Structural Realism, Scientific Change, and Partial Structures

Abstract. Scientific change has two important dimensions: conceptual change and structural change. In this paper, I argue that the existence of conceptual change brings serious difficulties for scientific realism, and the existence of structural change makes structural realism look quite implausible. I then sketch an alternative account of scientific change, in terms of partial structures, that accommodates both conceptual and structural changes. The proposal, however, is not realist, and supports a structuralist version of van Fraassen’s constructive empiricism (structural empiricism).

Keywords: Structural realism, scientific change, partial structures, realism, constructive empiricism, truth, reference.

1. Introduction

As is well known, scientific change has two important dimensions: conceptual change and structural change. The formulation of a new theory involves the introduction of new concepts to explain empirical phenomena, and this process often leads to substantial reformulations in the old theory’s conceptual framework. As a result, the structures (in particular, the models) used in the formulation of the old theory are replaced by new ones. In this way, conceptual change and structural change go hand in hand. Since the work of Kuhn [1962], this phenomenon became a crucial factor in the agenda of any reasonable account of scientific change.

As is also well known, two types of realism have been put forward to explain scientific change. According to scientific realism, scientific change is accommodated in terms of the search for approximately true theories. The scientific realist acknowledges that there might be some sort of loss when the scientific community shifts from one theory to another. However, well-established parts of the old framework — in particular, reference to certain entities introduced in explanations — are typically preserved (see, for instance, Popper [1963] and Boyd [1990]). From a structural realist point of view, what remains stable over scientific change is not an ontology of entities, but the relevant relations defined over them; in other words,
the structure. It is structure that the realist should be realist about (see Worrall [1989], Chiappin [1989], Zahar [1996] and [1997], Ladyman [1998] and French [1999]).

In this paper, I have two aims. First, I shall argue that these two versions of realism are undermined by the inadequacy of accommodating the two dimensions of scientific change. The existence of conceptual change brings serious difficulties for scientific realism, and the existence of structural change makes structural realism look quite implausible. What is needed, then, is an account of scientific change that does justice to the two dimensions under consideration: conceptual and structural change.

The second aim of this paper is to sketch such an account. In a number of works, da Costa and French provided a new framework to accommodate the partiality of information in science (see da Costa and French [1989], [1990], [1993], and [2003]). Their main idea is to introduce a broader concept of structure (partial structure) and a weaker notion of truth (quasi-truth), appropriate for the “partialness” and the “openness” typically found in scientific life. The resulting account is called the partial structures approach. In terms of this conceptual framework, I shall sketch an alternative account of scientific change that accommodates both conceptual and structural changes. The proposal, however, is not realist, given the difficulties faced by the latter view. And it is compatible with van Fraassen’s constructive empiricism (van Fraassen [1980] and [1989]; see also Bueno [1997] and [1999a]).

2. Scientific realism and conceptual change

In the contemporary philosophical scene, there are several versions of realism (see, for instance, Leplin (ed.) [1984], Churchland and Hooker (eds.) [1985], and Papineau (ed.) [1996]). In this section, I shall focus on standard scientific realism, which can be roughly characterized by the claim that scientific theories are true (or approximately true) and their theoretical terms refer (see, for instance, Popper [1963] and Boyd [1990]).

One of the major motivations for advancing this proposal is that it provides the only explanation of the success of science that doesn’t make it a miracle, or so argues the realist (see Putnam [1979], p. 73). Since the realist takes scientific theories to be true (or approximately so), and given that their theoretical terms refer, it is not surprising that the empirical predictions made on the basis of these theories are so successful. Scientific theories provide an adequate description of reality, “carving nature at its joints”, and so they supply an empirically adequate and at least approximately true account of the world. Any other alternative, which doesn’t assume at least
the approximate truth of scientific theories, will turn the success of science into a miracle.

This is, of course, a major argument in the realism/anti-realism debate, called the “no-miracles” argument. For the scientific realist, it is a major advantage of his or her proposal that it allows one to accommodate so straightforwardly this aspect of scientific activity.

I’ve just mentioned the metaphor of “carving nature at its joints”. The scientific realist takes this talk literally. According to this proposal, there are natural kinds out there to be discovered and appropriately described by scientific theories. And the formulation of an (approximately) true theory depends on the adequate description of these natural kinds.

So the three major components of scientific realism are: (1) an emphasis on the truth (or approximate truth) of scientific theories; (2) the claim that the latter’s theoretical terms refer (to an independent reality); and (3) that such theories adequately describe the natural kinds which constitute the world.

But how successful is scientific realism in the articulation of this view? I think the proposal faces a number of difficulties; some of them have already been indicated in the literature (see, for instance, Laudan [1981]). A difficulty that is particularly pressing derives from the existence of scientific revolutions, which bring both conceptual change and change in the reference of theoretical terms. These two changes are, of course, related. The concepts introduced in a scientific theory are taken (by the realist) to have reference, and any change in the reference is taken as a change in the concepts used to refer to some part of the world.

But why is reference change a problem for scientific realism? Since one of the features of the proposal is that theoretical terms have reference, and (at least some of them) truly describe natural kinds, with a scientific revolution the realist will have to claim that the ultimate furniture of the world has changed. The world is no longer constituted by, say, phlogiston; now it is made of oxygen and other chemical substances. Mass is no longer an intrinsic, frame-independent property; it now depends on the frame of reference adopted and it is a relational property. But, of course, the world as such hasn’t changed; only our description of it has. It is clearly unacceptable, from the realist perspective, to claim that a change in the conceptual framework used to describe the world amounts to a change in the world itself.

