2. Models and Theories

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Both the received view (RV) and the semantic view (SV) of scientific theories are explained. The arguments against the RV are outlined in an effort to highlight how focusing on the syntactic character of theories led to the difficulty in characterizing theoretical terms, and thus to the difficulty in explicating how theories relate to experiment. The absence of the representational function of models in the picture drawn by the RV becomes evident; and one does not fail to see that the SV is in part a reaction to – what its adherents consider to be an – excessive focus on syntax by its predecessor and in part a reaction to the complete absence of models from its predecessor's philosophical attempt to explain the theory–experiment relation. The SV is explained in an effort to clarify its main features but also to elucidate the differences between its different versions. Finally, two kinds of criticism are explained that affect all versions of the SV but which do not affect the view that models have a warranted degree of importance in scientific theorizing.

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Scientists use the term *model* with reference to iconic or scaled representations, analogies, and mathematical (or abstract) descriptions. Although all kinds of models in science may be philosophically interesting, mathematical models stand out. Representation with iconic or scale models, for instance, mostly applies to a particular state of a system at a particular time, or it requires the mediation of a mathematical (or abstract) model in order to relate to theories. Representation via mathematical models, on the other hand, is of utmost interest because it applies to *types* of target systems and it can be used to draw inferences about the time-evolution of such systems, but more importantly for our purposes because of its obvious link to scientific theories.

In the history of philosophy of science, there have been two systematic attempts to explicate the relation of such models to theory. The first is what had been labeled the *received view* (RV) of scientific theories that grew out of the logical positivist tradition. According to this view, theories are construed as formal axiomatic calculi whose logical consequences extend to observational sentences. Models are thought to have no representational role; their role is understood metamathematically, as interpretative structures of subsets of sentences of the formal calculus. Ultimately it became clear that such a role ascribed to models does not do justice to how science achieves theoretical representations of phenomena. This conclusion was reached largely due to the advent of the second systematic attempt to explore the relation between theory and models, the *semantic view* (SV) or model-theoretic view of scientific theories. The semantic view regards theories as classes of models that are directly defined without resort to a formal calculus. Thus, models in this view are integral parts of theories, but they are also the devices by which representation of phenomena is achieved.

Although, the SV recognized the representational capacity of models and exposed that which was concealed by the logical positivist tradition, namely that one of the primary functions of scientific models is to apply the abstract theoretical principles in ways that actual physical systems can be represented, it also generated a debate concerning the complexities involved in scientific representation. This recent debate has significantly enhanced our understanding of the representational role of scientific models. At the same time it gave rise, among other things, to questions regarding the relation between models and theory. The adherents of the SV claim that a scientific theory is identified with a class of models, hence that models are constitutive parts of theory and thus they represent by means of the conceptual apparatus of theory. The critics of the SV, however, argue that those models that are successful representations of physical systems utilize a much richer conceptual apparatus than that provided by theory and thus claim that they should be understood as partially autonomous from theory.

A distinguishing characteristic of this debate is the notion of representational model, that is, a scientific entity which possesses the necessary features that render it representational of a physical system. In the SV, theoretical models, that is, mathematical models that are constitutive parts of theory structure, are considered to be representational of physical systems. Its critics, however, argue that in order to provide a model with the capacity to represent actual physical systems, the theoretical principles from which the model arises are typically supplemented with ingredients that derive from background knowledge, from semiempirical results and from experiment. In order to better understand the character of successful representational models, according to this latter view, we must move away from a purely theory-driven view of model construction and also place our emphasis on the idea that representational models are entities that consist of assortments of the aforementioned sorts of conceptual ingredients.

In order to attain insight into how models could relate to theory and also be able to use that insight in addressing other issues regarding models, in what follows, I focus on the RV and the SV of scientific theories. Each of the two led to a different conception of the nature of theory structure and subsequently to a different suggestion for what scientific models are, what they are used for, and how they function. In the process of explicating these two conceptions of theory structure, I will also review the main arguments that have been proposed against them. The RV has long been abandoned for reasons that I shall explore in Sect. 2.1, but the SV lives on despite certain inadequacies that I shall also explore in Sect. 2.2. Toward the end of Sect. 2.2, in Sect. 2.2.4, I shall very briefly touch upon the more recent view that the relation between theory and models is far more complex than advocates of the RV or the SV have claimed, and that models in science demonstrate a certain degree of partial autonomy from the theory that prompts their construction and because of this a unitary account of models obscures significant features of scientific modeling practices.

2.1 The Received View of Scientific Theories

What has come to be called the RV of scientific theories is a conception of the structure of scientific theories that is associated with logical positivism, and which was the predominant view for a large part of the twentieth century. It is nowadays by and large overlooked hence it is anything but received. Despite its inappropriate label, clarifying its major features as well as understanding the major philosophical arguments that revealed its inadequacies would not only facilitate acquaintance with the historical background of the debate about the structure of scientific theories and give the reader a flavor of the difficulties involved in characterizing theory structure, but it would also be helpful in understanding some characteristics of contemporary views and how models came to occupy central stage in current debates on how theories represent and explain phenomena. With this intention in mind, I proceed in this section by briefly explaining the major features of the RV and continue with sketching the arguments that exposed its weaknesses in Sects. 2.1.1–2.1.6.

The RV construes scientific theories as Hilbert-style formal axiomatic calculi, that is, axiomatized sets of sentences in first-order predicate calculus with identity. A scientific theory is thus identified with a formal language, L, having the following features. The nonlogical terms of L are divided into two disjoint classes: (1) the theoretical terms that constitute the theoretical vocabulary, V_T, of L and (2) the observation terms that constitute the observation vocabulary, V_O, of L. Thus, L can be thought of as consisting of an observation language, L_O, that is, a language that consists only of observation terms, a theoretical language, L_T, that is, a language that consists only of theoretical terms, and a part that consists of mixed sentences that are made up of both observation and theoretical terms. The theoretical postulates or the axioms of the theory, T (i. e., what we, commonly, refer to as the high-level scientific laws), consist only of terms from V_{T}. This construal of theories is a syntactic system, which naturally requires semantics in order to be useful as a model of scientific theories.

It is further assumed that the terms of V_{O} refer to directly observable physical objects and directly observable properties and relations of physical objects. Thus the semantic interpretation of such terms, and the sentences belonging to L_{O}, is provided by direct observation. The terms of V_{T}, and subsequently all the other sentences of L not belonging to L_{O}, are partially interpreted via the theoretical postulates, T, and – a finite set of postulates that has come to be known as – the *correspondence rules*, C. The latter are mixed sentences of L, that is, they are constructed with at least one term from each of the two classes V_{T} and V_{O}. (The reader could consult *Suppe* [2.1] for a detailed exposition of the RV, but also for a detailed philosophical study of the developments that the RV underwent under the weight of several criticisms until it reached, what Suppe calls, the "final version of the RV").

We could synopsize how scientific theories are conceived according to the RV as follows: The scientific laws, which as noted constitute the axioms of the theory, specify relations holding between the theoretical terms. Via a set of *correspondence rules*, theoretical terms are reduced to, or defined by, observation terms. Observation terms refer to objects and relations of the physical world and thus are interpreted. Hence, a scientific theory, according to the RV, is a formal axiomatic system having as point of departure a set of theoretical postulates, which when augmented with a set of correspondence rules has deductive consequences that stretch all the way to terms, and sentences consisting of such terms, that refer to the directly observable physical objects. Since according to this view, the backbone of a scientific theory is the set of theoretical postulates, T, and a partial interpretation of L is given via the set of correspondence rules, C, let TC (i. e., the union set of T and C) designate the scientific theory.

From this sketch, it can be inferred that the RV implies several philosophically interesting things. For the purposes of this chapter, it suffices to limit the discussion only to those implications of the RV that are relevant to the criticisms that have contributed to its downfall. These implications, which in one way or another relate to the difficulty in characterizing V_{T} terms, are:

1. It relies on an observational–theoretical distinction of the terms of L.
2. It embodies an analytic–synthetic distinction of the sentences of L.
3. It employs the obscure notion of correspondence rules to account for the interpretation of theoretical terms and to account for theory application.
4. It does not assign a representational function to models.
5. It assigns a deductive status to the relation between empirical theories and experiment.
6. It commits to a theory consistency condition and to a meaning invariance condition.

2.1.1 The Observation–Theory Distinction

The separation of L into V_{O} and V_{T} terms implies that the RV requires an observational–theoretical distinction in the terms of the vocabulary of the theory. This idea was criticized in two ways. The first kind of objection to the observation–theory distinction relied on a twofold argument. On the one hand, the critics claim that an observation–theory distinction of scientific terms cannot be drawn; and on the other, that a classification of terms following such a distinction would give rise to a distinction of observational–theoretical statements, which also cannot be drawn for scientific languages. The second kind of objection to the distinction relies on attempts to establish accounts of *observation* that are incompatible with the observation–theory distinction and on showing that observation statements are theory laden.

The Untenability of the Observation–Theory Distinction

The argument of the first kind that focuses on the untenability of the observation–theory distinction is due to *Achinstein* [2.2, 3] and *Putnam* [2.4]. Achinstein explores the sense of observation relevant to science, that is, "the sense in which observing involves visually attending to something," and he claims that this sense exhibits the following characteristics:

1. Observation involves attention to the various aspects or features of an item depending on the observer's concerns and knowledge.
2. Observation does not necessarily involve recognition of the item.
3. Observation does not imply that whatever is observed is in the visual field or in the line of sight of the observer.
4. Observation could be achieved indirectly.
5. The description of what one observes can be done in different ways (The reader could refer to *Achinstein* [2.3, pp. 160–165] for an explication of these characteristics of observation by the use of specific examples).

If now one urges an observation–theory distinction by simply constructing lists of observable and unobservable terms (as proponents of the RV according to Achinstein do), the distinction becomes untenable. For example, according to typical lists of unobservables, *electron* is a theoretical term. But based on points (3) and (4) above, Achinstein claims, this could be rejected. Similarly based on point (5), Achinstein also rejects the tenability of such a distinction at the level of statements, because "what scientists as well as others observe is describable in many different ways, using terms from both vocabularies" [2.3, p. 165].

Furthermore, if, as proponents of the RV have often claimed, (For instance, *Hempel* [2.5], *Carnap* [2.6] and [2.7]), items in the observational list are directly observable whereas those in the theoretical list are not, then *Achinstein* [2.3, pp. 172–177] claims that a close construal of *directly observable* reveals that the desired classification of terms into the two lists fails. He explains that *directly observable* could mean that it can be observed without the use of instruments. If this is what advocates of the RV require, then it does not warrant the distinction. First, it is not precise enough to classify things seen by images and reflections. Second, if something is not observable without instruments means that no aspect of it is observable without instruments then things like temperature and mass would be observables, since some aspects of them are detected without instruments. If however directly observable means that no instruments are required to detect its presence, then it would be insufficient because one cannot talk about the presence of temperature. Finally, if it means that no instruments are required to measure it or its properties, then such terms as volume, weight, etc. would have to be classified as theoretical terms. Hence, Achinstein concludes that the notion of direct observability is unclear and thus fails to draw the desired observation–theory distinction.

