Mathematical Structural Realism

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Abstract: Epistemic structural realists have argued that we are in a better epistemic position with respect to the structural claims made by our theories than the non-structural claims. Critics have objected that we cannot make the structure/non-structure distinction precise. I respond that a focus on mathematical structure leads to a clearer understanding of this debate. Unfortunately for the structural realist, however, the contribution that mathematics makes to scientific representation undermines any general confidence we might have in the structural claims made by our theories. Thinking about the role of mathematics in science may also complicate other versions of realism.

I.

Unrestricted or global scientific realism is the view that we should take seriously the whole content of empirically successful scientific theories. This attitude requires us to believe that the theoretical claims of the theory are true, or approximately true, and that scientific progress consists in increasing the scope and accuracy of these theories. A series of devastating objections to this position has been developed based on an examination of both the history and practice of science. On the history side, it is arguable that a majority of empirically successful scientific theories are not anywhere near approximately true as we now have evidence that the entities they posited do not exist. The practice of contemporary science raises different and more subtle concerns. Here we find scientists engaging in a wide array of seemingly ad hoc techniques of idealization and approximation. This suggests that we cannot explain the success of our theories by appeal to their truth as the assumptions deployed in the application of these theories have little bearing on the truth of the theoretical claims made by the theory.

Most scientific realists, then, have shifted to a more modest limited form of realism that endorses only those parts of our scientific theories that play a crucial role in the empirical success of our theories. Psillos' "divide and conquer" approach is the most sophisticated and influential proposal along these lines. Psillos responds to the history objections by examining the details of some past successful scientific theories and arguing that the scientists then had good reason to believe only certain parts of those theories. For example, commitment to the ether was not warranted by the success of Maxwell's electromagnetic theory. This is because the ether did not indispensably contribute to the generation of any successful prediction, in the following sense:

> Suppose that H together with another set of hypotheses H' (and some auxiliaries A) entail a prediction P. H indispensably contributes to the generation of P if H' and A alone cannot yield P and no other available hypothesis H* which is consistent with H' and A can replace H without loss of the relevant derivation of P (Psillos 1999, 110).

Still, belief in some features of what turned out to be the electromagnetic field were justified and so a limited realism about Maxwell's theory was appropriate at the time. The objections to realism based on scientific practice are also considered by Psillos in his discussion of modeling and idealizing. While here I believe there is more work for Psillos to do, the broad outlines of a viable limited realism are now in place.

Psillos developed his own brand of limited realism with reference to an even more limited form of scientific realism, namely the structural realism advocated by Worrall. This is the view that, when it comes to the theoretical parts of our empirically successful theories, we only have a good reason to believe the structural parts.[1] For example, in the Maxwell case, Worrall argues that scientists had good reason to believe that something

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[1] This is sometimes taken to be a claim about what it is in principle possible for us to know. I will not include this stronger position in my definition of structural realism. Also, I will not discuss here the view known as ontic structural realism, which maintains that the physical world is itself purely structural.

answered to the series of structural claims found in Maxwell's equations. This structural knowledge is consistent with the scientists not having a good reason to believe anything about the things that were responsible for the correctness of these structural claims. So, unlike Psillos, who says that belief in the ether was not warranted but that some beliefs about the electromagnetic field were, Worrall would argue that neither kind of belief was justified by the empirical success of the theory.

Worrall is explicitly focused on the objections to scientific realism based on history. By restricting what counts as an empirically successful theory, and narrowing his realism to structural claims, he is able to avoid many of the problematic cases from the history of science. In the examples he emphasizes, we can reconstruct a series of theories that preserve the original structural claims made by a successful scientific theory. Above all, Worrall focuses on mathematical relationships between theories when he tries to make this notion of preservation of structure more precise. In the ideal case for his proposal, we have the very same mathematical equation appearing in the succeeding theory. This occurs fairly rarely, though, so Worrall also allows cases where the mathematics of the two theories stand in a limiting relationship: "The much more common pattern is that the old equations appear as limiting cases of the new – that is, the old and new equations are strictly inconsistent, but the new tend to the old as some quantity tends to some limit" (Worrall 1996, 160). For example, Maxwell's equations do not appear in either quantum mechanics or general relativity theory, but taking limits on the equations of the contemporary theories is said to recover Maxwell's equations.

