Semantic approaches in the philosophy of science

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In this article I give an overview of some recent work in philosophy of science dedicated to analysing the scientific process in terms of (conceptual) mathematical models of theories and the various semantic relations between such models, scientific theories, and aspects of reality. In current philosophy of science, the most interesting questions centre around the ways in which writers distinguish between theories and the mathematical structures that interpret them and in which they are true, i.e. between scientific theories as linguistic systems and their non-linguistic models. In philosophy of science literature there are two main approaches to the structure of scientific theories, the statement or syntactic approach – advocated by Carnap, Hempel and Nagel – and the non-statement or semantic approach – advocated, among others, by Suppes, the structuralists, Beth, Van Fraassen, Giere, Wójcicki. In conclusion, I briefly review some of the usual realist inspired questions about the possibility and character of relations between scientific theories and reality as implied by the various approaches I discuss in the course of the article. The models of a scientific theory should indeed be adequate to the phenomena, but if the *theory* is 'adequate' to (true in) its conceptual (mathematical) models as well, we have a model-theoretic realism that addresses the possible meaning and reference of 'theoretical entities' without relapsing into the metaphysics typical of the usual scientific realist approaches.

1. Introduction

In this article I give an overview of recent work in philosophy of science dedicated to analysing the scientific process in terms of (conceptual) mathematical models of theories and the various semantic relations between such models and scientific theories. Such analyses also touch on questions regarding the nature of scientific progress and the usual realist inspired questions about the possibility and character of relations between scientific theories and reality.

The use of the notion of model is nothing new in either philosophy of science or the (empirical) sciences themselves. Writers such an Achinstein (1968), Hesse (1963), and, more recently, Redhead (1980) have paid much attention to the heuristic uses of models in the development of scientific theories. In his article, entitled 'A comparison of the meaning and uses of models in mathematics and the empirical sciences', Patrick Suppes (1960) reviews the various uses of the notion in mathematical statistics, psychology, economics, and physics. Nancy Cartwright (1989, 1995a, 1995b) also often makes use of the analogies between the ways in which models are used in econometrics and theoretical physics to illustrate her views of the nature of scientific theories.

In current philosophy of science the most interesting questions centre around the ways in which writers distinguish between theories and the mathematical structures that interpret them and in which they are true, i.e. between scientific theories as linguistic systems and their non-linguistic models.

In philosophy of science literature there are two approaches to the structure of scientific theories, the 'statement' or syntactic approach, and the 'non-statement' or semantic approach. The statement approach is characteristic of philosophers and logicians like Carnap, Hempel, and Nagel. The advocates of this approach use the tools of mathematical logic to depict theories as axiomatic systems in some well-defined language, and study the syntax and semantics of theories via the proof and model theories of language. The advocates of the non-statement view of scientific theories emphasise the tools of algebra and set theory. This approach originated with Poincaré's work in geometry and mechanics and has started developing through semantic analyses of non-statement reconstructions of certain scientific theories done by Von Neumann (1955), Adams (1959) and Suppes [see McKinsey, Sugar & Suppes (1953), Suppes (1959)], and also Montague (1962).

The statement or 'received'[1] approach depicts the rational reconstruction of the language of science as a syntactic system with an axiomatised deductive theory formulated within that system. Its defenders usually characterise theories in terms of two parts. First they identify an abstract formal calculus (a symbolic language) in which the primitive symbols (which in this case are terms that do not have obvious relations with 'observation' terms, i.e. so-called 'theoretical' terms like 'electron', 'particle', 'mass', and so on) of the theory are set out. The second part of the structure of a scientific theory they depict as a set of rules (called 'correspondence rules' by Carnap[2] and 'bridge principles' by Hempel) that assigns empirical (observational) content to the logical calculus by providing 'co-ordinating definitions' or 'empirical interpretations' for at least some of the primitive and defined symbols of the calculus, and in that way – supposedly – establishes *direct* links between elements of the theory and elements of reality.

Advocates of the 'non-statement' or semantic approach view the rational reconstruction of the language of science in terms of a syntactic system and a family of interpretations (or models) of that syntax. In opposition to the statement approach's belief that theories are formulated in some (first-order) symbolic language with direct links to reality, the defenders of the non-statement approach believe in an analysis where the language in which the theory is formulated plays a much smaller role. They hold that foundational problems in the various sciences can in general be better addressed by focusing on the models these sciences employ than by reformulating the products of these sciences in some appropriate language.

The model-theoretic realism expounded in this article retains the notion of a scientific theory as a (deductively closed) set of sentences, while simultaneously emphasising the interpretative role of the conceptual (i.a. mathematical) models of these theories. My criticism against the non-statement approach is based on the fact that merely 'giving' the theory 'in terms of' its mathematical structures leaves out any real interpretation of the nature and role of general terms in science. Against the statement approach's 'direct' linking of general theoretical terms to reality, my approach interpolates models between theories and (aspects of) reality in the interpretative chain. I shall first set out my own approach in Section 2, before continuing to discuss some of the main views in the non-statement school of thought, using my approach as a unifying meta-view.

The choice between the statement and non-statement approaches seems trivial as far as theories formulated in first-order languages are concerned. As Theo Kuipers (Kuakkanen 1994:5) points out, the set of models (that is, structures for which the statements of the theory are true) of theories formulated as a set of statements of some first-order language is exactly the kind of (set of) structures that the defenders of the non-statement approach view as the building blocks of empirical theories. In other words, possible interrelations between the two approaches exist in so far as an axiomatised theory may be characterised by a class of interpretations which satisfy it, and an interpretation (or class of interpretations) may be characterised by a set of sentences which it satisfies (and in neither case will the characterisation be unique). I believe though that clarification of certain core problems in philosophy of science – such as the relations between scientific theories and reality, and the notion of truth – is more likely when following the emphasis on models that the semantic approach offers, while retaining the statement view's analysis of a theory as a deductively closed set of sentences.

The non-statement approach has had several branchings since Patrick Suppes emphasised – against the metamathematical musings of the advocates of the received view – the clarifying advantages of set-theoretical reconstructions of empirical theories. I shall discuss Suppes' approach in Section 3. The structuralist programme lead by Sneed, Stegmüller, Moulines, and Balzer offers a structural analysis of scientific theories, and I shall discuss this programme in Section 4.[3] The semantic approach offers an examination of the content of theories via Beth's notion of state-spaces and is supported by Van Fraassen, Suppe, and the (naturalistic) view of science offered by Ronald Giere. These notions are discussed in Sections 5.1 and 5.2. Finally, I shall discuss the approach offered by a kind of affiliation to the semantic approach, which is headed by Wójcicki, and Przelewski[4]– who both concentrate on offering an empiricist semantics for science while also working on the problem of 'analyticity'[5] – and also followed by Tuomela and Rantala[6] who apply this approach respectively to problems concerning the nature of theoretical terms, and the problems of definability and indefinability in science. Wójcicki's approach is briefly discussed in Section 5.3. I shall devote the whole of Section 6 to Nancy Cartwright and her analysis of the fundamental laws of physics. Cartwright is not a non-statement defender, but neither does she really fit into the statement framework. She does retain a (syntactic) notion of a theory, in the sense that she often refers to sets of field equations as theories, but it is not always clear what her views are on the notion of theories as deductively closed sets of sentences. Her continued claims concerning the 'falsity' of the fundamental laws contained in scientific theories seem to indicate that, like the advocates of the non-statement approach, she views the role that theories (as linguistic entities) play in the processes of science redundant. Thus, like mine, her account of science has statement and non-statement characteristics and also she addresses the issue of realism in various ways throughout her account.

2. A model-theoretic approach to science

2.1 Introduction

The model-theoretic analysis of the process of science that I propose is done in terms of a logical reconstruction of the 'life' of a typical scientific theory, and is offered against a stratified view of the process of science. This stratification is three-fold: it consists of an empirical level, a middle conceptual level, and a purely linguistic level.[7] The terms of the first level are very particular, those of the second are more general, although still specific, while the final level is a level of complete generality.

The 'purely empirical' level I interpret as consisting of various systems in reality and our interactions with them, while I view the final level as a level of linguistic systems at which a scientific theory is formally formulated and suitably expressed in some appropriate language. The middle conceptual level is a very complex one in the sense that it has various facets that, in their turn, may be seen in terms of a certain kind of hierarchy. It is at this level that both models interpreting the theory and making it true are constructed, and from which the issue of adequate reference to real systems is examined. I claim the latter relations of adequacy to consist of various scientific experimental and observational activities, which may lead to the establishment of a relation of isomorphism between some model of a theory and some 'empirical' model of some real system and the experimental relations executed in that system.

2.2 Terminological note

In this article I shall take examples from the natural sciences – mostly from physics and astronomy – to support and illustrate my arguments. A model of science such as the one that I shall set out in this article works very well for the natural sciences, because all three main aspects of such a model are simple and clearly portrayed in the natural sciences. As far as the aspects of reality studied go, an electron is a far simpler concept than a human being; the models employed at the 'middle' level of my interpretation of science can often be mathematical in the natural sciences, which they cannot necessarily be in other sciences; and finally, the languages can be formalised quite easily in the natural sciences (in first-order logic for instance), while other sciences often use full natural language which is tremendously more complex. I am convinced though that the model that I am proposing for the philosophical interpretation of science is also applicable to the so-called 'social sciences', although the stage of theory formulation and the interpretative stage of the scientific process will differ from that of the natural sciences as far as certain emphases on context dependency and other related issues are concerned. Apart form referring to certain parallel issues in economics and econometrics here and there, the scope of this article however does not allow me to go into these matters in any more detail.

Before proceeding, it is necessary to give at least an informal explanation of the notions of 'theory' and 'model' as I shall be using them. Let us choose a first-order predicate language, L, in which a deductive theory T is formulated.[8] The only condition I set with regards to language L, is that it should be appropriate for formulating statements about mathematical structures. Let us say that theory T is the (deductively closed) set of all formulae which can be deduced from a consistent set (system) of axioms, Σ, in formal language L. Now, in this language, L, there will be – among other things[9] – an infinite, countable set of individual variables and a nonempty set of predicate letters. Then, a mathematician (or 'scientist' for my purposes) may give meaning to these symbols used to formulate the sentences in theory T in language L, by constructing a certain mathematical structure, call it U, suitable to be described by the language L.[10] An interpretation of language L will consist of a set over which we consider the individual variables to range, and predicates or relations defined on this set as interpretations of the predicate symbols in L. Thus as soon as every n-ary predicate symbol in language L is associated with an *n*-ary relation in structure U, we can say that this mathematical structure is an *interpretation* of the language L, and thus, by implication, of any sentence in L.[11] Note that of course, for *every other definition* of the domain of the mathematical structure and of the relations defined on it, one is confronted with *another interpretation* of the language. There are thus no 'rigid designators' across interpretations. A *model* of any formula such that every free occurrence of variables in it refers to an element in the domain of an interpretation of L by means of a specific valuation, will be an interpretation under which that formula is true by the specific valuation defined for its (the formula's) variables.[12] Now, a *sentence* is a formula with no free occurrence of variables. Thus the definition of the truth of a formula implies that a sentence will either be true under an interpretation by all possible valuations or false under all valuations. Hence for sentences we may speak of truth under an interpretation without mentioning valuations. And, a set of sentences is true under an interpretation if every sentence of that set is true under that interpretation. Thus a *model* of a theory (being a set of sentences in some formal language L) will be an interpretation under which that set of sentences (i.e. the theory) is true.

2.3 The formulation of scientific theories

Now, turning to the epistemic process leading to the formulation of a scientific theory, the following. No-one – not even scientists – ever studies reality in all its complexity. The way in which we come to knowledge is determined by acts of abstraction and simplification. Thus, rather than focusing on the colourful richness of reality, scientists typically will decide to focus on a particular aspect of reality. Moreover, intensifying their initial selective actions, scientists will also decide to concentrate only on particular features of the real system they have picked out.

At the start of a particular line of research, the first encounters between scientists and the relevant system in reality have an interesting feature. Although traditionally viewed as happening at the lowest level of scientific activity – 'lowest' in the sense of least abstract and not least dependent on historical, social, and cultural factors – these encounters are already not 'objective' in the sense of being neutral to any kind of external influence. This is because of the influence various contingent factors have on the actions of scientists and their arguments. These factors range from extremely specific to broad combinations of general factors influencing scientists at a given time.

They include personal factors such as the personal interests of scientists involved, their particular research goals, and the social context in which their research is done. Then there are also factors that pertain more to the 'theory-ladenness' of the choice of experiments, the interpretations of data, and so on. These factors include the paradigm or research tradition in which the scientists are working, the state of their discipline at the time, the level of technology and experimental apparatus available at the time to those particular scientists, and also the body of 'already established' theories (as the background) against which these particular scientists will work. Also included here are factors to which Gerald Holton (1995) refers as 'themata'. These factors are on the one hand, a cross between the specific motivation behind the choice of addressing results and problems within one particular scientific framework rather than another, and, on the other hand, the scientists' world view at the time.[14] In this sense, I agree with the constructivists: no scientific activity takes place in some kind of objective vacuum.

The inherent conditioning, or refining, abstractive nature (and potentially idealising power) of activities carried out at this level leads ensuing activities to a level more general in scope than that of the original encounters with specific aspects of reality. At this level scientists create conceptual 'models' – which I call 'intended models' for obvious reasons – of the real system in question. These models are obviously not (yet) formally identifiable as interpretations of any sentences of the language in which the final theory will be formulated. However, after theory-formulation, at the stage where possible interpretations and (empirical) applications of the theory in question are considered, it will become clear that the intended 'model' of the theory in question is also one of the possible mathematical models (i.e. interpretations under which the theory in question is true) of the relevant theory. (Thus, at the formulation stage of scientific theories, my use of the term 'model' is perhaps not strictly in the same sense as the one model theorists use.)

For example, let us consider briefly the formulation of Newton's laws of motion and his law of gravitation. Newton wanted i.a. to continue Kepler's research about the movement – and positioning – of the planets in *our* solar system. Kepler's laws originated, it seems, largely because of his own interest in specifically the movement and positions of the planets in our solar system. His intended model thus may be said to have been supported by data concerning only these (and related) planetary features. His research was based on observational data regarding the positioning of the planets at different times, much of which was the original work of Tycho Brahe. His interpretation of his observational data would have been, for instance – and probably among other things – influenced or 'laden' by his mathematical idealism – e.g. his claim that the planetary orbits should fit exactly into nested Platonic solids. Finally, Kepler's research culminated in his formulation of his three laws – the first two in 1609 and the third in 1618.

Now, Newton could not study all the complexities of our solar system as it manifests itself in the manifold of reality. He was also, as mentioned above, interested in examining planetary motion in our solar system. Thus he identified the details necessary for his research goal by abstracting from this system in reality only those specific features in which he was interested. He would have discarded, for instance, the fact that the sun's rays are hot, that Mars seems to be reddish in colour, and so on. But, in this way, because these abstractions were so closely guided by his intentions – and certainly influenced by both Brahe and Kepler, and also Galileo's findings – he would never really have been dealing with the bare data that he had extracted from reality. He would, rather, in fact, have been dealing with a conceptualised model of our solar system that would in the end lead him to the formalisation of *the* theory of solar systems itself.

