to model the underlying theoretical processes of electrical and magnetic phenomenon, instead recommending an agnosticism regarding their reality. It is true that he held out hope for a mechanical account of these processes, but explicitly required that until a fully worked-out theory was at hand we should treat such models merely as useful heuristic devices.

Some might claim Fresnel was in a similarly agnostic position regarding the ether, especially since his assumption that the propagation of light as transverse waves was in direct conflict with the assumption that the carrier of these waves was an elastic-solid medium.[13] However, we've seen that he certainly imparted mass to Laplacian point particles, and that he did commit to some kind of medium consisting of such particles. This significant disparity has been the cause for much realist hand-wringing, but it need not bother the local correspondence realist, who is happy to accept a lack of correspondence between theoretical components of our best models, given his minimal interpretation of the theories under consideration.

[13] The elastic-solid mechanics with which Fresnel worked dictated that linearly propagating waves in an elastic-solid generated longitudinal, not merely transverse waves.

The last kind of correspondence that needs addressing is that of structure, when taken as a particularly ‘abstract’ mathematical component of theory transition. We don’t see such high-level correspondence here in our case study, but it undoubtedly exists in other cases of theory transition (recall the group invariance under transformations we saw Ladyman appeal to in the move through conceptions of particles in modern physics). Still, this category of correspondence is perhaps the weakest in terms of providing epistemic justification for our minimal interpretations. Some would argue that since there are in fact many correlations between formal structures throughout science (dispersion equations in both biology and physics for example) then these forms of correspondence must play no role in theoretical transitions from one domain to another. Why should we think transitions for these common factors are therefore a type of verification for their correctly representing the unobservable within a domain? Well, I don’t think we have to be this skeptical. If we see a direct correspondence between even high-level structures within theories that fall into the pattern of ‘predecessor-successor’ within a scientific discipline, then it seems much more likely that this indicates something about the nature of reality than if we claim continuity of structure across very different disciplines. I grant that the continuity may be merely a result of our preference for that form of representation or calculation, but suggest that when this pattern is repeated the chances of such luck diminish rapidly. Regardless of one’s optimism about such issues, the first question is always going to be whether the high-level structure derives confirmation from some form of empirical predictive success, and that is very much going to depend on our theories of evidence. Again, this is an issue we don’t have room to investigate here, but that should not undermine local correspondence realism—after all, even Semirealism leaves open the question of how one empirically differentiates detection from auxiliary properties.

There are many other examples of where minimal interpretations combined with existing correspondence relations provide us with a local form of realism, even though for any one instance not all of the correspondence relations from section 5.2 will fall within the domain of local empirical justifications given that the correspondence realist requires only a minimal interpretation of our theories. A full list of those elements a correspondence realist can confidently assert to exist requires detailed analysis of each particular case, and we certainly don't have space for that here. We can however indicate some prime candidates for which it seems we might have good local evidential arguments as well as at least some evidence of correspondence with prior theory. For example, in Newtonian Mechanics the notions of 'mass' and 'inertia' are retained in the subsequent theory of Special Relativity. We find the notion of 'electron' retained from its earliest usage through to modern Quantum Electrodynamics. The Periodic table of the elements has retained its elements even though their definitions themselves have changed. Again in Newtonian Mechanics, the Second Law and momentum relations are later reformulated relativistically. We find here of course that the Special Relativistic formulations degenerate into those for classical physics when $v/c = 0$. There is a similar degeneration for the laws of Non-Relativistic Quantum Mechanics when $h = 0$. Faraday's Law, Ampère's Law, and Coulomb's Law each correspond in early form to their counterparts found in Maxwell's equations. Heisenberg's Uncertainty Principle remains constant through varioous formulations of quantum theory. In Statistical Mechanics, the Boltzmann Relation and the Partition Function each have corresponding Quantum Statistical Mechanical formulations. Through multiple theoretical changes around them we have retained constant values for the electron charge/mass ratio, Avagadro's number, the ratio of proton to electron mass, Boltzmann's constant, Faraday's constant, the velocity of light, Planck's constant, the Fine Structure constant…the list goes on.

I take it from such correspondence relations, with the appropriate evidential warrant, it would be miraculous for our theories to be wrong about any of these theory components. This is the source of the local realist's optimism. Granted, most of our confidence may arise from local evidential considerations, but I counsel that we should bear in mind the list of counter-examples which comprise the PMI. Local optimism is not on the surface unreasonable in some cases, but a more refined, more cautious realism is to be preferred and this can be accomplished by appeal to the CP.

