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Shared structure need not be shared set-structure

Elaine Landry

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Abstract Recent semantic approaches to scientific structuralism, aiming to make precise the concept of shared structure between models, formally frame a model as a type of set-structure. This framework is then used to provide a semantic account of (a) the structure of a scientific theory, (b) the applicability of a mathematical theory to a physical theory, and (c) the structural realist's appeal to the structural continuity between successive physical theories. In this paper, I challenge the idea that, to be so used, the concept of a model and so the concept of shared structure between models must be formally framed within a single unified framework, set-theoretic or other. I first investigate the Bourbaki-inspired assumption that structures are types of set-structured systems and next consider the extent to which this problematic assumption underpins both Suppes' and recent semantic views of the structure of a scientific theory. I then use this investigation to show that, when it comes to using the concept of shared structure, there is no need to agree with French that "without a formal framework for explicating this concept of 'structure-similarity' it remains vague, just as Giere's concept of similarity between models does ..." (French, 2000, *Synthese*, *125*, pp. 103–120, p. 114). Neither concept is vague; either can be made precise by appealing to the concept of a morphism, *but* it is the context (and not any set-theoretic type) that determines the appropriate kind of morphism. I make use of French's (1999, *From physics to philosophy* (pp. 187–207). Cambridge: Cambridge University Press) own example from the development of quantum theory to show that, for both Weyl and Wigner's programmes, it was the context of considering the 'relevant symmetries' that determined that the appropriate kind of morphism was the one that preserved the shared Lie-group structure of both the theoretical and phenomenological models.

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E. Landry (✉)
University of Calgary, Calgary, Alberta, Canada T2N 1N4
e-mail: elandry@ucalgary.ca

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1 Introduction

Recent semantic approaches to scientific structuralism (Da Costa, Bueno, & French, 1997; Da Costa & French 1990; French & Da Costa 2000; French 1999, 2000), aiming to make precise the concept of shared structure between models, formally frame a model as a type of set-structure. This framework is then used to provide a semantic account of (a) the structure of a scientific theory, (b) the applicability of a mathematical theory to a physical theory,[1] and (c) the structural realist's appeal to the structural continuity between successive physical theories. In this paper, I challenge the idea that, to be so used, the concept of a model and so the concept of shared structure between models must be formally framed within a single unified framework, set-theoretic or other.

I first investigate the Bourbaki-inspired assumption that structures are types of set-structured systems and next consider the extent to which this problematic assumption underpins both Suppes' and recent semantic views of the structure of a scientific theory. I point out that, mathematically speaking, there is no reason for our continuing to assume that structures and/or morphisms are 'made-up' of sets. Thus, to account for the fact that two models share structure we do not have to specify what models, *qua* types of set-structures, are. It is enough to say that, in the context under consideration, there is a morphism between the two systems, *qua* mathematical or physical models,[2] that makes precise the claim that they share the appropriate kind of structure.

I then use this investigation to show that when it comes to using the concept of shared structure — to account for the structure of scientific theories, the applicability of a mathematical theory to a physical theory, and the structural continuity between successive theories—there is no need to agree with French that "without a formal framework for explicating this concept of 'structure-similarity' it remains vague, just as Giere's concept of similarity between models does…" (French, 2000, p. 114). Neither concept is vague; either can be made precise by appealing to the concept of a morphism, *but* it is the context (and not any set-theoretic type) that determines the appropriate kind of morphism.[3] I make use of French's (1999) own example

[1] In accounting for applicability, this use also attempts to frame Redhead's (1975, 1980, 1995) concept of 'surplus structure'. See page 9 and footnote 21.

[2] There are at least three ways to go here. First, one might take a model itself at face value, viz., to be understood, in the Tarskian sense, as an interpretation that makes a set of sentences true *without* adding that a model *is* a set-theoretic entity. Second, one might offer, along the lines of van Fraassen, a state-space account of what is meant by mathematical model, so that a physical model is a model (in the Tarskian sense) of a mathematical model *qua* a state-space. Finally, one might forego giving an account of models per se and instead offer an account of what is meant by the term 'structured system' so that one may then consider a model as a specific kind of structured system. In Landry and Marquis (2005), for example, we formally frame the concept of an abstract, mathematically, structured system in category-theoretic terms, i.e., as a kind of cat-structured system. I am not suggesting, however, that one of these three ways is preferable to the other; I am only pointing out that none of these requires us to take models as types of set-structures or types of set-structured systems.

[3] I thus take the concept of a morphism at face value, i.e., as a map between two kinds of structured systems, *qua* mathematical or physical models, where, as explained in footnote 2, there are at least three options other than taking models themselves as types of set-structures. For example, one could

from the development of quantum theory to show that, for both Weyl and Wigner's programmes, it was the context of considering the 'relevant symmetries' that determined that the appropriate kind of morphism was the one that preserved the shared Lie-group structure of both the theoretical and phenomenological models.[4]

2 The Bourbaki Beginnings

The semantic view of scientific theories is typically presented as the view that the structure of a scientific theory is captured by the collection of its models. As is well known, the semantic view was first suggested as an alternative to the syntactic view. Instead of linguistically characterizing a theory as a partially interpreted calculus, and having to appeal to rules (or definitions) to connect this 'linguistic entity' to the world, one characterizes a theory as a collection of models and appeals to the shared structure between these theoretical models and models of the phenomena. What is less known,[5] however, is that there is more to the semantic view than the slogan that a theory is a collection of models, or a family of structures.[6] As first presented by Suppes, the semantic view is framed by set theory: it is only when we frame a theory by a set-theoretic predicate that we can make use of the semantic view and so extract