In order to study the dependence of the force of gravity on the distance from the centre of the earth, Newton compared the fall of a stone (the alleged apple) on the surface of the earth with the motion of the moon. Newton discovered that the '... forces of terrestrial gravity decrease as the inverse square of the distance from the centre of the earth'

(Gamov 1962: 62). He consequently generalised this result to 'all material bodies in the universe' (ibid.) and so formulated his universal law of gravitation.

Scientists thus conceptualise their objectives in the light of the data they gather – and may still be gathering – with an eye on their research goals and guided by the specific scientific tradition, community, theoretical network or paradigm they are working from.[15] The creative context of this stage of the scientific process offers scientists the chance to test and constantly reformulate their conceptual structures and to receive results under the conditions set by their goals and the context within which they work. This implies that these (intended) models have an idealised nature[16] in the sense that they are the results of extremely focused actions which typically disregard factors in the empirical system in question that could muddy the waters of their research.

The progression of generalisation common to this stage of science may perhaps roughly be logically reconstructed to range from

- scientists' initial sensations (possibly mediated by an apparatus) of 'real' objects and their behaviour in some real system (broadly, of aspects of reality), to
- the construction of percepts of these sensations, to
- the construction of concepts of these percepts, to
- the construction of conceptual models which are structured sets of these concepts, and which
- may then – in certain sciences – culminate in the formulation of mathematical models.

Finally, these (abstracting) actions culminate[17] in the formulation of a general (abstract) theory – expressed in some suitable language – in the field of research in which the relevant scientists have been working. The nature of this level at which theories are formalised is abstract, general, and simple in the sense that the values (meanings) of the parameters in the general theories are essentially *unconditioned* and the meaning of theoretical terms (such as 'electron' or 'mass') is in principle open to valuations or interpretations made by scientists interested in applying or implementing the theory. This implies naturally that a potentially infinite number of conceptual (or mathematical) models can be constructed of one and the same theory.

The aim of Kepler's research surely was to formulate some kind of law (or laws) concerning planetary motion. It is sometimes claimed that Kepler's laws do not constitute a scientific theory, however. For instance Dilworth (1994: 135) claims:

> The main reason usually given for Kepler's laws, taken together, not ranking as a theory is their unequivocally empirical character – i.e. the fact that they are 'instantiated' in the sense that they refer to the individual planets in the solar system and, unlike a theory, are capable of being tested more or less directly. The present view [Dilworth's] supports a distinction along these lines, and in fact provides an explanation of it, viz., that, unlike Newton's theory, Kepler's laws are not integrally related to a model ... .

Well, it is indeed the case that Kepler's laws are very 'empirical' and less general in scope than Newton's laws of motion and his law of gravity, since the latter do not give any particular value or parameter to their theoretical terms.[18]

However, Kepler does generalise from his intended model in the sense that his laws are about the motion and position of all the planets – or any planet – (known to him) in our solar system, and not particularly about any one specific planet. Moreover, the application of his laws to the motion and position of any one planet may be seen to imply the construction of a specific conceptual model concentrating only on the particulars concerning that specific planet. Although his laws are indeed far less general than Newton's, and also far more like the kind of 'empirical' theory philosophers in the Popperian tradition advocate, his laws can be viewed as part of an 'intellectual system' of the kind Torretti (1990: 24) supports. Torretti's argument comes down to the following: Should it be found (as perhaps from a certain perspective it was discovered much later) that a particular planet does not obey Kepler's laws it would imply a revision of our scientific thoughts concerning planetary motion. I also take this as sufficient motivation to view Kepler's laws as comprising a theory (albeit perhaps a 'low-level' one).

It is the case that Newton's laws of motion and his law of gravity could in fact finally explain Kepler's laws, and thus that Kepler's laws may perhaps not be said to explain the positioning of the planets but rather merely to describe their motion.[19] I think that we have at least to accept the legacy of the advocates of the deductive-nomological model of explanation in so far as we accept that scientific explanation is really some kind of inference, the conclusion of which describes the facts to be explained. Generally, Newton's laws *explain* Kepler's in the sense that a conceptual model of Newton's laws may be given by our solar system, and the data giving positions and motions of a particular planet in that solar system may be viewed to constitute an empirical model that is isomorphically embedded in the above conceptual model. More formally, the application of Newton's universal law of gravity to the motion of our solar system's planets around the sun (conceptual model) enables one to mathematically derive Kepler's three laws (empirical model). More specifically for instance, Newton's law of gravitation explains Kepler's third law in so far as it shows that Kepler's third law is based on a force that is exerted towards the centre of the sun and that is inversely proportional to the square of the distance between the sun and the planet in question. However, it has to be noted that although Newton's laws explain Kepler's they do *not*, after all, explain e.g. gravity – they merely describe it.

Do Newton's laws constitute a theory? Should his laws be viewed as the axioms of this theory or as empirical laws in the Popperian way? In the model-theoretic approach to science that I advocate, the axioms of a theory describe the conceptual model(s) in which they (the laws they represent) are true. Such a model then sets out the calculation of values of certain functions and the interpretation of certain theoretical terms in the context of the model. In this case, for instance, it could mean that bodies are conceived of as 'mass-points' without extension. The theory in general sets down the nature of the relations between the terms in its conceptual models. Thus on my account, Newton's laws of motion and his law of gravity do constitute a theory since they are general enough, or broad enough in scope to offer different conceptual models and interpretations of its terms in different contexts of application.[20] They can be viewed as 'empirical' laws only via the interpreting mediation of conceptual models and some empirical substructures representing observational data and other empirical calculations, which brings us to the next section on the interpretation of scientific theories.

Now, in order for scientists to give 'reference' to the multi-interpretable theoretical terms and parameters[21] of scientific theories, more models – other than the original one leading to the formulation of the theory – may be constructed. In other words, because of the possibility of all these different models of theory T (in language L), the theory – say in our example, Newton's laws – may be (in principle at least) related to any (mathematical and thus conceptual) model in which it is true and not only to the intended one. If we take the set of axioms, Σ, to be Newton's three laws of motion and his law of gravitation, expressed formally, then they will hold in any planetary system (because of the general nature in which they were formulated) and then we can pick any 'planet' in such a system and be sure that its orbit will be an ellipse and the system's 'sun' will be in one focus. For example, a model in which, say, Jupiter is the 'sun' and its satellites are the 'planets' may now be constructed.

These models are thus interpretations of the theory[22], each of which is also in its turn determined by – among other factors – the research intentions and thematic preferences of the scientists wishing to apply or study the theory. Note here that the first most obvious model of the theory is its original 'intended' one. However, since different groups of scientists will be applying the theory in question – perhaps for different reasons – at various times, this is not necessarily the model that will be chosen as the one via which the theory is to be applied or interpreted. The intended model is thus in nature no different from the mathematical structures that will be constructed to interpret the theory in such a way that the theory will be true in them. These models simply differ as far as the nature of their origins is concerned, and features common to both – such as the role of thematic preferences – are simply emphasised differently in each case. In this sense for example, the intended models have more of an organising and guiding role in the sense of being the first conceptual means via which scientists are able to make the first abstractions from reality. The conceptual models will give reference and meaning to or 'fill in' the content of (some of) the general terms (e.g. electron, mass, velocity, temperature) used in the theory[23] and specify values for the parameters of formulae in the theory in such a way that the theory turns out to be true in these models. The models thus constructed are then obviously mathematical models in the Tarskian sense.[24]

Although more specific than the theory, these models are still general in nature in so far as they are idealisations in the same way as the intended models are. Nancy Cartwright (for instance Cartwright, 1983, 1986, 1989b) sees this as problematic as far as the possibility of theories offering descriptions of reality is concerned. I disagree, as will be discussed in Section 6. It is impossible to give clear-cut rules or conditions for the adequacy of our conceptions or the real existence of objects in systems in reality, because of the *open-ended* generality of theories in the sense of the different models in which they may be true and which is a consequence of the 'abstract' character of theories and also because of the open-endedness of the conceptual models in the sense of their 'ideal' nature. The role played by the empirical activities of science in establishing the last referential link between some system in reality and some model of the theory needs a suspension at the conceptual level of the *ceteris paribus* clauses at play at the linguistic level of science in order to fill in the details that, at this level, have been 'idealised' and 'abstracted' away.

Here is an example to illustrate the construction of different models interpreting the same theory. Newton's laws made it possible to calculate very precisely the motion of the planets in our solar system (any solar system for that matter) under the influence of mutual gravitational attraction. Up to 1820, scientists interpreting (or applying) Newton's laws of motion and his law of gravitation to our solar system had worked in a model of these laws (comprising Newton's 'theory') which consisted of only seven planets. Then, in 1820, calculations carried out within this model started to give 'wrong' predictions, and it became apparent that the motion of Uranus 'did not conform to Newton's grand scheme' (Schwinger 1986: 195). The possibility that the motion of Uranus could be affected by the gravitational attraction of another planet seemed a good solution to the problem though. So, scientists thought of postulating the existence of an eighth planet, and consequently constructed a different model of Newton's theory, now with eight planets. In 1845 John Adams calculated the position of this 'new' planet – Neptune – in our solar system, and shortly afterwards Urbain Leverrier's calculations confirmed Adams's findings.

Applications of the same Newtonian 'theory' (his three laws of motion plus his law of gravity) includes the 'discovery' of Pluto in 1930 as the result of theoretical calculations based on the universal law of gravity. Also Newton gave the first explanation of the 'precession of the equinoxes' since the time of the Greeks by applying his law of gravity to the motion of the earth. And, a last example, aspects of the motion of the tides of the sea could be explained by applying the universal law of gravity to the earth's perihelion and aphelion motions (i.e. the movement of the earth far from and close to the sun). Thus new information results in different (new) models still constructed to attain the same (previous) goal, but also different aims result in different models.

I take the relations that exist between some theory and the mathematical (conceptual, semantic) models that are interpretations of the theory's language and in which the sentences of the theory are true, as the first set of relations that determine the possibility of reference to some real system. The goal of a formal logician will be to prove that his deduction (theory) is valid, i.e. true in all possible worlds allowed by the axioms, i.e. true in all possible models (in the conceptual system), one of which may or may not be in its turn 'about' some system in reality. Thus, for the formal logician, the question of whether it is possible to construct a 'second set' of interpretations or models (to retrace the steps of the original scientist – representing the group of scientists that "formulated" the theory in question – even further back to reality), is rather irrelevant.

Scientists, however, will definitely be interested to know whether one of the conceptual models of their theory can have a system in reality as some further interpretation or model, because they formulated their theory precisely to enable them to make some sort of claim about a certain aspect of some real system. The method of verification of each of these (conceptual, mathematical) models (i.e. how well do each of them reflect the system in the real world?), will be decided by the specific nature of the specific (conceptual, mathematical) model in question, *as well as* by the nature of the specific real system in question. It could be that an observation through a telescope is needed, or an observation through a microscope, or some sort of calculation, which has less to do with observation, and so on and so on. In other words, neither Tarski, nor anyone else, could or can really give a general criterion for the truth of the sentences in this last set of interpretations.

Should some of the elements and relations of one such a conceptual model be interpreted to correspond to objects and relations of some system in the 'real world' however, I claim a further mathematical model may be identified such that its objects and relations (representing the relevant empirical data) constitute a mathematical structure which is (isomorphic to) a substructure of the relevant conceptual model. If the phenomena in some real system and the experimental data concerned with those phenomena are logically reconstructed in terms of such a mathematical structure – call it an 'empirical' model – the relation of empirical adequacy (characterised by the various actions mentioned above) then becomes a relation which is an isomorphism between the empirical model and some substructure of the relevant (conceptual, semantic) model of the theory in question.

For example, take a conceptual model of the theory constituted by Newton's laws of motion and his law of gravity as the elliptical orbit of some planet – say Pluto – around the sun, with the sun in one of its foci. Say 117 individual observations of Pluto on this elliptical course are made by scientists working in this model. Then these observations, jointly (interpreted as 117 points, at different times, on an ellipse) represent an empirical model of the theory.[25]

Thus, my notion of empirical models implies that both the models assigning reference to the general terms of the theory and the empirical models representing the (usually quantifiable) aspects of verifying the conceptual models of some theory have to be 'constructed'. 'Construction' in the first case is more formal and less complex than 'construction' in the second case. In the second case it implies that data about some system in reality is 'found' or 'discovered' via the necessary experiments co-determined and executed in terms set out by the relevant (conceptual) model of the theory. And the results of this 'construction' may then be represented by some empirical model in which those results about the relevant real system are true.

Note also that the referential relations between the theory and its models are much more simple – although also neither rigid nor absolutely fixable – than the relations between conceptual models and empirical models of systems in reality. The latter are extremely complex and never passive (or absolute), because in this case there are so many more variable factors to take into account when considering this more informal and supple relation. I quote Sir Allan Cook's (1994: 141) explanation of the link between observation, models and theory in physics to show this more clearly:

> Observation is never an isolated activity. The way that we observe depends on human capabilities and properties of nature. Observation may affect the objects observed and our observational procedures depend upon the state of technology and are guided by theory. The results of observation [represented by my empirical models] have to be derived by procedures that depend upon some theoretical model [one of my conceptual models] as well as upon experimental techniques, ... The harder we question nature, [and] the more fundamental the observations we make, the more dependent are the results on technique and theory.

Moreover we as philosophers cannot tell – especially not before the fact – which specific conceptual construct (which interpretation of some theory) provides the most adequate description of some relevant system in reality.[26] Only science itself can offer us – at some more mature stage of scientific development – an ontology (or ontologies) which can specify the *contents* or *detail* of the structures reality contains and the particular ways in which they behave.[27] Thus, neither the adequacy ('truth') of our conceptions nor the 'reality' of the system as described by some theory, is absolute, because both are products of epistemically relative interpretations and subject to change. 'Adequate' scientific statements may, indeed, say of reality that it is the way it is. It is, however, only through the mediation of (conceptual and empirical) models that this can be established, and never directly by somehow comparing theories to reality.

