On truth and reference in postmodern science

Emma Ruttkamp

Department of Political Sciences and Philosophy
Discipline of Philosophy
University of South Africa
PO Box 392, 0003 UNISA
E-mail: <RUTTKEB@unisa.ac.za>

Abstract:
If the defenders of typical postmodern accounts of science (and their less extreme social-constructivist partners) are at one end of the scale in current philosophy of science, who shall we place at the other end? Old-style metaphysical realists? Neo-neo-positivists? ... Are the choices concerning realist issues as simple as being centered around either, on the one hand, whether it is the way reality is "constructed" in accordance with some contingent language game that determines scientific "truth"; or, on the other hand, whether it is the way things are in an independent reality that makes our theories true or false? If, in terms of realism, "strong" implies "metaphysical" in the traditional sense, and "weak" implies "non-absolutist" or "non-unique", what – if anything – could realism after Rorty's shattering of the mirror of nature still entail? In accordance with my position as a model-theoretic realist, I shall show in this article the relevance of the assumption of an independent reality for postmodern (philosophy of) science – against Lyotard's dismissal of the necessity of this assumption for science which he interprets as a non-privileged game among many others. I shall imply that science is neither the "child" of positivist philosophy who has outgrown her mother, freeing herself from metaphysics and epistemology, nor is science, at the other end of the scale, foundationless and up for grabs.

1 Non-classical logics

The interpretation of philosophy of science, and so, by implication, of science, that I shall offer in this paper might be characterised as postmodern mainly in so far that it accommodates a certain notion of diversity and that it posits and deals with various possibilities of relationships between theory and empirical practices. However, a distinction is still made between the content of science (i.e. its theories, methods, and reasoning) and the context in which science is practised, although it is *not* only the content of science that is taken to be fundamental for understanding the processes of science. On the other hand, the account offered here may turn out not to be postmodern in any way, since my interpretation depicts the history of science as a realist narrative of rationality, progress, and truth which does perhaps under a certain interpretation serve to legitimate the products of science. The settling of this point is somehow though not really crucial to me here and also this article is not primarily meant as a critique of Lyotard's (or anyone else's) notion of science. Rather what is important is the objective of this article to offer an alternative possibility to make sense of what we have now realised in terms of the varied constructed nature of science in general and the implications there-of for philosophy of science in particular.[1]

My claim is that the defeasible nature of scientific knowledge does not necessarily presuppose the abandonment of philosophy in relation to the knowledge claims made by science, it merely means the abandonment of a *foundationalist interpretation* of philosophy in relation to science. In science we need *doxai* to get to epist'm', and, epist'm' is not the absolute unbreakable product that some have taken it to be, but rather something much more human. However in my context this implies no form of playful relativism, but rather a new understanding of the relationship between science and philosophy showing us that scientific knowledge is "certain" and "true" and "refers" to reality in qualified contextual ways that are traceable with the aid of certain variations of formal semantics in general and some forms of contemporary non-classical logics in particular. My analysis here is semantical, and in particular the basic tenet of the model-theoretic realism I advocate is that the same language terms can refer to more than one entity in some models of the relevant language (theory), and also, in the opposite direction, the same object – or range of objects, or relations between objects – in some real system can be referred to by more than one model, and, most importantly, these relations of reference can be traced, or articulated, by using Tarskian model-theoretic tools. And the multi-interpretability of scientific theories is the realist counter – surprisingly enough perhaps – to the abstract nature of science.

A final objective of this paper is to show that, contrary to traditional characterisations of logic as the archetype of rigidity and absolute truth, tying logical analyses to exact – in the sense of "unique" – determinations of the meaning of linguistic expressions cannot succeed, given the fact that these determinations are contingent on the nature of the very models they help define. This does not cut out logic from our depiction of science and its philosophy at all though, as we shall see below.

My views regarding the nature of scientific knowledge fits well with the following characterisation of science and its method(s) offered by Illka Niiniluoto (1999: 5). He depicts science as a "local belief system" comparable with other methods of acquiring beliefs about the world such as myths, religion, metaphysics, and even common sense (1999: 5). He describes science as "a source of cognitive attitudes about the world, characterised by its reliance on the self-corrective scientific method [a Peirce-ian notion]" (1999: 4), and claims that "for the most part, scientific activities do not involve belief in the sense of holding-to-be-true: rather ... [based on certain assumptions or so-called 'background knowledge'] scientists propose hypotheses and pursue research programmes in investigating the limits of the correctness of their theories" (1999: 5). He concludes that science, if successful, "will ... have tentative results, in principle always open to further challenge by later investigations, which constitute what is usually referred to as the 'scientific knowledge' of the day" (1999: 5). Scientific knowledge, in these terms, is defeasible and may be represented in many different ways, but it is also cumulative and rational.