But perhaps the realist would reply that there has been no change in the world; he or she was just wrong in claiming that the theories before the scientific revolution were true (or approximately so). It was thought that the world was made of phlogiston and populated by objects with intrinsic mass.
But this was just the wrong picture. Now we know that the world is made of oxygen and other compounds, and its objects have masses that depend on the frame of reference we consider. After all, the realist goes on, we are all fallibilist these days.

Indeed we are! But the reply conflates two distinct issues that we had better keep apart. On the one hand, there is the ontological issue about the ultimate constituents of reality, and the realist needs a strong line on this issue, introducing the metaphysics of natural kinds (as well as properties and relations). On the other hand, there is the epistemological issue about how we know that a scientific theory has provided the “appropriate” kinds, properties and relations. The realist may well be a fallibilist about the second issue: “We thought we had the adequate framework; but we realize that we don’t”. But if this means that one should revise the answer to the first issue as well, the realist will be unable to claim that he or she has explained the success of science. After all, the explanation is cashed out in terms of natural kinds and reference of the ultimate components of reality; if the latter change, the putative explanation becomes unacceptable. The realist must provide another framework to explain science’s success.

At this point, the realist may claim that it is only the “ideal science”, the ultimate description of the world (to which our current scientific theories are asymptotically approaching) that will provide the adequate explanation of the success of science. Current scientific theories just give us a pale picture.

The problem with this move is that (i) it strongly depends on an account of truth approximation, or at least (ii) a description of the relationship between current scientific theories and the “ideal theory”. Otherwise, instead of providing an explanation of the success of science, the realist just supplies a promissory note. Now, despite the huge amount of work on the issue of truth approximation, no unproblematic proposal that accommodates the issue has been presented. Of course, this doesn’t mean that none will ever be.

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1Even scientific realists grant that (see Boyd [1990]). One of the key problems is that the proposals so far face the problem of language dependence: measures of truth approximation are not invariant under translations into equivalent languages (see Miller [1994], pp. 209–231). As a result, two logically equivalent theories may differ in their truth approximation depending on the way in which they are expressed. Here’s a problem that this brings: suppose that two logically equivalent theories only differ in their linguistic formulations. If they are not equally close to the truth, then the way in which each theory is linguistically formulated matters for its truth. But since the theories are logically equivalent, they have exactly the same truth conditions, which should be invariant under linguistic transformations. But then it’s hard to see how they could differ in their truth approximation. Of course, some linguistic formulations of a theory may be more useful than others. If we don’t know Chinese, having the theory expressed in English makes a big
But until then, the realist is unable to claim that he or she has solved the problem of the success of science.

A similar difficulty is faced by (ii). The use of an ideal theory in the philosophy of science has been considered for a long time. Duhem, in the turn of the century, has articulated a version of realism in which the ideal theory played a crucial role (see Duhem [1906]). The idea is that the ideal theory provides a natural classification of physical laws, and in this sense, it accomplished a representation of them. Now, in order for the ideal theory to do the required job, one has to provide an account of the relationship between the ideal theory and current ones, so that we can evaluate how close we are to that theory, and can determine the adequacy of current scientific explanations vis-à-vis the ideal theory. No such account, however, has been put forward, and without it, any version of realism that depends on the convergence to the ideal theory provides, at best, the promise of a solution, rather than an actual solution.² (Further difficulties faced by convergence models of scientific realism can be found in Laudan [1981]. Of course, since Laudan’s paper appeared, quite some work has been devoted to developing a better account of truth approximation. But there is still no consensus as to whether such accounts work.)

Perhaps the realist could adopt a different line of defense. He or she could acknowledge that there are scientific revolutions and thus that there are cases of reference change. However, the realist would deny that the latter undermine realism. At least such cases don’t undermine a more sophisticated version of scientific realism, according to which scientific theories (being approximately true) have terms that only partially denote (and, in some cases, do not fully denote). In this way, the existence of scientific revolutions can be made compatible with scientific realism. Changes in reference arise from the fact that the terms involved only partially denote: they partially denote an entity \( x \), partially denote another entity \( y \). For example, they partially denote intrinsic mass (from Newtonian theory), partially denote relational mass (from Einstein’s theory). So even if a term used prior to a scientific revolution doesn’t fully denote anything, at least it partially denotes something. And this allows the realist to explain why the theory

difference to us. However, why should that pragmatic feature make any difference with regard to the truth approximation of the theory?

²It has been argued that Duhem’s proposal is better conceived as a structural realist view (see Chiappin [1989]). Independently of this, what is at stake here is whether Duhem’s proposal works. Without a proper account of the relationship between current scientific theories and the ideal theory, clearly it doesn’t. I shall consider the structural version of realism in section 3.
in question successfully explained the phenomena (since partial denotation provides some continuity in theory change).

Of course, in order for this proposal to work, the realist has to provide an account of partial denotation that is compatible with realism. Boyd ([1990], p. 226) has argued that such account is actually provided by Field (see Field [1973] and [1972]). Field has argued that the notion of truth can be defined in terms of partial denotation (of a term) and partial signification (of a predicate). Partial denotation and signification are taken by him as primitive. The idea is that the term mass, as used in Newtonian mechanics, partially denotes intrinsic mass, partially denotes relativistic mass, and doesn’t fully denote either (Field [1973], p. 474). Similarly, an indeterminate predicate partially signifies each of its partial extensions (ibid., p. 477), that is, each of a number of disjoint sets of objects which constitute the predicate’s extension.

