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OVERDETERMINATION OF THEORIES BY EMPIRICAL MODELS: A REALIST INTERPRETATION OF EMPIRICAL CHOICES

ABSTRACT. A model-theoretic realist account of science places linguistic systems and their corresponding non-linguistic structures at different stages or different levels of abstraction of the scientific process. Apart from the obvious problem of underdetermination of theories by data, philosophers of science are also faced with the inverse (and very real) problem of overdetermination of theories by their empirical models, which is what this article will focus on. I acknowledge the contingency of the factors determining the nature – and choice – of a certain model at a certain time, but in my terms, this is a matter about which we can talk and whose structure we can formalise. In this article a mechanism for tracing "empirical choices" and their particularized observational-theoretical entanglements will be offered in the form of Yoav Shoham's version of non-monotonic logic. Such an analysis of the structure of scientific theories may clarify the motivations underlying choices in favor of certain empirical models (and not others) in a way that shows that "disentangling" theoretical and observation terms is more deeply model-specific than theory-specific. This kind of analysis offers a method for getting an articulable grip on the overdetermination of theories by their models – implied by empirical equivalence – which Kuipers' structuralist analysis of the structure of theories does not offer.

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1. Introduction

Almost all projects that aim at demarcating the "purely" observational (in the sense of so-called "raw sense data") from the theoretical are beset with certain difficulties which are invariably the result of two major issues. On the one hand, these difficulties arise as a result of the nature of the links postulated to exist between these two kinds of entity and the languages with which they are described, and, on the other hand, the difficulties are caused by the nature of the set of "intended applications" of a theory, especially in terms of the existence of more than one "empirical model" as the "real" domain of reference of the terms of theories. I claim here that a model-theoretic realist analysis of the structure of scientific theories may clarify the motivations underlying choices in favor of certain empirical models (and not others) in the above context of demarcation in a way that shows that "disentangling" theoretical and observation terms is more profoundly model-specific than theory-specific. A mechanism for tracing "empirical choices" and their particularized observational-theoretical entanglements is offered in the form of Shoham's version of non-monotonic logic.

A model-theoretic realist account (see Ruttkamp 1999, and Ruttkamp 2002) of science places linguistic systems and their corresponding non-linguistic structures at different stages or different levels of abstraction of the scientific process. The philosophy of science literature offers two main approaches to the structure of scientific knowledge analyzed in terms of theories and their models: the "statement" and the "nonstatement" approaches. The statement depiction of scientific theories is cast in terms of an analysis of scientific knowledge as embodied by theories formulated in some (appropriate first-order) symbolic language with certain observational links of correspondence to reality. Defenders of the nonstatement approach (such as Suppes, the structuralists including Theo Kuipers, Beth and Suppe), in their turn, place more emphasis on the (mathematical) structures satisfying the sentences of some scientific theory in the Tarskian sense, than they do on the language in which the particular theory is formulated.

A model-theoretic realism retains the notion of a scientific theory as a (deductively closed) set of sentences (usually formulated in some first-order language), while simultaneously emphasizing the interpretative and referential role of the conceptual (i.a. mathematical) models of these theories. Rather than looking to typical statement approaches's notions of correspondence rules, or bridge principles to address observational-theoretical translations and referential questions concerning terms in theories, a model-theoretic approach acknowledges the re-interpretability of the language(s) in which theories are formulated and so turns to mathematical models of theories as the crucial links in the interpretative and referential chain of science. Merely "presenting" the theory "in terms of" its mathematical structures (or the set-theoretical predicates representing the class of these structures), which is typical of the so-called nonstatement accounts of theories, is not considered sufficient, since these accounts seem to eliminate – or at least de-prioritize – the possibility of addressing within a realist context the nature and role of general terms and laws – expressed in some appropriate formal language – in science. Model-theoretically speaking, this is unacceptable, since the links between the terms of scientific theories (as linguistic entities) and their interpretations in the various models of these theories in this context are taken to regulate the whole referential process, since such links offer particularized theoretical/ observation distinctions.

Advocates of the structuralist program take $<M_P, M> = K$ (Moulines 1991, p. 319; Balzer, Moulines, Sneed 1987, pp. 36*ff.*) 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. Sneed's answer to the questions

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surrounding the question of theoreticity is roughly close to the criterion that Kuipers (2001, Chapter 12) uses to denote epistemological stratification, i.e. a criterion refering to the theory in which the concept under discussion appears. Kuipers (2001, Chapter 12) offers a more simple formulation than Sneed's for a general distinction between two kinds of "non-logico-mathematical" terms in relation to a statement S, but here I shall explain the more general formulation of so-called T-theoreticalness as Stegmüller (1979, p. 116) sets it out, following Sneed.

Stegmüller (p. 116) summarizes this criterion as follows: "... a quantity *f* is theoretical relative to a theory T iff the values of *f* are calculated in a T-dependent manner". Stegmüller (pp. 117-118) stresses the pragmatic implications of Sneed's criterion when he remarks that it may be viewed as a "... partial explication of the phrase 'meaning as use'." The structuralist emphasis on the *use* of laws determining the latter's empirical extensions fits in with the default framework for choices of empirical models, sketched in the following sections. The consequence of the application of this "T"-criterion to the structure M (i.e. to the structure representing the so-called "fundamental" laws, which holds for every application of the relevant theory) is a "decomposition" (p. 118) of M, as follows: the class M_P is the class of possible models of the "full conceptual apparatus". (In most cases M will only form a small subset of M_P.) Removing all theoretical components from M_P, leaves us with the set M_{PP} of partial potential models.

This further class of partial potential models M_{PP} is obtained by taking the elements of M_P and for each of them forming what we could call – following Kuipers (2001, Chapter 12) – an "observational reduct." Recall that a "reduct" in model-theoretic terms is created by leaving out of the language and its interpretations some of the relations and functions originally contained in these entities. In the structuralist case it is relations, functions, and constants which correspond to T-theoretical terms that are left out to define such a reduct.

In Kuipers' terms this comes down to the fact that within the class of partial potential models lies the class πM of the observational reducts of the structures in the class of actual models, M. Also in the class M_{PP} lies I, the class of intended applications. The empirical claim associated with a certain theory then, is that I is a subset of πM. The question to be asked within the context of this article is whether this implies that the structuralist theoretic/observational distinction might be as naive as the positivist one, in the sense that they do not relativize their reduct to particular applications of M. Surely more than one reduct exists, both of the class of potential models and of the class of actual models, depending on both the real system under consideration and the nature of the classes M_P and M, since non-isomorphic models may have isomorphic empirical substructures – so the structuralist reduct projections may be many-to-one – without any harm done either to (moderate) realist ideals or to theory-observation disentanglements.

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An obvious motivation (on which both realists and anti-realists would surely agree) for empirical theory construction is the (successful) application, in one way or the other, of that (empirical) theory. That is why it is not completely correct to claim that we know what an empirical theory looks like if we know its core. We also need some information on the nature of its intended applications. Structurally speaking, then, if we take *I* as the set of intended applications of a given empirical theory identified by a specific given *K*, we have to know the nature of the elements of *I*, as well as the extension of *I*. Note again that cores of theories and the applications of theories together – i.e. M_P, *M*, and *I* – are the "material" out of which empirical claims may be formulated.

