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Stud. Hist. Phil. Sci. 37 (2006) 370–377
Studies in History and Philosophy of Science
www.elsevier.com/locate/shpsa

Essay review

Scientific models, partial structures and the new received view of theories

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Science and partial truth: A unitary approach to models and scientific reasoning
Newton da Costa and Steven French; Oxford University Press, Oxford & New York, 2003, pp. x + 259, Price $55.00 £32.99 hardback, ISBN 0-19-515651.

1.

For almost two decades, Newton da Costa and Steven French have led a research programme that has explored a number of interrelated issues in philosophical logic, metaphysics and philosophy of science. The publication of *Science and partial truth* represents the culmination of this programme and an ideal occasion to assess its success.

Science and partial truth is ideally divided in two parts. In the first part, which consists of the first three chapters, da Costa and French introduce the notions of partial structures and partial truth and illustrate how they can provide philosophers of science with a unified framework in which to think about scientific theories and scientific models. The first chapter is almost entirely dedicated to presenting the notion of partial structure and the related notion of partial truth, which attempts to capture the intuitions that underlie the conceptions of truth held by pragmatists such as Charles Sanders Peirce and William James.

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doi:10.1016/j.shpsa.2006.03.007

Unlike a total structure, a *partial structure* is a structure in which n-ary relations are not necessarily defined for all n-tuple of objects in its domain. In other words, beside the set of ordered n-tuples of objects among which a certain n-ary relation holds and the set of n-tuples among which it does not hold, there may be a third set of n-tuples for which it is undetermined whether or not the relation holds. A total structure *extends* a partial structure if they have the same domain and, for any n-ary relation, the set of ordered n-tuples among which a certain n-ary relation holds in the partial structure and the set of ordered n-tuples among which the relation does not hold in the partial structure are, respectively, a subset of the set of ordered n-tuples in the total structure among which the relation holds and the set of ordered n-tuples in the total structure among which the relation does not hold. Finally, a sentence is *partially true* (in a partial structure) if it is true in some total structure that extends that partial structure.

The second chapter outlines the reasons underlying the demise of the syntactic view of theories and discusses the many versions of what today can be considered its main successor: the semantic view. Semanticists maintain that the syntactic view conflates scientific theories with their linguistic formulations. According to the semantic view, a theory is better represented as the set of models (or, more precisely, set-theoretic structures) that satisfy the different linguistic formulations of the theory.

In the third chapter, da Costa and French turn to the problematic relationship between what semanticists and logicians refer to as models (namely, set-theoretic structures) and what scientists and most philosophers of science refer to as models (namely, objects such as Bohr's model of the atom and Crick and Watson's model of the structure of DNA). Da Costa and French maintain that scientific models *can be represented as* set-theoretic structures, while they leave open the question of whether or not they *are* set-theoretic structures. More specifically, da Costa and French claim that models should be represented as partial structures because partial structures can accommodate abstract, approximate and idealized aspects of models.

In the remaining chapters, which form the second part of the book, da Costa and French show how it is possible to use the framework outlined in the first three chapters to solve many problems in philosophy of science and epistemology. The fourth chapter, for example, discusses the relation between acceptance of a theory and belief in its truth. In it, da Costa and French maintain that acceptance of a theory does not imply belief in its truth but only belief in its pragmatic truth. Supposedly, this would provide an explanation as to how theories that are strictly false (such as classical mechanics) or even inconsistent (such as Bohr's 'theory' of the atom) can be still accepted within certain domains: in those domains, these theories are accepted as pragmatically true (where 'x is pragmatically true' means 'some of the consequences of x are true in the correspondence sense'). The issue of inconsistent theories is examined more extensively in the fifth chapter, which explains how scientists can accept contradictory theories without entertaining inconsistent beliefs. This is achieved, again, by dissociating acceptance of a theory from belief in its truth in the correspondence sense and by adopting a form of para-consistent logic. A scientist can, thereby, believe a contradiction (say, believe 'p and not p') without having inconsistent beliefs (i.e. believing 'p' and not believing 'p'). By distinguishing belief that not-p from belief that not-p is true in the correspondence sense, one can deny that believing that *not-p* implies *not* believing that p. In this way, one can prevent the inconsistency to spread from a single belief to the whole set of beliefs.

The remaining chapters touch upon numerous other issues, which cannot be adequately addressed within the scope of this essay. In the sixth chapter, da Costa and French discuss how partial structures can accommodate the heuristic role played by models and theories in scientific practice. The seventh chapter presents us with the outline of a theory of 'pragmatic' induction. The eighth chapter outlines the realism/anti-realism debate in philosophy of science and shows how the partial structure framework can be used successfully by both sides of the debate as exemplified by the structural realism of James Ladyman and the structural empiricism of Ottavio Bueno.

