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# HOW TO DO THINGS WITH THEORIES: AN INTERACTIVE VIEW OF LANGUAGE AND MODELS IN SCIENCE*

## 1. Introduction

There are two major approaches to the individuation of scientific theories, that have been called syntactic and semantic. We prefer to call them the linguistic and non-linguistic conceptions. On the linguistic view, also known as the received view, theories are identified with (pieces of) languages. On the non-linguistic view, theories are identified with extra-linguistic structures, known as models. We would like to distinguish between strong and weak formulations of each approach. On the strong version of the linguistic approach, theories are *identified* with certain formal-syntactic calculi, whereas on a weaker reading, theories are merely analyzed as collections of claims or propositions. Correspondingly, the strong semantic approach *identifies*

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theories with families of models, whereas on a weaker reading the semantic conception merely shifts analytical focus, and the burden of representation, from language to models. To exploit a distinction drawn by Patrick Suppes, the strong version of the linguistic approach strives for an "intrinsic characterization" of theories, whereas the strong version of the non-linguistic approach strives for an "extrinsic characterization." An "intrinsic characterization" of a theory characterizes it as the set of the logical consequences of a set of given axioms; and to give an "extrinsic characterization" is "simply to define the intended class of models of the theory" (Suppes 1967, pp. 1-9).

After critically reviewing the two approaches in sections 2 and 3, we move on (in sections 4-6), to advance and defend an *interactive* view of theories. One of our main claims will be that arguments currently available are telling against the strong versions of the two standard approaches, and that their weak versions can happily coexist in our interactive approach.

## 2. Theories as Languages

The so-called "syntactic" view of theories was not purely syntactic for it was consistent with, and made room for, the view that theories are *interpreted* linguistic frameworks. This approach, as developed by Rudolf Carnap (1939) brought together the Duhem-Poincaré view that theories are systems of hypotheses whose ultimate aim is to save the phenomena, and the Hilbert formalization program, according to which theories (mathematical theories, to be sure) should be reconstructed as formal axiomatic systems. The *prima facie* advantage of a Hilbert-style formalization of a scientific theory is that it lays bare logical structure and unambiguously identifies its content: the theory consists of the set of logical consequences of the axioms, or fundamental hypotheses of the theory. But formalization does not preclude questions of interpretation. In fact, a Hilbert-style characterization makes it possible to circumscribe the class of admissible interpretations of the theory: they are just those which satisfy its axioms. As such, it amounts to an implicit definition of its basic predicates. An implicit definition is a kind of indefinite description: it delineates a whole class of classes of entities which can realize the logical structure of the theory, as defined by the axioms.

After Alfred Tarski's work in model theory, the class of admissible interpretations can be identified with the class of models of the theory. Still, no purely linguistic — or syntactic — consideration can single out one class of models as the intended one. The intended interpretation gets singled out by application of the formal system to a certain domain: what, for example, makes a certain formal language a *theory of mechanical phenomena* is that it finds an interpretation in these phenomena. Carnap's case is quite instructive. He took it to be the case that the "calculus of mechanics" — a fully syntactical axiomatic characterization of classical mechanics — could be interpreted via semantical rules so that it becomes a physical theory: one that states "physical laws." But he added:

> [t]he relation of this theory [i.e. the interpreted physical theory of classical mechanics] to the calculus of mechanics is entirely analogous to the relation of physical to mathematical geometry. The customary division into theoretical and experimental physics corresponds roughly to the distinction between calculus and interpreted system (1939, p. 57).

Despite the qualifier 'roughly', it is clear, and obvious from the surrounding text, that Carnap conceived of the interpretation of the calculus (i.e., of the theory) to be made at the point of its application to the physical (and in particular, the observable) world. In light of this, the correct statement of the strong version of the linguistic view should be that theories are identified with formal languages (calculi), whose interpretation — what the calculus is a theory *of* — is fixed at the point of application.

The upshot is that by *identifying* theories with formal languages, the strong version of the linguistic approach divorces the theory from its intended content: what a theory is a theory *of* need not be a feature of the theory, conceived by itself; rather it is tacked onto it at the point of application. This divorce is even more obvious if we take account of two further points. Firstly, although it is clear that for Carnap and other "syntacticists" the interpretation of the calculus is effected by means of semantical rules, the semantical rules were not taken to be part of what individuates a theory. Secondly, Carnap did not take to it be a requirement of having a theory of a domain *X* that this theory be *fully* interpreted. Consider the following quotation:

> To be sure, in order to pass judgement about the applicability of a given physical calculus we have to confront it in some way or other with observation, and for this purpose an interpretation is necessary. But we need no explicit interpretation of the axioms, nor even of any theorems. The empirical examination of a physical theory given in the form of a calculus with rules of interpretation is not made by interpreting and understanding the axioms and then considering whether they are true on the basis of our factual knowledge. (Carnap 1939, pp. 66-67)

Instead, Carnap explained, "[w]e construct derivations in the calculus with premises which are singular sentences describing the results of our observations, and with singular sentences which we can test by observations as conclusions" (1939, p. 67). So, the semantical rules need only apply to the singular sentences of the calculus which purport to refer to observations and predictions. As for the rest of the sentences of the theory, "we need not make their interpretation explicit in order to be able to construct the derivation [of a prediction] and to apply it" (p. 66).

By identifying theories with formal languages, and first-order languages with identity in particular, the strong version of the linguistic approach drastically impoverished them as means of representation. How, for instance, can they reasonably be seen as able to represent the real-number continuum? And as Suppes (1967, pp. 1-11) has rightly stressed, it is often more practical, and even theoretically more plausible, to start with a class of models and then inquire whether there is a set of axioms such that the models in the given class are its models. By concentrating on clean axiomatic presentations of theories, the strong version of the linguistic approach centered on what can at best be refined analyses, rather than complicated and messy scientific theories.

These objections to the strongly linguistic approach to theories are no longer news. But it is important that the baby should not be thrown out with the bath water, for the linguistic approach is right to assume that language is a central means by which theories represent their domain. Problems rather arise from two contingent features of the setting in which that insight was pursued: firstly that formalization was thought to be necessary for the adequate characterization of theories; and secondly that the epistemological doctrine of empiricism had come to be expressed in a peculiarly linguistic form. Although empiricists like Carnap never abandoned the quest for formalization, the demand for *first-order* formalization was relaxed: in mature formulations of the empiricist account of theories-as-languages (see Carnap 1956), the underlying logical apparatus is so strong as to include virtually the whole of set theory. Carnap and others found solace in formalization because it seemed to offer a way to study theories without being committed to any *particular* interpretation of the so-called theoretical terms and predicates, and hence without being committed to any unwanted implications about unobservable entities. Having identified a theory with a formal language, it was thought enough to interpret only part of it — that which is apt for the representation of observable phenomena when the theory is applied to a certain domain — leaving the rest uninterpreted. Faced with the objection that this would concede too much to instrumentalism, they appealed to correspondence rules in order to show how *some* meaning can be given to theoretical discourse, by means of fusion with the interpreted observational terms. Thus began the well-known problems of partial interpretation, the alleged dichotomy between observational and theoretical terms, and the analytic-synthetic distinction, which cannot in any case be maintained given that correspondence rules play a dual role, contributing to the meaning of theoretical terms, but also delineating the empirical content of the theory.[1] More generally, the very idea of correspondence rules raised the question of what relation they bear to theories conceived as formal languages, a problem noted by Fred Suppe (1977, pp. 102-109): are they parts of theories or not? If the former, then modification of the correspondence rules entails modification of the theory, and conditions of theoretical identity become either vague or counterintuitive. If the latter, then theories become free-floaters, devoid of content.

