What's wrong with the semantic conception of scientific theories

Towards a pragmatic view

Abstract

In contrast to the syntactic conception of scientific theories, the semantic conception posits that theories are not statements about the world, but families of models. Recent debates have tended to blur the differences between these two views. Practice-oriented philosophers of science have also challenged both views, arguing that models are central to science, but autonomous from theories. However, they have not proposed any alternative. This article aims to sharpen the challenges faced by the semantic view, with the goal of paving the way for a truly pragmatic conception of theories. I examine various ways in which a focus on models could make the semantic view distinct from the syntactic view. Ultimately, both views share the same resources for individuating theories, account equally well for different levels of abstraction, and face similar issues regarding the relation between formal representation and informal experience. I contend that the only relevant contrast lies not between linguistic and non-linguistic representation, but between descriptive and schematic representation, which ultimately favours a pragmatic view by implying a role for context in theoretical interpretation. I outline an account of schematic representation that could serve as a foundation for a pragmatic conception of theories.

1 Introduction

Theories, such as quantum mechanics, classical mechanics, relativity theory or the theory of evolution, are central to science. They figure in most of its activities, and some might even regard them as the primary product of science. An important task for the philosophy of science is to help us understand what theories are and how to interpret them, that is, to explicate their relations to the world, our experiences, users and other representations, including models and other theories. This task can take the form of providing a *conception of scientific theories*: an analytic framework from which analyses of theoretical equivalence, reduction, explanation, confirmation, and so on can be carried out.

The current dominant such framework is usually referred to as the *semantic view*. It emerged largely in opposition to the so-called *syntactic view*, which holds that a theory is a linguistic entity, namely a set of general statements about the world. On this approach, theoretical interpretation was considered a matter of clarifying the meaning of these statements through linguistic analysis, by making explicit their logical structure and their semantic relation to the world. By contrast, according to the semantic view, a theory is best characterised as a family of structures, its models, and the proper means of interpreting a theory lies not in linguistic analysis, but in the mathematical analysis of its structure.

Semanticists have argued that their approach offers a better understanding of science by bringing into focus the central role played by modelling activities. According to them, the syntactic view, with its focus on language, is too idealistic and disconnected from actual scientific practice. It is not entirely clear, however, what the distinction between linguistic and structural interpretation amounts to, and recent debates have tended to blur the differences between the two views. Practice-oriented philosophers of science have also challenged both the syntactic and semantic views on the ground that models, although centrally involved in theory application, are much more autonomous from theories than the semantic view assumes.

This could give us hope that a pragmatic conception of theories could supersede the semantic view and provide a better understanding of scientific theories. Unfortunately, pragmatic philosophers do not seem very interested in fleshing out this alternative. They typically put much focus on modelling activities while downplaying the role of theories, and they often express their criticism of the semantic view only in passing. I believe this is unfortunate. My aim in this article is to sharpen the pragmatist challenge against the semantic view, and to pave the way for a truly pragmatic conception of theories.

The pragmatist challenge, as I understand it, lies in the fact that the semantic view cannot differentiate itself from the syntactic view without granting autonomy to models in a self-undermining way. In the following sections, I examine various ways in which the semantic view could differ from the syntactic view: in terms of theory individuation (section 2), by postulating two stages in theoretical representation instead of one (section 3), or by claiming that theoretical representation is non-linguistic all the way through (section 4). None of these options really works: either the two views do not differ in the respect considered, or the semantic view is inconsistent or implausible. Then I examine one last option: the idea that theoretical representation is schematic rather than descriptive (section 5). I argue that this idea captures an important aspect of science that is not accounted for by the syntactic view, but that it ultimately favours a pragmatic conception of scientific theories rather than a semantic one. Finally I take stock and outline prospects for such a pragmatic view (section 6).

2 The syntactic view and the semantic view

According to the syntactic conception of scientific theories, which was the orthodox view until the 1960s, a theory is a set of general statements about the world, and theoretical interpretation is a matter of clarifying their content, which involves first expressing the theory in the form of an axiomatic system in a formal language, so as to explicate its logical structure, and then analysing the meaning of its vocabulary by providing correspondence rules between theoretical terms and an external (typically observational) vocabulary that is already interpreted. This is the programme championed by logical empiricism (Carnap 1956; Hempel 1958), which was foundational for philosophy of science. Within this framework, one can analyse theoretical explanations, for instance, as arguments that use theoretical statements and descriptions of relevant circumstances to conclude what needs to be explained (Hempel and Oppenheim 1948), theoretical reductions as explanations of one theory from another one with the help of bridge laws relating their respective vocabularies (Nagel 1961), and so on.

By contrast, the semantic view holds that scientific theories are best characterised as families of structures, their models. This implies that the statements found in science textbooks do not aim to describe the world directly; they describe models, and these models are not themselves linguistic descriptions of real objects, but rather mathematical structures that can be compared to them. The relation between the formulation of the theory and its models is generally analysed, following Patrick Suppes (1960), in terms of Tarskian model-theory: the models of a theory are the structures that satisfy (or make true) the statements given in its formulations. As Frederick Suppe (1989) explains:

> The Semantic Conception [...] construes theories as what their formulations refer to when the formulations are given a (formal) semantic interpretation. (p.4)

On the surface, this conception of scientific theories as families of models appears to be at odds with the common-sense assumption that theories are truth-bearer (not truth-makers): they can be true or false, believed or denied, accepted or rejected, whereas a collection of structures simply is. Note, however, that theoretical models can represent real systems, and so, a scientific theory can also be qualified by one or several statements specifying how they do so, and whether they do so accurately. Ronald Giere (1988, 85), in particular, mentions "various hypotheses linking those models with systems in the real world", and Suppe (1989, 4), "a theoretical hypothesis claiming that real-world phenomena [...] stand in some mapping relationship to the theory structure". These links can be cashed out in terms of similarity (Giere 1988), isomorphism (van Fraassen 1989, 226) or counterfactual correspondence (Suppe 1989).

This means that on this view, a scientific theory is essentially a claim that a certain relation holds between the models of a given family and the world. It follows that the proper means of interpreting a theory is not through axiomatisation and linguistic analysis, but through structural comparison and mathematical analysis. For example, we can understand empirical confrontation in terms of a comparison between theoretical models and data models, with the involvement of models of experiment.