3. Patrick Suppes's set-theoretic reconstructions of empirical theories

Suppes offers one of the first viable alternatives to the 'received (statement) view' of scientific theories and so brings about a radical turn in philosophy of science. Other than the structuralists who stress the use of formal semantics and meta science to appeal to the structural aspects of theories, Suppes finds the axioms of set-theory sufficient, and claims mathematics, rather than meta-mathematics to be the language of science.[28]

According to Suppes (1967: 57) the problem with the statement approach's co-ordinating correspondence rules is that they do not in the sense of modern logic offer an adequate semantics for the axiomatic calculus of the theory. Wójcicki (in Humphreys 1994: 127) explains that '... if for the logical positivists the right way to define an empirical theory T was to define a set of axioms from which all the other sentences valid in T are logically derivable, Suppes suggests that to define T is to define a set-theoretical predicate that denotes all the set-theoretical structures [semantical models] of which T is true in the Tarski sense'. Suppes does not so much emphasise the non-statement approach versus the statement approach though. He (Suppes 1954: 244) rather stresses the advantages of analysing empirical theories within a set-theoretical framework rather than a meta-mathematical one.[29]

Suppes (in Morgenbesser 1967: 60) points out that one of the simplest ways in which to provide an extrinsic characterisation of a theory is to define the intended class of models of the theory; and then asking if the theory can be axiomatised, merely comes down to asking if a set of axioms can be stated such that the models of these axioms are precisely the models in the defined class. The class of structures (systems) under consideration is thus described by giving one 'generic' structure, with parameters, which can be specified to deliver all the systems in the class. He (Suppes in Morgenbesser 1967: 61, 62) remarks however that '... the problem of intrinsic axiomatisation of a scientific theory is more complicated and considerably more subtle .... Fortunately, it is precisely by explicit consideration of the class of models of the theory that the problem can be put into proper perspective and formulated in a fashion that makes possible consideration of its exact solution.' And, in model-theoretic terms, even more positively, such consideration of the class of models of a given theory shows the continuous character of science.

Suppes addresses the philosophically problematic relations between empirical systems and theories (i.e. my 'second set' of interpretational relations) in terms of a hierarchy of models that focuses on the complex nature of the experimental process.[30] He (Suppes 1954:243) already points out very early on in his work that progress in foundational studies of philosophy of science requires distinction between theory and experiment, since the reconstruction of the experimental stage of science is rather more problematic in comparison to the theoretical stage which may be axiomatised 'quite easily' with the help of set-theoretic predicates. He (Suppes 1954:246) wants to provide philosophy of science with '... a kind of algebra of experimentally realisable operations and relations' and emphasises that discussion of the empirical interpretations of the primitive notions for certain defined notions of some empirical theory imply interpretations of quantitative notions, which necessitates some systematic theory of measurement.[31] He is not interested in the classic notion of absolute objective truth, nor is he interested in the kind of framework offered by the instrumentalists, rather he wants to speak about truth in terms of modern statistical decision theory.

Thus, one of the most important issues in Suppes's philosophy of science is the emphasis he puts on the 'experimental stage' of science.[32] Empirical interpretations of the primitive notions for certain defined notions of some empirical theory are interpretations of quantitative notions, which necessitates some systematic theory of measurement, as already mentioned:

> ... the point of a theory of measurement is to lay bare the structure of a collection of empirical relations which may be used to measure the characteristics of empirical phenomena corresponding to the concept. Why a collection of relations? From an abstract standpoint a set of empirical data consists of a collection of relations between specified objects. For example, data on the relative weights of a set of physical objects are easily represented by an ordering relation on the set; additional data, and a fortiori an additional relation, are needed to yield a satisfactory quantitative measurement of the masses of objects (Scott & Suppes 1958: 113).

Thus, as far as the co-ordinating principles or bridge principles of the statement approach are concerned, Suppes stresses (in Morgenbesser 1967:62) that the practice of testing scientific theories is a much more complicated issue than is implied by the usual comment about these issues.[33] I agree with this, but I do not see the philosophical need for turning almost exclusively to the statistical methodology to examine these relations that Suppes (in Morgenbesser, 1967, and also Suppes, 1969, Suppes, 1989, and Suppes, 1993) insists on.[34] I think that for the purposes of philosophy of science, it is sufficient – and a more philosophically challenging prospect, I might add – to look to the various model-theoretic relations involved, and to be able to point out all of (or as many as possible of) the factors involved in these connections.[35]

Suppes and Dana Scott in their article 'Foundational aspects of theories of measurement' (1958) ground the foundational analysis of measurement in general model theory. Suppes (in Morgenbesser 1967:58) points out that the essential characteristic of a theory of measurement is that it can study (in a precise way) the transformation or development of 'qualitative observations' into the 'quantitative assertions' characteristic of the more theoretical stages of the scientific process. He approaches this problem in terms of representation theorems, mainly because he views the models of the theory and the models of the data (see below) to be of different logical types:

> Given an axiomatised theory of measurement of some empirical quantity such as mass, distance, or force, the mathematical task is to prove a representation theorem for models of the theory which establishes, roughly speaking, that any empirical model is isomorphic to some numerical model of the theory. The existence of

> this isomorphism between models justifies the application of numbers to things. ... What we can do is to show that the structure of a set of phenomena under certain empirical operations is the same as the structure of some set of numbers under arithmetical operations and relations (Suppes in Morgenbesser 1967:58).[36]

Although I would read 'conceptual model' for his 'empirical model' and 'empirical model' for his 'numerical model', this is essentially my view of the 'verification' of the models of scientific theories too. In my approach it is however not necessary to use a separate language – from the one talking about the content of a theory's conceptual models – to talk about the empirical models of theories – although of course it can be done, and then Suppes's use of representation theorems will become applicable too.[37]

Suppes articulates a more complex stratified view of the relations between models of theories and systems in reality, than I do in this thesis. However, I too go to great lengths to point out the elaborate sophistication of the manoeuvres needed to find the possible links between particular systems in reality and certain models of a theory being examined at a given time. He (Suppes 1989: 25) wants to show that the study of the relations between (empirical) theories and their data demands a study in terms of a hierarchy of models of different logical type.[38] He sees the empirical relation between a conceptual model (of a given theory or class of systems) and a system in reality as a highly articulated, composite relation,[39] with an articulation which depends upon the experimental or observational situation in question.[40]

I am in complete agreement with this view, but for my (philosophical) purposes I collapse this complex relationship to a much simpler relation, indicated by 'empirical adequacy' (in my terms). This simple relation results in fitting the empirical data – however elaborately extracted from the physical system, and subsequently formulated conceptually, i.e. mathematically – into the relevant conceptual model of the theory in question (i.e. the relevant conceptualisation of the empirical data in question forms a substructure of the conceptual model in question). A simple example: Observations over time deliver 113 different spatial positions $(x_1, y_1), (x_2, y_2), ..., (x_{113}, y_{113})$ for the planet Neptune in the x-y-plane (of the planets in our solar system) of a coordinate system centred on the sun. These are the data. All 113 points *lie on* the (near-) ellipse with its uncountably many points which is the conceptual model of the orbit of Neptune (which in its turn is part of the conceptual model of the solar system, in which the – Newtonian or Einsteinian – theory of our solar system is true). What is suppressed or collapsed here is the process of distilling the data $(x_1, y_1), (x_2, y_2), ..., (x_{113}, y_{113})$, which is a process which involves theories, models, and practices relating to telescopes, the human eye and visual system, light, movement of the earth, clocks, and so on, and so on.

He (Suppes, 1989: 34) writes:

> One of the besetting sins of philosophy of science is to overly simplify the structure of science. ... What I have attempted to argue is that a whole hierarchy of models stands between the model of the basic theory and the complete experimental experience. Moreover, for each level of the hierarchy, there is a theory in its own right. Theory at one level is given empirical meaning by making formal connections with theory at a lower level.

I am in complete agreement thus far, but again, my version of a model-theoretic view of the structure of theories can fully accommodate this kind of hierarchy. There is a principle of transitivity at work here – accommodated by a model-theoretic account of empirical adequacy – that does the same philosophical work as Suppes's intricate comments on measurement theory and the role of representation theorems.

4. The structuralists

Stegmüller (1976, 1979) places the structuralist programme in the 'non-statement' tradition, because its main methodological principle is to view theories as *structures*, or sets of structures (in the standard set-theoretic sense) in the place of sets of statements. Structuralists typically depict a scientific theory as a '... *conceptual structure* that can generate a variety of empirical claims about a loosely specified, but not completely unspecified, *range of applications*' (Sneed 1976:120). This view, taken literally, is essentially what I hold too, except for the fact that a scientific theory to me still is a *linguistic* expression that may be *interpreted* by a set of (conceptual) structures (i.e. models in which the theory's sentences are true). I take these models (and not the theory itself) then to be the 'generators' of ' a variety of empirical claims' about a certain range of applications of the theory.

The structuralist programme dates from the early sixties and is a development of Patrick Suppes's view. The programme originally started (Sneed 1983:350) as an attempt to describe more precisely the empirical claims of theories with considerable mathematical apparatus. Since the early seventies Wolfgang Stegmüller, with his colleague Joseph Sneed, assisted by Wolfgang Balzer and also Ulises Moulines, started refining this approach. The structuralist approach is a meta-theoretical approach to scientific (empirical) theories that essentially focuses on the nature of scientific theories, interrelations between theories (especially reduction, equivalence, and approximation), and theory progress or evolution.

The basic elements of theory identification as set out by the structuralist programme may be summarised as follows (note that structuralists claim that all these components can be precisely explained in purely structural – i.e. set-theoretical – terms). 'Theories' are taken to consist of (classes of) models in the Tarskian sense of formal semantics, i.e. a model of a theory T is a possible realisation in which all the sentences (or at least a set of given axioms) of that theory are satisfied.[41] The identity of a 'theory' is first and foremost given by a class of models, which we may call *M* (following Stegmüller and his colleagues).[42] The models are determined by a given set of axioms (the 'tautologies' of the theory), but the structuralists claim these axioms to be secondary to the determination of the identity of a theory, since any set of axioms may be chosen just as long as it is satisfied by the same set of models, *M*.

The structuralists try to 'fit' models to theories, since they do not acknowledge the theory as a linguistic entity to start off with. A defender of a model-theoretic account of science works towards the formulation of a theory as a linguistic expression, and then, in applying the theory, starts off with the theory as a linguistic entity and tries to construct models in which that particular theory will be true.

However, although it is true that in the structuralist view the specific set of axioms in question does not really play a primary role in the identification of the theory, distinguishing between two different kinds of axiom does. In the literature, these two types of axiom are usually referred to (Moulines in Schurz & Dorn 1991:317) as *framework conditions,*[43] which are mainly the accepted body of theories, or background knowledge, or paradigm within which scientists work, and '*proper*' *axioms* which are taken to be substantial empirical laws. In the example of Newtonian mechanics, Newton's Second Law is a 'proper' axiom, while the (implicit) condition of the differentiability of the position function would be a framework condition. The framework conditions define the basic notions about the structure of each of the fundamental notions of the theory, i.e. the 'base set' (Balzer, Moulines, Sneed 1987: 5) of the theory; while the 'proper' axioms state the law-like relations between these basic notions, i.e. 'what conditions have to be satisfied for a possible candidate to really be a structure of this kind?'. The latter are the 'fundamental laws' (Balzer, Moulines, Sneed 1987: 19) that 'connect' all the terms of a theory in one 'big formula'.

Structures determined only by framework conditions are called *potential models*, the class of which may be denoted by M_p, or $M_p(T)$, while structures fulfilling both the framework conditions and proper axioms of a given theory, are called *actual models*, the class of which may be denoted by the familiar M, or $M(T)$. Obviously $M(T)$ is a subset of $M_p(T)$ – so that $M \subseteq MP$. Methodologically speaking, clear distinction between these two classes of models is necessary in any logical reconstruction of a theory.[44] Thus the set-theoretic predicates determining the set M_p(T) are defined by statements about the set-theoretic properties of the base set of the theory, and typifications and characterisations of basic relations. The determination of M(T), on the other hand, relies on the specification of the laws (axioms) identified in the theory as well.[45]

Also part of the determination of the identity of a theory is a distinction between the theoretical and the non-theoretical terms of the same theory, the interrelationships ('constraints') between models of the same theory, as well as the intertheoretical links between models of different theories (concerning different sets of potential models).

Advocates of the structuralist programme take $<M_p, M> = K$ (Moulines in Schurz & Dorn 1991:319, and Balzer, Moulines, Sneed 1987: 36ff.) to be the (conceptual) 'theory-core' of a particular theory. The core K plus the class of intended applications, call it I, form the simplest set-theoretic structure that may serve as a logical reconstruction of an empirical theory. K and I are called theory elements. K is a purely formal mathematical structure and it says 'something' or is 'about' the class of intended applications. More complex theories are 'built' of theory-elements that are linked or related in certain ways.

The class of partial potential models M_{pp} is a class of mathematical structures that consists of parts of the potential models that can be interpreted independently of the theory in question.[46] These partial potential models are characterised in terms of a *theory-relevant* theoretic/nontheoretic distinction among the components of the class of potential models. An empirical claim is associated with a particular theory element in terms of the part of the content of that theory element which forms the class of partial potential models that is 'compatible' with the laws, constraints, and intertheoretic links associated with the particular theory element in question. This claim is simply the claim that the particular intended application of the theory in question is in K.

Sneed (1976: 124)[47] claims that a theory-element core is used to make empirical claims in the sense that a subset of M_{pp}, call it $A(K)$ (ibid.), is selected such that theoretical elements can be added to each of its members in such a way that it yields a subset of the set of actual models M (this means that each member of the subset of M_{pp} will satisfy the theoretical laws of the theory).[48] The empirical claim that the particular theory element is thus making, is that descriptions of phenomena that actually occur is indeed a part of the theory core. In other words, if we have a theory-element $E = <K, I>$, where K is the elaborated theory core above, and I remains the set of intended applications, then the claim that E is making is that I is an element of the subset $A(K)$ of M_{pp}. That means that the theory-element core K narrows down the set M_{pp} to the subset $A(K)$, thus restricting the possible models of the theory (containing only non-theoretical components) such that the result is I. This 'narrowing down' is done via constraints and intertheoretical links.[49] Balzer, Moulines, and Sneed (1987: 87) point out that the assumption that the intended applications of T have the structure of its partial potential models is the 'most economical and most natural' assumption to make. Thus we should assume that $I(\mathrm{T}) \subseteq M_{pp}(\mathrm{T})$. There will, of course, still be unwanted applications, even if we take M_{pp} as the set of all possible applications of the theory, but these gentlemen (1987: 87) claim it is enough that '[w]e can say something precise [after all], namely that an intended application is a partial potential model, but we cannot be precise about every feature of intended applications'.[50]

In model-theoretic terms the notions of inter-theoretical links and constraints are mostly addressed in terms of underdetermination via the various models of a particular theory. The 'narrowing down' of the set of applications of a theory is best done by amending the axioms of the theory itself, although the various relations possible between theories and models of the same theory as well as models of other, possibly related, theories also may be applied in this sense. As far as the last remark above is concerned, also in a model-theoretic account of science is it not possible to depict all the possible (intended) applications of a specific theory, as has already been pointed out often.

