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The Nature and Structure of Scientific Theories

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C. Ulises Moulines‡

Abstract

In philosophy of science two questions become central in the discussion of the nature of empirical science: 1) What is a (scientific) theory, i.e. how is it built up, how does it work? And: 2) How does a theory relate to its corresponding experiential basis? To deal with these two questions modern philosophy of science has devised various (meta-theoretical) 'models' on the nature and working of scientific theories. Some aspects of these models are widely held within the community of philosophers of science, but others are still being discussed quite controversially. In this paper, we will consider both kinds of aspects. Particularly, we will analyze how the meaning of scientific concepts is determined; the axiomatic construction of a scientific theory; the idea of model building views as a bridge between theory and experience; the holistic semantic thesis of science; the question about the truth of scientific theories and, finally, the hierarchic structure of theories.

Keywords: scientific theories - structure - semantics - models

Resumen

En filosofía de la ciencia, dos cuestiones resultan centrales en la discusión acerca de la naturaleza de la ciencia empírica: 1) ¿qué es una teoría (científica)?, es decir, ¿cómo está constituida y cómo funciona? y 2) ¿cómo se relaciona una teoría con su correspondiente base experiencial? Para tratar estas dos cuestiones, la moderna filosofía de la ciencia ha desarrollado varios "modelos" (metateóricos) sobre la naturaleza y el funcionamiento de las teorías científicas. Algunos aspectos de estos modelos son ampliamente aceptados por la comunidad de filósofos de la ciencia, mientras que otros son todavía discutidos de manera bastante controvertida. En este trabajo, consideraremos ambos tipos de aspectos. En particular, analizaremos cómo se determina el significado de los conceptos teóricos, la construcción axiomática de una teoría científica, la idea de las concepciones sobre la construcción de modelos como puente entre la teoría y la experiencia, la tesis semántica holista de la ciencia, la cuestión acerca de la verdad de las teorías científicas y, finalmente, la estructura jerárquica de las teorías.

Palabras clave: teorías científicas - estructura - semántica - modelos

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1. Introduction

Empirical science is a complex building consisting of many different components: instruments and methods of observation, experimentation and computation, technological applications, methodological and ethical values, underlying ideological and/or metaphysical motivations and assumptions, and scientific communities studying a particular range of human experience with some particular goals in mind. But above all, empirical science consists of a particular sort of abstract entities known as *theories*. Instruments, methods, values, goals, research communities and all the rest make sense only with respect to some particular theories accepted and used by scientists. The notion of a scientific theory is essential to understand the nature of empirical science. Therefore, it is very important for the philosophy of science to make clear what kind of entity a (scientific) theory is, and how it works.

In the present article, the use of the term "(scientific) theory" will be restricted to theories of *empirical* science, i.e. theories that, in the last analysis, directly or indirectly, have some link to human (sensorial) experience. The consideration of purely logical or mathematical theories falls out of the scope of this article, though some of the insights obtained in the philosophy of logic and mathematics are relevant to some issues concerning empirical theories.

In present-day philosophy of science it is generally agreed that the idea of an "absolute experience," completely independent of any theoretical considerations, is untenable, at least in science. For this reason, two questions become central in the discussion of the nature of empirical science: 1) what is a (scientific) theory, i.e. how is it built up, how does it work? and 2) how does a theory relate to its corresponding experiential basis? The two questions are obviously interrelated. To deal with these two questions modern philosophy of science has devised various (meta-theoretical) 'models' on the nature and working of scientific theories–i.e. it has devised its own *meta-theories,* as it were, on theories. Some of these models, or rather some aspects of these models, are widely held within the community of philosophers of science, but others are still being discussed quite controversially. We shall begin by considering the first kind of aspects, and then go on to the more controversial ones.[1]

2. Determining the meaning of scientific concepts

The first important step when constructing a scientific theory consists in the indication of a series of *specific concepts* as well as *specific principles* (laws viz. gen-

