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Whither Structuralism for Scientific Representation?

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by Francesca Pero

Abstract

The present dissertation analyses the relationship between the concept of scientific representation and the concept of structure. According to the recent literature on scientific representation, such relationship is rather problematic.

The problematic character of the relationship is partially due to the notions it calls for. Indeed, neither the concept of scientific representation – generally intended as the construction and the application of models to the phenomena investigated – nor the concept of structure – generally intended as a set of objects and relations among these objects – are employed unambiguously.

Moreover, it is widely acknowledged that the use of the concept of structure in the analysis of scientific representation is controversial since it can lead to two undesirable consequences. First, focusing solely on structures might lead to neglect the pragmatic aspects of scientific representation in favor of the relationship between the vehicle and the target of representation. Second, focusing on structures alone may require to define the relationship between the model and its target in structural terms (e.g., by resorting to the use of morphisms) in order to justify the representational relationship.

The dissertation is articulated in three chapters. In each chapter the problem is analysed by examining the relationship between scientific representation and a particular concept of structure, that is, respectively, structure as pattern-ascription, structure as presented within the semantic view of scientific theories, structure as

presented within (a particular instance of) the so-called structural approaches to scientific representation. The analysis aims to identify *if* and *to what extent* the concept of structure can be integrated into the philosophical analysis of scientific representation without leading to the two undesirable consequences above.

Chapter 1 provides a critical review of some of the most recent approaches to scientific representation. In particular, the so-called *deflationist* approaches are considered, which I group into two categories. The first category comprises approaches that *deflate the problem* of scientific representation. According to these approaches, scientific representation is not crucially different from other forms of representation, so it does not pose any special problem to be solved. The second category comprises approaches that *deflate the concept* of scientific representation. According to these approaches, it is not possible to give necessary and sufficient conditions for all occurrences of representation in science. My stance on these two forms of deflationism is twofold. I reject the deflationism about the problem of representation by appealing to a concept of structure conceived as pattern ascription – which I label *epistemic structure ascription*. As for the deflationism about the concept of representation, I subscribe its claim, and I argue that it can be fruitfully combined with the concept of epistemic structure ascription.

Chapter 2 examines the relationship between the semantic view of scientific theories and scientific representation. The analysis of this relationship is relevant for two reasons. First, the semantic view, conceived as an analysis of scientific theories focusing on models (rather than on language), is widely acknowledged among the analyses contributing to the development of the issue of scientific representation. Second, being presented as a formal analysis of models as structures, the semantic view is often charged of leading to the undesirable consequences mentioned above, i.e., to neglect the pragmatic aspects of scientific representation and to define the relationship between models and target systems solely in structural terms. The aim of this chapter is to show that the semantic view, at least in its early formulation, is not the legitimate target for these criticisms.

Chapter 3*, identifies one legitimate target of the criticisms usually directed against

*Chapter 3 is based on a joint work with Mauricio Suárez.

the semantic view: the structural accounts of scientific representation and, in particular, the structural account propounded by Bartels (2006). Applying the argument from the *misrepresentation* objection – the fact that structural accounts are not capable to explain the possibility of inaccurate representation – we show that if the concept of structure is isolated from all the pragmatic aspects of representation, then it is not sufficient to account for misrepresentation and, hence, to account for representation *tout court*.

The overall conclusion of this dissertation is the following: the concept of structure can still be useful within the philosophical analysis of scientific representation, provided that the limitations on its use are plainly specified. More precisely, as stressed in Chapter 1 by appealing to the notion of epistemic structural ascription, the concept of structure sheds light on what makes models accomplish their representational task; as pointed out in Chapter 2, where a fair assessment of the semantic view as an account of representation is attempted, the concept of structure *can* be employed to provide a rational reconstruction of the use of models in the scientific practice. However, as argued in Chapter 3, the concept of structure exceeds its scope when it is employed either to reconstruct the scientific theorizing independently of pragmatic aspects, or to set the necessary and sufficient conditions for *all* the occurrences of scientific representation.

Abstract
(in italiano)

In questa tesi viene analizza la relazione tra il concetto di rappresentazione scientifica e quello di struttura. Questa relazione risulta essere particolarmente problematica secondo la letteratura più recente sulla rappresentazione scientifica.

