Ramsey Sentence Realism as an Answer to the Pessimistic Meta-Induction

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John Worrall recently provided an account of epistemic structural realism, which explains the success of science by arguing for the correct mathematical structure of our theories. He accounts for the historical failures of science by pointing to bloated ontological interpretations of theoretical terms. In this paper I argue that Worrall's account suffers from five serious problems. I also show that Pierre Cruse and David Papineau have developed a rival structural realism that solves all of the problems faced by Worrall. This Ramsey sentence realism is a significant advance in the debate, but still ultimately fails for its incomplete account of reference

1. Introduction. The No Miracles Argument for scientific realism states that scientific realism is the only philosophy of science that doesn't make the success of science a miracle. Without adopting realism concerning the entities, processes and theoretical laws of mature and successful contemporary scientific theories, we are left with no explanation for scientific accomplishments (such as our remarkable ability to predict the outcome of certain quantum phenomena to ten decimal places). We should therefore accept scientific realism. On the other hand, the pessimistic meta-induction reminds us that the history of science shows that most of what we previously considered to be mature, successful scientific theories were to a greater or lesser extent, false. The historical evidence should lead us to conclude that our inference methods in the sciences are not reliable, and hence we should also conclude that our current theories are most likely false; scientific realism should be rejected.

Philosophers who advocate either one of these arguments are hard

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pressed to explain away the cogency of the opposing view. Those who advocate a full blooded scientific realism are stymied by examples from the history of science of very successful, yet false theories. Examples include the caloric theory of heat, phlogiston theory, and theories of the luminiferous ether. Antirealists are hard pressed to give an explanation for the tremendous success of theories which are, they believe, false.

Progress has been made. Realists have recognized the need for a more sophisticated view that doesn't argue for the truth of whole theories, but rather sees their success as due to their being only mostly correct. Theories are permitted a degree of inaccuracy in descriptions of theoretical entities, while still ultimately qualifying as approximately true. Antirealists object that such notions are vague and unsatisfactory. They argue that using a rule that infers from the success to the truth of our best theories is illegitimate. Not only does success fail to license claims to correct reference of theoretical entities, which is a necessary condition for the truth of a theory, but also approximate truth is a hopelessly vague concept that encourages realists to generate ad hoc historical accounts of anomalous cases like phlogiston or the ether.

In response to such accusations realists have further refined their accounts of what it is that we are justified as claiming to be true in our scientific theories. Some have moved to entity realism, and argue that we can infer from empirical success to the existence of the entities of a theory, just not to all of the theoretical claims attached to those entities. Some have adopted what I shall call 'essential' realism, arguing that a theory's success is due to its correctly referring to just those elements (entities, processes, etc.) that were essential to the derivation of its predictions. Others have adopted a structural realism, where what is right in scientific claims is just the structure of those theories. It is this last view that concerns us here.

2. Structural Realism. Structural realism comes in at least two forms: epistemic and ontic. In what follows I shall focus only on the former (the latter is far more contentious, and as yet overly restrictive).

John Worrall recently provided an account of epistemic structural realism (Worrall 1989). On his view, the history of science does indeed show dramatic discontinuities at the theoretical level, and hence we are justified in our 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 our optimistic No Miracles Argument is justified. Since our 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 of these continuities. The claim is that structural realism provides an epistemic constraint on what it is possible for us to know about the world. This idea obviously relies upon a clear distinction between the structure and content of our theories. 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 revolutions, we are justified in believing we have accurately hooked onto the world. His 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 is to overcome the pessimistic induction with an account of successful science that marks out what is true, and at the same time explains radical theoretical discontinuity. To illustrate what he means he uses as an example the transition from Fresnel's to Maxwell's theory of light.

The structural realist claims that Fresnel's theory made correct predictions because it accurately identified certain relations between optical phenomena, and 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 not exactly. This case is peculiar in that the equations were transmitted entirely in tact. Worrall thinks that in other cases the equations will be limiting cases of the new equations, and hence, strictly speaking inconsistent. To defend this idea he appeals to what he calls the 'Correspondence Principle': *mathematical equations of the old theory are limiting* *cases of those in the new*. This principle actually acts as a heuristic in developing new theories. It is applicable purely to mathematics, and not to the theoretical terms that might be used when interpreting the mathematics. It is a rule that seems to be at play in the history of physics, and is one that legitimizes structural realism over traditional realism.