In the remainder of this section I shall explore recent developments in formal semantics and knowledge representation to establish that relationships between logic and philosophy in the context of reflections on science – yes, and even in terms of

1 See also in this context Hennie Lötter's (1994: 155-156) discussion of for instance Peter Galison's philosophy of science in terms of postmodern content.

"postmodern" science – are alive and well. In Section 2 I shall briefly show how a model-theoretic preferential analysis of science can offer the possibility of a rational discussion of science and its processes. This is possible even if science is analysed in terms of non-unique interpretations and complex clusters of model-specific theory applications. In Section 3 we shall see that there is a way in which truth and reference are still intelligible albeit in a heavily qualified way, and finally in Section 4 I shall discuss some postmodern ideas on philosophy and science.

Now, let us briefly consider the role of logic in studies of knowledge representation of real systems. The notion of a formal language has its foundation in Frege's 1879 *Begriffsschrift*, in which he developed Leibniz's notion of a "calculus of signs, an artificially constructed language having a precisely defined grammar and unambiguous sentences" (Heidema & Labuschagne ,1999). Frege used his notion of a formal language to construct a foundation for mathematics, and to show that the truth of mathematics follows from universal logical principles. The objective of logicism was thus the construction of "one large and complex formal language, the universally valid sentences of which would represent the basic truths of mathematics" (Heidema & Labuschagne, 1999) – much as the universal meta-language as proposed by philosophy in its traditional foundationalist sense, was by some supposed to represent the basic truths of natural science. By the beginning of the 20th century knowledge representation was still caught up in Russell's rigid logical atomist paradigm, according to which the denotation of constants and the consequent meaning of sentences were taken to be uniquely assigned. In their continuance of Frege's programme Russell and Whitehead however, came up against "legitimate mathematical assumptions that were not universal logical principles, most notably the axioms of infinity and choice" (Heidema & Labuschagne, 1999).

These anomalies eventually contributed to a shift in the focus of logic studies towards sentences that could be regarded as representations of knowledge of *particular* systems *without* being universally valid. These sentences were not taken to belong to *one universal super* language, but rather to *different calculi*, "each having a vocabulary designed to suit the representation of knowledge about the components of [some relevant] ... system" in reality (Heidema & Labuschagne, 1999). The most important consequence of these developments was the acknowledgement that each formal language "admits a large collection of possible interpretations" (Heidema & Labuschagne, 1999). By the nineteen fifties Alfred Tarski's model theory, and his views in particular on truth and logical consequence, had matured into a definition of truth in terms of relations between sentences and interpretations. His studies of the properties between sets of sentences and classes of interpretations opened up new horizons for studies in formal logic in general, and knowledge representation in particular.

This new development in studies in knowledge representation and its application in non-classical logics undermine the connotation of "absoluteness" traditionally given to the word "knowledge" that used to rule reference to "laws of nature" (Heidema & Labuschagne 2001). Given my view of science as a body of defeasible knowledge claims standing in certain time-bound relationships to reality, I advocate applications of contemporary non-classical artificial intelligence logics (such as non-monotonic logic, epistemic modal logic, and temporal logic) to defeasible scientific knowledge without giving up on rationally accounting for either the processes of science in general or, in particular, for the motivations behind particular choices for certain representations of real systems above others at certain times. I view the application of these non-classical logics to the problem of partial or defeasible knowledge, not in the sense of knowledge of individual agents as is the case in artificial intelligence and cognitive science applications, but rather in the sense of representing knowledge in terms of the various contexts in which the processes of science take place – covering the whole spectrum from very general disciplinary matrices as background to a certain theory or set of theories, to much more particular empirical models representing aspects of real systems.[2]

For reasons of space I shall now briefly describe only the structure of a non-monotonic logic, in particular one akin to Yoav Shoham's. A non-monotonic logic consists (for our present expository purposes) of a propositional language over a finite set A of atoms, together with a minimal model semantics. This semantics allocates truth values to sentences with the aid of the usual valuations, but uses a total pre-order on the set of valuations to define a new semantic consequence relation between sentences, namely the defeasible entailment relation.[3] This entailment relation represents the "key distinction between defeasible and indefeasible inferences" (Ginsberg, 1987: 9) since it makes explicit the difference between monotonic and non-monotonic reasoning. In classical logic A ⊨ C if C is true in all the models of A, however unwanted or inapplicable. Moreover, since all the models of A B are also models of A, it follows that A B ⊨ C, and hence that an increase in the knowledge represented by the antecedent of an entailment relation in classical logic does not invalidate the knowledge represented by the consequent of the relation, and so classical logic is "monotonic".

