Models and structures: Phenomenological and partial

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ABSTRACT

In a recent paper, Suárez and Cartwright (2008) return to the example of London and London’s construction of a model for superconductivity and raise a number of concerns against the account of this construction presented in French and Ladyman (1997) and elsewhere. In this discussion note, we examine the challenge they raised and offer our responses.

1. Introduction

Suárez and Cartwright (2008) have recently returned to the example of London and London’s construction of a model for superconductivity in order to raise various concerns against the account of this construction presented in French and Ladyman (1997) and elsewhere. In this discussion note, we discuss the challenge they raised and provide our responses.

2. Borrowing and phenomenological models

Suárez and Cartwright (2008, p. 63) suggest that what is going on in the construction of this model is a kind of piecemeal borrowing from an earlier model of superconductivity: borrowing that takes some assumptions but leaves others behind. On this we agree (see Bueno, French, & Ladyman, in press), although the fact that this borrowing was informed by theoretical factors, as well as empirical, undermines the inter-twined claims that the London and London case is an example of phenomenological model building and that it exemplifies model building that is independent of theory in methods and aims (Cartwright, Shomar, & Suárez, 1995, p. 148; for criticism, see French & Ladyman, 1997).

The claim that the model construction here was phenomenological in the sense of being detached from theoretical considerations is not borne out by the principal participants’ own understanding of how the model was constructed. Fritz London’s classic text Superfluids (1950) expresses very clearly London’s decidedly firm opinion on whether the model should be counted as phenomenological or not:

the present theory [. . .] is not, in a strict sense, a phenomenological theory of superconductivity, as it has occasionally been considered. Although it was originally aimed at merely bringing the two phenomena, superconductivity and the Meissner effect, under a common principle, it actually goes considerably beyond what is given by measurements on matter in bulk (London, 1950, p. 30; his emphasis).

Indeed, as Gavroglu notes, on being awarded the Lorentz Medal in 1953. London made it clear that although the equations “have often been called phenomenological [. . .] actually they go beyond what is directly given by the phenomena” (Gavroglu, 1995, p. 144; see also pp. 247–252). And they go beyond in terms of the sketch of the ‘microscopical’ interpretation of the London–London model, where this interpretation was set, in large part, by the diamagnetic analogy (Bueno et al., in press). That

1 Having studied superconductivity for his final year project of the physics degree in 1978 and having read this text in particular, French was very much aware of London’s views on the matter, when he worked with Ladyman on the 1997 analysis.
London should hold such definite views about the status of his theories should not come as any great surprise, given his philosophical background and his own well-developed philosophy of science (see French & Ladyman, 1997).

More importantly, we contest the claim that the construction of the London–London model proceeded largely independently of theory in method and aims. With regard to the aims of London and London's work, it is here that London's intentions and his own view of the work with his brother come into play. It is not by chance that he made it clear that the aim was not to construct a phenomenological model (in Cartwright's subsequently elaborated sense), but to develop what he called a 'macroscopic' description or interpretation of superconductivity that would function as the necessary reformulation of the phenomenon prior to developing a new theoretical, or as he called it, microsopic, explanation. Such an explanation, although deficient in not offering all the details of the mechanism involved, did at least reduce the range of possible such mechanisms. This programme subsequently became, in Gavroglu apt phrase, a "valuable heuristic" for the work of Bardeen and the idea of coupled electrons that the Londons suggested came to be expressed in the concept of "Cooper pairs" (Gavroglu, 1995, p. 209). Thus, the aims of the Londons seem entirely bound up with theoretical considerations.

As for the method involved, although the discovery of the Meissner effect was a key experimental result, it would be astonishing were theoretical developments not responsive to new experimental work in this way. What it drove was a shift in the over-riding analogy, from that with ferromagnetism in the case of the old 'pre-Meissner' model, to that with diamagnetism in the case of London and London's. It is crucial to emphasize the role of this analogy in understanding the London and London model (see French & Ladyman, 1997), following the explicit use by the Londons themselves (London & London, 1935, p. 88; London, 1935, p. 26) and the emphasis placed upon it in the work of Dahl (1992, p. 179) and Gavroglu (1995, p. 128, for example). As a result, once again, theoretical considerations become crucial.

3. Partial structures, analogy and theory change

Suárez and Cartwright (2008) also challenge the view that the above developments can be captured within the partial structures account (see da Costa & French, 2003). In particular, they claim that two problems arise with this account:

**Problem 1.** The representations involved capture almost none of the interesting features of how the transition occurred, including what kinds of motivations the Londons had, what techniques they were familiar with, what information they had to hand, etc. Suárez and Cartwright "see no way to express these common assumptions as relations between set-theoretical structures" (2008, p. 74).