2.

Da Costa and French's undertaking is ambitious in many respects. *Science and partial truth* touches upon a great variety of issues that are central to philosophical logic (pragmatic conceptions of truth, partial truth and partial structures), philosophy of science (the nature of theory and models, induction, the realism/antirealism debate), and epistemology (belief and acceptance). The book also attempts, although I think unsuccessfully, to settle some important, unresolved issues that haunt the so-called semantic conception of theories, such as the relation between theories and models and the relation between models and structures. The book also raises some crucial questions that have been inexplicably neglected in the literature (for one, the question of the ontological status of scientific models, which is repeatedly raised but, regrettably, not addressed).

Moreover, as the subtitle of the book suggests, da Costa and French intend to develop and defend a unitary account of scientific models, a project that is particularly ambitious in a time when most philosophers of science seem to believe that no single account of scientific models can do justice to their variety.

The ambitiousness of the project is commendable and, in some respects, *Science and partial truth* is successful. In particular, the reader can find here one of the most thorough and reflective discussions of the semantic conception of theories and of the realism/anti-realism debate in the literature. However, I find the overall result unconvincing. This is mainly due to the fact that da Costa and French fail to persuade me as an unconverted reader that the twin notions of partial structures and partial truth provide a framework for philosophy of science in general and for scientific models in particular. The success of the whole enterprise crucially hinges on whether the framework laid out in the first part of the book is appropriate and, in particular, on whether models can be suitably represented as partial structures. There seems to be good reasons to doubt that this is the case.

Da Costa and French, like most fellow semanticists, assume that scientific models can be represented as set-theoretical structures. This assumption, however, is, to say the least, doubtful and da Costa and French's attempts to dispel these doubts are largely unsuccessful. A first, well-known concern stems from the problem that set-theoretic structures are not rich enough to represent scientific models. A crucial characteristic of set-theoretic structures is that the properties and relations within a structure are purely extensional. In other words, two properties or relations are the same if they have the same extension. If we were to represent models as structures, there would be cases in which *different* models would be represented as the *same* structure (or, more precisely, two isomorphic structures).[1] Consider Bohr's model of the atom for example. If we were to represent Bohr's model of the atom as a structure, we would use the structure $\mathrm{A} = \langle \mathrm{A}, R_1^1, R_2^1, R_3^1, R_4^2, R_5^2 \rangle$, where the only two elements of A, a and b, designate, respectively, the nucleus and the electron, which are the only two objects in the domain A, R_1^1, where the superscript indicates the n-ariety of the relation, designates any property that the nucleus has but the electron does not have (e.g. the property of *being positively charged*); R_2^1 designates any property that the electron has and the nucleus does not have (e.g. that of *being negatively charged*); R_3^1 designates any property that both the nucleus and the electron have (e.g.: that of *being charged*). R_4^2 is any relation that holds between the nucleus and the electron (e.g.: that of *being attracted to*) and R_5^2 is any relation that holds between the electron and the nucleus (e.g. that of *being more massive than*). Consider now a completely different model: the inclined frictionless plane model from Classical Mechanics. In the model, a box stands on an inclined frictionless plane. Now, if we were to represent this model as a structure, we would represent it as the structure $\mathrm{A}' = \langle \mathrm{A}', R_{1'}^1, R_2^{1'}, R_3^{2'}, R_4^{2'}, R_5^{2'}, a', b' \rangle$, where a' designates the plane and the constant b' designates the box; $R_1^{1'}$ designates any property that the plane has but the box does not have (e.g. the property of *being frictionless*), $R_2^{1'}$ is any property that the box has and the plane does not have (e.g. that of *having mass*), $R_3^{1'}$ any property that both the plane and the box have (e.g. that of *being inclined*); $R_1^{2'}$ is any relation that holds between the plane and the box (e.g. that of *being under*) and $R_2^{2'}$ is any relation that holds between the plane and the box (e.g. that of *being on*).

Thus, if we were to represent models just as structures, we would be unable to distinguish between what we usually take to be two completely distinct models. This suggests that scientific models cannot be adequately represented *just* as set-theoretic structures.

3.