This proposal is interesting, but also puzzling in some respect. We need a language to describe structures and compare them, and on a surface level, abstracting away from interpretive issues, there is little difference between presenting statements, which is what presenting a theory amounts to according to the syntactic view, and presenting a collection of structures by means of these statements, which are the structures that satisfy them, as per the semantic view. One might suspect that both parties would use the same statements for this purpose and recognise them as presentations of the same theories. This would be the case if both views individuate theories with the right level of granularity: they never present in the same way theories that are actually different, or, conversely, see a difference between two presentations of the same theory. If this is so, then the contrast between the two views must lie somewhere else: in the way theories (the statements or their models) are supposed to be related to the world, and it remains to be explained what the difference amounts to exactly.

However, this suspicion that both views correctly individuate theories was not shared by most participants in the debates between syntacticists and semanticists. Many of these debates can actually be framed as arguments to the effect that the conception of theories of the other camp is either too fine-grained, too coarse-grained, or both at the same time in different respects. Some of these criticisms have played an important role in motivating or challenging the semantic view. Let us review them.

Is the syntactic view too fine-grained? An important motivation for the semantic view stems from the idea that the syntactic view is too fine-grained: because of its focus on language, it sees differences that are merely linguistic, but not scientifically significant. According to Bas van Fraassen (1989, 222) in particular, it was a "crucial mistake [...] to confuse a theory with the formulation of a theory in a particular language". This criticism is also expressed by Suppe (1989):

> To say that something is a linguistic entity is to imply that changes in its linguistic features, including the formulation of its axiom system, produce a new entity. Thus on the Received [syntactic] View, change in the formulation of a theory is a change in theory. However, scientific theories have different individuation properties, and a given theory admits of a variety of different full and partial formulations. (p. 3-4)

The problem with this criticism is that it arguably rests on a misunderstanding of the syntactic view. According to Carl Hempel (cited in (Lutz 2012, sec. 5.2)), "[T]he standard construal [...] was intended [...] as a schematic explication that would clearly exhibit certain logical and epistemological characteristics of scientific theories". So, the point of axiomatisation is not to provide identity conditions for theories, but to exhibit their logico-mathematical structure. Given this, there is no reason to assume that any two different formulations would be identified with two different theories by syntacticists. It is more likely that they would consider them to correspond to the same theory if they satisfy some syntactic criteria of equivalence, such as logical equivalence, definitional equivalence or mutual interpretability (see Lutz 2015, sec. 3).

In this regard, it is noteworthy that logical empiricists typically subscribed to empirical criteria of meaning. One would naturally assume, from their perspective, that empirically equivalent axiomatic systems express the same theory, that they share the same content, and are therefore equivalent. However, empirical equivalence is not fine-grained at all compared to other criteria of theoretical equivalence (Weatherall 2019). In fact, logical empiricists could be accused of not being fine-grained enough in their individuation of theories. But of course, one is free to adopt one or the other criteria of equivalence to adjust their view. This is not a criticism of the syntactic view per se, but rather a criticism of a certain theory of meaning and associated criteria of equivalence. It is up to the semanticist to show that no acceptable criteria can be framed in a syntactic view. However, it seems that there is no way to do so if we accept that these criteria can be analysed either syntactically or semantically, from a meta-language, in terms of having the same models or being compatible with the same possible worlds, for instance (which is how Rudolf Carnap (1947) analysed intensions and meaning). Logical empiricists did not refrain from carrying out this kind of analysis, and they amount to specifying a certain collection of structures, as per the semantic view (see Leitgeb and Carus 2025, supp. E sec. 5 for related comments).

In light of this, there is no reason to think that the focus of the syntactic view on linguistic formulations makes it too fine-grained or inadequate.

Another way in which the syntactic view can be considered too fine-grained is in the way it focuses on theoretical terms and interprets them through correspondence rules relating them to an external vocabulary. These rules are required, under the syntactic view, for a theory to have an empirical interpretation at all; otherwise a theory would be a pure formal entity. However, correspondence rules lump together heterogeneous aspects, such as causal assumptions, theories of instruments and experimental designs and techniques. Moreover, these aspects generally evolve without anyone believing that the theory has changed. As Suppe (1989) says:

> [S]ince the correspondence rules, inter alia, specify experimental methods and measurement procedures, on the Received View any improvements or breakthroughs in experimental design or measurement result in the replacement of the existing theory by an improved replacement theory. In actual scientific practice, however, theories are not individuated that way. Improvements in experimental procedures, new measurement techniques, and the like, do not automatically alter the theory they are used with. (p. 4-5)

The question here is how much of the relation between a theory and experience should count as part of its individuating features. Claiming that the syntactic view, as expounded by logical empiricists, is too fine-grained in this respect looks like a fair criticism. The problem is, again, that it does not concern the idea that theories are linguistic entities per se, but only one way of analysing their content through correspondence rules, associated with an empirical conception of meaning. A direct response to this worry is to assume that theoretical terms should be interpreted "literally", or realistically, in terms of direct reference to natural properties, or simply to remain uncommitted regarding what the right theory of meaning is (Hendry and Psillos (2007) refer to this approach as a "weak" syntactic view). However, it is not clear what is achieved by moving to a semantic view.

Suppe's citation above continues as follows:

> On the Semantic Conception such experimental and measurement techniques are used to mediate or assess the mapping relationships asserted to hold between theory structure and phenomena, but are not specified by the theory structure and so are not individuating features of theories. (p. 5)

This seems to imply that the semantic view individuates theories by their structures only, not by any kind of relation to experience. However, a similar move is available to the syntacticist: just consider that correspondence rules are not part of the theory. This move is rightly viewed as problematic, for then it seems that the theory has no empirical interpretation in itself. The syntactic view is then too coarse-grained: it incorrectly identifies theories with the same internal logical structure but a different domain of application. However, there is no reason why the same problem would not affect the semantic view if it individuates theories by their structures only (and as we will see below, this charge has indeed been levelled against it).

So, if a theory is more than a purely formal entity (an abstract mathematical structure or collection thereof), then part of its individuality must be constituted by some links to the empirical world (a claim that the structure bears some relation to the world), and the way of explicating these links while abstracting away from changes in experimental techniques that do not alter the theory is not necessarily obvious. Opting for a semantic conception seems to leave the problem untouched, or at least, the burden is on the semanticist to explain why moving away from a focus on theoretical terms and their interpretation would constitute an advantage. There are reasons to doubt it: the question of why theories remain unaffected by changes in experimental techniques seems to boil down to the question of why scientists still use the same theoretical terms after these changes. So again, there is no reason to assume that the syntactic view is too fine-grained simply because of its focus on language.

