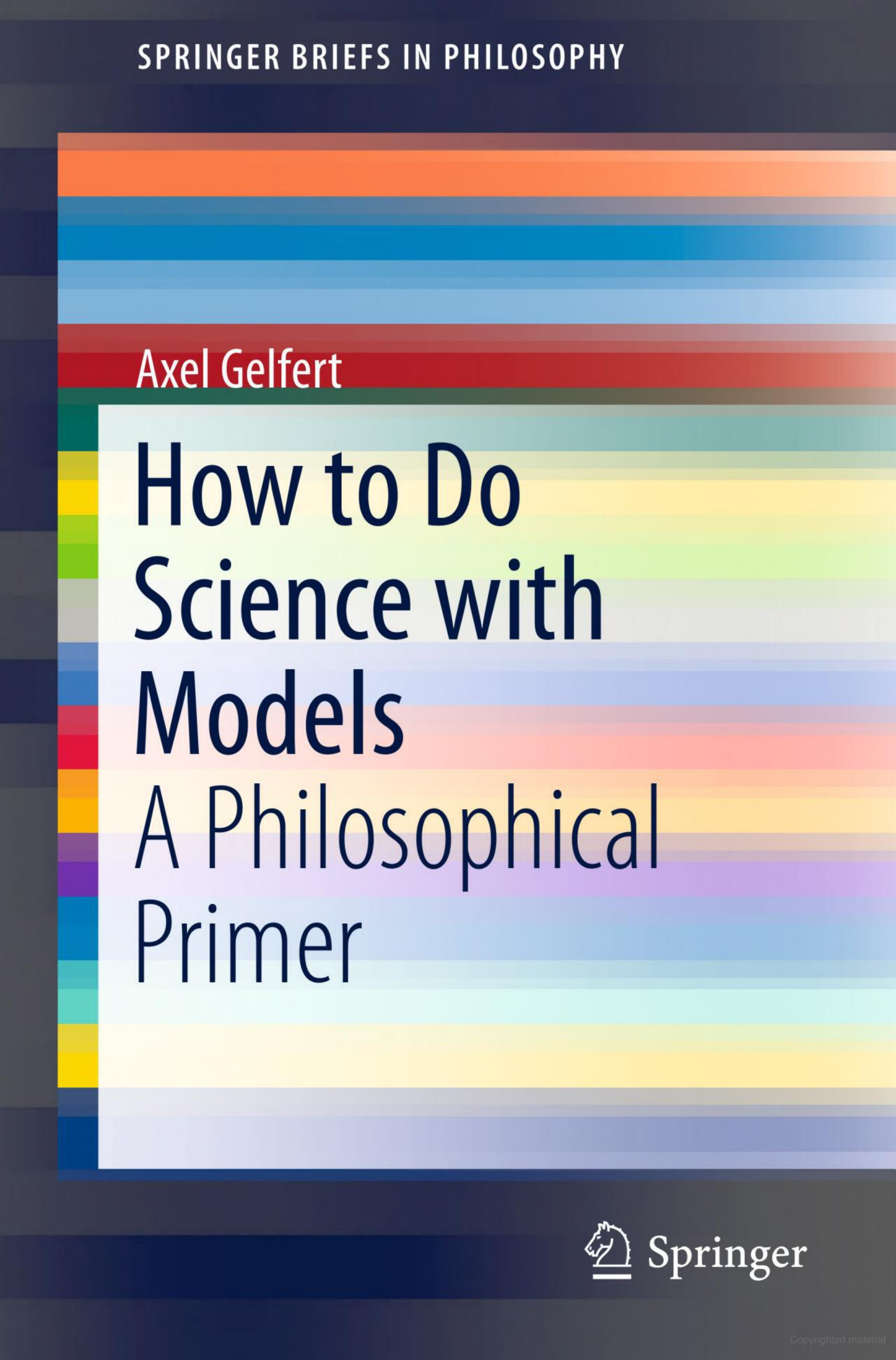

SPRINGER BRIEFS IN PHILOSOPHY

Axel Gelfert

How to Do Science with Models

A Philosophical Primer

Springer

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ISSN 2211-4548 ISSN 2211-4556 (electronic)
SpringerBriefs in Philosophy
ISBN 978-3-319-27952-7 ISBN 978-3-319-27954-1 (eBook)
DOI 10.1007/978-3-319-27954-1

Library of Congress Control Number: 2015958321

Printed on acid-free paper

This Springer imprint is published by SpringerNature
The registered company is Springer International Publishing AG Switzerland

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Chapter 1
Between Theory and Phenomena: What are Scientific Models?