[1] Two classical accounts of the two central questions just mentioned, and in particular the question of the relationship between theory and experience, are to be found in Nagel (1961), and in Stegmüller (1970). A different, more recent approach to these questions is offered by the so-called semantic view of theories (see, e.g., van Fraassen 1980, and Suppe 1989). The so-called structuralist view of theories offers the most complex account so far of the structure of theories and their relationships to the "outside world". The standard exposition of this view is Balzer, Moulines & Sneed (1987). Díez & Moulines (1999) contains a detailed exposition of these different conceptions. A comparison of the different philosophical views on theories from a historical perspective is offered in Moulines (2006).

eral statements). Any particular scientific theory aims at the investigation of a particular domain of our experience, but it does so by assuming a specific, thoroughly worked-out conceptual framework, and it makes some general claims about the domain considered; these general claims are statements formulated with the notions of the specific conceptual framework. This means that the domain of experience to be investigated has to be, first of all, interpreted or reconstructed in terms of the assumed conceptual framework. As has already been pointed out, the idea of a pre-conceptual experience is untenable in a scientific context. Consider, for example, the domain of experience relevant for the science of mechanics: This domain is interpreted or conceived in terms of such notions as *particle*, *position, time, velocity, mass,* etc. On the other hand, if we want to deal with the domain corresponding to the aims of decision theory, we will rather use such notions as *action, uncertainty, expected utility, subjective probability,* and the like.

It is important to note the specificity and precision of the concepts relevant for the construction of an adequate scientific theory. They are quite different in nature from the usual notions we employ in everyday life. True, scientific concepts are often *expressed* by means of words coming from everyday language (a feature that usually suggests the historical origins of the discipline in question); however, these expressions normally have a heavily transformed, and above all more precise, usage when compared with everyday language. The usage of the English words "force" or "field" in a physics textbook is only remotely, if at all, related to their usage in everyday English. Moreover, in many scientific theories we find terms that are complete neologisms (think of "entropy," "spin," "gene"...) and that either have no use in everyday language, or else, if they now have such a use, it has been imported from science ("gene" would be a good example of this). The rationale for introducing such terms is not the wish to disorient the layman and invent a cryptic language only known to a clique, but rather to avoid false associations and expectations, and to be as precise as possible.

Now, if the meaning of scientific terms usually doesn't coincide with the meaning of everyday expressions, the question immediately arises: how do they obtain their proper meaning? This is no trivial question at all; rather, it reflects a quite central problem in the philosophy of science. The answer to this question is all but simple. The different approaches that have been offered to answer it belong, at least in part, to some of the most controversial points of modern philosophy of science. We'll come back to this question below. For the moment, let's just make the following remark. Scientific concepts never obtain their proper meaning one by one –there is no such thing as an 'isolated' scientific concept; they get their meaning in the context of a whole series of *other well-determined concepts*; together, they build a specific conceptual framework.

It is often said that the standard way to determine the meaning of scientific concepts is, other than in the case of everyday notions, to *define* them rigorously. This is only partially true, and this for two reasons. First, to define a term

A means to set it in a systematic relationship with other terms, say, *B, C, D*… Now, if this is going to be a real definition, it has to be guaranteed that the information content provided by *A* is exactly the same as the one provided by the combination of *B, C, D*… This means that *A* and the combination of *B, C, D*… have to be *semantically equivalent*.[2] (For example, the notion of an average velocity in mechanics may be defined by means of the concepts "distance" and "time interval" because the first notion is semantically equivalent to "distance run divided by elapsed time interval.") However, not all conceptual connections appearing in a scientific context can be interpreted as such *semantic equivalences*. In many cases, what we have are only more or less partial connections that don't amount to a full conceptual coincidence. (Think of "defining" the energy of a gas, in thermodynamics, as the partial derivative of pressure with respect to volume: this can only be accepted if other relevant parameters are supposed to be held constant.)