In line with the fact that "defeasible conclusions may need to be retracted in the presence of additional information" (Ginsberg, 1987: 9), in a non-monotonic framework we have that A |~ C if C is true in all *preferred* models of A, which implies that we choose only a subset of the models of A, according to some preference we have for them at a given time. Furthermore, in terms of a change perhaps of our knowledge of the system at issue, A B may have preferred models that are not preferred models of A, and so the consequents of A are not necessarily included anymore among those of A B in the non-monotonic context. The meaning of a formula in classical logic is the set of interpretations that satisfies it, or its set of models. In the context of a non-monotonic logic we only accept a subset of those models, that is, those that are 'preferable' in a certain respect (these preferred models are sometimes called "minimal models").

The main idea is that an agent (a community of scientists working in some disciplinary matrix) may have two kinds of knowledge (Heidema & Labuschagne, 2001): sentential information about the aspects of the real system at issue, and which may be expressed in the "designer-built vocabulary" of the relevant formal language (or calculus) (Heidema & Labuschagne, 2001); and heuristic meta-information depicted in terms of so-called "default rules" in non-monotonic terms, and motivating certain

2 I choose the semantics related to non-classical logics above numerical semantics (such as probability theory in any form (either pure or applied), many-valued logics, and fuzzy logic) since I view the nature of the processes of science and of scientific knowledge as too supple to always allow numerical assignment of values to choices made in scientific reasoning. Another reason why I prefer for instance the minimal model semantics of non-monotonic logics to numerical semantics in the context of scientific knowledge: the notion of preference underlying default rules (represented in terms of total pre-orders) is comparative rather than absolute (Heidema & Labuschagne, 1999).

3 See the formal definitions given in the Appendix.

choices the agent/scientist makes at a given time. Notice that there is no grand scheme of absolute knowledge ‘serving’ these agents as it were, but rather that meta-information here is a local changeable concept.

The standard representation of meta-information is as a relation on the set of states [of a system]. In the case of the minimal model semantics related to non-monotonic logics this relation – which determines defeasible entailment in a given context – is a preference relation on states (worlds, models) and is depicted as a “total pre-order”, which is a reflexive, transitive relation capable of effecting comparisons between arbitrary states. Intuitively, such relations are thought of as allocating states (of some real system) to levels of normality, or preference. Total pre-orders are the formal expressions of non-numerical default rules stating preference for certain models above others.

EXAMPLE 1: A LIGHT-FAN SYSTEM I[4]

- A physical light-fan system
- A two-valued propositional language with atoms p and q, where
  - p: “the light is on”
  - q: “the fan is on”
    and
  - p can be T/F (1/0) or q can be T/F (1/0)
  - and where
  - a specific valuation depicts a specific state of the system
  - such that the four possible states of the system are depicted by 11, 10, 01, 00
- Say we theorise that p   q
  - This presents us with a reduced frame of the language containing 11, 10, 01
- Say the empirical situation is such that we can only observe the light, and we determine that the light is on
  - This presents us with a further reduced frame containing only 11 and 10
- But, say now when reading a description of the light-fan system we determine that whenever the light is on, so should the fan be
  - This presents us with only one most likely state of the system, namely 11

Hence, the default rule “Experience and background knowledge have shown that usually when the light is on, so is the fan”, presents us with the following ordering of states:

01   00
10
11

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” 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. “Thus minimal model semantics provides one way to make precise the notion of

4 Example from Ruttkamp (2003).

a defeasible belief: a sentence supported by the agent's knowledge in the sense of being true in the minimal models of that knowledge” (Heidema & Labuschagne, 1999).

In terms of philosophy of science the above offers a mechanism for showing and expressing the fact that we do sometimes reflect on knowledge, and its acquisition, communication, and representation from some meta-stance, *but* that such stances are *local*, NOT in the sense that they can only merely be reduced to context, but rather, also, in the sense that they represent amendable or defeasible viewpoints.

2.1 Application: the problem of over-determination

(a) A model-theoretic view of science[5]