Of course to try to represent set-theoretically London and London's motivations would be an entirely misconceived endeavour, akin to the attempt by set-theoretic structuralists to accommodate sociological factors in theory change by letting $S$ be a set of scientists. The factors that Cartwright and Suárez mention will feature in our account indirectly, via the heuristic factors that they manifest in practice. This perhaps represents one of the fault lines that separate the two sides on this set of issues, and it can be posed in the form of a question: to what extent should the scientists' intentions, motivations and psychological states feature in our account, at the level of the philosophy of science, of their practices? Cartwright and Suárez appear to hold that such aspects should feature directly in such an account, whereas we maintain that they are accommodated indirectly, through the appropriate meta-level (that is, at the level of the philosophy of science) representation of the relations that hold between theories and theories and data etc., together with the heuristic moves that establish such relations. Certainly we maintain that we can directly capture the information that scientists such as the Londons had available to them, in the form of these relations with other theories and models and background information in general.

**Problem 2.** The application of partial isomorphisms is inappropriate because the theories do not contain any partial relations. Furthermore, the two models—the older and the London–London—are not isomorphic since the superconductors in each case have distinct properties. This characterisation doesn't include the crucial shift to the diamagnetic analogy. Cartwright and Suárez argue that this shift cannot, in fact, be accommodated via partial isomorphisms:

The acceleration equation theory asserted (and this is precisely why it had to be abandoned) that all superconductors conserve the flux during the phase transition; so according to that theory all superconductors are like ferromagnets [. . .]. Hence there is no partial isomorphism between the two theories (Suárez & Cartwright, 2008, p. 75).

In response, while we agree that the two models in question are of course incompatible, the relevant relations need to be sought and characterized at a different level, where the role of the diamagnetic analogy will be critical. Thus, as French and Ladyman (1997) emphasized, with regard to London's claim that a superconductor could be considered to be a single diamagnetic atom:

Obviously a 'total' superconductor is not, in fact, a single diamagnetic atom, but it can be regarded as such because its behaviour resembles that of such an atom. This relationship of resemblance can be captured, we claim, by the notion of a partial isomorphism holding between the relevant models. Thus, a subset of the relations common to the models of diamagnetic behaviour will also feature in London and London's model of the superconductor: in particular those expressing the dependence of the current on the magnetic field (a dependence which defines diamagnetism). The heuristic value of analogy can also be spelled out in terms of similarity of structure, in turn understood by means of partial isomorphisms holding between the theoretical structures concerned (French & Ladyman, 1997).

So, just to be clear, the partial isomorphism here holds between the London and London model and models of diamagnetic behaviour. Likewise the appropriate relation holds between the older model and that of a ferromagnet. Clearly, the inter-relationships between these analogies and the shift to the London and London model are complex, but as long as we understand the appropriate level of representation, there is nothing that prevents partial structures from being used in this context (see also Bueno et al., in press).
4. Autonomy and mediation

Suárez and Cartwright (2008) emphasise that their work can be set within Morrison’s framework that holds models to be autonomous, although it has to be said that this was not apparent from the Toolbox paper (Cartwright et al., 1995) to which French and Ladyman (1997) were responding. They also reject the subsequent attempts to characterise and remove the epistemic sting from this sense of autonomy (see, for example, Bueno, French, & Ladyman, 2002; da Costa & French, 2003). Thus they write that models are autonomous from theory in the sense that “they play essential roles that are separate from any role they play in constituting theory” (Suárez & Cartwright, 2008, p. 64). This corresponds to what Morrison calls “functional autonomy”, and we have no fundamental problem with this sense of the word. Indeed, it is surely a matter of common agreement between all of us who have focused on the roles of models in science that these may include acting as the locus for knowledge claims, providing the basis for further developments.

However, we have previously tried to articulate and define an alternative sense of autonomy that we thought Cartwright and Suárez had in mind, which was related to the above-mentioned contentious notion of being independent from theory. It seemed to us that there were two forms of autonomy one could discern in this context: a model might be autonomous from theory in the sense that it could not be related to or deduced from high level theory; or it might be autonomous in the sense that at the time it was proposed, the relevant relations to the appropriate theory could not be discerned or established (think, for example, of the liquid drop model of the nucleus; da Costa & French, 2003). The second sense seems to us to be uncontentious; the first is problematic, insofar as one is left wondering as to the grounds for the model construction in the first place (for further discussion, see Bueno et al., in press).