Now, the elements of *I* are taken – by the structuralists – to be not "simply the 'real things', independent of any conceptualisation, to which the theory is supposed to apply" (Moulines 1991, p. 319)[1], but rather systems, which are nothing other than structures that present us with ways of "... conceptually carving up reality in pieces and putting these pieces in certain relationships" (ibid., p. 320). Thus, we can take a system, *s*, to be a structure of the form $<A_1, \ldots, A_m, R_1, \ldots, R_n>$. Sneed (1994, p. 196) points out that *I* should be seen as the "totality" of potential data for which the theory in question is supposed to account. I agree, and model-theoretically speaking "real systems" are just such structures (i.e. elements of I). These structures are represented in model-theoretic terms as empirical conceptualizations of data – more about this in the following section.

Determining the identity of I for a given theory is something to which, structuralists stress, there is *no purely semantic answer*. Any kind of approach to this issue has to be preceded by what they term "pragmatic-diachronic considerations" (Moulines 1991, p. 321), because of the fact that for every given theory core, *K*, there has to exist a *scientific community* that will *use* (in Stegmüller's sense mentioned above) the theory identified by the core in "real life." Because *I* is dependent on the scientific community within which the theory under consideration has been constructed or will be applied, the structuralists refer to the class of intended applications as a "genidentical" (p. 322) entity. It is this kind of scientific community-relativity (or rather disciplinary matrix-relativity) plus the constant being-in-motion of science that I claim non-monotonic logic can rationally represent in a model-theoretic account of science – see Section 4.

Recall that in Kuipers' terms, modifications aiming at better – or stricter – definitions of I are made to the mathematical structure *M* in terms of the structuralist notion of T-theoretical-ness, so-called "constraints," and "special laws." I shall discuss below a new non-classical method of analysing choices concerning the members of the class I at specific times, which is adequate for the purposes of establishing the continuance of science from a realist point of view, and which also focuses on certain subsets of the class *M*. Before I explain this

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[1] In my terms, the elements of *I* would be *representations* of systems of the "real things."

further I shall briefly outline what I mean by a "model-theoretic" account of scientific theories (see also Ruttkamp 1999).

In what follows I shall first briefly offer a sketch of the framework of a model-theoretic account of science. The next section focuses on the problem of overdetermination of theories by empirical models, or, as I refer to it sometimes, the problem of "empirical proliferation." Thereafter I offer a model-theoretic non-monotonic default model for dealing with the problem of empirical model choice. Finally I make a few comments on the implications for realism of the semantic use of models in analyses of scientific theories and show the relations between model-theoretic and constructive realism.

2. A Model-Theoretic View of Science

As mentioned above, in model-theoretic terms both the linguistic and the non-linguistic aspects of scientific knowledge and its expression(s) are woven into an articulated referential chain. In such an account, models of theories are defined in the usual Tarski sense. The method of ("empirical") verification of each of these models (i.e. how well do each of them reflect the system in the real world?), is decided by the specific nature of the specific model in question, *as well as* by the nature of the specific real system in question. Hence (see Figure 1) I claim that if the phenomena in some real system and the experimental data concerned with those phenomena are logically reconstructed in terms of a mathematical structure – call it an "empirical" model – the relation of empirical adequacy then becomes – close to Van Fraassen's depiction – a relation which is an isomorphism from the empirical model into some empirical reduct of the relevant model of the theory in question.

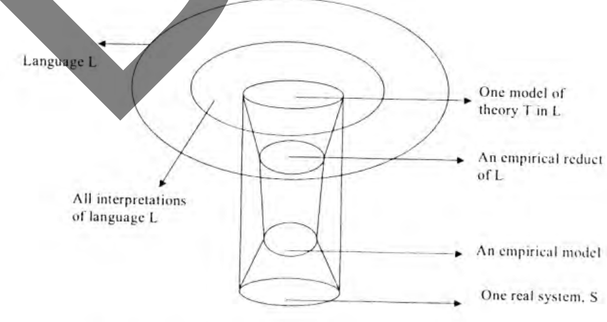

Fig. 1. A model-theoretic account of science I

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Consider what it really means to formulate a model of a particular theory. A model of a theory sees to it that every predicate of the language of the theory has a definitive extension in the underlying domain of the model. Now, focusing on a particular real system at issue in the context of applying a theory, which in turn implies a specific empirical set-up in terms of the measurable quantities of that particular real system, it makes sense to concentrate only on the predicates in the mathematical model of the theory under consideration that may be termed "empirical" predicates. This is how in my context an empirical reduct is formulated. Recall that a "reduct" in model-theoretic terms is created by leaving out of the language and its interpretations some of the relations and functions originally contained in these entities. This kind of structure thus has the same domain as the model in question but contains only the extensions of the empirical predicates of the model. Notice that these extensions may be infinite since they still are the full extensions of the predicates in question.

Now, as sketched above, from the experimental activities carried out in relation to the real system on which we are focusing, a conceptualization of the results of these activities, i.e. of the data resulting from certain interactions with this system, may be formulated. This (mathematical) conceptualization of data is refered to as an empirical model. Then, if it is the case that there exists some relation of reference between our original theory and the real system we are considering, we may then find that there is a one-to-one embedding function from the empirical model into the empirical reduct in question. Why? The empirical model contains finite extensions of the empirical predicates at issue in the empirical reduct, since only a finite number of observations can be made at a certain time.

To summarize: the interpretative model interprets all terms in the appropriate relevant language and satisfies the theory at issue. In the empirical reduct are interpreted only the terms called "empirical" in the particular relevant context of application or empirical situation. Think of this substructure of the interpretative model as representing the set of all atomic sentences expressible in the particular empirical terminology true in the model. An empirical model – still a mathematical structure – can be represented as a finite subset of these sentences, and contains empirical data formulated in the relevant language of the theory. See Figure 2 for the example following below.

Say we take Newtonian mechanics as our theory. Take our solar system as a model, M, of the theory. Take one empirical reduct of this model, call it E_{Red}, a substructure of M, containing (only) events, that is, four-tuples (x, y, z, t) pinpointing the position(s) of Mars on its elliptical orbit. Notice that we acknowledge that the elliptical form of the orbit is an approximation, since we assume for now that the sun is heavier than any of the other planets and that we exclude predicates concerning forces, accelerations, and other so-called

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theoretical predicates – such as mass – which are not the "direct" result of observations in this case[2]. This subset E_{Red} then is the set of all points (x, y, z, t) lying mathematically on the elliptical orbit of Mars. Should we now consider the empirical models that resulted from observations of countless astronomers through the ages, we would find empirical models E_{emp}^{i}, $\forall i \in N$ all isomorphically embedded in our empirical reduct E_{Red} (assuming for our purposes here that Mars's orbit has not shifted for any reason). Thus we find that the conceptual four-tuples we get (at a certain time) from observing the positions of Mars in space and time, that is, the elements of some empirical model E_{emp}, are amongst the elements of E_{Red}, that is, the four-tuples (x, y, z, t) showing us the position of Mars at various time instances.[3]