5. The 'semantic approach' in the non-statement tradition

5.1 Beth, Van Fraassen, and Suppe

Bas Van Fraassen developed a semantic approach to philosophy of science by building on the work of Evert Beth,[51] in which physical systems are depicted in terms of their possible states. This position was further developed by Frederick Suppe.[52] The foundational claim of this approach is that any scientific (physical) theory is taken – by scientists themselves – to have many alternative linguistic formulations. (Think of the Lagrangian and Hamiltonian formulations of classical particle mechanics.) Theories thus cannot be identified with their linguistic formulations. Suppe (1973:130) claims that it is rather the case that '... scientific theories are extra-linguistic entities which are referred to and described by their various linguistic formulations ... [thus] theories are to be constructed as abstract *structures* which serve as models for the sets of interpreted sentences [that] constitute their linguistic formulations (i.e. that they are meta-mathematical models of their linguistic formulations), where the same structure (theory) may be the model for a number of different, and possibly inequivalent, sets of sentences or linguistic formulations of the theory'.[53] Here, then, is a more radical approach than either that of Suppes or the structuralist programme in the sense that the theory is *identified*, as it were, with the notion of model. A theory is a model to the defenders of this approach, they do not merely talk about discarding the linguistic features of theories in logical reconstructions, they claim a theory to be 'extra-linguistic'.

Beth[54] developed what is referred to as a 'state-spaces' view (related to the older phase and configuration space view of mechanics and that of Von Neumann (1955) for quantum mechanics) in three articles (1948/49), (1949), and (1961). Van Fraassen (1970:327), following Beth, believes that the meaning structure of a certain part of natural language becomes suitable for a technical role in some scientific language if it has a representation in terms of a model, in the sense of a mathematical structure. Then the scientific language can be formally reconstructed as an artificial language whose semantics is determined with reference to this mathematical structure or model. Such a language Van Fraassen (1967) calls a 'partial or semi-interpreted language'.[55] Note that the 'meaning structure' of a part of natural language that may be represented by some mathematical model may here then be described in some appropriate formal language. In a model-theoretic approach the semantic content of the linguistic expression of the theory in some appropriate formal language is determined by the initial conceptual model, but also, the linguistic theoretical expression is then interpreted by other conceptual models during the application stages of the theory,[56] which implies nothing more than that the 'meaning structure' of the linguistic expression is then represented yet again by other (or the same) mathematical structures. However, this is not what Van Fraassen and Beth really claim. To them the model is the mechanism that may determine the semantics of the formal language in which the theory may be formulated, as are my initial conceptual models. However the theory itself remains a non-linguistic entity, since nowhere do they mention the possible interpretation of the theory in terms of other mathematical structures in the model-theoretic sense.[57]

Now, in Beth's approach, the notions of a 'physical system' of a theory and the various 'states' in which this system can be at given times, are foundational. These notions are however easier to understand if some of Suppe's notions are introduced first. The specific class of phenomena that the (linguistic) formulation of a theory is meant to characterise, is called the *intended scope of the theory*. Theories do not characterise these phenomena in their complexity, though. Suppe (1973: 131) gives as illustration the fact that classical particle mechanics characterises mechanical phenomena as if they depend only on the abstracted position and momentum parameters, while actually various other 'unselected' parameters usually also influence the phenomena. Thus a theory's characterisation describes what the relevant phenomena *would have been like had* the abstracted parameters – those the theory's formulation focuses on for whatever reason – been the only parameters influencing them. This is essentially what happens both in my original intended models and in the later conceptual models of a given scientific theory. More about this a little later in this section.

In this sense, theories may be said to characterise physical systems, because they are about the behaviour of certain abstract systems in the sense that this behaviour is dependent only on the parameters selected by the theory. Physical systems are relational systems whose domains consist of states, and relations and laws ranging over these states.[58] Van Fraassen (1970: 330) states that the function of a law in Beth's approach is to describe the behaviour of the physical system with which the theory is occupied at the time in terms of its possible states, its normal evolution through time, and its behaviour in interaction with other factors.[59] These laws thus indicate which states are physically possible for the various physical systems; and they also determine which combinations of states are so-called theory induced physical systems (notion explained below) and which are not. 'Thus the relations of the theory determine all and only those sequences which are the behaviours of physical systems in the class of theory induced physical systems' (Suppe 1973: 133).

The selected parameters abstracted from the phenomena can wholly describe the behaviour of physical systems, and so they are called the *defining parameters of the physical system*. The values of these parameters are physical quantities which may be determinate or statistical (Suppe 1973: 131). A set of simultaneous values for the parameters of a physical system is a *possible state* of the system. Note that any physical system is at any time in exactly one of its possible states, although that state may change over time. The behaviour of a physical system is given in terms of these state changes. In this way, the behaviour of a system is the system's history and each physical system has a unique sequence of states (in the deterministic case) or a set of possible sequences of states with associated probabilities (in the statistical case) that it assumes over time. Each physical system is characterised fully by a specification of the possible states it can assume and the sequences of states it assumes over time.

The class of *causally possible physical systems* for a theory is the class of physical systems which correspond in the following way to causally possible phenomena P, within the theory's intended scope: any P in the theory's intended scope corresponds to a (causally) possible system P' such that P' is what any causally possible phenomenon P would have been were the idealised conditions imposed by the theory met and the phenomenon P influenced only by the selected parameters. Obviously then, one of the tasks of a theory is to describe the class of causally possible physical systems for the associated theory. This is done by the theory describing a class of physical systems known as the *theory induced class of physical systems*, such that this class is identical to the class of causally possible physical systems.

A theory then is *empirically true* if the theory induced class of physical systems and the class of causally possible physical systems for the theory are indeed identical. Testing of theories involves determining whether this identity in fact exists between these two types of classes of systems and is usually done in statistical terms.

As far as the semantic content of the theory formulation[60] is concerned, Van Fraassen (1970:328) writes that the 'set of *states* of some physical system are represented by elements of a certain mathematical space, called the *state-space*.[61] Apart from the state-space, these theories use a certain set of parameters – referred to in the above – to characterise the particular physical system. This yields the theory's set of elementary statements about the system in question. These are initial and boundary conditions which are part of the relations determining empirical adequacy in my terms. These statements are such (ibid.) that each elementary statement U formulates a proposition to the effect that a certain physical magnitude m has a certain value r at a certain time t.[62] The truth of such a proposition U always depends on (or is relative to) the particular state of the system at that time – in some states m will have the value r and in some states it will not have that value.[63]

A 'satisfaction function' determines whether the system's actual state is represented by an element of the mathematical structure consisting of all the relevant state-spaces of the theory or not: 'The mapping h [of the theory] (the *satisfaction function*) ... connects the state-spaces with the elementary statements, and hence, the mathematical model provided by the theory with empirical measurement results.[64] ... The exact relation between ... [elementary statement U] and the outcome of an actual experiment is the subject of an auxiliary theory of measurement, of which the notion of 'correspondence rule' gives only the shallowest characterisation" (Van Fraassen, 1970:329).[65] A description of a set of state-spaces plus the satisfaction function are thus offered in the place of the statement approach's axioms or postulates concerning the 'primitive' symbols of the scientific language.[66] I agree, although I offer the entire model-theoretic stratified process of science in the place of these postulates.

Now if a physical system is in the class of theory induced systems, then the domain of the physical system will be a subset of the domain of the theory and the sequence of states of that system will be one determined by the theory's relations (laws). These physical systems are meant to be *replicas* of the actual systems in reality, and so by describing the physical systems the theory '... indirectly gives a counterfactual characterisation of the actual phenomena' (Suppe 1973: 131). Also, it may happen that theories give an *idealisation* of some physical system – Suppe's (1973: 131) illustration again is from classical particle mechanics. These kinds of systems are 'isolated systems with dimensionless point masses interacting in a vacuum'. Such idealised physical systems are still abstract replicas of phenomena, but with the additional feature that certain idealised conditions (such as being isolated systems of dimensionless point masses) are imposed on these systems *which actual phenomena can never actually meet*. Thus (Suppe 1973: 139) '[o]nly some of the propositions which are true of the theory will be true of a particular physical system in the class of theory-induced systems, but every proposition true of a physical system in that class will be true of the theory'. If a theory is empirically true, the semantic relations holding between propositions in the theory-formulation language and the class of causally possible physical systems for the theory will be exactly the same as those holding for the theory-formulation language and the class of theory-induced physical systems. Moreover, every proposition in the theory-formulation language which is true of a causally possible physical system will be true of the theory.

A few remarks on this semantic approach to scientific theories in terms of a model-theoretic account of these matters. In model-theoretic terms the intended scope of a theory is indeed the 'class of phenomena' the (formulation of) the theory is meant to characterise. It is simply the case that within the latter kind of account we usually speak of real systems rather than phenomena or classes of phenomena. Also, in the latter approach, a theory can do no more (as Nancy Cartwright so delights in pointing out) than characterise the real system it means to describe as it would have been had the abstracted parameters of the theory been the only ones influencing the system in question.

Each possible state of a physical system – in Suppe's terms – might be viewed as a conceptual model in model-theoretic terms, seeing that a possible state of a system is given in terms of a simultaneous set of values for the parameters of the theory in question. The theory-induced physical systems would then perhaps best be viewed in terms of the empirical models of a model-theoretic account. The reason for this is that the class of causally possible systems turns out to be systems in which the idealised conditions set by the theory have been realised, influenced only by the selected parameters of the theory, and that a theory is said to be empirically true in Suppe's terms if this class of systems is identical to the class of theory-induced physical systems. The relation of empirical adequacy between conceptual and empirical models of some theory in model-theoretic terms is then very close to this relation of identity between causally possible physical systems and theory-induced physical systems. This becomes even clearer if we take into account that the 'satisfaction function' of Suppe's semantic framework determines whether the actual state – i.e. the causally possible state – of a physical system is represented by the mathematical structure representing the theory-induced physical systems.

5.2 Ronald Giere's 'understanding of science'

Giere (1985: 75) agrees with Van Fraassen that the logical positivists' (statement view) pre-occupation with the linguistic structure of scientific theories obscures the important role models in which those theories are true have to play in the scientific process. He, however, does not waste much time in pursuing any of the semantic categories of reference and meaning in the way Van Fraassen does (via his notion of elementary statements yielding semi-interpreted languages). He (Giere 1985: 77) states clearly that he '... will simply ignore such issues. ... the theory of science need not wait on the development of adequate general theories of meaning and reference to proceed. We need not know in detail *how* general terms such as *mass* come to be associated with terms in an abstract mathematical structure. We know *that* it can be done because it *is* done'. This is close to what I have been implying all along. I have stressed that, because of the complex and changeable nature of these issues, questions concerning experimental design, measurement theories, and criteria determining the 'fit' of some model to a system in reality, are best left to science itself to answer. I do however think philosophy of science has *something* to say about these issues, at least as far as showing their place in the structure of science as a whole (i.e. in the relations between the possible empirical and conceptual models of some theory), and their implications for the structure of scientific theories in particular (i.e. their possible reference to real systems).

Giere sees himself as a supporter of the semantic view of theories, preferring Beth's state-space approach to Suppes' set-theoretic one. In general, the structure of a theory consists, according to Giere (1994: 277), of a 'family' of models or predicates, where the linguistic structure corresponding to a predicate is a definition instead of an axiomatic system (as in the traditional statement approach). Giere addresses these issues in terms of 'theoretical models' (the models (or set of models) created by defining a certain real system), and 'theoretical hypotheses' which are statements picking out similarities between theoretical models and some system in reality.[67]

He (Giere 1983: 271) makes it clear from the start that theoretical models (as definitions of systems in reality) have no empirical content. They may, however be used to make claims about reality via the theoretical hypotheses that identify elements of some theoretical model with elements of real systems and then claim that the real system exhibits the structure of the model in question. Giere (1984: 11) acknowledges the idealised nature of models by stating (Giere 1985: 79) that a theoretical model is not 'a faithful replica in all detail' of the object modelled. He (Giere 1984:12) goes on to explain that theoretical models also come in various degrees of specificity, but points out that no such thing as a maximally specific model exists, this kind of model is always relative in the sense that it is a model of a designated type. Giere (1984: 12) also refers to the underdetermination of a theory by its models: 'There are many ways of filling in a highly nonspecific model to achieve a highly specific version of that model. The relationship between non-specific and specific models, therefore, should not be confused with the relationship between general and particular, as in the relationship between general laws and particular instances'.[68]

In Giere (1983: 272), he stresses again that a theory is not simply a general model. He blames scientists for thinking that theories have empirical content and accuses them of using the term 'theory' to refer to a 'more or less' generalised theoretical hypothesis asserting that one or more specified kinds of system fit a given type of model. He (1985: 78) accuses the logical positivists of conflating two separate functions of a theory, namely to offer general interpretations of theoretical terms such as 'mass'; and to provide the means of identifying particular instances of these terms. Van Fraassen (1980) disagrees with the claim that the latter is a matter for philosophical analysis, and I agree with him, as pointed out already.

Giere, however, wants to study 'how we as human beings use abstract models in describing particular objects in the real world' – which reminds one of Wartofski's approach.[69] He does this not by means of that favourite realist notion of approximate truth, nor in terms of Van Fraassen's (1980: 9, 45ff.) notion of approximation in the sense of one model of a class of models fitting the real system, but rather by means of a particular notion of 'similarity'. Giere (1985: 80) claims that theoretical hypotheses assert that '[t]he designated real system is similar to the proposed model in specified respects and to specified degrees'.[70] He adds that the precision associated with any hypothesis is always less than or at most equal to the precision of the measurement techniques employed at the time.[71] Giere (1983: 269) thus finds the rationality of science in the testing of 'highly specified theoretical models against empirical data'.[72]

5.3 Wójcicki's empiricist semantics of science

Wójcicki (in Humphreys 1994:125) argues that the coordination of set-theoretic definitions of theories such that the statements of scientific theories can be related to empirical hypotheses (i.e. theoretical hypotheses 'decided' by observation) is an extra-theoretical issue. Philosophers of science should therefore not attempt to address this issue within their analyses of the structure of science by notions such as the structuralist notion of 'intended applications'. Wójcicki thus replaces 'observability' with 'empirical decidability'. I agree in the sense that philosophy of science is concerned with matters of empirical decidability, rather than with observations themselves, and also wish to point again to the overlap with and elaboration of Van Fraassen's constructive empiricism here.[73]

When turning to the nature of empirical interpretations, Wójcicki (in Humphreys 1994:131) also refers to the underdetermination of theories by data: '[n]o scientific theory is just true about the empirical systems to which it applies. ... Empirical systems are, to appeal to a Peircean metaphor, "nebular". They are never fully separable from their environments; moreover they consist of objects whose properties are not uniquely determined and may not be exactly such as is required by the theory. Thereby, if a theory applies to any such system it applies to it in an "approximate" way'. However, as a result of Wójcicki's model-theoretic sentiments, he (Wójcicki in Humphreys 1994:132) stresses – much in accordance with my own approach[74] – that it is not so much the issue of approximation and its determination that is important, or the related fact that '... scientific theories dramatically fail to be totally true of the states of affairs to which they refer' that should drive one's notion of truth. Rather, he views the implications of a model-theoretic approach to imply that theories are 'partially' or 'relatively' true, in the sense of being true 'in certain selected respects'. And it is such an approach, he – rightly – claims (ibid.) that can indeed account for the ways in which scientific theories refer to real phenomena.