[1] For a statement of Carnap's final views on these matters, see his (2000).

Problems such as the above, which contributed so much to disillusionment with the linguistic approach and to the demise of its strong version, do not follow from the thought that language is a medium by which theories represent. Dissociated from the empiricist predicament and the quest for formalization, the linguistic conception is clearly consistent with the claim that theories are not merely formal-axiomatic calculi looking for (partial) interpretation, but collections of statements. Language here is a means of representing an extra-linguistic domain (a collection of worldly phenomena and their causes), and constituent sentences are interpreted by understanding them literally. Perhaps the logical relationships among these statements can be investigated through formal features of their canonical linguistic formulations, or perhaps not. But this cannot be a condition of adequacy (or good scientific standing) of the representation offered by the theory. We identify this commonsense understanding of the linguistic approach as its "weak version." It is at least arguable that practicing scientists who reflected on the nature of theories, such as Henri Poincaré and Pierre Duhem, understood theories in this way.

This weak version of the linguistic approach seems immune to most of the problems that plagued its strong counterpart. Still, in so far as it focuses *all* attention on linguistic representation, it obscures some fundamental ways in which theories represent the world. Language is certainly a vehicle of representation, but not the only one, and not always the most important one. The weak view, for instance, neglects the role of models in scientific theorizing: it is no accident that proponents of the linguistic view struggled with the thought — or better, the fact — that theories represent by setting up and investigating iconic (or analogical) models of the physical systems they target (see Psillos 1995).[2] It also neglects the fact, stressed rightly and repeatedly by Suppes (1969) and Suppe (1977; 1989), that theories confront not the phenomena themselves but models of the phenomena. Where the linguistic approach goes deeply wrong is in its implication

[2] A case in point here is Duhem (1954 [1906]), whom we identified as one of the proponents of the weak linguistic view. His resistance to models as a means by which theories represent the world is notorious.

that either a domain *X* satisfies theory *T* or it does not. The linguistic approach (in all its guises) cannot easily accommodate more complex representational relations that might hold between a domain and what the theory says about this domain.

This needs to be stressed. Even the weak version of the linguistic approach seems committed to the following naïve view: a good theory, viewed as a collection of statements, directly represents the world in that the world (or a certain domain) directly satisfies the theory (i.e. makes it true, or empirically adequate or what have you). The naïveté of this view is apparent when one thinks of the idealizations, approximations, simplifications and *ceteris paribus* clauses that are so typical of scientific theorizing.[3] Now, this is a naïveté that Leszek Nowak (2000) has done probably more than anyone else to highlight. His detailed study of the nature of idealization shows emphatically that, in the first instance, theories represent ideal systems which are constructed from real systems by eliminating factors that are thought to be secondary (see 2000, p. 117). According to Nowak's insightful idea, a theory of a certain domain consists, in fact, of a *series* of theories, each being less abstract (or more concrete) than the other. The starting element of the series is an abstract version of the theory which applies to the idealized description of the phenomena under study, while its terminal element is a realistic (because de-idealized) description of the relevant phenomena — a description which has taken into account all, or most, of the secondary factors that the abstract version of the theory had neglected but which, nonetheless, influence the phenomena under study. Though this outline of Nowak's view of idealization is very sketchy, we think it brings to light two important thoughts. Firstly, the way theories represent is much more complex than the linguistic approach has assumed. Secondly, idealization does not detract from representation. This last thought is strengthened by two further considerations: an idealized theory admits of concretizations, which enhance its representational capacity, and

[3] Here Duhem (1954 [1906]) got it right. For though he worked within the weak linguistic approach, he did try to accommodate idealizations, approximations and the like.

even without the concretizations, there is still a sense in which an idealized theory *does* represent the phenomena it studies. For, as Nowak (2000, pp. 117-118) puts it, a theory studies the behavior of real entities or magnitudes, even though it offers an idealized description of them.

## 3. Theories as Families of Models

The single major advantage of the alternative non-linguistic approach is that it naturally accommodates all these more complex representational relations between the theory and the physical world. But, in so far as it strives for an "extrinsic characterization" of theories, it does so at the cost of neglecting the role of language in representation. On a strong reading of the non-linguistic approach, theories are identified with families of models, where the term 'model' is to be understood in the logician's sense: a structure that makes some statement true. Thus Suppes has it that "the meaning of the concept of model is the same in mathematics and the empirical science" (1967, pp. 2-6), and Suppe has urged that "theories be construed as propounded abstract *structures* serving as models for sets of interpreted sentences that constitute the linguistic formulations," where these structures are "*metamathematical models* of their linguistic formulations" (1989, p. 82).[4] This identification of theories with extra-linguistic entities is supposedly suggested by a distinction between a theory and its formulations, a distinction which is the backbone of the non-linguistic approach. In its turn, that distinction is motivated by the "multiple formulations argument" (see Suppe 1977, pp. 204-205; 1989, p. 82). Suppose, the argument goes, that a theory formulated first in English is translated into French. If we would deny that a *new* theory had been offered in the French, we must identify the

[4] For some relevant thoughts, see also Lloyd (1988, p. 15) and van Fraassen (1989, p. 366, n. 4), who understand 'models' as mathematical structures, but distinguish the latter from the logician's model-as-an-interpretation of a set of statements, which includes a mapping from the terms of the language in which the statements are made.

theory with something extra-linguistic that has been formulated in two languages. As more telling examples, Suppe cites the equivalent formulations of the quantum theory offered by matrix and wave mechanics (1977, p. 205) and of classical particle mechanics by its Lagrangian and Hamiltonian formulations (1989, p. 82). In all cases, Suppe concludes, since we must distinguish between a theory and its formulations, it follows that a theory should be identified with something extra-linguistic, that is with something which can admit of different linguistic formulations. And what else could this plausibly be, if not an abstract mathematical structure?