The main objection that I will develop here against epistemic structural realists is that they have not clarified the positive contribution that mathematics makes to scientific theories in general or in the cases they have examined. This objection can be thought of as a special case of the objection to realism based on scientific practice that was noted earlier. For unless the structural realist can say what work the mathematics is doing, she has not dealt with the worry that the mathematics is unrelated to those aspects of the physical world that we have some reason to believe in. As I will argue below, it is not enough to say that the mathematics is ineliminable or indispensable to the generation of successful empirical predictions. The mathematics could very well be an indispensable part of the successful scientific theory and yet be a poor indicator of genuine features of physical world. In the end, I will do more than just raise this concern as an abstract possibility. Instead, I will argue that the most plausible proposal for explaining the value of mathematics to scientific representation entails that if some mathematics is indispensable to a given scientific theory, then there is good reason to think that the mathematics does not track the physical world. This stronger claim is hard to support conclusively, but it appears to put the burden of proof on the structural realist to say exactly when mathematics is needed and can be taken to represent the physical world more or less accurately.

II.

Before turning to this objection to structural realism, it is worth explaining why I do not endorse the stronger objection developed by Psillos that structural realism is not even a coherent position. Psillos claims that there is no reasonable way to draw a line between the structural and non-structural claims made by a scientific theory:

> to say what an entity is is to show how this entity is structured: what are its properties, in what relations it stands to other objects, etc. An exhaustive specification of this set of properties and relations leaves nothing left out. Any talk of something else remaining uncaptured when this specification is made is, I

> think, obscure. I conclude, then, that the 'nature' of an entity forms a continuum with its 'structure', and that knowing the one involves and entails knowing the other (Psillos 1999, 156-157).

We can take this worry to be based on the idea that all knowledge involves knowledge of some nature of the entities in question in addition to knowledge of their structural relations. So, there is no way to call into question all knowledge of the natures of the entities without also eliminating the structural knowledge as well.

The first step in responding to this objection is to give a proposal for what a scientific theory is. Broadly in step with the semantic view of theories, I will identify scientific theories with a collection of wholly mathematical models combined with a set of claims about how aspects of these models relate to aspects of the physical world. In the cases we will consider, the mathematical equations of the theory pick out the mathematical models. The models will be all the mathematical models consistent with the equations for all mathematically possible initial and boundary conditions. The claims of the theory will then link parts of each of these models to properties of the physical systems that the theory aims to represent. For example, in the case of Maxwell's electromagnetic theory, we might have a class of models representing discrete particles in motion.[2] Each particle could then be associated with six real numbers, representing its spatial coordinates at a time along with the mass and charge of the particle in some units. Additional components of each mathematical model might then represent the field strengths at each spatial point at each time. Combining the mathematical models and the claims places a vast array of stringent conditions on a physical system. When these conditions are met by some system, we will say that the model accurately represents the

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[2] This theory also has continuous models, but I will not pursue the complications that arise from theories with models whose structures diverge to such a degree.

system. The hope, then, is that a theory will contain enough models to accurately represent all the systems in its domain.

Now, what are the structural aspects of the theory on this proposal and how do they differ from the non-structural aspects of the theory? My suggestion is that the structural claims of the theory are the propositions that follow from the weakest claims needed to link the wholly mathematical models to the relevant physical systems. Equivalently, the non-structural aspects of the theory are the propositions that are independent of the weakest claims needed to link the wholly mathematical models to the relevant physical systems. In our example, we have a required claim that this particular aspect of the mathematical models stands for charge. Without this claim linking these numbers to charge in some units that crucial part of the mathematical model would be idle. But any additional commitments that a scientist may have about charge would go beyond this. For example, the scientist may believe that charge is not a fundamental physical property and that it is due to some intra-particle features not otherwise represented by the theory in question. Relative to this theory, this additional claim is a non-structural assumption. It is idle with respect to the minimal conditions that the theory places on the physical systems in its domain.

