Theories as Complexes of Representational Media

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Abstract In this paper, we review two standard analyses of scientific theories in terms of linguistic and non-linguistic structures respectively. We show that by focusing exclusively on either linguistic or extralinguistic representational media, both of the standard views fail to capture correctly the complex nature of scientific theories. We argue primarily against strong versions of the two approaches and suggest that the virtues of their weaker versions can be brought together under our own interactionist approach. As historical individuals, theories are complex consortia of different representational media: words, equations, diagrams, analogies and models of different kinds. To replace this complexity with a monistic account is to ignore the representational diversity that characterises even the most abstract physical theory.

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 linguistic and extralinguistic conceptions. On the linguistic view, also known as the received view, theories are identified with languages. On the extralinguistic view, theories are identified with extralinguistic structures, known as models. Perhaps it is pertinent to distinguish between strong and weak formulations of both approaches. On the strong version of the linguistic approach, theories are identified with certain formal-syntactic calculi, whereas on a weaker reading, theories are sets of claims (propositions) that are made in (natural-mathematical) language, whose logical relationships are investigated through the formal features of canonical linguistic formulations. In a similar fashion, the strong semantic approach identifies theories with families of models, whereas on a weaker reading the semantic conception merely shifts analytical focus from language to models.

2. Theories as Languages? A point that is often overlooked is that the so-called ‘syntactic’ view of theories was not really syntactic. This approach, as it was, for instance, developed by Carnap (1939), brought together the Duhem-Poincaré view that theories are systems of hypotheses whose ultimate aim is to save the phenomena, with the Hilbert formalisation programme, according to which theories (mathematical

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theories, to be sure) should be reconstructed along the lines of formal axiomatic systems. The prima facie advantage of a Hilbert-style formalisation of a scientific theory is that it lays bare logical structure and gives an unambiguous way to identify content: the theory consists in the set of logical consequences of the axioms, the latter being the set of fundamental hypotheses of the theory. But formalisation does not preclude questions of interpretation. In fact, a Hilbert-style characterisation makes possible the specification of the class of admissible interpretations of the theory: they are just those which satisfy the axioms of the theory. This follows from the fact that a Hilbert-style characterisation of the theory amounts to an implicit definition of its basic predicates. An implicit definition is a kind of indefinite description: it circumscribes a whole class of classes of entities which can realise the logical structure of the theory, as defined by the axioms.

After Tarski's work in model theory, the above class of interpretations is identified with the class of models of the theory. Clearly, no purely linguistic—or syntactic— consideration can single out one of the models as being the intended one. The proponent of the linguistic approach can say that the intended interpretation gets singled out by the application of the theory to a certain domain: what makes a certain formal language a theory of electromagnetic phenomena is that it finds an interpretation in these phenomena. So, the correct statement of the linguistic view is that the interpretation of a formal language—what the language is a theory of—is fixed at the point of application. This is already enough of an unjustified distinction. For, by identifying theories with formal languages, this strong version of the linguistic approach divorces the theory from its intended content: what a theory is a theory of is not a feature of the theory, conceived purely as a formal language; rather it's tacked onto the theory, at the point of application.

This is where the linguistic approach went wrong. Our diagnosis is that the problem has its origin not so much in the thought that theories are languages, but in the way empiricists thought of theories-as-languages. Carnap and co. found solace in the Hilbert programme because they found in it a way to study theories without being committed to any particular interpretation of the theoretical terms, and hence without being committed to any unwanted implications about unobservable entities. So, they thought that it's enough to interpret a part of the formal language, that which is apt for the representation of observable phenomena, while leaving the rest uninterpreted. Faced with the objection that this attitude 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 observational terms. So they ended up with all the well known problems of partial interpretation, the alleged dichotomy between observational and theoretical terms, and the analytic-synthetic distinction.[2]

All these problems which contributed to the demise of the linguistic approach are, we think, accidents of the empiricist use of this approach, rather than essential elements of the thought that language is a medium by which theories represent. To be sure, there are also well-known problems with the thought that the language of theories should be first-order (how, for instance, can they reasonably be seen as able to represent the real-number continuum?). But in any case, in mature formulations of the empiricist account of theories-as-languages (see, for instance, Carnap 1956), the underlying logical apparatus is so strong as to be virtually the whole of set theory. But dissociated from the empiricist predicament, the linguistic conception is consistent with the claim that languages are not merely formal calculi looking for interpretation. They are interpreted languages, their interpretation being fixed by understanding the theory literally. We may call this last view the 'weak version' of the linguistic approach.