1.1 Introduction

Models can be found across a wide range of scientific contexts and disciplines. Examples include the Bohr model of the atom (still used today in the context of science education), the billiard ball model of gases, the DNA double helix model, scale models in engineering, the Lotka-Volterra model of predator–prey dynamics in population biology, agent-based models in economics, the Mississippi River Basin model (a 200-acre hydraulic model of the waterways in the entire Mississippi River basin!), and general circulation models (GCM) which allow scientists to run simulations of the Earth's climate system. The list could be continued indefinitely, with the number of models across the natural and social sciences growing day by day. Indeed, the deployment of models has not only become central to the scientific enterprise at large, but also to the very image scientists have of themselves. As John von Neumann put it, with some hyperbole: 'The sciences do not try to explain, they hardly even try to interpret, they mainly make models' [1, p. 492]. Whatever shape and form the scientific enterprise might have taken in the absence of models, given their de facto pervasiveness across many disciplines and subdisciplines, it seems safe to say that science without models would not look anything like science as we presently know it.

Philosophical discussions of scientific models likewise distinguish between a bewildering array of different kinds of models. The *Stanford Encyclopedia of Philosophy* gives the following list of model-types that have been discussed by philosophers of science: 'Probing models, phenomenological models, computational models, developmental models, explanatory models, impoverished models, testing models, idealized models, theoretical models, scale models, heuristic models, caricature models, didactic models, fantasy models, toy models, imaginary models, mathematical models, substitute models, iconic models, formal models, analogue models and instrumental models' [2]. As early as 1968, the proliferation of models and model-types, in the sciences as well as in the philosophical literature,

A. Gelfert, *How to Do Science with Models*, SpringerBriefs in Philosophy,
DOI 10.1007/978-3-319-27954-1_1

led Nelson Goodman to lament in his book *Languages of Art:* 'Few terms are used in popular and scientific discourse more promiscuously than "model"' [3, p. 171]. If this was true of science and popular discourse in the late 1960s, it is all the more true of twenty-first century philosophy of science.

As an example of a mathematical model in physics, consider the *Ising model*, proposed in 1925 by the German physicist Ernst Ising as a model of ferromagnetism in certain metals. The model starts from the idea that a macroscopic magnet can be thought of as a collection of elementary magnets, whose orientation determines the overall magnetization. If all the elementary magnets are aligned along the same axis, then the system will be perfectly ordered and will display a maximum value of the magnetization. In the simplest one-dimensional case, such a state can be visualized as a chain of 'elementary magnets', all pointing the same way:

$$\cdots \uparrow\uparrow\uparrow\uparrow\uparrow\uparrow\uparrow\uparrow\uparrow\uparrow\uparrow\uparrow\uparrow\uparrow \cdots$$

The alignment of elementary magnets can be brought about either by a sufficiently strong external magnetic field or it can occur spontaneously, as will happen below a critical temperature, when certain substances (such as iron and nickel) undergo a ferromagnetic phase transition. Whether or not a system will undergo a phase transition, according to thermodynamics, depends on its energy function which, in turn, is determined by the interactions between the component parts of the system. For example, if neighbouring 'elementary magnets' interact in such a way as to favour alignment, there is a good chance that a spontaneous phase transition may occur below a certain temperature. The energy function, then, is crucial to the model and, in the case of the Ising model, is defined as

$$E = -\sum_{i,j} J_{ij} S_i S_j$$

with the variable S_i representing the orientation (+1 or −1) of an elementary magnet at site i in the crystal lattice and J_{ij} representing the strength of interaction between two such elementary magnets at different lattice sites i, j.

Contrast this with *model organisms* in biology, the most famous example of which is the fruit fly *Drosophila melanogaster*. Model organisms are real organisms —actual plants and animals that are alive and reproduce—yet they are used as representations either of another organism (for example when rats are used in place of humans in medical research) or of a biological phenomenon that is more universal (e.g., when fruit flies are used to study the effects of crossover between homologous chromosomes). Model organisms are often bred for specific purposes and are subject to artificial selection pressures, so as to purify and 'standardize' certain features (e.g., genetic defects or variants) that would not normally occur or would occur only occasionally in populations in the wild. As Rachel Ankeny and Sabina Leonelli put it, in their ideal form 'model organisms are thought to be a relatively simplified form of the class of organism of interest' [4, p. 318]; yet it often takes considerable effort to work out the actual relationships between the model organism and its target system (whether it be a certain biological phenomenon or a specific class of target organisms). Tractability and various experimental desiderata—e.g., a short life cycle (to allow for quick breeding) and a relatively small and compact genome (to allow for the quick identification of variants)—take precedence over theoretical questions in the choice of model organisms; unlike for the Ising model, there is no simple mathematical formula that one can rely on to study how one's model behaves, only the messy world of real, living systems.