Is the syntactic view too coarse-grained? Another important motivation for the semantic view lies in the idea that the syntactic view would be limited by first-order logic. First-order logic is less expressive than set theory or ordinary mathematical language, as it is not possible to identify the intended model of a finite set of first-order axioms when dealing with infinite domains. For instance, there are non-standard models of Peano arithmetic that can only be excluded by quantifying over predicates or sets. Requesting that theories be axiomatisable in first-order logic seems too stringent a demand: aren't syntacticists pursuing an unattainable ideal of rigour?

This concern is notably expressed by van Fraassen (1989), who suggests that philosophers of science should use mathematical formulations directly, rather than relying on first-order logic:

> [W]hen a theory is presented by defining the class of its models, that class of structures cannot generally be identified with an elementary class of models of any first-order language. In a trivial sense, everything is axiomatizable, because a thing must be described in order to be discussed at all. But for logicians, 'axiomatizable' is not a vacuous term, and a scientific theory need not be axiomatizable in their sense–or, as they say, the family of models may not be an elementary class. (p. 211)

This means that the syntactic view is too coarse-grained, as it cannot distinguish between different theories that are compatible with the same first-order axioms.

It is not immediately clear how such meta-mathematical considerations related to infinite cardinalities are relevant to interpreting theories, particularly from an empiricist perspective (isn't it enough to assume that the world is one of the models of the theory, intended or not? Aren't our observations finite anyway?). In any case, Sebastian Lutz (2012) has argued quite convincingly that this is a straw-man: there were never any such commitments from syntacticists to theories being axiomatisable in first-order logic. In fact, all the distinctions between more or less expressive languages and their significance only became clear after Carnap initially formulated his views. Moreover, Carnap himself relied on type theory, which is just as expressive as set-theory (he also suggested later expressing theories using Ramsey sentences, which are second-order logic sentences). It is true that logical empiricists had a certain ideal of rigour: they believed that making explicit the structure of theories by expressing them in formal languages is desirable, but this does not involve any restriction to first-order logic, and there are no particular expressive limitations for the syntactic view in this regard.

Is the semantic view too coarse-grained or too fine-grained? In the other direction, perhaps the main syntacticist criticism of the semantic view lies in the idea that it would characterise theories using set-theory or mathematical language only, without resorting to any empirically interpreted theoretical term, in principle at least, since theories are pure structures. There are various ways of fleshing out this idea of linguistic independence (one issue being that formal languages figure in the model-theoretic definition of a model), but in any case, this has been the main source of criticism, with many authors arguing that theoretical language is needed for referential purposes, to endow theories with empirical content and a proper domain of application (e.g. Hendry and Psillos 2007; Frigg and Hartmann 2006; Chakravartty 2001). This is essentially the same issue as the one already discussed, regarding how much of the links to the world must be part of a theory's individuating features. This criticism is also similar in spirit to objections against structural realism, to the effect that claiming that the world has a given structure is not a substantive claim unless we characterise this structure using an interpreted language (see Ainsworth 2009).

Somehow relatedly, Hans Halvorson (2012) has argued that theoretical terms are needed on top of structure to connect the various models of a theory together, lest a theory be merely a disparate collection of unrelated structures: an electron in one model is of the same kind as an electron in another model, and this is crucial. In his view, it is more apt to regard a theory as a topology of models.

All this would imply that the semantic view is too coarse-grained in some respects: it identifies everything up to isomorphism and fails to distinguish theories that share the same structure but are about different things, or relate models differently.

Finally, Halvorson (2012) has also argued that the semantic view is too fine-grained in other respects, since it lacks the capacity to formulate proper criteria of theoretical equivalence. Without getting into technicalities, we can consider, in order to fix ideas, the way some surplus structure, such as gauge or coordinate systems, appears in the models of some formulations of a given theory, but not in others (for example, there are coordinate-free formulations of classical physics). If one formulation of a theory contains surplus structure, then strictly speaking, it does not have the same structure as other formulations. However, there are no purely structural means of identifying this surplus structure: here, reliance on language is required, otherwise we will see differences between theories that are actually equivalent.

In response to all these criticisms, van Fraassen (2014) claims that they are attacking a straw man. He has only ever suggested that the role of language should be limited to some "elementary statements" that locate physical systems in a state space in experimental practice (a restriction quite similar in spirit to Ramsey sentences!). For all the rest, structural considerations are more appropriate.

> The idea was not to banish language from scientific theorizing–a trivially absurd idea–but to put it in its proper place and to direct philosophers' attention away from vocabulary and grammar to models and modeling. (p. 281)

There is room for scepticism about this response. We have seen earlier how Suppe apparently wants to individuate theories by their structures only. Van Fraassen does mention elementary statements ("a very small fragment" of language) in (van Fraassen 1989), but then immediately suggests that in the end, structural similarity could be sufficient to capture meaning and reference:

> This gives us I think the required leeway for a programme in the

> theory of meaning. [...] First, certain expressions are assigned values in the family of models and their logical relations derive from relations among these values. Next, reference or denotation is gained indirectly because certain parts of the model may correspond to elements of reality. (p. 213-214)

However, the crucial point is that we need reference *before* any structural similarity can be assessed (see also Hendry and Psillos 2007, sec. 5).

Nevertheless, if we grant that some fragment of language is involved in the individuation of theories in the semantic view, presumably in the hypotheses linking models and real systems, then it is not necessarily too coarse-grained or too fine-grained. It might be possible to identify the surplus structure of a given formulation because it bears no referential link to real systems for instance, and we can assume that the elementary statements involved in experimentation are enough to fix a domain of application for theories. But of course, as already noted, the semanticists must tell us more about how these statements are interpreted, and why theories are not altered by changes in experimental techniques, and they share the same problems as the syntactic view in this respect.