The minimal non-mathematical claims needed to attach the mathematical models to the physical systems are included in the structural content of the theory on this proposal. This seems to be the sort of nature that Psillos has in mind when he argues that all knowledge of structure involves some knowledge of nature. As long as there is a principled way to divide this sort of nature from other more ambitious claims about the system, I do not see why making this concession would be problematic to an epistemic structural realist. The epistemic structural realist restricts our knowledge to what follows from the weakest claims needed to link the mathematical models to the physical systems. The non-structural realist aims to vindicate additional claims that involve additional features of the systems over and above this minimal structure.

This is my interpretation of what Worrall is after when he invokes gravity, in a Newtonian context, as a "primitive irreducible notion" (Worrall 1996, 162). Here Worrall helps himself to the basic semantic contents which are needed to attach the mathematics to the physical world. As long as these semantic contents are not combined with additional claims about the physical properties, I think it is fair to call the resulting content "structural". There are, of course, positions in the philosophy of science that would call into question the availability of these semantic contents, or that would demand that the philosopher of science explain how a scientist has access to them. Here I am thinking of the concerns presented most forcefully by William Demopoulos, and traceable to the writings of Russell and Ramsey among others (Demopoulos 2003a, 2003b). From Demopoulos' perspective, the epistemic problems raised by knowledge of the unobservable are deeply intertwined with the difficulty in referring to properties of unobservable entities. Worrall's direct appeal to such semantic content, then, looks suspiciously like 'theft over honest toil'. As these issues are complex and cannot be pursued here, I will assume that it is acceptable to appeal to the minimal semantic contents that Worrall needs. Even if this semantic concession is made, epistemic structural realism is still in trouble.[3]

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[3] A different version of structural realism tries to overcome these difficulties by appealing to Ramsey sentences. See Cei & French 2006 and Melia & Saatsi 2006 for some recent discussion and references. I cannot pursue the relationship between mathematical structural realism and Ramsey-sentence realism here.

III.

As we have clarified it, epistemic mathematical structural realism is the view that we often have good reason to believe the structural claims made by our empirically successful scientific theories. These reasons are based on the role that the claims about mathematical structures play in allowing accurate predictions. Exactly what this role is has received almost no scrutiny in the philosophy of science, and this has led both advocates and critics of structural realism to miss the central problem with the view. In this section I will outline my proposal for what mathematics contributes to scientific representation. This view arose out of reflection on debates in the philosophy of mathematics about the indispensability of mathematics to science (Pincock 2007), but can be defended independently of those considerations. In the following section I will deploy this proposal to undermine mathematical structural realism.

In a nutshell, my proposal is that mathematics plays a crucial epistemic role in allowing the formulation of scientific theories that can be confirmed by the means that we have at our disposal. This epistemic proposal sits between two alternatives. On the one hand, there is the pragmatic view that says that mathematics is used purely for convenience. On this approach, for each mathematical scientific theory, there is a non-mathematical version of that scientific theory. Working with the non-mathematical version is said to be too difficult or practically daunting. But, in principle, all mathematics is dispensable from science. On the other hand, there is the metaphysical view which ascribes genuine physical significance to the entities studied in mathematics. On this view, the reason mathematics is so central to scientific theories is that

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However, I will say that I do not think Ramsey-sentence realism can overcome the problems I raise for mathematical structural realism.

mathematical entities figure as part of the physical world. So, just as we include talk of electrons in our theories, we should include talk of numbers as well.