The second reason why definitions cannot be the general rule for determining the meaning of scientific concepts is more fundamental. It is just logically impossible to define *all* relevant notions of a given scientific discipline: this would lead either to an infinite chain of definitions (an absurd idea), or else to a vicious circle. (Suppose, to simplify, that our discipline would contain only three concepts, *A, B, C*; we should first define *A* in terms of *B* and *C*; but then we should define *B* in terms of *A* and *C*–therefore not defining anything at all; the argument is applicable to any finite set of concepts.)

The consequence of this logical fact is that, in any given theory, we have to admit a certain number of concepts as *undefined*. In the terminology usual in formal philosophy of science, such notions are called "primitive concepts" or also "basic concepts." Once we have admitted one such set of basic concepts, we have to take care that the rest of the theory's specific notions can be introduced as defined concepts by means of rigorously constructed definition chains, all of its members being semantic equivalences. Since the pioneer times of formal philosophy of science there is wide consensus about the rules that the definition chains have to fulfil in order to obtain genuine definitions.[3] At any rate, it is quite clear how defined concepts get their meaning in a theory: through definition chains that eventually lead to the meaning of the basic concepts. But now we are still confronted with the fundamental question of how the basic concepts get their meaning determined.

3. The axiomatic construction of a scientific theory

To deal with the last question, we have to deepen our analysis of the structure of scientific theories. It has already been pointed out that scientific concepts

[2] A precise treatment of the theory of definition is to be found in Suppes (1957).

[3] We cannot go into the explication of these rules here. Their comprehensive exposition is to be found in Suppes (1957).

don't appear in isolation but in "clusters." This fact is particularly significant for the problem of determining the meaning of the basic concepts of a given theory. Typically, such concepts don't appear isolated in simple statements; rather, they appear inter-connected in the theory's general, fundamental principles–i.e. in its *axioms*. Certainly, a theory consists, in a first move, of a specific conceptual framework; but it is a framework within which some statements of fact about the world are supposed to be made; and when these statements are justified, we obtain some knowledge about the world. Now, the most important statements a theory makes are precisely those that are essential to obtain the pieces of knowledge we aim at. These are the theory's axioms. They consist in basic connections between basic concepts. At least in principle, all other statements of fact we want to make about the world in the theory have to be derived from the axioms as *theorems*.

Consequently, definitions, axioms, and theorems are the three fundamental categories of statements building a scientific theory. Whenever a theory is constructed in such a rigorous way that we may clearly determine which statements are genuine definitions, which ones are axioms and which ones are theorems, and when all members of the last category can logically be deduced from the axioms (and possibly the definitions), then we say that the theory has been *axiomatized*. It has to be remarked, however, that the *axiomatization* of a theory very often represents rather an ideal (a regulative principle) and not so much a reality within scientific practice. (Only within the formal disciplines of logic and mathematics it may be said that almost all existing theories have been rigorously *axiomatized*.) In the empirical sciences, the axiomatic construction of a theory is rather the exception than the rule. Only some empirical theories, which are considered particularly fundamental, have been *axiomatized* more or less thoroughly; we may mention the examples of Newtonian mechanics, thermodynamics, quantum mechanics and classical genetics. This situation is partly due to historical contingencies (the *axiomatization* tradition being much stronger in mathematics than in empirical science since Antiquity), partly due to systematic grounds (normally, empirical theories are structurally much more complex than mathematical theories). Be it as it may, from an epistemological point of view, the correct *axiomatization* of empirical theories still is a genuine ideal because it is only by this procedure that we can fruitfully address the question of the meaning and function of scientific concepts and statements. By the way, this explains the fact that, in the last decades, it has been above all the philosophers of science, and not so much the practicing scientists themselves, who have undertook the task of *axiomatizing* important empirical theories.[4]

[4] Several examples of rigorous *axiomatizations* of empirical theories within the framework of "classical" philosophy of science are to be found in Kyburg (1968). See also the anthology edited by Henkin, Suppes & Tarski (1959). For examples of *axiomatizations* within a different, "non-classical" meta-theoretical framework, see Balzer, Moulines & Sneed (1987). In this work, the so-called "structuralist" methodology of *axiomatization* is applied to several examples of theories from physics and chemistry. More examples of *axiomatizations* along these lines of theories from different disciplines, including the social sciences, may be found in Balzer, Moulines & Sneed (2000).