The Ising model of ferromagnetism and model organisms such as *Drosophila melanogaster* may be at opposite ends of the spectrum of scientific models. Yet the diversity of models that occupy the middle ground between theoretical description and experimental system is no less perplexing. How, one might wonder, can a philosophical account of scientific models aspire to any degree of unity or generality in the light of such variety? One obvious strategy is to begin by drawing distinctions between different overarching types of models. Thus, Max Black [5] distinguishes between four such types: scale models, analogue models, mathematical models, and theoretical models. The basic idea of scale and analogue models is straightforward: a scale model increases or decreases certain (e.g., spatial) features of the target system, so as to render them more manageable in the model; an analogue model also involves a change of medium (as in the Phillips machine, a once-popular hydraulic model of the economy, where the flow of money was represented by the flow of liquids through a system of pumps and valves). Mathematical models are constructed by first identifying a number of relevant variables and then developing empirical hypotheses concerning the relations that may hold between the variables; through (often drastic) simplification, a set of mathematical equations is derived, which may then be evaluated analytically or numerically and tested against novel observations. Theoretical models, finally, begin usually by extrapolating imaginatively from a set of observed facts and regularities, positing new entities and mechanisms, which may be integrated into a possible theoretical account of a phenomenon; comparison with empirical data usually comes only at a later stage, once the model has been formulated in a coherent way. Peter Achinstein [6] includes mathematical models in his definition of 'theoretical model', and proposes an analysis in terms of sets of assumptions about a model's target system. This allows him to include Bohr's model of the atom, the DNA double helix model—considered as a set of structural hypotheses rather than as a physical ball-and-stick model—the Ising model, and the Lotka-Volterra model among the class of theoretical systems.

When a scientist constructs a theoretical model, she may help herself to certain established principles of a more fundamental theory to which she is committed. These may then be adapted or modified, notably by introducing various new assumptions specific to the case at hand. Often, an inner structure or mechanism is posited which is thought to explain features of the target system. The great variety of models that may thus be generated makes vivid just how central the use of models is to the scientific enterprise. At the same time, it might make one wonder whether it is at all reasonable to look for a unitary philosophical account of models.

This has led some commentators to abandon the search for an account of the nature of models and further to the conclusion that, as Bernd Mahr puts it, modeling can only ‘be understood if one stops looking for an answer to the question *of the nature of the model* and starts asking instead *what justifies conceiving of something as a model*’ [7, p. 305]. In the absence of any widely accepted unified account of models, it may be natural to assume, as indeed many contributors to the debate have done, that ‘if all scientific models have something in common, this is not their *nature* but their *function*’ [8, p. 194]. Furthermore, ‘if we accept that models are functional entities, it should come as no surprise that when we deal with scientific models ontologically, we cannot remain silent on how such models function as carriers of scientific knowledge’ [9, p. 120]. Two broad classes of functional characterizations of models can be distinguished, according to which it is either *instantiation* or *representation* that lie at the heart of how models function.

As Ronald Giere [10] sees it, on the *instantial view*, models instantiate the axioms of a theory, where the latter is understood as being composed of linguistic statements, including mathematical statements and equations. By contrast, on the *representational view*, ‘language connects not directly with the world, but rather with a model, whose characteristics may be precisely defined’; the model then connects with the world ‘by way of similarity between a model and designated parts of the world’ [10, p. 56]. Other proponents of the representational view have de-emphasized the role of similarity, while still endorsing representation as one of the key functions of scientific models. Within the class of representational views, one can further distinguish between views that emphasize the *informational* aspects of models and those that take their *pragmatic* aspects to be more central. Anjan Chakravartty nicely characterizes the informational variety of the representational view as follows: ‘The idea here is that a scientific representation is something that bears an objective relation to the thing it represents, on the basis of which it contains information regarding that aspect of the world’ [11, p. 198]. The term ‘objective’ here simply means that the requisite relation obtains independently of the model user’s beliefs or intentions as well as independently of the specific representational conventions he or she might be employing. By contrast, the *pragmatic* variety of the representational view of models posits that models function as representations of their targets in virtue of the cognitive uses to which human reasoners put them. The basic idea is that a scientific model facilitates certain cognitive activities—such as the drawing of inferences about a target system, the derivation of predictions, or perhaps a deepening of the scientific understanding—on the part of its user and, therefore, necessarily involves the latter’s cognitive interests, beliefs, or intentions.