Assuming these notions as primitive, Field then provides a semantics for indeterminate expressions in the following way (ibid., p. 477). First, the notion of structure is introduced. A structure for a sentence is a function that assigns each name in the sentence to some object, and each predicate to some set. Secondly, a structure \( S \) corresponds to the sentence if each name in the sentence partially denotes the object that \( S \) assigns to it, and each predicate partially signifies the set that \( S \) assigns to it. Field then applies the standard Tarskian semantics for each structure \( S \) in order to determine whether the sentence is true or false relative to \( S \) (\( S \)-true or \( S \)-false). As he points out: “To say that the sentence is \([S]\)-true is to say that it would be true if the denotations and extensions of its terms were as specified by \([S]\)” (Field [1973], p. 477). A sentence is then true (false) if it is \( S \)-true (\( S \)-false) for every structure \( S \) that corresponds to it.\(^3\)

The question is whether this account of truth in terms of partial denotation can be used by the scientific realist to overcome the problem posed by scientific revolutions. Despite Boyd’s claim to the effect that it can, I think there are problems with this suggestion. First of all, strictly speaking, we don’t have here an account of partial denotation. We have an account of truth in terms of partial denotation. But since the latter is taken as primitive, no account of it is forthcoming. Rather it is assumed that we have an understanding of partial denotation by some other means. Of course, I don’t

\(^3\)Using these notions, Field then provides an account of what he calls a denotational refinement of a term after a scientific revolution. This happens when the set of things the term partially denoted after the revolution is a proper subset of the set of things that it partially denoted before (Field [1973], p. 479). The denotational refinement indicates some continuity of reference across scientific revolutions.
deny that we have some understanding of partial denotation. My point is only that the realist cannot use Field’s proposal to provide an account of this notion.

Now every theory has its primitive notions. Why can’t the realist simply assume (with Field) the notion of partial denotation as primitive? The problem with this move arises from the dialectics of the debate. The realist was led to introduce the notion of partial denotation because of the problem posed by scientific revolutions. The notion was introduced to avoid radical discontinuities in scientific change. Certain accounts of the physical world, although not entirely true, are not entirely false either. For the terms used by a theory before a scientific revolution at least partially denoted certain objects. And this makes room to accommodate the empirical success of such theories and some continuity between them.

However, for the realist’s account to have any explanatory force, the crucial notion (of partial denotation) can’t be primitive. After all, what the realist has to establish is that the relevant terms at least partially denote. In this way, the realist is only giving a name to one of the difficulties faced by his or her proposal. What we need is a proper account of this notion. And as we saw, this is something that Field’s approach doesn’t provide (and is not meant to provide either).

Moreover, as formulated by Field, partial denotation is too weak to support realism, since it is consistent with radical indeterminacy. In fact, as Field argues, there is no fact of the matter as to whether Newton’s notion of mass denoted relativistic mass or proper mass (Field [1973], p. 468). If there is no fact of the matter as to what Newton’s notion of mass actually denoted, the realist is in no position to claim that he or she has provided an explanation of the continuity between the two theories. On the contrary, the realist seems to have denied the existence of any grounds to claim that there is such continuity.

Now, as Field points out ([1973], p. 481), the notions of partial denotation and partial signification — similarly to full denotation and signification — are objective relations between words and extralinguistic entities (or sets of entities). Thus, why can’t the realist adopt such notions? After all, they are not relative to our conceptual scheme, and in this sense they have the objectivity typically required by the realist.

The difficulty here arises from the fact that Field’s approach not only assumes the notion of partial denotation as primitive, but also a primitive notion of modality. As noted above, in order to define S-truth, Field has to introduce a counterfactual to the effect that a sentence is S-true if it would be true if the denotations and extensions of its terms were as specified
by \( S \). Now, for \( S \)-truth to be objective in the way required by the realist, this counterfactual talk has to be cashed out in an objective way. In other words, the realist needs possible worlds, or related objects, to achieve the required objectivity with this modal discourse. Modality is widespread in science (given the use of idealizations and probability, for example), and every account of science sooner or later has to come to grips with this issue. The question is whether the scientific realist can have it both ways: to cash out modal talk (in particular, the counterfactual needed to define \( S \)-truth) in an objective way and still maintain the scientific component of realism. For the usual way of providing an objective account of modality makes use of possible worlds (see Lewis [1986]). However, the latter are causally inert and inaccessible objects. Thus, any claim about them and using them can’t be empirically tested. So the scientific realist will get the objectivity of modal talk at the expense of realism’s scientific aspect. The objectivity is achieved by the postulation of a plurality of worlds, combined with the assumption that modal discourse is about them. However, this is a metaphysical claim, motivated by the need for accommodating the counterfactual claim involved in the semantics of partial denotation. But since modal claims using possible worlds can’t be tested, they can’t be part of scientific realism.

Of course, even scientific realism has its metaphysical assumptions. But why can’t the introduction of possible worlds be one of them? In brief, given that possible worlds are causally inaccessible, they seem to jeopardize the scientific component of realism. In response, perhaps the scientific realist could point to the usefulness of positing possible worlds as a reason to believe in their existence. It’s unclear, however, that this move works. After all, why should the usefulness of possible worlds provide any reason for their existence? It’s certainly useful to invoke perfectly frictionless inclined planes to explain the behavior of certain moving bodies near the surface of the earth. But this doesn’t give us reason to believe that such planes actually exist. We may have pragmatic reasons to accept certain entities, but these need not be reasons to believe that the entities in question exist (see Bueno and Shalkowski [2004], and van Fraassen [1980]).