Let us suppose that this argument does establish the distinction between theories and their formulations: even then the intended conclusion, that theories should be *identified* with abstract structures, does not follow. In case we think that sets of statements in two or more different languages constitute formulations of the same theory, the theory should not be identified with one particular set of statements, but rather with all those linguistic formulations which are theoretically equivalent. An analogy with the problem of meanings in the philosophy of language is irresistible. How shall we account for semantic relations between 'snow is white', 'la neige est blanche' and 'der Schnee ist weiß'? We need not invoke an *abstract* extra-linguistic entity, but can merely say that there is something that can be said equivalently in the languages of the different formulations. If we do invoke something extra-linguistic, we appeal to identity of truth-conditions. Now matrix and wave mechanics are a case in point: here we have historically independent but "equivalent" formulations. But are they formulations of the same theory? On our view of the individuation of theories (see below), this is an open question. An intimate mathematical relationship between the two theories was proved by Schrödinger: that a (semantic) model of one could be turned into a model of the other. But mathematics aside, it is hard to imagine two theories that were further apart in what they had to say about the nature of the physical world, in their "fleshly clothing" (1928 [1926], p. 59) as Schrödinger himself once put it. If historical hindsight has deemed the two theories to be one, this may be as much a product of their joint mathematical subsumption under the later Hilbert-space formalism as it is a sign of equivalence in any sense wider than the mathematical.[5]

Both Bas van Fraassen (1995/1996, pp. 5-6) and Ronald Giere (1988, p. 84) have employed the multiple formulations argument. However, van Fraassen's (1989, p. 222) endorsement of the argument's conclusion seems to be conditional: "[I]f the theory as such is to be identified with anything at all — if theories are to be reified — then a theory should be identified with its class of models." If the proviso indicates a worry about whether theories have the well-defined identity conditions that identifying them with sets of models would entail, the point is well taken.[6] But van Fraassen then goes on to claim that "the semantic view of theories makes language largely irrelevant to the subject [of theory structure]" (1989, 222). Of course, language cannot totally be neglected because "to present a theory, we must present it in and by language," since "any effective communication proceeds by language" (1989, p. 222). But as we shall see in section 5, van Fraassen thinks that in the "discussion of the structure of theories it [i.e. language] can largely be ignored" (1989, p. 222).

Two points are worth making here. Firstly, language is an ineliminable element in theoretical representation, and not just in the banal sense allowed by van Fraassen. Any theory of theories should include language in the account of how theories represent. These central thoughts will be taken up in sections 5 and 6. Secondly, to consider sets of models in isolation from language, or some other means of making a representational claim, as van Fraassen's point seems to imply, is to render them unsuitable for representing, and the theories of which they are part devoid of empirical content. Let us see why this is so.

[5] Indeed Müller (1997) argues that joint subsumption was achieved at the cost of "chopping" much "excess" mathematical structure from the two subsumed theories. Hendry (forthcoming) argues that Schrödinger himself thought that mathematical equivalence falls short of equivalence simpliciter.

[6] See also Giere (2000), p. 524. More recently, van Fraassen (1995/1996, p. 6) has made the unconditional statement that "a theory can be identified *through* its class of models." Although a theory is not identical with its class of models, the class of models would be sufficient to identify the theory.

If theories are identified with, or even through, families of models, then the non-linguistic approach is committed to some distinction between theories themselves, and the claims that they can be used to make when applied to real-worldly systems. Classical mechanics, according to Giere's elegant analysis (1988, Chapter 3) consists of hierarchically arranged clusters of models, picked out and ordered by Newton's laws of motion plus the various force-functions. On Giere's view (also endorsed by van Fraassen 1989, p. 222), the relationship between a law-statement and a model is a definitional one: a model is an abstract entity that satisfies the definition. But neither the definition itself nor the resulting model tells us which physical systems, if any, the model represents, or how. Construed as Giere suggests, classical mechanics makes *no* claims about physical systems. It only identifies a cluster of models: abstract mathematical entities that may or may not have physical counterparts. Giere reinstates the empirical content by means of what he calls "theoretical hypotheses," linguistic items expressing representational relationships, in specified respects and to specified degrees, between abstract structures and given classes of real systems. Now this seems to imply that a detailed theoretical treatment of the processes that underlie some domain of phenomena must involve two linguistic components: the definition and the hypothesis. But Giere tries to resist this implication, as it would put "too much emphasis on matters linguistic" (1988, p. 85). Hence he substitutes "the models [i.e. the abstract structures] for the definitions" (p. 85). But this leads him to counter-intuitive results, in two ways. The first way is highlighted by Giere's following dictum: "Thus, what one finds in the textbooks is not literally the theory itself, but statements defining the models that are part of the theory" (p. 85). To say that we find only theory-formulations in textbooks seems to us a strange category mistake: what we find in textbooks are statements that are being used to present the theory. The theory itself is inseparable from the statements that in any particular instance express it, and if it is not to be found where they are, we do not know where else to find it. The second way is highlighted by the following consideration. To define the class of models is not yet to say anything about the world: that requires something linguistic, a theoretical hypothesis.[7] Although Giere insists that we have models "occupying center stage" (1988, 79), in the analysis of theories considered in isolation from their applications, he has to admit that a linguistic element is indispensable if models are to do any representational work.

Note that the appearance of this distinction between 'theoretical definition' and 'theoretical hypothesis' is historically ironic: although the correspondence rules of the received view came in for much (justified) criticism from the founders of the semantic view, here we find theoretical hypotheses playing a parallel role of tying free-floating structures to the empirical world, albeit in the context of a more sophisticated and diverse account of theory-world relations. Giere resists this comparison, stressing that correspondence rules linked "terms with things or terms with other terms" (1988, p. 86). But this does not discredit the proposed parallel between theoretical hypotheses and correspondence rules. Not only must we interpret the elements of the abstract mathematical model so that they are apt for representing physical content (solving what Giere calls the "interpretation problem", see his 1988, p. 75), we must also treat theoretical hypotheses as bridge principles which give the theory whatever empirical content it has (solving Giere's "identification problem").

Our suggested parallel between correspondence rules and theoretical hypotheses might seem a bit too quick. Criticizing the appeal to correspondence rules made by the linguistic approach, Suppe (1989, pp. 69-72) suggests that a major advantage of the semantic view is its replacement of correspondence rules with a more sophisticated (and non-linguistic) characterization of how theories relate to phenomena. Correspondence rules, Suppe argues, were ill-motivated because they aimed to "eliminate the physical system," purporting to

[7] 'Linguistic' is used here in the sense intended in our formulation of the weak linguistic view of theories, to cover items that are of no *one* language, but are constitutively connected to language. Thus it includes, for instance, propositions and statements.

link the postulates of the theory directly with observation reports. According to Suppe (1989, p. 67), a physical system is an abstract or idealized replica of the phenomena, described in the vocabulary of the theory. The role attributed to correspondence rules is replaced by a two-stage process. The first stage converts the "raw phenomena" (1989, p. 69) into "hard data" describing the behavior of a physical system, which involves correcting them and expressing them in the vocabulary of the theory. The second stage connects the "hard data" to the postulates of the theory. Suppe (1989, pp. 69, 71) notes that the second stage of the process, being essentially mathematical, is part of the theory, while the first stage is not. Instead, it constitutes the application of the theory to the phenomena and is therefore experimental or empirical. So for Suppe, the connection between theory and phenomena is not linguistic, as it is when made through correspondence rules. To use Giere's terminology, the theoretical hypotheses connecting the models to physical systems are unlike correspondence rules in that (i) they are part of the theory; and (ii) they do not directly link the theory with observational reports.