Neither of these alternatives to the epistemic view is particularly attractive. The main problem with the pragmatic view is that it proves very difficult to formulate acceptable non-mathematical versions of our best scientific theories. The metaphysical view has the drawback that our best theories of mathematical entities, i.e. the theories developed by mathematicians, do not place these entities in the physical world or endow them with any direct physical significance. It is true that some philosophers of mathematics, for example David Lewis (Lewis 1993), have developed physical interpretations of mathematics. But nobody claims that these physical interpretations of mathematics relate to the ways mathematics is actually used in science.

The epistemic view emphasizes the boost in confirmation that results from formulating scientific theories in mathematical terms. In one respect, this point is fairly trivial. Consider, for example, the statistical techniques of data analysis that are used throughout the sciences to link raw data to scientific hypotheses. It is only after processing the data in mathematical terms that we are able to tell what the significance of the data is for a given theory. This mathematical analysis can give us a good reason to think that no bias of a certain kind has entered into the data collection process.

While this sort of deployment of mathematics is crucial to science and fits with the epistemic view, it has little bearing on a different use of mathematics in science, namely in formulating equations, analyzing their relationships and solving them. This is the central use of mathematics relevant to debates about scientific realism because it is these equations that we use when deciding what to believe about the physical world. Here

I want to argue that the main benefit of using mathematics to formulate these equations is that mathematics allows us to specify a restricted range of claims about the physical systems in question. This restriction is crucial to developing claims that are modest enough to have a chance of being experimentally confirmed by the sorts of experiments we can actually run.

Consider, again, a case from classical electrodynamics. In an experimental context we might devise a system with a number of discrete particles with a certain charge distribution and see how their trajectories are altered when we impose some uniform magnetic field. In describing this system we appeal to some initial and boundary conditions that we believe accord with the experimental setup. In addition to this, we make claims only about the charge distribution, the position and momenta of the particles. We do not take an interest in the smaller or larger scale features of system. This restriction in the content of our claims is carried out using mathematics. We can assign a charge to a particle, for example, without being required to say why that particle has that charge or how that charge relates to entities outside the boundary of the system. More importantly, we can make testable claims about the electromagnetic field, how it interacts with charge, and how it will deflect the trajectory of the particles without taking a stand on what the electromagnetic field is or what its further physical basis might be.

This contribution by mathematics to restricting the content of our scientific claims occurs so routinely in science that we are liable to miss its significance. Imagine, then, how hard it would be to formulate a set of restricted claims about a given system of charged particles if we did not have mathematics at our disposal. One strategy would be to specify the causal mechanisms responsible for the observable features of the system.

But typically these causal mechanisms operate on a scale that is not accessible to the scientist and an ingenious scientist can envision any number of ways in which microscale processes could contribute to what is observed. It is only by restricting our focus in some way that we can make a start at understanding the system. Here mathematics makes its crucial contribution by allowing the scientist to remain neutral on the wide range of questions that she does not know the answer to.

The epistemic proposal is consistent with the prospect of eliminating mathematics in some ideal end of science. This would occur if the scientific community was gradually able to zero in on the underlying causal mechanisms by first isolating stable larger scale mathematical structures and then proceeding to consider possible smaller scale underlying mechanisms. Perhaps this is what has started to happen in the case of Maxwell's electromagnetic theory. But our remaining ignorance of some details of the systems in question is signaled by the highly mathematical character of our current physical theories and the corresponding interpretative doubts that they give rise to. This is entirely in keeping with what the epistemic proposal would predict. On this proposal, mathematics is most useful when we are ignorant of basic causal mechanisms. Mathematical scientific theories about larger scale structures can be well confirmed, but what is confirmed are only beliefs about things at this scale. Highly mathematical theories have the benefit that we can actually confirm some of their claims, but only at the cost of undermining our confidence in our understanding of the underlying causal mechanisms responsible for the phenomena we are observing.

IV.