Even if we concede for the sake of the argument that models can be represented as set-theoretic structures, partial structures do not fare significantly better than total structures at representing models. In particular, partial structures can only accommodate a very specific manner in which models misrepresent their target systems. Philosophers of science usually distinguish three main sources of unfaithfulness in models: abstraction, approximation, and idealization.[2]

Every case in which a property of an object in the system has no counterpart in the model that represents the system is a case of *abstraction*.[3] For example, in any real

[1] Strictly speaking, the two models are represented by two isomorphic structures. However, this does not seem to solve the problem. One important aspect of the research programme led by da Costa and French is that a model represents a certain system in virtue of the model exemplifying a structure that is partially isomorphic to the structure exemplified by the system (see, e.g., French & Ladyman, 1999). However, if Bohr's model of the atom represents the hydrogen atom in virtue of the fact its structure is partially isomorphic to the structure of the hydrogen atom, the inclined plane model would represent the hydrogen atom as well. Since the structure of the inclined plane and that of Bohr's model of the atom are isomorphic, the structure of the inclined plane is partially isomorphic to the structure of the hydrogen atom. Insisting that the two models are represented by distinct isomorphic structures would not seem to strengthen da Costa and French's position.

[2] See, for example, Cartwright (1983) and McMullin (1985).

[3] For the sake of simplicity, in what follows, I only talk of properties and not of relations. However, the same remarks apply, *mutatis mutandis*, to relations.

pendulum, the bob is made of some material or other. However, the ideal pendulum model abstracts from the material the bob is made of. We could say that the bob of the ideal pendulum is neither made of brass nor not made of brass.[4] Partial structures can easily account for abstraction. In the partial structure that represents the model, it is undetermined whether or not the bob exemplifies the property *being made of brass*, while, in the total structure that represents the system, the pendulum either exemplifies that property or not.

Every case in which a quantitative property of an object in the system is substituted in the model by a property that has an approximately similar value is a case of *approximation*. For example, the gravitational acceleration experienced by the bob of the ideal pendulum is often set as $9.8\ \mathrm{m/sec}^2$, which only approximates the gravitational acceleration experienced by the bob of any real pendulum. In other words, the gravitational acceleration experienced by the bob in the model is (slightly) different from the one experienced by the bob of a real pendulum. Partial structures cannot account for approximation as well as they account for abstraction. Unlike abstraction, the problem with approximation is not that a certain object in the model neither exemplifies a certain property nor the complementary property. Rather, the problem is that, whereas the object in the model exemplifies a certain quantitative property, the corresponding object in the system exemplifies one with a slightly different value. Thus, partial structures do not have better resources than total structures to accommodate approximation.

Every case in which an object in the model exemplifies a property that is known to have no counterpart in the system (and often it is not exemplified by anything in the world) is a case of *idealization*. For example, in the ideal pendulum model, the string is inextensible. However, since no real string is completely inextensible, this property of the string has no counterpart in a property of the string of any pendulum in the real world. On the contrary, the string in any real pendulum exemplifies the complementary property of *being extensible*. Partial structures do not seem to have the resources to accommodate idealization. The problem with idealization is not that it is undetermined whether or not a certain object in the model exemplifies a certain property but that an object in the model has a property while the corresponding object in the real system has the complementary property.

Da Costa and French repeatedly claim that approximation and idealization can be accommodated by introducing another crucial notion, that of *partial isomorphism* (see, for example, pp. 102, 122). Unfortunately, the notion of partial isomorphism as defined in the book (p. 49) is too vague and the explanation of how this notion is supposed to accommodate approximation and idealization is unclear. One can therefore only speculate how partial isomorphism is supposed to accommodate approximation and idealization. Da Costa and French define a partial isomorphism between A and B as an isomorphism between two partial substructures A and B, where, presumably, A′ is a substructure of A if and only if the domain of A′ is a subset of the domain of A and A extends A′. Consider again the ideal pendulum case. There are some partial substructures of the structure that represents the model in which it is undetermined whether or not the object that represents the string from which the pendulum hangs has the property of *being inextensible*. There are also some partial substructures of the structure that is exemplified by the real pendulum in which it is undetermined whether or not the string has the property of *being inextensible*.

[4] To use a terminology often used in regards to fictional objects, we could say that models are incomplete objects (Parsons, 1980).

And, plausibly, some of these partial substructures are isomorphic. If I interpret da Costa and French's intentions correctly, it is in virtue of the partial isomorphism between these two partial substructures that a system can be represented by a model that contains idealizations and approximations.

The problem is that partial isomorphism seems a very weak condition for a model to represent a system. It is actually a condition that is trivially met by any model for any system. In fact, any structure has as a partial substructure a limit case of partial structure: the structure $A^\circ = \langle A^\circ, R_1^{1^\circ} \rangle$ whose domain A° contains only one individual, a°, for which it is undetermined whether or not the property $R_1^{1^\circ}$ holds. All these partial structures are trivially isomorphic to each other. Any two structures, thus, are partially isomorphic. Therefore, if any model and any system exemplify partially isomorphic structures and if any model that exemplifies a structure that is partially isomorphic to a structure exemplified by a system represents that system, then any model represents any system.