These examples and classifications are necessarily rough sketches, and much of the rest of this book is devoted to giving more depth to our philosophical picture of scientific models. This will involve giving detailed discussions of various cases from across the sciences and exploring their implications for how we should best understand scientific models. The central assumption of this approach is that the key to answering any of the more fundamental questions about scientific models lies in the diversity of their varied uses and functions. While this will require careful attention to the actual uses and applications of scientific models, it would be quite misguided to think that a descriptive approach to scientific modeling could, by itself, tell us what makes something a model, let alone a *good* model. For this, we will need to look beyond the level of case studies and identify possible vantage points from which to judge the success or fruitfulness of a model. A number of philosophical theories have, of course, attempted just that, for example by thinking of models in the same terms as scientific theories. However, one should remain open to the possibility that careful attention to scientific modeling as a practice may itself turn up a 'middle range' of factors which, though strictly speaking not universal, nonetheless help us explain both the success of model-based science and certain recurring patterns of how models are deployed across different disciplines. As I shall argue in later parts of the book, some of the uses and functions of scientific models—e.g., their exploratory role in inquiry—are more akin to certain types of experimentation than to the traditional goals of scientific theories. Furthermore, models often contribute new elements to the theoretical description and empirical investigation of their target systems—elements which are neither part of the fundamental theory nor can be easily 'read off' from the data. Before turning to these questions in more depth, however, it will be instructive to first look more closely at the history of the term 'model' in science, so as to gain a better understanding of what motivates the use of models in scientific inquiry in the first place.

1.2 Models, Analogies, and Metaphor

Given their centrality to contemporary science, it should come as no surprise that scientific models have enjoyed a long and varied history. With our current concepts in hand, it may seem easy to identify past instances of models being employed in science. However, the term 'model' has itself undergone a number of changes throughout the history of science. Indeed, it was not until the nineteenth century that scientists began to engage in systematic self-reflection on the uses and limitations of models. Philosophers of science took even longer to pay attention to models in science, focusing instead on the role and significance of scientific theories. Only from the middle of the twentieth century onwards did models begin to attract significant philosophical interest in their own right. Yet in both science and philosophy, the term 'model' underwent important transformations. In this section, one such transformation—from a narrow focus on mechanical models to our much broader contemporary understanding of the term 'scientific model'—will be traced.

Take, for example, Pierre Duhem's dismissal, in 1914, of what he takes to be the excessive use of models in Maxwell's theory of electromagnetism, as presented in an English textbook published at the end of the nineteenth century:

> Here is a book intended to expound the modern theories of electricity and to expound a new theory. In it there are nothing but strings which move round pulleys which roll around drums, which go through pearl beads, which carry weights; and tubes which pump water

> while others swell and contract; toothed wheels which are geared to one another and engage hooks. We thought we were entering the tranquil and neatly ordered abode of reason, but we find ourselves in a factory. [12, p. 7]

What Duhem is mocking in this passage, which is taken from a chapter titled 'Abstract Theories and Mechanical Models', is a style of reasoning that is dominated by the desire to *visualize* physical processes in purely mechanical terms. His hostility is thus directed at *mechanical* models only—as the implied contrast in the chapter title makes clear—and does not extend to the more liberal understanding of the term 'scientific model' in philosophy of science today.

Indeed, when it comes to the use of *analogy* in science, Duhem is much more forgiving. The term 'analogy', which derives from the Greek expression for 'proportion', itself has multiple uses, depending on whether one considers its use as a rhetorical device or as a tool for scientific understanding. Its general form is that of 'pointing to a resemblance between relations in two different domains, i.e. *A* is related to *B* like *C* is related to *D*' [13, p. 110]. An analogy may be considered merely *formal*, when only the relations (but not the relata) resemble one another, or it may be *material*, when the relata from the two domains (i.e., *A* and *B* on one side, *C* and *D* on the other) have certain attributes or characteristics in common. Duhem's understanding of 'analogy' is more specific, in that he conceives of analogy as being a relation between two sets of statements, such as between one theory and another:

> Analogies consist in bringing together two abstract systems; either one of them already known serves to help us guess the form of the other not yet known, or both being formulated, they clarify the other. There is nothing here that can astonish the most rigorous logician, but there is nothing either that recalls the procedures dear to ample but shallow minds. [12, p. 97]

Consider the following example: When Christiaan Huygens (1629–1695) proposed his theory of light, he did so on the basis of *analogy* with the theory of sound waves: the relations between the various attributes and characteristics of light are similar to those described by acoustic theory for the rather different domain of sound. Thus understood, analogy becomes a legitimate instrument for learning about one domain on the basis of what we know about another. In modern parlance, we might want to say that sound waves provided Huygens with a good *theoretical model*—at least given what was known at the time—for the behaviour of light.

There is, however, a risk of ambiguity in that last sentence—an ambiguity which, as D.H. Mellor [14, p. 283] has argued, it would be wrong to consider harmless. Saying that 'sound waves provide a good model for the theory of light' appears to equate the model *with the sound waves*—as though one physical object (sound waves) could be identified with the model. At first sight this might seem unproblematic, given that, as far as wave-like behaviour is concerned, we do take light and sound to be relevantly analogous. However, while it is indeed the case that 'some of the constructs called "analogy" in the nineteenth century would today be routinely referred to as "models"' [15, p. 46], it is important to distinguish between, on the one hand, 'analogy' as the similarity relation that exists between a theory and another set of statements and, on the other hand, the latter set of statements as the 'analogue' *of the theory*. Furthermore, we need to distinguish between the analogue (e.g., the theory of sound waves, in Huygens's case) and the set of entities *of which the analogue is true* (e.g., the sound waves themselves). (On this point, see [14, p. 283].) What Duhem resents about the naive use of what he refers to as 'mechanical models', is the hasty conflation of the visualized entities—(imaginary) pulleys, drums, pearl beads, and toothed wheels—with what is *in fact* scientifically valuable, namely the relation of analogy that exists between, say, the theory of light and the theory of sound.

This interpretation resolves an often mentioned tension—partly perpetuated by Duhem himself, through his identification of different styles of reasoning (the 'English' style of physics with its emphasis on mechanical models, and the 'Continental' style which prizes mathematical principles above all)—between Duhem's account of models and that of the English physicist Norman Robert Campbell (1880–1949). Thus, Mary Hesse, in her seminal 1963 essay *Models and Analogies in Science* [16], imagines a dialogue between a 'Campbellian' and a 'Duhemist'. At the start of the dialogue, the Campbellian attributes to the Duhemist the following view: 'I imagine that along with most contemporary philosophers of science, you would wish to say that the use of models or analogues is not essential to scientific theorizing and that […] the theory as a whole does not require to be interpreted by means of any model.' To this, the Duhemist, who admits that 'models may be useful guides in suggesting theories', replies: 'When we have found an acceptable theory, any model that may have led us to it can be thrown away. Kekulé is said to have arrived at the structure of the benzene ring after dreaming of a snake with its tail in its mouth, but no account of the snake appears in the textbooks of organic chemistry.' The Campbellian's rejoinder is as follows: 'I, on the other hand, want to argue that models in some sense *are* essential to the logic of scientific theories' [16, pp. 8–9]. The quoted part of Hesse's dialogue has often been interpreted as suggesting that the bone of contention between Duhem and Campbell is the status of *models in general* (in the modern sense that includes theoretical models), with Campbell arguing in favour and Duhem arguing against.

But we have already seen that Duhem, using the language of 'analogy', *does* allow for theoretical models to play an important part in science. This apparent tension can be resolved by being more precise about the target of his criticism: 'Kekulé's snake dream might illustrate the use of a visualizable model, but it certainly does not illustrate the use of an analogy, in Duhem and Campbell's sense' [14, p. 285]. In other words, Duhem is not opposed to scientific models in general, but to its mechanical variety in particular. And, on the point of overreliance on mechanical models, Campbell, too, recognizes that dogmatic attachment to such a style of reasoning is 'open to criticism'. Such a dogmatic view would hold 'that theories are completely satisfactory only if the analogy on which they are based is mechanical, that is to say, if the analogy is with the laws of mechanics' [17, p. 154]. Campbell is clearly more sympathetic than Duhem towards our 'craving for mechanical theories', which he takes to be firmly rooted in our psychology. But he insists that 'we should notice that the considerations which have been offered justify only the attempt to adopt some form of theory involving ideas closely related to those of force and motion; it does not justify the attempt to force all such theories into the Newtonian mould' [17, p. 156]. To be sure, significant differences between Duhem and Campbell remain, notably concerning what *kinds* of uses of analogies in science (or, in today's terminology, of scientific—including theoretical—models) are appropriate. For Duhem, such uses are limited to a heuristic role in the discovery of scientific theories. By contrast, Campbell claims that 'in order that a theory may be valuable […] it must display analogy' [17, p. 129]—though, it should be emphasized again, not necessarily analogy *of the mechanical sort*. (As Mellor argues, Duhem and Campbell differ chiefly in their views of scientific theories and less so in their take on analogy, with Duhem adopting a more 'static' perspective regarding theories and Campbell taking a more realist stance that makes room for scientific models as a way of confirming and modifying a scientific theory; see [14, pp. 286–287].)