The weak version of the linguistic approach doesn't go very far, however. By focusing all attention on linguistic representation, the linguistic approach obscures fundamental ways by 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 root of the problem lies with the following naive view to which even the weak version of the linguistic approach seems committed: that a good theory directly represents the world, i.e., that the world (or a certain domain) directly satisfies the theory. The naiveté of this view is apparent when we think of all the idealisations, approximations, simplifications and ceteris paribus clauses that are part and parcel of any typical scientific theory. It also becomes apparent from the fact, stressed rightly and repeatedly by Suppes (1967) and Suppe (1977; 1989), that theories confront not the world itself but models of the data and of the phenomena. Where the linguistic approach goes wrong is with its implication that either a domain X satisfies theory T or it does not. The linguistic approach cannot easily accommodate more complex representational relations that might hold between a domain and what the theory says about this domain.

[2] Which cannot be maintained in light of the fact that correspondence rules play a dual role, contributing to the meaning of theoretical terms, but also delineating the empirical content of the theory

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 it does so at the cost of neglecting the role of language as a representational medium. Under this view, models take ‘centre stage’ in the analysis of theories, where the term ‘model’ is to be understood in the logician’s sense: a structure that makes some statement (theory) true. According to van Fraassen and Giere, to present a theory is to present a class of models. Does this imply the strong semantic view, that theories are to be identified with (collections of) abstract, non-linguistic entities? Suppe (1977, 204-5; 1989, 82) has explicitly made this identification, arguing from the possibility of different linguistic formulations of a single theory, but van Fraassen (1989, 188) and Giere (1988, 84) have hinted at similar claims, on similar grounds, although Giere adds important qualifications. Suppose, says Suppe, 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 theory with something extralinguistic that has been presented in two languages. As more telling examples, he cites the equivalent formulations of the quantum theory offered by matrix and wave mechanics (1977, 205) and of classical particle mechanics by its Lagrangian and Hamiltonian formulations (1989, 82). In all cases, the argument goes, we must distinguish between a theory and its formulations: a theory should be identified with some extralinguistic abstract structure, that is with something which can admit of different linguistic formulations.

There is something in this argument, but the intended conclusion doesn’t follow. In case we think that sets of sentences in two or more different languages are one and the same theory, the theory should not be identified with one particular formulation, but rather with all those linguistic formulations which are theoretically equivalent. At this point, 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 extralinguistic 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 extralinguistic, we appeal to identity of truth-conditions. We don’t, however, identify the statement with its truth-conditions. To do that is to obscure a distinction between what we say the world is like and what makes our claims true, if they are true. 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: 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” (1926, 59) as Schrödinger 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 a later (Hilbert-space) formalism as it is a sign of equivalence in any sense wider than the mathematical.[3]

The master argument for the claim that theories are non-linguistic is that it allows a more perspicuous analysis of theories and theorising than the linguistic view, one that is closer to the structure of theoretical texts, the practice of theorising, and scientists’ usage of such central terms as ‘theory’ and ‘model’. It is to the credit of the semantic conception that it has allowed more sophisticated accounts of approximation, idealisation and relations between theory and data, as well as illuminating analyses of individual theories (e.g. Lloyd (1988) on evolutionary theories). But do theories really turn out to be non-linguistic under the semantic conception? Classical mechanics, on 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 account (also endorsed by van Fraassen (1989, 222) and Suppe (1989, 4)), the relationship between statement and model is a definitional one: classical mechanics so construed makes <u>no</u> empirical claims. Empirical content comes only with the addition of theoretical hypotheses, which are linguistic items that express representational relationships between abstract structures and given classes of real systems. So on Giere’s view, a detailed theoretical treatment of the processes that underlie some domain of phenomena will essentially involve a linguistic component. Although we have models “occupying center stage” (1988, 79), in the analysis of theories considered in isolation from their applications, the linguistic element is indispensable if the theory’s representational tools (the models) are to do any representational work.