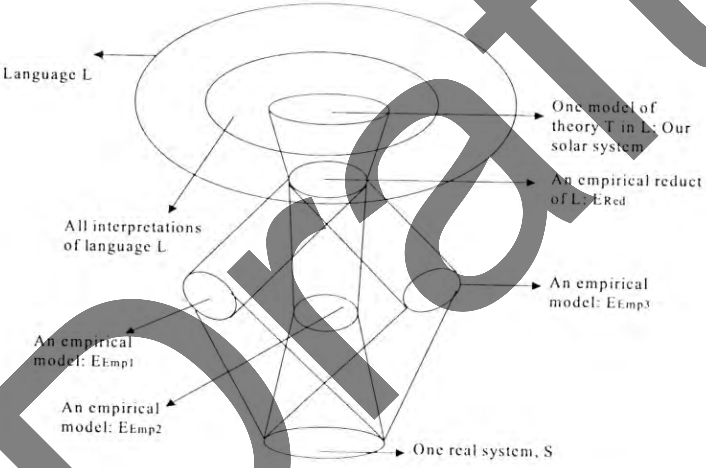

Fig. 2. A model-theoretic account of science (Newton's theory)

In terms of theory-observation distinctions in this context, notice the following: the requirement for a set of *c*-rules (or "postulates") to connect theoretical terms to their observational counterparts was supposed by some to be the tool for actualizing the positivist dream of rooting out all forms of pseudoscience, but, in a sense, turned into the biggest enemy of the positivist

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[2] Note that this distinction between so-called "theoretical" and "empirical" predicates is model-specific rather than unique or absolute.

[3] Note that in this case, the embedding function simply is the identity function, mapping elements of E_{emp} onto elements of E_{Red}.

ideal. Briefly, the reason for this is that it is impossible – given all of the above – to find one clear, unambiguous method in which to draw the observational/theoretical distinction, mainly because of, on the one hand, the spurious nature of the positivist definition of *c*-rules; but also, on the other hand, because of the fluid nature of scientific knowledge.

In Chapter 2 of *Structures in Science* (Kuipers 2001), Kuipers comments on the problems concerning theory-observation distinctions. He writes (p. 37): "The law-distinction [i.e. the distinction between experimental or observational laws and proper theories (my insert)] forms a crucial construction principle for the hierarchy of knowledge and therefore an important heuristic factor in the dynamics of knowledge development." Obviously this distinction is closely related to theory-observation distinctions (as he also points out). In model-theoretic terms it can also be shown – focusing on models rather than theories as units of construction – that theory-observation distinctions are constructive of different levels of knowledge. This notion of the "multi-level-ness" of science also reminds of the structuralist notion of theory-nets built up in terms of T-theoretical distinctions.

Model-theoretically the prominent issue in formulating a realism containing both linguistic and non-linguistic systems may be viewed in terms of reconciling intensive and extensive definitions of terms in theories (intensive definitions are linguistic descriptions, while extensive definitions are listings of cases). The formulation of a theory in terms of some appropriate first-order language offers no more than an intensive definition of the terms in theories concerned, i.e. theories are systematic descriptions of the defining attributes of terms in theories in such a way that the "basic terms of the theory are 'implicitly defined' by the postulates of the theory" in Nagel's terms (Nagel 1961, p. 91). Against the notion of a "fully articulated scientific theory" (p. 91) having "embedded in it an abstract calculus that constitutes the skeletal [deductive] structure of the theory" and thus the conviction that connotations of terms in theories are irrelevant to this bare deductive skeleton, in a model-theoretic context, however, the connotations of the terms in theories are important in so far as they are relevant to the interpretation of the deductive elaboration of the postulates of the theory. In this sense it is, though, still the case – in a typical statement way – that the "fundamental assumptions of the theory formulate nothing but an abstract relational structure" (p. 91) since the terms in theories are not "tied down to definite observational [situations] by way of a fixed set of experimental procedures" (p. 89) and are thus general enough for these terms to be applicable to "diverse areas" (p. 89) in the empirical sense.

The role of the connotations of terms in theories becomes most evident at the level of the (conceptual) models interpreting these terms, since here the connotations of these terms serve to present the first referential links of these

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terms by making more precise or particular their general intensive definitions by interpreting them in such a way that the sentences of the definitions come out true. The denotation or extension of at least some of the terms in theories, i.e. the classes of all the individual cases to which the terms in theories in question apply, is given by the notion of empirical models isomorphically embedded into some empirical reduct of some mathematical model of the theory concerned. Model-theoretically thus, "rules of correspondence" (and thus, extensive definitions of some terms in theories) are given by the reduction functions fashioning empirical reducts from models, and also by empirical models and the isomorphic relations between such models and empirical reducts. Note that in this context the distinction between so-called "theoretical" and "empirical" predicates is model-specific rather than unique or absolute, which points towards a changeable – although traceable – model-specific interpretation of theory-observation "entanglements."

Notice that non-isomorphic models may have isomorphic empirical substructures. Also, theories are interpreted by many different models – think of the difficulties involved in pinning down standard models of theories. Moreover, theories, as well as their models, are also further referentially linked to many empirical reducts. In other words the theory/observation distinction cannot be a unique one, but must, of necessity, be model-specific first, but also empirical reduct-specific. This should not lead to conclusions of rampant relativism, however, since these distinctions can all be precisely defined and articulated in terms of model theory such that theory-observation distinctions are actually accepted as contingent on particular theory-model-empirical reduct-interpretative links.

Nagel (1961) offers one of the most well-known distinctions between so-called "experimental laws" and "proper theories." In his sense experimental laws contain only so-called observational terms, while the purpose of the formulation of proper theories is to explain experimental laws by the theoretical terms they introduce. However, Kuipers (2001, Chapter 2) points out the equally well-known fact – stated above – that this distinction is far from a clear cut or neat division. Kuipers (p. 3) claims that the so-called "law-distinction" should be viewed on the basis of "… a theory-relative explication of theoretical and observation terms … [This] suggests a disentanglement of the so-called theory-ladenness of observations. In particular, an observation may not only be laden by a theory, if unladen by it, it may nevertheless be relevant for it, and even be guided by it." The above analysis implies that Kuipers's specification of theory-relativeness (typical of structuralists) is too weak to embody the full complexity of theory-observation distinctions, since these distinctions concern only T-theoretical-ness. Obviously, pointing out the theory-relativity of these distinctions is a step in right direction, but it does not take into account – or perhaps, can at least not fully

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account for – the potentially changing (semantic) relations between models, empirical reducts, and empirical models.