Footnote 3 continued
use category theory to provide a formal framework for the concept of an abstract structured system and too for the concept of a morphism as a map between such cat-structured systems, *but* that is quite beside my point here. My point is that, *regardless* of formal frameworks, it will be a specific context that determines what kind of morphism is appropriate. For example, in the context of speaking about the shared structure of systems structured by space-time theories the appropriate kind of morphism is a diffeomorphism, and this regardless of what a diffeomorphism *is*, i.e., regardless of whether it is a function between set-theoretic elements or an arrow between category–theoretic objects. Note also that if we narrow this context we must likewise narrow the kind of morphism. For example, while for generally relativistic theories the morphism between the dynamically possible models will be any diffeomorphism, for specially relativistic theories the morphism between models will be a restricted kind of diffeomorphism called a Poinçare transformation and for Newtonian Mechanics it will be another restricted kind diffeomorphism called a Galilean transformation. The groups of Poinçare and Galilean transformations being subgroups of the diffeomorphism group.

[4] The attention to use/context is thus intended to be amenable to the Cartwright, Shomar, and Suarez (1995) view of the importance of 'phenomenological' models, but, contra Suarez (2003, 2004), it does not go as far as rejecting 'isomorphism' accounts of shared structure. Rather it seeks to present a 'morphism' account of shared structure wherein the morphism that preserves the appropriate kind of structure is determined by the use/context and not by a presumption that any one type of morphism is, or is not, *the* type that makes precise the concept of shared structure and, in so doing, fixes its use for any context.

[5] Of course, the set-theoretic, even Bourbaki, underpinnings of some accounts of the structure of scientific theories are both explicitly made and well-known. See, for example, Sneed (1981) and Stegmüller (1979).

[6] For accounts of a scientific theory as a family of structures see van Fraassen (1980, 1987), French and Da Costa (2000) and French, (1999, 2000). Note, however, that van Fraassen, in distinguishing the Tarski-Suppes 'set-theoretic' approach from the Weyl-Beth 'state-space' approach and in adopting the latter, is not committed to the use of set theory as "the canonical form for the formulation of theories" (van Fraassen, 1980, pp. 65–66). But he does claim that "[t]he general concepts [of isomorphism and embeddability] used in the discussion of empirical adequacy… pertain to scientific theories conceived in either way" (Ibid., 67).

the claim that the connection between the theory and the phenomena can be captured in terms of the same structure[7] of their respective models. As Suppes claims:

> [w]hen a branch of empirical science is stated in exact form, that is, when the theory is axiomatized within a standard set-theoretical framework, the familiar question raised about models of the theory in pure mathematics [to make precise the concept of same structure in terms of isomorphisms between models] may also be raised for models of the precisely formulated empirical theory. (Suppes, 1960, p. 295)

What Suppes is after, then, is a means by which we can characterize the structure of a scientific theory so that we may use this to account for the applicability[8] of a mathematical theory to a set of phenomena in terms of their shared structure. For example, in the case of applying numbers to 'things' like mass, distance or force,

> the mathematical task is… to establish that any mathematical model is isomorphic to some numerical model of the theory. The existence of this isomorphism between models justifies the application of numbers to things. We cannot literally take a number in our hands and apply it to a physical object. What we can do is to show that the structure of a set of phenomena under certain empirical operations is the same as the structure of some set of numbers under arithmetical operations and relations. The definition of isomorphism of models in the given context makes the intuitive ideas of *same structure* precise. (Suppes, 1967a, pp. 58–59)

Suppes' argument for the necessity of using a set-theoretic framework can be reconstructed as follows:

1. We need the concept of same structure between models to talk about the *applicability* of a mathematical theory to the theory of the phenomena.[9]
2. We need to present theories as *collections of models* to forgo the problems traditionally associated with the syntactic view's account of applicability.
3. We need to mathematize both the concept of *model* and the concept of *same structure* to make their meaning precise.
4. The best way to mathematize both the concept of model and the concept of shared structure is via *set theory*.

Therefore,

5. We need to *formally frame* a theory by a set-theoretic predicate to make use of the precise (mathematical) concept of (i) a model as a type of set-structure, and (ii) same structure expressed as an isomorphism between models so characterized.

[7] Note here that Suppes' seeks to formalize the concept of *same* structure in terms of isomorphisms. As we will see, the focus of recent semantic approaches, specifically those of French and Da Costa, is to formalize the broader concept of *shared* structure in terms of some other kind of morphism, e.g., in terms of homeomorphisms or partial isomorphisms.

[8] Suppes also uses isomorphism between models to give an account of reductionism: one theory T_1 reduces to another theory T_2 if it can be shown that for any model of T_1 it is possible to construct an isomorphic model within T_2 (see Suppes, 1960, pp. 295–296; 1967a, p. 59).

[9] I use the phrase 'theory of the phenomena' to capture the Suppesian idea (see Suppes, 1960, 1962) that data models are used to present the phenomena as experimentally structured, i.e., as structured by the 'theory of the experiment'. This so that the applicability of a mathematical theory to the theory of the phenomena can be accounted for in terms of the same structure of the theoretical and data models.