It should be said, though, that Hesse's 'Campbellian' and 'Duhemist' are at least partly intended as caricatures and serve as a foil for Hesse's own account of models as analogies. The account hinges on a three-part distinction between 'positive', 'negative', and 'neutral' analogies [16]. Using the billiard ball model of gases as her primary example, Hesse notes that some characteristics are shared between the billiard balls and the gas atoms (or, rather, are ascribed by the billiard ball model to the gas atoms); these include velocity, momentum, and collision. Together, these constitute the *positive* analogy. Those properties we know to belong to billiard balls, but not to gas atoms—such as colour—constitute the *negative* analogy of the model. However, there will typically be properties of the model (i.e., the billiard ball system) of which we do not (yet) know whether they also apply to its target (in this case, the gas atoms). These form the *neutral* analogy of the model. Far from being unimportant, the neutral analogy is crucial to the fruitful use of models in scientific inquiry, since it holds out the promise of acquiring new knowledge about the target system by studying the model in its place: 'If gases are really like collections of billiard balls, except in regard to the known negative analogy, then from our knowledge of the mechanics of billiard balls we may be able to make new predictions about the expected behaviour of gases' [16, p. 10].

In dealing with scientific models we may choose to disregard the negative analogy (which results in what Hesse calls 'model$_1$') and consider only the known positive analogy and the neutral analogy—that is, only those properties that are shared, or for all we know may turn out to be shared, between the target system and its analogue. (On Black's and Achinstein's terminology, mentioned in Sect. 1.1 above, model$_1$ would qualify as a 'theoretical model'.) This, Hesse argues, typically describes our use of models for the purpose of explanation: we resolve to treat model$_1$ as taking the place of the phenomena themselves. Alternatively, we may actively include the negative analogy in our considerations, resulting in what Hesse calls 'model$_2$' or a form of analogue model. Given that, let us assume, the model system (e.g., the billiard balls) was chosen because it was observable—or, at any rate, more accessible than the target system (e.g., the gas)—model$_2$ allows us to study the similarities and dissimilarities between the two analogous domains;

model_2, qua being a model for its target, thus has a deeper structure than the system of billiard balls considered in isolation—and, like model_1, importantly includes the neutral analogy, which holds out the promise of novel insights and predictions. As Hesse puts it, in the voice of her Campbellian interlocutor: 'My whole argument is going to depend on these features [of the neutral analogy] and so I want to make it clear that I am not dealing with static and formalized theories, corresponding only to the known positive analogy, but with theories in the process of growth' [16, pp. 12–13].

Models have been discussed not only in terms of analogy, but also in terms of metaphor. 'Metaphor', more explicitly than 'analogy', refers to the linguistic realm: a metaphor is a linguistic expression that involves at least one part that is being transferred from a domain of discourse where it is common to another—the target domain—where it is uncommon. The existence of an analogy may facilitate such a transfer of linguistic expression; at the same time, it is entirely possible that 'it is the metaphor that prompts the recognition of analogy' [13, p. 114]—both are compatible with one another and neither is obviously prior to the other. Metaphorical language is widespread in science, not just in connection with models: for example, physicists routinely speak of 'black holes' and 'quantum tunneling' as important predictions of general relativity theory and quantum theory, respectively. Yet, as Janet Soskice and Rom Harré note, there is a special affinity between models and metaphor:

> The relationship of model and metaphor is this: if we use the image of a fluid to explicate the supposed action of the electrical energy, we say that the fluid is functioning as a model for our conception of the nature of electricity. If, however, we then go on to speak of the 'rate of flow' of an 'electrical current', we are using metaphorical language based on the fluid model. [18, p. 302]

In spite of this affinity, it would not be fruitful to simply equate the two—let alone jump to the conclusion that, in the notion of 'metaphor', we have found an answer to the question 'What is a model?'. Models and metaphors both deal in descriptions, and as such they may draw on analogies we have identified between two otherwise distinct domains; more, however, needs to be said about the nature of the relations that need to be in place for something to be considered a (successful) model of its target system or phenomenon.

