

CHAPTER TWO

A MODEL-THEORETIC ACCOUNT OF SCIENCE¹

1. INTRODUCTION

What is it that philosophers can or should say about science? Should we explain the actions of scientists? Should we explain the methodology of science? Should we explain the process and progress of science? Should we study the knowledge claims offered by scientists and decide which of these are so-called "scientific" knowledge claims? Should we study the history of science and its influence on current scientific practice? What — if anything — should we say about reality and the links — again, if any — that exist between science and reality?

In his article on philosophy of science in *The Oxford Companion to Philosophy* (1995) David Papineau describes philosophy of science as being divided into what he calls the "epistemology of science" and the "metaphysics of science". He claims the former to be concerned with questions about the justification and objective nature of scientific knowledge and the latter to be occupied with "philosophically puzzling aspects of the reality uncovered by science" (ibid.:809). I agree that philosophy of science consists of, among other things, an epistemology of science. I would also agree to describe such an epistemology in terms of justification for scientific knowledge, under condition that it is understood that a scientific epistemology should also — perhaps before anything else — offer an analysis of the nature of scientific knowledge, and the process by which we come to such knowledge claims.

As far as a metaphysics of science is concerned though, rather than any kind of metaphysical musings concerning philosophical questions about the "philosophically puzzling" parts of reality that science "uncovers", a philosophy of science should offer an ontology of *science*. If we do assume — as Papineau obviously does — that science can "uncover" certain aspects of reality a philosophy of *science* should surely focus rather on the structure of such an enterprise than that of reality? We have long moved beyond the time of so-called "natural philosophy", and so should acknowledge that philosophy of science (as it should) is first "about" science and not first "about" reality. Of course, if it is assumed that science "uncovers" aspects of reality, we have to say something about reality as it is linked to science, but never should we, as philosophers of science, try to write an ontology or (ontologies) of reality.

But, what then of realism? I shall argue in this text that the only kind of realism possible is indeed one that focuses more on science than it does on reality. For a workable scientific realism the only necessary and sufficient condition that is concerned with the nature of reality is, simply, the assumption that there exists such a reality independent of whatever happens in science. All the other conditions for such a realism should be concerned with science — its nature, its progress, and its justification. For, how can we claim that science is about reality if we do not study science and its processes? Perhaps it seems as if this question implies an equally strong motive for studying reality. However the assumption of the independent existence of reality takes care of that part of the question, simply because, in the end, a philosophy of science is about science, and science is about reality. When it comes to studying the links between science and reality, the original existential assumption concerning reality may be augmented by certain general ("common-sensical")² ontological orientations concerning the nature of reality; for instance its complex and rich nature, as well as its similarities and regularities, should be taken into account. But the bottom line remains the same since, even then, the reason for these ontological orientations is that the way science links itself to reality cannot be motivated or understood without these orientations.

The underlying reason for the complexity of the science-reality connection is that science and reality represent different logical classes and therefore they cannot simply be compared to each other directly — especially not in a one-to-one relation of correspondence — without committing a chain of serious category mistakes. A discussion of the process of science in model-theoretic terms, that analyses the development of scientific theories from their origin to their applications forms the foundation of a model-theoretic realism. In this chapter, in what follows, I shall analyse science and its enterprises model-theoretically. In Chapter 3 I shall examine the statement account of science in the context of model-theoretic realism and then, in Chapter 4, I shall move on to discuss some of the main non-statement analyses of science, and also comment on and discuss Cartwright's analysis of science and her specific kind of realist oriented problems. Finally I shall discuss the realist features of science in model-theoretic terms (Chapter 5).

I am advocating a philosophical approach to science which introduces a mediating factor (the role of "models") between scientific statements and "real" objects and events. I shall show that such an account of science examines and make sense of the fact that there is no unique kind of empirical linkage between theories and real systems, but rather a variety of many-to-many links between theories, their (interpretative and empirical) models, and systems in reality. A model-theoretic account of science implies an articulation, a kind of "doubling up" — by splitting up — of the relations between scientific statements and objects in reality, because it implies these relations have to be regarded as "filtering" through the models in the case of both the formulation of a scientific theory and its application. In this view a conciliation of the statement and non-statement views of science is achieved.