Overall, my aim in this paper is to investigate the grounds for this conclusion and to challenge its acceptance by some as an essential component of the semantic view.

Let us begin with the assumption that a model is a type of set-structure. That Suppes makes such an assumption is clear:

> … a model of a theory may be defined as a possible realization in which all valid sentences of the theory are satisfied, and a possible realization of the theory is an entity of the appropriate set-theoretical structure. (Suppes, 1962, p. 252).

Surely, the first part of this characterization is the now familiar Tarskian concept of a model, but why does Suppes feel the need to add that a model is “an entity of the appropriate *set-theoretical* structure”? To understand this addition, we must appreciate two things: one, that the set-theoretic foundationalist programme for mathematics is in full swing and, two, that one such programme, viz., that undertaken by Bourbaki, construed all structures as types of set-structures.[10] Suppes' use of both of these programmes is evidenced by his claim that:

> … there is no theoretical way of drawing a sharp distinction between a piece of pure mathematics and a piece of theoretical science. The set-theoretical definitions of the theory of mechanics, the theory of thermodynamics, the theory of learning, to give three rather disparate examples, are on all fours with the definitions of the purely mathematical theories of groups, rings, fields, etc. From the philosophical standpoint there is no sharp distinction between pure and applied mathematics, in spite of much talk to the contrary. (Suppes, 1967b, pp. 29–30).

The hidden presumption here is that the theory of groups, rings and fields can be reduced to, or unified by, the theory of sets, so that any mathematical theory, as expressed by its axioms, can be represented by its models *qua* types of set-structures. Forgoing the set-theoretic foundationalist's agenda, I want to point out that, mathematically speaking, there is no reason for our continuing to assume that structures and/or the morphisms between them are 'made-up' of sets. There is no reason to presume that set theory founds mathematics or that only set theory can provide the formally precise concept of structure. Indeed, as has been argued elsewhere,[11] category theory can be used to provide both; but this claim is quite beside my point here. My point is simply this: to make precise the concept of shared structure between models, we do not need to specify what models are *qua* types of set-structures, nor what morphisms are *qua* maps between such types.

3 The French connection

My objective for this section is to show that, to account for the fact that two models share structure and to make philosophical use of this fact, it is not necessary to specify what models are *qua* types of set-structures. It is enough to say that, in the context

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[10] It is debatable whether Bourbaki intended their programme to be foundationalist in the sense of calling for a reduction of all of mathematics to set theory, or whether they intended it to provide a unification of the concept of structure. I thank Dan Isaacson for this point.

[11] See Landry and Marquis (2005).

under consideration, there is a morphism between the two structured systems (mathematical or physical) that makes precise the claim that they share the appropriate kind of structure. I turn to consider, then, the extent to which recent approaches to scientific structuralism needlessly, and problematically, continue to presume a set-theoretic underpinning. Since such presumptions are most explicitly made in the shared works of Da Costa and French, I place my focus here. For example, Da Costa and French (1990), make clear their connection to Bourbaki:

> [i]n a certain strong sense it can be said that the axiomatic method defines the essence of mathematics… and with the work of the Bourbaki group it can be further claimed that this method reached perhaps its highest level of development… For Bourbaki, to axiomatize a mathematical theory was no more or less that to define a kind, or species, of structure in set-theoretic terms. (Da Costa & French, 1990, p. 252)

They go on to claim that the Bourbaki concept of a mathematical structure is purely syntactic,[12] but find hope in Suppes' semantic approach, which, as we have seen, begins with the claim that to *axiomatize* a theory is to define a set-theoretical predicate and, yet, a theory *is* the collection of its (isomorphic) models. Relying, then, on the work of Da Costa and Chuaqui (1988), which has "convincingly argued that these set-theoretical predicates can be identified with the Bourbaki species of structures" (Da Costa & French, 1990, p.253), they next claim that, given this identification,

> [a] scientific theory may thus be characterized by a set-theoretic predicate in such a way as to connect this approach with standard mathematical model theory… So, to axiomatize is to define a set-theoretical predicate… and the structures which satisfy this predicate are the models of the theory. That is, when a theory is formalized in this way, the mathematical structures which satisfy the predicate are the models of this predicate, or the structures of this species of structure. (Da Costa & French, 1990, pp. 253–254)

In their 1990 paper, Da Costa and French put this set-theoretic frame for a semantic view of the structure of scientific theories to work to provide an account of 'pragmatic structures' in terms of set-theoretic 'partial structures'. These are then used to offer an account of 'pragmatic truth', which is itself used both to inform the semantically framed empiricist/realist debate (as set out by, for example, Friedman, 1983; Giere, 1985; van Fraassen, 1980, 1985) and to offer a description of the role of analogy with the aim of distinguishing between iconic[13] and mathematical models and show too how the partial structures approach can account for the use of both in science.

In subsequent papers, French uses this set-theoretic framework to provide a semantic account of

[12] I, however, doubt this claim. Bourbaki's aim was axiomatic but it was not syntactic; identifying models, in the sense of Tarski, was as important as identifying 'species of structures'. This because their aim was not to supply a foundation for what mathematics is about; rather, it was to characterize an architectonic for mathematical structure. To this end, set theory was used to provide a taxonomy of the various types of structure in terms of 'species' of set-structures.