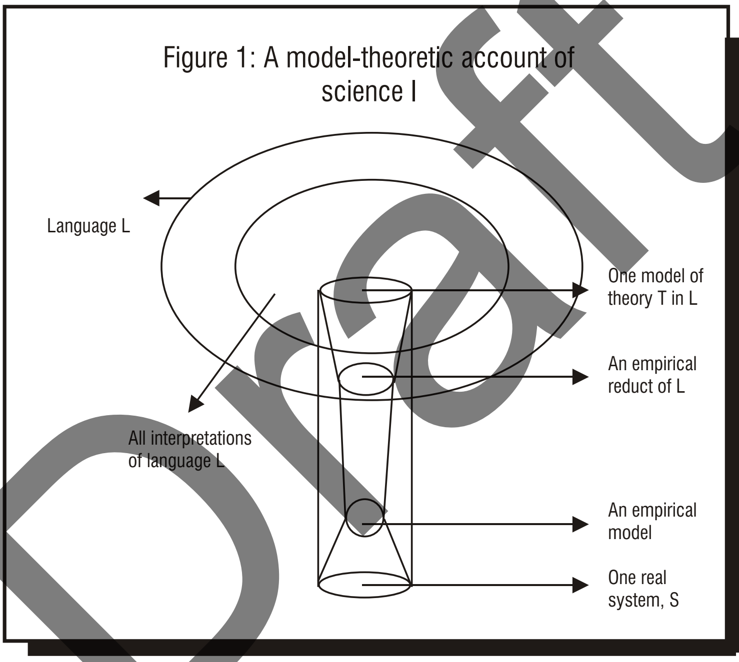

Figure 1: A model-theoretic account of science I

We know from the structure of mathematical model theory and its definitions of interpretations of (sentences in) formal languages of the possibility of many different models of a given theory T (in language L). These models are interpretations of T's language in the Tarskian sense and the choices between them are determined by – among other factors – the research intentions and thematic preferences of the scientists applying T within some accepted Kuhnian disciplinary matrix, or “against” some background meta-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, when focusing on a particular real system at issue in the context of applying a theory, which in its 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

5 Figure from Ruttkamp (2002).

in the mathematical model of the theory under consideration that may be termed "empirical" predicates (in the particular context of application).

This is how 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, from the experimental activities carried out in relation to the real system focused on at a particular time, a conceptualisation of the results of these activities, i.e. of the data resulting from certain interactions with this system (such as performing certain experimental tests), may be formulated. This (mathematical) conceptualisation of data is represented as an empirical model. Then, we may find that there is a one-to-one isomorphic embedding function from the empirical model into the empirical reduct in question, which would imply that there exists some relation of reference between our original theory and the real system we are considering. 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 summarise: Interpretative models interpret all terms in the appropriate relevant language in which a given theory is formulated, and satisfy the theory at issue.[6] In empirical reducts are interpreted only the terms called "empirical" in relevant contexts of application or empirical situations. Think of these substructures of the interpretative models as representing sets of all atomic sentences expressible in the particular empirical terminologies true in the interpretative models. Empirical models – still mathematical structures – can be represented as finite subsets of these sets of atomic sentences, and contain empirical data formulated in the relevant language of the theory.[7]

In these terms 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. Hence, model-theoretically speaking, theories are *over*-determined by data and by their individual models. Let me first clarify what I mean by the term "over-determination", building on my comments in the previous sections. "Empirical proliferation" or the "over-determination" of theories by models is in a sense the reverse of the traditional under-determination of theories by data scenario.[8] In the context of under-determination of theories by data, the bottom line is that empirical data are too incomplete to determine uniquely any one theory. Within a model-theoretic framework the other side of the coin – i.e. over-determination of theories by their models – becomes evident in a twofold way. First, if we accept the re-interpretability of the language of a relevant theory, one theory may be

6 Please note my particular use of the term "interpretative model". There are other definitions for it in general philosophy of science literature that are not applicable here.

7 Most of the above is also discussed, and, in places, at somewhat greater length, in both Ruttkamp (2002) and Ruttkamp (2003).

8 More precisely, traditionally the nature of under-determination 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.

true in many different models, and stand in relations of empirical adequacy to many different empirical models. Second, the information models carry is complete in the sense that every term in the vocabulary of the language of the relevant theory is interpreted in terms of the models' domain(s) of discourse. In this article the focus is in particular on the first interpretation of the term “over-determination”, since the second, although related, is not problematic in the same way.

In the scientific context I claim a default rule containing at least the following two conditions – or orderings – might be useful in allowing us to get a grip on the complexity of relations between theories and their various models in terms of a particular kind of preferential ranking of these models, and so to find a new perspective on the complexity of knowledge representation in science.[9] This ordering induces an ordering of empirical models, of empirical reducts and models of theories themselves, and may ultimately, by the defeasible entailment relation, even result in a ranking of theories.

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. An empirical model, M_1, is preferable to another one if the sentences of the relevant language are more precise or exact in M_1 than in other relevant empirical models. For now, I am considering cases here where we have to choose among different equivalent empirical models of which all may be embedded into empirical reducts of the same type. The second condition that I would include in my default rule, is however more often concerned, together with a choice of empirical model, also with a choice of empirical reduct, since here the condition implies a ranking of empirical models that may induce a ranking of empirical reducts. Here the rule states that empirical models that can be embedded into empirical reducts of a type that contains a larger class of empirical terms than others, are preferable.

Both conditions imply a ranking of the “strength” of links between theories and reality. This is trivial in the case of the first condition, since precision is an obvious advantage. In the case of the second condition such a ranking in terms of strength of reference occurs because 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. In addition, the first condition also represents the cumulative progress of science, especially in terms of technological advances. As far as the second condition goes, in terms of the progress of science, it might be preferable to have a mechanism justifying including into a particular model of a theory previously exogenous factors as endogenous ones. (Think of the problems related to such changes in philosophy of economics, and how a non-monotonic preferential analysis might impact on resolving those problems.)