4. Model building as a bridge between theory and experience

Whenever a theory has been thoroughly *axiomatized*, we know exactly which concepts, among its specific notions, belong to the set of defined notions and which ones belong to the set of basic or primitive concepts. We have already seen that the meaning of the defined concepts reduces to the meaning of some combinations of the defining concepts, and this means, in the last analysis, combinations of the basic concepts. The meaning of the basic concepts, in turn, is then *partially* determined by the connections between them that are expressed in the axioms. We could also say: the axioms or principles postulated provide by themselves the meaning of the basic concepts. But this is only partially true. If the meaning of the basic concepts of any given theory were exclusively determined by its axioms, then there would be no difference in principle between an empirical and a purely mathematical theory, since it is precisely a characteristic feature of mathematical theories that their proper conceptual framework is settled only 'internally' (axiomatically). On the other hand, the conceptual framework of an empirical theory has not been conceived to float freely in the heaven of abstract ideas. The goal of a genuinely empirical theory is to be anchored in the world of experience, and it is this anchoring *too* that provides their meaning to the basic concepts. The conceptual framework (and therefore the statements we make within it) has to be *interpreted* in the empirical reality. How may we attain this goal?

Even though there is still no uniformly accepted approach in contemporary philosophy of science with respect to this question, there is at present a wide consensus about the significance of the notion of a *model* to deal with the question at stake. The concept of a model we refer here to goes back to developments in formal semantics, especially in the work of Alfred Tarski.[5] Claiming that a theory's conceptual framework can be interpreted in a particular domain of experience amounts to claiming that this domain (even though in a simplified or idealized form) can be conceived as a *model* of the theory's axioms. It is in this way that the concepts appearing in the axioms get their empirical content. In a different, though essentially equivalent, way of speaking, we may say that the theory (that is, its conceptual framework and the statements made within it) *represents* the domain in question by building a model. Still another way of putting it is that a model is a structure constructed by means of the theory's concepts which *covers* the experiential domain we intend to study (in a more or less idealized manner).

Instead of providing a general, abstract explication of the notion of a model and of the associated procedure of building models, let's lay out the essential aspects of this conception by means of a simple, schematic example. Suppose that a group of scientists (a "scientific community") is interested in the theoretical investigation of a particular domain of experience, say, a series of light points

[5] See Tarski (1956). An application of Tarski's notion of model to empirical theories is to be found in Suppes (1957).

on the nightly sky that move slowly and take different positions on the sky every night at the same time. The first step to make for the group of researchers is to *codify* the immediate observations they make on the light points with the naked eyes or with the help of particular devices like telescopes into a so-called *data structure*, usually called also a "*data model.*" This "codification" implies that the observations made will be idealized drastically (that is, their more complex aspects will just be put aside) and *interpreted* in a certain way. For example, the light points will be conceived as perfect spheres that are at a precise position in space at any particular time point. For the moment, this is all that appears to be interesting with respect to the light points–and not, say, what colour they have or whether they appear bigger or smaller than other light points. The spherical bodies together with the indication of their positions and times constitute the observed "data." Their coherent and systematic assemblage constitutes the *data model* the scientists are interested in explaining.[6] In a second step, the data model will be *extrapolated* to continuous curves of a particular form, for example, ellipses. It is to be noted already at this point the strongly *hypothetical* nature of the procedure of our group of researchers: They 'bet' that the assumption that the interesting light points are like perfect spheres moving on ellipses is a promising approach towards an appropriate theoretical interpretation of the domain of experience at stake. But, of course, they don't have any formal proof that this must be so. The next step is still more daring. It consists in assigning to the observed light points a certain set of "parameters;" they are, most of the time, *magnitudes*, i.e. assignments of numbers to empirical objects. They are supposed to help explaining the motions of the observed things. In our example, such magnitudes could be, say, velocity, acceleration, mass, and force; they are either defined or basic concepts appearing in a given theory (let's say Newtonian mechanics). Further, it will be assumed that those magnitudes always have precise values in the observations already made or in those still to be made. (We cannot go here into the subject of the determination of such values; this is an issue that belongs to one of the most complex chapters of philosophy of science–the foundations of measurement.[7]) After all these steps have been made, the spherical bodies with the indications of their positions and times as well as the other magnitudes mentioned constitute a *mathematical structure* (which, however mathematical, ultimately roots in empirical data). Of this structure, the researchers claim now that it satisfies (at least approximately) the principles (axioms) of a particular theory–say, Newtonian mechanics; in other words, they claim that it is a *model* of Newtonian mechanics. If this claim comes out as true (that is, if no inconsistencies appear within a previously determined, acceptable margin of error, between the observations and measurements made on the one hand and those expected according to Newton's laws on the other), then we