Granting all this, it seems that the differences between the syntactic and semantic views have been largely oversold in this debate. At least when it comes to individuating theories, there is no substantial difference: both camps have the same linguistic resources (an expressive language and a theoretical vocabulary), and both could agree in principle on criteria of theoretical equivalence and on an empirical interpretation for relevant theoretical terms. We might suspect that this equivalence extends to other aspects, such as inter-theory relations or confirmation theory: as Steven French (2020) says, "formally, at least, anything the Semantic Approach can do, its Syntactic rival can do also" (p. 39) (although French believes that the semantic view is superior for practical purposes, notably when accounting for idealisations; we will return to this issue in section 4).

3 Staged representation

By focusing on theory individuation, we have covered most of the criticisms that have been addressed to the syntactic and semantic views on purely formal grounds, and it seems, as a result, that the distinction between the two views has become blurred. Now an important difference remains, of course: claiming that a theory is best characterised by statements, or by what these statements describe, is clearly not the same. But if the individuation of theories these views imply is equivalent, meaning that the same statements can be used to present the same theories in both views, then their difference must lie somewhere else: at the level of interpretation, or in the way theories relate to the world. This is where van Fraassen's wish to "direct philosophers' attention to models and modelling" might become important: the central message of the semantic view could be that representation of the world by means of models is at the heart of the theory–world relation, something that was unduly neglected by syntacticists. But what shall we make of this?

As a preliminary, it is worth noting that the word "model" is inherently ambiguous. It has two almost opposite understandings, as explained by Balzer, Moulines, and Sneed (1987) (whose structuralist programme is often assimilated to a semantic view):

> If we consider the term "model" with respect to the relationship between a "picture" of something and the thing "depicted", it appears that "model" is sometimes used in the sense of picture and sometimes in the sense of the thing depicted. Empirical scientists tend to use "model" in the sense of "picture", as when they say that a certain set of equations "is a model" of some subatomic phenomena or certain real-life market situations. Logicians and mathematicians consistently use "model" in the sense of the thing depicted by a picture (= by a theory). (p. 2)

Suppes (1960), who was the first to suggest using model theory in the context of a semantic conception, viewed this ambiguity as "one very common tendency, namely, to confuse or to amalgamate what logicians would call the model and the theory of the model" (p. 289, see also p. 292-3), and sided with the logical understanding of models as "the thing depicted" in order to analyse scientific models (or "model systems"). On the contrary, Giere (1999, ch. 6, notably footnote. 1) explicitly departed from this understanding and did not refrain from presenting scientific models as linguistic entities representing the world, thus taking some distance from the use of model theory in philosophy of science. Finally, van Fraassen (2024) suggests that a scientific model could fulfil both roles at the same time. As he explains (with a reference to (Morrison 2007)):

> Models look in two directions at once: a model is a model of a phenomenon (it represents the phenomenon) and a model is a model of a theory (it satisfies the equations the theory gives us). Both aspects of modeling need to be studied to arrive at a balanced view. If we neglect the first, we will be restricted to pure meta-mathematics, and lose touch with the specific character of the empirical sciences. If we neglect the second, we'll land in puzzlement with Margaret Morrison's apt question: where have all the theories gone?

The two-stage picture Let's consider this option seriously. If models are described by statements, but are themselves representational entities, then there are at least three levels of representation and two representation relations: phenomena are represented by models, which are themselves described by statements. Why assume these three levels and two stages?

One possible reason is that we would need two representation relations, from phenomena to models and from models to statements, to account for theoretical interpretation, because they would correspond to two different kinds of scientific activity: theorising and experimenting. With its single, linguistic representation relation, the syntactic view would be unable to capture this important aspect of science (remember how correspondence rules lump together heterogeneous aspects). This passage from Suppe (1989) seems to go in this direction:

> What we have here, then, is a two-stage move from raw phenomena to statements of the theory–first a move from phenomena to "hard" data about the physical system in question, and then a second move from the physical system to the postulates, and so on, of the theory. The two sorts of moves are qualitatively quite different, the former being essentially empirical or experimental (being, in effect, a "translation" from the phenomena to an idealized description of it in the vocabulary of the theory's formalism), and the latter being essentially mathematical or computational in nature. (p. 69)

This two-stage picture is later contrasted with correspondence rules: "in place of the correspondence rules providing a bridge between theory and phenomena, we now have a two-stage transition" (p. 71).

The idea that experimenting and theorising are distinct kinds of activities is intuitive, and it can be linked to van Fraassen's idea that models "look in two directions", thus playing a pivotal role in articulating these two kinds of activities. Some representation relation such as isomorphism or similarity would capture the first experimental stage between phenomena and models, while model-theory ("meta-mathematics") would capture the second, theoretical stage, by analysing models (physical systems in Suppe's terminology) as structures that satisfy theoretical postulates. This reliance on two levels of representation could be seen as the main significance of the semantic view, and its main difference with the syntactic view. Notwithstanding, this interpretation is highly dubious.

Note, first, that viewing models as mediators between theories and phenomena is in line with the way pragmatic philosophers understand them (Morgan and Morrison 1999). However, these authors, and notably Margaret Morrison (2007) cited by van Fraassen in the passage above, criticise the semantic view precisely for not accounting for this aspect by not distinguishing models and theories sufficiently enough. If theories are characterised by their models, then models are not really mediators: so, how do we get the two stages? If a collection of models is already given to us by the theory, then there is not much room for the second stage. All we need is to relate these models to the phenomena.

To put the problem differently, in the three-level picture (phenomena, models, statements), theories are naturally identified with the third level, namely statements, and models with the second level. This is how logicians talk, and the two quotes above from Suppe and van Fraassen seem to vindicate these associations. However, the semantic view is usually framed as the idea that theories are *not* statements. So, they cannot constitute the third level!

There seems to be an inherent tension, if not a plain inconsistency, between this two-stage picture and the semantic view itself.

Is a stage-picture specifically semantic? The response on behalf of the semanticist could be that although a theory is best characterised by the intermediate level of models, the second, theoretical stage we are talking about is something like the exploration of theory structure using linguistic tools (what all its models have in common, etc). However, there is a final problem with this interpretation: the idea that the syntactic view does not account for our two stages is simply confused. The distinction between correspondence rules and theoretical postulates invoked by syntacticists actually captures these two stages very well. So, this cannot be the main contrast between a semantic and a syntactic view.

