

Overextending Partial Structures: Idealization and Abstraction

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The partial structures program of da Costa, French and others offers a unified framework within which to handle a wide range of issues central to contemporary philosophy of science. I argue that the program is inadequately equipped to account for simple cases where idealizations are used to construct abstract, mathematical models of physical systems. These problems show that da Costa and French have not overcome the objections raised by Cartwright and Suárez to using model-theoretic techniques in the philosophy of science. However, my concerns arise independently of the more controversial assumptions that Cartwright and Suárez have employed.

1. Introduction. The publication of da Costa and French's *Science and Partial Truth: A Unitary Approach to Models and Scientific Reasoning* (da Costa and French 2003) offers an important chance to evaluate the success of what they and their collaborators call the partial structures program in the philosophy of science.¹ As the title of the book suggests, the goal is to articulate a unified framework within which the central philosophical issues surrounding scientific reasoning can be clearly debated and perhaps even resolved. Two central examples are their openness to both realist and constructive empiricist interpretations of the goals of science and their attempted explanations of data from the history of science, especially those surrounding Kuhn's work and the pessimistic metainduction.

At the center of the partial structures program is a rather innocuous sounding generalization of model-theoretic structures. Traditionally, a

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1. I also draw on French and Ladyman 1997, French and Ladyman 1998, and French 2003.

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model or structure is an ordered n -tuple, where the first position is a set of individuals, known as the domain of the model, and the other positions are occupied by the properties and relations defined on this domain. The approach is of course thoroughly extensional, so that, for example, a two-place relation is just the set of ordered pairs of objects drawn from the domain that stand in this relation. A partial structure is a different type of set-theoretic object that reduces to these more traditional structures as special cases. Unlike a traditional or total structure, in a partial structure each property and relation is identified with an ordered triple $\langle R_1, R_2, R_3 \rangle$. For a two-place relation R an ordered pair is in R_1 when that ordered pair stands in the relation R , the ordered pair is in R_2 when it does not stand in the relation R , and, finally, being in R_3 indicates that it is “left open” (da Costa and French 2003, 19) or indeterminate whether or not the ordered pair stands in the relation R . Any given pair from the domain of the partial model must be in exactly one of R_1 , R_2 , or R_3 (and similarly for properties and other kinds of relations). In the limiting case where each R_3 is empty, a partial structure becomes a total structure by replacing the ordered triples $\langle R_1, R_2, R_3 \rangle$ by R_1 .

It is this widened perspective that puts partial models in place of total models that forms the backbone of the unified framework for the philosophy of science championed by the partial structures program. In particular, da Costa and French advocate the use of partial models in investigations of the dynamics of rational theory change over time as well as in answering questions about what a scientific theory says about the world at a given time. In the former diachronic case, the partiality of partial structures is exploited to indicate how our knowledge can change over time. Thus, when focused on these historical issues, da Costa and French think of R_1 as representing those pairs where we know R to obtain, R_2 where we know R not to obtain and, crucially, R_3 to give those pairs where we are as yet unsure. In the synchronic case of what a theory's commitments are at a given time, partial models are useful in indicating what the theory remains silent about. So, for a given domain, my theory may state that R obtains between some pairs, does not obtain between others and remain silent about the remaining pairs. Such incomplete and even inconsistent theories fall within the purview of the partial structures framework.

This flexibility and broad scope is of course to be commended. Still, its very ambitiousness leaves the entire project open to some very serious objections. Da Costa and French clearly take as one of their targets Cartwright's approach to the philosophy of science, but it is far from clear that they have the resources to deal with the problematic cases of

scientific representation that Cartwright has herself focused on.² These cases involve both abstract mathematical laws and highly idealized scientific models. Cartwright and Suárez, though in quite different ways, have argued that in such cases any straightforward set-theoretic approach to representation is bound to fail.³ In its place, Cartwright has offered an involved picture of local and constrained ‘mediating’ causal models. Most controversially, she has argued that because our theoretical laws apply only to situations via these models, we have no good reason to think that our laws are universal in scope.

I believe that da Costa and French are right to reject many of Cartwright’s more pessimistic conclusions about the scope of scientific results. At its worst, they involve a kind of Aristotelian posit that the physical world is too messy for mathematics, and so mathematics can only apply if we distort the physical world in an unnatural way.⁴ This said, I want to defend here what I take to be the core strength of Cartwright’s observations against the criticisms of da Costa and French. Da Costa and French claim that an account of representation in terms of partial structures is sufficient to handle cases with abstract mathematical laws and idealization. This is not the case. By arguing for this conclusion it should be clear that I am not arguing that Cartwright’s positive account of representation in these cases is correct. Instead, I am only agreeing with her that such cases pose a serious problem for our understanding of scientific representation.