[6] The notion of "data model" or "models of data" goes back to Suppes (1962).

[7] The standard reference work on this area is Krantz, Luce, Suppes & Tversky (1971). An historical exposition of the subject is to be found in Díez (1997a), and Díez (1997b). For further developments see Moulines & Díez (1994).

may say that the data model originally constructed by the research group out of their observations of the light points, is "covered" or "represented" by a model of Newtonian mechanics.

The foregoing example, which is avowedly quite schematic and strongly simplified, already allows for taking notice of the complexity and the different levels of the process of model building; this is the process that connects a given theory with the domain of experience it is supposed to apply to. Within a scientific context, there never is a sort of 'direct encounter' of theory and experience.

5. The holistic semantics of science

Let's now come back to the semantic problem that our simple example was intended to illustrate: how do the basic concepts of, say, Newtonian mechanics (particle, position, time, mass, force) get their proper meaning? The answer is two-dimensional. On the one hand, as we have already seen before, their meaning is partially determined by the way they are 'bound together' within some general principles, for example, the Newtonian axioms. But in an empirical theory like mechanics, this is only one of the dimensions of the determination of the conceptual content. The other dimension consists in the fact that those basic concepts get linked, even though quite indirectly, to the moving light points on the sky (our "immediate" experience) through the complex, multi-level process of model building just described. The first dimension of meaning determination may be characterized as "formal-axiomatic" (and, in this sense, it is completely analogous to the meaning determination of basic concepts in purely mathematical theories); the second dimension, on the other hand, may be described as "empirical/application-oriented" (and it is precisely a characteristic feature of empirical theories). It is to be noted that this second aspect of the interpretation of scientific concepts may be visualized as a kind of "two-ways-road": on the one hand, the elements of the process of model building get interpreted through the kind of original experience they are supposed to cover; for example, it can be argued that nobody fully *understands* what "force," "mass," etc. actually mean, unless he/she knows that these notions may be used to the conceptual apprehension of, among other things, certain light points on the nightly sky. But we have to take into account the other direction of the 'interpretation road' as well: the success in the application of the theory's model allows us to claim that the observations originally made on the nightly sky refer to "*particles* that, to any given *time*, have a particular *position* in space and a *mass*, and are subject to some particular *forces*." That is, the original light points become re-interpreted as particles having all those properties and magnitudes.

Due to the complexity of the interpretation process for scientific concepts, many contemporary philosophers of science have argued that the semantics of scientific concepts (that is, the structure of their meaning determination) has a *holistic* character. This means that it is always a non-decomposable conceptual totality that is the subject of a meaning determination. The precise explication of the structure of such semantic wholes is a primary task for modern philosophy of science.[8]

The degree of complexity of the semantic wholes characteristic of theoretical science may become still higher because of the following feature of empirical theories. In all those theories that are somewhat well-developed, we notice that their respective conceptual frameworks are used not only to conceptually apprehend just one domain of experience. Quite the contrary, the same conceptual framework may be applied to very different domains. Going back to our example: the concepts "particle," "force," "mass," etc., as well as their mutual connections as expressed in the Newtonian axioms, are used not only to interpret the observations on the nightly sky, but also to cover quite different domains like experiments with pendulums, freely falling bodies, oscillators, collisions of billiard balls, and many other things. In this sense, philosophers of science use to say that models of well-developed theories and their constituting concepts are "semantically *multivocal*."