5.5 Why Correspondence Realism isn't Standard Realism

It should by now be obvious that correspondence realism is a distinct position in regard to those structural realist accounts we've analyzed throughout our survey. There might however remain some concern over how this picture differs from some of the other more plausible non-structural realist accounts on the market—in particular why my form of realism is not to be confused with the Essential realism we met at the end of chapter one. In this section then I want to make perfectly clear why correspondence realism is importantly different from Essential realism. The reason for diverging onto this topic is twofold. First of all, having sketched in outline form the general spirit of my view, comparing it only to structuralist positions limits our view of its independence from other accounts. By looking at superficially similar realist positions one can come to appreciate how much rejecting the singularist strategy splits with the current literature. Second, Essential realism is in many ways similar to Semirealism, and as a successor to Semirealism, my view needs to be able to avoid some of the difficulties the former position encounters. By briefly surveying how Essential realism answers our favorite case study (and others) and pointing to its deficiencies, we will simultaneously enhance the view of correspondence realism's response to similar difficulties.

Let's start by going backwards. In chapter one (section 1.3) I explained that Essential realism, advocated by Kitcher and by Psillos in slightly different ways, adopts a two prong strategy for answering the PMI: explain the presence of problematic theoretical entities like 'ether' or 'phlogiston' by appealing to (i) a refined causal-descriptivist theory of reference, (ii) a theory of justification which licenses belief only in the theoretical entities, processes, or laws that were *essential* in deriving *novel* predictive successes for our *mature* and *highly successful* theories. I argued that this approach is problematic because its way of fixing reference is too liberal and hence runs the risk of triviality—Newton for example would have to be taken to have referred to space-time curvature when talking about gravity. I also suggested that the line between what is to be considered *essential* to a derivation for some novel prediction is vague. In particular to determine whether, for example, Maxwell needed the ether or not to derive his theory of light propagation, ought to be irrelevant to whether *we* should believe in the ether. Our resources for making the derivation today may be very different from our predecessors' and just because they needed some theoretically problematic mechanism, that doesn't mean it plays an essential role in our derivation of the same empirical predictions.

Still, these difficulties are a little vague, so let's take a look specifically at how Psillos handles the case of the ether. Looking back to Fresnel, he was able to determine that the nature of light propagation was transverse and yet still oscillatory only by considering how a mechanical ether might move. His own derivations for the Sine and Tangent Laws depended upon his assumption that light has a transverse oscillatory component, and hence he required the idea of an ethereal medium. But Psillos suggests that it was merely the causal roles of essential properties, like transverse oscillation, that were necessary in the derivation of the laws. Fresnel was correct when referring to these causal properties because these are the same causal properties we see retained in the successor theory developed by Maxwell. Importantly, the ether was capable of storing energy, and it was the manner in which light energy was stored and transferred across space and time which we see retained. It is to these causal properties that reference was made, and made correctly. What once was thought to be an ethereal medium is now taken to be the electromagnetic field—but reference is secured via correct reference to causal properties responsible for specific empirical predictions.

Through a causal-descriptivist account of reference Psillos hopes to be able to show how 'ether' was a term whose causally efficacious properties, which were essential to the empirical success of Fresnel's theory, were the same as those properties which fuel the derivation of the same predictions from Maxwell's equations. In this way we are at liberty to say that in some sense (for some of its causal properties) the term 'ether' genuinely referred, as does the term 'electromagnetic field'—even though modern physics has now replaced our fundamental notion of what this is with the new concept of a quantum field.

The case of 'caloric' is similar, but also importantly different. The similarity resides in the fact that scientists who believed in caloric were able to generate some undoubtedly successful novel predictions and they did so using this notion of a material substance which moved like a fluid and manifested the properties we associate with heat. The case is however different from our 'ether' example because we do not nowadays have anything which can be described as approaching a material substance or fluid which transfers heat—not only was the term dropped, but we can't even find causal properties which were retained under some part of that term's initial description. That is, we've lost all the properties which may once have belonged to the notion 'caloric'. As a result Psillos has to treat this case as one where the caloric never was an essential part of the derivation of successful empirical predictions. Only by arguing that the caloric was an entirely 'idle wheel' can the essential realist avoid his position from embracing counterexamples.

This move is however controversial for two reasons. First of all, the claim that 'caloric' failed to refer hinges on the supporting claim that scientists of the time did not need to commit to it being a material substance, but rather just used it heuristically even though they made appeal to the properties of a material fluid substance. 'Ether' on the other hand was considered a substance, and its material properties were the causally efficacious ones that helped to generate successful predictions. Regardless then of what the scientists themselves thought, what difference is there between the two cases if they both had causally efficacious properties? Why treat one as a substance and the other not as a substance? An answer to this question relies heavily on fine details of the historical case, which is not itself reprehensible, but hanging the distinction between *essential* and *non-essential* components of a theory on this difference seems risky.