3. Structural realism and structure change

Given the difficulties faced by standard scientific realism, we shall consider another version of realism which has been formulated to overcome the weaknesses of the earlier proposal. The idea is to accommodate the argument of scientific revolutions (which typically supports anti-realism) without losing the ability of accommodating the no-miracles argument (which is taken to
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support realism). And the crucial change introduced by structural realism is the idea that we should focus on structure rather than the nature of entities which constitute a particular domain (see Worrall [1989], Chiappin [1989], Zahar [1996] and [1997], Ladyman [1998], French and Ladyman [2003], and French [1999]).

According to the structural realist, what is preserved in theory change are not the entities, but the structure underlying the phenomena. What true (or approximately true) theories capture is the “structure of the world”, and this is why such theories are empirically successful and able to predict novel phenomena (phenomena which haven’t been used in the construction of the theory). Thus, by focusing on structure, the structural realist is able to explain why scientific theories are empirically successful, and thus he or she accommodates the no-miracles argument. Moreover, with the emphasis on structure, the structural realist is also able to accommodate the existence of scientific revolutions. The latter bring changes at the level of entities (but typically the structure of the theories is preserved). In this way, the changes that occur in the development of science are easily taken into account by the structural realist. They are changes at the “furniture” of the world, which are irrelevant (according to the structural realist) for a description of the world. What matters is structure, and it is structure which is preserved in theory change. For example, when we move from Fresnel’s theory of light to Maxwell’s electromagnetism, although the nature of light changes, the structure underlying the theories remains the same (see Worrall [1989]). And that is why the structural realist focuses on structure, rather than nature, when he or she examines scientific change.

Structural realism comes in two forms (see Ladyman [1998]). According to the epistemic version, we should restrict our knowledge claims to the structure (of the phenomena). Structure is all we can know. According to the ontic version of structural realism, all there is to nature is structure. As Ladyman argues, structural realism is better conceived as a metaphysical, ontic position, rather than an epistemological one. The latter version is ultimately untenable: either it is uninformative or it has to introduce non-structural features to accommodate the world. If all we can know about the world is structure, then all that can be known about the latter is the cardinality of the domain under consideration. For provided there are enough objects in the domain, every kind of structure can be formulated. In order

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4The epistemic version is advanced by Poincaré [1905], Russell [1927] and, in some passages, Worrall [1989]; see also Chiappin [1989].

5This is the version favored by Ladyman [1998]; see also French and Ladyman [2003].
to overcome the triviality of the proposal, constraints have to be introduced to indicate which are the relevant (or intended) structures of the domain in question. But such constraints are not structural, but pragmatic (see van Fraassen [1997]). And so their introduction undermines structuralism.\(^6\)

But is the ontic version of structural realism subject to the same difficulty? If it is not exactly the same, at least a related difficulty can be raised to this version. The problem is as follows. As is well known, from the Löwenheim-Skolem theorem for first-order logic, if a theory has a model with an infinite domain, then it has models with each infinite cardinality. In other words, first-order theories with infinite domains have non-standard models: models that are not isomorphic but are elementarily equivalent (i.e. a sentence is true in one of them if and only if it is true in the other). Now, since for the structural realist what matters is structure (in the ontic version, that is all there is to nature), which of these non-equivalent models describes the world? The structural realist can’t say that it doesn’t matter, since each of these models provides a different structure to accommodate reality.\(^7\)

The point here is that the structural realist (even adopting the ontic version) faces the difficulty of having to choose between non-equivalent interpretations — as far as structure is concerned — which deliver the same results, with regard to the truth-values of the sentences under consideration. Now, why can’t the structural realist simply claim that he or she will choose the intended interpretation? Because this is a non-structural feature, and as

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\(^6\)This is essentially the problem raised by the mathematician Newman to Russell’s epistemic formulation of structural realism (see Newman [1928] and Russell [1927]). For discussion and references, see Demopoulos and Friedman [1985], van Fraassen [1997] and Ladyman [1998].

\(^7\)This argument is a version of Putnam’s well-known model-theoretic argument against metaphysical realism (see Putnam [1980]). For a version of this argument that doesn’t rely on the Löwenheim-Skolem theorem, but on the permutation of the objects in the domain of interpretation, see Putnam [1981]. This latter version of the argument is important. For in order to block Putnam’s original argument, the structural realist may try to adopt second-order logic, for which the Löwenheim-Skolem theorem doesn’t hold (at least with the standard semantics). The problem, however, is that second-order logic also has the so-called “Henkin models”, in which Löwenheim-Skolem theorem does hold. As Putnam points out, the formalism doesn’t uniquely determine the interpretation of the second-order quantifier, and so there will always be interpretations in which non-intended models are generated (Putnam [1980], p. 23). Thus, in Putnam’s view, the move to second-order logic doesn’t avoid the conclusion of his argument. Moreover, the permutation variant of Putnam’s argument also goes through in second-order logic: there will be non-intended models which are elementarily equivalent. (However, the models generated by the permutation argument are isomorphic, and so the structural realist can accommodate them. This is not the case, though, with the original version of Putnam’s argument, which relies on the Löwenheim-Skolem theorem.)
such it goes beyond the purely structural features allowed by structuralism. The notion of intended interpretation is a pragmatic notion, not a structural one. And if the structural realist were to introduce a pragmatic notion at this point, the grounds to support realism would be lost. After all, what is at stake is the determination of the structure that describes the world. And if the structural realist’s choice between alternative non-equivalent structures is only pragmatic — and not epistemic, since it is not structural\(^8\) — this choice would be compatible with the one made by the empiricist. For the empiricist stresses the role of pragmatic factors in theory choice (see van Fraassen [1985] and [1997]). But then the move is too weak to support realism.