This disastrous consequence does not seem to follow just from the particular definition of partial isomorphism found in da Costa and French's book. Similar consequences seem to follow whenever one attempts to accommodate *both* idealization *and* abstraction by means of a morphism between the structures exemplified by a model and that exemplified a system. Idealization and abstraction pull in different directions: idealization occurs when there are properties of objects in the model that do not have any counterpart in the system, while abstraction occurs when there are properties of objects in the system that do not have any counterpart in the model. Any formal relation between two structures that allows for both is a necessarily weak condition to be met.

4.

The above considerations cast serious doubts on the prospects of success of an account of scientific models that represents them as partial structures and, given the centrality of this notion to the programme led by da Costa and French, these doubts reflect on the whole enterprise. However, this is not the only cause for concern. Throughout *Science and partial truth*, a tension between the non-technical nature of the book and the inescapably technical nature of some of the notions that play a crucial role in it emerges over and over again. As a result, many crucial notions are vaguely defined and many readers will have to refer to more technical works by da Costa, French and their collaborators in order to fully understand what da Costa and French's positions on many issues are.

For one, the notion of partial truth (as defined on pp. 18–19) does not seem to be able to fulfill the different functions that da Costa and French want it to. The less indulgent reader is likely to suspect that 'partial truth', 'quasi-truth', 'pragmatic truth' and the plethora of other related expressions that can be found throughout the book are used rather liberally to mean different things in different contexts.[5] For example, if one wants to talk of classical mechanics being true in some sense or other, they could argue that some of the consequences of the theory are still accepted as *approximately* true in some domains (whatever one means by 'approximately true'), but not that it is *partially* or *pragmatically* true. If there is any problem with classical mechanics (and, as I will argue later, I doubt there is), the problem is not that it is incomplete (i.e. that its models offer a partial representation of the world). In fact, all representations are intrinsically partial. Nor is the problem that it is

[5] For a more rigorous definition of pragmatic truth, see Mikenberg, da Costa, & Chuaqui (1985).

pragmatically true (in the sense specified by da Costa and French that some of its consequences are true in the correspondence sense). The problem is that all of its consequences are, strictly speaking, false or, if one prefers, that only some of its consequences are approximately true.

What is probably even more concerning is that, by exploiting the vagueness of the notion of partial truth and the distinction between an extrinsic and an intrinsic characterization of models and theories, da Costa and French ultimately promote a conservative agenda for philosophy of science. Despite much talk of models and representation, philosophy of science, as envisaged by da Costa and French, continues, in fact, to be mainly concerned with theories and their truth. Da Costa and French, I think, underestimate the more profound implications of framing problems in terms of models and representation and, as a consequence, fail to engage with these implications.

When framed in these terms, many of the problems that da Costa and French consider seem to disappear. For example, in this framework, it seems easy to explain why models from classical mechanics are still used to represent systems in the world even after more fundamental theories such as special relativity, quantum mechanics, and quantum field theory have emerged. Like any other representation, a model can represent its object more or less realistically and more or less faithfully, but no model, whether based on classical mechanics or relativistic quantum mechanics, is ever a perfect replica of what it represents. Therefore, the difference between models based on classical mechanics and those based on some theory that we deem more fundamental is a matter of degree, not of kind. In other words, when we frame the problem in terms of models and representation, we do not need an explanation of why models based on classical mechanics are *still* used more than we need an explanation of why models based on relativistic quantum mechanics are used.

5.

Over the last few decades, the semantic view has increasingly replaced the syntactic view as the received view of theories. Da Costa and French are a good example of how semanticists are so confident of the status the semantic view has acquired that they do not feel the need to discuss or justify its fundamental tenets. The semantic view, however, has never enjoyed the widespread consensus that the syntactic view enjoyed in its heyday and books like *Science and partial truth*, which preach the gospel of the semantic view mostly to the converted, are unlikely to make proselytes, especially in a time when the discontent towards this new received view seems to be mounting.

Philosophers of science are increasingly realizing that the differences between the syntactic and the semantic view are less significant than semanticists would have it and that, ultimately, neither is a suitable framework within which to think about scientific theories and models.[6] The crucial divide in philosophy of science, I think, is not the one between advocates of the syntactic view and advocates of the semantic view, but the one between those who think that philosophy of science needs a formal framework or other and those who think otherwise. As da Costa and French acknowledge, this decision ultimately depends on whether or not the framework in question helps us to highlight and solve the problems in which we are interested. The framework outlined by da Costa and French

[6] Both Cartwright, Shomar, & Suárez (1995) and Chakravartty (2001) make this point, although on different grounds.

in *Science and partial truth*, I suspect tends to obscure rather than highlight some of the more interesting problems in philosophy of science today.

Acknowledgements

I would like to thank Nancy Cartwright for many useful comments on a previous draft of this essay and the Royal Institute of Philosophy for the financial support while writing this paper.

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