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Models, Theories, and Structures: Thirty Years On

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Thirty years after the conference that gave rise to *The Structure of Scientific Theories*, there is renewed interest in the nature of theories and models. However, certain crucial issues from thirty years ago are reprised in current discussions; specifically: whether the diversity of models in the science can be captured by some unitary account; and whether the temporal dimension of scientific practice can be represented by such an account. After reviewing recent developments we suggest that these issues can be accommodated within the partial structures formulation of the semantic or model-theoretic approach.

1. Introduction: The Received View of Models. One of the many things that *The Structure of Scientific Theories* did was to thoroughly and forensically analyze the demise of the Received View. A critical factor in its death was the apparent failure to adequately accommodate the nature and role of models in scientific practice. Looking back thirty years we get a strong sense of what that well known philosopher of sport, Yogi Berra, called "déjà vu all over again": in 1968 Achinstein presented an array of different kinds of models and argued that they simply could not be embraced by the Received View (Achinstein 1968), while McMullin insisted that, when it came to model construction and elaboration, "the logician leaves aside

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the temporal dimension of scientific procedures" (McMullin 1968, 390). Returning to the present, we find these two claims reprised in a series of criticisms of the Semantic Approach. Before considering these more recent works, it is useful to briefly remind ourselves of the earlier accounts.

According to the Received View, a model is a *model for a theory* and thus just another interpretation of the theory's calculus (see, e.g., Braithwaite 1965). Why, then, use models at all in scientific thinking? Here Braithwaite, for example, draws a distinction between the 'modelists' and the 'contextualists'. The former answer that models are needed to provide an *understanding* of the theory. According to the 'contextualist', however, such understanding is given by the (theoretical) context, where this requires a semantic ascent from an uninterpreted calculus to an interpretation. Models are therefore ultimately dispensable (cf. Carnap 1939, 68). At best they have an aesthetic, didactic, or heuristic value (ibid.) but are 'quite unnecessary' when it comes to understanding or successfully applying a theory.

Moving forward thirty years, this answer has been seen as too meager even by the Received View's own standards. If models are just interpretations, Psillos asks, "Why not abandon [them] altogether and go just for theories?" (Psillos 1995, 109). That is just what Braithwaite and Carnap do, in effect. Of course, the Received View's answer seems too meager by *current* standards which call upon models "to concretise, specify or approximately realise assumptions about the physical system described by the theory, as for instance is the case with the Bohr model of the atom and its embedding atomic theory" (ibid.). This is to insist on the 'modelist' view of models as *models of systems*. The instance given of the Bohr model is telling: part of the motivation for the Received View's insistence that understanding should not be tied to the provision of a model surely lies with the perception that, as standardly interpreted, no such models and hence no such understanding could be supplied for the powerful new quantum mechanics that ultimately swept aside Bohr's model.

However, not all theories are like quantum mechanics; does this account put the Received View radically out of step with scientific practice? Not necessarily: as an interpretation a model need not be sound; its initial propositions need not be true, or thought to be true (Braithwaite 1965, 225). As Braithwaite acknowledges, "Scientists frequently use quite imaginary models" (1965, 226), giving as an example 19th century mechanical models for optical theory (Psillos 1995). At this point the critic may focus on the *structures* involved: it is implausible to require that the formal structure of the theory and model be *identical*. The 'imaginary' optical models are not an interpretation of the calculus of any theory that would be recognized as such (Achinstein 1968, 239); the 'identity' at work here is typically only *partial*.

Nagel set greater store by the use of models, but still, by regarding them as just another interpretation of the theory, albeit one that was articulated "in terms of more or less familiar conceptual or visualisable materials" (Nagel 1961, 90), they are incorporated within 'as-if' thinking. Hence it would seem that the Received View must answer in the negative a second question: "Can models ever be a tool of discovering the furniture of the world?" (Psillos 1995, 110; cf. McMullin 1968, 385). Is the Received View then committed to antirealism (Psillos 1995, 111)? Not necessarily; a proponent might argue that insofar as an 'as-if' attitude is *not* adopted with regard to those aspects of scientific practice that we are nevertheless uncomfortable in regarding as fully-fledged theories, they can be called '*theoruncula*', or (affectionately) '*theorita*' and still be understood in terms of interpretations of a calculus (Braithwaite 1965, 225).