In general the structuralist and Hempelian accounts of theoretical-observational distinctions terms were taken simply as a new kind of interpretation of the old two-level distinction between the theoretical and observational levels. Kuipers (p. 38) claims rather that these accounts – perhaps especially that of Sneed's – point to a new multi-level distinction between these kinds of terms. He (p. 38) explains that in terms of the long-term dynamics of science, if some proper theory is accepted as "approximately true" it is usually possible to set up criteria for the determination of its theoretical terms. Then, he claims, as soon as the theoretical terms are identified the proper theory "becomes" (p. 38) an observation theory, and "the corresponding theoretical level transforms into a higher observational level, enabling new observations and hence the establishment of new observational laws, asking for new, 'deeper' theories to explain them" (p. 38).[4]

I find Kuipers' remarks concerning a multi-level interpretation of science insightful, and view them, as mentioned already, as related to the structuralist notions of specializations and theory nets. In my terms the theoretical terms in a proper theory will be "identified" as soon as an interpretation of the theory is formulated in terms of some model. The proper theory "becomes an observational theory" when some reducing function has "reduced" the relevant model to an empirical reduct (substructure) containing only "observational" terms (in that particular context). Notice again that the reducing function is changable in the sense of "reducing" the same model to different empirical reducts. Recall here that the set I of intended applications is not a "Platonic entity" but "an open class frequently originating through gradual expansion from a paradigmatic original class" (Stegmüller 1979, p. 116). This shows that the evolution of "corresponding theoretical levels" into "higher observational levels" is further complicated by the ever growing class of empirical models (intended applications in structuralist terms) the elements of which (may) contain different entities and relations available as possible referents of terms in a specific theory. The following section focuses on a way to articulate decisions made for a particular relation of empirical adequacy at a particular time.

More precisely, in the second half of this article I show how a model-theoretic account of scientific theories, augmented, at the level of empirical reducts, by the machinery of non-monotonic logic, may enable us to express reference relations between theories and empirical (observational) models in the face of theory

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[4] This also recalls Patrick Suppes' (1967) hierarchy of theories and models – he articulates 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, with an articulation that depends on the experimental or observational situation in question.

change in general, and multiple model choice in particular. Rather than focusing only on progress in terms of a gradings of truth and success, I want to focus on the choices made when one is faced with more than one empirical model and the motivations for these choices. Finding a way to trace these motivations and link them with the formulation of models of theories might help to refine the relations between target sets and their approximations, in Kuipers' sense (or between the "actual" and the "nomic"; Kuipers 2001, Chapter 8), and so, in the end, might also have something to add to our conception of scientific progress.

3. The Problem of Empirical Proliferation

My answer when confronted with questions concerning model choice has usually been that these are about very particular concerns that will depend on the particular intentions of a particular scientific community at a particular time – notice the echoes of the structuralist concerns regarding the limits of the mechanisms of pure semantics to present these intentional choices. Although I still claim this to be the case, I have always been dissatisfied with the – at least apparent – informal character of such an answer. In this context, I want to consider with you the possibility of introducing into the wide empirical equivalence debate, concentrated on issues concerning overdetermination of theories by data, the non-monotonic mechanism of default reasoning, refined into a model-theoretic non-monotonic logic (based on the logic of Yoav Shoham) offering a formal method to rank models.

In terms of what I call "temporary knowledge" we need at least to consider the following questions: Where in the process of science would we find these particular pockets of temporary knowledge? In what sense exactly may scientific knowledge be temporary? How does such knowledge affect our final judgments about the nature of scientific progress?

Briefly, in answer to these questions: where do we find such pockets of temporary knowledge? We find such knowledge everywhere in the process of science, obviously, since we know that even the "best" theory at a certain time might in all probability be refuted at some point in the future. However we find the most extreme form of it at the level of the process of science where empirical adequacy is determined, that is, in my terms, the level at which we are considering so-called "empirical reducts" and their relations to so-called "empirical models."

The sense in which I mean this knowledge to be "temporary"is the one in which we make choices for certain models (and so sometimes for certain theories) at certain times. The context of this discussion is that of empirical equivalence in Van Fraassen's sense of the notion: he (1980, p. 67) explains that if for every

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model M of theory T there is a model M' of T' such that all empirical substructures of M are isomorphic to empirical substructures of M', then T' is *empirically at least as strong as T*. Earlier Van Fraassen (1976, p. 631) wrote that "Theories T and T' [each being as least as strong as the other in the above sense] are *empirically equivalent* exactly if neither is empirically stronger than the other. In that case ... each is empirically adequate if and only if the other is."

But what is the status of the models or empirical reducts – or even the relations of empirical adequacy – we do not choose at a specific time, then? The knowledge or information about the particular empirical model(s) in question that they carry, certainly still *is* knowledge, is it not? Well, yes and no. What we need is a formal mechanism by which we can depict our choices, the motivations for our choices, and the change of both of these, should there be a change of context within which we are applying some theory. We choose to work with a certain model or empirical reduct at a certain time, but we may always change our minds and make a different choice, which might imply a change in the set of knowledge claims (and the meta-tracings of reference links and theory-observation distinctions) our theory is offering, and this is where non-monotonic logic in the form of default reasoning comes in, as I explain below.

Related to this, as far as the nature of scientific progress is concerned, my (multi-level) view is the following. Theories change very slowly, conceptual models more quickly, and empirical reducts, and the empirical databases (the accumulation of empirical data via observations and experiments) they depict, change the quickest. The general theory of relativity was formulated by Einstein (and Hilbert) in 1915. For more than 80 years now physicists have been constructing literally dozens of different types of models – all models of precisely the same theory – to fit both experimental and observational data about the space-time structure of the real universe *and* certain paradigmatic preferences. Now, in this sense, I agree with Kuhn that neither the content of science nor any system in reality should be claimed to be "uniquely exemplified" by scientific theories from the viewpoint of studies of "finished scientific achievements." And, therefore, one has to accept the open-endedness (see Section 1 again) of theories as a permanent feature of the total process of science. Notice, though, that this open-endedness to me is represented by the ebb and flow of the models (including their empirical reducts) of the theory which ensures the continuity of science at least at a formal (meta) level of analysis.

Hence I imply that issues of theory succession or reduction are often, for long periods of time better – or at a finer level of analysis – interpreted as issues of model succession or reduction, and that this implies that certain aspects of our knowledge are more temporary than others. I claim that the terms of an already established theory can be said to be "about" an ongoing potential of entities in some system of reality to give reference to some objects and relations in *any*

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model of that theory. The actualization of this potential requires human action in the sense of finding and finally articulating "satisfying" referential relations between systems in reality and certain empirical aspects (reducts) of models of the theory. And it is the nature of these referential relations that will be the topic of the rest of this article.

Let us now focus on what I term "empirical proliferation." In a sense this is the reverse of the traditional scenario of the underdetermination of theories by data. In the philosophy of science the issue of the underdetermination of theories by data is the original problem of explaining – and perhaps justifying – the existence of empirically equivalent, yet incompatible, scientific theories. In the history of science instances of such theories are quite common – think of the various ways in which an electromagnetic field has been described, from Faraday through Einstein to Feynman.[5] In the context of underdetermination of theories by data, the bottom line thus is that empirical data are too incomplete to determine uniquely any one theory.

Turning now to the flipside of underdetermination, notice that we interpret "empirical equivalence" in the traditional (Van Fraassen-ian) way – i.e. theories are empirically equivalent just in case they have the same class of empirical consequences. Also bear in mind that contact between scientists and real systems that result in scientific data is relative to the state of scientific knowledge and of technological development at the time, as well as the research tradition or disciplinary matrix in which scientists work at that given time. Scientific knowledge is amendable and even defeasible, because of its contingent and particularized links with the reality it describes (and explains). Recall that according to Van Fraassen (1976, p. 631) a theory is empirically adequate if "all appearances are isomorphic to empirical substructures in at least one of its models." This view leads the way to the model-theoretic interpretation of empirical equivalence according to which theories with the same empirical reducts, or at least some empirical models, are empirically equivalent. These definitions point to the reverse case of traditional underdetermination of theories by data, i.e. a specifically model-theoretic interpretation of traditional underdetermination – i.e. underdetermination of data by theories. This article focuses on this very important (and different) aspect of traditional empirical equivalence.