[13] An iconic model, taken in terms of Hesse (1963), Achinstein (1968), Suppe (1977) and Redhead (1980), is a concrete physical system that functions as an icon for another system in such a way that the properties that hold of the first can be said to hold, perhaps by analogy, of the second. For example, the solar system is often taken as an iconic model for the orbital theory of the atom. See also Suppes (1967b, pp. 290–291) where he characterizes the concept of an iconic model as a 'physical model' or, equally, as the 'physicists' concept' of a model and further explains how it, like mathematical models, can be formalized in terms of set-theoretic models.

(i) the *applicability* of a mathematical theory to a physical theory by appealing to a type of morphism to make precise the claim that their models (as set-theoretically framed 'structures') share the same structure.
(ii) the ontic[14] structural realist's appeal to the *structural continuity* between successive physical theories, again by appealing to a type of morphism to make precise the claim that their models (again, as set-theoretically framed 'structures') share the same structure.

Bringing these two uses together, Da Costa et al. (1997) seek to account for the structure of various kinds of space-time theories with the aim of offering the structural realist conclusion that "there is a common underlying structure — spatio-temporal in this context — about which we can come to know more and more. The appropriate representation of this structure, we believe, is in set-theoretic terms." (Da Costa et al. 1997, p. 277) In sum, attempts to formally frame the concept of shared structure between theories (either between mathematical and physical theories or between successive physical theories) by appealing to types of morphisms between their respective models (as types of set-theoretic 'structures') have been made in terms of:

(a) homeomorphisms between models *qua* types of lattice structures (Da Costa et al., 1997),
(b) partial isomorphisms between models *qua* partial structures in a function-space (French, 1999), and
(c) partial homomorphisms between models *qua* partial structures (French, 2000).

What remains open for discussion, and what underlies much of the realist/empiricist structural realism debates, is the question of whether we are to read the appropriate kind of structure from the world or from the theory.[15] Forgoing this question for the moment, what the success of the use of shared structure, in either case, is taken to rely upon is an attempt to make this concept *formally precise*. As noted, it is assumed that "without a formal framework for explicating this concept of 'structure-similarity' it remains vague…" (French, 2000, p. 114). What recent attempts to formally frame this concept have in common is that they seek to specify the kind of shared structure at work in terms of some *type* of morphism[16] between models as some *type* of set-structure.

[14] See page 11 for an albeit brief distinction between ontic and epistemic structural realism.

[15] For example, for the ontic structural realist like French the appropriateness of the models of a theory is supposed to rely upon 'how the world is'; the structure of the phenomena *qua* "nothing but structure" tells us that the world and the models (both data and theoretical models) share the same kind of structure. In contrast, for a structural empiricist like van Fraassen (1999), the models of a theory (if they are embeddable) tell us not about the structure of 'the world', but rather speak to the appropriateness of the theory for making claims about what we can know about the structure of the phenomena, i.e., it tells us about the empirical adequacy of the theory.

[16] Note that van Fraassen too makes this assumption; in his 1970, he makes use of isomorphisms between models as state-spaces (interpreted by Beth semantics); and in his 1980, he makes use of the embeddability of empirical substructures into theoretical structures, again as state-spaces. Yet, while his semantic approach seeks to formally frame the concept of shared structure in terms of some type of morphism, he (see footnote 6) remains open to the possibility that there may be frameworks other than set-theoretic. So while van Fraassen is committed to formally framing the concept of shared structure as some type of morphism, he is not committed to the Bourbaki/Suppesian claims that to do so (i) a model need be formalized as a type of set-structure or (ii) a theory need be formalized by a set-theoretic predicate.

In contrast to such attempts, I want to distinguish between semantic accounts that consider what the concept of shared structure *is* (what the appropriate type of structure is for *formally framing the concept* of shared structure in terms of some type of morphism) and those that consider what the presence of shared structure *tells us* (what the appropriate kind of structure is for *characterizing the use* of shared structure in terms of some kind of morphism as determined by some context), and to place focus on the latter. In so doing, we say simply that two models share structure if there exists a morphism between them that preserves the 'appropriate kind' of structure, regardless of our having to specify this kind as a precise type of morphism. Moreover, we note that the 'appropriate kind' of structure depends on the details of the particular task at hand.[17] Thus, what shared structure tells us cannot be ascertained by looking at the types of set-structures (or types of morphisms): the philosophical proof of the efficacy and utility of appeals to shared structure is in the scientific pudding, not the mathematical recipe.

In response, then, to French's (2000) claim, I want to suggest that neither 'structure similarity' nor 'similarity' is vague; both can be made precise by appealing to the concept of a morphism, but it is the context (and not the set-theoretic type) that determines the appropriate kind of morphism. I now turn to making use of French's own example from the development of quantum theory to demonstrate that, for both Weyl and Wigner's programmes,[18] it was the context of considering the 'relevant symmetries' that determined that the appropriate kind of morphism was the one that preserved the shared Lie-group structure of both the theoretical and phenomenological models.