In logical empiricists' version of the syntactic view, one can perfectly reason on the basis of theoretical descriptions alone, staying within the axiomatic system of the theory so to speak, by applying its postulates to descriptions of particular circumstances, such as boundary conditions or an initial state, expressed in a theoretical vocabulary. This is, in effect, building a model: Hempel, for example, considered that a theoretical model "has the character of a theory with more or less limited scope of application" (cited in Lutz 2012, sec. 4), and both semanticists and syntacticists agree that this process is somehow computational or formal (which is precisely where pragmatists disagree). This corresponds exactly to Suppe's second stage. Then correspondence rules can be used to relate these theoretical descriptions to observation reports, which are statements about instruments, experimental designs or experimental results. This means either deriving observational predictions from the model or (perhaps more in line with scientific practice) enriching observation reports until some theoretical statements are obtained that can be compared directly with the model: correspondence rules are used to translate observation reports into a "description in the vocabulary of the theory's formalism", as Suppe says. The process can involve auxiliary theories (notably theories of instruments) and bridge laws, which were typically invoked by authors working in the syntactic tradition. And this corresponds exactly to Suppe's first stage (I assume that there is no particular temporal order between the two stages).

Of course, for the syntacticist, this is linguistic representation all the way through. Comparing experimental results and predictions is checking if two linguistic descriptions of the same situation that come from different sources, one experimental and the other theoretical, are consistent with each other, or evaluating how similar they are. If model theory is involved, then the represented situation plays the role of the model (the thing depicted). And of course, there are reasons to disagree with this account, particularly its reliance on correspondence rules and the distinction between a theoretical and observational vocabulary it presupposes; nevertheless, the point is that logical empiricists broadly agreed that there are (at least) two distinct stages involved in theory interpretation, one experimental and the other purely theoretical, since they distinguished between correspondence rules and theoretical postulates.

Not only is this staged picture largely shared by syntactic and pragmatic views, but they seem to make better sense of it. In particular, they can easily accommodate more than two stages in a way that the semantic view so understood cannot. Many authors, notably Giere (1999), believe that there are multiple levels of abstraction in scientific representation, with models organised hierarchically from the specific ones closest to experience up to the abstract models of fundamental theories. Experimental activities are associated with more concrete levels, and theoretical activities with more abstract ones. However, the distinction between phenomena, models and statements is not obviously hierarchical, and more importantly, it is clear-cut, implying exactly three levels and two "qualitatively quite different" representation relations, no more and no less. In this respect, this understanding of the semantic view lacks the flexibility that other approaches can provide.

Are there stages of representation? Finally, even putting aside this issue, I believe that there is something deeply wrong with the idea that the two stages of this proposal correspond to different types of activities.

If the point is that language is only relevant at the theoretical stage, when describing entire families of structures with unifying statements, this is clearly false: language can be used quite concretely in everyday life to describe very specific circumstances, and as we saw earlier, van Fraassen himself claims that elementary statements are involved in experimentation. In the end, statements are needed both when theorising and when applying theories: we need them in order to present fundamental postulates and abstract models, but also more concrete models, such as phenomenological models, models of experiment or data models. It's not as if at some point we would only need the structures and could set aside the statements describing them. Furthermore, one could argue that the fact that models or statements aim to represent specific situations or kinds of phenomena is also implicitly present even when formulating theoretical laws or presenting a theoretical structure using abstract mathematical statements. In all likelihood, there is still at least a vague sense of intended application to the world in theoretical exploration and abstract model building activities.

All this to say that the two different kinds of representation relation put forth by this understanding of the semantic view seem largely orthogonal to the levels of abstraction allegedly involved in scientific representation. Levels of abstraction do not necessarily imply layers of representation. In Giere's account of a hierarchy of models, abstract models do not *represent* more concrete ones, they make them more specific, which is different (remember that Giere did not subscribe to the three level picture). In the end, granting that the theory–world relation is best expressed in broadly representational terms, only one kind of representation relation is enough to accommodate levels of abstraction: the hierarchy lies on the representational side of it, starting from models of data and of experiment, or even perceptual representations maybe, through phenomenological models, and up to fundamental theories, while the world (systems, phenomena, laws of nature perhaps) lies on the object side of it all. Furthermore, most of these representations might well be linguistic in format, so the move from statements to structures is not obviously needed either. Or at least, this is what I will argue in the next section.

4 Non-linguistic representation

In the previous sections, we have already rejected two possible understandings of the difference between the semantic and syntactic views: one bearing on theory individuation, and the other on the idea that there are layers of representation associated with different kinds of activities. However, there is a different way of fleshing out the semanticist idea that representation by means of model is central to the theory–world relation, without relying on layers of representation. It consists in assuming that theoretical representation is non-linguistic *all the way through.* There would be something specific about theoretical representation in science (by means of models) that is entirely unlike describing physical systems in a language. This is why language would be largely irrelevant for understanding scientific theories. If we want to maintain the three-level picture mentioned in the previous section, we can see it as a mere way to accommodate a secondary role for language, because we need it to describe structures, while putting non-linguistic representation in the foreground.

On this view, a theory provides us not with literal descriptions of the world, but with a certain way of representing it, or a certain family of representational vehicles, its models, which are non-linguistic in nature. These models are involved at any stage, whether abstract or concrete. However, the way they represent the world is not adequately captured by correspondence rules, nor by any linguistic theory for that matter: notions such as isomorphism or similarity should be used instead, because this form of representation is markedly *non-linguistic.*

I believe that this is the most sensible and faithful understanding of the semantic view. However, I believe that it also makes the view implausible.

What does it mean exactly to represent the world using an abstract structure, as opposed to linguistic descriptions? In the end, theoretical models themselves are expressed using mathematical statements, or formulas in a given language. In concrete applications, aren't these formulas (for example, those describing the oscillation period of a pendulum) taken to describe real systems, at least approximately? Data models too are often expressed using formulas stating correlations between quantities and the like. Aren't they direct descriptions of experimental results? And what is the difference between comparing different models of a system, a theoretical model and a data model for example, and comparing two different descriptions of the same system, one derived from an experiment and the other from a theory? Isn't it more or less the same thing, once we acknowledge that the statements involved are the same? Or shall we assume that at some point, abstract structures are somehow mysteriously seized and used by our intellect to represent reality in a way that is entirely different from describing it using statements? (See (Levy 2015) for similar criticisms against indirect representation.)