Nevertheless Cartwright and Suárez reject both senses because for them theory independence and its counter-part, theory-drivenness, have nothing to do with deducibility; rather what is at issue here is how changes that cannot be deduced from pure theory are justified (Suárez & Cartwright, 2008, p. 68). Thus:

The model is independent from theory if it was not built as a de-idealization of high-level theory by improvements legitimated by independently acceptable descriptions of the phenomena (Suárez & Cartwright, 2008, p. 68).

In this sense they agree that the independence is historical and the autonomy that grounds it is only temporary. However, this seems a much narrower and less contentious sense of independence than we took them to be defending in the Toolbox paper (Cartwright et al., 1995). If this is all that is at stake then we can come to an agreement on this score: we certainly agree that not all models are obtained by de-idealizing moves from high level theory. But of course they are tied to theory through other kinds of moves and in this, perhaps more interesting sense, they are not independent, nor are they autonomous.

5. Structuralism without structure

Cartwright and Suárez also attempt to reach an accommodation between the semantic approach and their ‘toolbox’ view of theories. They characterise us as adhering to the (perhaps standard) view that there is one kind of relation (e.g. partial isomorphism) that can be used to represent the relationships that hold between all models, so that a global account of modelling can be obtained (e.g. in terms of partial structures). They oppose this with their own suggestion that for the given models, a set of relations can be found that will hold between them but insist that this is different for different cases.

Now, their characterisation is not quite correct in at least one sense, since we allow that although the inter-relationships between physical structures are most appropriately captured by partial isomorphisms, those between mathematical structures and both physical structures and other mathematical structures are best captured via partial homomorphisms (Bueno et al., 2002). So it is certainly not a case of one relation fits all. With that in mind, we do think that all the examples from scientific practice we have come across so far can be represented in terms of partial structures in this way. Of course, there may be examples where it can be argued something other than partial iso- or homo-morphisms are required and in that case we would hope that the partial structures programme can be further expanded to accommodate these further morphisms. Furthermore, there are of course other versions of the semantic approach on the market, as it were, and it may be that one of these is better at capturing certain aspects of scientific practice. In that case we would be prepared to adopt a more pluralist position, along the lines we have already adopted with regard to category theory (see da Costa & French, 2003). The latter may well be a more appropriate framework for representing certain structuralist commitments, but we believe our set-theoretic account better accommodates the kinds of theory–theory and theory–data relationships that the philosopher of science is interested in (see French, 2010 & Bueno, 1997).

Cartwright and Suárez’s own attempt to come up with a more liberal form of the semantic approach has limited appeal. Here they adopt a Suppesian hierarchy of levels of models à la Bueno (1997), but insist that each layer in the hierarchy may involve a change of elements in the relevant domain or a change in the set of relations. The move from one level to the next is not grounded upon any features of the structures themselves or on the relations that hold between them but upon what Kaiser (1991) called “inference tickets”. This, they say, can precisely accommodate the way in which models can be constructed independently of theory.

Now, there is nothing in the partial structures approach per se that is inherently incompatible with the role of “inference tickets” (cf. da Costa & French, 2003). And this is because such tickets are not themselves primitive or ungrounded but ride on the back of the relevant relationships between the structures, relationships that can then be captured by partial isomorphisms, homomorphisms or whatever. This is what is unclear about Cartwright and Suárez’s claim above: if the move from one structure to the other is not grounded upon any features of the structures themselves, then what licences the issuing of the relevant inference ticket? Suppose the given structures did not possess any of the relevant features that would licence a move from one to the other; then obviously no such move could be justified (consider, for example, a structure plucked from loop quantum gravity and another taken from molecular biology). If they do possess the relevant features, then appropriate relationships can be established between them and those can in turn be represented, within the set-theoretic framework of the semantic approach, by the kinds of relations we have been discussing here.

6. Conclusion

Fundamentally, Cartwright and Suárez are unclear about the nature of the game. It is not the case that we have, in some sense, “raw” structures and we then seek to establish set-theoretic relations between them; rather it is that we—philosophers of science, that is—are presented with the structures that arise out of scientific practice and we then characterise, or, if one prefers, represent, these structures in such a way as to illuminate those features that we, as philosophers of science, are interested in.
This may open the door to some form of conciliation since of course we, and Cartwright and Suárez, may just be interested in different features of science and perhaps some form of pluralism might be adopted\(^4\). Nevertheless we think that for those features that we take to be central to the philosophy of science—namely those that have to do with the inter-relationships between theories, between theories and data models, between theories and mathematical structures and so on—the partial structures not only offers a unitary framework in the sense of capturing all aspects of those inter-relationships, it also offers a significant framework, since its inherent partiality allows it to accommodate the openness, multifacetedness and complexity of the scientific enterprise.

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References


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\(^4\) A form of conciliation is in fact suggested in Le Bihan (in press) as part of the defense of a modest version of the semantic approach.