More specifically, the model-theoretic analysis of the process of science that I propose is done in terms of an account of science that offers a rational (conceptual) reconstruction of the "life" of a typical scientific enterprise, and is offered against a

stratified view of the process of science. This stratification is three-fold: it consists of an empirical level, a middle conceptual (interpretative) level, and a linguistic level.³ The terms of the first level are very particular, those of the second are more general, although still specific, while the terms at the final level are at the highest level of generality. The distinctions and relationships between the levels are complex (and often interchangeable), and so the process of science will be presented as a development in time from the first level through the middle level to the third level, and then back again.

The "purely empirical" level I interpret as based on the existence of various systems in reality and consists of our interactions with them, while I view the final level as a level of linguistic systems at which a scientific theory is formally formulated and suitably expressed in some appropriate language. The middle conceptual level is a very complex one in the sense that it has various facets that, in their turn, may be seen in terms of a certain kind of hierarchy. It is at this level that models both interpreting scientific theories and making them true are constructed, and, also at this level, the issue of adequate reference to real systems is touched on for the first time. I claim the latter relations of adequacy to be the result of various scientific experimental and observational activities, which may lead to the establishment of a relation of isomorphism between a special kind of substructure of some model of a theory and some "empirical" model of some real system (conceptualising the experimental relations executed in that system).

Before I get on with a discussion of my model-theoretic view, some comments on the Kuhnian meaning of the notions of "paradigm" and "model". In the Postscript to *The structure of scientific revolutions* (1970), Kuhn admits that at least two main and "very different usages" of the term paradigm are possible. I shall in what follows briefly discuss these two usages — paradigms as "disciplinary matrices" and as "exemplars" — in terms of a model-theoretic account of science. I agree that the *Structure's* notion of paradigm in the sense of the context within which theories are interpreted and applied is a valid and unmistakable part of the conceptual processes of science. As shall be explained in the remaining sections of this chapter, my usage of the notion of model identifies models as components of these kinds of paradigmatic context.

Kuhn (1970, p.175) claims that the two main senses in which he meant the notion of "paradigm" to be interpreted are:

- as standing for the "entire constellation of beliefs, values, techniques, and so on shared by the members of a given community";
- and as denoting "one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can *replace explicit rules* as a basis for the solution of the remaining puzzles of normal science".

Kuhn (*ibid.*, p.182) acknowledges that scientists would rather speak of sharing a theory, or a set of theories, than of sharing a paradigm or a set of paradigms. This led him to formulate the term "disciplinary matrix" to denote that which is shared by members of a scientific community and that accounts for the success of their communication and their agreement about professional judgements. He (*ibid.*) explains that "disciplinary" refers to the common possession of members of a specific discipline,

and that "matrix" refers to the fact that what is shared is an ordered system which consists of elements of various kinds, each needing further specification. Disciplinary matrices (ibid., p.184) consist of mainly three components:

- first, symbolic generalisations — either formalised, or expressed in words, like "elements combine in constant proportion by weight";
- second, what he referred to as the "metaphysical" parts of paradigms and which he explained as commitments to beliefs such as "all perceptible phenomena are due to the interaction of qualitatively neutral atoms in the void, or, alternatively to matter and force, or to fields" and which he described as beliefs in models; and,
- lastly, values such as predictive power, simplicity, consistency, plausibility — and he stressed that the application of values may be considerably affected by individual factors such as personality and biography.

In my exposition of the conceptual process of science, the need for a disciplinary matrix — something which is common to a certain group of scientists — will become obvious. Whether in the sense of offering the theoretical background against which scientists gather data from systems in reality, or in the sense of forming research objectives and directing conceptualisations of real systems, or in guiding interpretations of theories in their application or implementation, a context somehow common to all scientists of a certain research group is indeed indicated.