The second problem with Psillos' account of 'caloric' is simply that we've already seen Hasok Chang argue that it is just historically inaccurate. In section 4.3 we looked at Chang's argument that the success of caloric theory really did depend crucially on treating the referent of the term as a material substance. If this is correct, Psillos can't argue that caloric theory's success is explained by reference to causal properties not essentially tied to an entity we have long since rejected.

We should now consider how Essential realism is both similar and different from the correspondence realism advocated here. After all, where there are similarities between the views, those that lead to problems on Psillos' account may well lead to problems for my account. First let's deal with the similarities. Immediately we can see a similarity in that correspondence realism adopts the strategy that we ought to believe only in the components of a theory that are essential in deriving empirical predictions. The reason for this is clearly to avoid commitment to the unconfirmed, and later rejected parts of theories. The distinction between what is and is not essential to a theory is therefore similarly important for my account. Another similarity is that the correspondence realist is trying to answer the PMI without some single notion of what is supposed to be retained in theory change. We've given up the task of providing a single kind of correspondence between theories, and although Essential realism is similarly diverse (appealing to causally efficacious components of theories, rather than something more limited like 'structure') the position I support does not have to hang its realism on saving full reference in cases like the ether or phlogiston or caloric. The correspondence realist doesn't have to show that there were correct causal properties in these theories to which partial reference was successful. All we have to do is show that some elements of the theories were retained, and that they played at least some minimal role in the theory's success—not all of it. Furthermore, when a correspondence holds between theories, sometimes in combination with local evidential support, this provides justification for belief in some theoretical entity or process. This means this process is always going to remain with us.

Another similarity between the Essential realist and my position lies in their both adopting a causal-descriptivist theory of reference. I haven't talked much about this, but since my position begins with Semirealism, yet also can absorb reference failure for some theoretical entities, I see no reason to give up on the causal-descriptivist category of theories of reference. Reference is important to us all in this debate, I think, and especially so since we have seen some of the difficulties that plagued the Ramsey approach to structuralism advocated by Cruse and Papineau, where it was thought that reference could be discarded. In particular it seemed that such accounts beg the question by trying to smuggle an intuitive notion of reference in the back door. I therefore have no wish to treat correspondence realism as a form of non-referential realism.

There are however glaring differences between the correspondence view and that held by Psillos and Kitcher, none the least of which is that they don't care at all about correspondence relations except in so far as these reflect the preservation of their particular thing in a given set of theories that is doing the predictive work and will be retained through history. I won't dwell on such differences however, since they don't appear to illustrate the points between our views where the essentialist runs into difficulties and I avoid them. It is probably more useful at this point to close with a brief discussion of some potential objections to the local correspondence realist position.

5.6 Objections to Local Correspondence Realism

Although the above sketch of how we might extend current minimalist versions of realism is so far developed only in outline form, it may already be raising some concerns. Here I will address just a couple of the most obvious reactions.

First of all, how is one supposed to know when we have established something as a genuinely existing theoretical entity? This brings us to the important question of weighing both local justification and evidence of correspondence against the cautiousness generated by the PMI. In the first place, the local correspondence realist is quite demanding in his requirements for accepting theoretical properties as detection properties. We must have a plausible account of why we think some specific properties have been detected, which comes from our theories of epistemic warrant. This means that we require two strands of epistemic warrant, one coming from the evidence for some property, and the other from prior historical cases. We certainly don't want to commit to any particular theory of empirical knowledge, nor dictate what does or does not meet some standard of evidence for such knowledge. The appropriate epistemology is beyond the scope of this project. On the other hand, as an addendum to whichever form of warrant is adopted, the local correspondence realist argues that we also require the correspondence of one or another kind listed above. The basis for this claim, I have argued, is that realists have not been successful in either providing an account of realism that accomplishes the task of providing a preserved notion of structure, nor have they shown why such a preservativist strategy should reasonably be expected to work. We can, however, still capitalize on the notion that preserved structure, even over a short sequence of theories, provides us with a restricted No Miracles Argument for some particular property retained. Thus, we should expect to see future accounts of, for example the electron, retain some of its original properties, like charge and mass, because it would be a miracle for these to have been retained across transitions if they didn't reflect an objective reality. At the same time we should not be surprised when new developments in science, such as we have with the development of modern physics, posit radically new theoretical processes. It is unwise of us to accept the properties of new theories, even if successful, until we have seen them develop and transform while retaining essential components. In this way we maintain a pessimistic realism about what science posits, while not turning our backs on the plausibility of kinds of properties retained across theory change. Furthermore, the degree to which we confer faith in an entity is precisely down to an estimate of how cautious one ought to be and how 'heavy' is our evidence—both empirical and historical. That is, we ought to believe in something when we reasonably judge the weight of the empirical justifications for the presence of an entity, (when combined with the weight of the correspondence relations), is greater than the weight of the PMI. This is understandably going to differ a little between individuals, but rational reflection on the same evidence ought to generate similar conclusions about most cases.