1.3 The Syntactic View of Theories

The syntactic view of theories originated from combining the fundamental tenets of two research programmes: the philosophical programme, going back to Pierre Duhem (1861–1961) and Henri Poincaré (1854–1912), of treating (physical) theories as systems of hypotheses designed to 'save the phenomena', and the mathematical programme, pioneered by David Hilbert (1862–1943), which sought to formalize (mathematical) theories as axiomatic systems. By linking the two, it seemed possible to identify a theory with the set of logical consequences that could be derived from its fundamental principles (which were to be treated as axioms), using only the rules of the language in which the theory was formulated.

The label 'syntactic' derives from the distinction, typically drawn in the study of formal languages and their interpretations, between the syntax and the semantics of a language L. The *syntax* of a language L is made up of the vocabulary of L, along with the rules that determine which sequence of symbols counts as a well-formed expression in L; in turn, the *semantics* of L provides interpretations of the symbolic expressions in L, by mapping them onto another relational structure R, such that all well-formed expressions in L are rendered intelligible (for example via rules of composition) and can be assessed in terms of their truth or falsity in R. Though this distinction is sharpest in logic, the restriction to formal languages is often dropped, such that it provides a framework for thinking also about scientific theories (which are often formulated in ways that are closer to natural language than to logic). The contrast between the syntax and the semantics of a language allows for two different perspectives on the notion of a 'theory'. A theory T may either be defined syntactically, as the set of all those sentences that can be derived, through a proper application of the syntactic rules, from a set of axioms (that is, statements that are taken to be fundamental); or it may be defined semantically, as all those (first-order) sentences that a particular structure, M, satisfies. The syntactic view adopts the former perspective and seeks to fashion scientific theories in the image of fully axiomatized systems of statements, perhaps the best example of which would be Euclidean geometry, which consists of five axioms and all the theorems derivable from them using geometrical rules.

In spite of its emphasis on syntax, the syntactic view is not entirely divorced from questions of semantics. When it comes to scientific theories, we are almost always dealing with *interpreted* sets of sentences, some of which—the fundamental principles or axioms—are more basic than others, with the rest derivable using syntactic rules. The question then arises at which level interpretation of the various elements of a theory is to take place. This is where the slogan 'to save the phenomena' points us in the right direction: on the syntactic view, interpretation only properly enters at the level of matching singular theoretical predictions, formulated in strictly observational terms, with the observable phenomena. Higher-level interpretations—for example pertaining to purely theoretical terms of a theory (such as posited unobservable entities, causal mechanisms, laws etc.)—would be addressed through *correspondence rules*, which offered at least a partial interpretation, so that the meaning of such higher-level terms of a theory could be explicated in observational sentences. In order for this approach to work, a clear distinction between theoretical and observational terms needs to be maintained. As the growing recognition of the theory-ladenness of observation began to call this distinction into question, so correspondence rules began to lose much of their appeal.

As an illustration of what the syntactic view looks like in practice, consider the example of classical mechanics. Similar to how Euclidean geometry can be fully derived from a set of five axioms, classical mechanics is fully determined by

Newton's laws of mechanics. At a purely formal level, it is possible to provide a fully syntactic axiomatization in terms of the relevant symbols, variables, and rules for their manipulation—that is, in terms of what Rudolf Carnap (1891–1970) called the 'calculus of mechanics'. If one takes the latter as one's starting point, it requires interpretation of the results derived from within this formal framework, in order for the calculus to be recognizable as a theory of mechanics, i.e. of physical phenomena. In the case of mechanics, we may have no difficulty stating the axioms in the form of the (physically interpreted) *Newtonian laws of mechanics*, but in other cases—perhaps in quantum mechanics—making this connection with observables may not be so straightforward. As Carnap notes, '[t]he relation of this theory [= the physically interpreted theory of mechanics] to the calculus of mechanics is entirely analogous to the relation of physical to mathematical geometry' [19, p. 57]. As in the Euclidean case, the syntactic view identifies the theory with a formal language or calculus (including, in the case of scientific theories, relevant correspondence rules), 'whose interpretation—what the calculus is a theory *of*—is fixed at the point of application' [20, p. 125].