2. Structures. I begin with a review of how the traditional total structure approach to such cases fails. Consider, for example, a model of a simple pendulum. Such a system is often treated as a simple harmonic oscillator, and so it is said to obey the equations (Thornton and Marion 1995, 114):

$$\theta(t) = A \sin(\omega_0 t - \delta), \quad (1)$$

$$\theta'(t) = A\omega_0 \cos(\omega_0 t - \delta), \quad (2)$$

$$\theta^2/A^2 + \theta'^2/(A^2\omega_0^2) = 1. \quad (3)$$

2. See, for example, Cartwright 1999; Morgan and Morrison 1999. See Herfel et al. 1995 and Shanks 1998 for further discussion of some of these issues.

3. Mauricio Suárez has offered the most sustained treatment of these issues to date. See Suárez 1999, 2003, 2004. His disagreements with Cartwright 1999 are most clearly stated in Suárez forthcoming.

4. Here I draw on McMullin 1985.

In the phase space with axes θ and θ' , equation (3) restricts the paths of the pendulum to an ellipse determined by the length of the pendulum, the gravitational constant and the initial state of the pendulum at t_0 . For a specific case our model might just be a selection from the domain of possible states at times, (θ, θ', t) , in which the initial state of the pendulum is included as well as all other states that fit with it in accordance with equation (3). In order to incorporate this kind of model into the framework already given, we could say that these states are all *SHO*-related by the complex relation set out in equation (3). Our most concrete model would then consist of the domain of possible states and one relation containing all those states that are in the *SHO*-relation to our initial state.

Clearly, this sort of concrete model is not likely to be too useful or enlightening. In its place, we work with more general models that include a wider variety of possible cases in their scope. This is easily accomplished by including in our total model not only the trajectory that is *SHO*-related to the initial state of that pendulum (which I will call a *SHO*-chain), but also the trajectories for all possible initial states. That is, for every state of the form $(\theta, \theta', 0)$, our model will have an appropriate *SHO*-related trajectory. Equation (3) pares down the model from all the logically possible state-time pairings or trajectories to only those that satisfy the complex relation imposed by the equation. For example, no trajectory in the model will have the pendulum at $\theta = 0$ and $\theta' = 0$ at $t = 0$ and $\theta = 1/2$ and $\theta' = 0$ at $t = 1$. Rather, in a set with $\theta = 0$ and $\theta' = 0$ at any time, all other entries must be $\theta = 0$ and $\theta' = 0$ in order to be in the model.⁵

By now most philosophers of science would agree that this focus on a nonlinguistic entity, the model, is an improvement on the 'syntactic' focus on statements in a specific, usually artificial, language. But shifting our attention to models is not a panacea. In particular, once we think of theories as collections of models it becomes much harder to say in virtue of what a theory represents a given situation or state of affairs. In our above example we *say* that our model is a model of a simple pendulum, but it is harder to spell out why this is the case. At least the syntactic view could draw on a more general story about how sentences represent states of affairs. We seem to have less to go on with models.

Pressures arise from two directions when we try to say what our models represent. On the one hand, we are inclined to tie a model to a situation by putting the relevant constituents of the situation in the model itself. We put the possible states of that pendulum in the domain of the model and this seems to easily explain why the model is about that situation

5. For simplicity, I ignore the fact that (3) breaks down in this case. I here assume that the units of θ are radians and time is given in seconds.

and what it 'says', as it were, about it. On the other hand, our models are not just models of particular physical systems or even physical systems of a definite type. Simple harmonic oscillators are found throughout nature, including spring systems, strings, and even some electromagnetic phenomena (Thornton and Marion 1995, 131). So if we are to use the same model to represent all of these various types of systems, we cannot get by with a model whose domain is too concrete, e.g., whose constituents are possible physical states of the pendulum in my lab or even of an arbitrarily chosen pendulum.

This tension is resolved by making our models more abstract and usually this is accomplished by making our models wholly mathematical. That is, in the domain we will not have possible states of the pendulum, but some mathematical substitute. In this case the substitute will be an ordered triple of real numbers, with the first entry standing for the physical magnitudes θ , the second for θ' and the third for time. Such mathematical models are trivially available if we assume a rich enough background mathematical theory. At the same time, something new must be said about why this mathematical model represents our pendulum. That is, the abstractness of the model complicates our account of its representational powers. If we are to preserve the generality of the model, we also need to leave open the possibility that the very same model represents a diverse array of concrete, physical systems.