He (Wójcicki in Humphreys 1994:133) sets out his approach as follows: 'An application of an empirical theory T, by itself or combined with some auxiliary hypotheses, to an empirical phenomenon Π in order to solve a specific problem Q concerning Π may require the formation of a *theoretical model*[75] (one may prefer to say *mathematical model*) of the phenomenon...'. This implies defining an abstract system M meant to satisfy the following conditions: M is a realisation of T, and M is a faithful representation of Π under all the respects that are relevant to Q.

Thus, if A is the solution to Q, then A is factually true, i.e. true of Π if and only if A is true of M. A theoretical model of Π is a model of an aspect Π/Q of the phenomenon Π – each other problem connected to Π may lead to the formulation of a new model M′.[76] This notion of a theoretical model is very close to my notion of an intended model. The 'truth' of the solution depends on the accuracy and reliability of the steps resulting in the solution offered by theoretical model M, and is a matter of statistical analysis of all the steps of the procedure.[77] In order for the model's equations to have a unique solution, Wójcicki requires that the values of the parameters of the equations should either be established experimentally or be deduced from the available experimental data with the help of hypotheses we consider to be confirmed, i.e. the laws of the theory. This points towards an assumption of the same kind of hierarchy of models as Suppes's.[78]

I should mention that Wójcicki (in Humphreys 1994:138) however does not view theoretical models as 'the right candidates for the role of intended applications in the Adams-Sneed sense'. According to him the problem lies in their identity as realisations of the theories which are intended to refer to certain empirical systems.[79] He (ibid.) allows that, even though theoretical models are not empirical systems, they can be viewed as some idealised representations of these systems. Why then does he not allow for the kind of transitive reference or interpretation relation I argue exists, with the theoretical models mediating between their theories and empirical systems? If (theoretical) models may both be realisations of theories and representations of empirical systems, why can they not serve as a semantic link between theories and empirical systems? His acknowledgement (in Humphreys 1994: 138) of the fact that a structuralist intended application may indeed be a structure which may turn out to be a realisation of the theory in question, simply makes his comments on this issue more puzzling. He (Wójcicki 1979: 158), however, views the notion of 'semantic model' as synonymous to that of 'realisation', so that it does seem as if the problem here is distinguishing between the use of the various terms in certain contexts, especially given that he continues to state that '[c]learly, the same system may happen to be a model for an instance of an empirical phenomenon [my intended and his theoretical model] and at the same time a model for a set of laws [my conceptual model interpreting the theory and his semantic model] which describe the behaviour of that phenomenon' (Wójcicki' 1979: 158).

The problem might lie in the fact that Suppes and a few others want to distinguish between the 'theoretical' model and a 'semantic' one in a fundamental way, and perhaps Wójcicki simply does not point out clearly enough that someone like Suppes does not assume the two roles that a mathematical model can play in the process of science in the way we do. In this sense, Suppes (1960: 291) claims the set-theoretical model – Wójcicki's semantic models and my conceptual models – to be more fundamental than the 'physical' one – in Wójcicki's terms, the theoretical one, and my intended model. I fail to see why the same kind of mathematical model might not be seen as playing both roles at different times of the scientific process.[80]

Suppes seems to reduce this interplay to interplay between the semantic model and the theory, which is acceptable as long as one accepts that this 'semantic' model may sometimes play the role of Wójcicki's 'theoretical' model. Thus, neither is Wójcicki nor am I introducing theoretical models as a separate class of formal models opposed to semantic models – we rather emphasise that the important thing is the stage at which one is creating a model: either 'as a method for finding a new theory [theoretical model] or as a test of a given theory [semantic model]' (Wójcicki in Humphreys, 1994: 146).[81]

Wójcicki (1979: 158–160) also points out that the semantic use of the term 'model' is often confused with the use of the term as a set of mathematical equations describing some kind of regularity in the behaviour of a particular phenomenon. I agree with him that this should be avoided and that the latter use should rather be changed such that it is taken that the meaning of 'model' in that sense is 'theory'. Wójcicki (1979: 160) remarks that '[i]t seems more reasonable then, when speaking about mathematical models, to speak about mathematical entities the equations define [and] which serve as abstract representations of the phenomena examined rather than about the equations themselves'. This seems to imply that the sets of equations should be referred to as the 'theory'.[82]

6. Nancy Cartwright's version of the semantic approach

What – if anything – do abstract claims have to say about the real world? What kind of image of the world can possibly 'correspond' to our abstract theories? Can scientific theories be 'true'? Do scientific theories 'explain' aspects of reality or do they 'describe' the behaviour of real objects? Nancy Cartwright is one of the most influential philosophers currently writing on the role of models in the process of science. She does not fit into the semantic approach[83] since she acknowledges the role of scientific theories although mostly in terms of the fundamental laws stated by sets of equations. However she does ascribe some role to models interpreting these fundamental laws and should therefore be mentioned here.

Cartwright's main claim is that scientific theories (or rather, the 'fundamental laws' which are part of the theories' content) have, since they are valid only under certain circumstances or given certain conditions that do not strictly hold in reality, absolutely nothing to say about reality. She (Cartwright, 1983) describes these laws as 'general, abstract equations; ... not about any particular happenings in any particular circumstances.'[84]

She distinguishes between *phenomenological* and *fundamental* laws. Phenomenological laws are complex descriptions of actual situations in very specific terms – what can be confirmed through tests and comparisons with observations are phenomenological laws – comparatively detailed descriptions of concrete situations, which because of their richness in detail, do not have great generality. However, it is important to understand that Cartwright, by referring to phenomenological laws as low-level generalisations, means to say that they too, have an abstract nature in the sense of being *idealised* descriptions of real objects in reality. So, then – because fundamental laws can supposedly do no more than explain the content of phenomenological laws, and good explanations are supposed to be simple (abstract) and general – fundamental laws can indeed never *directly* be about any *particular* aspect of reality. Moreover, as Chalmers (1993:199) also points out, quite in accordance with the fact that phenomenological laws too, are generalisations that involve *ceteris paribus* conditions, these laws themselves sometimes fail to adequately describe the behaviour of real objects. (Think, in this regard, most simply of Cartwright's (1983: 55ff.) example of the instructor's manual of lasers – surely lasers can malfunction?) Cartwright (1983:129) claims that it is phenomenological laws that fulfil the 'traditional role' of laws in the sense that they describe empirical regularities – which fundamental laws cannot do because they cannot account for the actually observed variety in the behaviour of objects in reality, because they are too general and much too simple. Fundamental laws do not have anything to say about 'regularities' (constant conjunctions of events in Humean terms), because describing regular behaviour requires more and more complicated descriptions of the situation.

Cartwright quotes (1983:55ff.) the universal law of gravitation, Schrödinger's equation, and Maxwell's equations as further examples of fundamental laws, and gives (1983:2) Airy's law of Faraday's magneto-optical effect as an example of a phenomenological law, because Airy's law does not *explain* Faraday's law (in the way that the more theoretical treatment of it in terms of electron theory by Lorentz does), but rather *describes* the actual changes in Faraday's dense borosilicate glass as magnetic fields rotate the plane of polarisation of light (while Lorentz's formulation of the Faraday effect appeals to the electron theory, and so has an underlying explanatory content to it, that Airy's law does not have).

Claims like the following about fundamental laws worry me however – '... fundamental laws ... do not hold for the most part, or even approximately for the most part, and conversely, those laws which are more or less true much of the time are not fundamental'

(1989b:174) – not necessarily because what she says about the nature of these laws is untrue, but rather because her claim seems to imply that she still believes in some absolute notion of truth. She stresses that fundamental laws can – at most possibly – explain the content of phenomenological laws by organising or classifying them, and that fundamental laws therefore, do not describe the behaviour of real objects in the world. However, as Rueger and Sharp (1996:95) point out, she (Cartwright, 1983, 1989b) acknowledges that fundamental laws are at least useful to her even though they are not 'true descriptions' of real objects or their behaviour, because they organise and classify knowledge, much as Duhem (1914) sets out. She creates the impression in *How the laws of physics lie* (1983) though, that she views these organising features as a particular weakness of these kinds of law, because she puts so much emphasis on the fact that 'the cost of explanatory power is descriptive adequacy' (1983:3).

In my version of the scientific process, however, that is no problem, and, I might add, neither should it be in hers, because we both accept and acknowledge from the outset that truth is a very local and limited notion, albeit in a more complex way than is ordinarily thought. In other words the fact that she denies that 'explanation is a guide to truth' (1983:5), surely is only problematic if one thinks of truth as a universal notion. She does, in a sense, make amends in *Nature's capacities and their measurement* (1989b), as well as specifically stressing pretty clearly in her article entitled *Fables and models* (1986), the fact that questions of truth are not necessarily questions of universality.

But how then does Cartwright conceive of relating fundamental with phenomenological laws and either (or any) of these sets with real objects? The 'content' of fundamental laws is filled in by various abstract models.[85] In Cartwright's (1989:4) scheme of things, these models mediate between theories and fundamental laws on the one hand, and phenomenological laws and reality on the other. According to the model-theoretic interpretation of the process of science that I am offering, they simply mediate between theories and (empirical models of) systems in reality. Phenomenological laws, in my terms, would simply be part of the content (or properties) of the models interpreting scientific theories, and they would be expressible as sentences true in the model(s) under consideration – as well as possibly true in some real system.[86]

Cartwright worries too much about the ideal character of the models and the idealising role of the *ceteris paribus* clauses needed by models to connect fundamental laws to phenomenological laws. The view that portrays these conditions as some kind of ingenious device cunningly designed by naive realists or staunch fundamentalists to 'save theories from point-to-point testing' (Rueger & Sharp 1996: 103)[87] is completely misguided. First, however, as far as bridge principles are concerned – they do not hold *ceteris paribus*. There is no absolute set of rules describing these kinds of correspondence relation. Rather these rules hold with respect to a certain model within whose boundaries the theory is true. The best that can be said about 'bridge principles' in my terms is that perhaps one can speak of a set of bridging 'procedures' or 'links' that extracts data from the relevant real system relative to a specific empirical context, and then injects these data, as an empirical model into the model under consideration.

The theory holds *ceteris paribus* yes, but not in Cartwright's sense of the word. In my terms, to say that a theory 'holds' means, per definition, that it holds (is 'true') in a particular one of its models. To say now that it holds '*ceteris paribus*' adds *nothing* to simply saying that it is true. Moreover, there *is* nothing else about which it can be stipulated that it stays the same – *everything* is given in the model. *Ceteris paribus* clauses seem in Cartwright's terms to play a more and more important role the further away one moves from fundamental laws. In model-theoretic terms, however, they are necessary only at the level of scientific theories or linguistic systems, and become less and less active the closer to reality one moves. I claim – see also Section 2 – that they are *suspended* in their generality as soon as the theory in question is interpreted in specific models, rather than *activated*. The idealised nature of conceptual – and even empirical models – is not the result of specific *ceteris paribus* clauses, but indeed simply true to the nature of science. No real system can ever be examined, represented, explained, or described in its full complexity. That is simply not science's function.

Following Duhem (Duhem 1914: 7), Cartwright (1983: 96) considers the notion of scientific explanation in terms of description in the sense that explaining a set of phenomena means giving a physical theory of them, 'a physical theory in Duhem's sense, one that summarises ... and logically classifies them' (Cartwright 1983: 96). Rueger and Sharp (1996: 95) refer to the problem she has with the covering law account of explanation as the 'unsoundness argument' and sets it out as follows: 'If ... phenomenological laws could be *soundly* derived from more fundamental laws as the traditional [covering law] view would have it, then any successful comparison of the phenomenological consequences of the theory with the observations would count unproblematically as inductive support for the theory. Confirmation would flow upwards from the phenomenological level to the fundamental level. This flow, however, is staunched ... because phenomenological laws typically cannot be soundly deduced from more fundamental theories. To derive the former we usually need assumptions [*ceteris paribus* clauses] which are either false (distorted representations of the situation of application) or which contradict the fundamental laws themselves. Inductive support cannot, therefore, be transmitted.'

Claiming that phenomenological laws cannot 'typically' be deduced from fundamental ones, is perhaps jumping the gun a bit. Is it not the case that Kepler's laws can be deduced from Newton's in a very sound way? Moreover the *ceteris paribus* clauses and other additional assumptions needed to validate the fundamental laws are suspended when models are constructed of some theory – as remarked above – and thus these clauses become more and more concretely realised as they set the boundaries for the truth of the theory, i.e. the clauses themselves (e.g. 'no other forces act differentially on components of the system') become realised, i.e. true in the relevant models. Thus it is rather unclear how they can be understood to 'contradict' the fundamental laws themselves (which are also true in these models).[88] These *ceteris paribus* conditions or clauses will usually be incorporated into the formulation of the law explicitly (as when stating that Hooke's law holds as long as the elastic limit has not been exceeded), or else implicitly and tacitly by common understanding.

So, it seems that to Cartwright, in order to fit some phenomenon into the mathematical framework of some theory, a model of that phenomenon 'which re-describes it in terms which are amendable to mathematical theoretical treatment" (Chalmers 1993: 200) has to be constructed. 'Before we can apply the abstract concepts of basic theory – assign a quantum field, a tensor, a Hamiltonian, ... or write down a force function – we must first produce a model of the situation in terms the theory can handle' (Cartwright 1994b: 282). I do agree with this, but not with Cartwright's conclusion that models are thus (in her terms) merely devices used to fit phenomena into the theoretical frameworks of theories which in their turn classify and organise groups of phenomena. – '... the point of the kind of models I'm interested in is to bring the phenomenon under the equations of the theory' (Cartwright 1983: 157). This is not the *only* role models have. They do have this function, but it is *by virtue of* this very function that they offer ways to link the theory with aspects of reality.

Phenomenological laws describe actual events, because although they are usually mathematically formulated in physics, no fundamental explanation of the mathematical formulae nor of the mechanisms underlying these formulae are assumed in these laws.[89] The situations described by phenomenological laws are real insofar these laws are 'adequate' (true) or insofar as they 'fit' some actual situation – regardless of whether or not theoretical entities are involved.[90] It is however difficult to see how the very specific link with reality can be given by a law, even if it is a phenomenological one. For example, if Newton's laws are fundamental, Kepler's are phenomenological (and deducible from Newton's), but the direct observations (done in both cases) are specific activities (looking in a particular precise direction) carried out at specific times (specific to the second). Statements describing these kinds of activity surely are not laws.

Cartwright is very ambiguous in her statements about the links between theory and reality, though I find nothing wrong with the following remark (Cartwright 1989: 211): 'I think we cannot understand what theory says about the world nor how it bears on it until we have come to understand the various kinds of abstraction that occur in science, as well as the converse processes of concretisation that tie these to real material objects.' I agree that the context-dependence (or ideal character) of the (abstracted) factors 'added back' (after the suspension of *ceteris paribus* clauses) when the model is constructed should be taken into consideration, but, in my account, this is done by linking the empirical adequacy of the theory with the particular conceptual and empirical model, and not by denying the possibility of any relation between theory and reality.