Placing both these conditions together into 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, and which, may, ultimately, induce rankings of theories via the non-monotonic (defeasible) entailment relation. Thus non-monotonic preferential default rules and consequent rankings enable us to reduce both the

9 The possibility of over-determination is introduced and its nature specifically discussed in Ruttkamp (2003).

available – or possible – choices of models, empirical reducts, and empirical models[10], while still allowing a rational articulation of the various contingent links between theories and their different models at certain stages of theory development.

EXAMPLE 2: A LIGHT-FAN SYSTEM II [11]

- Theory: p q T
- Empirical situation: Only the light can be observed, and is seen to be on
  This implies that
  p: empirical term
  q: theoretical term
-

Models of T (in random order)	Empirical Reducts (in random order)	Empirical models (in random order)
11 10 01	1- 1- 0-	1-

- The observation of the light being on, excludes the empirical reduct 0-, which in turn excludes the model 01
- Our choice of empirical model thus induces the following ordering of empirical reducts:
  0-
  1-
  which includes the following ordering of models
  01
  11 10
- This changes our theory to T' p

We *can* thus 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, rationally discuss the choices we make concerning so-called "empirically equivalent" models. Thus, rather than celebrating the loss of a one-to-one relation of truth and reference indicating the emptiness of philosophy in the foundationalist sense, I use non-classical analyses of science – such as the model-theoretic preferential one briefly touched on here – that can deal with the processes of science resulting in a body of contingent data about systems in reality that offers us "glimpses" of different aspects of real systems at different times. 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

10 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.

11 Example from Ruttkamp (2003).

alternative models, each of which may represent, explain, and predict different aspects of the same (or different) real system(s) via the same or different empirical reducts isomorphically linked to the same or different empirical models.

There are many (more and more diverse) application areas of non-monotonic reasoning, such as Minsky's discussions of vision, McCarthy's suggestion that default reasoning is used as a “communication convention” in the area of natural language processing, Reiter's work on so-called closed-world databases, the field of temporal logic, work in philosophy of science, and obviously, in general, the applications in AI programming.[12]

2.2 Truth and reference

In a model-theoretic representation of science such as mine the basic ontological assumption made is that science is about something that exists independently of it. This ontological assumption has however as little metaphysical content as possible.[13] In model-theoretic terms science is an individual and social construction. Science is “transitive” in Roy Bhaskar's sense as against the “intransitivity” of reality. Science is undoubtedly about “Nature”, and about discovering the intricacies of the mechanisms according to which “Nature” operates. In model-theoretic terms the aim of science simply is to offer certain idealised “insights” into the complex workings of “Nature”.

And, moreover, model-theoretic realism does imply that the terms in theories refer to objects or relations in systems in reality. The re-interpretability of the language of science, or of theories in particular, together with the fact that the “empirical” nature of reducts is contingent on a certain interpretation and empirical situation, imply that claiming model-theoretic reference is sufficient to establish a form of realism, since in this *referential-semantic* sense it can be shown that unobervables “exist” in real systems (in the sense that 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. Thus, saying that the theoretical term “electron” “refers” (or “exists”) without any reference to models or interpretations or reducts is simply not really sensible.

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 is much stronger and more flexible than typical metaphysical scientific realism.

Notice that reflections on whether we are examining a “correct” or “true” representation of reality remain, at the least, naive. The slogan of a model-theoretic realism is “truth without universality”. “Truth” is relative to specific models. Questions of truth can indeed only be settled by focusing on conditions of verification, but in the semantic sense of defining interpretations of the scientific language on specified domains of discourse. So, there are elements of conventionalism in a model-theoretic approach to theories, in the sense that “truth” is something that we “create” by our (heuristic or

12 Other applications of non-monotonic logic in philosophical accounts of science which I discuss in a model-theoretic preferential context are the study of different stages of theory corroboration (Ruttkamp forthcoming(b)), and the issue of theory reduction in the wider context of the unity of science (Ruttkamp forthcoming(a)).

13 See Ruttkamp (2002) for a more in-depth discussion of the metaphysical nature of certain of the basic tenets of traditional scientific realism.

pragmatic) choices (of interpretation), and not something dictated to us by nature (or philosophy). At the same time though, we understand that the "truth" of a theory in one model means the same as "truth" of the same or another theory in another model – in other words the notion of truth is transcendental, in the sense of being philosophically effable even though it can only be given content in particular (different) contexts. This conventionalism however does not collapse into the kind of reductionism against which Quine had it in "Two dogma's" (1953). The range of verifying conditions corresponding to each statement of a language are not determinable *a priori*, because the choice of (verification) conditions rests on the nature of the interpretational domain of denotation for a given language, which, in its turn, is determined by extra-logical and extra-scientific factors inherent to disciplinary matrices and goals of theory application expressed in different sets of default rules (among others).