Along similar lines, *Putnam* [2.4] argues that the distinction is completely *broken-backed* mainly for three reasons. First, if an observation term is one that only refers to observables then there are no observation terms. For example, the term *red* is in the observable class but it was used by Newton to refer to a theoretical term, namely red corpuscles. Second, many terms that refer primarily to the class of unobservables are not theoretical terms. Third, some theoretical terms, that are of course the outcome of a scientific theory, refer primarily to observables. For example, the theory of evolution, as put forward by Darwin, referred to observables by employing theoretical terms.

What these arguments accomplish is to highlight the fact that scientific languages employ terms that cannot clearly and easily be classified into observational or theoretical. They do not however show the untenability of the observation–theory distinction as employed by the RV. As *Suppe* [2.8] correctly observes, what they show is that the RV needs a sufficiently rich artificial language for science, no matter how complex it may turn out to be. Such a language, in which presumably the observation–theory distinction is tenable, must have a plethora of terms, such that, to use his example, the designated term red_{o} will refer to the observable occurrences of the predicate red, and the designated term red_{t} will refer to the unobservable occurrences.

The Theory-Ladenness of Observation

Hanson's argument is a good example of the second kind, in which an attempt is made to show that there is no theory-neutral observation language and that observation is theory-laden and thus establish an account of *observation* that is incompatible with the observation–theory distinction required by the RV (*Hanson* [2.9, pp. 4–30]. *Hanson* [2.10, pp. 59–198]. Also *Suppe* [2.1, pp. 151–166]). He does this by attempting to establish that an observation language that intersubjectively can be given a theory-independent semantic interpretation, as the RV purports, cannot exist.

He begins by asking whether two people see the same things when holding different theories. We could follow his argument by reference to asking whether Kepler and Tycho Brahe see the same thing when looking at the sun rising. Kepler, of course, holds that the earth revolves around the sun, while Tycho holds that the sun revolves around the earth. Hanson addresses this question by considering ambiguous figures, that is, figures that sometimes can be seen as one thing and other times as another. The most familiar example of this kind is the duck–rabbit figure.

When confronted with such figures, viewers see either a duck or a rabbit depending on the perspective they take, but in both cases they see the same distal object (i. e., the object that emits the rays of light that impinge the retina). Hanson uses this fact to develop a sequence of arguments to counter the standard interpretations of his time. There were two standard interpretations at the time. The first was that the perceptual system delivers the same visual representation and then cognition (thought) interprets this either as a duck or as a rabbit. The other was that the perceptual system outputs both representations and then cognition chooses one of the two. Both interpretations are strongly linked with the idea that the perceptual process and the cognitive process function independently of one another, that is, the perceptual system delivers its output independent of any cognitive influences. However, Hanson challenges the assumption that the two observers see the same thing and via thought they interpret it differently. He claims that perception does not deliver either a duck or a rabbit, or an ambiguous figure, and then via some other independent process thought chooses one or the other. On the contrary, the switch from seeing one thing to seeing the other seems to take place spontaneously and moreover a process of back and forth seeing without any thinking seems to be involved. He goes on to ask, what could account for the difference in what is seen? His answer is that what changes is the organization of the ambiguous figure as a result of the conceptual background of each viewer. This entails that what one sees, the percept, depends on the conceptual background that results from one's experience and knowledge, which means that thought affects the formation of the percept; thus perception and cognition become intertwined. When Tycho and Kepler look at the sun, they are confronted with the same distal object but they see different things because their conceptual organizations of their experiences are vastly different. In other words, Hanson's view is that the percept depends on background knowledge, which means that cognition influences perceptual processing. Consequently, observation is theory laden, namely, observation is conditional on background knowledge.

By this argument, Hanson undermines the RV's position, which entails that Kepler and Brahe see the same thing but interpret it differently; and also establishes that conceptual organizations are features of *seeing* that are indispensable to scientific observation and thus that Kepler and Brahe see two different things because perception inherently involves interpretation, since the former is conditional on background knowledge. It is, however, questionable whether Hanson's arguments are conclusive. *Fodor* [2.11–13], *Pylyshyn* [2.14, 15], and *Raftopoulos* [2.16–18], for example, have extensively argued on empirical grounds that perception, or at least a part of it, is theory independent and have proposed explanations of the ambiguous figures that do not invoke cognitive effects in explaining the percept and the switch between the two interpretations of the figure. This debate, therefore, has not yet reached its conclusion; and many today would argue that fifty or so years after Hanson the arguments against the theory ladenness of observation are much more tenable.

2.1.2 The Analytic–Synthetic Distinction

The RV's dependence on the observation–theory distinction is intimately linked to the requirement for an analytic–synthetic distinction. An argument to defend this claim is given by *Suppe* [2.1, pp. 68–80]. Here is a sketch of that argument. The analytic–synthetic distinction is embodied in the RV, because (as suggested by *Carnap* [2.19]) implicit in *TC* are meaning postulates (or semantical rules) that specify the meanings of sentences in L. However, if meaning specification were the only function of *TC* then *TC* would be analytic, and in such case it would not be subject to empirical investigation. *TC* must therefore have a factual component, and the meaning postulates must separate the meaning from the factual component. This would imply an analytic–synthetic separation, if those sentences in L that are logical truths or logical consequences of the meaning postulates are analytic and all nonanalytic sentences are understood to be synthetic. Moreover, any nonanalytic sentence in L taken in conjunction with the class of meaning postulates would have certain empirical consequences. If the conjunction is refuted or confirmed by directly observable evidence, this will reflect only on the truth value of the conjunction and not on the meaning postulates. Hence such conjunctive sentences can only be synthetic. Thus every nonanalytic sentence of L_O and every sentence of L constituted by a mixed vocabulary is synthetic. So the observation–theory distinction supports an analytic–synthetic distinction of the sentences of L.

The main criticism against the analytic–synthetic distinction consists of attempts to show its untenability. *Quine* [2.20] points out that there are two kinds of analytic statements: (a) logical truths, which remain true under all interpretations, and (b) statements that are true by virtue of the meaning of their nonlogical terms, for example, *No bachelor is married*. He then argues that the analyticity of statements of the second kind cannot be established without resort to the notion of synonymy, and that the latter notion is just as problematic as the notion of analyticity. The argument runs roughly as follows. Given that meaning (or intension) is clearly distinguished from its extension, that is, the class of entities to which it refers, a theory of meaning is primarily concerned with cognitive synonymy (i. e., the synonymy of linguistic forms). For example, to say that *bachelor* and *unmarried man* are cognitively synonymous is to say that they are interchangeable in all contexts without change of truth value. If such were the case then the statement *No bachelor is married* would become *No unmarried man is married*, which would be a logical truth. In other words, statements of kind (b) are reduced to statements of kind (a) if only we could interchange synonyms for synonyms. But as Quine argues, the notion of interchangeability salva veritate is an extensional concept and hence does not help with analyticity. In fact, no analysis of the interchangeability salva veritate account of synonymy is possible without recourse to analyticity, thus making such an effort circular, unless interchangeability is "[. . .] relativized to a language whose extent is specified in relevant respects" [2.20, p. 30]. That is to say, we first need to know what statements are analytic in order to decide which expressions are synonymous; hence appeal to synonymy does not help with the notion of analyticity.

Similarly *White* [2.21] argues that an artificial language, L_1, can be constructed with appropriate definitional rules, in which the predicates P_1 and Q_1 are synonymous whereas P_1 and Q_2 are not; hence making such sentences as $\forall x\,(P_1(x) \to Q_1(x))$ logical truths and such sentences as $\forall x\,(P_1(x) \to Q_2(x))$ synthetic. In a different artificial language L_2, P_1 could be defined to be synonymous to Q_2 and not to Q_1, hence making the sentence $\forall x\,(P_1(x) \to Q_2(x))$ a logical truth and the sentence $\forall x\,(P_1(x) \to Q_1(x))$ synthetic. This relies merely upon convention. However, he asks, in a natural language what rules are there that dictate what choice of synonymy can be made such that one formula is a synthetic truth rather than analytic? The key point of the argument is therefore that in a natural language or in a scientific language, which are not artificially constructed and which do not contain definitional rules, the notion of analyticity is unclear.

Nevertheless, it could be argued that such arguments as the above are not entirely conclusive, primarily because the RV is not intended as a description of actual scientific theories. Rather, the RV is offered as a *rational reconstruction* of scientific theories, that is, an explication of the structure of scientific theories. It does not aim to describe how actual theories are formulated, but only to indicate a logical framework (i. e., a canonical linguistic formulation) in which theories can be essentially reformulated. Therefore, all that proponents of the RV, needed to show was that the analytic–synthetic distinction is tenable in some artificial language (with meaning postulates) in which scientific theories could potentially be reformulated. In view of this, in order for the RV to overcome the obscurity of the notion of analyticity, pointed out by Quine and White, it would require the conclusion of a project that Carnap begun: To spell out a clear way by which to characterize meaning postulates for a specified theoretical language (This is clearly *Carnap*'s intention in his [2.19]).

2.1.3 Correspondence Rules

In order to distinguish the character and function of theoretical terms from speculative metaphysical ones (e.g., *unicorn*), logical positivists sought for a connection of theoretical to observational terms by giving an analysis of the empirical nature of theoretical terms contrary to that of metaphysical terms. This connection was formulated in what we can call, following *Achinstein* [2.22], the *Thesis of Partial Interpretation*, which is basically the following: As indicated above, in the brief sketch of the main features of the RV, the RV allows that a complete empirical semantic interpretation in terms of directly observables is given to V_O terms and to sentences that belong to L_O. However, no such interpretation is intended for V_T terms and consequently for sentences of L containing them. It is *TC* as a whole that supplies the empirical content of V_T terms. Such terms receive a partial observational meaning indirectly by being related to sets of observation terms via correspondence rules. To use one of Achinstein's examples [2.22, p. 90]:

> "it is in virtue of [a correspondence-rule] which connects a sentence containing the theoretical term *electron* to a sentence containing the observational term *spectral line* that the former theoretical term gains empirical meaning within the Bohr theory of the atom"

Correspondence rules were initially introduced to serve three functions in the RV:

1. To define theoretical terms.
2. To guarantee the cognitive significance of theoretical terms.
3. To specify the empirical procedures for applying theory to phenomena.

In the initial stages of logical positivism it was assumed that if observational terms were cognitively significant, then theoretical terms were cognitively significant if and only if they were explicitly defined in terms of observational terms. The criteria of explicit definition and cognitive significance were abandoned once proponents of the RV became convinced that dispositional terms, which are cognitively significant, do not admit of explicit definitions (*Carnap* [2.23, 24], also *Hempel* [2.25, pp. 23–29], and *Hempel* [2.5]). Consider, for example, the dispositional term *tearable* (let us assume all the necessary conditions for an object to be torn apart hold), if we try to explicitly define it in terms of observables we end up with something like this:

> "An object x is tearable if and only if, if it is pulled sharply apart at time t then it will tear at t (assuming for simplicity that pulling and tearing occur simultaneously)."