Although we do not agree with Suppe that the relationship between the systems studied by theories and the phenomena to which these systems somehow apply is one of replication (instead, as it will be shown in section 6, we take some notion of abstraction to be operative[8]), we do think that his main insight, that theories represent natural phenomena *indirectly*, is essentially correct. It is also true that, by focusing exclusively on linguistic representation, the linguistic approach obscured this fundamental aspect of representation. Yet it still does not follow from this that theoretical hypotheses are not, on Suppe's account, the modern-day analogue of correspondence rules. The latter gave empirical content to the abstract linguistic calculus of the theory, and empirical content — at least on the empiricist account of those rules — was "cashed out" in terms of observational reports. Given Suppe's insight that empirical content accrues through the theory's descriptions of physical systems, it is easy to see that this

[8] For more on the notion of "abstraction," see Nowak and Nowakowa (2000, pp. 116*ff*).

reliance on observational reports is unnecessary and ill-motivated. But even on Suppe's account, theories *qua* families of abstract entities are free-floaters, unless these abstract entities are suitably connected with concrete physical systems and phenomena. This is precisely what theoretical hypotheses do: anchor models to the world, by showing how their descriptions are relevantly true of empirical systems. In this sense, theoretical hypotheses do play the same role in the strong non-linguistic view as did the correspondence rules in the strong linguistic view.

So, unless appeal is made to theoretical hypotheses — which are essentially linguistic devices — the strong semantic view divorces the theory from its empirical content no less than the rival linguistic conception. Such a divorce would be mistaken, in our view, for two kinds of reason. Firstly, it is a curious use of the term 'theory' that allows a particular theory to be individuated independently of what it is a theory *of*. The models that are associated with a particular set of equations must naturally play an important role in delineating the content of theories that use them, but they cannot provide the entire story. Part of what individuates a theory, and determines its empirical content, is surely its intended domain of application. It was essential to Bohr's 1913 theory, for instance, that it was a theory of the structure of *atoms*; if its subject-matter had been different, it would have been a different theory. Secondly, even if models do play an essential role in the representation of a theory's content, it is a category mistake to infer that the models themselves constitute that content. Models are central to the theory of theories just because they are a means of theoretical representation: theories represent via models. In saying *that* (some part of) the world is some particular way, a theory may *ipso facto* invoke some representational relation between a model and part of the world. But in so far as the theory embodies a representational relationship between model and world, it must reach out beyond the model to the world itself. The content of a physical theory is what it has to say about real-worldly physical systems. We use equations to say these things, and the central insight of the semantic view is precisely in identifying models as the means by which equations convey their message. But the models themselves are not the message.

The master argument for the semantic conception is that a focus on models allows a more perspicuous analysis of theories and theorizing than the linguistic view, one that is closer to the structure of theoretical texts, the practice of theorizing, and scientists' usage of such central terms as 'theory' and 'model'. As noted in the beginning of this section, it is to the credit of the semantic conception that it has indeed allowed more sophisticated accounts of relations between theory and data (Suppes 1969; van Fraassen 1980), approximation and idealization (Suppe 1989; French and Ladyman 1998), and illuminating analyses of particular theories (e.g. Lloyd 1988 on evolutionary theories; Giere 1988 on classical mechanics; van Fraassen 1991 on quantum mechanics). But these points speak only in favor of a weak version of the semantic view: that a focus on models must play an important part in any viable analysis of theories. As such, the weak version is consistent with the interactive conception which we will articulate and defend in the next three sections, according to which the central representational medium of the non-linguistic conception (models) must find a place alongside the central medium of the linguistic approach (language) in any viable account of how theories represent.

## 4. Varieties of Theoretical Representation

In its theories, science provides representations of the world. Some of these are successful, others less so. That much is truism, for it is consistent with any mainstream philosophical view of science and scientific theories. Defenders of both linguistic and non-linguistic approaches to theories can agree on it, for both accept that theories can be used to explain and predict, and as such must represent parts of the world as being certain ways. Now mathematics is a central representational medium, at least for the physical sciences. In this section we will compare it, as a representational medium, to some others.

Historians of science have documented the many roles that non-linguistic modes of representation (diagrams, concrete physical models) have played in theoretical representation, and how they have been central to the development of physics, chemistry and biology.

Nineteenth-century structural chemistry, for instance, coalesced around particular schemes of diagrammatic representation and ball-and-stick renderings of particular molecular structures.[9] A few general points of relevance to our discussion emerge: diagrams and models are governed in use by evolving traditions of visual representation, by the practical limitations of the media for reproducing them, and by the broader aims and beliefs of those who use them. In short, they are creatures of the theory, technology and society of their time. In use, they appear in conjunction with text or speech. A written or spoken argument gives the diagram a representational role: to illustrate, motivate and support things that are said in the text.[10] The partnership of text, tradition and image (or object) allows the effect achieved by a particular representation to transcend the inherent limitations of the medium of which it is an instance: suitably explained, or within a particular tradition of use, two-dimensional diagrams can, for instance, allow claims to be made about three-dimensional structures. It matters not whether the relevant traditions are thought of as (explicit or implicit) conventions, or as more diffuse constraints on right interpretation, issuing in a Tarski-style truth-theory for a language in use.

If pictorial representation seems too peripheral, turn now to the iconic (or analogical) models of Hesse (1966), which directly involve the representational use of mathematics. One physical system (the Source) is a model of another (the Target) in virtue of positive

[9] Knight (1996) traces the development of the "visual language" of nineteenth-century chemistry, considering portraits and illustrations in addition to theoretical diagrams. Francoeur (1997) gives a vivid account of the interplay of theoretical and practical constraints on the development of molecular modeling systems in the twentieth century. Knight (1985) argues that even in natural history texts, illustrations need to be read in conjunction with a rich cultural context that includes the accompanying text. See Hendry 2001, Sections 2 and 3 for an overview.