Nagel's characterization takes the model supplied as part of the (partial) interpretation of the theory as both a semantic model and an iconic one (Suppe 1977, 96–98). Thus the billiard ball model of a gas is iconic in that a collection of billiard balls is obviously only similar and not identical to a collection of atoms; and it is semantic in that an interpretation is given to the theorems of the theory in terms of the system of billiard balls such that the theorems come out true under the interpretation. However, Suppe argued that if the theorems are empirically true, then the semantic model would be the world and the billiard ball model could never be such. The likes of Braithwaite might reply that this is precisely why models are so dangerous: they encourage a confusion of their domain with that of the theory.

What about McMullin's temporal dimension? Here it is worth recalling Carnap's insistence that the interpretation of the calculus is necessarily incomplete and hence the system is left open to allow for the addition of new correspondence rules. The partiality of the interpretation and the openness of the system is expressed via the incompleteness of the set of such rules. As far as 'normal' science is concerned, what we have is the steady addition of new rules and the continual modification of the interpretation.

As we all know, these correspondence rules were regarded by Suppes (1962) as obscuring the complex structural relationships between theories and the data and by Suppe as posing problems for theory individuation when change occurs at the experimental level (1977, 102–109). Both rejected the Received View for identifying a theory with its linguistic formulation—the combination of theorems and correspondence rules. Instead, theories are seen as extralinguistic entities which can be described by but not identified with, their linguistic formulations (Suppe 1977, 221–230). In Suppe's terms, if such a formulation is empirically true it will describe both the actual world and the theory; hence the formulation will have two mathematical models. And if it is accepted that if the theory is correct then it can be said to stand in an iconic relation to the world, then the relationship between these two models is also ('probably') iconic. The issue of iconic models was thus addressed by the Semantic Approach from early on.

2. Thirty Years On. Eight years on, in the 'Afterword' to *Structures*, Suppe claimed that "The semantic conception of theories . . . is the only serious contender to emerge as a replacement for the Received View analysis of theories" (1977, 709). Twenty years on, he insisted that "The Semantic Conception of Theories today probably is the philosophical analysis of the nature of theories most widely held among philosophers of science" (Suppe 1989, 3). Thirty years on and where are we? A vast amount of work has been undertaken on the nature of scientific models, their uses, and their relationship with theory. The Semantic Approach has been developed further and applied across a range of case studies by van Fraassen, Giere, Hughes, Lloyd, Thompson, and Suppe himself. It is important to recall that at the heart of this approach lies the fundamental point that theories are to be regarded as *structures*. In modern mathematics, as is well known, the general notion of 'structure' has undergone extensive development, culminating in the (essentially syntactic) treatment of Bourbaki. The Bourbaki species of structures can then be identified with Suppes' set-theoretical predicates, thus providing the bridge to standard (mathematical) model theory (da Costa and Chuaqui 1988). In these terms, to axiomatize a theory is to define a set-theoretical predicate and the structures which satisfy this predicate are the models of the theory.

However, even granted these developments, Suppe's claim might seem optimistic. At one extreme the structuralists continue to unfold their programme, struggling to overcome the criticism that the scholastic niceties of theory holons, partial potential models, and so forth, have been gained at the cost of a shallow analysis of the practice supposedly represented (Truesdell 1984). Despite recent attempts at greater depth, the programme occasionally dips into the absurd, as when the social aspects of science are accommodated by waving the set-theoretic wand with the magic words "Let S be the set of scientists . . . !"

Towards the other extreme, the 'foregrounding' of practice leaves the structural aspects in the dark. Cartwright, famously, has argued that much of what goes on in science involves modeling which is independent from theory in methods and aims (Cartwright, Shomar, and Suarez 1995). In similar vein, Morrison has suggested that models may be 'functionally autonomous' from theories and hence 'mediate' between them and the phenomena (Morrison forthcoming). This emphasis on the autonomy of models brings with it a welcome re-focus on both their diversity and heuristic rôle. In both cases a challenge is set for the Semantic Approach just as it was for the Received View; can it be met?