6. Are scientific theories true?

Up to this point, we have dealt only with one big topic of modern philosophy of science: the semantics of a theory's conceptual framework. There is, however, another topic that is at least equally central for the philosophical analysis of science: the problem of the *truth-content* of empirical theories. Theories are built not only with the aim of conceptualizing a given range of phenomena, but also for the purpose of explaining, predicting, controlling and perhaps even manipulating phenomena, and of doing this *successfully*. For example, the conceptual framework of Newtonian mechanics was put up not just for the sake of conceptually *systematizing* and *interpreting* the observations made in several domains of experience, but also in order to *explain* the motions of bodies, to *predict* their future positions and to *construct* some useful devices and machines, or to facilitate navigation. The explanations, predictions, manipulations of phenomena made on the basis of a given theory have to *fit*, and when this is the case, intuitively we would like to claim that the theory is *true*.

Two questions arise when trying to explicate this intuitive expectation about theories in more precise terms. The two questions are interrelated but it is convenient to formulate them in separate terms. The first question we have to deal with is whether a theory is really the kind of entity of which we should want to predicate *truth*; perhaps we should rather be satisfied (at least in the case of empirical theories) with a somewhat weaker kind of assertion, like claiming that the theory in question appears to be *useful*, or *fruitful*, or *empirically adequate*, or

[8] The forerunner of holism in philosophy of science is Duhem ([1906] 2001). A more radical version of holism in science is represented by W.V.O. Quine, for example, in Quine (1953). A more sophisticated version of holism is to be found in Stegmüller (1973), and in Balzer, Moulines & Sneed (1987).

something of the sort. Realistically minded philosophers of science tend to apply the predicate “true” (or its opposite “false”) to theories,[9] while non-realists tend to use the weaker kinds of predicates just mentioned.[10]

Let’s assume for the moment that it makes sense to predicate truth (or falsity) of theories. (We’ll consider the ‘weaker’ alternative below.) The second question is then to find out the logical and methodological conditions that must be satisfied in order to be justified in claiming the truth of a theory.

In the pioneer times of modern philosophy of science, it was assumed quite naturally that it would be enough to have a sufficiently great number of observational data that agree with the theory’s statements to guarantee that the theory is actually true. This essentially is the classical view of *inductivism*: The truth, or at least the “probable truth,” of a theory is guaranteed by a sufficiently great (but finite) number of its positive instances. Contrary to the case of pure mathematics, *induction* would appear to be the typical methodology of empirical science. (For this reason, empirical disciplines were often characterized as “inductive sciences”–by contraposition to logic and mathematics as “deductive sciences.”) In the 20th century, this view was prominently set forth by Rudolf Carnap. He even developed a quite sophisticated “system of inductive logic” aimed at setting the formal rules intended to ensure the truth, or probable truth, of a theory out of a finite set of application instances.[11]

In explicit and sharp opposition to Carnap, another influential philosopher of science of the 20th century, Karl Popper, proposed the methodology of *falsificationism*.[12] According to it, scientists should certainly try to develop true theories; but they will never be able to ensure that their theories are actually true, not even probably true. Even a very great number of positive instances in the application of a theory doesn’t warrant the assertion of its truth, and not even of its probability. The reason is that a genuinely scientific theory (other than a mere data register) always is, as a matter of principle, more general than the observational data it covers at any given time; therefore, it can never be ruled out that a newly made observation implies that the assumed theory is false after all. What scientists can do is not to prove that a given theory is true, but only that it is *false*: Even if we have a great number of positive data, it suffices to get a single negative case to declare the theory as false. The reason for this, in turn, is a purely logical principle: the rule known as *modus tollens*. Let’s explain it by means of a simple example: Suppose we have laid out the ‘theory’ that all

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9 For a classical defense of this point of view, see Popper (1958), which is the English revised and enlarged edition of the German original Popper (1934); see also Popper (1972).