Unsurprisingly, the proposed role for mathematics in science fits well with the conception of scientific theories developed earlier. We use the mathematics as a scaffolding and attach to it the restricted claims about the physical system. The mathematics does its job by allowing us to make testable claims about the system without forcing us to go further and engage in interpretative debate. On a first pass, this seems to accord perfectly with the position of the structural realist. For the structural realist restricts her belief to those parts of the theory that are required to minimally attach these mathematical models to the physical systems. So, she explicitly limits the scope of her claims about the system to what the mathematics is telling us. It seems that if anything is well confirmed in the theory, it will be the part that the structural realist has isolated. And the sort of empirical success that Worrall emphasizes seems to be all that is needed to reasonably conclude that some part of the theory is well confirmed.

This optimistic conclusion ignores the risks associated with mathematical scientific theories discussed in the last section. We can contrast two ways in which mathematics might be doing essential work in generating successful scientific predictions. First, the mathematics might be mirroring the structure of a stable phenomenon at a given scale of description. This would allow the scientist to confirm a whole host of claims about that phenomenon and there would then be every reason to think that these claims would survive further improvements in the theory. However, a second scenario is possible. This is that the mathematics does not mirror the structure of any stable phenomenon at the relevant scale. The disconnect might be because the mathematics has too much structure or too little. Too much structure would be involved if the mathematics involves more complexity than is needed to track the dynamics of the system. For example, we could imagine a competing electromagnetic theory which ascribed two kinds of charge to particles and had complicated equations relating these different kinds of charges to each other and to the trajectories of the relevant particles. It might appear that all this surplus mathematical structure was necessary to deriving the correct predictions, but this appearance would be illusory. Conversely, the mathematical scientific theory might have too little structure. Suppose (as in fact appears to be case) that electromagnetic interactions are affected by intra-particle forces at high energy regimes. Maxwell's electromagnetic theory ignored these possibilities and so this parameter did not appear in that theory. Additional structure is needed to account for phenomena at higher energies, but we have no hint of this from the experiments conducted in Maxwell's time.

My general objection to epistemic mathematical structural realism, then, is that the use of mathematics in deriving successful empirical predictions gives us no general assurance that the mathematics is mirroring the structure of the phenomenon in question. It could be appealing to too much structure or it might not have enough structure. If either of these mismatches occur, then there is no reason to think that the mathematical structures will persist through further developments in the theory. Note that on the epistemic view of the value of mathematics to science, this is not a merely abstract possibility. It is a consequence of that view that the presence of mathematics signals an ignorance of the underlying causal mechanisms. So, more often than not, we should expect that the structures invoked by a mathematical scientific theory will not mirror the underlying causal mechanisms. But if we lack this assurance, then there is no reason to think that the structures appearing in the successful theory will be preserved.

We can raise the same concern in a different way by focusing on the multiple realizability of any structure picked out by an abstract mathematical description. Consider again the successful description of an experiment involving discrete charged particles moving through a magnetic field. The description offered by Maxwell's electromagnetic theory, which the structural realist endorses, is consistent with any number of microscale realizations of the nature of charge, the electromagnetic field, the physical basis of the boundary conditions and so on.[4] This flexibility goes hand in hand with the description being given in mathematical terms and is precisely what allows us to confirm that the description is correct without knowing anything about the microscale interactions. Still, this has the disadvantage that the structural realist cannot be confident that the structures appealed to in this description will persist as the microscale details are resolved by further scientific investigation. It should strike the structural realist as extremely unlikely that this description happens to have hit on exactly the right level of structural detail. But unless we have exactly the right level of structural detail, there is no reason to think that the structures appealed to will survive theoretical change.