Parte del problema dipende dalla difficoltà di definire in modo esauriente sia il concetto di rappresentazione scientifica, generalmente inteso come costruzione e applicazione dei modelli ai sistemi indagati, sia il concetto di struttura, generalmente inteso come un insieme di elementi e un insieme di relazioni definite sugli elementi.

Inoltre, si ritiene che il ricorso al concetto di struttura nell'analisi della rappresentazione scientifica sia controverso dal momento che può portare a due conseguenze indesiderate. In primo luogo, focalizzarsi esclusivamente sul concetto di struttura può indurre ad ignorare gli aspetti pragmatici della rappresentazione scientifica in favore della sola relazione tra rappresentante e rappresentato. Secondariamente, a meno che non si definisca strutturalmente anche la relazione tra il modello e il sistema indagato (ad esempio, ricorrendo alla nozione di morfismo), tale relazione rimane priva di giustificazione.

La tesi è articolata in tre capitoli. In ciascun capitolo il problema è analizzato esaminando la relazione tra il concetto di rappresentazione e una particolare formulazione del concetto di struttura. Nel primo capitolo il concetto di struttura considerato è quello di *ascrizione* o *riconoscimento* di un ordine (*pattern*). Nel secondo capitolo il concetto è quello presentato dalla concezione semantica delle teorie scientifiche. Nel terzo capitolo il concetto è quello presentato da una particolare istanza di approccio strutturale alla rappresentazione scientifica. Lo scopo dell'analisi è quello di stabilire *se* e *fino a che punto* il concetto di struttura possa essere integrato nell'analisi filosofica della rappresentazione scientifica senza implicare le conseguenze indesiderate menzionate.

Nel capitolo 1 viene fornita un' analisi critica di alcuni degli approcci più recenti alla rappresentazione scientifica. In particolare, sono presi in esame gli approcci

deflazionisti alla rappresentazione, che distinguo in due categorie. La prima categoria si applica al *problema* della rappresentazione: la rappresentazione scientifica non è diversa da altre forme di rappresentazione, pertanto essa non pone alcun problema *speciale* da risolvere. La seconda categoria di deflazionismo si applica al *concetto* della rappresentazione scientifica: non è possibile fornire una definizione di rappresentazione che ne individui le condizioni necessarie e sufficienti. Nel capitolo sostengo che gli approcci che rientrano nella prima categoria devono essere rifiutati e che il concetto di struttura inteso come *ascrizione di un ordine* – concetto che chiamo '*ascrizione epistemica di struttura*' (*epistemic structure ascription*) – ha un ruolo decisivo in questo rifiuto. Per quanto riguarda gli approcci che rientrano nella seconda categoria, sostengo che questi debbano essere accettati e suggerisco una loro integrazione con l'ascrizione epistemica di struttura.

Nel capitolo 2 viene esaminato il rapporto tra concezione semantica delle teorie scientifiche e la rappresentazione scientifica. L'analisi del rapporto è rilevante per due ragioni. In primo luogo perché la concezione semantica, essendo un'analisi delle teorie scientifiche che si focalizza sui modelli (più che sul linguaggio) è considerata tra i fattori che hanno concorso a stimolare il dibattito filosofico sulla rappresentazione scientifica. In secondo luogo, venendo considerata un'analisi formale centrata sui modelli come strutture, la concezione semantica è generalmente tacciata di portare alle conseguenze indesiderate citate inizialmente, vale a dire, l'ignoranza degli aspetti pragmatici della rappresentazione scientifica e la definizione in termini puramente strutturali della relazione tra il modello e il sistema indagato. L'obiettivo del capitolo è argomentare che la concezione semantica, almeno nella sua formulazione iniziale, non è il corretto destinatario di queste critiche.

Nel capitolo 3[†] si identifica il corretto destinatario delle critiche mosse alla concezione semantica, cioè gli approcci strutturali alla rappresentazione scientifica e, in particolare, l'approccio strutturale formulato da Bartels (2006). Viene dunque considerato uno degli argomenti usati in letteratura per indebolire gli approcci strutturali, detto 'della rappresentazione fallace' (*misrepresentation*). Secondo questo argomento, gli approcci strutturali non possono giustificare la possibilità di una rappresentazione fallace. Ricorrendo a queso argomento, dimostriamo che, se il concetto di struttura viene completamente isolato dagli aspetti pragmatici della

[†]Il capitolo 3 è basato su un lavoro in collaborazione con Mauricio Suárez.

rappresentazione, allora esso non è sufficiente per giustificare la rappresentazione fallace e, dunque, per giustificare la rappresentazione *tout court*.