3. The Autonomy of Models. The focus of Cartwright et al.'s concern is the 'covering law' account of the relationship between theories and models. Thus they claim that

> This account gives us a kind of homunculus image of model creation: Theories have a belly-full of tiny already-formed models buried within them. It takes only a the midwife of deduction to bring them forth. On the semantic view, theories are just collections of models; this view offers then a modern Japanese-style automated version of the covering-law account that does away even with the midwife. (Cartwright, Shomar, and Suarez 1995, 139)

The counterexample they give is that of the London and London model of superconductivity which, they claim, was developed at the phenomenological level, independent of theory in methods and aims. This latter claim has been considered and rejected elsewhere (French and Ladyman 1997); what we want to focus on here is the assertion that the semantic view is nothing more than an up-to-date version of the covering-law account. The argument goes as follows: According to the Semantic Approach theories are families of mathematical models; if this approach is an adequate representation of scientific practice then any scientific model should feature as a member of such a family; however, there are models which do not so feature, since they are developed independently of theory; hence the Semantic Approach is not an adequate representation of scientific practice. While valid, this argument is not sound since the second premise does not represent the correct understanding of the semantic approach's view of models. Let us suppose it is true that there exist models which were developed in a manner that was independent of theory. Still, they can be represented in terms of structures which satisfy certain Suppes predicates.

Similar remarks apply to Morrison's account of the 'autonomy' of models. She presents the example of Prandtl's model of a fluid with a very thin boundary layer (Morrison 1999). This solved the problem of the apparent empirical inadequacy of the 'classical' treatment of viscous flow. The origin of this model lies in Prandtl's own experiments using a water tunnel in which the thin layer of high viscous stress around a solid body becomes visible. With this conceptual division of the fluid into two regions one can obtain two sets of solvable equations, one for the boundary layer, the other for the rest of the fluid, treated as ideal. Although these solutions are dependent on the structural constraints supplied by the classical equations, and so there is no independence from theory in that sense, the model itself was developed not by simplifying the mathematics of the classical treatment but came directly from the phenomenology of the physics. Again it is suggested that the Semantic Approach is incapable of accommodating such models.

We can simply repeat what we noted above: that whether a model is obtained by deduction from theory or by reflecting on experiment, it can be brought under the wings of the Semantic Approach by representing it in structural terms. There is a general point here: surely no one in their right minds would argue that all model development in the sciences proceeds deductively! Of course experimental considerations may play a role and in particular they may lead to a reconceptualization of the phenomena. The London and London and Prandtl examples are identical in this respect: in both cases there was a crucial experiment-driven reconceptualization of the relevant phenomena. In the London and London case, it was the realization, driven by the discovery of the Meissner Effect, that the phenomenon of superconductivity should be understood, not as a case of infinite conductivity, but as analogous to diamagnetism. In the Prandtl case, the reconceptualization was, of course, the representation of fluid flow in terms of two different regions, again driven by Prandtl's own experiments. Other cases of model development dependent on such reconceptualisations of phenomena may be extracted from the history of science. That such models may then become the focus of scientific activity and thus become 'functionally autonomous' is not surprising given the difficulties in relating them to the appropriate theory. In the London and London example it took another twenty years or so before the development of the BCS theory, although the notion of 'Cooper pairs' was already hinted at in their 1935 paper. Fritz London himself insisted that such autonomy should be regarded as only temporary and indicated via structural similarities with high level (quantum) theory how one might proceed to the essential further stage of showing how the model could be obtained from such theory (French and Ladyman 1997).

A further argument is that models are *representationally* autonomous from theory, again in a way that cannot be captured by the Semantic Approach. Morrison insists that models are explanatory because "they exhibit certain kinds of structural dependencies" (1999, 39). However, models make these structural dependencies evident in a way that abstract theory cannot and hence, again, they act as "autonomous agents" (ibid., 40). In the cases of hydrodynamic and nuclear models, in particular, "they provide the only mechanism that allows us to represent and understand . . . experimental phenomena" (ibid.). Thus models are representational, whereas theories are not.[1] However, two sorts of cases need to be considered in order to evaluate such a claim.

In the first, a theory exists but the relationship with the relevant model is not the standard one. An example might be the models of quantum chemistry which, because of the many-body problem, for one reason, cannot be deduced directly from Schrödinger's equation (Hendry 1998). Nevertheless high-level considerations play a role in constraining the form of the possible models at the lower, 'phenomenological', level and it is difficult to see how they could be straightforwardly ruled out as completely non-representational. Indeed, Morrison herself admits to a spectrum of explanatory and representational power as she acknowledges that models incorporate "more detail" about structural dependencies than high level theory.