The above definition could be rendered as $\forall x\,(T(x) \leftrightarrow \forall t(P(x,t) \to Q(x,t)))$, where, T is the theoretical term *tearable*, P is the observational term *pulled apart*, and Q is the observational term *tears*. But this does not correctly define the actual dispositional property *tearable*, because the right-hand side of the biconditional will be true of objects that are never pulled apart. As a result, some objects that are not tearable and have never being pulled apart will by definition have the property *tearable*.

Because of this, *Carnap* [2.23, 24] proposed to replace the construal of correspondence rules as explicit definitions, by *reduction sentences* that partially determine the observational content of theoretical terms. A reduction sentence defined the dispositional property tearable as follows: $\forall x \forall t\ (P(x,t) \rightarrow (Q(x,t) \leftrightarrow T(x)))$. That is, (*Carnap* calls such sentences *bilateral reduction sentences* [2.23, 24]):

> "If an object x is pulled-apart at time t, then it tears at time t if and only if it is tearable."

Unlike the explicit definition case, if a is a non-tearable object that is never pulled apart then it is not implied that $T(a)$ is true. What will be implied, in such case, is that $\forall t\ (P(a,t) \rightarrow (Q(a,t) \leftrightarrow T(a)))$, is true. Thus the above shortcoming of explicit definitions is avoided, because a reduction sentence does not completely define a disposition term. In fact, this is also the reason why correspondence rules supply only partial observational content, since many other reduction sentences can be used to supply other empirical aspects of the term *tearable*, for example, being torn by excessively strong shaking. Consequently, although correspondence rules were initially meant to provide explicit definitions and cognitive significance to V_T terms, these functions were abandoned and substituted by *reduction sentences* and *partial interpretation* (A detailed explication of the changes in the use of correspondence rules through the development of the RV can be found in [2.1]).

Therefore, in its most defensible version the RV could be construed to assign the following functions to correspondence rules: First, they specify empirical procedures for the application of theory to phenomena and second, as a constitutive part of TC, they supply V_T and L_T with partial interpretation. Partial interpretation in the above sense is all the RV needs since, given its goal of distinguishing theoretical from speculative metaphysical terms, it only needs a way to link the V_T terms to the V_O terms. The version of the RV that employs correspondence rules for these two purposes motivated two sorts of criticisms. The first concerns the idea that correspondence rules provide partial interpretation to V_T terms, and the second concerns the function of correspondence rules for providing theory application.

The thesis of partial interpretation came under attack from *Putnam* [2.4] and *Achinstein* [2.3, 22]. The structure of their arguments is similar. They both think that partial interpretation is unclear and they attempt to clarify the concept. They do so by suggesting plausible explications for *partial interpretation*. Then they show that for each plausible explication that each of them suggests partial interpretation is either an incoherent notion or inadequate for the needs of the RV. Thus, they both conclude that any attempt to elucidate the notion of partial interpretation is problematic and that partial interpretation of V_T terms cannot be adequately explicated. For example, Putnam gives the following plausible explications for *partial interpretation*:

1. To partially interpret V_T terms is to specify a class of intended models.
2. To partially interpret a term is to specify a verification–refutation procedure that applies only to a proper subset of the extension of the term.
3. To partially interpret a formal language L is to interpret only part of the language.

In similar spirit, Achinstein gives three other plausible explications. One of Putnam's counterexamples is that (1) above cannot meet its purpose because the class of intended models, that is, the semantic structures or interpretations that satisfy TC and which are so intended by scientists, is not well defined (A critical assessment of these arguments can be found in [2.1]).

The other function of correspondence rules, that of specifying empirical procedures for theory application to phenomena, also came under criticism. *Suppe* [2.1, pp. 102–109] argued that the account of correspondence rules inherent in the RV is inadequate for understanding actual science on the following three grounds:

1. They are mistakenly viewed as components of the theory rather than as auxiliary hypotheses.
2. The sorts of connections (e.g., explanatory causal chains) that hold between theories and phenomena are inadequately captured.
3. They oversimplify the ways in which theories are applied to phenomena.

The first argument is that the RV considers TC as postulates of the theory. Hence C is assumed to be an integral part of the theory. But, if a new experimental procedure is discovered it would have to be incorporated into C, and the result would be a new set of rules C' that subsequently leads to a new theory TC'. But obviously the theory does not undergo any change. When new experimental procedures are discovered we only improve our knowledge of how to apply theory to phenomena. So we must think of correspondence rules as auxiliary hypotheses distinct from theory.

The second argument is based upon *Schaffner*'s [2.26] observation that there is a way in which theories are applied to phenomena, which is not captured by the RV's account of correspondence rules. This is the case when various auxiliary theories (independent of T) are used to describe a *causal sequence*, which obtains between the states described by T and the observation reports. These causal sequences are descriptions of the mechanisms involved within physical systems to cause the measurement apparatus to behave as it does. Thus, they supplement theoretical explanations of the observed behavior of the apparatus by linking the theory to the observation reports via a causal story. For example, such auxiliary hypotheses are used to establish a causal link between the motion of an electron (V_T term) and the spectral line (V_O term) in a spectrometer photograph. Schaffner's point is that the relation between theory and observation reports is frequently achieved by the use of these auxiliary hypotheses that establish explanations of the behavior of physical systems via causal mechanisms. Without recognizing the use of these auxiliaries the RV may only describe a type of theory application whereby theoretical states are just correlated to observational states. If these kinds of auxiliaries were to be viewed as part of C then it is best that C is dissociated from the core theory and is regarded as a separate set of auxiliary hypotheses required for establishing the relation between theory and experiment, because such auxiliaries are obviously not theory driven, but if they are not to be considered part of C then C does not adequately explain the theory–experiment relation.

Finally, the third argument is based on *Suppes*' [2.27, 28] analysis of the complications involved in relating theoretical predictions to observation reports. Suppes observes that in order to reach the point where the two can meaningfully be compared, several epistemologically important modifications must take place on the side of the observation report. For example, Suppes claims, on the side of theory we typically have predictions derived from continuous functions, and on the side of an observation report we have a set of discrete data. The two can only be compared after the observation report is modified accordingly. Similarly, the theory's predictions may be based on the assumption that certain idealizing conditions hold, for example, no friction. Assuming that in the actual experiment these conditions did not hold, it would mean that to achieve a reasonable comparison between theory and experiment the observational data will have to be converted into a corresponding set that reflects the result of an *ideal* experiment. In other words, the actual observational data must be converted into what they would have been had the idealizing conditions obtained. According to Suppes, these sorts of conversion are obtained by employing appropriate *theories of data*. So, frequently, there will not be a direct comparison between theory and observation, but a comparison between theory and observation-altered-by-theory-of-data.

By further developing Suppes' analysis, *Suppe* [2.8] argues that because of its reliance on the observation–theory distinction, the RV employs correspondence rules in such a way as to blend together unrelated aspects of the scientific enterprise. Such aspects are the design of experiments, the interpretation of theories, the various calibration procedures, the employment of results and procedures of related branches of science, etc. All these unrelated aspects are compounded into the correspondence rules. Contrary to the implications of the RV, Suppe claims, in applying a theory to phenomena we do not have any direct link between theoretical terms and observational terms. In a scientific experiment we collect data about the phenomena, and often enough the process of collecting the data involves rather sophisticated bodies of theory. Experimental design and control, instrumentation, and reliability checks are necessary for the collection of data. Moreover, sometimes generally accepted laws or theories are also employed in collecting these data. All these features of experimentation and data collection are then employed in ways as to structure the data into forms (which Suppe calls, *hard data*) that allow meaningful comparison to theoretical predictions. In fact, theory application according to Suppe involves contrasting theoretical predictions to *hard data*, and not to something directly observed [2.8, p. 11]:

> "Accordingly, the correspondence rules for a theory should not correlate direct-observation statements with theoretical statements, but rather should correlate *hard data* with theoretical statements."

In a nutshell, although both Suppes' and Suppe's arguments do not establish with clarity how the theory–experiment relation is achieved they do make the following point: Actual scientific practice, and in particular theory–application, is far more complex than the description given by the RV's account of correspondence rules.

2.1.4 The Cosmetic Role of Models According to the RV

The objection that the RV obscures several epistemologically important features of scientific theories is implicitly present in all versions of the SV of theories. Suppe, however, brings this out explicitly in the form of a criticism (*Suppe* [2.1, 29, 30]). To clarify the sort of criticism presented by Suppe, we need to make use of some elements of the alternative picture of scientific theories given by the SV, which we shall explore in detail in Sect. 2.2.

The reasoning behind Suppe's argument is the following. Science, he claims, has managed so far to go about its business without involving the observation–theory distinction and all the complexities that it gives rise to. Since, he suggests, the distinction is not required by science, it is important to ask not only whether an analysis of scientific theories that employs the distinction is adequate or not, that is, the issue on which (as we have seen so far) many of the criticisms of the RV have focused, but whether or not the observation–theory distinction which leads to the notion of correspondence rules subsequently steers toward obscuring epistemological aspects of scientific theorizing.

The sciences, he argues, do not deal with all the detailed features of phenomena and not with phenomena in all their complexity. Rather they isolate a certain number of physical parameters by abstraction and idealization and use these parameters to characterize *physical systems* (Suppe's terminology is idiosyncratic, he uses the term *physical system* to refer to the abstract entity that an idealized model of the theory represents and not to the actual target physical system), which are highly abstract and idealized replicas of phenomena. A classical mechanical description of the earth–sun system of our solar system, would not deal with the actual system, but with a physical system in which some relevant parameters are abstracted (e.g., mass, displacement, velocity) from the complex features of the actual system. And in which some other parameters are ignored, for example, the intensity of illumination by the sun, the presence of electromagnetic fields, the presence of organic life. In addition, these abstracted parameters are not used in their full complexity to characterize the physical system. Indeed, the description would idealize the physical system by ignoring certain factors or features of the actual system that may plausibly be causally relevant to the actual system. For instance, it may assume that the planets are point masses, or that their gravitational fields are uniform, or that there are no disturbances to the system by external factors and that the system is in a vacuum. What scientific theories do is attempt to characterize the behavior of such physical systems not the behavior of directly observable phenomena.

Although this is admittedly a rough sketch of Suppe's view, it is not hard to see that the aim of the argument is to lead to the conclusion that the directly observable phenomena are connected to a scientific theory via the physical system. That is to say, (if we put together this idea with the one presented at the end of Sect. 2.1.3 above) the connection between the theory and the phenomena, according to Suppe, requires an analysis of theories and of theory–application that involves a two-stage move. The first move involves the connection between raw phenomena and the *hard data* about the particular target system in question. The second move involves the connection between the *physical system* that represents the *hard data* and the theoretical postulates of the theory. According to Suppe's understanding of the theory–experiment relation, the physical system plays the intermediate role between phenomena and theory and this role, which is operative in theory–application, is what needs to be illuminated. The RV implies that the correspondence rules "[...] amalgamate together the two sorts of moves [...] so as to eliminate the physical system" [2.29, p. 16], thus obscuring this important epistemological feature of scientific theorizing.