Here are some possible responses on behalf of the semanticist. Perhaps theoretical representation is non-linguistic because:

1. The relation between theoretical models and phenomena is partly informal. Applying a model to a concrete situation requires non-linguistic input, notably experimental abilities. Therefore, thinking of it in linguistic terms is inappropriate.
2. Its format is different. Representation by means of theoretical models is more "pictorial" than linguistic description. Therefore, syntactic analyses of theories are inappropriate.
3. Model descriptions are not strictly true of the real systems that models represent due to idealisations and the like. Successful theoretical representation is not a matter of linguistic truth, but rather a matter of structural similarity.
4. Theoretical models are not directly descriptive. They can be used in various ways and contexts for diverse purposes. They are more like epistemic tools than sentences.

Let us examine the first three proposals in turn. The fourth one will be addressed in the next section.

Informal representation I believe that option (1) about the informal aspects of experimentation is a red-herring. In practice, the mappings or comparisons invoked by semanticists are mappings between different models or representations of the same object, for instance theoretical and data models, which is not very different from correspondence rules linking observation reports to theoretical statements. Importantly, the informal aspects of experimentation are not involved in these comparisons, but upstream of them: when building data models to be compared to theoretical ones, and as already explained, van Fraassen sees a role for language there. As discussed in section 2, without this, a theory would not have an empirical interpretation.

Similarly, producing observation reports, to which correspondence rules are applied in the syntactic view, requires contextual input that is not linguistic, but rather perceptual, and that can rest on informal abilities. How these informal aspects relate to linguistic interpretation is a difficult question, and arguably, invoking correspondence rules all the way down to hypothetical sense-data is problematic, but this idea was abandoned by logical empiricists early on. In any case, the two views are on the same boat in this respect, and focusing on structures instead of language seems quite irrelevant: we do not need the notion of a model to address the informal aspects of experimentation, since models are just as formal entities as statements.

It is interesting, in this respect, that Giere (2006, 84) mentions favourably usage-based approaches to language, and that van Fraassen (1989) criticises ideal language semantics, advancing "a conception of natural language as not being constituted by any one realization of any such logical skeleton". (p. 212) We find a similar reaction against ideal language semantics in ordinary language philosophy, which led to the development of pragmatics. I find it worth considering the idea that the true remedy against logical empiricist's problematic reliance on systematic correspondence rules lies there, and not in a move away from linguistic representation. Pragmatics allows for an involvement of contexts and speakers in meaning in a way that corresponds to my understanding of a pragmatic, as opposed to semantic, conception of theories (this will be explained in section 5).

Pictorial representation The second idea, that theoretical representation is more pictorial than linguistic, could be associated with Giere's cognitive account of science, but it is also a red-herring in my view. Surely, diagrams, graphs and pictures are sometimes used in science in relation to theoretical models. However, diagrams and graphs are quite similar to sentences. A circle on paper does not make true the mathematical formula for a circle, since it is never a true mathematical circle; it rather represents one in a format that is rich and easier to process visually than normal text. So, graphs and diagrams cannot be identified with the notion of models as truth-makers for statements invoked by semanticists. But even if this was what semanticists were after, it seems to me that diagrams, graphs and pictures (including mental pictures) are primarily illustrative or heuristic. Science strives for precision, and the true content of a theoretical model usually lies in precise mathematical formulas or descriptions of the represented system, not in pictorial representations. Pictorial representation doesn't play the mediation role between abstract statements and real systems that semanticists see models play.

Idealised representation The third option, that theoretical representation is non-linguistic because models are idealised, is one of our best contenders. It is notably mentioned by French (2020, 31, see also p. 67-69 on inconsistencies) as a criticism of the syntactic view, and is also important for Suppe (1989)'s two-staged picture:

> Physical systems, then, are highly abstract and idealized replicas of phenomena, being characterizations of how the phenomena would have behaved had the idealized conditions been met. (p. 65)

It is true that in general, model descriptions aren't strictly true of real systems. They can describe infinite gases, dimensionless solids, frictionless planes, and the like. However, modellers must and often do specify what to take seriously or not in their models for these models to be scientifically useful (or at the very least, making everything explicit can do no harm). They will say, for example, that no real gas is infinite, that no solid is actually dimensionless, or that friction is "negligible" (but no actually absent), thereby implying that the model does not aim to be perfectly accurate in *those* particular respects, although it aims to be accurate in other respects (a pendulum is a bob attached to a string, it oscillates, etc.). A syntacticist could account for these idealisations by claiming that there are implicit correspondence rules qualifying the empirical interpretation of models. If the point is that model descriptions should not be interpreted literally, this is good news for logical empiricists, since they typically did not do so!

This solution may seem as implausible as the logical empiricists' theory of meaning, but then the question is whether the fact that model descriptions aren't all taken to be literally true by modellers is a problem for any more plausible linguistic theory, or more generally, whether it is unlike anything found in natural languages, which would force us to assume a different format of representation. Arguably, we sometimes idealise in ordinary language ("Italy is a boot"). Accounts of partial truth, initially developed by philosophers of language, have also been used to analyse idealisations in science (Pincock 2021). So the prospects might not be as bad as it seems for a syntactic view.

However, the main problem with this option is that invoking idealisations in favour of the semantic view is a double edge sword. One might suspect that the way to handle idealisations is contextual, in that it depends on the disciplinary context where the model is proposed. This cannot be addressed by universal rules of interpretation, so this is surely a problem for the logical empiricists' framing of the syntactic view, but it also affects the semantic view: if we cannot specify clear model–system links at the theory level, then a theory understood in the semantic way is fundamentally under-defined, and if we fall back on purely structural relations, it loses any substantial empirical content (as per the criticisms examined in section 2). Furthermore, idealisations often distort theoretical laws, so idealised models are not strict truth-makers for theoretical statements. Even claiming that a theory is a well defined family of models becomes problematic. Here again, a linguistic construal rooted in pragmatics, with an involvement of contextual aspects, and a sharper distinction between theories and their models, might offer better prospects.

These issues of contextual interpretation are crucial when it comes to contrasting a semantic and a pragmatic view, and they become particularly salient with regard to our last option, which is the idea that theoretical models are more like epistemic tools than like plain descriptions. So, let's now turn to this last option.