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. 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.

3. Problems with Worrall's Account. The account given by Worrall is an advance over traditional realism in some respects, but suffers from at least five serious problems.

First, there is ambiguity in Worrall's 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 his 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.[1]

Second, if Worrall 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 dubious distinction between *structure* and *nature*.[2] 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.

Third, structural realism hinges on the observation that mathematical structure is preserved across theory transition. However, as Psillos has argued, mathematical continuity alone is not sufficient to answer the pes-

1. This point was originally made by M. H. A. Newman (1928) in response to Russell's causal theory of perception, but has been revisited by Demopolous and Friedman (1985).

2. This is a point argued for by Psillos (1995, 1999), Papineau (1996), and Ladyman (1998).

simistic meta-induction, we need a positive argument that identifies the mathematics as responsible for a theory's empirical success. Worrall needs a separate argument to show that the mathematical equations *represent* the structure of the world; retention through theory change is not sufficient (Psillos 1999, 152). To defend against this criticism Psillos says that Worrall needs 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, if formulated by Worrall, would have to commit itself to the view that it is the mathematical content *alone* which is responsible for the empirical success of our theories. This is a possibility Psillos denies on the grounds that any prediction requires auxiliary assumptions and theoretical hypotheses. More specifically, Worrall would have to use an argument for structural representation akin to the No Miracles Argument (Psillos 1999, 153). That is, both empirical success and mathematical structure are cumulative through scientific revolutions. 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 substantive properties being attributed to those entities for which the relations hold. The argument requires predictive success, which requires the kind of substantive properties for theoretical entities that Worrall wants to remain agnostic about.

Fourth, it looks like the concept that Worrall focuses on as the way to redeem scientific realism, (i.e. the retention of mathematical structure through theory change), is itself the most vulnerable element in his position. To the extent that scientific realism is a view that is supposed to apply to all sciences across the board, structural realism is a form that fails precisely because it is limited to only the mathematical sciences. 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 nonmathematical 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. If so, then either the structural realist needs to show us how these retained elements from the biological 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, Worrall is 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 nonmathematical sciences, is going to have a very difficult time preserving the distinction between structure and nature distinction that Psillos attacks.

Fifth, the last problem derives from Worrall'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 the 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.

There have been several responses to the problems raised for Worrall's account that in one way or another advocate an epistemic variety of structural realism. We look at one possible route below.

4. Ramsey Sentence Realism. Although they themselves do not directly offer a response to the pessimistic meta-induction, Pierre Cruse and David Papineau defend a form of epistemic structural realism by claiming that on one interpretation of the realist thesis, the referential status of theoretical terms is irrelevant (Cruse and Papineau 2002). This interpretation claims that the cognitive content of a scientific theory lies in its Ramsey sentence. A Ramsey sentence of a theory replaces all theoretical constants with distinct variables, and then binds these variables by placing an equal number of existential quantifiers in front of the resulting formula. I discuss these sentences in more detail shortly. By staking the explanation of the success of a theory on the approximate truth of its Ramsey sentence, they think scientific realism is no longer hostage to any particular theory of reference. 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. In what follows I shall consider how Cruse and Papineau's Ramsey sentence realism fairs as a response along these lines.

Since Larry Laudan formulated his version of the pessimistic meta-induction (Laudan 1981) there have been several attempts to rescue scientific realism by appeal to intricate theories of reference. Many opt for a hybrid causal descriptivist account, which they hope will capture both the ways in which past theories achieved correct reference, and also how they avoided incorrect reference. Cruse and Papineau think that a better approach to saving realism is through explaining the empirical success of science via the approximate truth, not of its theories, but of their Ramsey sentences. The crucial point here is to deny the assumption that identifying existing entities requires correct reference to those entities. Why not accept the existence of the entities at hand without committing to whether the theory correctly refers to them? If we could do this, then scientific realism would be free from theories of reference. This is precisely what the Ramsey sentence is used for.