Cartwright's notions concerning *ceteris paribus* clauses seem to be at the basis of a lot of the aspects of her work that are unacceptable to me. These notions are exemplified by her treatment of Newton's gravitational law: she enfeebles the absolutely general law by rendering it as if it were saying that (as far as we know) the gravitational force only kicks in when no other forces are around; and she ignores the fact that we know how to add forces and, more generally, in many cases how to merge what different theories are saying about different aspects of the same type of system. Apart from this misguided rendering of fundamental laws, she also misinterprets the reason for the idealised nature of conceptual models as the result of *ceteris paribus* clauses' stabilising influence. As I have pointed out before, the idealised nature of these models is far rather the consequence of the formal aspects of the truth (satisfaction) relations between theories and their models. The focused nature of science has nothing to do with some vicious influence that these clauses have on the enterprises of science, but is simply part of the definition of scientific knowledge.

Another of the main shortcomings of her work lies in the fact that she rarely seems to take the stages of theory formulation into account, while model-theoretic realism is based on exactly that assumption. If the structure of scientific theories is analysed both as far as theoretical formulation and application are concerned, the model-theoretic nature of the act of securing some referential link between some theory's terms and the entities and objects of some system in reality becomes clear and obvious. Moreover science does not aim – as Cartwright seems to think – at offering complete descriptions of particular concrete real systems in all their detail. Even if some system could be isolated – which it cannot be – no scientist is ever interested in *all* its aspects!

A last untenable aspect of Cartwright's views lies in her strong metaphysical leanings as far as proving the referential nature of fundamental laws is concerned. According to her, only if it is taken that fundamental laws refer to capacities of Nature, i.e. that these laws are capacity claims, can they be about something in reality. Model-theoretically speaking, the specific ontological nature of that about which fundamental laws are, is not only not necessarily causal or couched in terms of capacities, but basically irrelevant.

7. Conclusion

The different relations between theories, models, and systems in reality offered by the various non-statement approaches to the scientific process all offer – among other things – variations on the theme of scientific realism. The model-theoretic tools these views are equipped with seem to offer a very good chance of, on the one hand showing that there are, indeed, such relations, and on the other hand, to define the nature of these relations more precisely than before. A model-theoretic rather than a non-statement (or semantic) approach promises the most at this point since it not only 'speaks about' the relations of the models (of some theory) to reality (or rather physical systems in reality), but also about the relations between these models and the theory itself. In this way a truly model-*and*-theoretic realism can be achieved. The non-statement elimination of the theories as linguistic expressions actually do away with half of the realism issue. That is the main point of difference between my approach and the non-statement approaches to science discussed in the above.

There are however among these approaches and mine also quite a few common aspects concerning especially the final 'empirical' links between models (of some theory) and real systems. Adams (1959) was the first person whom I know of who, in terms that may be interpreted in a realist way, described an empirical theory in terms of (i.e. as consisting of) two classes of structures: a class consisting of all the theory's 'realisations', and a class consisting of all the intended applications of the theory in question. The latter class is merely a class of empirical structures (i.e. physical – or 'real' – systems) of which the theory is (expected to be) true. As I see it, the problem is not only then to show that the theory is true of these empirical structures, but also to describe the relations – if any – between the 'realisations' of the theory and these more physical structures making the theory true.

Now, as I have said often in the above, these are the two most difficult questions a realist, model-theoretically speaking, has to face. However, these questions can only be answered on the basis of – and analogous to, in a certain sense – the (formal) relations between a theory and its conceptual models (which I take to be Adams's 'realisations'). In model-theoretic terms the answer to both the above 'difficult' questions lies in the claim that a(n empirical) theory is true of Adams's empirical structures, because it is true (formally) in its conceptual model(s) within which we find the particular empirical structure(s) in question to be (isomorphically) embedded.

Most of the non-statement advocates discussed above offer notions concerning the last empirical model-theoretic link between theories and real systems – i.e. notions concerning the idea of empirical submodels embedded isomorphically into some conceptual model(s) of a given scientific theory – that are at least reminiscent of those of a model-theoretic approach:

- Suppes's hierarchy of theories and models expresses in far more detail the more simple model-theoretic relation of isomorphic embedding at this last 'empirical stage' of theory development.
- Without a distinction between theoretical and non-theoretical terms, structuralists simply say that a particular intended application is an element of M_p. If such a distinction is made, they say that a particular intended application belongs to the class of partial potential models, M_{pp}, which is formally derivable from M_p. Sneed and his colleagues link these classes of models (whether M_p or M_{pp} – whichever one is applicable) with an empirical claim associated with the core of the theory in question. Such an empirical claim states that the set of (partial) potential models that satisfies the conditions set by the laws, constraints, and intertheoretic links of the theory in question, is indeed in K, the theory's core. The role of these empirical claims would, in my terms, be fulfilled by the isomorphic relations between the conceptual model(s) and empirical submodel(s) of a given theory, which – at least partly – are determined by the empirical expressions giving the empirical data in question.
- In terms of Beth's state-space approach the link to reality is given via some satisfaction function between some (mathematical) state-space describing some physical system, and a set of elementary statements concerned with physical measurements. This means that the actual state of a physical system at a certain time may be given by defining some state-space representing the possible states of that system and some satisfaction function, which holds if the actual state of the system (described by some elementary statement) is an element in the domain of the relevant state-space. Suppe claims a theory to be empirically true if the semantic relations holding between the propositions in the theory-formulation language and the class of causally possible physical systems are those that hold in the case of theory-induced physical systems as well. If a physical system is in the class of theory-induced systems, the domain of the physical system will be a subset of the domain of the theory, and the sequence of states of that physical system will be determined by the theory's laws.[91] The relations determining the isomorphic embeddings of empirical models into conceptual models of a given theory are fairly close to Beth's 'satisfaction' function. Also Suppe's definition of empirical truth makes a lot of sense translated into model-theoretic terms – i.e. 'empirical submodels' for 'causally possible physical systems', and 'conceptual models' for 'theory-induced physical systems'.

- Van Fraassen announces a theory empirically adequate if some model of the theory is such that (real) structures describable in experimental and measurement reports are isomorphic to an empirical substructure of the relevant model of the theory. This is obviously the approach closest to mine. Van Fraassen's empiricism and non-statement sympathies however keep his views still sufficiently 'non' – if not 'anti' – realistic for me not to subscribe to them unconditionally.
- Giere (1991: 29) views theories as being represented by a family of theoretical models and a set consisting of these theoretical hypotheses that 'pick out things ... that may fit one or another of the models in the family' (ibid.). Evaluating the truth of such hypotheses is a matter of statistical methodology (Giere, 1991), since he claims (1985: 80) a real system to 'have the same structure as a model' if the system is similar to the model to specified degrees and in specified respects.[92] This is yet another – albeit perhaps a weaker one – version of the model-theoretic isomorphic embedding between empirical submodels and conceptual models of scientific theories.
- Wójcicki (in Humphreys, 1994) speaks of factual truth if the solution offered by some theory is true of the phenomena the theory wants to explain, as well as true in a model of the theory. This model should be a realisation of the theory and should represent the phenomena in question in all relevant respects – which essentially is a shortened version of Adams's original approach. The strong agreement between Wójcicki's and my approaches is illustrated by the following remark. He (Wójcicki in Humphreys 1994:137) writes: 'The fact that formation of a theoretical model presupposes formation of a model of the data as well as the fact that formation of a model of the data can be controlled by the requirement of consistency of the model with the corresponding theory are of key significance for proper understanding of the interplay between the data and the theories, and thus for proper accounting for both the corrigibility of the data and the falsifiability of the theoretical claims.'

On the whole Suppes's approach seems to me to hold the most promise as far as solving problems concerned with possible relations between theoretical entities, empirical data, and phenomena go. Obviously Van Fraassen's notion of empirical adequacy determined by certain relations of isomorphism is very close to my own ideas on these issues. In my terms, however, proving the existence of relations of isomorphic embedding between empirical models and conceptual models (which incorporate Suppes's hierarchy of models between, at the highest level, theories of experiments, and the notion of experimental design, closest to reality) offers a way – the possibility of which Van Fraassen denies – in which to refer to the contingent and complex relations between real systems and theories via their mathematical models in a precise way.[93]

From this brief summary it is obvious that the reasons why I am not fully satisfied with the non-statement approach to the nature of scientific theories and their links with reality do not lie in their use of the term 'model' nor in their interpretation of it. The problem is rather that in a model-theoretic account of science the general terms of the theory are a prerequisite for linking these theories to (systems in) reality. No realism concerning real entities that is still a scientific realism can work without the organising role of the general linguistic terms of the theory.

In this sense Cartwright becomes important again. There is a very fine difference in emphasis between Cartwright's account (of the process of science) and mine, but it is a very important one, because it relates to our attitudes towards the realism of our models. I would prefer to say, rather than simply claiming that theories cannot say anything about the aspect of reality that their models may be linked with, that theories can only explain in that little piece of reality that each of their models 'refers' to (or rather might refer to). In other words, taking a previous example: the solar system model of Newtonian mechanics consisting of only seven planets, (used before Neptune was discovered) did indeed refer to the real situation. Although, in reality there were nine planets all along, the fact remains that it is quite possible to concentrate only on some of them and not on all at once. Whether and to what degree Newton's laws were empirically adequate when using this 'restricted' model might seem to be a more difficult issue to deal with. But it is not really, since I claim – in agreement with Cartwright – that empirical adequacy (or truth) is a notion that can only be used meaningfully if linked with the model offering the relevant interpretation of the theory being considered at the time, and the relevant empirical model available at the time.

Notes

1. See Putnam (1960).
2. See Carnap (1958).
3. Closely related to the structuralist approach is the approach of Ludwig (1990), which I shall not analyse here, but which is worth mentioning, since it is clearly important, although it does not receive much attention in the English literature on philosophy of science.
4. Przelwski (1969) analyses the structure of scientific theories in terms of theories formulated in first-order predicate logic. He wants to develop an empirical semantics for such theories, and thus needs to offer some analysis of the empirical interpretation of the basic predicates of scientific theories. He offers this analysis in model-theoretic terms: 'In part, an interpretation of a given language is identified with a model theoretic entity – a model M of language L. M assigns to each non-logical constant of L a suitable set-theoretic entity as its denotation. Thus, e.g., a one-place predicate of L is interpreted by M as denoting a certain set of objects from the universe of L. ... my ultimate aim [in (1969)] has been to answer the question ... how is an empirical interpretation possible ...' (Przelewski 1974: 401, 402). Tuomela (1972), and (1974) goes to great lengths to point out the problems involved in Przelwski's assumptions concerning the fixing of the universe of the language L in advance – which Przelwski claims is necessary to do in order to 'explain the fact of empirical interpretations' (Przelwski 1974: 404). As space does not permit more, I shall leave further discussion of these gentlemen's work for another time.
5. See Przelewski and Wójcicki (1969).
6. Pearce and Rantala (1983) claim that they offer a view in which theories are 'abstract systems' free of any explicit logical interpretation. This is very interesting, especially since it seems to allow for problems concerning theories too complex to reconstruct in elementary terms, and also since it makes it possible to allow the choice of logic to be an extra-logical (maybe philosophical?) issue. Sadly, limitations of space prevents me from saying more here.

7. One may ask how – and even if – it is possible to distinguish between conceptual and linguistic levels without giving a clear and valid answer to the question of whether it is possible to think without language. I am however not making rigid distinctions here. What I am doing, in fact, is to depict the development of scientific research by emphasising one-by-one the real, conceptual and linguistic aspects of this developmental process. And, moreover, I am not only claiming that there is interplay between these aspects, but I am requiring that there necessarily always is interplay between them.
8. Remember that set theory and hence (most of) mathematics can be formulated in first-order predicate logic.
9. In a language such as L, we usually have the following eight categories of basic symbols available:
   - a countable infinite set $\{v_i\}$ of individual variables
   - a (possibly empty) set of individual constants
   - a nonempty set $\{P\alpha\}$ of predicate letters, and, associated with each predicate letter P_α, there is a positive integer $\sigma(\alpha)$ called the arity of P_α, which gives the number of individual variables which are predicated by P_α
   - a (possibly empty) set of function symbols
   - the equality symbol '='
   - logical connectives (details not important for my purposes here)
   - quantifier symbols (ditto)
   - punctuation symbols (ditto).
10. A mathematical structure $U = \langle A, \{R_\alpha\} \rangle$ consists of a set A, the domain of U, and a set of relations R_α (one for each from some index set) defined on domain A. The sets A and $\{R_\alpha\}$ both may be infinite. A relation R_α on domain A is defined as a set of ordered $\mu(\alpha)$-tuples of elements from domain A, where $\mu(\alpha)$ is a unique non-negative integer associated with the relation R_α.
11. In other words the mathematical structure U will count as an interpretation of the language L if and only if the arity of the relations R correspond to the arity of the predicate letters P_α. (That is, if $\sigma(\alpha) = \mu(\alpha)$.) In this case U is called a realisation of the language L (and we can say that L is appropriate for the structure U). We call the relation R_α the value of P_α in the realisation U of language L. (If L has constant and function symbols, they are interpreted as elements of A and functions – of the proper arities – on A.).
12. E.g., consider the formula Pxy. If P is interpreted as the relation $<$ and if x and y are given the values of 3 and 5 respectively, then we say that Pxy is *true under that interpretation* and we say that formula Pxy in language L is *satisfied* by the valuation in the domain of interpretation U, ascribing the given values to variables x and y. (Because 3 is indeed smaller than 5.)
13. Note that a realisation of language L is in principle a realisation of all the sentences in L, and this implies that every sentence in L is either true or false in that particular realisation.
14. Einstein referred to these convictions as 'free conventions' (Holton 1995: 464). 'These themata, to which [he] was obstinately devoted, explain why he would continue his work in a given direction even when tests against experience were difficult or unavailable (as in General Theory of Relativity), or, conversely, why he refused to accept theories well

supported by phenomena, but, as in the case of Bohr's quantum mechanics, based on presuppositions opposite to his own, ...' (ibid.: 457).