La conclusione della tesi è in sostanza la seguente: il concetto di struttura può essere ancora utile per l'analisi filosofica della rappresentazione scientifica, purché i suoi limiti vengano chiaramente individuati. Più precisamente, come sottolineato nel Capitolo 1 riferendomi alla nozione di ascrizione epistemica di struttura, il concetto di struttura è utile a chiarire cosa consente ai modelli di assolvere la loro funzione rappresentazionale; come ho messo in rilievo nel Capitolo 2, dove ho tentato di fornire una ricostruzione il più fedele possibile alle prime formulazioni della concezione semantica delle teorie scientifiche, il concetto di struttura *può* essere usato al fine di fornire una ricostruzione razionale dei modelli utilizzati nella pratica scientifica. Tuttavia, come abbiamo argomentato con l'analisi offerta nel Capitolo 3, il concetto di struttura *non può* essere usato per fornire una ricostruzione della rappresentazione scientifica nella quale vengono ignorati gli elementi pragmatici, o per individuare le condizioni necessarie e sufficienti valide per *tutte* le istanze di rappresentazione scientifica.

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Introduction

Scientific representation is a term which is still lacking a univocal interpretation notwithstanding many scientists and philosophers agree that science amounts to a representational activity. The issue of scientific representation has arisen as a *philosophical* attempt to systematize the epistemic process of acquiring knowledge on a specific target, and to analyse thoroughly the devices available to carry out such a process. However, as Hughes – one of the main contributor to the philosophical debate on the issue – points out, to call the byproduct of the theorizing activity "model", and the relation between a model and target system "representation", does not take us very far (Hughes, 1997, p.325). Scientific models come in too many varieties, and equally varied are the possible representational relationships holding between specific kinds of models and their target systems, not to mention the fact that models may not be the only means through which scientific representation can be carried out.

Philosophical attempts to bring some order to the complexity of the representational issue should be dated back to the 1960s, when the semantic view arose as an alternative to the syntactic view of scientific theories, drawing the philosophers' attention to models – presented as proper explanatory tools, rather than as mere heuristic devices. The semantic view has focused its analysis of theories on the notion of models as *structures*, either model-theoretically or set-theoretically understood. Since then, the possibility to understand the explanatory roles of models in terms of either logical or mathematical properties has offered a quick route for the philosopher of science to a clearer overview of scientific representation. Already in the early 1980s, the approaches based on the notion of structure started to be criticized. In particular, the notion of model put forward by the 'semanticists' has been criticized as too theory-oriented: conceiving models as semantic realizations of a theory, the semantic view does not account for models which do not directly depend on theories for their construction, as well as for models which do not find in one single theory the formal background for their construction. More importantly, focusing on formal aspects of theory construction and application, the semantic view has been charged of not taking into account the pragmatic aspects proper to scientific theorizing. In the 1990s, structural approaches have been put forward and presented as formulations of the semantic view. As the semantic view, structural approaches conceive models in structural terms, and justify the applicability of models to their target system in terms of structural relationships of some sort (mainly, morphisms). Notwithstanding structural approaches present themselves as formulations of the semantic view, they go one step further with respect to the latter. This step consists in positing structural relationship as necessary and sufficient conditions for representation. The skepticism already fostered towards representational accounts based on the notion of structures then has grown further. Indeed, within structural approaches, the role of model-users and the pragmatic aspects of model construction are blurred up to the point to question their relevance for the philosophical analysis.

Consequently, when it comes to the issue of representation, it seems that the philosopher of science has to choose between two clashing schools of thought. On the one hand, the philosopher could opt for privileging accounts of representation mainly focused on structural features, and leaving to, e.g., the sociologist of science the analysis of the pragmatic aspects of model construction and application. On the other hand, she could opt for focusing on the pragmatics of modelling. In the latter case, formal concepts are not strictly necessary for the analysis and the philosopher might be content with examining *particular occurrences* of scientific representation, and with drawing conclusions on the grounds of these occurrences. However, I think that a halfway house between the two schools of thought can be found. The philosophical analysis of scientific representation should not confine itself within the safe boundaries of case studies. Nor it should confine itself within the safe boundaries of any sort of formalism. It should rather attempt to provide a rational reconstruction of scientific representation where both the pragmatic and the formal aspects of representation have their own place.