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[5] More precisely, traditionally the nature of underdetermination has been understood in terms of two kinds of relations between the "real world" and scientific theories. The first kind is taken to exist between phenomena (or whole systems) in reality and the observation terms of theories, while the second kind of relation is said to exist between sets of protocol sentences (formed from the observation terms and expressing data) and possible theories incorporating or explaining such a set of protocol sentences – that is, the existence of incompatible but empirically equivalent theories.

In general scientific theories, depicted as syntactic (linguistic) entities that need to be interpreted to be given semantic meaning and reference, are not able to uniquely capture their semantic content. In terms of theory application, within a model-theoretic context, two sets of relations are conducive to empirical proliferation: the set of relations between the terms in some theory and their extensions in its various models; and the set of relations between the terms of models (or of only one model) – via an empirical reduct (or empirical reducts) of that (those) model(s) – and the objects and relations of some real system (or systems) conceptualized in one or many empirical models.

Retaining the notion of scientific theories as linguistic expressions at the "top" level of science solves the problems regarding the justification of the existence of many (conceptual) models as interpretations of any one theory by the simple (formal) fact of the incompleteness of formal languages. Thus the possibility of a given scientific theory being interpreted in more than one mathematical model (structure) is natural in a very basic sense in model-theoretic terms. The second proliferation of relations between models and their empirical reducts and between these and empirical models may also turn out to be less counterintuitive than might be supposed at first glance, if it is understood that the possibility of articulating a chain of reference is *not* jeopardized under such circumstances.

Recall now that in model-theoretic realist terms, theories are empirically adequate if and only if they are true in certain models, some of the empirical reducts of which may conceptually encompass the empirical data of the relevant real system. In this sense the first step of the model-theoretic way to confront the model-theoretic overdetermination implied by either the choice of a model for interpreting a particular theory, or the choice of a model in which to embed certain empirical data, is to keep in mind the following structural fact regarding the scientific process. The choice of empirical reduct has to be such that it has embedded in it (an isomorphic copy of) some empirical model in which certain "observation" sentences are true. However, simultaneously, the mathematical model of which this empirical reduct is a substructure must be one that also "makes" or "keeps" true the sentences in the language of the theory that is shown to be empirically adequate.

This characteristic of a model-theoretic analysis of scientific realism ensures that tracing theory-model-reality links – even if presenting a rather complicated undertaking – is still articulable. Simultaneously, however, this also shows the complexity of theory-model-data links. In what follows I claim in particular that an application of non-monotonic default logic to situations of overdetermination of theories by models and data may enable us to formalize and get a grip on this complexity in terms of a particular kind of preferential ranking of these models. My claim is further that this ordering induces an ordering both of empirical

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reducts and models of theories themselves, and may ultimately even result in a ranking of theories.

4. Empirical Proliferation on a Model-Theoretic "Default" Model

The context of looking to non-monotonic reasoning as a possibility of rationalizing model choice is that of abduction.[6] Simply put, in the face of overdetermination of theories by empirically equivalent models, we are faced with a situation analogous to inference to the best explanation, since we have a "theory" but have to choose under certain particular contingent circumstances, out of many options one empirical reduct – and first a model – via which it (i.e. the theory) is linked to a particular empirical model and so to a particular system in reality. Kuipers (1999, p. 307) states that abduction is "the search for an acceptable explanatory hypothesis for a surprising or anomalous (individual or general) observational fact." The fact that our knowledge at the level of empirical models is finite and incomplete and therefore changeable does *not*, however, imply that we cannot discover *some* rational aspects of the kind of abductive reasoning required in this context.

Yoav Shoham (1988, p. 80) points out that in certain issues regarding incomplete information, we should concentrate on distinguishing between the meaning of sentences on the one hand, and our reasons for adopting that particular meaning and no other, on the other. The latter will naturally be outside the domain of the system of logic within which we are working at the time. I agree and acknowledge the contingency of the factors determining the nature – and choice – of a certain model at a certain time. But in my terms this is a matter to be articulated or pinpointed via the empirical models of the theory (about the construction of which admittedly not much can be said external to some particular context of application of the theory in question). Once confronted with more than one empirical model though, I claim we may make use of Shoham's kind of extra-logical motivations to rank these empirical models in a certain order.

Formalizing this is a rather complex task. One way in which to do so might be to take all existing possibilities present at a certain time into account, and summarizing the reasons for picking a certain empirical model – and so a particular empirical reduct of a certain model – at a certain time in such a way that the existence of other models – and other empirical reducts – is not denied, but simply, for a certain period of time, put on hold, as it were. A method for doing

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[6] Heidema and Burger (forthcoming), p.1 note Paul's (1993) remark that abduction is often related to conjecture; diagnosis, induction, inference to the best explanation, hypothesis formulation, disambiguation, and pattern recognition.

this is offered to us by the nature of non-monotonic logic in general. In particular for our purposes here Shoham's model-theoretic non-monotonic logic is preferable, since it offers a fairly simple way of ranking models, which perhaps is not as adequately possible in other versions of non-monotonic logic.[7]

The general idea behind Shoham's reasoning that I find has some appeal in our context is that it is necessary sometimes to take "decisions" in our reasoning, while ignoring some information that is potentially relevant, but at the same time accepting or expecting to "pay the price of having to retract some of the conclusions in the face of contradicting evidence" (1988 p. 80). The trick is to have some rational way of keeping track of these retractions. Traditionally, logic is concerned with cautious and conservative reasoning. It finds its natural home in mathematics, the theorems of which are immune to fashion and the passage of time. But life in general and science in particular need more than mathematics – we need common sense and contextualization. This involves the capacity to cope with situations in which one lacks sufficient information for one's decisions to be logically determined, so that one has to try to distinguish between possibilities that are more plausible (i.e. "normal") and those that are less plausible at a given time.

Shoham (1988), pp. 71-72 sets out his non-monotonic scheme as follows:

> The meaning of a formula in classical logic is the set of interpretations that satisfy it, or its set of *models*[8] ... One gets a non-monotonic logic by changing the rules of the game, and accepting only a subset of those models, those that are 'preferable' in a certain respect (these preferred models are sometimes called 'minimal models' ...). The reason this transition makes the logic non-monotonic is as follows. In classical logic $A \rightarrow C$ if C is true in all the models of A. Since all the models of $A \wedge B$ are also models of A, it follows that $A \wedge B \rightarrow C$, and hence that the logic is monotonic. In the new scheme we have that $A \rightarrow C$ if C is true in all *preferred* models of A, but $A \wedge B$ may have preferred models that are not preferred models of A. In fact, the class of preferred models of $A \wedge B$ and the class of preferred models of A may be completely disjoint! Many different preference criteria are possible, all resulting in different non-monotonic logics. The trick is to identify the preference criterion that is appropriate for a given purpose.

In other words, inference from uncertain laws is non-monotonic since additional knowledge may make previously derived consequences underivable (Schurz 1995, p. 287).