[10] This is not to say that proper understanding of a diagram requires no work on the part of the reader or hearer. To the extent that diagrams or models can be used to say anything, or represent (part of) the world as being a certain way, their interpretation is constrained (see also van Fraassen and Sigman 1993, pp. 93*ff*).

analogies between them. Given the positive analogies, fragments of mathematics that have been used to describe the behavior of the Source are transferred to the Target, along with (ideas about) causal structure, and attendant auxiliary assumptions in mathematical form (see also Hughes 1993). Thus did Bohr redescribe the hydrogen atom as a solar-system (albeit an atypical one), and the hydrogen atom inherit the solar-system's mathematics (see Hendry 1999); the elastic-solid models of the luminiferous ether provide another case in point (see Psillos 1995). The benefits of this kind of analogical transfer can be both pragmatic and heuristic: equations with well-known solutions are used, and to the extent that the analogy is helpful, the user benefits from seeing the complex behavior of the Target in terms of the readily-interpreted mathematics of the Source. Bohr, for instance, used the well understood equations of central-force mechanics, and was thereby able to separate electronic motions into well-understood components, opening up a detailed program of theoretical development starting with single electrons describing circular orbits, followed by precessing elliptical orbits, many-electron atoms, and perturbing fields. Along the way, he was able to account for unforeseen empirical anomalies (in the exchange with Fowler, see Lakatos 1970, pp. 140-154) in terms of some natural de-idealizations of his initial model: natural, that is, in the context of the analogical connection. In so far as Bohr used mathematics to represent, he relied on its prior uses. The upshot is that Bohr succeeded in saying some things, primarily about hydrogen atoms, and he said them using some text and some equations.

'precessing' — is this correct?

Now the semantic view has an obvious way of accommodating all this: in so far as they determinately represent the world as being some way, diagram and text serve to pick out a model, or structure. In a cleaned-up analysis of a historical theory, this structure could also be picked out — using the language of mathematics, following Suppes' and van Fraassen's injunction — by a set-theoretic predicate. But in our view this could be misleading, for three reasons. Firstly it privileges mathematics, which is but one representational medium among many, and one whose central role in modern science is, after all, contingent. In any case, in some of the foregoing examples (the nineteenth-century molecular models, for instance), mathematics was not obviously involved. Secondly, in so far as "structures" are understood as abstract *objects*, diagrams and equations do not pick out structures; rather they pick out *objects with structures* (where a structure should now be thought of as a mode: a way for a concrete object to be). Many of the nineteenth-century diagrams and models — those employed by the realist-minded Kekulé and van t'Hoff, for instance[11] — were intended to pick out three-dimensional structures, possible occupants of three-dimensional physical space, not mathematical structures, which are "three-dimensional" in a metaphorical sense at best (for our positive approach to this issue, see section 6). Thirdly, in so far as equations are items of mathematical language, they are as bound by tradition, and as much in need of interpretation, as other media. Where mathematics is used within object-level science, rather than its metalinguistic description, the trivially set-theoretic move from equation to structure obscures the need for interpretation.

Our contention is not that Hessean analogy cannot be accommodated within the semantic view: it has been, and elegantly so, within the "partial structures" approach of da Costa and French (see da Costa and French 1990; French and Ladyman 1999).[12] Rather, we claim that in concentrating on shared (set-theoretical) structure, the temptation is to reify structure, viewing the analogy as a relationship that Source and Target each bear to some third object. The analogical connection is the *end* of a long process, where theoretical assumptions, analogies and idealizations are orchestrated to lay bare a level of description of the two systems (Source and Target) in which they share structure.

[11] For an account of the interplay between visual representations of molecular structure and the mathematics of quantum mechanics in the formation of quantum chemistry, see Hendry (2001).

[12] Discussing Hesse's critique of correspondence rules in the received view, Suppe (1977, pp. 95-102) questions whether Hesse establishes the indispensability of iconic models. This seems to us to get the burden of proof the wrong way round. Hesse and others give examples of iconic models at work: it is for the semantic view to show that every iconic model can be reduced to a semantic model, in a way that fully explains, rather than re-describes, the heuristic power of the iconic model.

## 5. The Limits of Structural Representation

The lesson we would draw from the Bohr atom is that Bohr's mathematical equations, just like the earlier visual media, represent what they do only given their historical context. In this section we seek to generalize this conclusion and assimilate it into our understanding of how models represent. Considering representation under its most general description, as an intentional relation, we argue that paradox and error arise when models, conceived as abstract mathematical structures, are considered outside of the contexts in which they are capable of realizing their role in science, representation.

Representation has two elements: success and denotation. To the extent that it can be evaluated in terms of faithfulness or unfaithfulness, representation should at least involve either some comparative criterion according to which (in stated respects) some representations are better than others, or perhaps some absolute criterion that only good representations meet. Let us call this the criterion of success: it is, we think, irresistible to take it to play a role like that of truth in semantics. Now, proponents of the semantic view have proposed isomorphism (Suppe), embeddability (van Fraassen), similarity (Giere) and partial truth (da Costa and French) as criteria of success. For van Fraassen, to claim empirical adequacy for a theory is to claim that a model of the appearances can be embedded in a model of the theory. Giere measures success in terms of similarity (in specified respects, and to specified degrees) between a real-worldly system and some specified model. But success can only be one dimension of representation, the other being denotation. Besides, representational success cannot be reduced to a purely structural relation.

Long ago, Goodman (1968, Chapter 1) noted that to identify representation with resemblance is to trivialize it, for there are too many resemblances (that is, instances of the two-place relationship *x* resembles *y*).[13] Firstly, any *thing* resembles any other *thing* in some respect and to some degree. So it must already be understood which elements (and which of their features) are represented, and how: this is denotation. Secondly, resemblance is reflexive and symmetric, while representation is irreflexive and anti-symmetric: resemblance fails to capture the intentional element in representation. When *A* resembles *B*, it follows only that *A might be used* to represent *B* (and indeed vice versa), not that *A* represents *B*. Denotation is part of representation, and representation can admit of non-trivial success or failure only given prior relations of denotation. Of course Goodman went further, famously arguing that resemblance is not necessary for representation either. This sounds right when it comes to linguistic representation. But in general, denotation without some implied measure of representational success is empty. Perhaps any *thing* can be used to stand for any other *thing* regardless of their resemblance: thus can a pepper-pot represent the Duke of Wellington's forces in a table-top re-enactment of the Battle of Waterloo. But "standing for" in this sense succeeds by stipulation: it cannot admit of failure. To the extent that representation in science can succeed or fail, it cannot be so established by mere denotation. When mathematics is used to represent some part of the world, the representation is partly a matter of denotation (that is, more-or-less implicit stipulation) and partly some other relation whose obtaining is a matter of empirical investigation. So there is more to representation than success. In fact, it makes no sense to speak of representational success unless denotational relations are already in place. To use the oft-paraphrased Kantian precept, representational success without denotation is blind, whereas denotation without representational success is empty.