At this point the advocate of total structures can appeal to the existence of isomorphisms from the physical state of affairs we are trying to represent to the elements of the model. Isomorphisms establish identity of structure, and this is all that we need to cash out our representational claims. Which isomorphisms exist needs to be specified in some way or else all models will represent all situations that have the right cardinality. So, again to revert to our example, we would require that the isomorphism map the angles of pendulum displacement to θ and the rate of change of such displacement to θ' at a given time. This specification, and not merely the description of the mathematical model, is required for the model to meaningfully represent the pendulum. Other physical magnitudes are involved for other types of systems. For example, in a simple spring, the distance of displacement from equilibrium is at issue, not the angle θ .

Right away we have a problem because the above approach conflates representational power with accuracy. We want some models to be false, i.e., to represent a system inaccurately. But if we tie whether a model represents at all to the existence of isomorphisms of the appropriate kind, then whenever a model represents it will also represent truly. What, then, is the difference between an inaccurate model and a model that fails to represent at all? One option is to bring in the intentions of the agents who employ the model. On this approach representational powers do not

flow simply from the model itself, but result instead from how the model is used. Then, we can say that the model represents a situation when agents use it to understand the properties of that situation because they believe that the right kind of isomorphism exists. Accurate or true representations arise when these intentions are in place and when the required isomorphism actually exists. In those cases where we take there to be an isomorphism, but in fact there is no isomorphism, we have an inaccurate or false representation.

Here I must disagree with Suárez when he claims that model-theoretic approaches generally, and the partial structures program specifically, cannot appeal to intentions of agents because they are attempts to ‘naturalize’ representation. On the contrary, da Costa and French often appeal to heuristics that scientists use to pick out the structures they are interested in. They offer no formal reduction of these heuristic techniques and it seems fair to include the intentions of scientists in the range of resources that a partial structures program can employ.⁶

3. Idealization. This proposal is considerably complicated by the existence of idealizations. In this paper I will call a mathematical model an *idealized* model when it satisfies two conditions: (i) there is no isomorphism relating it to the situation that it purportedly represents and (ii) the relevant agents are aware of this. Based on what I have said so far it is impossible for an idealized model to represent accurately. First, the agents are aware that there is no isomorphism of the required kind, so the model does not even represent the situation. Second, even if this problem can somehow be overcome, the actual lack of an isomorphism means the model will automatically be inaccurate or false. Here we are confronted with a mismatch between this proposal and scientific practice. Idealized models are often used to represent situations, and while scientists stop short of calling such models true, there is still a clear sense in which some idealized models are better representations than others. Our pendulum case is in fact a case in point: our actual pendulum is not a simple harmonic oscillator. It is dampened at least by air resistance and friction. Furthermore, in order to derive our equations (1)–(3), we have made use of the *small oscillation assumption*, which tells us that $\sin \theta \approx \theta$, for small θ (Thornton and Marion 1995, 110–111). Both types of idealizing assumptions imply that no trajectory in the model will be isomorphic to the actual trajectory. The crux of the problem, though, is that we seem to

6. Suárez 2003, 226, 235, 240; da Costa and French 2003, 28–36, 124. Suárez develops other concerns about isomorphism-based accounts of representation that I cannot address here.

have no other resources beyond isomorphisms to explain how abstract models represent.

It is precisely here that partial structures are supposed to help. Rather than requiring complete identity of structure between a total model and a situation, we loosen our requirements on representation to allow partial identity of structure between partial models and a situation. Partial models are defined as above and ‘partial identity of structure’ is spelled out in terms of the existence of partial isomorphisms. For partial models $A = \langle A, R_i \rangle$ and $A' = \langle A', R'_i \rangle$,

A is partially isomorphic to A' when a partial substructure of A is isomorphic to a partial substructure of A' . The notion of a partial structure (or substructure) is so conceived that a total structure (or substructure) constitutes a particular case of a partial structure (or substructure). In other words, we can say that, with regard to a partial isomorphism, certain of the R_i —some subfamily—stand in a one-to-one correspondence to certain of the R'_i . (da Costa and French 2003, 49)

When it comes to idealized models, then, we can still say that the idealized model of the pendulum represents the pendulum because there will be a partial isomorphism between the situation and the model (da Costa and French 2003, 102).

As with isomorphisms, it is important to note that even in highly idealized cases, partial isomorphisms will trivially obtain. Presumably, though, it is not supposed to be trivial for there to be a partial isomorphism of the appropriate sort. Similarly, in order to allow for inaccurate models, we can invoke the intentions and beliefs of agents using the model. When these agents suppose there to be an appropriate partial isomorphism, the model will represent the situation. And, even when idealization is involved, when a partial isomorphism really exists, then the model represents accurately or truly to some degree.