We should not ask questions regarding any overall aim of science, but rather focus on different local aims of disciplines and even disciplinary matrices within disciplines. Recall Fine's (1986: 173) warning against the logical fallacy of deducing "There is an aim they all have" from "They all have aims". A variety of questions can be asked about any aspect of science at any time – i.e. no global assertions of truth or reference are acceptable, but rather we should focus on local assertions negotiated by scientists themselves for use in their contexts and reflected in non-classical analyses of scientific practice. The essence of science is contingent, historical and it is constantly growing – or at least changing – as a result of internal and external pressures.

3 Some ideas on postmodern philosophy and science

Modern and also postmodern philosophy of science may be characterised in many different ways. Jean-François Lyotard's definition of postmodernism implies that philosophy in the foundationalist sense has lost its credibility in terms of unifying all knowledge, and that we are faced with a proliferation of discourses (or language games), which are "determined locally" (Cilliers, 1995: 127), and not "legitimated externally" (Cilliers, 1995: 127). So "[t]here are many different language games – a heterogeneity of elements" (Lyotard in Baynes 1987: 74). Also keep in mind here Lyotard's subsequent demotion of philosophy (Cloete 2002) to just another discourse or language game among many others. In these terms science "has no special legitimacy, no basis outside the agreement among members of a community" (Holub, 1991: 147). Against this, my point is that scientists do not necessarily ultimately agree with each other because they are part of the same "language game" or disciplinary matrix or context of theory application. They agree (or disagree) because they can understand the reasons for each other's actions and choices and, most importantly, because they can *rationally* represent and explain these choices.

The main focus for postmodernists in this context is Lyotard's denial of the translatability of the rules of one language game to those of another, which causes the pragmatic realm (Holub, 1991: 141) of language games to be "ungoverned by transcendental or pre-established rules" (Holub, 1991: 141), so-called "meta-prescriptives". Thus Lyotard (1984: 65) denies that it is possible to come to consensus on the universal validity of certain meta-prescriptives or rules for language games, and so in other words, he denies the possibility of a universal meta-language. But we have seen that it is possible to do without such a meta-language and still trace the processes of science in a rational way. I offer one way in which such discussion become possible by turning to the mechanism of non-monotonic default logic and its minimal model semantics represented in terms of default rules telling us why certain choices were made in certain contexts. These default rules are outside the logic employed to represent the knowledge of a certain system in reality, but not part of any grand narrative because they are heuristic and contingent on the context within which they are applied.

Lyotard claims though that it is the “function of the differential/imaginitative/ paralogical activity of the current pragmatics of science to point out ... meta-prescriptives (science's 'pre-suppositions') and to petition players to accept different ones” (Lyotard, 1984: 65). I agree, except that my interpretation of the pragmatics of science does not depict it in terms of being para-logical (non-logical), but rather in terms of offering contextual always-amendable ways to represent knowledge of real systems. Cilliers (1995), in an article entitled “Postmodern knowledge and complexity”, writes that Lyotard uses the word “paralogy” to show that “logical descriptions are not adequate when dealing with the richness and contradictions of contingent complexity” (Lyotard, 1984: 131). He continues, “[m]any stories, even contradictory ones, can be told about single events or phenomena. ... Paralogy is ‘a move played in the pragmatics of knowledge’, the consequences of which cannot be determined *a priori*” (Lyotard, 1984: 131). Classical logic's descriptions are perhaps not adequate to deal with complex contingencies, but the mechanisms of non-classical logics have been designed to do exactly that. Indeed no implications of “moves” played in the processes (or pragmatics) of science can be offered *a priori*, rather these can only be determined historically by picking out certain contextual referential links in the complex web between theories and real systems as part of the content of science. Also it is not the quest for paralogy, but rather the fact that we can rationally trace (by making use of the heuristics of non-classical logics) our decisions and their implications in given contexts of the process of science, that validates the adoption of rules or meta-prescriptives – albeit only temporarily.

4 Philosophy of science today

Lyotard did not condemn scientific knowledge as such, but only a certain understanding or representation of scientific knowledge, namely the modernist notion of philosophy as incorporating all knowledge claims into one grand meta-narrative. I, too, considered here a certain understanding of scientific knowledge and philosophy. My arguments thus far imply that scientific actions – and their authority – are not about Nature in the traditional confirmational sense of satisfying some set of a-temporal methodological rules and offering a body of neutral pure objective data about reality. But, neither do the entire scientific enterprise and its products offer simply sets of (false) context-specific data. Rather, science is about “Nature” in the sense that it is a system of knowledge claims that may be analysed according to a set of (defeasible) rules that results in a body of contingent data about systems in reality that offers us “snapshots” of “Nature”.