The point can be sharpened by drawing on Wimsatt's discussion of how rare it is that a physical property is aggregative. Aggregative properties are those that are scale invariant. Mass in classical physics is one of the few features of physical things that pass this test. So, if we understand how large objects move in virtue of their mass, we can extrapolate from this description at this scale to how smaller objects would move in virtue of their mass. Charge in classical physics initially also had this feature of scale invariance, but it was eventually realized that there was a minimal unit of charge. If we

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[4] I do not intend to draw the difference between the macroscale and microscale at the level of what is observable. Also, it is worth noting that the same point could be made about the relationship between the medium scale description of the theory and the larger scale features of the physical system.

had a genuinely aggregative property and had some reason to think that it was aggregative, then the difficulties related to multiple realizability just canvassed would not arise. The structures observed at the medium scale would be reproduced at smaller and larger scales. Confidence about the medium scale behavior of the objects would warrant confidence in the behavior at different scales.[5]

It should be clear that we have little evidence that any of the features identified by contemporary science are aggregative in this sense. Scale matters, as shown by the presence of physical constants like Planck's constant in our most successful physical theories. This shows that as we scale up or down from the phenomena that we understand with our current best scientific theories, we should be prepared for a break with the structures countenanced by our current best science. Focusing on the role of mathematics, thus, in the end undermines the link between structure and predictive success so central to the structural realist position. Without this link, the position is no longer tenable.

V.

There is an aspect of the structural realist position that I have not yet discussed in any detail that the persistent structural realist might insist is adequate to respond to my objections. This is the claim that mathematical structure need not be maintained exactly across theory change, but that limiting relationships are sufficient for the preservation of structure that the structural realist is after. Perhaps by emphasizing these limiting relationships the structural realist position can be made flexible enough to deal with the structural disconnect worries presented above.

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[5] Techniques for this sort of scaling and their interpretative significance have been discussed extensively by Batterman. See, for example, Batterman 2002.

It is worth asking why, on the structural realist position, these correspondences should be expected or how they accord with the history and practice of science. To begin with, as Mark Newman has recently noted, different scientific theories may draw on completely different mathematical theories: “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” (Newman 2005, 1378). More generally, the mathematics used in classical physics changed considerably from Newton’s original presentation through the 18th and 19th centuries (Lange 2004). If the mathematical theories used in the series of scientific theories are different in kind, it is hard to see what sort of limiting relationship might obtain between the two.[6]

Even when the mathematics is sufficiently similar, it seems to me that Worrall underestimates the complex interpretative questions that these limiting relationships give rise to. Typically what philosophers of science have in mind when they talk about taking limits is mathematically transforming one set of equations to another set of equations by letting one or more elements in the former set go to 0 or infinity. For example, classical dynamics is said to result from quantum mechanics if we take Planck’s constant to 0. Assuming such mathematical transformations exist, the structural realist must still offer some reason to think that these transformations preserve claims about the structure of the physical systems that we should take seriously. Reverting to the worries of the last section, the existence of a mathematical transformation between sets of equations is perfectly consistent with both sets of equations failing to mirror the requisite structure. Simply because one set of equations involves ascribing more structure than another set, it

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[6] Cf. Worrall & Zahar 2001, 250.

does not follow that the former set is doing better or is preserving the structural claims of the latter set that we should take seriously.

We can think of cases of structure preservation in this sense as ordered between cases where mathematical considerations dominate and cases where physical considerations dominate. Mathematical structure may be preserved across theory change simply because some areas of mathematics are better understood than others. To take an extreme example, if only one mathematical theory is available that can do the job the scientists require, then the mathematics will be preserved across scientific theory change because there are no viable mathematical alternatives. So, the structural realist must explain why preservation of mathematical structure is not dictated by these mathematical considerations. If mathematical concerns are driving things, then there is little reason to believe the structural claims of the scientific theory.

Even at the other end of the spectrum, where it can be somehow demonstrated that physical reasons motivate the details of the mathematical transformations, things may not turn out as the structural realist desires. Consider, for example, a case recently discussed by Batterman. Batterman focuses on the relationship between thermodynamics and statistical mechanics in their representation of critical points in phase transitions. An example is the disappearance of self-generated magnetic fields in some materials as temperature increases past a certain threshold. Batterman's point in developing this example is that the more recent and in some sense more fundamental theory, statistical mechanics, is not able to adequately represent these phase transitions without the aid of the less fundamental theory, thermodynamics. This is because it is only by taking the "thermodynamic limit" of the equations of statistical mechanics, in which the number of particles is increased to infinity, that the phase transitions can be recovered:

> thermodynamics is correct to characterize phase transitions as real physical discontinuities and it is correct to represent them mathematically as singularities. Further, without the thermodynamic limit [of infinitely many particles], statistical mechanics would completely fail to capture a genuine feature of the world. Without the thermodynamic limit, in fact, statistical mechanics is incapable of even establishing the existence of distinct phases of systems (Batterman 2005, 234).