The process of making informed guesses on the basis of a mixture of definite knowledge and default rules is called defeasible reasoning. The word "defeasible"

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[7] For instance: Clark's (1978) predicate completion, Reiter's (1980) default logic, McDermott and Doyle's (1980) non-monotonic logic, McCarthy's (1981) circumscription, or McDermott's (1982) non-monotonic logic II. See also Ginsberg (1987), Kraus, Lehmann and Magidor (1990), and Shoham (1987).

[8] Where 'interpretation' means "truth assignment for [propositional calculus], a first-order interpretation for [first-order predicate calculus], and a <Kripke interpretation, world>-pair for modal logic." (Shoham 1988, pp. 71-72)

reflects the fact that our guess may turn out to be wrong, in other words that the default rule may be "defeated" by exceptional circumstances, or a change of circumstances caused by a change in the content of our knowledge. Defeasible inferences are inherently non-monotonic, since amending our system of knowledge might change our conclusions.

As an example of the need to go beyond the irrefutable logical consequences of one's definite information, consider a simple physical light-fan system.[9] Say we take an ordinary two-valued propositional language with atoms p and q, where p: the light is on, and q: the fan is on. p can be T/F (1/0) or q can be T/F (1/0) such that the four possible states of the system are depicted by the set $\mathbf{W} = \{11,10,01,00\}$ (where a specific valuation depicts a specific state of a system). Say, now, that we determine theoretically that it is the case that $p \vee q$, this reduces the frame of our language to $\{11, 10, 01\}$. Then we – or some of us at least – discover say, in reality, that we can see whether the light is on, but are too far away to see or hear whether the fan is on. Thus we have limited knowledge about the system. Now suppose the system is really in state 11, i.e. that the light and the fan are both on. We will know only that the light is on, i.e. that p is the case, not that both components are on, i.e. not that p and q are both the case. Our definite knowledge suffices to cut our current frame of states down even more to the frame consisting of the models of p, i.e. $\mathrm{Mod}(p) = \{11, 10\}$. So far, so good. Where's the problem?

Suppose we urgently need to know what the state of the system is, because state 10 is an unwanted state for whatever reason. This implies that we want to cut down the frame $\mathrm{Mod}(p) = \{11, 10\}$ to a frame with just one element in it. We need to go beyond our definite (although incomplete) knowledge, but without making blind guesses. How can we do this in a reasoned way? We can use a default rule such as "Experience and descriptions of the system have shown that when the light is on, the fan is normally on too" to make the informed guess that the state is actually 11.

Exactly how do default rules justify cutting down the set of models of our definite knowledge though? Or rather, what would we be willing to regard as a default rule? After all, not every rule of thumb can be taken seriously as a default rule. The standard representation of "meta"-information – motivating choices scientists make at given times (in our case), and distinct from "sentential" information about aspects of real systems – is as a relation on the set of states – or possible worlds – (of a system).[10] (In the context of our example, the possible

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[9] This example is borrowed from discussions with Willem Labuschagne from the Department of Computer Science at Otago University, Dunedin, New Zealand.

[10] There are two approaches to ordering possible worlds: by using numbers, or without using numbers. The best known numerical ways are those using *fuzzy sets* or using *probabilities*. Neither of these would give us the kind of formal mechanism I am looking for in the current context.

worlds are just the states of the system, namely **W** = {11, 10, 01, 00}.) In the case of the minimal model semantics related to non-monotonic logics this relation is a preference relation and is depicted as a "total pre-order," which is a reflexive, transitive relation capable of effecting comparisons between arbitrary elements. Intuitively, such relations are thought of as allocating states to levels of normality, or preference.

Shoham (1988) requires that a default rule should be expressible as such an ordering on possible worlds (or models). He focuses on using non-numerical default rules, such as the rule "11 is more normal than 10, which in turn is more normal than 01 and 00" as the basis for "informed guesswork". All we require is that the rule arranges the states of the system in levels, with the most normal states occupying the lowest level, then the next most normal states, and so on, until the least normal, least typical, least likely states are put into the top level. The given rule yields the ordering:

01   00
10
11

Now we can choose between the two models of p in our previous example, because 11 is below 10. Our choice reflects not merely our definite knowledge that p is the case, but also our default knowledge that 11 is a more preferred state of the system than 10 is (by the default rule stated above). (See the Appendix for formal definitions.)

In summary, default rules may be used to justify defeasible reasoning as follows: order the possible states of the system from bottom to top in levels representing decreasing preference; given definite knowledge α, look at the states in Mod(α) – the set of all models of α; pick out the states in Mod(α) that are *minimal*, i.e. lowest in the ordering; then any sentence true in each of these minimal models of α may be regarded as plausible, i.e. as a good guess. So, whereas α classically entails β, i.e. α → β, when among ALL the models of α no counterexample to β can be found, α defeasibly entails β when among all the most PREFERRED models of α no counterexample to β can be found.

Note though that a default rule is not an absolute guarantee. Our informed guess may turn out to be wrong. Normally if Tweety is a bird then Tweety is able to fly. But exceptional circumstances may defeat the default rule. Tweety may be a penguin or an ostrich. Tweety may be in Sylvester's tummy. Abnormal states or a change in the content of the body of knowledge concerning a certain situation can sometimes occur. That is why, after all, in such cases we call our reasoning "defeasible."

Now, back to the context of science, given all of the above, the possibility of after-the-fact semantic reconstructions of reference links from theories to some

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real systems formulated with the help of model theory and non-monotonic logic offers a way to get us out of at least some of the apparent difficulties implied by overdetermination and empirical equivalence in the model-theoretic way as follows. In the scientific context I claim that a default rule containing at least the following two conditions – or orderings – might be useful. The first condition induces an ordering or ranking of empirical models in terms of precision or accuracy. This condition has to do with the highest quality of data and the finest level of technology. For now, I am considering cases here where we have to choose among different equivalent empirical models, all of which may be embedded into the same reduct, or at least empirical reducts of the same type. The second condition that I would include in my default rule is also more often concerned with a choice of empirical reduct, together with a choice of empirical model, since here the condition implies a ranking of empirical models are preferred that may induce a ranking of empirical reducts. Here the rule states that empirical models are preferred that can be embedded into empirical reducts of a type that contains a larger class of empirical terms from the theory than others.

The second condition has two noteworthy implications. First it shows how such a ranking distinguishes between weaker and stronger links between theories and reality, since a theory that is model-theoretically linked to an empirical model embedded into an empirical reduct containing a larger class of empirical terms than others, may be said to be more effectively "about" some real system than would otherwise be the case. Also, in terms of the progress of science it might be preferable to have a mechanism for justifying the inclusion of previously exogenous factors as endogenous ones in a particular model of a theory. This becomes possible if we enlarge the type of empirical reducts.