Partial isomorphisms bring greater flexibility, but also a greater danger of trivializing our representational relationships. To present the problem in an especially vivid way, I introduce what I will call the full model F . F has as its domain the ordered triples of real numbers that go proxy for the possible states of a pendulum (θ, θ', t) . F is a full model because all pairs definitely do stand in its one relation $R - F$. Now for any trajectory of an actual pendulum, we can find partial isomorphisms from the trajectory to F . One of these partial isomorphisms will relate the right physical magnitudes to a trajectory in the model, but it seems like the full model does not represent the pendulum in any interesting way. This shows that once we allow partial isomorphisms it is no longer sufficient for a model to represent for it to permit a mapping whose domain is restricted

to the appropriate physical magnitudes. Additional constraints are needed in specifying what the *range* of the mapping can be. Intuitively, we disqualify this model because it takes no risks: however the actual pendulum moves, we could find its trajectory in the model. This suggests a way to disqualify the full model. When we have equation (3), we give the clearest kind of description possible of the mathematical structure of the pairs that fit in the model. In the full model, we have no description of what the range of the mapping will be. We rule out the full model, then, if we demand that we have some means to restrict what is in the range of the mapping. Ideally this is done using a mathematical equation, but other more qualitative means are no doubt also sometimes satisfactory. Without this additional constraint we are forced to conclude that the full model represents not only any pendulum, but any system with three physical magnitudes measured by real numbers. More of course should be said about how restrictive our requirements on the range must be in order for a model to represent. Perhaps a 'half model' would not go far enough. Still, something like this constraint on the mappings is needed for the partial structures program to have a chance of success. To date I do not believe da Costa and French have addressed this problem, although they may think of it as subsumed under their informal treatment of heuristics guiding scientific investigation.

4. A Problem. Assuming all of these restrictions are in place, a more fundamental problem still arises. The partial structures program is forced, I will argue, into thinking that all idealizations are treatable as a series of approximations. This leaves them unable to rank or evaluate idealizations in a way consonant with scientific practice. Approximation, even when shifted to the vocabulary of the partial structures program, is too coarse an instrument to discriminate among idealizations.

Reconsider the case where we had a total mathematical model of a simple pendulum and we failed to cash out its representational power in terms of an appropriate isomorphism with a concrete physical pendulum. In order to introduce an analogous partial structure, imagine that we take the equation to pick out not our original model, but instead a partial model that acknowledged that the equation was only giving a partial picture of what was going on in the actual world. There are many ways this might be done. I will focus on just one because it seems that any alternative will lead to essentially the same problem. According to this procedure, we include not only the *SHO*-chains that are exactly related to each (θ, θ', t) , but also allow in chains where there is some limited variation. To include these trajectories we might just replace the '1' on

the right hand side of equation (3) by ‘ $1 \pm \delta$ ’, where δ was a contextually determined term reflecting the looseness of the idealization:

$$\theta^2/A^2 + \theta'^2/(A^2\omega_0^2) = 1 \pm \delta. \quad (3\text{-APP})$$

So, for example, *SHO*-linked to $(0, 0, 0)$ would be not only all triples $(0, 0, t)$ for all t , but all those trajectories where the values of θ and θ' fell within the boundaries set by δ . As it stands the model is not yet partial, but this could also be introduced by stipulating that, in some area around these boundaries, it is indeterminate whether or not the states form a *SHO*-chain. A partial isomorphism will exist between the actual situation and this model just in case the image of the actual trajectory is caught by our more generous selection from all the possible trajectories.

Here we have found a way to incorporate more trajectories than the ones that exactly satisfy equation (3), but have stopped short of the full model F by using (3-APP) to specify what the range of our mapping must look like. Do we have the resources needed to give an account of idealizations using these sorts of approximating techniques? I claim the answer to this question is *no*. When constructing and evaluating idealizations we are interested in *more* than what this analysis of idealization says we are. To see this consider the model picked out by our more generous equation (3-APP). Instead of picking out ellipses in our phase space, we will instead get a series of tubes in the phase space of width determined by our error term δ . If the actual trajectory of the pendulum begins and ends in one such tube, then we will have a partial isomorphism between the system and our model. The problem is that it is possible to pick out these same tubes using a different and much less satisfactory series of idealizations. Consider the exact equation:

$$\theta^2/A^2 + \theta'^2/(A^2\omega_0^2) = 1 + \delta. \quad (3\text{-}\delta)$$

A trajectory that satisfied (3- δ) would be an ellipse whose major and minor axes were δ units too large. This exact equation with different idealizing assumptions could lead to the very same partial model constructed using (3-APP). To see this suppose we allowed only approximations ‘inward’, up to a threshold of 2δ and set as indeterminate any trajectories in the immediate neighborhood of the resulting tubes. While in such a case we again end up with a partial isomorphism, there is something defective about this sort of idealization, both in its original equations that deliver the ‘widened’ trajectories and in its approximating techniques.