So, according to Suppe, correspondence rules must give way to this two-stage move, if we are to identify and elucidate the epistemic features of *physical systems*. Suppe's suggestion is that the only way to accommodate physical systems into our understanding of how theories relate to phenomena is to give models of the theory their representational status. The representational means of the RV are linguistic entities, for example, sentences. Models, within the RV, are denied any representational function. They are conceived exclusively as interpretative devices of the formal calculus, that is, as structures that satisfy subsets of sentences of the theory. This reduces models to metamathematical entities that are employed in order to make intelligible the abstract calculus, which amounts to treating them as more or less *cosmetic* aspects of science. But this understanding of the role of models leads to the incapacity of the RV to elucidate the epistemic features of physical systems, and thus obscures – what Suppe considers to be – epistemologically important features of scientific theorizing.

2.1.5 Hempel's Provisos Argument

In one of his last writings, *Hempel* [2.31] raises a problem that suggests a flaw in interpreting the link between empirical theories and experimental reports as mere deduction. Assuming that a theory is a formal axiomatic system consisting of T and C, as we did so far, consider Hempel's example. If we try to apply the theory of magnetism for a simple case we are faced with the following inferential situation. From the observational sentence *b is a metal bar to which iron filings are clinging* (S_{O1}), by means of a suitable correspondence rule we infer the theoretical sentence *b is a magnet* (S_{T1}). Then by using the theoretical postulates in T, we infer *if b is broken into two bars, then both are magnets and their poles will attract or repel each other* (S_{T2}). Finally using further correspondence rules we derive the observational sentence *if b is broken into two shorter bars and these are suspended, by long thin threads, close to each other at the same distance from the ground, they will orient themselves so as to fall into a straight line* (S_{O2}) ([2.31, p. 20]). If the inferential structure is assumed to be deductive then the above structure can be read as follows: S_{O1} in combination with the theory deductively implies S_{O2}. Hempel concludes that this deductivist construal faces a difficulty, which he calls *the problem of provisos*.

To clarify the problem of provisos, we must look into the third inferential step from S_{T2} to S_{O2}. What is necessary here is for the theory of magnetism to provide correspondence rules that would turn this step into a deductive inference. The theory however, as Hempel points out, clearly does not do this. In fact, the theory allows for the possibility that the magnets orient themselves in a way other than a straight line, for example, if an external magnetic field of suitable strength and direction is present. This leads to recognizing that the third inferential step presupposes the additional assumption that there are no disturbing influences to the system of concern. *Hempel* uses the term *provisos*, "[...] to refer to assumptions [of this kind] [...], which are essential, but generally unstated, presuppositions of theoretical inferences" [2.31, p. 23]. Therefore, provisos are presupposed in the application of a theory to phenomena (The problem we saw in Sect. 2.1.3 which Suppes raises, namely that in science theoretical predictions are not confronted with raw observation reports but with observation-altered-by-theory-of-data reports, neighbors this problem but it is not the same. Hempel's problem of provisos concerns whether it is possible to deductively link theory to observational statements no matter how the latter are constructed).

What is the character of provisos? Hempel suggests we may view provisos as *assumptions of completeness*. For example, in a theoretical inference from a sentence S_1 to another S_2, a proviso is required that asserts that in a given case "[...] no factors other than those specified in S_1 are present that could affect the event described by S_2" [2.31, p. 29]. As, for example, is the case in the application of the Newtonian theory to a two-body system, where it is presupposed that their mutual gravitational attraction are the only forces the system is subjected to. It is clear that [2.31, p. 26]:

> "[...] a proviso as here understood is not a clause that can be attached to a theory as a whole and vouchsafe its deductive potency by asserting that in all particular situations to which the theory is applied, disturbing factors are absent. Rather, a proviso has to be conceived as a clause that pertains to some particular application of a given theory and asserts that in the case at hand, no effective factors are present other than those explicitly taken into account."

Thus, if a theory is conceived as a deductively closed set of statements and its axioms conceived as empirical universal generalizations, as the RV purports, then to apply theory to phenomena, that is, to deductively link theoretical to observational statements, provisos are required. However, in many theory applications there would be an indefinitely large number of provisos, thus trivializing the concept of scientific laws understood as empirical universal generalizations. In other cases, some provisos would not even be expressible in the language of the theory, thus making the deductive step impossible. Hempel's challenge is that theory–applications presuppose provisos and this does not cohere with the view that theory relates to observation sentences deductively (For an interesting discussion of Hempel's problem of provisos, see [2.32–35]).

2.1.6 Theory Consistency and Meaning Invariance

Feyerabend criticized the logical positivist conception of scientific theories on the ground that it imposes on them a *meaning invariance condition* and a *consistency condition*. By the consistency condition he meant that [2.36, p. 164]

> "[...] only such theories are [...] admissible in a given domain which either *contain* the theories already used in this domain, or which are at least *consistent* with them inside the domain."

By the condition of meaning invariance he meant that [2.36, p. 164]:

> "[...] meanings will have to be invariant with respect to scientific progress; that is, all future theories will have to be framed in such a manner that their use in explanations [or reductions] does not affect what is said by the theories, or factual reports to be explained"

Feyerabend's criticisms are not aimed directly at the RV, but rather at two other claims of logical positivism that are intimately connected to the RV, namely the theses of *the development of theories by reduction* and *the covering law model of scientific explanation*.

A brief digression, in order to look into the aforementioned theses, would be helpful. The development of theories by reduction involves the reduction of one theory (secondary) into a second more inclusive theory (primary). In such developments, the former theory may employ [2.37, p. 342]

> "[...] in its formulations [...] a number of distinctive descriptive predicates that are not included in the basic theoretical terms or in the associated rules of correspondence of the primary [theory] [...]."

That is to say, the V_T terms of the secondary theory are not necessarily all included in the theoretical vocabulary of the primary theory. Nagel builds up his case based on the example of the reduction of thermodynamics to statistical mechanics. There are several requirements that have to be satisfied for theory reduction to take place, two of which are: (1) the V_T terms for both theories involved in the reduction must have unambiguously fixed meanings by codified rules of usage or by established procedures appropriate to each discipline, for example, theoretical postulates or correspondence rules. (2) for every V_T term in the secondary theory that is absent from the theoretical vocabulary of the primary theory, assumptions must be introduced that postulate suitable relations between these terms and corresponding theoretical terms in the primary theory. (See *Nagel* [2.37, pp. 345–358]. In fact *Nagel* presents a larger set of conditions that have to hold in order for reduction to take place [2.37, pp. 336–397], but these are the only two relevant to Feyerabend's arguments).

The covering law model of scientific explanation is, in a nutshell, explanation in terms of a deductively valid argument. The sentence to be explained (explanandum) is a logical consequence of a set of law-premises together with a set of premises consisting of initial conditions or other particular facts involved (explanans). For the special case when the explanandum is a scientific theory, T', the covering law model can be formulated as follows: A theory T explains T' if and only if T together with initial conditions constitute a deductively valid inference with consequence T'. In other words, if T' is derivable from T together with statements of particular facts involved then T' is explained by T. It seems that reduction and explanation of theories go hand in hand, that is, if T' is reduced to T, then T explains T' and conversely.

Feyerabend points out that Nagel's two assumptions – (1) and (2) above – for theory reduction respectively impose a condition of meaning invariance and a consistency condition to scientific progress. The thesis of development of theories by reduction condemns science to restrict itself to theories that are mutually consistent. But the consistency condition requires that terms in the admissible theories for a domain must be used with the same meanings. Similarly, it can be shown that the covering law model of explanation also imposes these two conditions. In fact, the consistency condition follows from the requirement that the explanandum must be a logical consequence of the explanans, and since the meanings of the terms and statements in a logically valid argument must remain constant, an obvious demand for explanation – imposed by the covering law model – is that meanings must be invariant. Feyerabend objects to the meaning invariance and the consistency conditions and argues his case inductively by drawing from historical examples of theory change. For example, the concept of mass does not have the same meaning in relativity theory as it does in classical mechanics. Relativistic mass is a relational concept between an object and its velocity, whereas in classical mechanics mass is a monadic property of an object. Similarly, Galileo's law asserts that acceleration due to gravity is constant, but if Newton's law of gravitation is applied to the surface of the earth it yields a variable acceleration due to gravity. Hence, Galileo's law cannot be derived from Newton's law. By such examples, he attempts to undermine Nagel's assumptions (1) and (2) above and establish that neither meaning invariance nor the related notion of theory consistency characterize actual science and scientific progress (see *Feyerabend* [2.36, 38–40]. Numerous authors have criticized Feyerabend's views. For instance, objections to his views have been raised based on his idiosyncratic analysis of *meaning*, on which his arguments rely. His views are hence not presented here as conclusive criticisms of the RV; but only to highlight that they cast doubt on the adequacy of the theses of theory development by reduction and the covering law model of explanation).

2.1.7 General Remark on the Received View

The RV is intended as an explicative and not a descriptive view of scientific theories. We have seen that even as such it is vulnerable to a great deal of criticism. One way or another, all these criticisms rely on one weakness of the RV: Its inability to clearly spell out the nature of theoretical terms (and how they acquire their meaning) and its inability to specify how sentences consisting of such terms relate to experimental reports. This is a weakness that has been understood by the RV's critics to stem from the former's focus on syntax. By shifting attention away from the representational function of models and attempting to characterize theory structure in syntactic terms, the RV makes itself vulnerable to such objections. Despite all of the above criticisms pointing to the difficulty in explicating how theoretical terms relate to observation, I do not think that any one of them is conclusive in the ultimate sense of rebutting the RV. Nevertheless, the subsequent result was that under the weight of all of these criticisms together the RV eventually made room for its successor.

2.2 The Semantic View of Scientific Theories

The SV has for the last few decades been the standard-bearer of the view that theories are families of models. The slogan *theories are families of models* was meant by the philosophers that originally put forward the SV to stand for the claim that it is more suitable – for understanding scientific theorizing – that the structure of theory is identified with, or presented as, classes of models. A logical consequence of identifying theory structure with classes of models is that models and modeling are turned into crucial components of scientific theorizing. Indeed, this has been one of the major contributions of the SV, since it unquestionably assisted in putting models and modeling at the forefront of philosophical attention. However, identifying theory structure with classes of models is not a logical consequence of the thesis that models (and modeling) are important components of scientific theorizing. Some philosophers who came to this conclusion have since defended the view that although models are crucial to scientific theorizing, the relation between theory and models is much more complex than that of set-theoretical inclusion. I shall proceed in this section by articulating the major features of the SV; in the process I shall try to clarify the notion of model inherent in the view and also explain – what I consider to be – the main difference among its proponents, and finally I will briefly discuss the criticisms against it, which, nevertheless, do not undermine the importance of models in science.