If we combine both these conditions together in one default rule, we may find that the resulting rankings of empirical models induce rankings of empirical reducts, which might induce rankings of models themselves, which may ultimately induce rankings of theories. Let us look at a simple example, again in terms of our light-fan system. Theory: $p \vee q \equiv \mathrm{T}$

- Empirical situation: Only the light can be observed
  This implies that
    - p: empirical term
    - q: theoretical term

Models of T	Empirical Reducts	Empirical models
11	1-	1-
10	1-	
01	0-	

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- The observation of the light in an on position cancels the empirical reduct 0-, which in turn cancels the model 01
- Our choice of empirical model thus induces the following ordering of empirical reducts:

  0-
  1-

  and the following ordering of models:

  11 10

- This changes our theory to $T' \equiv p$
- Suppose the empirical situation is enhanced by developments in technology and we can observe that whenever the light is on the fan is off. Then our frames of models become

Models of T'	Empirical Reducts	Empirical Models
11	11	10
10	10	

- The result of our observations now is that the empirical model "cancels" the empirical reduct 11, and this, in turn "cancels" the model 11
- Our new enhanced empirical model now induces the following ordering of empirical reducts:

  11
  10

  and the following ordering of models:

  11
  10

- This changes our theory to $T'' \equiv p \wedge \neg q$

Recall that, given my view of scientific progress, generally theories change much more slowly than models. Specifically, theory changes usually occur only when the possibility of changing and modifying the models of the theory concerned has been exhausted, which confirms the continuity of scientific knowledge. This view may be viewed as a different kind of "multi-level" view than the one that Kuipers (2001, Chapter 2) advocates. The difference in terms of a model-specific notion of truth and a notion of approximate truth is not important here, what is important is the acceptance of the fact that science's processes are realized at different levels.

Returning to the conclusions I draw from the above, I claim that non-monotonic default rules and consequent rankings enable us to reduce the available

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– or possible – choices of models, empirical reducts, and empirical models. This kind of analysis offers a method for gaining an articulable grip on empirical equivalence of any kind. The mechanism of non-monotonic logic fulfils what Kuipers (1999, p. 307) calls the "main abduction task," i.e. "the instrumentalist task of theory revision aiming at an empirically more successful theory, relative to the available data, but not necessarily compatible with them," although this is done here mostly through revision – or change – of relations of empirical adequacy, implying possible revision of choices concerning empirical models, empirical reducts and (conceptual) models. Although the above application of non-monotonic logic starts at a finer level of analysis than is usually the case in non-monotonic contexts (where we simply look at rankings of the states – models – of the system in question), the model-theoretic structuring of relations between models, empirical reducts, and empirical models makes possible the kind of "carrying over" of rankings that I have set out above.

Notice that relations of empirical adequacy are thus temporary and contextual, as Laudan and Leplin (1991) also concluded in their 1991 article *Empirical equivalence and underdetermination*. Science progresses fastest at the level of empirical models, but continuity is ensured by the fact that these models remain conceptualizations of observations, even if these observations are also contextual. The point of a model-theoretic realism is exactly that, instead of offering simply one intended model of "reality," a theory is depicted as a way of constructing or specifying a collection of alternative models, each of which may represent, explain, and predict different aspects of the same real systems (or different ones) via the same or different empirical reducts isomorphically linked to the same or different empirical models.

Above we have mostly concentrated on cases of empirical equivalence in terms of model-theoretic overdetermination. What – in terms of realist concerns – about underdetermination in the traditional (Laudan/Leplin) sense? (I.e., different theories, same empirical model.) In this sense – in a realist context – a scientist can "know" – or at least determine – that she is working with the "same phenomenon", even if using "different" theories or "different" models, because of the possibility of analyses that a model-theoretic realism offers of the different empirical links between different empirical models of different (conceptual) models of (perhaps) different theories. Detailed analyses of these empirical links will reveal common factors on the reality side of the link (e.g. light blobs observed through different telescopes by different people at different times indicating – by careful analyses – a common factor called "Neptune") which entails the "same phenomenon." And, moreover, cases where the same empirical model is embedded in different empirical reducts also show the continuity of science at the empirical level. Kepler took Brahe's precise empirical observations, i.e. the empirical data forming the empirical model of the theories in terms of

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celestial spheres that Brahe worked with, and fitted these data – i.e. Brahe's empirical model – into his (Kepler's) theory in terms of elliptical orbits. Applying non-monotonic logic within a model-theoretic context also may help to minimize traditional underdetermination of theories by models and data within a context of scientific progress, since it leads to choices of more accurate, more encompassing (empirical models and so) empirical reducts, and in certain cases it may even help to eliminate certain models or, ultimately, even theories.

5. Conclusion

Thus, even in the face of the fact that our fallible sensory experience and the finiteness of experimental data at a given time indicate that our knowledge of reality at such a time is limited, contextual, and temporary, we *can* rationally discuss the choices we make concerning so-called "empirically equivalent" models *and* keep track of changing theory-observation distinctions. It might be then possible, after all – contrary to Popper – to give some kind of rational motivation for the so-called "creative" leap that we make from data to theories. Kuipers (2001, Chapter 10) also comments that "… discovery, contrary to traditional opinion in philosophy of science, is accessible for methodological analysis …" (p. 287), although he chooses to show this by his distinction between different kinds of research programs and explores relations between discovery, evaluation and revision by means of computational philosophy of science mechanisms. A non-monotonic logical analysis of empirical model choice admittedly does not "simulate" the "processes in the minds or brains of scientists" Kuipers (2001, p. 290), but rather it makes sense of the motivations underlying certain of these scientists's actions, based on the status and development of the knowledge claims they make.

I do not necessarily agree with Kuipers' claim (2001, p. 201) that "the realist ultimately aims to approach the strongest true hypotheses, if any, i.e. the theoretical-cum-observational truth about the subject matter". Perhaps this may be said to be the case for a certain kind of realist. A realist with a more sophisticated, moderate view of science and its processes ultimately aims at establishing reference relations between terms in theories and entities in real systems and is content with acknowledging that questions of truth are contextual and temporary matters. Questions of truth cannot be settled before questions of reference are settled. Accepting this will go a long way towards accepting the contingent and defeasible nature of science without harming the (realist) status of scientific theories in any important way. Recall also my emphasis on the re-interpretability of the language of science, or of theories in particular, and then it will be clear that claiming *model-theoretic reference* is sufficient to establish *some* form of

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realism, since in this *referential semantic* sense it *can* be shown that unobervables "exist" in real systems (i.e. terms in theories might after all be shown to refer to them). The contextually empirical terms refer directly, and the contextually theoretical terms indirectly, "by implication," via their conceptual and logical links to the empirical terms established by the theory. Some philosophers might be scornful about this kind of "weak" realism, while actually this realism is "weak" only because "strong" means traditional metaphysical realism. "Weak" means non-absolutist, and in that sense model-theoretic realism (supported by a non-monotonic semantics) is much stronger and more flexible than typical metaphysical scientific realism.

In general, then, I conclude that scientific theories may indeed say something about reality, but it is not possible when faced with an *uninterpreted* theory and possibilities of overdetermination of the theory by both data and models to determine or claim that it will definitely or uniquely be applicable to a certain aspect of reality and to no other. The model-theoretic notion of articulated reference and truth, augmented by non-monotonic mechanisms to get a grip on empirical overdetermination, may render the process of science expressible to rather finer and more accessible detail than may be possible on other accounts of science. When reference is traced via model-theoretic relations between theories, models, and data, and extra-logical default rules are used to formally order our choices in a rationally responsible way, Quine's inscrutability of reference becomes an even vaguer notion than before. Hence reference – at least in this sense – does not appear to be indeterminate after all. Secondly, this implies that the content of the meta-verification procedures for the processes of science cannot be given uniquely, but is rather a result of the context-specific actions and constructions of human scientists. In other words, theory-observation distinctions – or the definition of *c*-rules – remain somewhat less precise than one might wish in a positivist sense, but overall at least these distinctions remain articulable in the model-theoretic sense – which is more important for the success of a realist quest.