I intend this case as a counterexample to the claim that idealization can be suitably analyzed using the tools that the partial structures program has employed to date. What is needed is some way of disqualifying (3- δ). Given that it generates the same model as our (3-APP), it is hard to see how to do this. If we just focus on the models, we only have the

resources to discriminate proposals based on the trajectories we end up with. In cases of idealization, this pushes the proposal into a rather unsatisfactory version of an instrumentalist approach to models. We are not allowed to investigate *how* the class of trajectories was picked out. But as our example shows, this can be crucial when evaluating idealizations. (3-APP) was arrived at by a series of supposedly well motivated assumptions: the forces due to friction and air resistance are ignored and the small angle substitution is used to replace $\sin \theta$ by θ . (3- δ) might have resulted from some bizarre new ‘hidden variable’ proposal or it might be completely unmotivated, and this is why we want to rule it out or at least rate it very low as an idealization.⁷

My objection began with the claim that the partial structures program is forced into thinking of idealizations as approximations. Da Costa and French might challenge this assumption and go on to offer a different way of accounting for the ‘looseness of fit’ in this case and others. I concede that there are certainly other ways to think of the relation between a simple harmonic oscillator model and a concrete system like our pendulum. For example, one might stick with the exact equation (3) and loosen our restrictions on what physical magnitudes the partial isomorphism must respect. That is, we loosen up the isomorphism, and not the model, when we countenance idealizations.

I claim, however, that all such alternative strategies fail to distinguish well motivated from poorly motivated idealizations when the two idealizations agree on the models or isomorphisms at issue. In particular, there will be many ways to pick out the looser kind of isomorphism required on our second suggestion, and only a few of these ways will be related to acceptable idealizations. The problem remains that the partial structures program only has the resources to focus on the results of these moves.

What we see, then, is that we cannot ignore how or why a mathematical model is introduced if we are to give a reasonable account of representation and accuracy in terms of partial isomorphisms. In particular, our judgments about the appropriateness of an idealized model depend not just on the trajectories that we end up with, but how we cooked up these particular trajectories in the first place. Cartwright would point to the ways in which (3-APP) gets the causal story right and how (3- δ) is a miserable failure in these respects. For her, this implies that models are not abstract, mathematical entities, but rather a new type of entity that mediates between our abstract mathematical theories and the messy physical world.

7. McMullin 1985 also emphasizes the need for idealizations to be well-motivated.

5. Conclusion. Da Costa and French might respond by bringing in elements of their partial structures program that I have not yet mentioned. They often invoke a hierarchy of wholly mathematical models. At the bottom of this hierarchy are the data models that encode the observed data from experiments. Above this we find increasingly rigorous and general models that encode the more general laws appropriate for that kind of situation. At the top it appears that we are meant to find models satisfying the fundamental equations of our current best theory. The issue becomes one of fitting our (3-APP) into a hierarchy of models that terminates in the data model for this situation and of giving principled reasons for excluding deviant cases like (3- δ) from an analogous hierarchy. As before, each link in this hierarchy is accomplished by appropriate partial isomorphisms between partial models.

The example I have focused on this paper is of course a low-level one, and so we might imagine a simple hierarchy with three levels: the data model, the simple harmonic oscillator model and the theoretical model for Newtonian classical mechanics. Given that (3-APP) and (3- δ) are the very same model, I do not see how any model-theoretic relations to other models can distinguish the two. There will be partial isomorphisms from (3-APP) ‘upwards’ to the theoretical model or ‘downwards’ to the data model just in case the same mappings obtain for (3- δ). Even though we have seen that the intentions of agents must be appealed to in our analysis of representation, these appeals do not distinguish the two cases either. The same magnitudes are in the domain of the mappings and the ranges are both restricted in the way that we saw was required to rule out the full model.

In conclusion, then, it seems clear that something more is needed, but I fail to see what more da Costa and French could appeal to without making their models concrete. This shows that the partial structures program as it currently stands cannot give an adequate account of abstract and idealized models. Of course, it does not show that the only or best way to resolve these issues is to invoke, along with Cartwright, causes or capacities.

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