Goodman's points about resemblance generalize to the structural notions of success favored by defenders of the semantic view. Representation cannot reduce to isomorphism, simply because there are too many isomorphisms. A particular relation-instance of isomorphism

[13] Hughes' (1997) DDI account of theoretical representation invokes Goodman's insight, and Suárez (1999) also argues for an intentional element to scientific representation.

can be representational only in the context of a scheme of use that fixes what is to be related to what, and how. We think that these points have some grave implications for the structuralism[14] that is sometimes associated with the semantic view of theories. For instance, van Fraassen (1997, p. 522) argues that since scientific representation comes down to isomorphism, and isomorphism preserves just structure, the semantic view is committed to the thought that "science's description of its subject-matter is solely of structure." Why isomorphism? Van Fraassen offers the following argument: models are mathematical objects, that is, not relevantly differentiated beyond isomorphism: *qua* mathematical entities, models possess only structure. So when we use a model to represent some part of the world, the resultant description of the world cannot " 'cut through' structure" (1997, p. 522).

But given what we said above, there are just too many isomorphisms, and all of them are equally good representations, if representation cannot "cut through" isomorphism. In fact there are two problems here. Firstly, *qua* structure, there is nothing to distinguish a data-model of the simple periodic motion of a pendulum from that of a suitably-described economic cycle. Secondly, even when the subject of the model is fixed, we can *define* a relational structure on its subject domain, cardinality permitting, in such a way as to guarantee isomorphism. If there is nothing more to empirical adequacy than isomorphism, we need pursue no empirical investigation to assure ourselves of the empirical adequacy of our theory (again, cardinality permitting). Now van Fraassen is well aware of these objections.[15] He notes that "if one structure can represent the phenomena, then so can any isomorphic structure, mutatis mutandis"

[14] Structuralism is the view that science provides only structural (mathematical, or set-theoretic) information about processes in nature. One version of structuralism has it that it is possible to know only the truth of a theory's Ramsey sentence, not the truth of the theory itself. For more on this, see Psillos (2001).

[15] The second problem was first raised, in its essence, against Russell's (1927) structuralism by M. H. A. Newman (1928); see Demopoulos and Friedman (1989, p. 189) and Psillos (1999, chapter 3).

(1997, p. 522). The Latin clause is crucial. It seems to give, in two words, what van Fraassen misses: that other, non-structural, considerations single out one among the many isomorphisms as that which is intended. Van Fraassen concedes that the structural relationship which on his version of the semantic view is constitutive of empirical adequacy is representational only in the particular context of use: ". . . in science models are used to represent nature, used by us, and of the many possible ways to use them, the actual way matters and fixes the relevant relation between model and nature — relevant, that is, to the evaluation as well as application of that theory" (1997, p. 523). Moreover, that context is conditioned by history and theory. This concession arose in the context of an acknowledgement qualifying (but not, he insists, weakening) the structuralist commitment of the non-linguistic view.

But in view of this concession, does van Fraassen's structuralist conclusion still follow from his premise, the semantic view? That conclusion depends on two erroneous assumptions about scientific uses of mathematics: firstly, that mathematical descriptions provide only structural information about the objects they pick out (because these objects are "structures" in the technical sense); secondly, that descriptions of families of models are *all there is* to scientific theories (this is just the strong non-linguistic view of theories). But these assumptions have already lapsed in van Fraassen's attempt to come to terms with the fact that isomorphism is too weak a representational relation. It is instructive to see why.

To overcome the ubiquity of isomorphism, van Fraassen appeals to pragmatics, and the specific denotative relations that are part of a language in use, admitting that structures represent non-trivially only in contexts that are partly determined by these relations. If invocation of the denotative context is to dissolve the "too-many-isomorphisms" problem, context must be sufficient to determine that the subject-matter of the equations appearing in a *physics* text are, *physical* systems (e.g., gas molecules), rather than, say, populations of bacteria.[16]

[16] It may well be that van Fraassen's appeal to language-in-use is meant to apply only to *data models*, so as to differentiate data models of bacteria

So the objects picked out by the equations of a physical theory are physical systems, not abstract structures. To be sure, if the equations are understood as they would be in a mathematics text, we might concede for the sake of argument that they pick out classes of objects only *sub specie formae*. But if the context is that of a physics paper, the equations are accompanied and motivated by text (written, perhaps, in physics-Greek, physics-English, or physics-French), which is as much part of (the particular presentation of) the theory as are the equations themselves. The point is not merely that anything that can be said is said in a language, although that is true enough. Rather it is that the equations do not pick *anything* out non-trivially except in a richer context, and in the richer context under consideration here, the equations are being used — *in conjunction with text* — to make claims about the possible physical states of certain kinds of physical system. Hence structuralism lapses, for it is premised on a mistaken view of theoretical representation: even if the discipline of mathematics is interested in structure for its own sake, science is an activity that is directed outward to the world, and hence *uses* mathematical structure to represent things. We should not be tempted to reify structure as something attributed in our descriptions: to consider a thing under an abstract (or structural) description is neither to think of it as an abstract object, nor to think of it as something that bears a structural relation to one. Embedded in theories, mathematical equations can be used to make sophisticated and abstract claims about real physical systems; the representational cash value of mathematics, within science, must lie in the truth-conditions of the claims it can be used to make about them. There is no more reason to think that it can be used to convey only structural information than there is to think that two-dimensional images can be used to convey only information about two-dimensional objects.

populations from those of radioactive decay. In that case much of what follows does not apply. But then the subject-matter of the theory is fixed only at the point of contact with the data model, a deeply counterintuitive consequence that we considered in Section 3. In any case, from the point of view of semantics van Fraassen has previously treated the observational and the theoretical symmetrically.

It may be objected here that, as noted above, van Fraassen's appeal to the language in which the structural claim is made is driven by pragmatic rather than metaphysical considerations (see van Fraassen 1997, p. 525). But even if it were to be conceded that no metaphysical implications flow from the use of a language to describe the structural claims made by theories, it would still *not* save the argument for structuralism. Mixing structuralism with the thought that pragmatics determines choice of one structure among many yields *no less* explosive a brew than mixing it with the thought that one structure *described in a preferred language* offers the correct description of the world. In either case, pure structuralism must go, even though different metaphysical pictures are left in its wake.[17]

## 6. The Interactive Approach

It is time now to turn to our positive view of how the weak versions of the two grand approaches to theories can co-habit in what we call an "interactive approach." Parts of our positive view have already emerged in previous sections. Our starting point is a well-known quote from Heinrich Hertz:

> To the question "What is Maxwell's theory?" I know of no shorter or more definite answer than the following: Maxwell's Theory is Maxwell's system of equations. Every theory which leads to the same system of equations, and therefore comprises the same possible phenomena, I would consider as being a form or special case of Maxwell's theory; every theory which leads to different equations, and therefore to different possible phenomena is a different theory. (1893, p. 21)

How far off the mark, if at all, was this claim? Leaving aside issues of historical interpretation, Hertz was concerned with the problem of individuating theories.[18] Maxwell's equations were important because,

[17] This issue is discussed in more detail in Demopoulos and Friedman (1989), and Psillos (1999, chapter 3).