Second, a model might be 'autonomous' in the sense that it is just not clear *yet* how it might be related to high level theory. This was the case of the London-London model *with respect to quantum theory* (it was clearly not autonomous from Maxwellian electrodynamics). A further example is Fritz London's own explanation of the behavior of liquid helium in terms of Bose-Einstein statistics, where the extent of the idealizations required to connect the quantum statistics of an ideal gas with a superfluid force the detachability of the model (at least until Feynman came along; see London 1954, 59–60)

In these cases the 'autonomy' is relative and only temporary. Hartmann has explored this idea that such models are examples of 'preliminary physics' (Hartmann 1995, 52; see also Redhead 1980). Of course this preliminary stage may last many years, as in the superconductivity case and, as Hartmann himself acknowledges, this is not sufficient grounds for distinguishing models from theories.

4. The Diachronic Development of Models. In his 1995, Hartmann draws a distinction between what he calls the 'static' and 'diachronic' view of models. The former describes what models are; the latter is concerned with their construction and development. The crux of the above criticisms of the Semantic Approach is that by being wedded to a particular *static* view of models—by tying them deductively to theories—a particular *diachronic* view is forced in which model development can only proceed 'from the top down'. We are arguing here that the Semantic Approach is not so

1. What is needed is further discussion of the way in which models and theories represent. Hughes offers a semantic account, according to which 'global' theories, such as quantum mechanics represent insofar as they define a class of 'local' models—such as Bohr's model of the atom —which denote, and thus represent, types of systems (Hughes 1997). On such a view, both models and theories are representational.

wedded and that the diachronic aspects of actual practice can be accommodated. As McMullin noted, thirty years ago, it is crucial to this diachronic aspect that models contain 'surplus content', which will allow for extensions which, in turn, may be both suggested and yet unexpected (391; cf. Hesse 1963, Braithwaite 1962); that is, the models are heuristically fruitful. Can the Semantic Approach accommodate this suggested unexpectedness (or what Peirce called the 'esperable uberty') and hence respond to McMullin's criticism?

One possibility is to extend it by incorporating partial relations into the set-theoretical structures, where a partial relation defined on a set[2] A can be introduced as follows: if R is binary, then it is taken to be an ordered triple $\langle R_1, R_2, R_3\rangle$, where R_1, R_2, and R_3 are mutually disjoint sets such that $R_1 \cup R_2 \cup R_3 = A^2$; R_1 is the set of ordered pairs which satisfy R, R_2 is the set of ordered pairs which do not satisfy R, and R_3 is the set of ordered pairs for which it is left open whether they satisfy R or not. When R_3 is empty, R constitutes a normal binary relation and can be identified with R_1. In this manner, the openness of scientific developments can be accommodated (Bueno 1997, da Costa and French 1990, French and Ladyman 1997). Room is now opened up for heuristic considerations and there is no obstacle in principle to capturing this further aspect of modeling, as emphasised by McMullin and reprised by Hartmann, using the resources afforded by set theory (French 1997).

5. Iconic Models and Analogies. Let us return to the example of nuclear models, for which Morrison argues that there can be no single high-level theory to carry representation and understanding, because the models are contradictory. Such a case goes right to the heart of our discussion as the possibility of accommodating just these models was one of the reasons explicitly given by Suppes for adopting the Semantic Approach (Suppes 1967). Each such model captures a certain important aspect of the behavior of the nucleus; fission, say, in the case of the liquid drop model, or angular momentum in that of the shell model. The role of analogy in constructing these models was, of course, fundamental. The representation of the nucleus as a charged liquid drop rides on the back of the saturation properties exhibited by nuclear forces. The analogy cannot be pushed too far: if it is to be conceptualized in this way, the nuclear interior is not a classical liquid, of course, but a Fermi-Dirac one (for details see Heyde 1994, 191–192). Here high-level theoretical aspects intrude into the 'semi-empirical model', leading, in the case of quantum statistics, all the way up to group theory. And group theory features prominently in the description

2. It is important to note that the set-theoretic models are constructed in set theories with Urelemente (individual, systems, portions of the universe, real things, . . .).

of the alternative model Morrison discusses, the shell model. Here the analogy is with the atom as a whole, specifically the picture of electrons moving in the atom, and more specifically still, it holds between the ionization energy of electrons and the neutron/proton separation energy. Again the analogy rests on certain crucial idealizations, since nucleons do not move independently in an average field, of course (see Heyde 1990). And again very high level theoretical considerations play an important role in articulating the model. Thus the models may be functionally independent from quantum chromodynamics, this being the result of a lack of appropriate meshing of the various energy regimes, but they are not so independent from all theory. Again, the functional independence may be transitory as physicists explore various ways of reconciling the apparent inconsistencies between these models.