For proponents of the syntactic view of theories, models played a marginal or at best auxiliary role. Carnap famously urged his readers 'to realize that the discovery of a model has no more than an aesthetic or didactic or at best a heuristic value, but is not at all essential for a successful application of the physical theory' [21, p. 210]. Others were more forgiving, but the role they attributed to models looks rather unlike how models are, in fact, employed in science. For example, Richard Braithwaite (1900–1990) made room for models as a way of—'hypothetically', as it were—addressing an epistemological challenge we face when we confront scientific theories. As he sees it, theoretical principles, though meant to explain observable facts and in this sense '*logically prior* to the lower-level hypotheses', are '*epistemologically posterior*' to them in the sense that the meaning of theoretical terms (and, by extension, the development of theory) is itself dependent on empirical findings [22, p. 89]. Models are a way of bringing these opposite movements into alignment, if only hypothetically: in a model, 'the logically prior premisses determine the meaning of the terms occurring in the representation in the calculus of the conclusion' [22, p. 90]. While Braithwaite acknowledges that this is 'frequently the most convenient way of thinking about the structure of the theory' [22, p. 91], he emphasizes that models lend themselves to abuse. In particular, he identifies two dangers: the hasty identification of a theory with a model for it, and the projection of characteristics of the model—notably, the logical necessity of some of its features—onto the theory. Though more accommodating than other syntactic theorists, Braithwaite's discussion still ends with a stern warning: 'The price of the employment of models is eternal vigilance' [22, p. 93].

A further criticism, which is directed at both the syntactic and the semantic view (see next section), argues that underlying both views is a misguided general picture of how science works. As Nancy Cartwright has pointedly argued, there is a shared —mistaken—assumption that theories are a bit like vending machines: '[Y]ou feed it input in certain prescribed forms for the desired output; it gurgitates for a while; then it drops out the sought-for representation, plonk, on the tray, fully formed, as

Athena from the brain of Zeus'. This limits what we can do with models, in that there are only two stages: firstly, 'eyeballing the phenomenon, measuring it up, trying to see what can be abstracted from it that has the right form and combination that the vending machine can take as input; secondly, […] we do either tedious deduction or clever approximation to get a facsimile of the output the vending machine would produce' [23, p. 247]. Cartwright rejects the assumed automatism implicit in the 'vending machine view', which she sees as fueling false hopes of a shortcut from evidence to theory, as though an assessment of the evidential significance of observations and their relation to our hypothesized models and theories could ever be divorced from the specific empirical circumstances of the case at hand. By contrast, real science, including model construction, 'is an incredibly difficult and creative activity'—in need of a 'much more textured, and […] much more laborious, account' [23, p. 247/248] than the 'vending machine view'. Even if this stark contrast may seem a little exaggerated, the fact remains that, by modeling theories after first-order formal languages, the syntactic view limits our understanding of what theories and models are and what we can do with them.

1.4 The Semantic View

One standard criticism of the syntactic view is that it conflates scientific theories with their linguistic formulations. Proponents of the semantic view argue that by adding a layer of (non-linguistic) structures between the linguistic formulations of theories and our assessment of them, one can side-step many of the problems faced by the syntactic view. According to the semantic view, a theory should be thought of as the set of set-theoretic structures that satisfy the different linguistic formulations of the theory. A structure that provides an interpretation for, and makes true, the set of sentences associated with a specific linguistic formulation of the theory is called a *model of the theory*. Hence, the semantic view is often characterized as conceiving of theories as 'collections of models'. This not only puts models—where these are to be understood in the logical sense outlined earlier—centre-stage in our account of scientific theories, but also renders the latter fundamentally *extra-linguistic* entities.

An apt characterization of the semantic view is given by Frederick Suppe as follows:

> This suggests that theories be construed as propounded abstract *structures* serving as models for sets of interpreted sentences that constitute the linguistic formulations. […W]hat the theory does is directly describe the behavior of abstract systems, known as *physical systems*, whose behaviors depend only on the selected parameters. However, physical systems are abstract replicas of actual phenomena, being what the phenomena *would have been* if no other parameters exerted an influence. [24, pp. 82–83]

According to a much-quoted remark by one of the main early proponents of the semantic view, Patrick Suppes, 'the meaning of the concept of model is the same in

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