One could, rather than speak of a specific scientific methodology in the sense of constructing a meta-narrative ruling all forms of knowledge, speak of scientific methods as ideally providing model-dependent model-modifiable strategies of scientific evolution, because such strategies (aided by the tools of non-monotonic logic in terms of minimal model semantics) offer – within a realist context – the possibility of modifying or amending our existing theories in the light of further research. The continuous self-corrective nature of science is also confirmed, since the methodological principles of a strategy like this will themselves depend on the theoretical picture provided by currently accepted theories. Both our new theories and the methodology by which we develop and apply them depend upon previously acquired theoretical knowledge. And this fact about the cumulation of scientific knowledge – as well as science's various relations to reality – can successfully be supported and explained by a model-theoretic preferential – realist – conception of scientific knowledge.

The role philosophy of science has to play in the new context we have found for science, becomes far more challenging and nuanced than before. After Kuhn, the choices for philosophy of science seemed to be between following a descriptive historical method, or continuing with the positivist quest for the logical and quantitative explication of concepts (Niiniluoto, 1999: 14). Illka Niiniluoto (1999) in his book entitled *Critical scientific realism* comments that a strict distinction between historical and formal methods in philosophy of science is unwise. Especially in the light of the new developments in non-classical logics and knowledge representation, logic (and quantitative studies) are not restricted to the mere study of "completed systems" (Niiniluoto, 1999), but can also study the processes, practice, and growth of science. Think for instance of applications of epistemic logic, non-monotonic logic, temporal logic, Carnap-Hintikka measures of semantic information, and Tich...-Oddie-Niiniluoto measures of verisimilitude to issues concerning scientific change.

The portrayal of the logical empiricists of philosophy of science as a prescriptive *a priori* account of scientific rationality (Niiniluoto, 1999: 14), may be contrasted in the history of philosophy with the naturalist turn that Lakatos developed (in the sense of his demand for methodology to be tested against the actual history of science), the pragmatist "natural philosophy" of Quine, and also the constructivist/sociological account of science, which all to various degrees claim that philosophy of science is a study of the actual practice of science. I agree though, with Niiniluoto (Niiniluoto, 1999: 15) that, although philosophy of science has a lot to learn from empirical disciplines such as cognitive science and the sociology and history of science, we should be careful to reduce epistemology to empirical psychology.

A conceptual objection against such a reduction is that although our beliefs may be the objects of so-called "naturalist" empirical studies, the definition of "truth" and "justification", "knowledge" and "confirmation", and many other epistemological concepts like these, is a matter of philosophical dispute (Niiniluoto, 1999: 15). The demarcation of science is for instance a basic problem in the philosophy of science, and "every attempt to study the actual history and practice of science already presupposes some answer to this question" (Niiniluoto, 1999: 15).

Another obstacle to a reduction of philosophy of science to some empirical discipline is normative in nature. Acknowledging that the descriptive study of how we think is relevant to the normative study of how we ought to think, does not imply that the latter can be taken care of by studying the former (Niiniluoto, 1999: 15). If we test methodology against the history of science, and thus accept the successes of using case studies to "test" or support certain views in philosophical accounts of science, we come up against the following circularity. Say for instance a case study shows that a group of scientists "favours 'bold hypotheses' and 'severe tests', then we may judge that they, or their teachers, have read Popper ..." Niiniluoto, 1999: . To avoid this circularity, it seems we need to find some group of scientists untouched by any methodological or philosophical ideas (Niiniluoto, 1999: 17), and in this sense, surely, naturalism is implausible.

The point here is that although philosophers of science should have a good knowledge of scientific practice in all its forms and so acknowledge a place for historical studies, they should also be able to criticise the way science is actually done. This implies that regardless of whether we are considering ontological, semantical, epistemological, axiological, methodological, or ethical problems in the context of science, although we need support from scientific knowledge, "genuinely philosophical aspects of these issues remain on the agenda" (Niiniluoto, 1999: 17).

This though, does *not* mean a return to foundational or "first" philosophy. Science does not need philosophy as its foundation – and I might add, perhaps philosophy also does not need science in this way either. The former position has been supported in at least two different ways: Defenders of the "positivist" view (Quine) – motivated by the belief of science as the paradigm of rationality – hold that science may be "a child of philosophy, but has grown completely independent of her mother, i.e. mature science has happily got rid of metaphysics and epistemology" (Niiniluoto, 1999: 17). Defenders of the "postmodern" view (Lyotard) hold the "anti-Kantian" view that nothing has foundations and so collapse science studies into historical or sociological description.

Does the negation of one extreme however necessarily imply the affirmation of its opposite? Just because philosophy turned out not to be a legitimating meta-narrative, does not mean there is no role for philosophy. Just because science is without the privileged protection of universality and absolute truth, does not mean there is no such thing as scientific knowledge. Just because science itself consists of innumerable different language games and is also itself one language game among many others does not mean that no communication or rational discussion of the processes of science is possible.