The way this works is as follows: A Ramsey sentence is what we get if we replace all the theoretical constants in a given theory with distinct variables, and then quantify over those 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 nontheoretical terms. The Ramsey sentence of this theory would look like this: $\exists! x_1, \exists! x_2, \ldots, \exists! x_n$ $T(x_1, x_2, \ldots, x_n : o_1, o_2, \ldots, o_m)$. Where the original theory assigned some properties to the referents of its theoretical terms, the Ramsey sentence says only that there exist certain unique things with those properties. It no longer makes any claim about what the referents of theoretical terms are because it expels all theoretical terms. As such, we end up with a translation of the original theory into entirely nontheoretical terms. Referential success or failure of the theoretical terms in a theory now becomes irrelevant to the approximate truth of the Ramsey sentence, since all that is required is the approximate truth of the existential claims, not of each posited theoretical term. So, for example, just because the luminiferous ether doesn't exist, and reference fails in this case, that doesn't mean we have to reject the notion of approximate truth. The success of the theory can now be explained by the approximate truth of its Ramsey sentence.

This approach provides a new way to argue from empirical success to approximate truth. Cruse and Papineau argue that although 'ether' failed to refer, the Ramsey sentences of ether theories can be assessed for their existential claims. These sentences will appear weaker than the original theory because they do not claim anything more than existence for some underlying entity, even though they assign it the same properties as the original theoretical entity. "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" (Cruse and Papineau 2002, 179).

All that they take to be necessary here 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.

Let's take a quick look at how this approach solves the problems that arose for Worrall's structural realism:

First, unlike Worrall's account, the structural realism advocated by Cruse and Papineau is not ambiguous on the term 'structure'. Their view holds that we can claim to know the nontheoretical 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.

Second, because Cruse and Papineau's use of 'structure' is concrete, the view they advocate is subject to the charge of making an arbitrary distinction between structure and nature. This accusation depends upon what is considered theoretical in one's theory, since the structure they claim to legitimize is of the theoretical entities which are replaced by variables in the Ramsification process. Since they believe we are licensed to retain all properties for these entities, their structure also remains as it was in the pre-Ramsified theory. This means all theoretical properties are structural, and the notion of the nature of an entity is absorbed by that of structure. There is therefore no arbitrary distinction between nature and structure.

Third, although Worrall's structural realism hinges on the observation that mathematical structure is preserved across theory transition, Cruse and Papineau's 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 the mathematics alone is responsible for a theory's empirical success.

Fourth, 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 nonmathematical 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.

Fifth, the last problem was Worrall's need to isolate similar structures across theory change; he needs to specify exactly what 'similar structure' is supposed to mean. It should be clear that 'similar structure' on Cruse and Papineau's account appeals to identity of the Ramsey sentences of two theories. If the objects have all the same properties as specified in the two theories, then the Ramsey sentences will be the same, provided the line between theoretical and nontheoretical is drawn in the same position on both accounts. The theories differ only in their ontologies. 'Similar structure' is therefore perfectly well specified on their account.

In what follows I consider how Cruse and Papineau refine their account in light of certain problems, but ultimately I think that each of their refining moves is inadequate to the task before it. 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 line between observable and unobservable, but in a manner first expounded by David Lewis (Lewis 1970). 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 nontheoretical 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,

"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 nonlogical relations as causation or correlation, or indeed to many kinds of unobservable things" (Cruse and Papineau 2002, 182).

I see several serious problems with this attempt to ground the structural realist's indispensable distinction between theoretical and nontheoretical terms.

First, 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 Lewisian 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 antirealist? 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 '60s and '70s 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.

Second, when it comes down to it, 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? 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 are Cruse and Papineau to distinguish those theories from which we can adopt a term, once theoretical and now (in a new theory) nontheoretical, from those theories in which a new term is introduced, but which we now consider illegitimate for introducing such a term? For example, what distinguishes the legitimacy of oxygen theory and not that of caloric? Their 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 nontheoretical. 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.

Third, 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 the fifth objection 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, entirely different meaning. How can Cruse and Papineau maintain even simple concepts like 'mass' across Ramsey sentences? The terms have completely 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.

5. Conclusion. Quick and easy solutions to the scientific realism problem, like Ramsey sentence realism, are not going to get us anywhere. Even if Cruse and Papineau were to justify their notion of 'antecedently understood', they still face the more general task of selecting legitimate from illegitimate usage of theoretical terms. On top of that they also run into something akin to Worrall's similarity problem. Their solution is, alas, no solution at all.

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