Alternatively, perhaps the ontic structural realist could insist that there are (structural) natural kinds, and in terms of the latter, a selection can be made of the appropriate structures. But this move requires (i) a characterization of such structural natural kinds, and (ii) an account of our knowledge of such kinds. It’s unclear, however, how the structural realist could characterize such structural natural kinds and have the corresponding knowledge of them. First, such kinds cannot be thought of as objects, given that this would bring the wrong ontological category from the structural realist’s point of view. But if these kinds are structures, what sort of structure are they (set-theoretic, mereological, or some other form of structure)? Second, it’s not obvious how the structural realist could characterize structural natural kinds independently of knowing what are the relevant structures in the world. However, if the structural realist already has such knowledge, there’s no need for invoking natural kinds in the first place. In any case, the structural realist would need to develop this move in some detail before it can be properly assessed. I conclude that, ultimately, the ontic version of structural realism seems to face the same problem that undermined the epistemic formulation of structuralism.

However, structural realism also faces a further difficulty. It arises not from the existence of different structures that do the same job, but from the existence of structural losses in scientific change. There are well-known cases that support the existence of these losses. For example, when we moved from Descartes’s celestial mechanics to Newton’s, the structure provided by Descartes’s theory of vortices was entirely lost. The latter theory explained why the planets moved in the direction that they did, and this was an issue

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\(^8\)Since for the (ontic) structural realist, all there is to nature is structure, the crucial epistemic reason to believe in a theory is given by the structure that it provides in the description of the phenomena.
left unexplained by Newton’s own theory. In other words, some *structure* was lost in this case (for a discussion and further examples of this kind of losses, see Laudan [1996]).

It might be argued that Descartes’s theory of vortices was in fact a piece of unsupported metaphysics, and thus it is not surprising that it was eventually eliminated. However, even if this evaluation is right, there are other cases of structural losses in science that don’t invite the same assessment. Consider the case of quantum mechanics. As is well known, in 1932, von Neumann provided a unified formulation of quantum mechanics in terms of Hilbert spaces (see von Neumann [1932]). One of the major advantages of this approach is that not only it rigorously established the equivalence between Heisenberg’s and Schrödinger’s mechanics, but it also provided a way of introducing probability in quantum mechanics without recourse to Dirac’s inconsistent $\delta$-functions (for a discussion, see Muller [1997] and Rédei [1997]). What is less well known is that right after the publication of his book on quantum mechanics, von Neumann started to have doubts about the Hilbert spaces formalism (see Rédei [1997] and [1998]). The reason for this shift arose from the probabilistic side of quantum theory. Although Hilbert spaces generate the right probabilities if we consider quantum systems with a finite number of degrees of freedom, these spaces are inadequate for systems with an infinite number of them. What is needed, according to von Neumann, is an entirely *new structure*, which is called type II$_1$ factor algebra. It arose from von Neumann’s work on the theory of operators. (This algebra is one of a family of structures that we now call von Neumann algebras.) By introducing this kind of structure, a unified approach to the introduction of probability in quantum mechanics (encompassing both finite and infinite systems) is provided. And according to von Neumann, this is the kind of structure that we should use in quantum mechanics (for references and details, see Rédei [1997]).

However, type II$_1$ factor algebra does not generate the same structure than the one provided by the more familiar algebra of bounded operators in a Hilbert space. When we move from the latter to the former, we *lose* structure. The former is modular and non-atomic, the latter is modular, atomic and non-distributive if the dimension of the Hilbert space is greater or equal to 2 (see Rédei [1997], p. 505). However, the two algebras lead to the same empirical results. In this sense, both frameworks are adequate for the development of quantum mechanics. In other words, the reason to prefer the type II$_1$ factor algebra *cannot* be empirical. It cannot be *structural* either, since the structure that arises from Hilbert spaces and type II$_1$ factor algebra are *not* equivalent. And the reason for changing the structure does not depend
on a metaphysical picture, as opposed perhaps to the case of Descartes’s theory of vortices. The choice between the frameworks is conceptual, broadly speaking: the type $II_1$ factor algebra provides the appropriate framework to *define probability* in quantum mechanics. But this is *not* a matter of the structure of the world; at best, it concerns our *representation* of the latter. However, if all there is to nature is structure, how can the structural realist accommodate cases of structural loss such as this?

A related difficulty arises here. As we saw, the structural realist abandons the requirement of reference to *entities* as part of an explanation of scientific change, and stresses *structure* preservation as the crucial feature. Thus, the structural realist is of course in a better position than the scientific realist to accommodate the case of radical indeterminacy considered by Field. After all, the structural realist could try to show the “structural equivalence” between Newton’s and Einstein’s notions of mass. The problem, however, is that there is no *full* structural equivalence between Newtonian and relativistic mechanics. As is well known, there are *structural losses* in this case of theory shift.

I think what is needed is a different way of conceptualizing structuralism; a more open-ended way which allows one to accommodate even structural losses in scientific change. However, as we shall see below, given the difficulties faced by realism, I think the best way to articulate a defensible version of structuralism is to combine it with *empiricism* (in particular, with van Fraassen’s constructive empiricism; see van Fraassen [1980], [1989], and [1991]). In what follows, I shall indicate some features of this proposal, and how it accommodates the difficulties to realism we have considered thus far. But before doing that I shall first consider the formal framework in which this alternative will be developed: da Costa and French’s partial structures approach.