5 Schematic representation

Semanticists seek to draw our attention to models and modelling, and it is undeniable that models are crucial in science, and that they differ from descriptive statements in several respects. For instance, a model is not generally said to be true or false, but rather good or bad, and it serves as a well-defined unit of representation that can be applied in various contexts to represent different objects for diverse purposes. In this regard, models appear more akin to epistemic tools than to descriptive statements: representational tools that is, assuming that the way these tools function is still best captured in representational terms (this holds at least in the minimal sense that in concrete use, a model "stands for" or "is about" some aspects of a target system, and typically enables its users to make inferences about them, which aligns with minimalist conceptions of representation such as Mauricio Suárez (2024)'s). I will assume this in what follows.

These aspects are not accounted for by the syntactic view, but they are not necessarily at odds with a linguistic format. So far, we have tried to make sense of the semantic view by focusing on a contrast between structural and linguistic representation, a contrast put forth by semanticists themselves. Perhaps this was a mistake, and the real contrast lies between plain description and this tool-like, yet still representational aspect, what I shall call *schematic representation*.

Schematic representation One way to make sense of schematic representation that is *not* linguistic is to claim, as semanticists generally do, that a model is an abstract structure that can be compared to many different systems in various ways and for various purposes. However, as we have seen, this pretension of linguistic independence is not particularly felicitous, as it puts issues of interpretation under the rug instead of addressing them explicitly. Fortunately, there is another way to make sense of this notion of schematic representation by linguistic means, one that stays close to scientific practice. It consists in assuming that a theoretical model is a set of open statements with free variables and parameters. This could be, for example, the description of a system S with mass M, a centre of coordinate O, the position of the system x along an axis, and so on, together with statements establishing relations and constraints on the values these free variables can take. Such constraints are conveniently expressed as constraints on a state space, in which case this conception of model is not very different from the one entertained by van Fraassen.

The fact that these variables are free means that they only refer or take value in contexts of application, and that they will have different referents and values in different contexts. This means that the same model can be used to represent many different situations of a given kind. To put it differently, models are like *indexicals* (Ruyant 2025): just like "I" and "now", what they refer to varies with the context. Applying or interpreting a model in a particular context involves coordinating it with a concrete target system by assigning a reference to (or binding) its free variables. A coordinated model is accurate in a context if its statements are true once the free variables have been bound. Of course, once the model is coordinated, its statements are no longer open, but the fact that the *same* model (for example, *the* model of the pendulum) can be applied in different contexts implies that it should be identified with the open statements rather than the contextual statements.

Coordination of a model with a context is what van Fraassen (2008, notably ch. 7) would describe as locating oneself with respect to the representation. However, these aspects can be framed linguistically. We do not need any abstract model system to account for them, or only as a fictional placeholder, a support for imagination that is, to bind free variables in situations where no concrete target system is present, such as when presenting the model in a classroom or scientific article (see Ruyant 2025, sec. 4.2).

Accounting for how models are used for concrete or hypothetical inferences about target systems is quite straightforward in this view. Modellers perform deductions or demonstrations using open statements, and then they interpret their conclusions in terms of a target system by binding the free variables of these statements. They can also interpret these conclusions in terms of a collection of potential targets by considering a set of possible bindings, or even engage in hypothetical reasoning by imagining counterfactual bindings (non actual parameter values, etc.).

Are rules of coordination local or global? I take this notion of schematic representation to be compatible with various stances leaning towards either a semantic or pragmatic view. A semantic view, for instance, could claim that a theory is a collection of schematic representations that are logically consistent with certain laws. However, this framing allows us to see where different views diverge.

Notably, the fact that models are coordinated in context does not necessarily entail that the way of interpreting the model is established at the contextual level, nor that it depends on users' aims and intentions (although it could). Despite being context-sensitive, indexicals such as "I" and "now" adhere to systematic rules of interpretation in English, which can be formulated as functions from context to content ("I" refers to the speaker in any context). Nevertheless, it is true that meaning is sometimes modulated by context in more informal ways, as when "heavy" is applied to either a bag or a meal, or "here" refers to a broader or narrower area depending on the subject matter of the conversation. By analogy, model coordination might follow standardised rules of interpretation that are established at the level of the theory (a temperature variable should be mapped to a measured temperature, etc.), but we might wonder whether these rules are sometimes more context-dependent and informal, perhaps influenced by local standards of accuracy or a focus on specific aspects, for instance. The idea that user aims and intentions are centrally involved in modelling activities has often been emphasised in the philosophical literature on scientific representation (see Frigg and Nguyen 2021). This could be where the main disagreement between a semantic and a pragmatic approach lies.

This is also where our notion of schematic representation, with its linguistic framing, bears fruit, for it forces us to be precise when claiming that interpretation or representation is contextual: the interpretation of which theoretical terms (all, only some)? Following which rules (anything goes)? And at which level? In particular, how much of coordination rules is constrained by theoretical frameworks at the disciplinary level, how much is fixed by more specific research programmes, and how much during concrete experiments only? How much of them informally depends on users, aims, and other contextual features, and how much on more objective aspects of contexts in a standardised way? How much of them can evolve without altering the theory? All this would translate into different analyses of theoretical terms, free variables and associated coordination rules. This puts the focus on the right kind of question that a conception of scientific theories must answer. And this is also where pragmatic criticisms of the semantic view become relevant.

Problems for the semantic view According to Nancy Cartwright, both the syntactic and semantic views are instances of the *vending machine view*: the idea that one can simply input a description of a situation, and the theory will output predictions, in the form of a model or statements. In our context, this corresponds quite precisely to this idea that all coordination rules for models are specified at the theory-level (which, remember, is compatible with schematicity), and that applying a theory merely amounts to following these rules. Cartwright argues, against this view, that the way a model is built to represent a given phenomenon is only loosely constrained by the theory. For instance, in the case of superconductivity that she analyses, quantum theory only provides stock models with standard Hamiltonian operators, but these need to be adapted to the specific case and combined using various assumptions about the phenomenon at hands (Cartwright 1999 ch. 8). Similarly, in classical mechanics, the theoretical framework states a relation between force, mass and acceleration, but force functions are not specified by the theory beyond a few stock models. These are aspects that models add on top of the theory, and it is for this reason that models are indispensable bridges between the theory and the phenomena.

The problem for semanticists is that this implies that rules of model coordination are introduced at the level of a local research context, rather than at the level of the theoretical framework. We can consider the BCS model of superconductivity that Cartwright analyses as a set of open statements with free variables, the various parameters of the model, and some of these free variables probably have standard rules of coordination entrenched in experimental practice at the disciplinary level (mass, temperature, conductivity, etc.). However, arguably, at least one free variable of the model is coordinated in a way that is not specified at the theory level: the variable that denotes the full system that is represented, characterised as a superconductor, which should be bound to particular instances of the kind.