4 Shared structure and applicability

In two papers (1999, 2000), French seeks to account for the role of group theory in quantum mechanics by appealing to the concept of shared structure, as formalized by some type of morphism, between models *qua* types of set-structures. In both papers, his aim is to show, by example, that the applicability[19] of a mathematical theory to a physical theory can be represented by employing

> a model-theoretic framework in which 'physical' structures are regarded as embedded in 'mathematical' structures… this then allows the possibility of representing the relation of mathematics to physics in terms of embedding a theory T in a mathematical structure M′, in the usual set-theoretic sense of there existing an isomorphism between T and a sub-structure M of M′. (French, 1999, pp. 187–188)

In particular, in his 1999 paper, French attempts to show that the shared structure between group theory and quantum mechanics can be formally expressed in terms

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[17] One might think that this contextualization of the concept of shared structural runs the risk of trivializing it, yet what I am pointing out here is that it is the *use* of the concept that determines the appropriate kind and so *characterizes its meaning* in the given context; this as opposed to taking the formalism as that which both *defines its meaning* as a type of set-structure and thereby *justifies its use* in all contexts.

[18] See Mackey (1993) for a more in-depth analysis of both the Weyl and Wigner programmes.

[19] Two types of applicability are herein considered; applicability in the sense of presenting a theory and in the sense of representing the phenomena. The focus of French (1999) is on the latter, while French (2000) considers both in terms of Weyl and Wigner's programmes, respectively.

of partial isomorphisms between models *qua* partial structures in a function-space.[20] French here claims that both the function-space approach and the state-space approach of van Fraassen (1980, 1989) "can be related to the more general and, perhaps, more fundamental set-theoretic approach advocated by Suppes" (French, 1999, p. 191).[21]

French then notes two caveats: (1) since a scientific theory typically only partially models its respective domain it is best characterized as a collection of models *qua* partial structures, so that the morphism that expresses the shared structure between such theoretical models is a partial isomorphism, and (2) since there is often structure that is surplus,[22] the relationship of shared structure between theoretical models and data models must also be expressed in terms of partial isomorphisms.[23] Finally, after setting up his set-theoretic framework, French concludes that "[i]t is precisely such a [Suppesian set-theoretic] framework that is required to capture the developing relationship between group theory and quantum mechanics" (French, 1999, p. 192; see also French, 2000, p. 104).

In detailing the historical development of the appropriate theory for quantum mechanics French makes the following 'observations':

(i) Dirac explicitly noted that the theory itself could not determine which form was appropriate but that extra-theoretical considerations (like satisfying Pauli's Exclusion Principle) had to be appealed to … it is this analysis of quantum statistics in terms of the permutation of indistinguishable particles which heuristically motivates the construction of the 'bridge' underpinning the embedding of quantum mechanics into group theory (French, 1999, p. 193).

(ii) As Weyl noted, it is with the representation of groups by linear transformations that their investigation became a 'connected and complete theory' and 'it is exactly this mathematically most important part which is necessary for an adequate description of quantum mechanical relations'… group theory, as the 'appropriate language' reveals 'the essential features which are not contingent on a special form of the dynamical laws nor on special assumptions concerning the forces involved' (French, 1999, p. 194).

(iii) … group theory gives us surplus structure which we rule out for purposes of application through the invocation of further physical principles, themselves to be embedded within the mathematics (as Dirac indicated in the case of the Exclusion Principle) (French, 1999, p. 195).

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20 See Redhead (1975) for the development of the 'function space' approach and Redhead (1980) for the use of this approach for analyzing the role of models in physics.

21 Lest one be tempted to interpret this quote as a reluctance on French's part to say that the set-theoretic approach is more, or indeed is the most, fundamental approach, I point the reader to the fact that French continues here with a reference to Suppes' claim that: "[b]y defining [scientific theories using] set-theoretic predicates … one can specify either a state space, [a function space], a relational system, or some other representing mathematical structure or class of structure" (Suppes, 1989, p. 4 quoted in French 1999, p. 190). To further support my set-theoretic reading of French, see also Da Costa et al. (1997) p. 277, where it is claimed that "[t]he appropriate representation of this [common underlying—spatio-temporal in this context] structure, we believe, is in set-theoretic terms."

22 The concept of model *qua* partial structure is thus used for two ends: to formally frame the concepts of both 'surplus structure' and 'openness'—these concepts are then used to account for the various sorts of models used in the sciences, e.g., iconic, mathematical, theoretical, etc.

23 This in contrast to both the Giere/van Fraassen appeal to 'theoretical hypotheses' to make the connection between theoretical and data models and to the Cartwright/Suarez claim that such a clear delineation of theoretical and 'phenomenological' models is not possible.

French uses these and other observations to fine-tune his account of applicability, claiming finally that

> [w]hat we have, roughly, is the following scheme: idealizations are introduced, such as ignoring the indistinguishable nature of particles, giving rise to models which can then be structurally embedded *in group theory*, which in turn *is used to generate the appropriate results*. (French, 1999, p. 197)

More specifically, in French 2000, he uses his partial structures approach to frame two contexts of applicability: one relating to foundations and the other to the representation of physical phenomena. To exemplify both, he considers the role of group theory for both the 'Weyl programme' (which was concerned with the group-theoretic elucidation of the foundations of quantum mechanics) and the 'Wigner programme' (which was concerned with the utilization of group theory in the application of quantum mechanics itself to physical phenomena). Of the Wigner programme, French notes:

> Wigner himself emphasized the dual role played by group theory in physics; the establishment of laws—that is, fundamental symmetry principles—which the laws of nature have to obey; and the development of 'approximate' applications which allowed physicists to obtain results that were difficult or impossible to obtain by other means. (French, 2000, p. 107)