Patrick Suppes was the first to attempt a model-theoretic account of theory structure. He was one of the major denouncers of the attempts by the logical positivists to characterize theories as first-order calculi supplemented by a set of correspondence rules. (See [2.27, 28, 41–43]; much of the work developed in these papers is included in [2.44]). His objections to the RV led him on the one hand to suggest that in scientific practice the theory–experiment relation is more sophisticated than what is implicit in the RV and that theories are not confronted with raw experimental data (as we have seen in Sect. 2.1) but with, what has since been dubbed, *models of data*. On the other hand, he proposed that theories be construed as collections of models. The models are possible realizations (in the Tarskian sense) that satisfy sets of statements of theory, and these models, according to Suppes, are entities of the appropriate set-theoretical structure. Both of these insights have been operative in shaping the SV.

Suppes urged against standard formalizations of scientific theories. First, no substantive example of a scientific theory is worked out in a formal calculus, and second the [2.28, p. 57]

> "[. . .] very sketchiness [of standard formalizations] makes it possible to omit both important properties of theories and significant distinctions that may be introduced between different theories."

He opts for set-theoretical axiomatization as the way by which to overcome the shortcomings of standard formalization. As mentioned by *Gelfert*, Chap. 1, Suppe's example of a set-theoretical axiomatization is classical particle mechanics (CPM). Three axioms of kinematics and four axioms of dynamics (explicitly stated in Chap. 1 of this volume: *The Ontology of Models*) are articulated by the use of predicates that are defined in terms of set theoretical notions. The structure $\wp = \langle P, T, s, m, f, g \rangle$ can then be understood to be a model of CPM if and only if it satisfies those axioms [2.41, p. 294]. Such a structure is what logicians would label a (semantic) model of the theory, or more accurately a class of models. In general, the model–theoretic notion of a structure, S, is that of an entity consisting of a nonempty set of individuals, D, and a set of relations defined upon the former, R, that is, $S = \langle D, R \rangle$. The set D specifies the domain of the structure and the set R specifies the relations that hold between the individuals in D. (Note that as far as the notion of a structure is concerned, it only matters how many individuals are there and not what they are, and it only matters that the relations in R hold between such and such individuals of D and not what the relations are. For more on this point and a detailed analysis of the notion of structure *Frigg* and *Nguyen*, Chap. 3).

Models of data, according to Suppes, are possible realizations of the experimental data. It is to models of data that models of the theory are contrasted. The RV would have it that the theoretical predictions have a *direct analogue* in the observation statements. This view however, is, according to Suppes, a distorting simplification. As we have seen in Sect. 2.1.3, Suppes defends the claim that by the use of theories of experimental design and other auxiliary theories, the raw data are regimented into a structural form that bears a relation to the models of the theory. To structure the data, as we saw earlier, various influencing factors that the theory does not account for, but are known to influence the experimental data, must be accommodated by an appropriate conversion of the data into canonical form. This regimentation results in a finished product that Suppes dubbed *models of data*, which are structures that could reasonably be contrasted to the models of the theory. Suppes' picture of science as an enterprise of theory construction and empirical testing of theories involves establishing a *hierarchy of models*, roughly consisting of the general categories of models of the theory and models of the data. Furthermore, since the theory–experiment relation is construed as no more than a comparison (i. e., a mapping) of mathematical structures, he invokes the mathematical notion of *isomorphism* of structure to account for the link between theory and experiment. (An isomorphism between structures U and V exists, if there is a function that maps each element of U onto each element of V). Hence, Suppes can be read as urging the thesis that defining the models of the theory and checking for isomorphism with models of data, is a rational reconstruction that does more justice to actual science than the RV does.

The backbone of Suppes' account is the sharp distinction between models of theory and models of data. In his view, the traditional syntactic account of the relation between theory and evidence, which could be captured by the schema: $(T\&A) \rightarrow E$ (where, T stands for theory, A for auxiliaries, E for empirical evidence), is replaced by theses (1), (2), and (3) below:

1. $M_{\mathrm{T}} \subseteq TS$, where M_{T} stands for model of the theory TS for the theory structure, and $\subseteq$ for the relation of inclusion
2. $(A\&E\&D) \mapsto M_{\mathrm{D}}$, where M_{D} stands for model of data, A for auxiliary theories, E for theories of experimental design etc., D for raw empirical data, and $\mapsto$ for ... *used in the construction of* ...
3. $M_{\mathrm{T}} \approx M_{\mathrm{D}}$, where $\approx$ stands for mapping of the elements and relations of one structure onto the other.

$M_{\mathrm{T}} \subseteq TS$ expresses Suppes' view that by defining a theory structure a class of models is laid down for the representation of physical systems. $(A\&E\&D) \mapsto M_{\mathrm{D}}$ is meant to show how Suppes distances himself from past conceptions of the theory–experiment relation, by claiming that theories are not directly confronted with raw experimental data (collected from the target physical systems) but rather that the latter are used, together with much of the rest of the scientific inventory, in the construction of data structures, M_{D}. These data structures are then contrasted to a theoretical model, and the theory–experiment relation consists in an isomorphism, or more generally in a mapping of a data onto a theoretical structure, that is, $M_{\mathrm{T}} \approx M_{\mathrm{D}}$. The proponents of the SV would, I believe, concur to the above three general theses. Furthermore, they would concur with two of the theses' corollaries: that scientific representation of phenomena can be explicated exclusively by mapping of structures, and that all scientific models constructed within the framework of a particular scientific theory are united under a common mathematical or relational structure. We shall look into these two contentions of the SV toward the end of this section. For now, let me turn our attention to some putative differences between the various proponents of the SV.

Despite agreeing about focusing on the mathematical structure of theories for giving a unitary account of models, it is not hard to notice in the relevant literature that different proponents of the SV have spelled out the details of thesis (1) in different ways. This is because different proponents of the SV have chosen different mathematical entities with which to characterize theory structure. As we saw above, Suppes chooses set theoretical predicates a choice that seems to be shared by *da Costa* and *French* [2.45, 46]. *Van Fraassen* [2.47] on the other hand prefers state-spaces, and *Suppe* [2.30] uses relational systems.

Let us, by way of example, briefly look into van Fraassen's state-space approach. The objects of concern of scientific theories are physical systems. Typically, mathematical models represent physical systems that can generally be conceived as admitting of a certain set of states. *State-spaces* are the mathematical spaces the elements of which can be used to represent the states of physical systems. It is a generic notion that refers to what, for example, physicists would label as phase space in classical mechanics or Hilbert space in quantum mechanics. A simple example of a state-space would be that of an n-particle system. In CPM, the state of each particle at a given time is specified by its position $\boldsymbol{q} = (q_{\mathrm{x}}, q_{\mathrm{y}}, q_{\mathrm{z}})$ and momentum $\boldsymbol{p} = (p_{\mathrm{x}}, p_{\mathrm{y}}, p_{\mathrm{z}})$ vectors. Hence the state-space of an n-particle system would be a Euclidean $6n$-dimensional space, whose points are the $6n$-tuples of real numbers

$$\langle q_{1\mathrm{x}}, q_{1\mathrm{y}}, q_{1\mathrm{z}}, \ldots, q_{\mathrm{nx}}, q_{\mathrm{ny}}, q_{\mathrm{nz}}, \\ p_{1\mathrm{x}}, p_{1\mathrm{y}}, p_{1\mathrm{z}}, \ldots, p_{\mathrm{nx}}, p_{\mathrm{ny}}, p_{\mathrm{nz}}\rangle \,.$$

More generally, a state-space is the collection of mathematical entities such as, vectors, functions, or numbers, which is used to specify the set of possible states for a particular physical system. A model, in van Fraassen's characterization of theory structure, is a particular sequence of states of the state-space over time, that is, the state of the modeled physical system evolves over time according to the particular sequence of states admitted by the model. State-spaces unite clusters of models of a theory, and they can be used to single out the class of intended models just as set-theoretical predicates would in Suppes' approach. The presentation of a scientific theory, according to van Fraassen, consists of a description of *a class of state-space types*. As *van Fraassen* explains [2.47, p. 44]:

> "[w]henever certain parameters are left unspecified in the description of a structure, it would be more

> accurate to say [...] that we described a structure type."

The Bohr model of the atom, for example, does not refer to a single structure, but to a structure type. Once the necessary characteristics are specified, it gives rise to a structure for the hydrogen atom, a structure for the helium atom, and so forth.

The different choices of different authors on how theory structure is characterized, however, belong to the realm of personal preference and do not introduce any significant differences on the substance of thesis (1) of the SV, which is that all models of the theory are united under an all-inclusive theory structure. So, irrespective of the particular means used to characterize theory structure, the SV construes models as structures (or structure types) and theories as collections of such structures. Neither have disagreements been voiced regarding thesis (2). On the contrary, there seems to be a consensus among adherents of the SV that models of theory are confronted with models of data and not the direct result of an experimental setup (Not much work has been done to convincingly analyze particular scientific examples and to show the details of the use of *models of data* in science; rather, adherents of the SV repeatedly use the notion with reference to something very general with unclear applications in actual scientific contexts).

2.2.1 On the Notion of Model in the SV

An obvious objection to thesis (1) would be that a standard formalization could be used to express the theory and subsequently define the class of semantic models metamathematically, as the class of structures that satisfy the sentences of the theory, despite Suppes suggestion that such a procedure would be unnecessarily complex and tedious.

In fact, proponents of the SV have often encouraged this objection. *Van Fraassen* and Suppe are notable examples as the following quotations suggest [2.48, p. 326]:

> "There are natural interrelations between the two approaches [i. e., the RV and the SV]: An axiomatic theory may be characterized by the class of interpretations which satisfy it, and an interpretation may be characterized by the set of sentences which it satisfies; though in neither case is the characterization unique. These interrelations [...] would make implausible any claim of philosophical superiority for either approach. But the questions asked and methods used are different, and with respect to fruitfulness and insight they may not be on a par with specific contexts or for special purposes."

Suppe [2.30, p. 82]:

> "This suggests that theories be construed as propounded abstract structures serving as models for sets of interpreted sentences that constitute the linguistic formulations. These structures are metamathematical models of their linguistic formulations, where the same structure may be the model for a number of different, and possibly nonequivalent, sets of sentences or linguistic formulations of the theory."

From such remarks, one is justifiably led to believe that propounding a theory as a class of models directly defined, without recourse to its syntax, only aims at convenience in avoiding the hustle of constructing a standard formalization, and at easier adaptability of our reconstruction with common scientific practices. Epigrammatically, the difference – between the SV and the RV – would then be methodological and heuristic. Reasons such as this have led some authors to question the *logical* difference between defining the class of models directly as opposed to metamathematically.