10 A decidedly anti-realistic interpretation of theories is to be found in van Fraassen (1980). For a more qualified view on this issue, see Balzer, Moulines & Sneed (1987).

11 Carnap’s classical exposition of his inductive logic is to be found in Carnap (1952); further developments are included in Carnap & Jeffrey (1971).

12 For the first time in Popper (1934).

ravens are black, and suppose we have observed thousands of black ravens; it suffices to observe one day a raven of a different colour to dismiss the theory as false. Grounding on these considerations, Popper proposed *falsification* as the only genuine research method in the empirical disciplines.

Few philosophers of science today would accept either Carnap's inductivism or Popper's falsificationism in their original versions. Both encounter very serious problems. The difficulties of inductivism are mainly of formal-logical nature: the systems of inductive logic devised by Carnap and his followers proved to be either formally inconsistent or not applicable to really existing theories.[13] The difficulties of falsificationism are rather of a methodological character: The semantic 'multivocality' of the basic concepts in different models of the one and the same theory as well as the idealizing and approximative nature of the axioms render any well-developed scientific theory flexible enough to assume the presence of 'perturbing factors' that may explain a theory's negative application instances without giving up the theory itself.[14] Nevertheless, both inductivism and falsificationism contain a 'grain of truth'–the first by pointing out the importance of probabilistic reasoning in the application of empirical theories, the second by emphasizing the thoroughly hypothetical character of a theory's basic principles and the need for a serious consideration of those cases where a theory goes astray.

However, the really fundamental shortcomings of the classical approaches in the philosophy of science, such as those of Carnap, Popper, and their followers, lie on a deeper level: They come from their too simple-minded view of the internal structure of a scientific theory. These approaches see the identity of a given theory simply given by a set of axioms and their logical consequences. With such an understanding of the nature of a scientific theory, it obviously follows that a theory can only be either true (if all its axioms are true) or false (if the axioms lead to false consequences). In this kind of approaches, the notion of a model, which we have seen is so important for understanding the way theories relate to experience, doesn't play any essential role; this explains, at least in part, the simple-mindedness of the classical approaches in philosophy of science and their shortcomings when dealing with the question of the relationship between theory and experience. If, on the other hand, we put the notion of a model into the focus of meta-theoretical considerations–as most contemporary approaches in the philosophy of science do–,[15] then we get at a more sophisticated, more flexible, and more adequate view of the structure of scientific theories and of the way they are applied. Conceiving theories not so much as a set of axioms, definitions, and theorems, but rather as a class of models which are structurally similar to each other but different in their empirical interpreta-

[13] For an extensive analysis and criticism of Carnap's inductive logic, see Stegmüller (1974).

[14] The most prominent critic of Popper's falsificationism from a historical point of view is Thomas S. Kuhn in his Kuhn (1962). See also, from a more formal perspective, Stegmüller (1973).

[15] See, for example, van Fraassen (1980), Suppe (1989), or Balzer, Moulines & Sneed (1987).

tion, and taking into account the complexity of the process connecting a "data model" with its corresponding full model, we get the following, more balanced and flexible view of the structure and working of theories.[16] Any well-developed scientific theory consists of an open-ended array of models, all of them formally determined by the same formulae taken as axioms, but receiving different interpretations according to the data model each model is intended to cover. It may then happen (and this is precisely what happens in the normal case) that a single theory consists of some models that *represent* (in the sense explicated above) their data models very well, of some other models that represent them less well, and finally of some models that fit very badly, or not at all. Under this conception, the acceptance or dismissal of a theory cannot be viewed as an 'all-or-nothing' affair. All we can say is that there are theories that work better than others in the sense that they cover more data models, and cover them better, than their rivals. Of course, if a theory is such that none of its models ever covers any of the data models for which the first have been devised, then we can safely dismiss the theory completely. But this is not a case that will usually happen in any scientific discipline satisfying some minimal methodological standards of rigour and intellectual honesty. On the other hand, there has probably been no theory in the history of science such that *all of its models* work perfectly well.