Without this binding rule, the model cannot be taken to represent a superconductor in particular. However, "superconductor" is not a term from the quantum framework, but a more specific term associated with experimental capabilities aimed at stabilising a particular class of phenomenon that the model was designed to explain within a specific research context. This context and its associated experimental abilities provide tacit rules for binding the free variable of the model that denotes the full system, while the model makes explicit hypotheses about the corresponding kind of target system, its dynamics and physical configuration, along with idealisations that may also imply contextual interpretation rules. In effect, the way of coordinating a model of superconductivity with the world is not fixed at the level of the theory, but rather belongs to the local research context where the target phenomenon is identified.

The semantic view holds that a theory can be characterised by a well-defined family of models that satisfy certain statements. However, if Cartwright's analysis is correct, then the set of structures satisfying these statements is either much too large to be of scientific relevance, or too open ended to constitute a well-defined family. It is too large if any arbitrary force function or Hamiltonian is allowed, since most of those are irrelevant in our universe. However, if we attempt to fix the relevant set by considering only the models that are actually deployed in practice, then this set becomes open ended, as it expands every time new technical terms, such as "superconductor", are introduced in relation to experimental practice. Yet new experimental techniques do not alter the theory: recall that this was one of the reasons for rejecting the syntactic view. Therefore, the idea that a new vocabulary and associated coordination rules can be introduced in local research contexts within specific theoretical models is incompatible with the semantic view.

The only way to maintain a semantic view is to argue that model autonomy is not as relevant as it seems, or that the aspects mentioned here are only relevant when focusing on the process of model building (figuring out which model of the theory fits which part of the universe) but disappear in the end product (see van Fraassen 2008's response to Cartwright p. 309-311). Perhaps an ideally complete theory would specify all relevant forces and how to apply them in any context by strict interpretation rules. However, this raises a different issue: if this is the case, then we are back to the vending machine view. In this context, "messy" models lose their indispensability as mediators. They mostly play a heuristic role for devising theories. This is also how logical empiricists considered them, and we have no more options available to explain how the semantic view differs from the syntactic view.

6 Towards a pragmatic view

My aim in this article was to mount a dilemma against the semantic conception of theories, by examining various ways in which its focus on models could make it significantly different from the syntactic view, and arguing that the only viable option, schematicity, ultimately undermines the view. We have identified, in the course of our arguments, several aspects that seem crucial to understanding scientific theories:

1. Theories are not pure structure, but have an empirical interpretation associated with a vocabulary (section 2).

2. The interpretation of abstract theories in relation to phenomena is mediated by more concrete models, which may be organised in a hierarchy (section 3).
3. The most concrete or contextual levels of representation, associated with perceptual and experimental abilities, are generally informal; abstract or intermediary levels are generally idealised (section 4).
4. Theoretical representations are schematic rather than descriptive. They can be applied in different contexts with varying interpretations that are not necessarily fixed at the theory level (section 5).

As we have seen, the claim that theories are families of models does not account for these aspects any better than the syntactic view. However, we can grant that the intention behind this claim was to draw our attention to some of these aspects through the notion of model, and that was a laudable intention. To supersede the semantic view, a pragmatic conception should acknowledge these aspects and strive to make sense of all four points above. Much remains to be worked out in this respect, but I believe that the notion of schematic representation presented in the previous section as a means to address point (4) is an important step in this direction.

One way of making sense of Cartwright's analysis in particular consists in extending our notion of schematic representation to theories: a theory is not a collection of models; it is itself a schematic representation at a higher level of abstraction incorporating force functions and Hamiltonian operators (or fitness functions in evolutionary theory) as *second-order* free variables that are bound in model construction. These variables are second-order because they are meant to be bound to *types* of systems, not to their individual instances. They take different values in different contexts, but we are no longer referring to concrete experimental contexts where particular objects are found. We are talking about more abstract research contexts, such as research on superconductivity, where *types* of objects and phenomena are identified. In the case analysed by Cartwright, the BCS model binds the free variable associated with the Hamiltonian operator by making it describe the dynamics of superconductors. However, the model introduces new free first-order variables in the process: the parameters of the model, as well as the variable denoting the full system and its parts, which are meant to be bound to particular quantities and objects in concrete experimental contexts. Thus models remain (first-order) schematic representations.

The virtue of this approach is that it can avoid two pitfalls. On the one hand, it avoids treating theories as pure formal systems, thereby addressing point (1) from our list above. There is room for an empirically interpreted vocabulary in this view, since the interpretation of some theoretical terms such as position or electric current might well be standardised at the theory level. This is the case assuming that there is a way of characterising these terms as fixed points even when experimental practice evolves (see Arabatzis 2012 for a proposal in this respect). On the other hand, since the theory remains schematic with regard to other terms such as "force" or "fitness", models that bind their free variables and fix domain-specific coordination rules in the process remain indispensable mediators. This helps us avoid the "vending-machine view", thus addressing point (2) as well. Besides this approach leaves room for multiple levels: theoretical representation could well be hierarchical, or, perhaps better, *modular* (see Darrigol 2008). Incidentally, it allows for an involvement of user aims, practical abilities and contextual aspects in theoretical interpretation at various levels of context. What we have is not a semantic view, nor a syntactic view, but a pragmatic view. Point (3) remains to be addressed, but hopefully, the context-dependency of coordination rules might shed light on how abstract representations are idealised when encompassing many potential contexts, yet ultimately rooted in low-level informal abilities.

A recurring challenge that concerns any conception of theories is to understand how abstract theories can maintain a certain level of independence from low-level evolution in experimental practice, while still having an empirical interpretation. Associated with this empirical interpretation is the idea that a theory is a truth-bearer, that it can be believed or negated, or at least accepted or rejected. This poses a specific challenge for a pragmatic view that assumes that part of the theoretical vocabulary, such as "force" in classical mechanics, is only precisely defined within particular contexts. Can a theory be considered true or false if this is the case? In all likelihood, this feature is only compatible with a pragmatist understanding of theoretical truth. Elaborating the pragmatic view in all the respects mentioned here is a topic for future work.

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