The analogies in these cases only hold in certain respects; in others they definitely do not, and in still others, we simply do not know. As is well known, Hesse captured this tripartite nature of analogy in her division into positive, negative, and neutral analogies (Hesse 1962). The last, in particular, was seen as fundamental by Hesse herself, since it allowed models to be genuinely predictive and thus essential to scientific practice (see Suppe 1977, 99–102).[3] It is the exploration of the space of these neutral analogies which underpins the crucial heuristic role of models (Psillos 1995, 113). How might the openness of this space and the concomitant neutrality of the analogy be represented?

Again, this can be done using partial structures, where Hesse's three distinctions map nicely onto R_1, R_2, and R_3 above respectively. Likewise her distinction between formal and material analogies can be straightforwardly accommodated. With regard to the former, these may be captured by introducing partial isomorphisms (cf. Czarnocka 1995, 31) holding between the partial structures (French and Ladyman forthcoming, Bueno 1997, French and Ladyman 1997). This gives us a formal, set-theoretical and of course, essentially structuralist, grasp on both the openness to further developments of theories and models, and the interrelationships between them. The inconsistency between different nuclear models may then be resolved as scientists explore the open parts of the structures and relate them to further theoretical ones.[4]

Notice the shift that has occurred: Suppe tied the iconic aspect of mod-

3. Psillos has recently revived Hesse's 'analogical' approach (Psillos 1995); for an earlier criticism see Achinstein 1968.

4. Frisch has also pointed out that no (classical) models can be used to represent an inconsistent theory, such as Maxwellian electrodynamics (Frisch preprint); that the issue of inconsistency in science poses no special problems for the Semantic Approach has been argued in da Costa and French forthcoming.

els to high-level theory. Cartwright, Morrison, and others have objected that models exist which are not so tied. The partial structures approach offers a means of representing the latter directly and accommodating the various relationships which hold between models and theories. In this manner we can represent the role of analogies in model construction and also its piecemeal nature which recent accounts have emphasized (the liquid drop model in particular is dealt with in da Costa and French 1990).[5]

Again, there is a sense in which we have been here before. Schaffner pointed out that a range of these 'interlevel, middle range' models might be drawn upon in any one description of a particular system and suggested that fuzzy sets be introduced to accommodate them (Suppe 1989, 275). Suppe has rejected such an extension of the Semantic Approach, arguing that these models may be accommodated via 'laws of quasi-succession', in which only a subset of the basic parameters of the theory are listed, where this subset gives the 'internal substate' of the system (1989, 158). With those parameters outside of this subset characterizing the 'external substate' of the system, a law of quasi-succession characterizes systems for which subsequent internal substates but not external substates are a function of prior complete states of the system (1989, 159). Those situations in which relevant aspects of the states of a system are determined from 'outside' the theory concerned, can then be represented. However, if (some of) the parameters defining the external substate are unknown, the laws of quasi-succession might be represented set-theoretically in terms of partial structures.

6. Conclusion: The Representation of Practice. Perhaps the most fundamental issues we are faced with in the philosophy of science is the representation of scientific practice. As philosophers, sociologists, historians, or whatever, we are faced with this rich, complex practice, or set of practices, which are tied up with theories, models, hypotheses, instruments, etc. The issue then becomes how we are to 'get a handle' on, how we are to represent, these elements in order to better understand this practice. At one extreme we might employ a highly developed formal approach which seeks to represent various distinctions found in scientific practice in highly technical terms. The dangers of such an approach are well known: seduced by the scholastic angels dancing on the formal pinhead, we lose sight of the practice we are trying to understand. At the other extreme we might adopt an Austinian line, beginning with some nuanced taxonomy and describing the various ins and outs, differences, and similarities of practice

5. More generally, it has been argued that certain sorts of models are just not amenable to a set-theoretic treatment (Downes 1992). A response in terms of this framework is given in French and Ladyman 1999.

in ordinary language terms. The dangers here are equally well known: without a clear unifying framework, our account collapses into dry recitation of the 'facts' of practice—a kind of crude positivism at the meta-level. The obvious move is to a point between these extremes, where the desire for some unitary framework is balanced with the need to keep a close eye on scientific practice itself. The analyses of theories and models of the past thirty years have contributed enormously to our understanding of scientific practice. Our intention here has been merely to indicate that a unitary and formal account might still possess the resources to accommodate this richness and complexity.

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[Footnotes]

[1] **Models and Representation**

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