Now the strong version of the semantic view, which identifies a theory with (collections of) extralinguistic items, must invoke some distinction between what theories ‘say’ (their content), and the claims that they can be used to make when applied to real-

[3] Hendry (forthcoming) argues that mathematical equivalence doesn’t show equivalence <u>simpliciter</u>.

worldly systems. The appearance of this distinction 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. 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" (1988, 75)), but we also need to treat theoretical hypotheses as bridge principles which give the theory whatever empirical content it has (solving Giere's "identification problem").

No less than the rival linguistic conception, the semantic approach—in its strong version—divorces the theory's content from its applications. 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 about. 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 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 it had had a different subject matter, it would have been a different theory. Secondly, even if models (as abstract mathematical structures) do play an essential role in the individuation of a theory's content, it is a (category) mistake to infer that the models themselves constitute that content. 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 semantic view, in its weak version, is right in stressing models as the means by which equations convey their message. But the models themselves are not the message.

With all this in mind, let's now show how the central representational medium of the non-linguistic conception (the model) finds its place alongside the central medium of the linguistic approach (the language) in our interactionist conception.

4. Theories as Complexes of Representational Media Hertz once said that "Maxwell's theory is in Maxwell's system of equations." 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. Maxwell's equations were important because, by systematising the most fundamental laws of behaviour of the electromagnetic field, they put an order to what Hermann von Helmholtz (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 behaviour, lie at the heart of any typical scientific theory (at least in mathematical physics). They are the core means by which a theory represents. What is noteworthy is that 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. They are written down on paper. They are solved. They are translated into different, but equivalent, notation. They are interpretable, and indeed interpreted and re-interpreted. Clearly an equation itself is not an extralinguistic entity. It may say something about one or more extralinguistic entities, but it is not one of them. So here we have linguistic representation at work at the heart of the 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.

But how and exactly what do mathematical equations represent? They describe the behaviour of, and inter-connections among, physical magnitudes. These magnitudes, e.g., the strengths of the electric and magnetic fields, are represented by mathematical entities, e.g., vectors. It is by virtue of this representation that their time-evolution and their law-like dependencies can be stated and calculated. However, one should not lose sight of the fact that the mathematical symbols which feature in the equations stand for such physical magnitudes as electric field intensity. As a result, what is being described <u>is</u> a physical system. Equations are a linguistic medium of representation, but in a physical theory, what the equations describe, if anything, are not abstract mathematical structures, but rather physical systems. To be sure, the description will typically be idealised and hence the system-type described will, strictly speaking, be unactualised. A linear harmonic oscillator (of the type described in any physics textbook) is an unactualised physical system (henceforth UPS), not an abstract entity. It's just that in the physical world, as we know it, there are no physical systems which are identical to the UPS described by the equations of the theories. Theories nonetheless do represent these physical systems, if only indirectly. Their content ultimately is the behaviour of real-worldly physical systems, in particular those systems which (suitably prepared, perhaps in laboratories) closely resemble the UPS.

One can usefully call these UPS "theoretical models", or as Hertz put it "dynamical models" (1894, 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 the actual physical system. Actual physical systems, the subject matter of a scientific theory, are represented by the dynamical models in virtue of the fact that their own dynamics is similar to the dynamics of the model: if the theory is correct, then actual physical systems would behave exactly as the corresponding theoretical model does, if they were exact tokens of the type picked by the model.

Our last point is not dramatically different from the weak version of the semantic view, as advanced by Giere (1988)[4]. But this is as it should be. Where we do differ, and this needs to be stressed, is that the process by which theories, via mathematical equations, represent involves two steps, which hook together two representational media. The first step is the construction of an equation (a linguistic medium) which represents the behaviour of a UPS. The second step is the comparison of the UPS (a non-linguistic medium) with what it’s meant to represent: an actual physical system (or a class thereof). The UPS is both the subject of representation, by an equation, and also a vehicle of representation, of actual physical systems: idealised, the latter is seen as a token of the type described by the UPS. We should certainly leave open the possibility that a theory directly describes actual physical systems in that the UPS it studies have real-worldly tokens. But, it’s much more typical that the theory’s UPS are only similar to actual physical systems. Nothing of philosophical importance hangs on this issue. What is philosophically important is that if we leave out of an account of theories one of the above media, we leave out an essential part of the process by which theories represent.