### 4. Partial structures and quasi-truth

The partial structures approach relies on three main notions: partial relation, partial structure and quasi-truth.\(^9\) One of the main motivations for introducing this proposal comes from the need for supplying a formal framework in which the “openness” and “incompleteness” of scientific practice and knowledge can be accommodated in a unified way (da Costa and French [1990]). This is accomplished by extending, on the one hand, the usual notion of

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\(^9\)This approach was first presented in Mikenberg *et al.* [1986], and in da Costa [1986]. Since then it has been extended and developed in several different ways; see, for instance, da Costa and French [1989], [1990], [1993], Bueno [1997] and [1999a].
structure, in order to model the partialness of information we have about a certain domain (introducing then the notion of a partial structure), and on the other hand, by generalizing the Tarskian characterization of the concept of truth for such “partial” contexts (advancing the corresponding concept of quasi-truth).

In order to introduce partial structures, the first step is to formulate an appropriate notion of partial relation. When investigating a domain of knowledge, $\Delta$, we formulate a framework to systematize and organize the information about it. This domain is then represented by a set $D$ of objects, and is studied by the examination of the relations holding among $D$’s elements. However, it often happens that, given a relation $R$ on $D$, we do not know whether all the objects of $D$ (or $n$-tuples thereof) are related by $R$. This is part and parcel of the “incompleteness” of our information about $\Delta$, and is formally accommodated by the concept of partial relation. The latter can be characterized as follows. Let $D$ be a non-empty set. An $n$-place partial relation $R$ over $D$ is a triple $\langle R_1, R_2, R_3 \rangle$, where $R_1$, $R_2$, and $R_3$ are mutually disjoint sets, with $R_1 \cup R_2 \cup R_3 = D^n$, and such that: $R_1$ is the set of $n$-tuples that (we know that) belong to $R$, $R_2$ is the set of $n$-tuples that (we know that) do not belong to $R$, and $R_3$ is the set of $n$-tuples for which we do not know whether they belong or not to $R$. (Notice that if $R_3$ is empty, $R$ is a usual $n$-place relation which can be identified with $R_1$.)

However, to represent the information about the domain in question, we need a notion of structure. The following characterization, spelled out in terms of partial relations and based on the standard concept of structure, supplies a notion that is broad enough to accommodate the partiality usually found in scientific practice. The main work is done by the partial relations. A partial structure $S$ is an ordered pair $\langle D, (R_i)_{i \in I} \rangle$, where $D$ is a non-empty set, and $(R_i)_{i \in I}$ is a family of partial relations defined over $D$.

We have now defined two of three basic notions of the partial structures approach. In order to spell out the last one (quasi-truth), we need an auxiliary notion. The idea is to use, in the characterization of quasi-truth, the resources supplied by Tarski’s definition of truth. However, since the latter is only defined for full structures, we have to introduce an intermediary notion of structure to link it to the former. This is the first role of those structures that extend a partial structure $A$ into a full, total structure (which are

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10The “partiality” of partial relations and partial structures is due to the “incompleteness” of our knowledge about the domain under investigation. With further information, a partial relation may become total. Thus, the partiality modeled here is not understood as an intrinsic, ontological “partiality” in the world. We are concerned with an “epistemic”, not an “ontological” partialness.
called $A$-normal structures). Their second role is model-theoretic: to put forward an interpretation of a given language and, in terms of it, to characterize basic semantic notions.\footnote{For a different formulation of quasi-truth, independent of the notion of $A$-normal structure and in terms of quasi-satisfaction, see Bueno and de Souza [1996].} The question then is: how can $A$-normal structures be defined? Let $A = \langle D, R_i \rangle_{i \in I}$ be a partial structure. We say that the structure $B = \langle D', R'_i \rangle_{i \in I}$ is an $A$-normal structure if (i) $D = D'$, (ii) every constant of the language in question is interpreted by the same object both in $A$ and in $B$, and (iii) $R'_i$ extends the corresponding relation $R_i$ (in the sense that each $R'_i$, supposed of arity $n$, is defined for all $n$-tuples of elements of $D'$).

Note that, given a partial structure $A$, there are a lot of $A$-normal structures. We need to provide constraints to restrict the acceptable extensions of $A$. In order to do that, a further auxiliary notion is required (Mikenberg et al. [1986]). A pragmatic structure is a partial structure to which a third component has been added: a set of accepted sentences $P$, which represents the accepted information about the structure’s domain. (Depending on the interpretation of science that is adopted, different kinds of sentences are introduced in $P$: realists will typically include laws and theories, whereas empiricists will tend to add laws and observational statements about the domain in question.) A pragmatic structure is then a triple $A = \langle D, R_i, P \rangle_{i \in I}$, where $D$ is a non-empty set, $(R_i)_{i \in I}$ is a family of partial relations defined over $D$, and $P$ is a set of accepted sentences (which hold in $A$). The idea, as we shall see, is that $P$ introduces constraints on the ways that a partial structure can be extended.