15. Wójcicki (1994: 142) speaks of a 'factual interpretation' of a theory that is determined by the 'relevant world view', or the relevant paradigm, research programme, or research tradition. He says: 'One cannot understand an empirical theory and thus one can know neither what the theory is about nor how to form a theoretical model [my "intended" model] for specific problems relevant to the theory unless one has some idea what is the part (or aspect) of the world to which the theory refers, how this part is related to the others, which is the ontology of all these parts, and how both the claims of the theory and the empirical data on which it is based are related to the entities whose existence the ontology presupposes.' These factors are exactly the kind of factors that, in my terms, influence the construction of 'intended' models, and that come into play again when the models specifically constructed with an eye on interpreting or applying the theory are created.
16. Chalmers (1993, 202) gives another example of these events: 'We may abstract the falling of [a] ... leaf from other aspects of its motion ... We then apply the appropriate fundamental laws [axioms of mostly "background" theories] to [this model] that [is] the result of our abstraction. We apply Newton's laws to the leaf as a mass subject to the gravitational attraction of the earth only, and derive the law of fall from it. Of course, since we have abstracted from winds, air resistance and the like, our model will not in general serve to describe the fall of any particular leaf. After all, the model is an abstraction. Nevertheless, provided we understand the leaf to have a capacity to fall, governed by Newton's laws, the *theoretical treatment via the abstract model* does explain the falling of the leaf, as distinct from its fluttering in the breeze.' This is a point about which Nancy Cartwright has serious reservations, but with which I am in full agreement. See Section 6 for more on Cartwright.
17. Einstein referred to the movement from conceptual structures or models to theories as a 'creative leap' and in this sense referred to theories as 'free creations of the human mind'.
18. Kepler's laws:
First law: All planets follow elliptical orbits (and not circular ones, as Copernicus believed) with the sun situated in one of the foci of the ellipse.
Second law: The line connecting the sun and a planet sweeps over equal areas of the planetary orbit in equal intervals of time.
Third law: The squares of the periods of revolution of different planets around the sun stand in the same ratio (i.e. is proportional to) the cubes of their mean distances from the sun.
Newton's three laws of motion:
Every body continues in its state of rest, or of uniform motion in a straight line, unless it is compelled to change that state by forces impressed on it.
Change of motion is proportional to the force impressed, and is made in the direction of the straight line in which the force is impressed.
The forces two bodies exert on each other are always equal and opposite in direction.
His law of gravitation:
All material bodies attract each other with a force directly proportional to their masses and inversely proportional to the square of the distance between them.

19. This is close to Cartwright's notions of the role of scientific explanation and description.
20. See the examples of the discoveries of Neptune and Pluto, as well as other applications of Newton's theory in the following section.
21. Another type of approach to the interpretation of language terms is offered by Hans Lenk's methodological or schema interpretationism. See for instance Lenk (1993), (1995).
22. Whenever I speak of models of theories, I am referring to the notion of model in the Tarskian sense that a model of a theory is an interpretation of the theory under which the set of sentences comprising the theory is true. As mentioned in the previous section, at the start of theory formulation the intended 'model' scientists work with is not (initially) such a mathematical model, although at the stage of theory interpretation it becomes obvious that such (intended) models can be easily adapted such that they also are elements of the set of all (mathematical) models of the theory in question.
23. These terms are the terms traditionally referred to as 'theoretical' terms. Note that therefore I do not follow in the footsteps of advocates of the traditional version of the statement approach, in the sense that I do not need the kind of inadequate and much too simple distinction they make between theoretical and observational terms in the language of the theory. Rather than this forced division, I propose an approach in which theoretical and observational terms, as well as the difficult 'correspondence rules' or 'bridge principles' supposedly acting between these kinds of terms, all have natural interrelated and co-dependent roles to play at various levels of the scientific process.
24. I claim that Giere's theoretical models, Wójcicki's theoretical and semantic models, and Suppes's physical and set-theoretic models are all mathematical models in this sense. Some of these authors make a similar kind of distinction that I make between these models as 'intended' models – Wójcicki's theoretical models and Suppes's physical models – and these models as 'conceptual models' interpreting the theory – Giere's theoretical models, Wójcicki's semantic models, and Suppes's set-theoretic models; although not all of them seem to view all these notions as mathematical structures in the Tarskian sense. See the following sections.
25. Kuhn (1977: 301, 302) also refers to the two conceptual movements needed to conceptually move from a theory to some real system – i.e. from the theory to a conceptual model of the theory, and then from that model, to an empirical submodel of it – albeit in slightly different terms, and even if he claims that these two 'questions' are usually answered 'together' in scientific practice. He (ibid.) asks: 'How do scientists attach symbolic expressions to nature?', and then he (ibid.) writes: 'That is, in fact, two questions in one, for it may be asked either about a special symbolic generalisation designed for a particular experimental situation or about a singular symbolic consequence of that generalisation deduced for comparison with experiment.'
26. And neither can scientists. The best they can do is to react to unwanted models allowed by the theory by refining the theory's set of axioms in such a way that these models become impossible. But, obviously this is a very difficult task, especially in the first stages of the theory's formulation. And, moreover, trying to define these assumptions too finely, could in principle cancel the possibility of refining the theory in a positive way, i.e. in becoming aware of shortcomings or even errors in the formulation of the theory, via different models

of it, offered by other interested scientists. Einstein, for instance, had a static universe as his intended model for his general theory of relativity. It so happened, however, that other physicists constructed models in which the universe is anything but static. Then Einstein, initially rather upset, changed his original set of axioms in order to prevent the possibility of constructing such models. (Afterwards he conceded that he made a big mistake because of the implications of the expansion of the universe and the 'Big Bang' model.)

27. '... it is always legitimate for scientists to ask and sometimes possible for them to answer, questions about whether gasses are really composed of molecules or whether the earth really moves. Such questions cannot be rephrased as questions about the plausibility of our conceptions.' (Bhaskar 1978: 155). Well, the verification of our conceptual models depends on being able to show how experiments concerning the data in question may be linked to these models (via certain empirical models). However, what Bhaskar means, I think, is rather that science does not determine the structure of reality, but rather discovers it.
28. See Suppes (1954: 244).
29. Obviously the axiomatisation of theories in a set-theoretical framework does not necessarily imply a non-statement approach – it is quite possible to stick to the statement approach and define a set of valid sentences corresponding to the set-theoretic predicate by making use of the same axioms used to define the set-theoretic predicate in the first place. This would be closer to the model-theoretic approach spelled out in Section 2.
30. Suppes (in Wójcicki in Humphreys 1994: 148,149) writes: 'The more I think about scientific practice and reflect on how to give an accurate account of the complicated processes that go into experimentation, the more I am persuaded that there are a large number of distinctions needed to describe experimentation thoroughly, especially as data are purified for quantitative, and even more statistical, analysis. It is a long way from running around the laboratory doing one thing and then another, to having a set of data as printout or on a computer screen ready for analysis. That process still needs much more thorough attention ... gruesome details of exactly how data are purified and selected for analysis, not to speak of details of how they are generated, which itself may involve, as equipment becomes increasingly complicated, many different independent tests of reliability and accuracy of equipment.'
31. A clear manifestation of the empirical link between a model and some system in reality is given by the dimensional analysis in terms of the basic units in a derived physical quantity. According to the *Oxford concise science dictionary* (1996), a unit is the 'specified measure of a physical quantity such as length [e.g. centimetre], mass [e.g. gram], time [e.g. seconds], etc., specified multiples of which are used to express magnitudes of that physical quantity' (ibid.: 751). The basic units of physical quantities are multiplied and divided to get derived units with dimensions, e.g. a unit of the form $L^pM^qT^r$, where L, M, and T indicate length, mass, and time, respectively, and p,q and r are (usually) integers. Examples: length (distance): $L = L^1M^0T^0$; mass: $M = L^0M^1T^0$; time: $T = L^0M^0T^1$; frequency, that is 'per time': $1/T = L^0M^0T^{-1}$; speed: $L/T = L^1M^0T^{-1}$; acceleration: $(L/T)/T = L^1M^0T^{-2}$; momentum, that is mass × speed: $M(L/T) = L^1M^1T^{-1}$; force, that is mass × acceleration: $M(L/T^2) = L^1M^1T^{-2}$; energy, that is, work, that is momentum × speed = force × distance: $L^2M^1T^{-2}$; action, that is momentum × distance = energy × time: $L^2M^1T^{-1}$; power, that is force × distance ÷ time = work per time: $L^2M^1T^{-3}$. The definitions (given in

conceptual models of the measurement theory in question) of the basic units (and hence of the derived units) link these units empirically (calculations given by some empirical substructure of the conceptual model in question) to certain very definite aspects of reality. A second is the duration of 9 192 631 770 periods of a certain specific radiation emitted by a caesium-133 atom (that is the radiation corresponding to the transition between hyperfine levels of the ground state of this atom). A centimetre is the length of the path travelled by light in a vacuum during a time interval of $1/(2.99792458 \times 10^{10})$ second. A gram is one-thousandth of the mass of a certain platinum-iridium object kept by the International Bureau of Weights and Measures at Sèvres, near Paris in France.

As shown above, the science of measurement (metrology) offers some of the clearest examples of the empirical relations between models and aspects of reality. In physics too we have extraordinarily accurate theories. Penrose (1997: 51) writes: 'In quantum field theory, which is the combination of quantum mechanics with Maxwell's electrodynamics and Einstein's Special Theory of relativity, there are effects which can be computed to be accurate to about one part in 10^{11}. Specifically, in a set of units known as 'Dirac units', the magnetic moment of the electron is predicted to be 1.001159652(46), compared with the experimentally determined value of 1.0011596521(93)'. This last instance also shows that the highly regulated results of experimental situations may indeed be 'carried over' to the 'complexities' of reality, quite successfully and without too much ado.

32. The issues concerned with this stage of science were addressed by Paul Galison in his book entitled *How experiments end* (1987), and the details have now been worked out to unbelievable depths in his follow-up *Image and logic* (1997).

33. 'The kind of co-ordinating definitions often described by philosophers have their place in popular philosophical expositions of theories, but in the actual practice of testing scientific theories a more elaborate and more sophisticated formal machinery for relating a theory to data is required' (Suppes in Morgenbesser 1967: 62).

34. I agree with Wójcicki (in Humphreys 1994: 130) that Suppes's set-theoretical position may reduce philosophy of science to no more than 'selected problems of metamathematics'. Wójcicki writes: 'Needless to say, as long as an empirical theory is not provided with any factual interpretation, it remains merely a certain formal system. But ... one may wonder whether the *differentia specifica* allowing us to tell an empirical theory from a piece of pure mathematics does not consist in the fact that the former has some intended empirical applications.' He points out that this was essentially Adams's (1959) argument. He started the idea of an empirical theory consisting of two classes of structures, the one the class of all the theory's realisations and the other the class of all intended applications which is a class of empirical structures (physical systems) of which the theory is expected to be true. Adams also pointed out that not every intended application necessarily has to be a realisation of the theory. Sneed modified these notions in the sense that he requires that no component of an intended application be T-theoretical, while Adams saw the intended applications of structures of the same set-theoretic type as the realisations of the theory itself.

35. I am, of course, not denying the use of statistical methodology to clarify and determine as precisely as possible the chances of a theory's models having connections to some systems

in reality. It is merely the case that I rather advocate an elaboration of Van Fraassen's notion of empirical adequacy taking the place of the traditional literal notion of the 'truth of a scientific theory' than the mathematical tools of statistics being employed to answer these inherently philosophical questions about possible relations between science and reality.

36. To be able to establish a representation theorem for a theory implies that it can be proved that there is a class of models of the theory such that every model of the theory is isomorphic to some member of this class. Suppes (1960: 295) gives a few examples of such theorems, for instance, Cayley's theorem that every group is isomorphic to a group of transformations, and Stone's theorem that every Boolean algebra is isomorphic to a field of sets.
37. Suppes (1988: 254) claims that one of the most important and valuable uses of representation theorems in philosophy of science is that they help to increase (scientific) understanding of the represented object. Well, yes, if the conceptual models and their empirical subsets are of a different logical type, then obviously this may be the case.
38. The 'type' of a model is determined by the individual constant symbols, as well as by the relation and function symbols of the axiomatic calculus of the theory in question.
39. Suppes (1989: 27–29) argues that the fundamental theory and descriptions of apparatus are two extremes of hierarchy – in between are the models of the theory, the models of the experiment, and the models of the data. He (in Morgenbesser 1967: 62) emphasises over and over again that to be able to 'connect' experimental data to a relevant theory, the data have to be put through a 'conceptual grinder', which refers to this conceptual hierarchy he sets out from the 'raw' observations to the final 'fundamental' scientific theory. The theory of the experiment is the definition of all the possible realisations of the theory that is the first 'step down from the abstract level' (Suppes 1989: 28) of the fundamental theory. A possible realisation of the theory of the experiment is a model of the theory if the experimental conditions are satisfied. Models of the experiment represent experimental data in canonical form, but when is a possible realisation of the data a model of the data? Suppes (1989: 29) remarks again that an answer to this kind of question requires a 'detailed statistical theory of goodness of fit', since models of the data should incorporate 'all the information about the experiment which can be used in statistical tests of the adequacy of the theory' (Suppes 1989: 31), which means that he (Suppes 1989: 32) restricts the models of the data to those aspects of experiments which have a parametric analogue in the theory. In these terms, I would express Suppes's model of the scientific development of a theory (Suppes 1989: 31) as follows :Fundamental theory; Models of the fundamental theory; Theory of the experiment; Models of the experiment; Models of data; Experimental design; *Ceteris paribus* conditions.

    Suppes (1989: 32) characterises the *ceteris paribus* conditions at the bottom of the table as 'every intuitive consideration of experimental design that involves no formal statistics', which I presume refers to the context in which the concrete as-yet-untranslated-into-data 'first' observational activities are carried out. Note that in a model-theoretic account of theories the *ceteris paribus* conditions are at play in the formulation of the theory and not at the level of dealing with data. He seems to think, as Cartwright does, that the closer to reality we get, the more of these clauses we need, while I claim that only by a suspension of these clauses can we move to the more specific levels of the scientific process. Thus although I do not deny the idealised character of our conceptual models or even of the images of real systems our empirical models present, I claim that these 'idealisations' are not so much a result of *ceteris paribus* clauses as simply of the nature of scientific actions.

40. The model-theoretic notions of both conceptual and empirical models also have this kind of clarifying effect. It is the case, as he (Suppes 1989: 25) claims, that 'A radically different situation often obtains in the comparison of theory and experiment. Theoretical notions are used in the theory which have no direct observable analogue in the experimental data. In addition, it is common for models of a theory to contain continuous functions or infinite sequences although the confirming data are highly discrete and finitistic in character. ... Corresponding to possible realisations of the theory, I introduce possible realisations of the data. As should be apparent, from a logical standpoint possible realisations of data are defined in just the same way as possible realisations of the theory being tested by the experiment from which the data come. The precise definition of models of the data for any given experiment requires that there be a theory of the data in the sense of the experimental procedure, as well as in the ordinary sense of the empirical theory of the phenomena being studied.' He (Suppes 1989: 25) gives two reasons why a possible realisation of a theory cannot be a possible realisation of its data: no actual experiment can include an infinite number of discrete trials, and the parameter of the experiment is not directly observable and is not part of the recorded data. Note that although in a model-theoretic approach none of this is denied, this still does not necessarily imply that different languages are needed to talk about the content of the conceptual models and their empirical substructures.
41. Recall that in general, a model is a structure (interpretation) of the form $<A_1, \ldots, A_m, R_1, \ldots, R_n>$ where the A_i are the 'basic sets' or domains of the model (the ontology of the theory); and the R_j are relations on the A_i. Remember also that – at least for a language with a sound set of rules – satisfaction of the axioms implies satisfaction of the theory, for any interpretation.
42. We all know that a theory usually has many different models, but they all have one thing in common, which Balzer, Moulines and Sneed (1987: 3) identify as the same structure, while I emphasise also the fact that they are all models of the same (linguistically expressed) theory. A theory offers one formulation which binds together all these models (e.g. think of a theory as a set of field equations). That is why the model-theoretic approach is the one that I choose. This approach offers the possibility to focus on the linguistic nature of the theory *as well as* on its different models. Be that as it may, I do agree with Balzer, Moulines, and Sneed (1987: 3) that what is meant by models sharing the same structure, is that they all share the same conceptual framework (i.e. in my terms, they all have the same logical type or signature) and they all satisfy the same laws (theory).
43. Sometimes also referred to as 'conceptual determinations'.
44. '... this distinction may be understood as the model-theoretic explication of the distinction between the "analytic" and the "synthetic" components *within a particular theory*' (Moulines in Schurz & Dorn 1991: 318). Or perhaps, in my terms, this may be viewed as

the distinction between the themata and related context-specific factors, in so far as these co-determine the logical type (signature) of structures, and the linguistic formulation of the empirical claims suggested by the interpretation of the empirical data in question.