What is typical of all versions of the semantic view is their monistic understanding of the term ‘model’. Defenders of the semantic view have recognised that the term has many different uses (cf. e.g. Suppes 1960), but when it appears in the claim ‘to present a theory is to present a collection of models’, it is usually taken to invoke only one kind of thing: the semantic, or logical model. There are many things to be said here about how the semantic view misrepresents the role of exemplars in relations between theory and data models (see Hendry 1997). For brevity’s sake, we shall focus on the iconic (or analogical) models of Hesse (1966). One physical system (the source system) is a model of another (the target system) in virtue of positive analogies between them. Given the positive analogies, fragments of mathematics associated with

[4] See also Suppe (1989, 85), who relates idealised models to counterfactual situations

descriptions of the behaviour of the source system are transferred to the target system, along with (ideas about) causal structure, and attendant auxiliary assumptions in mathematical form (see also Hughes 1993 for an account of this transfer process). Thus did Bohr re-describe the hydrogen atom as a solar-system (albeit an atypical one), and the hydrogen atom inherit the solar-system's mathematics. The elastic-solid models of the luminiferous ether provide another case in point (for more on the construction of analogical models and their heuristic role see Psillos 1995). Viewing models as abstract structures which make equations true cannot account for the process highlighted, by which iconic models come to represent target physical systems. Models of the the source system and the target system might share structure, and they better had do. But that is the result of a long process, not the process itself. What gets transferred from source to target system is a set of equations which, suitably interpreted, are supposed to govern the behaviour of the target system, and hence form the basis for the development of a model of the target system. What is central to the iconic model's role is its dynamic heuristic relationship with the new equations, rather than its static internal mathematical structure. Discussing Hesse's critique of correspondence rules in the received view, Suppe (1977, 95-102) questions whether Hesse establishes that iconic models are indispensable parts of theories. 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 captures the heuristic power of the iconic model.

If, as we think, it is possible to make causal and theoretical claims using non-linguistic representational media—diagrams and real (that is, material) models—then it would seem that the list of possible representational tools should be extended even further. Take, for instance, structural chemistry in the nineteenth century, which coalesced around particular schemes of diagrammatic representation and ball-and-stick renderings of particular molecular structures. Even if some diagrammatic representations, potential energy surfaces for instance, are inherently tied to systems of equations, the claim that all such representations are so tied seems to us extravagant and unsupported. It might be objected that none of these items are indispensable, in the sense that their representational work could have been done in other media. In this case one should at least expect a plausible representational equivalent of the same type as the proposed analysans for theories, but even that would not be sufficient. Merely because something else could do the job of representing does not entail that the diagrams themselves don't do that job.

So theories are complex because the usual philosophical tools for individuating theories—models and statements—are separately unable to fulfil the central task of theories, 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 extralinguistic approach has obscured the role of language (in particular, of mathematical equations). But theories are yet more complex than that: entities other than sentences and (semantic) models can play central roles in theoretical representation.

5. 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 theorising 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 isn't 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 theorise 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.

The standard views have been comrades in their attempts to rationally reconstruct scientific theories. Where they differ is in the tools they are using. The received view went for axiomatisation, and if first-order formalisation predominated, this was a function of the availability and transparency of first-order methods. Proponents of the semantic view have not been so unanimous. Different approaches within the semantic conception utilise different tools and crave formalisation in considerably different degrees: from the axiomatise-everything-in-set-theory approach of Sneed and the German structuralists, through van Fraassen's early state-space approach and Bethsemantics, 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 don't want to doubt the usefulness of (moderate) formalisation and reconstruction. But we shouldn't 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. 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 of family resemblance among the constituent parts, and their subject matter: an evolving domain of physical systems (evolving because supposed sui generis optical phenomena turn out to be electromagnetic phenomena, for instance). 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 complexes of representational media held together by family resemblances. What calls for philosophical analysis instead is how they represent the world.

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