Note though, that Kuhn (ibid., p.184) sees it as the function of models to provide the relevant community with preferred or permissible analogies and metaphors, and so to help to determine what will be accepted as an explanation and puzzle-solving solution, as well as to determine as yet unsolved puzzles and their relative importance to the group. In my terms, although models help to do this, all of these are functions of the matrix, rather than of individual models. Models are rather the result than the cause of these things and are thus not to be identified with the matrix itself. In my view, models have a specialised role in the sense that they are specific interpretations of given theories in which these theories are true. So, the formulation of models has to be preceded by factors such as determining the nature of these models, whether they will offer an explanation for whatever problem is being studied, and so on. The disciplinary matrix should in my terms be seen as offering the background against which actions at all three (model-theoretic) levels of the scientific process take place.

Traditionally it is thought that scientific knowledge is embedded in theory and rules. A student cannot learn to solve problems before she has learned the theory and the rules for applying it. And then, after having solved many problems, such a student will find it less and less difficult to solve more and more complicated problems. As we know, Kuhn (ibid., p.188) claims that

... at the start and for some time after, doing problems is learning *consequential things* about nature. In the absence of such exemplars, the laws and theories [the student] has previously learned would have little empirical content.

He further goes on to say (ibid., p.190) that scientists solve puzzles by modelling them

on previous solutions. He wants to emphasise that what he refers to as "consequential knowledge of nature" is acquired while learning the similarity relationship and is afterwards embodied in a particular way of viewing nature — rather than by laws and rules.

He (*ibid.*, p.191) claims the verbal statements of laws taken by themselves to be "virtually impotent"⁴ and stressed that what actually results from the use of exemplars is something like Polanyi's "tacit knowledge" which is learned by doing science rather than by acquiring rules for doing it. Kuhn (*ibid.*, p.194) argues that claiming that rules and the ability to apply them are acquired from exemplars implies that there are, already at that stage, alternatives in the following sense. We might, he (*ibid.*) continues, "... have disobeyed a rule or misapplied a criterion, or experimented with some other way of seeing. [And those] are just the sorts of thing we cannot do." I would qualify this remark by adding that those are things scientists cannot do until after they have started making observations and thus taking part in the scientific process. Then they start abstracting data from real systems, constructing conceptual models, and maybe in the end, formulating theories, always directed by factors like research and personal goals and working from within a specific disciplinary matrix.

The only way in which we can have scientific contact with the world (that is, with systems in reality) is through actions involving selection, abstraction, and generalisation, which are always executed within some theoretical framework or disciplinary matrix, and are always teleological in character in the sense that these abstractions are made in order to theorise eventually about a specific aspect of some real system relevant for certain context-specific reasons. So in this sense there are indeed no absolute rules and laws guiding us towards the expression of our knowledge, because of the way our encounters with reality are structured. And, in this sense, it is indeed only by constructing various models of reality that we learn "consequential things about nature".

Thus, the way in which models function in a model-theoretic interpretation of the process of science has some of the features that Kuhn ascribed to this second use of the notion of "paradigm". Models can be seen as the basis for solving "puzzles" in the sense that they are actually constructed in the first place, when they form part of the process towards theory-formulation, to do just that. Also, more importantly, when considering the implementation or application of theories, already established models of theories may lead scientists to new interpretations of these theories, or to amending the existing models in order to offer "better" explanations for the problem the theory is addressing. To claim however — as Kuhn does — that models replace explicit rules as basis for puzzle-solving is maybe taking matters a bit far. It is not possible to find a unique description dictating the content of constructed models (whether during the process towards theory-formulation, or afterwards, when the theory is applied), simply because these constructions are so context— and theory—specific. However, this is part of the changeable nature of scientific knowledge, and so, the fact that models operate as a link between theories and systems in reality, and that they have an essential interpreting role to fulfil in the process of science, is a significant feature of scientific practice.

A Model-Theoretic Realist Interpretation of Science

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