[18] A careful reader might object that there is some unclarity in the Hertz quote about whether the notion of equation includes a physical interpretation of its terms. Could it not be the case that some non-electromagnetic domain could in principle be said to satisfy Maxwell's equations? Is it clear that Hertz would

by systematizing the most fundamental laws of behavior of the electromagnetic field, they put an order to what Hermann von Helmholtz (see Hertz 1955 [1894], p. 4) had called "the pathless wilderness" of the domain of electromagnetism.

Hertz's claim did not really miss the mark, because mathematical equations, which typically express laws of behavior, lie at the heart of any typical scientific theory, at least in mathematical physics. They are the core means by which a theory expresses its content and represents its domain. Now mathematical equations present a case in which two representational media work closely together: language and models of physical systems. Mathematical equations are bits of (a formal) language. (Giere is in agreement here: see his 1988, p. 86). They are written down on paper. They are translated into different, but equivalent, notation. They are interpretable, and are indeed interpreted and re-interpreted. Clearly an equation itself is not an extra-linguistic entity. It is a statement. It does say something about one or more extra-linguistic entities, but it is not one of them. So here we have linguistic representation at work at the heart of theory. Mathematical equations are surely part of what makes a theory what it is, and hence linguistic representational media are also part of makes a theory what it is.

How, and exactly what, do mathematical equations represent? They describe the behavior of, and inter-connections among, physical magnitudes. These magnitudes, such as the strengths of electric and magnetic fields, are represented by mathematical entities, such as vectors. But we should not lose sight of the fact that it is physical magnitudes and not mathematical entities of any sort (e.g. abstract

not allow this? If Hertz allowed for this, then we would like to make clear that his position is modified: it is equations *interpreted in the language of the theory* that we are interested in. But it is not hard to see that for Hertz too the possible interpretations of Maxwell's equations are doubly constrained. They are constrained from below: as he explicitly said in the quoted passage, the interpretation of Maxwell's equations should be able to account for the same "possible phenomena." But they are also constrained from above. Hertz's embarkation on the problems of the foundations of mechanics (1955 [1894]) was motivated by his attempt to unify the mechanical picture of the world with the then emerging electromagnetic picture.

mathematical structures) that are being described. Take, for instance, the linear harmonic oscillator. This is a system which has a certain lawlike behavior described by a mathematical equation. This system is *physical* (and not abstract-mathematical) precisely because it has physical properties (described in the language of physics), and if it exists, it is supposed to participate in causal interactions and processes. But a linear harmonic oscillator is an *idealized* physical system. It is *idealized* because some of the physical relationships holding within actual physical entities that can be modeled as linear harmonic oscillators (e.g. pendula and springs) have been eliminated, and because some of the parameters that influence (and determine) the behavior of such actual entities (e.g. pendula and springs) have been abstracted away.[19] Exactly for this reason, a linear harmonic oscillator can be seen as an unactualized physical system.[20] That is, strictly speaking, and as a matter of contingent fact about the world, it is uninstatiated. But this is if we talk strictly. For we have every reason to believe that a linear harmonic oscillator is *inexactly instantiated* in actual physical entities such as pendula and springs. Differently put, such physical systems are inexact counterparts of a linear harmonic oscillator. It is this fact of inexact instantiation that makes mathematical descriptions of linear harmonic oscillators so useful in describing and explaining the behavior of actual physical systems. From now on, we shall employ the expression 'unactualized physical system' (UPS) to refer to the systems studied by physical theories. This expression is meant to emphasize that the systems studied by physical theories are *not* the abstract mathematical entities of the semantic approach. If the entities described by physical theories were the abstract mathematical entities of the semantic approach, then in order to make them stand in any meaningful representational relation to actual physical systems (their actual counterparts), we would not only have to introduce theoretical hypotheses, but also interpret them physically. It follows

[19] For more on the relationship between idealization and abstraction, see Nowak and Nowakowa (2000, pp. 116-117).

[20] Suppe (1989, p. 85) also relates idealized models to counterfactual situations.

from our argument in Section 5 that a linear harmonic oscillator *qua* mathematical entity could not meaningfully represent pendula and springs. (As a reminder: the relation of isomorphism is not enough for representation.) *Qua* unactualized physical system, a linear harmonic oscillator can be said to have actual, but inexact, counterparts, which explains why it can be used to represent their behavior.

Our suggestion might create some unease. What, one might wonder, do theories represent? We think that this question admits of a canonical answer only if we take account of the contingent fact that the behavior of actual physical systems like springs, pendula and planetary systems is too complex to be studied directly, exactly and fully by physical theories we can devise. It is precisely because of this fact that there is need for "middle-men," that is, for representational devices that are themselves the objects of direct, exact and full treatment, which can then be taken to represent indirectly, inexactly and partially (all these depending on the particular case) actual physical systems. So, the answer we offer to the foregoing question is the following. Theories study, via mathematical equations, the behavior of UPS (the middle-men), which, nonetheless, represent actual physical systems. Hence equations represent actual systems, if only indirectly, inexactly and partially. So, ultimately, the content of theories is the behavior of this-worldly physical systems, in particular those systems which (suitably prepared, perhaps in laboratories) closely resemble the UPS posited to represent them. Theories posit and study UPS in order to capture (and represent) the behavior of actual physical systems. UPS are suitable for theoretical investigation. And in so far as they have (inexact) counterparts, they guarantee that theories have empirical content.

It is worth emphasizing two points. We should distinguish between the (clusters of) physical properties ascribed to unactualized physical systems in physical equations from the unactualized physical systems themselves. Only the latter can stand in relations of similarity to actual physical systems. Secondly there is the role of theoretical hypotheses. Where do we stand on this issue? We do admit that theoretical hypotheses sometimes have a role, as, for instance where exemplars are associated with a theory like quantum mechanics. In this case, there may be competing quantum-mechanical theories employing different exemplars to represent some class of physical systems. But we resist the view that theoretical hypotheses are indispensable. It seems a mistake to us to see theoretical hypotheses at work in Bohr's 1913 model of the atom. That model was not set up as an application of a more general mechanical theory. It is part of its historical identity that it was about *atomic structure*. Nor are theoretical hypotheses needed to interpret the equations that describe the UPS. These are already interpreted, albeit at a general level ('*m*' stands for mass, '*v*' stands for velocity and so on). Moreover, we have shown how theoretical hypotheses work, in so far as they do. Because the theoretical hypothesis links the UPS as described by the *interpreted* equation (rather than an abstract object) with actual systems, it is intelligible how the relation of similarity can apply. Do we consider theoretical hypotheses to be part of the theory when they *are* present? Not in the case of quantum mechanics, which was propounded, through its exemplars, prior to *its* model of the hydrogen atom. But quantum-mechanical equations still denote, via the standard interpretation of the terms that appear in the descriptions of its models. Yet they denote at such a general level that they can not be used to predict (a point often made by Nancy Cartwright, see her 1983).