Our problem now is, given a pragmatic structure $A$, what are the necessary and sufficient conditions for the existence of $A$-normal structures? We can now spell out one of these conditions (Mikenberg et al. [1986]). Let $A = \langle D, R_i, P \rangle_{i \in I}$ be a pragmatic structure. For each partial relation $R_i$, we construct a set $M_i$ of atomic sentences and negations of atomic sentences, such that the former correspond to the $n$-tuples that satisfy $R_i$, and the latter to those $n$-tuples which do not satisfy $R_i$. Let $M = \bigcup_{i \in I} M_i$. Therefore, a pragmatic structure $A$ admits an $A$-normal structure if, and only if, the set $M \cup P$ is consistent. As this condition makes it clear, the notion of consistency plays a crucial role in the partial structures approach. In fact, the very concept of quasi-truth, since it depends on the existence of $A$-normal structures, supposes the consistency of $M$ and $P$. I stress this point because it will be relevant to our discussion below.
Having said this, we are finally able to formulate the concept of quasi-truth. A sentence $\alpha$ is *quasi-true in a pragmatic structure* $A = \langle D, R_i, P \rangle_{i \in I}$ if there is an $A$-normal structure $B = \langle D', R'_i \rangle_{i \in I}$ such that $\alpha$ is true in $B$ (in the Tarskian sense). If $\alpha$ is not quasi-true in $A$, we say that $\alpha$ is *quasi-false in $A$*. Moreover, we say that $\alpha$ is *quasi-true* if there is a pragmatic structure $A$ and a corresponding $A$-normal structure $B$ such that $\alpha$ is true in $B$ (according to Tarski’s account). Otherwise, $\alpha$ is *quasi-false*.

Intuitively, a quasi-true sentence $\alpha$ does not describe completely the domain under investigation, but only an aspect of it — the one modeled by the relevant pragmatic structure $A$. After all, there are several different ways in which $A$ can be extended to a full structure, and in some of these extensions $\alpha$ may not be true. As a result, the notion of quasi-truth is strictly weaker than truth: although every true sentence is (trivially) quasi-true, a quasi-true sentence is not necessarily true (since it may be false in certain extensions of $A$). This is an important feature of this notion.

To illustrate the use of quasi-truth, let us briefly consider an example. As is well known, Newtonian mechanics is appropriate to explain the behavior of bodies under certain conditions (roughly speaking, if the speeds in question are “low” in comparison with that of light, the bodies are not subject to strong gravitational fields etc.). And with the formulation of special relativity, we learnt that if these conditions are not satisfied, Newtonian mechanics is false. In this sense, these conditions specify a family of partial relations, which delimit the context in which the theory holds. Although Newtonian mechanics is not true (and we know under what conditions it is false), it is *quasi-true*; that is, it is true in a given context, determined by a pragmatic structure and a corresponding $A$-normal one, which satisfy the conditions mentioned above (see da Costa and French [1993] and [2003]). It is time now to return to the discussion of scientific change. As we shall see, the latter can be addressed in a new way if we explore the formal resources provided by the above framework.

5. Partial denotation, partial structure preservation and scientific change

The main idea of the present view is that scientific theories need not be true to be good, but only quasi-true. It is, thus, an anti-realist account, since truth is not an aim of science (see van Fraassen [1980]). In scientific change, some structure is lost because typically we have only a *partial* structure
preservation. Only the parts of the theory that were *empirically* well supported are preserved. This aspect clearly sides with empiricism.

But can we formally capture the intuition underlying this claim? In terms of the partial structures approach, I think a positive answer can be given to this question. The notion of partial structure preservation can be formally represented by a partial isomorphism between the structures of the old and the new theory. More formally,\(^{12}\) let \(S_1 = (D, R_i)\) and \(S_2 = (D', R'_i)\) be two partial structures, where \(R_i = (R_1, R_2, R_3)\) and \(R'_i = (R'_1, R'_2, R'_3)\) are, say, binary partial relations. We say that a (partial) function \(f : D \rightarrow D'\) is a *partial isomorphism* between \(S_1\) and \(S_2\) if (i) \(f\) is bijective, (ii) for every \(x, y \in D\), \(R_1 xy \leftrightarrow R'_1 f(x)f(y)\) and \(R_2 xy \leftrightarrow R'_2 f(x)f(y)\). (Thus, if the third components, \(R_3\) and \(R'_3\), are empty, we obtain the usual notion of isomorphism.)

This notion of partial isomorphism can then be used to provide an account of partial structure preservation in scientific change, and it thus accommodates an important aspect of scientific practice. As we noticed above, there is no counterpart of Descartes’s theory of *vortices* in Newton’s mechanics. However, Descartes’s theory of *inertia* found its way into Newton’s. Hence, *some* structure was preserved in this theory shift, and the partial isomorphism between the models of the two theories accommodates that. The \(R_1\) and \(R_2\) components for which there was enough empirical evidence are carried over, by the partial isomorphism, into the models of the Newtonian theory. However, the \(R_3\) components, for which there wasn’t enough evidence, are left behind.

The same point also applies to von Neumann’s case. The algebra of operators in a Hilbert space and the type \(\text{II}_1\) factor algebra do not have the same structure. However, they *share* some properties (for instance, they are both modular). Thus, *some* structure (but only some) is preserved in theory change. And that is why a partial isomorphism between the relevant structures accommodates the relationship found between them.

The partial structure preservation accommodates the two dimensions of scientific change: structural and conceptual change. The existence of structural change after a scientific revolution is straightforwardly described in terms of the partiality of the isomorphism that holds between the models of the theories under consideration. As noted above, some structure is typically carried over in scientific change, but some is inevitably lost. Conceptual change, on the other hand, is usually associated with structure change.

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\(^{12}\)See French and Ladyman [1999], and Bueno [1997].
With the introduction of new structure, new concepts are formulated.\(^{13}\)

These concepts are then used to explore the domain of the new theory after the scientific revolution. Moreover, with structural losses, concepts are also lost. As we saw above, there is no room for Cartesian vortices in the Newtonian theory.