French then details the history of Wigner's programme as motivated by the search for the mathematics that would represent the needed symmetries—permutation and rotation, and which would further take account of spin. He next recalls a point he made about Weyl's programme, viz., that "behind these 'surface' relationships there may lie deeper, mathematical ones" (French, 2000, p. 109). One such deeper relation is the reciprocity between the permutation and linear groups which, as previously noted, Weyl refers to as 'the guiding principle' of his work and also as the 'bridge' within group theory. It is in considering this 'bridge' that French concludes:

> [t]hus with regard to the construction of the 'bridge' between the theoretical and the mathematical structures, represented by T and M′ above, on the quantum mechanical side we have the reduction of the state space into irreducible subspace and on the group theoretical side we have the reduction of representation. It is here we have the (partial) isomorphism between (partial) structures, (weakly) embedding T into M′… Interestingly, then, the construction of this bridge … crucially depends on a further one within group theory itself – the bridge that Weyl identified between the representations of the symmetry and unitary groups as expressed in the reciprocity laws. (French, 1999, pp. 198–199; see also French, 2000, pp. 109–110).

My question is: Where is set-structure doing any real work? It seems to me that there are two things doing work: (i) the quantum mechanical principles and/or 'experimental' results expressed as group-theoretic symmetries and (ii) the group-theoretic formalism and corresponding 'internal bridges'. Now one might claim that, to connect the models of each, we must appeal to the concept of partial isomorphism (or, as in French, 2000, partial homomorphism) and that this is, strictly speaking, a set-theoretic concept. To this I have two replies: (i) mathematically speaking, morphisms need not be set-theoretically expressed, e.g., they can be expressed in category-theoretic terms, and (ii) the morphisms that are doing the work to connect the models in this example are group-theoretic, not set-theoretic, e.g., they can be more easily expressed as natural transformations in the category of Lie-groups. In any case, what does the *real work* is not the framework of set theory (or even category theory); it is the group-theoretic morphisms alone that serve to tell us what the appropriate kind of structure is. So, it is simply a mistake to conclude that

> [t]hus the appropriate model-theoretic formulation would be one involving partial structures in general or partial function spaces in particular [19] and that the relations between the corresponding structures would consequently be those of partial isomorphism. Furthermore, each theory, group theory and quantum mechanics, is itself structured, in the [set-theoretical] manner indicated above. (French, 1999, p. 201)

What explains French's jump from the *role* of group theory to the use of partial structures to the *frame* of set theory is found in a footnote (aren't they always!). French, in the above quote, has a footnote [19] which reads "[t]he present work can be viewed as an extension of this application [the model-theoretic approach] in line with [Suppes] remark that 'The set-theoretical definitions of the theory of mechanics. …' " and continues to quote Suppes' (1967b) Bourbaki-belief that all kinds of structures reduce to types of set-structures. (See also French, 2000, p.104, where this same quote is appealed to, resulting in the claim that "[w]ithin such a [Suppesian set-theoretic] framework the applicability of mathematics to science comes to be understood in terms of the establishment of a relationship between one kind of structure and another".) Again, I ask: Why is such a set-theoretic (Bourbaki) bias, if not a more robust mathematical foundationalism, built into this project?

5 Shared structure and structural realism

In an attempt to answer this question, I now turn to the possibility that this assumption is required for the other use of shared structure, viz., to run the structural realist argument that there is continuity of structure between successive theories and that this structure is what we should be realists about. More specifically, the relationship of 'structural continuity' is of crucial interest to structural realists in their attempt to overcome the so-called 'pessimistic meta-induction' argument and, in so doing, to make way for a modified version of the 'no miracles' argument. The 'pessimistic meta-induction' argument relies upon the existence of radical ontological discontinuities between explanatorily and predictively successful predecessor and successor theories to argue that there is no good reason to believe in the 'things' that current and/or future theories might posit. The structural realist strategy for overcoming the associated pessimism, as proposed by Worrall (1989), depends on the claim that discontinuity at the ontological level is nonetheless accompanied by overall continuity at the structural level and that it is this 'structural continuity' that explains the success of those theories.

In support of the assertion of structural continuity between predecessor and successor theories, and this despite 'radical changes in ontology', Worrall points out that, for example, the mathematical equations of Fresnel's theory of light can, in a rough and ready way, be recovered from Maxwell's theory. Continuity of structure (as expressed by the equations) is maintained despite the fact that the two theories disagree over such weighty ontological issues as the 'nature' of light; this nature changing as we move from Fresnel's theory of light—replete with its hypothesized aether and conceiving of light as a 'aether-wave'—to Maxwell's theory according to which light is an electromagnetic wave (believed to propagate in a mechanical aether). The suggestion is that, by restricting ourselves to the relationship of shared structure between predecessor and successor theories, we are able to recover the needed continuity through theory change, and so are in a position to offer a structural realist version of the 'no miracles' argument.

For the structural realist, read now as an advocate of the semantic view,[24] the point of the above example is that this relationship can be expressed in terms of shared structure between the *models of* Maxwell's theory of light and those of Fresnel's theory. More generally, characterizing a theory as a collection of models (or family of structures) allows one to account for structural continuity by appealing to the shared structure between models of the predecessor and the successor theory. Seen in this light, a side-aim of French's semantic approach is to make the way clear for a structural realist argument;[25] he characterizes a scientific theory as a collection of models *qua* partial structures so that he can account for the structural continuity of successive theories by appealing to their shared structure as expressed by some type of morphism between their respective models. Thus, near the end of French 1999 we read that "our understanding of such developments [of the role of group theory in quantum mechanics] may impact on certain philosophical positions [such as structural realism]" (French, 1999, p. 201).