Examples are *Friedman* and *Worrall* who in their separate reviews of *van Fraassen* [2.47] ask whether the class of models that constitutes the theory, according to the proponents of the SV, is to be identified with an elementary class, that is, a class that contains all the models (structures) that satisfy a first-order theory. They both notice that not only does van Fraassen and other proponents of the SV offer no reason to oppose such a supposition, but also they even encourage it (as in the above quotations). But if that is the case [2.49, p. 276]:

> "[t]hen the completeness theorem immediately yields the equivalence of van Fraassen's account and the traditional syntactic account [i. e., that of the RV]."

In other words [2.50, p. 71]:

> "So far as logic is concerned, syntax and semantics go hand-in-hand – to every consistent set of first-order sentences there corresponds a nonempty set of models, and to every normal (elementary) set of models there corresponds a consistent set of first-order sentences."

If we assume (following Friedman and Worrall) that the proponents of the SV are referring to the elementary class of models then the preceding argument is sound. The SV, in agreement with the logical positivists, retains formal methods as the primary tool for philosophical analysis of science. The only new elements of its own would be the suggestions that first it is more convenient that rather than developing these methods using proof–theory we should instead use formal semantics (model-theory), and second we should assign to models (i. e., the semantic interpretations of sets of sentences) a representational capacity.

Van Fraassen, however, resists the construal of the class of models of the SV with an elementary class (See *van Fraassen* [2.51, pp. 301–303] and his [2.52]). Let me rehearse his argument. The SV claims that to present a theory is to define a class M of models. This is the class of structures the theory makes available for modeling its domain. For most scientific theories, the real number continuum would be included in this class. Now his argument goes, if we are able to formalize what is meant to be conveyed by M in some appropriate language, then we will be left with a class N of models of the language, that is, the class of models in which the axioms and theorems of the language are satisfied. Our hope is that every structure in M occurs in N. However, the real number continuum is infinite and [2.52, p. 120]:

> "[t]here is no elementary class of models of a denumerable first-order language each of which includes the real numbers. As soon as we go from mathematics to metamathematics, we reach a level of formalization where many mathematical distinctions cannot be captured."

Furthermore, "[t]he *Löwenheim–Skolem* theorems [...] tell us [...] that N contains many structures not isomorphic to any member of M" [2.51, p. 302]. Van Fraassen relies, here, on the following reasoning: The Löwenheim–Skolem theorem tells us that all satisfiable first-order theories that admit infinite models will have models of all different infinite cardinalities. Now models of different cardinality are nonisomorphic. Consequently, every theory that makes use of the real number continuum will have models that are not isomorphic to the intended models (i. e., nonstandard interpretations) but which satisfy the axioms of the theory. So van Fraassen is suggesting that M is the intended class of models, and since the limitative meta-theorems tell us that it cannot be uniquely determined by any set of first-order sentences we can only define it directly. Here is his concluding remark [2.51, p. 302]:

> "The set N contains [...] [an] image M^* of M, namely, the set of those members of N which consist of structures in M accompanied by interpretations therein of the syntax. But, moreover, [...] M^* is not an elementary class."

Evidently, van Fraassen's argument aims to establish that the directly defined class of models is not an elementary class. It is hard, however, to see that defining the models of the theory directly without resort to formal syntax yields only the intended models of theory (i. e., excludes all nonstandard models), despite the possibility that one could see the prospect of the SV being heuristically superior to the RV. (Of course, we must not forget that this superiority would not necessarily be the result of thesis (1) of the SV, but it could be the result of its consequence of putting particular emphasis on the significance of scientific models that, as noted earlier, does not logically entail thesis (1)).

Let us, for the sake of argument, ignore the Friedman–Worrall argument. Now, according to the SV, models of theory have a dual role. On the one hand, they are devices by which phenomena are represented, and on the other, they are structures that would satisfy a formal calculus were the theory formalized. The SV requires this dual role. First because the representational role of models is the way by which the SV accounts for scientific representation without the use of language; and second because the role of interpreting a set of axioms ensures that a unitary account of models is given. Now, *Thompson-Jones* [2.53] notices that the notion of model implicit in the SV is either that of an interpretation of a set of sentences or a mathematical structure (the disjunction is of course inclusive). He analyzes the two possible notions and argues that the SV becomes more tenable if the notion of model is only understood as that of a mathematical structure that functions as a representation device. If that were the case then the adherents of the SV could possibly claim that defining the class of structures directly indeed results in something distinct from the metamathematical models of a formal syntax. Thompson-Jones' suggestion, however, would give rise to new objections. Here is one. It would give rise to the following question: How could a theory be identified with a class of models (i. e., mathematical structures united under an all-inclusive theory structure) if the members of such a class do not attain membership in the class because they are interpretations of the same set of theory axioms? In other words, the proponents of the SV would have to explain what it is that *unites* the mathematical models other than the satisfaction relation they have to the theoretical axioms. To my knowledge, proponents of the SV have not offered an answer to this question. If Thompson-Jones' suggestion did indeed offer a plausible way to overcome the Friedman–Worrall argument then the SV would have to abandon the quest of giving a unitary account of models. Given the dual aim of the SV, namely to give a unitary account of models and to account for scientific representation by means of structural relations, it seems that the legitimate notion of model integral to this view must have these two-hard to reconcile-roles; namely, to function both as an interpretation of sets of sentences and as a representation of phenomena. (Notice that this dual function of models is an aspect of all versions of the SV, independent of how one chooses to characterize theory structure and of how one chooses to interpret that structure).

2.2.2 The Difference Between Various Versions of the SV

The main difference among the various versions of the SV relates to two intertwined issues that relate to thesis (3), namely how the theory structure is construed and how the theory–experiment mapping relation is construed. To a first approximation we could divide the different versions of the SV, from the perspective of these two issues, into two sorts. Those in which particular emphasis is given to the presence of abstraction and idealization in scientific theorizing for explicating the theory–experiment (or model–experiment) relation, and those in which the significance of this nature of scientific theorizing is underrated.

Idealization and Abstraction Underrated

Van Fraassen (Suppes most probably could be placed in this group too), for example, seems to be a clear case of this sort. Here is how he encapsulates his conception of scientific theories and of how theory relates to experiment [2.47, p. 64]:

> "To present a theory is to specify a family of structures, its *models*; and secondly, to specify certain parts of those *models* (*the empirical substructures*) as candidates for the direct representation of observable phenomena. The structures which can be described in experimental and measurement reports we can call *appearances*: The theory is empirically adequate if it has some model such that all appearances are isomorphic to empirical substructures of that model."

Appearances (which is van Fraassen's term for *models of data*) are relational structures of measurements of observable aspects of the target physical system, for example, relative distances and velocities. For example, in the Newtonian description of the solar system, as *van Fraassen* points out, the relative motions of the planets "[...] form relational structures defined by measuring relative distances, time intervals, and angles of separation" [2.47, p. 45]. Within the theoretical model for this physical system, "[...] we can define structures that are meant to be exact reflections of those appearances [...]" [2.47, p. 45]. Van Fraassen calls these *empirical substructures*. When a theory structure is defined each of its models, which are candidates for the representation of phenomena, includes empirical substructures. So within representational models we could specify a division between observable/nonobservable features (albeit this division is not drawn in linguistic terms), and the empirical substructures of such models are assumed to be isomorphic to the observable aspects of the physical system. In other words, the theory structure is interpreted as having distinctly divided observable and nonobservable features, and the theory–experiment relation is interpreted as being an isomorphic relation between the data model and the observable parts of the theoretical model. Now, the state-space is a class of models, it thus includes – for CPM – many models in which the world is a Newtonian mechanical system. In fact, it seems that the state-space includes (unites) all logically possible models, as the following dictum suggests ([2.52, p. 111], [2.54, p. 226]):

> "In one such model, nothing except the solar system exists at all; in another the fixed stars also exist, and in a third, the solar system exists and dolphins are its only rational inhabitants."

According to van Fraassen, the theory is empirically adequate if we can find a model of the theory in which we can specify empirical substructures that are isomorphic to the data model. The particular view of scientific representation that resides within this idea is this: *A model represents its target if and only if it is isomorphic to a data model constructed from measurements of the target*. Not much else seems to matter for a representation relation to hold but the *isomorphism condition*. Many would argue, however, that such a condition for the representation relation is too strong to explicate how actual scientific models relate to experimental results and would object to this view on the ground that for isomorphism to occur it would require that target physical systems occur under highly idealized conditions or in isolated circumstances. (Admittedly, it would not be such a strong requirement for models that would only describe observable aspects of the world. In such cases isomorphism could be achieved, but at the expense of the model's epistemic significance. I do not think, for instance, that such models would be of much value to a science like Physics as, more often than not, they would be useless in predicting the future behavior of their targets).

Idealization and Abstraction Highlighted

In the second camp of the SV, we encounter several varieties. One of these is *Suppe* [2.30], who interprets theory structure and the theory–experiment relation as follows. Theories characterize particular classes of target systems. However, target systems are not characterized in their full complexity, as already mentioned in Sect. 2.1.4. Instead, Suppe's understanding is that certain parameters are abstracted and employed in this characterization. In the case of CPM, these are the position and momentum vectors. These two parameters are abstracted from all other characteristics that target systems may possess. Furthermore, once the factors, which are assumed to influence the class of target systems in the theory's intended scope, have been abstracted the characterization of physical systems (as mentioned in Sect.2.1.4, physical systems in Suppe's terminology refer to the abstract entities that models of the theory represent and not to the actual target systems) still does not *fully* account for target systems. Physical systems are not concerned with the actual values of the parameters the particulars possess, for example, actual velocities, but with the values of these parameters under certain conditions that obtain only within the physical system itself. Thus in CPM, where the behavior of dimensionless point-masses are studied in isolation from outside interactions, physical systems characterize this behavior only by reference to the positions and momenta of the point-masses at given times.

An example can serve to demonstrate Suppe's idea in bit more detail. The linear harmonic oscillator, that is, a *mathematical instrument*, is expressed by the following equation of motion $\ddot{x}+(k/m)x=0$, which is the result of applying Newton's second law to a linear restoring force. The mathematical model is interpreted (and thus characterizes a *physical system*) as follows: Periodic oscillations are assumed to take place with respect to time, x is the displacement of an oscillating mass-point, and k and m are constant coefficients that may be replaced by others. When the mathematical parameters in the above equation are linked to features of a specific object, the equation can be used to model for instance the torsion pendulum, that is, an elastic rod connected to a disk that oscillates about an equilibrium position. This sort of linking of mathematical terms to features of objects could be understood to be a manifestation of what Giere calls identification. Giere introduces a useful distinction between *interpretation* and *identification* [2.55, p. 75]:

> "[...] [Interpretation] is the linking of the mathematical symbols with *general terms*, or concepts, such as *position*[...] [Identification] is the linking of a mathematical symbol with some feature of a *specific object*, such as *the position of the moon*."