The point that I want to make here is that this sort of relationship between scientific theories should be the best kind of case for the structural realist because the mathematical transformations are motivated by the need to explain some experimentally verified phenomena. Still, the relationship between the structural claims of the two theories is much more complex than what the structural realist is able to account for. Among other things, statistical mechanics does not preserve the relevant structural claims of thermodynamics because, as Batterman says, it is unable to even represent the phase transitions on its own. So there is no general conclusion that we can draw about the two theories simply in virtue of this limiting relationship obtaining between them. Furthermore, notice that the thermodynamic limit linking the two theories involves the assumption of infinitely many particles. As we believe this assumption is incorrect for the systems under consideration it is hard to see why the results of the claim should be taken realistically as reflecting genuine physical structures. Whatever the correct take on this complex interpretative question is, it is not something easily accommodated by epistemic structural realism.

We see, then, that invoking limiting relationships between theories does nothing to improve the plausibility of the structural realist position. Instead, it draws attention to the mathematical relationships between theories that may have nothing to do with those aspects of the theory that we should take seriously. And even when the mathematical relationships have some physical significance, it is far from clear that there is any general reason to think they can be fit into the structural realist position.

VI.

So far my differences with Psillos' brand of limited realism have been downplayed as part of my attempt to clarify what I take the most serious challenge to epistemic structural realism to be. Against Psillos, I do not think the most difficult challenge is in articulating a coherent restriction on our commitments to the structural content of a given scientific theory. Still, everything said so far might be accepted by Psillos as a small amendment to his concerns about structural realism. Perhaps, as with Psillos, I think the flaws of structural realism support a more traditional form of limited realism.

I am not so sure about this happy resolution. Psillos himself maintains that Worrall's structure/nature distinction is "orthogonal" (Psillos 1999, 155) to Psillos' own limited realism, but this does not seem to be the case. Recall Psillos' test for belief in a theoretical constituent: whether or not that constituent is indispensable to generating a successful empirical prediction. If what I have said so far is correct, and there are serious problems taking seriously even the structural claims of our empirically successful scientific theories, then any version of realism that endorses these structural claims and other claims as well is in jeopardy. It seems clear that in many cases the mathematics is an indispensable theoretical component, so in those cases Psillos' realism inherits all the worries I have raised for structural realism.

To see this, return once again to the example from classical electrodynamics. Given the empirical success of the theory, Psillos argues that we have a reason to believe that the key parts of the theory are approximately true. The key parts are those parts of the theory that are genuinely required to generate the successful predications. Based on the conception of the contribution of mathematics to the theory presented in section III, the mathematics of Maxwell's equations is making such a crucial contribution. But the traditional realist has not given any reason to think that the mathematics mirrors some underlying physical reality. It may very well be the case that Maxwell's equations give the structure of some underlying entity, namely the electromagnetic field. But as far as the success of the predictions go, the mathematics used in the equations may have too much or too little structure and so not reflect the features of any physical thing.

We see, then, that paying attention to the role of mathematics in science threatens not only structural realism, but any form of limited realism that moves directly from the parts of the theory required for a successful prediction to a belief in those parts of the theory. It seems that additional conditions have to be met for us to be confident that the mathematics is latching on to something genuine in the physical world. I, for one, am not optimistic that any single, general style of argument can make this sort of fine differentiation. Instead, I would argue that it is only by working through the details of this or that particular case that can we come to some determination about what is entailed by a successful prediction.

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