One might usefully call these UPS "theoretical models," or as Hertz put it "dynamical models" (1955 [1894], p. 175). They are models in the sense that a UPS and what it is a model of are, again according to Hertz, "dynamically similar." The qualifier 'dynamical' already takes care of the respect in which the model is similar to its actual but inexact counterparts. Actual physical systems, the subject-matter of scientific theories, are represented by dynamical models in virtue of the fact that their own dynamics can be subsumed under the dynamics of the model: if the theory is correct, then if they were exact instances of the kind of system represented by the model, the actual physical systems would behave exactly as the corresponding theoretical model does.

This last point captures the central insight behind the weak version of the semantic view, as advanced by Giere (1988) and Suppes (1989), and corrects it by de-mathematizing it. The proposed correction of the view propounded by Giere and Suppe lies in our insistence that language, as a means of representation, is indispensable in our theorizing about the world: it does ineliminable representational work within theories. The process by which mathematical equations represent involves two representational media, neither of which can be left out without representational loss. The first step is the construction of an equation (a linguistic medium) which represents the behavior of a UPS. The second step is the comparison of the UPS (a non-linguistic medium) with what it is meant to represent: actual physical systems. In our two-step process of representation, the UPS is *both* the subject of (linguistic) representation, by an equation, and also a vehicle of (non-linguistic) representation, of actual physical systems: idealized, the latter are seen as inexact instances (or counterparts) of the UPS. (Hence our approach is "interactive.") It should be stressed that these "steps" are merely notional, since one does not have to consider each separately, or in the suggested order, to interpret the model correctly. In any case we should certainly leave open the possibility that a theory directly describes actual physical systems in that the UPS it studies do have this-worldly exact counterparts. But it is much more typical that the theory's UPS are not exactly instantiated in the world. Nothing of philosophical importance hangs on this issue. What *is* important is that if we leave one of the above media out of an account of theories, we leave out an essential part of the process by which theories represent their domain and have determinate empirical content. Not only should models not be considered in isolation from language (on pain of conflating physical models with abstract structures) but, more importantly, language *in its own right* is a central representational device for theories.

It has been objected that the appeal to language as a representational device is inessential, since what really happens when the theory is applied to the world is the comparison of two objects, one abstract (the model) and one concrete (the worldly physical system) (see Giere 1988, p. 82). But care is needed here: to elaborate on a point made earlier, models should not be taken to be *abstract* objects, in the sense in which metaphysicians use that word. Take, for instance, a Newtonian model of the earth-moon system. To say that the earth-moon system has the structure of a two-body Newtonian system is to say that the latter is an abstraction of the former such that the two share structure. The description, in the language of mathematical physics, of a two-body Newtonian system is an abstraction in the sense that it subtracts such secondary features as the chemical constitution of the earth's and moon's masses. But from this it does *not* follow that a two-body Newtonian system is an abstract entity. Indeed it cannot be, if a two-body Newtonian system is to be comparable at all to the earth-moon system. For the latter inhabits ordinary physical space, it can enter into genuine causal relationships, its state changes over time. Of course, abstract and concrete systems can exhibit isomorphic structures, but as noted in section 5, mere isomorphism cannot do justice to representation; and if, as Giere, suggests, the required representational relation is similarity, it is not clear at all that it can obtain between abstract and concrete objects.

In conclusion, the usual philosophical tools for individuating theories — models and statements — are separately unable to explain how theories fulfill their central task: representing the world. The linguistic approach (in both of its versions) has obscured the role of models (i.e. of UPS) in representing, while the non-linguistic approach has obscured the role of language (in particular, of mathematical equations). Theories are complex entities because both language and models are used — side by side — in order to represent. But theories are yet more complex than that: as argued in section 4, entities other than sentences and (semantic) models can play central roles in theoretical representation. That theories are consortia of representational media may be the most general and informative thing to say by way of characterizing their composite nature.

## 7. Unreconstructed Theories

In pursuing their claims about particular scientific theories, philosophers and historians of science take as substrate the written, drawn and spoken products of scientific theorizing and distil the joint "content" of these products, using whatever formal tools are available to them. Of course the relationship between substrate and product in this process is not a simple one: the scientists' claims may have to be rounded out to capture a theory's commonly agreed implications, "filled in" with extra structure for completeness, or cleaned up in the interest of consistency. The nature and extent of this rational reconstruction will reflect the (philosophical or historical) purposes of the analysis. Ideally, it will aim to give an unambiguous answer to the question: What is Theory $X$? (e.g., the Caloric Theory, or Newtonian Mechanics, or the General Theory of Relativity). Answering this question is supposed to give us a handle on the following question: what is the world like according to theory $X$? Even more ideally, such reconstructions aim to provide the raw material for a more general attempt to theorize about what all these different theories have in common. This kind of "theory of theories" is what suffuses accounts of their relations to evidence, relations to other theories, and of the things they can be used to do, like predict and explain.

Both standard views have been comrades in their attempts to rationally reconstruct scientific theories. Where they differ is in the tools they use. The received (linguistic) view went for axiomatization, and if first-order formalization predominated, this was a function of the availability and transparency of first-order methods. Proponents of the semantic (non-linguistic) view have not been so unanimous. Different approaches within the semantic conception utilize different tools and crave formalization in considerably different degrees: from the axiomatize-everything-in-set-theory approach of Sneed and the German structuralists, through van Fraassen's early state-space approach and Beth-semantics, to Giere's and Suppe's more informal attempt to reconstruct theories as families of abstract entities (Suppe 1989, Chapter 1 provides a historical survey). Be that as it may, the standard views have alike aimed at *rational reconstruction*.

We do not want to doubt the usefulness of (moderate) formalization and reconstruction. But we should not lose sight of the fact that they *are* reconstructions, or mistake their products for the theories themselves. The question "What is a scientific theory?" seems to have escaped an answer which states necessary and sufficient conditions, and for good reasons. If our analysis so far has been correct, the complexity of the ways scientific theories represent does not allow the answer to the foregoing question to be reduced to simple recipes. The "formal perspective" takes no account of the fact that theories are historical entities, and in the history of science there are no clean-cut versions of theories. We have argued that theories are complex and evolving entities which involve basic hypotheses (typically expressed as equations), linguistic fragments loaded with analogical associations, causal stories as to how phenomena are produced, auxiliary assumptions and "bridge principles," abstract and concrete models, and diagrams. Holding everything together are the basic equations, which introduce relations among the constituent parts, and their subject-matter: an evolving domain of physical systems (evolving because, for instance, supposed *sui generis* optical phenomena turn out to be electromagnetic phenomena). Now it may be that what is said using any particular representational medium could have been said using another medium. We would acknowledge the *contingency* of particular historical manifestations of theories, and stress that any philosophical analysis of theories should take this into account. If this is right, there seems to be no more informative answer to the question of the "real nature" of scientific theories than that they are complex consortia of different representational media held together by family resemblances. What calls for philosophical analysis instead is the different ways they represent the world.

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