It might be that a model-theoretic realism aided by a non-monotonic ranking of models (empirical reducts and empirical models) offers, at least partly, some response to Laudan and Leplin's (1991) concerns about the "collapse" of epistemology into semantics in terms of traditional underdetermination and empirical equivalence issues, taken almost as two sides of the same coin. Non-monotonic default rules are extra-logical and are determined by the state of knowledge of a system at a particular time (i.e. "the agent knows that the light is on"). The new perspective on the consequence (entailment) relation that non-monotonic semantics offers might thus present us with a different way of looking at Laudan and Leplin's (1991) claim that evidential support for a theory should not be identified with the empirical consequences of the theory.

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To conclude this article I review a model-theoretic realism according to the five questions Kuipers asks in the beginning of *From Instrumentalism to Constructive Realism* (2000, Chapter 1, pp. 3*ff*) in order to show the common features and the differences between such an approach and that of Kuipers' constructive realism. The first question is "Does a world that is independent of human beings exist?" I agree with Kuipers that a positive answer to this question – especially in a philosophy of science and a realist context – interprets the question as "does a non-conceptualized natural world that is independent of human beings exist?" Both constructive realism and model-theoretic realism answer positively to the latter, and it is granted that the nomic version of this form of ontological realism is stronger than the actual one, since in such a case it is not only a particular actual possibility that is conceptualized, but rather many nomic ones.

The second question (the first of four epistemological ones) is "Can we claim to possess true claims to knowledge about the natural world?" (p. 3). Again I agree to interpret this question as asking whether "we can have good reasons for assuming that certain claims, by definition phrased in a certain vocabulary, about the natural world are true in some objective sense, while others are false" (p. 4). A supporter of model-theoretic realism will answer positively, but will qualify "some objective sense" as a methodological sense – i.e. the model-theoretic way to "trace" references to entities and relations in real systems – since such a supporter will believe in the actual contingency of such links. Thus both model-theoretic realism and constructive realism are forms of epistemological realism.

The third question Kuipers poses is "Can we claim to possess true claims to knowledge about the natural world beyond what is observable?" (Kuipers 2000, p. 4). Again, this should be interpreted as Kuipers (p. 4) claims, as asking whether more than observational knowledge is possible. Here I think Van Fraassen is correct in believing that the point in this context is not whether theoretical terms refer or not, or whether proper theories are true or false, as Kuipers (p. 5) points out. It is true that the point is rather whether theories are empirically adequate – or, in Kuipers' sense observationally true. The model-theoretic point, though, is that determining empirical adequacy is important since it is the *final* step in articulating the referential link between terms in theories and entities and relations in real systems. Determining empirical adequacy is not only *not* all that matters (as defenders of Van Fraassen's view claim), but also cannot be done – at least in a realist context – without certain preceding steps in terms of the construction of models interpreting the language in which theories are formulated (set out in Section 4).

The fourth question is "Can we claim to possess true claims to knowledge about the natural world beyond (what is observable and) reference claims concerning theoretical terms?" (p. 6). Here I classify model-theoretic realism with

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Cartwright and Hacking's referential realism, since an advocate of the former will also claim that "entity and attribute terms are intended to refer, and frequently we have good reasons to assume that they do or do not refer" (p. 6), although I do not support the metaphysical form of realism that Cartwright seems to favor in her later writings (e.g. Cartwright, 1989, 1994).

The final question that Kuipers considers is "Does there exist a correct or ideal conceptualization of the natural world?" (p. 7). My answer is no, and so is Kuipers'. Given the contingency and defeasible nature of our knowledge claims, linked as they are to disciplinary matrices and everything this entails, no other answer is possible. I agree with Kuipers that

> [v]ocabularies are constructed by the human mind, guided by previous results. ... one set of terms may fit better than another, in the sense that it produces, perhaps in cooperation with other related vocabularies, more ... interesting truths about the domain than another. The fruitfulness of alternative possibilities will usually be comparable, at least in a practical sense ... . There is however no reason to assume that there comes an end to the improvement of vocabularies (p. 8).

My point here is that representing a real system from a different perspective – i.e. linking some theory model-theoretically to a different empirical model than before – can augment the content of our knowledge claims regarding that system, but are not necessarily an "improvement" on the claims generated by the first linkage, although in both cases we can speak of "contextual" truth or truth of the theory in the particular chosen model.

6. APPENDIX: Formal Definitions

***Definition* 6.1.** Let G be any set. A relation $R \subseteq G \times G$ is a *total preorder* on G iff

- R is *reflexive* on G (i.e. for every $x \in G$, $(x, x) \in R$), and
- R is *transitive* (i.e. if $(x, y) \in R$ and $(y, z) \in R$, then $(x, z) \in R$), and
- R is *total* on G (i.e. for every $x \in G$ and $y \in G$, either $(x, y) \in R$ or else $(y, x) \in R$.

***Definition* 6.2.** Let L be a propositional language over some finite set **A** of atoms. Let **W** be the set of all local valuations of L (i.e. functions from **A** to {T, F}). A *ranked finite model* of L is a triple M = (G, R, V) such that

- G is a finite set of possible worlds,
- R is a total preorder on G, and
- V is a labelling function from G to **W**.

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By a *default* model of L we understand a ranked finite model (G, R, V) in which G = **W**, R is a total preorder on **W**, and V is the identity function (i.e. V(w) = w for all w∈**W**).

***Definition* 6.3.** Suppose that L is a propositional language over a finite set **A** of atoms, and that M = (G, R, V) is a ranked finite model of L. Given a sentence α of L and a possible world *x* ∈ G, the following rules determine whether M satisfies α at x:

- if α is an atom in **A**, then M satisfies α at *x* iff the valuation V(*x*) assigns to α the truth value T;
- if α is ¬β then M satisfies α at *x* iff M does not satisfy β at *x*;
- if α is β∧γ then M satisfies α at *x* iff M satisfies both β and γ at *x*;
- if α is β∨γ then M satisfies α at *x* iff M satisfies β at x or γ at *x*;
- if α is β→γ then M satisfies α at *x* iff M satisfies ¬β at *x* or satisfies γ at *x*;
- if α is β↔γ then M satisfies α at *x* iff M satisfies both β and γ at *x* or satisfies neither at *x*.

***Definition* 6.4.** Suppose L is a propositional language over a finite set **A** of atoms, and that M = (G, R, V) is a ranked finite model of L. Let α and β be any sentences of L. The sentence α *defeasibly entails* β iff M satisfies β at every possible world x such that

- M satisfies α at *x*, and
- *x* is minimal amongst the worlds satisfying α, i.e. there is no possible world y of M such that α is satisfied at y and (*y*, *x*) ∈ R and (*x*, *y*) ∉ R.

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