In the torsion pendulum model, x is identified with the angle of twist, k with the torsion constant, and m with the moment of inertia. By linking the mathematical symbols of a model to features of a target system we can reasonably assume, according to Suppe, that the model could be associated with an actual system of the world; the model characterizes, as Suppe would say in his own jargon, "a causally possible physical system."

However, even when a certain mathematical product of theory is identified with a causally possible physical system, we still know that typically the situation described by the physical system does not obtain. The actual torsion pendulum apparatus is subject to a number of different factors (or may have a number of different characteristics) that may or may not influence the process of oscillation. Some influencing factors are the amplitude of the angle of oscillation, the mass distribution of the rod and disc, the nonuniformity of the gravitational field of the earth, the buoyancy of the rod and disc, the resistance of the air and the stirring up of the air due to the oscillations. In modeling the torsion pendulum by means of the linear harmonic oscillator the physical system is abstracted from factors assumed to influence the oscillations in the same manner as from those assumed not to. Therefore, the replicating relation between the physical system, P, and the target system, S, which Suppe urges cannot be understood as one of identity or isomorphism. *Suppe* is explicit about this [2.30, p. 94]:

> "The attributes in P determine a sequence of states over time and thus indicate a possible behavior of S [...] Accordingly, P is a kind of *replica* of S; however, it need not replicate S in any straight-forward manner. For the state of P at t does not indicate what attributes the particulars in S possess at t; rather, it indicates what attributes they *would have* at t *were* the abstracted parameters the only ones influencing the behavior of S and were certain idealized conditions met. In order to see how P replicates S we need to investigate these abstractive and idealizing conditions holding between them."

In summary, the replicating relation is counterfactual: If the conditions assumed to hold for the description of the physical system were to hold for the target system, then the target system would behave in the way described by the physical system. The behavior of actual target systems, however, may be subject to other unselected parameters or other conditions, for which the theory does not account.

The divergence of Suppe's view from that of van Fraassen is one based primarily on the representation relation of theory to phenomena. Suppe understands the theory structure as being a highly abstract and idealized representation of the complexities of the real world. Van Fraassen disregards this because he is concerned with the observable aspects of theories and assumes that these can, to a high degree of accuracy, be captured by experiments. Thus van Fraassen regards theories as containing empirical substructures that stand in isomorphic relations to the observable aspects of the world. Suppe's understanding of theory structure, however, points to a significant drawback present in van Fraassen's view: How can isomorphism obtain between a data model and an empirical substructure of the model, given that the model is abstract and idealized? Suppe's difference with van Fraassen's view of the representation relation and of the epistemic inferences that can be drawn from it is this, if indeed it is the case that isomorphism obtains between a data model and an empirical substructure, then it is so for either of two reasons: (1) the experiment is highly idealized, or (2) the data model is converted to what the measurements *would have been* if the influences that are not accounted by the theory did not have any effect on the experimental setup. This is a significantly different claim from what van Fraassen would urge, to wit that the world or some part of it is isomorphic to the model. According to Suppe's understanding of theory structure, no part of the world is or can be isomorphic to a model of the theory, because abstraction and idealization are involved in scientific theorizing.

Geire [2.55] is another example of a version of the SV that places the emphasis on abstraction and idealization. Following Suppes and van Fraassen, Giere understands theories as classes of models. He does not have any special preference about the mathematical entities by which theory structure is characterized, but he is interested in looking at the characteristics of actual science and how these could be captured by the SV. This leads him to a similar claim as Suppe. He claims that although he does not see any logical reason why a real target system could not be isomorphic to a model, nevertheless for the examples of models found in mechanics texts, typically, no claim of isomorphism is made, indeed "[...] the texts often explicitly note respects in which the model fails to be isomorphic to the real system" [2.55, p. 80]. He attributes this to the abstract and idealized nature of models of the theory. His solution is to substitute the strict criterion of isomorphism, as a way by which to explicate the theory–experiment relation, with that of similarity in relevant respects and degrees between the model and its target.

Finally, there is another example of a version of the SV that also gives attention to idealization and abstraction, namely the version advocated by *da Costa* and *French* in [2.45, 46, 56]. They do this indirectly by interpreting theories as partial structures, that is, structures consisting of a domain of individuals and a set of partial relations defined on the domain, where a partial relation is one that is not defined for all the n-tuples of individuals of the domain for which it presumably holds. If models of theory are interpreted in this manner and if it is assumed that models of data are also partial structures, then the theory–experiment relation is explicated by *da Costa* and *French* [2.46] as a partial isomorphism. A partial isomorphism between two partial structures U and V exists when a partial substructure of U is isomorphic to a partial substructure of V. In other words, partial isomorphism exists when some elements of the set of relations in U are mapped onto elements of the set of relations in V. If a model of theory is partially isomorphic to a data model then, da Costa and French claim, the model is partially true. The notion of partial truth is meant to convey a pragmatic notion of truth, which plausibly could avoid the problems of correspondence or complete truth, and capture the commonplace idea that theories (or models) are incomplete or imperfect or abstract or idealized descriptions of target systems.

In conclusion, if we could speak of *different* versions of the SV and not just different formulations of the same idea, if, in other words, the proposed versions of the semantic conception of theories can be differentiated in any significant way amongst them, it is on the basis of how thesis (3) is conceived: There are those that understand the representation relation, $M_{\mathrm{T}} \approx M_{\mathrm{D}}$, as a strict isomorphic relation, and those that construe it more liberally, for example, as a similarity relation. In particular, van Fraassen prefers an isomorphic relation between theory and experiment, whereas Suppe and others understand theories as being abstract and idealized representations of phenomena. It would seem therefore that particular criticisms would not necessarily target both versions. This has not been the case however, as we shall examine in the next two subsections. Critics of the SV have either targeted theses (1) and (2) and the unitary account of models implicit in the SV, or thesis (3) and the representation relation however the latter is conceived. The arguments against the unitary account of scientific models, which obviously aim indiscriminately at all versions of the SV, will be explored in Sect.2.2.4. The arguments against the nature of the representation relation implied by the SV, which shall be explored in Sect.2.2.3, if properly adapted affect both versions of the SV.

2.2.3 Scientific Representation Does not Reduce to a Mapping of Structures

Suarez [2.57] presents five arguments against the idea that scientific representation can be explicated by appealing to a structural relation (like isomorphism or similarity) that may hold between the representational device and the represented target. (*Suarez* [2.57] also develops his arguments for other suggested interpretations of theses (3), such as partial isomorphism). These arguments, which are summarized below, imply that the representational capacity of scientific models cannot derive from having a structural relation with its target. Suarez's first argument is that in science many disparate things act as representational devices, for example, a mathematical equation, or a Feynman diagram, or an architect's model of a building, or the double helix macro-model of the DNA molecule. Neither isomorphism nor similarity can be applied to such disparate representational devices in order to explicate their representational function. A similar point is also made by *Downes* [2.58], who by also exploring some examples of scientific models, argues that models in science relate to their target systems in various ways, and that attempts to explicate this relation by appeal to isomorphism or similarity does little to serve the purpose of understanding the theory–experiment relation.

The second argument concerns the logical properties of representation vis-a-vis those of isomorphism and similarity. Suarez explains that representation is nonsymmetric, nonreflexive and nontransitive. If scientific representation is a type of representation then any attempt to explicate scientific representation cannot imply different logical features from representation. But appeal to a structural relation does not accomplish this, because "[...] similarity is reflexive and symmetric, and isomorphism is reflexive, symmetric and transitive" [2.55, p. 233].

His third argument is that any explication of representation must allow for misrepresentation or inaccurate representation. Misrepresentation, he explains, occurs either when the target of a representation is mistaken or when a representation is inaccurate because it is either incomplete or idealized. Neither isomorphism nor similarity allows for the first kind of misrepresentation and isomorphism does not allow for the second kind. Although, similarity does account for the second kind of representation, Suarez argues, it does so in a restrictive sense. That is, if we assume that an incomplete representation is given according to theory X then similarity does account for misrepresentation. However, if a complete representation were given according to theory X (i. e., if we have similarity in all relevant respects that X dictates) but the predictions of this representation still diverge from measurements of the values of the target's attributes then similarity does not account for this kind of misrepresentation.

The fourth argument is that neither isomorphism nor similarity is necessary for representation. Our intuitions about the notion of representation allow us to accept the representational device derived from theory X as a *representation* of its target, even though we may know that isomorphism or similarity does not obtain because, for example, an alternative theory Y not only gives us better predictions about the target but also tells us why X fails to produce representational devices that are isomorphic or similar to their targets. A different argument but with the same conclusion is given by *Portides* [2.59], who argues that isomorphism, or other forms of structural mapping, is not necessary for representation because it is possible to explicate the representational function of some successful quantum mechanical models, which are not isomorphic to their targets. Suarez's final argument is that neither isomorphism nor similarity is sufficient for representation. In other words, even though there may not be a representation relation between A and B, A and B may, however, be isomorphic or similar.

Aiming at the same feature of the SV as Suarez, *Frigg* [2.60] reiterates some of the arguments above and gives further reasons to fortify them, but he also presents two more arguments that undermine the notion of representation as dictated by thesis (3) of the SV. Employed in his first argument is a particular notion of abstractness of concepts advocated by *Cartwright* [2.61]. A concept is considered abstract in relation to a set of more concrete concepts if for the former to apply it is necessary that one of its concrete instances apply. One of Frigg's intuitive examples is that the concept of traveling is more abstract than the concept of sitting in a moving train. So according to this sense of *abstractness* the concept of traveling applies whenever one is sitting in a moving train and that the abstract concept does not apply if one is not performing some action that belongs to the set of concrete instances of traveling. *Frigg* then claims, "[...] that possessing a structure is abstract in exactly this sense and it therefore does not apply without some more concrete concepts applying as well" [2.60, p. 55]. He defends this claim with the following argument. Since to have a structure means to consist of a set of individuals which enter into some relations, then it follows that whenever the concept of possessing a structure applies to S the concept of being an individual applies to members of a set of S and the concept of being in a relation applies to some parts of that set. The concepts of being an individual and being in a relation are abstract in the above sense. For example, given the proper context, for *being an individual* to apply, *occupying a certain space-time region* has to apply. Similarly, given the proper context, for *being in a relation* to apply it must be the case that *being greater than* applies. Therefore, both being an individual and being in a relation are abstract. Thus Frigg concludes, *possessing a structure* is abstract; hence for it to apply, it must be the case that a concrete description of the target applies. Because, the claim that the representation relation can be construed as an isomorphism (or similarity) of structures presupposes that the target possesses a structure, Frigg concludes that such a claim "[...] presupposes that there is a more concrete description that is true of the [target] system" [2.60, p. 56]. This argument shows that to reduce the representation relation to a mapping of structures the proponents of the SV need to invoke nonstructural elements into their account of representation, so pure and simple reduction fails.