Another objection might be that local correspondence realism is too weak; just like the partial structures view, we get correspondence all over the place, so the position becomes trivial. I don't think this concern effects the position the way it was problematic for either the strong form of Ramsey-Sentence realism or for the partial-structures account. First of all, as mentioned above, this account is to be additional to a theory of empirical warrant, and as such doesn't just appeal to the correspondence version of a no miracles argument to justify its claims. Secondly, the correspondences that we require are of particular kinds, and I don't think they are trivial at all—some of these have been listed above. Perhaps there are other important forms of correspondence that I have missed, but these are not trivial connections. They range from the *apparently* trivial, like observational equivalence, to the very rigorous, like that of law-correspondence, which as we have seen in previous chapters can be quite difficult to secure. So, the local structural realist is no empiricist to be sure.

Alternatively, one might object with a suggestion from the other extreme: this position is too strong, it fails the historical record by not including those things that really were necessary for empirical success, yet were rejected in subsequent science, (like caloric or the rigid bodies of Lorentz' picture). But this localized version of realism, just because it is pessimistic and contextual, is not hopeless. 'Local' is just a label which really translates into the notion of ‘good reasons typical in a domain of science’, but still retaining some of the outlined correspondences. We don’t need to commit to Laplacean or Lorentzean pictures because we can accept that either these views were really auxiliaries, and even treated as such by their proponents, or we can argue that correspondence realism would only treat them instrumentally because there was not a sufficient history of correspondence. Such reasons will usually include some form of severe testing in addition to the requirement of correspondence.

Now there are no doubt other concerns about how much correspondence is enough to justify a change in belief, or why one form of correspondence is ‘weightier’ than another, and even whether there might not be a pessimistic induction on correspondence relations. These are important concerns for a fully developed theory of realism, and require further investigation. Still, I don’t intend on going into such matters here because it seems important first to deal with the larger problem of developing an adequate epistemology to accompany this form of minimal realism. We don’t have room for developing our own or evaluating other such theories here, but I recognize that this is also a necessary part of a fully articulated theory of local realism. Before closing however, I will just say a brief word about the notion of ‘Trumping’ which speaks to these issues.

One might think that there is a fundamental problem with correspondence realism because we can imagine the following scenario: A new theory is developed which is the ‘Supertheory’ of some science. This theory is perhaps a final theory for fundamental physics and is capable of capturing all of the empirical and explanatory successes of all prior theories while simultaneously generating new novel and successful predictions. It's great. The problem is, this new theory has no correspondence relations to any prior theory. None. The manner in which it determines and represents what we now consider fundamental constants, like the speed of light for example, is totally new and therefore has no correspondence relation to any predecessor. We therefore have a case where local empirical justifications for our supertheory are very, very strong, but we have no correspondence relations. According to the correspondence realist though, we shouldn't believe any of the theoretical aspects of this supertheory because correspondence relations are necessary conditions for belief. Surely, goes the argument, any theory of scientific realism that has these consequences is absurd. Therefore, correspondence realism should be scrapped.

Aside from claiming such a situation highly unlikely, and therefore a weak scenario with which to undermine my position, I think we can respond to this problem by accepting that such a supertheory indeed would overwhelm the cautiousness we reasonably retain in light of the historical record. Given that history shows our current theories are similar in kind to past theories, and these were often mostly false although successful, I don't think a high level of caution unreasonable when evaluating evidence for the existence of theoretical entities. However, in the rather remarkable situation of us developing a supertheory which is unlike any of its predecessors it seems appropriate to say that this historical picture no longer applies. Skepticism is drawn from the PMI precisely because we think our current theories are similar in kind to their ancestors, but once this link is broken it may very well be reasonable to suppress our caution—especially in light of extraordinary empirical evidence.

Here then we see a case where we are no longer really weighing evidence for or against a theory in some conventional manner, but rather are overwhelmed by the 'trumping' power of an alternative to all that has come before. The process will of course feel different, but the evaluation of warrant is still in the background.

To repeat, there is much work to be done on our theories of evidence with which we should be constructing local evidential arguments for theoretical entities, but once such arguments are made, we will be wise to restrict our beliefs only to those for which there are appropriate correspondence relations.

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