Of course, far more could be said about these two dimensions. My point here is only to indicate that the formal framework provided by the partial structures approach can accommodate some aspects of them (see also Bueno [2000]). It might be argued, however, that partial isomorphism is too weak a relation to accommodate these dimensions of scientific change. After all, the argument goes, there can be so many partial isomorphisms between the models of different theories that scientific change becomes too easy. Which of the partial isomorphisms are the relevant ones?

The complaint is fair: an account of scientific change shouldn’t be trivial. But it is not right: the complaint disregards the fact that the notion of relevance is pragmatic. What is relevant depends on our interests, on the specific context we consider. Which partial isomorphisms are relevant is relative to the particular domain of science under examination, and the particular theories in question. It is not the job of the partial structures formalism to decide a priori which particular relations between the old and the new theories scientists should select. Our task is only to indicate the kind of relation that provides a model of scientific change; that is, that allows us to represent one way in which scientific theories can change. Partial isomorphism is a notion that is broad enough to make room for structural losses in scientific practice.\(^{14}\) without being too broad. For it is not the case that any two partial structures are partially isomorphic to each other. In this sense, partial isomorphism is not too weak. It is not the case that anything goes. So the framework allows us to accommodate the “openness” of scientific practice in a clear formal setting. We have here a nice balance.

\(^{13}\)The introduction of new structures can be represented by the partial structures framework by the existence of a partial embedding from a model of a theory \(T_1\) into a model of a theory \(T_2\); i.e. by establishing a partial isomorphism between a model of \(T_1\) and a substructure of a given model of \(T_2\). This allows the possibility that \(T_2\) has more structure than \(T_1\) (see Bueno [2000]).

\(^{14}\)If a broader notion is required, it is easy to define a partial homomorphism between the partial structures in question (see Bueno, French, and Ladyman [2002], and Bueno [1999b]). Let \(S = \langle D, R_i \rangle_{i \in I}\) and \(S' = \langle D', R_i' \rangle_{i \in I}\) be partial structures. We say that \(f : D \rightarrow D'\) is a partial homomorphism from \(S\) to \(S'\) if for every \(x\) and every \(y\) in \(D\), \(R_i xy \rightarrow R_i' f(x)f(y)\) and \(R_2 xy \rightarrow R_2' f(x)f(y)\). In this way, \(S\) and \(S'\) need not have the same cardinality.
But what should one say about Putnam’s argument? As we noted, it raises a problem for the structural realist, given the plurality of non-isomorphic (but elementarily equivalent) structures that could be generated in the same theory and the realist doesn’t seem to have a way of choosing between them. The present account, however, is not realist. So the choice between different structures can be made on pragmatic grounds. The empiricist can choose the intended structure, on the grounds that this is the structure that we intend to talk about (see van Fraassen [1985] and van Fraassen [1997]). Of course, this is not an epistemic move. But the empiricist is the first to stress the role of pragmatic — and non-structural — features in theory choice. It is certainly a problem for the realist to allow non-epistemic factors in theory choice. In particular, it is problematic for the structural realist to allow non-structural features to decide between different structures. The problem, however, vanishes once we move to an empiricist setting. The crucial epistemic criterion of theory choice is empirical adequacy; the remaining criteria are essentially pragmatic. So, given the empiricist features of the present account and the use of the partial structures approach, it accommodates the difficulties faced by structural realism.

Turning now to scientific realism, as we noted, one of the problems of this view came from the existence of scientific revolutions. And an attempt was made to overcome the difficulty with the notion of partial denotation. Now, with the adoption of partial structures, the present proposal provides an account of partial denotation, without having to take the latter as primitive. We can say that a term \( t \) partially denotes entities \( o_1 \) and \( o_2 \) if \( t \) occurs in a sentence \( M \) that is quasi-true, and \( M \) is true if it is about \( o_1 \), and is false if it is about \( o_2 \). For example, consider the sentence, “Mass depends on the frame of reference” (\( M \)). Clearly, \( M \) is quasi-true: it is true in the context of relativity theory; moreover, it is false in the context of Newtonian mechanics. Thus, the term mass partially denotes relativistic mass, partially denotes intrinsic mass. As we saw, this is exactly the same result that is obtained by Field’s account. And the notion of partial denotation presented here is similarly weak, since it is compatible with the radical indeterminacy of the notion of mass: there is no fact of the matter as to whether Newton’s notion of mass denoted relativistic mass or intrinsic mass. However, as opposed to Field’s approach, we don’t have to introduce the notion of partial denotation as primitive, nor do we need to introduce a counterfactual to cash out the notion of truth. So, by moving to the partial structures approach, and by adopting an empiricist view, the difficulties faced by scientific realism can also be accommodated.


6. Conclusion: structural empiricism and scientific change

Using the partial structures framework, it is thus possible to provide an alternative account of scientific change that handles both conceptual and structural changes. Given the difficulties faced by the realist (discussed above), the account provided relinquishes realism. And given the features of the account, it is entirely compatible with an empiricist stance — in particular, with van Fraassen’s constructive empiricism (see Bueno [1997] and [1999a]).

As we saw, within this framework, an account of partial denotation in terms of partial structures can be provided. And the account ultimately supports empiricism, since only reference to components of the empirical domain is “preserved” in theory change (with regard to theoretical components, they only partially denote, since scientific theories are taken to be at best quasi-true). But the account also accommodates structural losses that so often are found in science. In terms of the existence of partial isomorphisms between the structures of the new and old theories, an account of partial structural preservation in science is presented. In this way, a clear sense can be made to the idea that only some structure is carried over in scientific change. As a result, an argument for an empiricist view of theory change is put forward. Given the structuralist and empiricist features of this view, I call the resulting proposal structural empiricism (see Bueno [1999a]).

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