I will not go into great depths of detail of the debates surrounding the various versions of structural realism. For the purposes of this paper, all we need to know is that the structural realist attempts to move past problems typically associated with the traditional realist presumption of continuity of ontology by placing their focus instead on continuity of structure. In so doing, it is further claimed that one can be a realist about such structure in two ways: epistemologically (and claim that all we know is structure) or ontologically (and claim both that all we know is structure and all there is, is structure).

Setting aside epistemic or ontic options, French asks an interesting question: Given the story just told about the application of group theory to quantum mechanics, which structures are we to be realists about? That is, "are we to focus on the equations of quantum mechanics, such as Schrödinger's equation, or on what Weyl calls 'the appropriate language', namely group theory?" (French, 1999, p. 202). French notes that, when we consider (both vertical and horizontal)[26] shared structure, our emphasis seems clearly on the latter type of structural continuity, this because "[w]hat the group

[24] See Ladyman (1998) for an excellent critical overview of the distinctions between the syntactic and semantic, and the epistemological and ontological, versions of structural realism.

[25] Here I have in mind the *ontic* structural realist semantic approaches of French (1999, 2000). Given Redhead's (1995, p. 18) stated belief that "detailed historical analysis often reveals more continuity than one suspects, at any rate at the level of structure rather than ontology", one might also include Redhead's (1980) function-space account as having a structural realist agenda as well. But it is far from clear whether he would approve of French's ontic stance. Telling against such an ontological reading of Redhead is the following remark found in French (1999), p. 204: "… it has been argued that it makes no sense to talk of structure without its component elements (Redhead, private discussion)".

[26] Vertical shared structure refers to the shared structure between models at different levels, for example, between higher-level theoretical models and lower-level phemomenological/data models. Horizontal shared structure refers to the shared structure between models at the same level, for example, the shared structure of two theoretical models of the same theory.

theoretic approach does … is embed such models in the more abstract representation of semi-simple Lie groups which set quantum theory in a unitary framework" (Ibid.).

In choosing to consider group theory as 'the appropriate language', and, in addition, adopting an ontological stance towards structural realism, French is left to face the following Psillos-inspired[27] problem: How can we talk of a group if we have done away with the elements that are grouped? French's reply is as follows:

> [w]e begin with a conceptualization of the phenomena… informed by a broadly classical metaphysics … in terms of which the entities involved are categorized as individuals. That categorization is projected into the quantum domain, where it breaks down and the fracture with the classical understanding is driven by the introduction of group theory; the entities are classified via the permutation group which imposes perhaps the most basic division into 'natural kinds', namely bosons and fermions. It is over this bridge that group theory is related to quantum mechanics as indicated above. (French, 1999, p. 204)

Making his ontic structural realist[28] conclusions more explicit, French notes that

> [t]he introduction of group theory into quantum mechanics provides a useful example, and one that has important … implications. Metaphysical: the very basis of the applicability of group theory lies in the non-classical indistinguishablity of quantum particles so that their permutation can be treated as a symmetry of the system. Furthermore, this emphasis on symmetry and invariance subsequently led to a metaphysical characterization of elementary particles as, ontologically, nothing more than sets of invariances. Epistemologically: this latter characterization can be adduced as a further aspect of an ontological version of structural realism which claims not simply that all we can *know* about the world is its structure but all that there *is* about the world is this structure… (French, 2000, p.103)

But again, I ask: Where is set-structure doing any real work? In French's story, clearly what 'drives' and 'imposes' our quantum mechanical 'natural kinds' is the shared Lie-group structure of the models (again, both vertical and horizontal); so why then do we need the additional claim that all such group-theoretic kinds are set-theoretic types? To this query, French might reply: "Well, yes in the *particular* example of quantum mechanics, and in so far as group theory provides *a* framework, what does the work is shared Lie-group structure. But I am looking for a *general* framework — *the* framework that frames the structure of all scientific theories, and, in so doing, not only accounts for the use of a kind of shared structure but formally frames the very concept in terms of some preferred type. In this *general context* this frame is provided, respectively, by a type of set-structure and a type of morphism. And, thus, what frames this semantic approach is set theory". But what do we gain from this frame of frames?[29] And, perhaps, more importantly, what do we lose?

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[27] See Psillos (1995, 2001) for exact details of his criticisms of structural realist attempts to separate nature from structure, relata from relations, etc.

[28] For critiques of French and Ladyman's (2003) more robust ontic structural realist arguments for abandoning an individuals-based ontology in favor of a purely structuralist account of ontology, see Cao (2003a, b), Chakravarrty (2003), Morganti (2004), and van Fraassen (1999).