Frigg's second argument, as he states, is inductive. He examines several examples of systems from different contexts in order to support the claim that a target system does not have a unique structure. For a system to have a structure it must be made of individuals and relations, but slicing up the physical systems of the world into individuals and relations is dependent on how we conceptualize the world. The world itself does not provide us with a unique slicing. "Because different conceptualizations may result in different structures there is no such thing as the one and only structure of a system" [2.60, p. 57]. One way that Frigg's argument could be read is this: Thesis (2) of the SV implies that the measurements of an experiment are structured to form a data model. But, according to Frigg, this structuring is not unique. So the claim of thesis (3), that there is, for example, an isomorphism between a theoretical model and a data model is not epistemically informative since there may be numerous other structures that could be constructed from the data that are not isomorphic to the theoretical model.

2.2.4 A Unitary Account of Models Does not Illuminate Scientific Modeling Practices

The second group of criticisms against the SV consists of several heterogeneous arguments stemming from different directions and treating a variety of features and functions of models. Despite this heterogeneity, they can be grouped together because they all indirectly undermine the idea that the unitary account of scientific models given by employing a set theoretical (or other mathematical) characterization of theory structure is adequate for understanding the notion of representational model and the model–experiment relation. This challenge to the SV is indirect because the main purpose of these arguments is to illuminate particular features of actual scientific models. In highlighting these features, these arguments illustrate that actual representational models in science are constructed in ways that are incompatible with the SV, they function in ways that the SV does not adequately account for and they represent in ways that is incompatible with the SV's account of representation; furthermore, they indicate that models in science are complex entities that cannot be thoroughly understood by unitary accounts such as set-theoretical inclusion. In other words, a consequence of most of these arguments is that the unitary account of models that the SV provides through thesis (1) that all models are constitutive parts of theory structure, obscures the particular features that representational scientific models demonstrate.

One such example is *Morrison* [2.62], who argues that models are partially autonomous from the theories that may be responsible for instigating their construction. This partial autonomy is something that may derive from the way they function but also from the way they are constructed. She discusses Prandtl's hydrodynamic model of the boundary layer in order to mark out that the inability of theory to provide an explanation of the phenomenon of fluid flow did not hinder scientific modeling. Prandtl constructed the model with little reliance on high-level theory and with a conceptual apparatus that was partially independent from the conceptual resources of theory. This partial independence in construction, according to Morrison, gives rise to functional independence and renders the model partially autonomous from theory. Furthermore, *Morrison* raises another issue (see [2.62], as well as [2.63]); that theories, and hence theoretical models as direct conceptual descendants of theory, are highly abstract and idealized descriptions of phenomena, and therefore they represent only the general features of phenomena and do not explain the specific mechanisms at work in physical systems. In contrast, actual representational scientific models – that she construes as partially autonomous mediators between theories and phenomena – are constructed in ways that allow them to function as explanations of the specific mechanisms and thus function as sources of knowledge about corresponding target systems and their constitutive parts. (As she makes clear in *Morrison* [2.64], to regard a model as partially independent from theory does not mean that theory plays an unimportant role in its construction). This argument, in which representational capacity is correlated to the explanatory power of models, is meant to achieve two goals. Firstly, to offer a way by which to go beyond the narrow understanding of scientific representation as a mapping relation of structure, and second, to offer a general way to understand the representational function of both kinds of models that physicists call theory-driven and phenomenological (In *Portides* [2.65] a more detailed contrast between Morrison's view of the representation relation and that of the SV is offered). *Cartwright* et al. [2.66] and *Portides* [2.67] have also argued that by focusing exclusively on theory-driven models and the mapping relation criterion, the SV obscures the representational function of phenomenological models and also many aspects of scientific theorizing that are the result of phenomenological methods.

It is noteworthy that the unitary account that the SV offers may be applicable to theory-driven models. Whether that is helpful or not is debatable. However, more often than not representation in science is achieved by the use of phenomenological models or phenomenological elements incorporated into theory-driven models. One aspect of Morrison's argument is that if we are not to dismiss the representational capacity of such models we should give up unitary accounts of models. Cartwright makes a similar point but her approach to the same problem is from another angle.

Cartwright [2.61, 68] claims that theories are highly abstract and thus do not and cannot represent what happens in actual situations. Cartwright's observation seems similar to versions of the SV such as Suppe's, however her approach is much more robust. To claim that theories represent what happens in actual situations, she argues, is to overlook that the concepts used in them – such as, *force functions* and *Hamiltonians* – are abstract. Such abstract concepts could only apply to the phenomena whenever more concrete descriptions (as those present in models) can stand-in for them and for this to happen the bridge principles of theory must mediate. Hence the abstract terms of theory apply to actual situations via bridge principles, and this makes bridge principles an operative aspect of theory-application to phenomena. It is only when bridge principles sanction the use of theoretical models that we are led to the construction of a model – with a relatively close relation to theory – that represents the target system. But Cartwright observes that there are only a small number of such theoretical models that can be used successfully to construct representations of physical systems and this is because there are only a handful of theory bridge principles. In most other cases, where no bridge principles exist that enable the use of a theoretical model, concrete descriptions of phenomena are achieved by constructing phenomenological models. Phenomenological models are constructed with minimal aid from theory, and surely there is no deductive (or structural) relation between them and theory. The relation between the two should be sought in the nature of the abstract–concrete distinction between scientific concepts, according to Cartwright. Models in science, whether constructed phenomenologically or by the use of available bridge principles, encompass descriptions that are in some way independent from theory because they are made up of more concrete conceptual ingredients. A weak reading of this argument is that the SV could be a plausible suggestion for understanding the structure of scientific theories for use in foundational work. But in the context of utilizing the theory to construct representations of phenomena, focusing on the structure of theory does not illuminate much because it is not sufficient as to account for the abstract–concrete distinction that exists between theory and models. A stronger reading of the argument is that the structure of theories is completely irrelevant to how theories represent the world, because they just do not represent it at all. Only models represent pieces of the world and they are partially independent from theory because they are constituted by concrete concepts that apply only to particular physical systems.

Other essays in the volume by *Morgan* and *Morrison* [2.69] discuss different aspects of partial independence of models from theory. Here are two brief examples that aim to show the partial independence of model construction from theory. *Suarez* [2.70] explains how simplifications and approximations that are introduced into representational models (such as the London brothers model of superconductivity) are decided independently of theory and of theoretical requirements. This process gives rise to a model that mediates in the sense that the model itself is the means by which corrections are established that may be incorporated into theory in order to facilitate its applications. But even in cases of models that are strongly linked to theory such as the MIT-bag model of quark confinement, *Hartmann* [2.71] argues, many parts of the model are not motivated by theory but by an accompanying *story* about quarks. From the empirical fact that quarks were not observed physicists were eventually led to the hypothesis that quarks are confined. But confinement is not something that follows from theory. Nevertheless, via the proper amalgam of theory and *story* about quarks the MIT-bag model was constructed to account for quark confinement.

I mentioned earlier in Sect. 2.2.2 that *Giere* [2.55] is also an advocate of the SV. However, his later writings [2.72, 73] suggest that he makes a gradual shift from his earlier conception of representational models in science to a view that neighbors that of Morrison and Cartwright. Even in *Giere* [2.55] the reader notices that he, unlike most other advocates of the SV, is less concerned with the attempt to give a unitary account of models and more concerned with the importance of models in actual scientific practices. But in [2.72] and [2.73] this becomes more explicit. *Giere* [2.55] espouses the idea that the laws of a theory are definitional devices of theoretical models. This view is compatible with the use of scientific laws in the SV. However, in *Giere* [2.72, p. 94] he suggests that scientific laws "[. . .] should be understood as rules devised by humans to be used in building models to represent specific aspects of the natural world." It is patent that operating as rules for building models is quite a different thing from understanding laws to be the means by which models are defined. The latter view is in line with the three theses of the SV; the former however is only in line with the view that models are important in scientific theorizing. Moreover, in *Giere* [2.73] he makes a more radical step in distinguishing between the abstract models (which he calls *abstract objects*) defined by the laws and those models used by scientists to represent physical systems (which he calls *representational models*). The latter [2.73, p. 63]

> "[...] are designed for use in representing aspects of the world. The abstract objects defined by scientific principles [i. e., scientific laws] are, on my view, not intended directly to represent the world."

Giere points to the important difference between the SV and its critics. The SV considers the models that the theory directly delivers representations of target systems of the world. Its critics do not think that; they argue that many successful representational models are constructed by a variety of conceptual ingredients and thus have a degree of autonomy from theory. But if each representational model is partially autonomous from the theory that prompted its construction then a unitary account of representational models does not seem to be much enlightening in enhancing our understanding of why models are so important in scientific theorizing.

2.2.5 General Remark on the Semantic View

Just like its predecessor the SV employs formal methods for the philosophical analysis of scientific theories. In the SV, models of the theory are directly defined by the laws of the theory, and are thus united under a common mathematical structure. Of course, mathematical equations satisfy a structure, no one disputes that mathematically formulated theories can be presented in terms of mathematical structures. Nonetheless, keen to overcome the philosophical problems associated with the RV and its focus on the syntactic elements of theories, the proponents of the SV take the idea of presenting theories structurally one step further. They claim that the SV not only offers a canonical structural formulation for theories, into which any theory can be given an equivalent reformulation (an idea that, no doubt, is useful for the philosophy of mathematics), but they also contend that a scientific theory represents phenomena *because* this structure can be linked to empirical data. To defend this assertion, the proponents of the SV assume that in science there is a sharp distinction between models of theory and models of data and argue that scientific representation is no more than a mapping relation between these two kinds of structures. As we have seen, serious arguments against the idea that representation can be reduced to structural mapping have surfaced; and these arguments counter the SV independently of how the details of the mapping relation is construed.

Furthermore, the SV implies that by defining a theory structure an indefinite number of models that are thought to be antecedently available for modeling the theory's domain are laid down. Neither this position has gone unnoticed. Critics of the SV claim that this idea does not do justice to actual science because it undervalues the complexities involved in actual scientific model construction and the variety of functions that models have in science, but more importantly because it obscures the features of representational models that distinguish them from the models that are direct descendants of theory.

I claimed that the SV employs a notion of model that has two functions – interpretation and representation. In addition, it requires models that have this dual role to be united under a common structure. It is hard to reconcile these two ideas and do justice to actual science. The devices by which the theoretical models are defined, according to the SV, are the laws of the theory. Hence the laws of the theory provide the constraints that determine the structure of these models. Now, it is not hard to see that models viewed as interpretations are indeed united under a common structure determined by the laws of the theory. What is problematic, however, is that the SV assumes that models that are interpretations also function as representations and this means that models functioning as representations can be united under a common structure. The truth value of the conjunction *models are interpretations and representations* is certainly not a trivial issue. When scientists construct representational models, they continuously impose constraints that alter their initial structure. The departure of the resulting constructs from the initial structure is such that it is no longer easily justified to think of them all as united under a common theory structure. Indeed, in many scientific cases this departure of individual representational models is such that they end up having features that may be incompatible with other models that are also instigated by the same theory. These observations lead to the thought that the model-theory and the model–experiment relations may in the end be too complex for our formal tools to capture.

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