Assuming this idea of theories centred around the concept of model, it becomes plain now why it is difficult to stick to an uncompromising realistic view of scientific theories with respect to their truth or falsity. Strictly speaking, we cannot say anymore that a given theory is either true or false, period. Rather, we should say that a theory is "perfectly true" in some models, "less true" in other models, and "not true at all" in still other models. But, from a rigorously formal point of view, this is not a quite sensible way of speaking. It would be better just to say that a theory is *perfectly adequate* (or "applies perfectly well") in some models, is *less adequate* (or "applies only to some extent") in some other models, and is *completely inadequate* (or "doesn't apply at all") in still other models. In sum, the methodological evaluation of scientific theories comes out as a *gradual* issue.

7. The hierarchic structure of scientific theories

We have seen that conceiving a theory as a plurality of models provides a more perspicuous analysis of its 'essence' than conceiving it as just a set of statements. Now, it is important to be aware of the fact that this plurality is multifarious in two respects. One has already been pointed out: Though all the models constituting one and the same theory satisfy the same axioms (as formulae), their content may vary according to the different empirical interpretations we give to these formulae and the basic terms occurring in them–differences which, in

[16] For a detailed exposition of this view on theories, see Balzer, Moulines & Sneed (1987).

turn, are prompted by the different data models we want to cover within our theory. But there is another important sense in which the plurality of models of a theory is multifarious: The various models of one and the same theory may appear to have different degrees of *generality*. Some models are more general or 'more abstract' than others; or, to put it the other way around, some models are 'nearer' to their corresponding data models than others. This comes, in turn, from the fact that not all the axioms determining the models have the same methodological and epistemological status. Some are 'nearer' to the experiential basis than others. Paraphrasing George Orwell's famous phrase, we could also say that, although all axioms are axiomatic, some axioms are 'more axiomatic' than others. Consider again the example of Newtonian mechanics. The models determined by Newton's three basic principles are extremely general. They are more general, at any rate, than those that in addition have to satisfy the law of gravitation. These, in turn, are more general than those that also have to satisfy the equations for some frictional forces, or whatever. There are also kinds of models that cannot be compared as to whether they are more or less general than other kinds. For example, Newtonian models satisfying equations for frictional forces are neither more nor less general than Newtonian models satisfying Coulomb's laws of electrostatics.[17] This means, by the way, that it would be completely inadequate to say that we consider as models of Newtonian mechanics only those structures that satisfy *all* particular laws appearing in a textbook on Newtonian mechanics. If we would take into consideration only those models that satisfy all of the mechanical laws, we would end up with a theory devoid of empirical content, since there is very likely not a single data model that is covered by all mechanical laws. To deal with some data models we only need, say, Newton's fundamental principles; to deal with others, we need in addition the law of gravitation; to deal with still others, we need Coulomb's laws in addition to the fundamental principles, and so on.

The picture that comes out of these considerations is that of a *hierarchically* constructed array of axioms and their corresponding kinds of models. Some axioms are extremely general and are supposed to be fulfilled in all models of the theory in question; some others are still quite general but are not supposed to be fulfilled in all cases; some others, finally, are very particular and are intended to deal only with very concrete applications, like those that interest engineers. Therefore, any empirical theory that has been developed to a certain extent has, graphically speaking, the form of a 'pyramid': on the top of it, we find very general axioms and their corresponding kinds of models; these become successively *specialized in different directions* until, at some points, we reach the level of very concrete applications.

It is at this point, at the latest, that the simple-minded idea of a scientific theory as a set of axioms completely breaks down. This idea is quite inadequate

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[17] The detailed reconstruction of this example along these lines is to be found in Balzer, Moulines & Sneed (1987), which is grounded on Balzer & Moulines (1982).

to represent the complex, strongly hierarchical and multi-level structure of scientific theories. It is an important task for contemporary philosophy of science to render explicit the general epistemological and methodological consequences of this new conception of theories.

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