[29] Again, one might be tempted to argue that a frame of frames is good for pragmatic reasons; that it provides us with a unified account of what a model *qua* structure *is* and so accounts for what a morphism *is* (as a map between structures so construed). However, what all of French's examples from

To highlight the philosophical impact of the above queries, and show just what we lose, I borrow a quote from Suppe's criticism of the syntactic/linguistic approach:

> Such an approach [to understanding scientific theories by an examination of the linguistic formulations of theories] can provide a detailed analysis of the characteristic features of theory formulations, but unless one assumes that analogues to these features are distinctive feature of theories themselves; (fn #550, For example as the early Wittgenstein assumed that the logical structure of reality was mirrored by the logical structure of a logically perfect language … [viz., first-order logic]) such an analysis reveals nothing about what is characteristic of theories except that their formulations have certain characteristics … Even if all the distinctive features of theory formulation were reflections of characteristics of theories, there is no guarantee that the most distinctive or characteristic features of theories are mirrored in the formulations of theories. (Suppe, 1977, p. 221)

Let's consider the above quote now, however, set-theoretically reconstructed:

> Such an approach [to understanding scientific theories by an examination of the *set-theoretic* formulations of theories] can provide a detailed analysis of the characteristic features of theory formulations, but unless one assumes that analogues to these features are distinctive feature of theories themselves; [For example as the early *Bourbaki* assumed that the *mathematical* structure of reality was mirrored by the *mathematical* structure of a *mathematically* perfect language [viz., *set theory*] …) such an analysis reveals noting about what is characteristic of theories except that their formulations have certain characteristics … Even if all the distinctive features of theory formulation were reflections of characteristics of theories, there is no guarantee that the most distinctive or characteristic features of theories are mirrored in the formulations of theories.

How, then, is the set-theoretically framed semantic approach any different than the linguistic?

Footnote 29 continued
physics show is that what does the *real work* , i.e., what accounts for (a) the structure of a scientific theory, (b) the applicability of a mathematical theory to a physical theory, and (c) the structural realist's appeal to the structural continuity between successive physical theories, is a specific kind of structured system and so a specific kind of morphism. My point is that this explanatory role is lost if we then reduce this specific kind to a more general set-(or category-) theoretic type. For example, in the case of considering the role of group theory in quantum mechanics, it is shared Lie-group structure that does the real work of explaining these uses and this explanatory role is lost if one reduces group-structures to partial-structures to set-structures (and likewise if one were to reduce them to cat-structures). That is, if group structures *really are* partial structures which *really are* set structures, then, for example, group-theoretic symmetries *really are* set-theoretic relations between either sets or elements. So, contra both what the history shows and what French argues, if we accept the set-theoretic story, what should do the work of accounting for the structure of quantum mechanical theory and quantum mechanical phenomena is set-theory, not group theory. Clearly, however, this is not so. And, as already noted, even if we appeal to the appropriate kind of morphism to capture the concept of shared structure, this latter concept is not trivialized without a formal framework which defines what we mean by morphism. Indeed, what we mean by both the appropriate kind structured system and the appropriate kind of morphism is fixed by its use in the context we are considering. Thus, whatever we might gain in philosophical unity by formally framing the concept of either structured system or morphism we lose in explanatory value.

French may find a response by appealing, in Suppes (1967a) style, to the distinction between intrinsic and extrinsic characterizations of theories.[30] He may claim that the linguistic formulation of a theory by a set-theoretic predicate is an intrinsic characterization to be used only to take up an epistemic stand on the truth of a theory and yet that what does the work of scientific representation is the extrinsic characterization of a theory as a collection of models (or family of structures). That is, if we recall that the formal framework is intended to be used to provide a semantic account of (a) the structure of a scientific theory, (b) the applicability of a mathematical theory to a physical theory, and (c) the structural realist's appeal to the structural continuity between successive physical theories, French might well respond that the intrinsic (set-theoretic/Bourbaki) characterization is only needed for (a), but the extrinsic (model/structure-theoretic) characterization is what accounts for (b) and (c). But as French himself notes, the work of scientific representation "concerns the structure of the theory, and the relationships between theories themselves and between theories and 'the world', understood in terms of that structure" (French & Saatsi 2005 manuscript, pp. 5–6).

In sum: I agree that what does the work of representation is a model *qua* an appropriate kind of structured system and what captures the needed concept of shared structure between them is the appropriate kind of morphism. But I think that what French's own example from quantum mechanics shows is that what determines the appropriate kind of morphism is the context under consideration. In the case of considering Lie-group structure as the appropriate kind of structure in quantum mechanics, as the Weyl and Wigner programmes exemplify, it was both the foundational context and the phenomenal context. And, in the phenomenal context, if one wants—as Wigner seems to have, and as the structural realist needs—to use this kind of structure as a tool to carve 'the world' into its 'natural kinds', then one cannot, in addition to claiming that group theory is 'the appropriate language', claim that all such group-theoretic kinds are set-theoretic types, *unless* one is ready to hold fast to, and provide justification for, the Bourbaki/Suppesian assumption that all scientifically useful kinds of mathematical structures *are* types of set-structures. Nor can one use this assumption to make a more robust, ontologically read, structural realist claim about the structure of 'the world', unless one wants to impose (or presume) that set theory cuts not only mathematics but, indeed, Nature at its joints.

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[30] See French (2005) where he says "[f]rom the intrinsic perspective… theories [as characterized by sentences expressed in a particular logico-linguistic formulation] can be taken to be the objects of epistemic attitudes, and be regarded as true, empirically adequate, approximately true, or whatever.", whereas, the extrinsic characterization [of a theory as collection of models] "concerns the structure of the theory, and the relationships between theories themselves and between theories and 'the world', understood in terms of that structure. From the 'extrinsic' perspective we regard theories from 'outside' a particular logico-linguistic formulation and it is in this respect that models play a representation role." (French (2005) manuscript, pp. 5–6).

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