45. See Balzer, Moulines & Sneed (1987: 19, 20).
46. See Sneed (1976), and Balzer, Moulines & Sneed (1987) for more on the class of partial potential models.
47. See also Sneed (1971), and Balzer, Moulines & Sneed (1987).
48. This is done (ibid.) in such a way that the whole array of theoretical components satisfies the constraints *C*.
49. See Balzer, Moulines & Sneed (1987: 57ff..).
50. All of the above is of course set out in idealised terms since it is not, in this context, taken into account that the empirical claim associated with a particular theory element will always – according to the structuralists – be only approximately true. What is relevant to this article in this connection is not the overwhelming literature on the technical aspects of the question of approximate truth, but rather, and much more simply, investigating exactly what the structuralists envisage the theory core's function to be in all of this. The briefest answer is, obviously, that the theory core identifies the theory content. More precisely, the theory core defines a set of possible situations or 'ways things could be' (Sneed in Humphreys 1994: 195), called content(K).
51. See Beth (1949), (1960), and Van Fraassen (1970).
52. See Suppe (1967), (1973), and (1989).
53. Note how such an approach focuses on the underdetermination of theories by models. This depiction of underdetermination differs though from that of a model-theoretic account of science.
54. 'Beth's approach does not require or presuppose the complete formalisation of the theory under analysis. ... His approach takes into account the essential role of models in science. In Beth's account, the mathematics is not part of the physical theory, but is used to construct the theoretical framework. The theoretical reasoning of the physicist is viewed as ordinary mathematical reasoning concerning the framework. ... Finally, Beth's approach makes possible the use of formal semantic concepts and methods' (Van Fraassen 1970:337, 338). Van Fraassen believes (1970:338) Beth's approach to be much closer to the actual foundational work done in physics than any variations of the statement approach.
55. Van Fraassen (1970) points out that Wilfred Sellars has since the late fifties been arguing for precisely such a meaning structure for the language of science. See Sellars (1957: 225–308), and (1963, chapters 4, 10 and 11).
56. See Section 2.4 again.
57. The context-dependency of mathematical models does however also enter in their view of scientific theories in sofar as they – especially Suppe (1973: 151ff.) – discuss the 'extra-theoretical factors' determining for instance the experimental design of a theory. These factors include 'regularities' (ibid.) such as other theories, laws or known regularities about the phenomena in question.
58. Giere (1983: 271) explains that a physical system '... is defined by a set of state variables and system laws that specify the physically possible states of the system and perhaps also

its possible evolutions'. Giere offers the example of classical thermodynamics which may be understood as defining an ideal gas in terms of three variables: pressure, volume, and temperature, and then specifying that these are related by the law $PV = KT$. (See also Suppe, 1973: 132.)

59. Van Fraassen (1970: 130–132) discusses three types of law:

(a) *Laws of succession* are relations of succession indicating the various sequences of states various physical systems will assume over time. These relations are such that the sequences may be deterministic or statistically determined, continuous or discrete. These laws (as far as they are non-statistical) thus select the physically possible trajectories in a particular state-space.

(b) *Laws of coexistence* are equivalence relations indicating which states are equivalent to which others, if the associated law is deterministic. If it is statistical it indicates which states are equally probable, i.e. it selects the physically possible subsets of the given state-space.

(c) *Laws of interaction* (either deterministic or statistical) determine which states result from the interaction between various systems. These laws are combinations of the first two kinds of law.

60. Suppe (1973: 136) elaborates on Beth's discussion of the usage of propositions and uses this elaboration to discuss the semantic relations between theory-formulation languages, theories, physical systems, and phenomena. He then defines a *formulation* of a theory as a set or class or 'collection' of propositions which are true of the theory. Such a formulation usually consists of a few specified propositions with all deductive consequences of the specified propositions under some 'logic'. These propositions are in a language called the 'theory-formulation language', and usually forms a subset of the propositions of that language. The following basic features of a theory formulation may be identified (Suppe 1973: 137–138):

(a) A set of elementary propositions in the theory-formulation language specifies that a certain physical parameter p has as value a certain physical quantity q at time t, such that an elementary proposition ϕ is true of state s in the theory's domain if at time t, s has q as the value of the parameter p.

(b) For each elementary proposition ϕ there is a maximal subset $h(\phi)$ of the theory's domain such that ϕ is true of all the states in that subset.

(c) The function h from elementary propositions to subsets of the theory is called the *satisfaction function* for the set of elementary propositions.

(d) Elementary propositions may be compounded together in accordance to the *logic of the theory* – the logic is such that every compound proposition is true of at least one state which is – according to the associated theory – physically possible; thus obviously the logic of a theory is theory-dependent and different theories may have different logics. Suppe's (1973: 137) illustration: classical particle mechanics impose a Boolean algebra mod-2 and quantum theory imposes a non-distributive lattice. In Beth and Van Fraassen's terms (Van Fraassen 1970: 335) the logic of the theory is essentially a syntactic description of the set of valid sentences and the semantic entailment relation in that language.

(e) A *language of description* is determined by the set of elementary propositions, the theory, the satisfaction function *h*, and the logic of the theory; this language is obviously a sublanguage of the theory-formulation language; this language can describe any physically possible state in a physical system.
(f) It might be possible that the logic of the theory enables one to deduce logical consequences of propositions in the language of description. However, usually the language of description has to be incorporated into a more complex language with an amended logic – namely none other than the theory-formulation language – which can express the laws of the theory and deduce predictions.
(g) The truth conditions for the theory-formulation language are specified in terms of the relations (laws) of the theory and the truth conditions for the language of description.
(h) A formulation of a theory is a set of propositions deductively closed under the logic of the theory-formulation language such that every proposition in the set is true of the theory.
(i) Finally, the theory-formulation language may be a natural or an artificial language, but typically is a language such as 'scientific English'.

For analysing semantical relations holding between propositions in the expanded theory-formulation language and phenomena, Suppe (1973: 140ff.) offers a operationalist account of factual truth that I shall not discuss here.

61. Especially non-relativistic physical theories typically use mathematical models to represent the behaviour of a certain kind of physical system – Van Fraassen (1970:328) gives as examples the use of Hilbert space in quantum mechanics, and the use of Euclidean 2*n*-space [*sic.*] as phase-space for *n* particles in classical mechanics. (He probably means '... for a system with *n* degrees of freedom ...'. For *n* particles 6*n*-space is needed!)
62. This is very much in agreement with my idea of a (conceptual) model of a theory and relations linking it to some real system via some empirical model offering a 'snap shot' view of the real system at a specific time.
63. 'For each elementary statement *U* there is a region *h*(*U*) of the state-spaces *H* such that *U* is true if and only if the system's actual state is represented by an element of *h*(*U*). (We also say that these elements *satisfy U* ...)' (Van Fraassen 1970: 329).
64. This notion of a 'satisfaction function' characterises exactly the kind of relation I see involved in determining the possible isomorphic embeddings of empirical models into conceptual models of some theory.
65. Van Fraassen's semi-interpreted language thus in these terms consists of the elementary statements connected to a certain physical system, the specific state-spaces in question, and the satisfaction function in question.
66. See Van Fraassen (1970: 337).
67. 'For our purposes, a scientific theory has two components. One is a family of [theoretical] models .... The second is a set of theoretical hypotheses that pick out things in the real world that may fit one or another of the models in the family' (Giere 1991: 29).
68. Think of Suppes's hierarchy of models.
69. See Wartofski (1974: 19).
70. He (Giere 1984: 13) gives as example of a theoretical hypothesis the statement that 'The positions and velocities of the earth and moon in the earth-moon system are very close to those of a two-body particle Newtonian model (with specified initial conditions).'

71. Another example of a notion of approximation by degree, as noted by Giere (1985:80), can be found in Beth's state-space approach in terms of the value r of a magnitude m at a time t in a given state-space. This notion also fits both my and Suppes's approaches to 'empirical' relations.

72. 'I agree with contemporary students of probability, induction, and the foundations of statistics that the individual hypothesis is a useful unit of analysis. On the other hand, I reject completely the idea that one can reduce the rationality of the scientific process to the rationality of individual agents. The rationality of science is to be found not so much in the heads of scientists as in the objective features of its methods and institutions' (Giere 1983: 270). He also (like Suppes, Van Fraassen, and Suppe) reflects on the complex nature and role of data in the testing of theoretical hypotheses: '... in order to determine whether a proposed model fits the world one needs some information about the part of the world in question. But not all information is relevant. We will use the term data ... to refer to the special information that may be relevant to deciding whether the model in question does fit' (Giere 1991: 29). Giere (1991: 37–40) offers a programme for the evaluation of theoretical hypotheses, but I will not go into the detail of that here.

73. Wójcicki's empiricism has some far reaching consequences that could not have been accommodated by classic empiricism. Newer forms, like that of Van Fraassen, are more flexible though. The classic version views observations in terms of the fixed point of view of the idealised 'normal objective observer', while Wójcicki now offers a more flexible empiricism in so far as the experimental accessibility of specific states of affairs depends heavily on the competence of the experimenter and her technical capabilities.

74. See my comments on this matter above, and in Ruttkamp (1997a).

75. This is Suppes's notion of a 'physical' model and my notion of an 'intended' model.

76. See Wójcicki's examples in this regard in Humphreys (1994: 134–136).

77. See Wójcicki in Humphreys (1994: 135).

78. 'The fact that formation of a theoretical model presupposes formation of a model of the data as well as the fact that formation of a model of the data can be controlled by the requirement of consistency of the model with the corresponding theory are of key significance for proper understanding of the interplay between the data and the theories, and thus for proper accounting for both the corrigibility of the data and the falsifiability of the theoretical claims' (Wójcicki in Humphreys 1994: 137).

79. Wójcicki (in Humphreys 1994: 138) writes '... let me point out that theoretical models are not empirical systems, unless we consider them to be empirical systems of some hypothetical or possible worlds. The reason is quite obvious. Even though the objects a model involves are supposed to be real ... still these objects are postulated to satisfy certain theoretical assumptions ... which they may not actually satisfy. A theoretical model is just a certain theoretical construct, an abstract entity, even if some of its parts appear to be parts of physical reality.' I agree. Suppes (1960: 290, 291) writes for instance that 'To define formally a model as a set-theoretical entity which is a certain kind of ordered tuple consisting of a set of objects and relations and operations on these objects is not to rule out the physical model of the kind which is appealing to physicists, for the physical model may be simply taken to define the set of objects in the set-theoretical model'. Instead of 'define'

it would be preferable to say 'be in one-to-one correspondence with'. Mathematical sets of non-mathematical objects are problematic.

80. Wójcicki (in Humphreys 1994: 140) apparently does see this though:

'There is no question that, on many occasions, the search for a theoretical model for a question Q is the search for a semantical model of a specific theory T such that this particular model will be the right one to examine question Q. This is the situation that obtains whenever one believes that T is the theory which allows for solving Q. If the formation of a theoretical model is from the very beginning meant to consist in selecting the right semantical model then, of course, the notion of a semantical model is prior to that of a theoretical one. ... Newtonian particle mechanics was viewed as the right theory to solve the question Q_t concerning Mercury's movement. However there are numerous occasions when the search for a model for Q starts when one does not consider any theory to be directly applicable to Q. Bohr's model of the atom, which in fact was a model of some specific problems on the dynamics of the particles of which the atom was believed to be composed, was certainly not meant to be a semantic model of the official theory; rather it was meant to be a certain tentative theory of the phenomenon. ... [Here] [t]he construction of a theoretical model precedes any semantic considerations and in this sense it is prior to the latter. ... the idea of a model is central for two, in a sense, opposite activities: applying theories and forming them. The interplay between the two is what largely determines the dynamics of science ... Now, while the concept of semantic model is central for the former, the theoretical model is central for the latter.'

81. This is what Wójcicki (1979: 159) is getting at when he remarks that '... there is an evident need for a term which both when a theory is being constructed and when it is finally set up can be applied to denote those parts of the theory that provide a relatively complete account of particular regularities of phenomena in the scope of the theory'.

82. See also Wójcicki's (1979: 161–163) example illustrating these problems.

83. 'On the semantic view, theories are just collections of models; this view offers then a modern Japanese-style automated version of the covering-law account that does away even with the midwife [of deduction]' (Cartwright, Shomar, & Suárez 1995: 139).

84. And so they should be, given that they form part of human scientific knowledge which, from the beginning, simply *is* based on activities of abstraction, because that simply is how we humans *know* anything.

85. Both Cartwright and I view these models as idealisations, although we differ about the implications of the ideal nature of these models for the process of science, as pointed out already.

86. In this sense Cartwright's 'phenomenological laws' remind very much of Suppes's (1989) models of data.

87. Cartwright illustrates this accusation with the following remarks: 'Not all radiometers that meet Maxwell's two descriptions have the distribution function Maxwell writes down; most have many other relevant features besides. This will probably continue to be true no matter how many further corrections we add. In general ... the bridge law between the medium of a radiometer and a proposed distribution can hold only *ceteris paribus*' (Cartwright 1983: 155).

88. There are cases in which we believe phenomenological laws to be soundly deducible from a certain set of fundamental laws, but find that the actual deduction is extremely difficult. These cases, however, do not prove in any way either that phenomenological laws 'typically' cannot be deduced from fundamental ones, or that the 'all things being equal' and additional assumptions needed in such deductions may be found to 'contradict' the original (set of) fundamental law(s).
89. Well, this is true of fundamental laws too – Newton says openly he offers no hypotheses concerning the *reasons why* his laws of gravitation are true.
90. According to Cartwright, regardless of whether an object is observable or not, if we can manipulate it, [intervene in its behaviour á la Hacking (1983)], we can formulate low-level generalisations which accurately describe the causal relations into which it enters.
91. This implies that propositions in the theory-formulation language may refer to and describe some physical system because a physical system in the theory-induced class of systems restricts the theory to a single sequence of states.
92. These notions form the core of Giere's 'constructive realism' (Giere, 1985).
93. Nancy Cartwright's recent attacks (1989, 1994a, 1994b, 1995a) on 'fundamentalism' seem rather pointless from this viewpoint.

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