Both extremes are wrong. Science is a rational cognitive but incomplete enterprise (Niiniluoto, 1999): its tentative results are always amendable and in need of interpretation and analysis, while its methods can be improved in terms of reliability. Philosophy of science cannot give and is not about giving absolute and final foundations for science, but "it cannot leave science as it is" (Niiniluoto, 1999: 17). Normative questions about scientific enquiry and knowledge have to be asked and answered, standards need to be constantly re-evaluated, and the activities of science need to be criticised if needed. Also the social role of the processes of science is in need of philosophical reflection and so also the ethics of science is in constant need of philosophical attention. Philosophy of science can however only successfully address these issues in conversation with science and its disciplines.

Thus, on the one hand it might be the case that we all "live in ongoing stories" (Rouse, 1990: 181) – even in science (Lötter, 1994: 157). On the other hand philosophical analyses of these stories are needed and possible. Evidence of the need is given simply by looking at the various accounts of science we have briefly touched on in this article. Perhaps, given an interpretation of science as complex and changeable, corrigible and tentative, we should not ask whether some philosophy of science is modern or postmodern, but rather simply check if it helps us comprehend science and its processes in new ways.

Definition 1

Let G be any set. A relation R G G is a *total preorder* on G iff

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

Definition 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**.

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 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 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 β is satisfied at y and (y,x) R and (x,y) R.

Bibliography

Cilliers, P. 1995. Postmodern knowledge and complexity (or why anything does not go). *South African Journal of Philosophy*, Volume 14(3): 124-132.

Cloete, M. 2000. Is Habermas a transcendental thinker? *South African Journal of Philosophy*, Volume 19(2): 95-118.

Fine, A. 1986. Unnatural attitudes: realist and instrumentalist attachments to science. *Mind*, Volume 95: 149-179.

Ginsberg, M.L.. 1987. Introduction. In: Ginsberg, M.L. (Ed.) 1987. *Readings in non-monotonic reasoning*. California: Morgan Kaufman: pp. 1-24.

Heidema, J. & W.A. Labuschagne. 2001. Knowledge and belief: The agent-oriented view. In: Roux, A.P.J. & P.H. Coetzee. (Eds.) 2001. *Culture in retrospect. Essays in honour of E.D. Prinsloo*. Pretoria: University of South Africa: pp. 194-214.

Heidema, J. & W.A. Labuschagne. 1999. Logics for all seasons. In: De Villiers, M.R., et al. Eds.) *Papers delivered at the symposium in honour of Professor George McGillivray on the occasion of his retirement.* Pretoria: University of South Africa: 15-28.

Holub, R.C. 1991. *Jürgen Habermas: Critic in the Public Sphere*. London and New York: Routledge.

Lötter, H.P.P.1994. A postmodern philosophy of science? *South African Journal of Philosophy*, Volume 13(3): 153-160.

Lyotard, J.-F. 1987. The postmodern condition (an excerpt). In: Baynes, K. et al. (Eds.) 1987. *After Philosophy: End or transformation?* Massachusetts: The MIT Press.

Lyotard, J.-F. 1984. *The postmodern condition: A report on knowledge*. Manchester: Manchester University Press.

Niiniluoto, I. 1999. *Critical scientific realism*. Oxford: Oxford University Press.

Quine, W.van O. 1951. Two dogmas of empiricism. *Philosophical Review*, Volume 60: 20-43.

Rouse, J. 1991. Philosophy of science and the persistent narratives of modernity. *Studies in the history and philosophy of science*, Volume 22(1): 141-162.

Rouse, J. 1990. The narrative reconstruction of science. *Inquiry*, Volume 33(2); 179-196.

Ruttkamp, E.B. Forthcoming (a). Reviewing reduction in a preferential model-theoretic context. Conference paper for *12th International Congress of Logic, Methodology and Philosophy of Science* 2003, Oviedo, Spain.

Ruttkamp, E.B. Forthcoming (b). Falsificationism non-classically analysed. Conference paper read at the *Karl Popper Centenary Conference* 2002, Vienna, Austria.

Ruttkamp, E.B. Forthcoming, 2003. Over determination of theories by empirical models: A realist interpretation of empirical choices. In: Festa, R. (Ed.) (Forthcoming.) *Logics of scientific cognition. Essays in debate with Theo Kuipers*. Poznán Studies in the Philosophy of the Sciences and the Humanities, Rodopi: Amsterdam, The Netherlands.

Ruttkamp, E.B. 2002. *A model-theoretic realist interpretation of science*. Synthese Library Series, Volume 311, Kluwer Academic Publishers: Dordrecht, The Netherlands.