The goal of this dissertation is to see if we can clear up the concept of structure from the limitations ascribed to it, and to see if the concept can be fruitfully used for a rational reconstruction of scientific representation. A rational reconstruction of scientific representation depends on the selection of relevant aspects of theorizing which characterize *particular* sciences (e.g. physics, biology, chemistry, etc). In the selection process, several details which actually characterize scientific theorizing, and which are distinctive of the different disciplines, are left out. Nonetheless, the rational reconstruction, in virtue of its approximate and general character, might help to shed light on the whole activity itself.

The dissertation is articulated in three chapters. Each chapter analyses *if* and *to what extent* the concept of structure can be integrated into the philosophical analysis of scientific representation. In each chapter a particular concept of structure is considered, that is, respectively, structure as pattern-ascription, structure as presented within the semantic view of scientific theories, structure as presented within (a particular instance of) the so-called structural approaches to scientific representation.

Chapter 1 introduces the issue of scientific representation (Section 1.1) and highlights a recent tendency in philosophy of science to dismiss the possibility of a rational reconstruction of the issue aiming at singling out the *general* features of representation in science (Section 1.2). The aim of the chapter is to see if this general tendency can be resisted and, in particular, to see if the concept of structure can play a role in reversing the trend. To this purpose, I consider different forms of deflationism concerning scientific representation which have influenced the debate in recent years.

In Section 1.3, I consider the form of deflationism put forward by Callender and Cohen (2006). This form of deflationism turns out to be quite detrimental for the debate on scientific representation since it aims at deflating the very *problem* of scientific representation. Indeed, Callender and Cohen argue that scientific representation has no special status with respect to other kinds of representation and, as any of these kinds, it can be reduced to just one fundamental form of representation. They frame their deflationism within the so-called *General Griceanism*, according to which representation obtains by stipulation, that is, representational vehicles represent their targets by virtue of their users' mental states, on the grounds of which users agree that the vehicle can be employed to represent.

In Section 1.4, I discuss two criticisms against General Griceanism, one made by Toon (2012) and the other by Liu (2015). These two criticisms, although not identical, are very similar. Indeed, both Toon and Liu reject Callender and Cohen's deflationism on the grounds that it does not explain the epistemic role that models have in scientific representation; hence, they argue, it fails to reduce scientific representation to a more fundamental form of representation. I agree with their conclusion about the impossibility for General Griceanism to account for scientific representation. However, I suggest that their criticisms need to take a further step in order to be fully effective. More precisely, I argue that we have to specify *what* kind of epistemic task models are built for and *how* a model-user can employ models to accomplish such a task. I conclude by suggesting that scientific models are built and used for two very specific epistemic tasks, i.e., *to explain* and *to predict* the behavior of target phenomena, and that it is possible to identify a core feature that characterizes how such epistemic tasks can be accomplished, namely, the capability of models to allow the model-user to ascribe a certain structure to a target system – which I call *epistemic structure ascription*.

The other form of deflationism which I consider in this chapter concerns the *concept* of scientific representation and it aims at showing that it is not possible to give necessary and sufficient conditions which hold for all occurrences of scientific representation. In Section 1.5, I consider this kind of deflationism in the form put forward by Suárez (2004), which goes under the label of *inferential conception*. In this account, the deflation of the concept is obtained by positing that, at most, only necessary conditions for representation can be given. These conditions roughly amount to the possibility for the model-user to draw inferences about the target of the model, according to the current norms of scientific practice.

In Section 1.6, I discuss the virtues of the inferential conception, also with respect to another deflationary account, i.e., the DDI account of representation advocated by (Hughes, 1997). Moreover, I suggest that the idea of epistemic structure ascription – i.e, the idea that models accomplish their epistemic role by allowing a user to ascribe a certain structure to their target – can be fruitfully integrated into Suárez's inferential conception, to form a sort of *combined account*. Such a suggestion, I hope, might be a development – although marginal – of the inferential conception. This is because the concept of epistemic structure ascription is faithful to the deflationary spirit of the inferential conception. In particular, it allows to strengthen one of the necessary conditions for representation set by the inferential account. This condition, I argue, can be fruitfully combined with a structural approach to the epistemic task of models and, hence, to scientific representation. Finally, Section 1.7 provides some concluding remarks.

Chapter 2 examines the relationship between the semantic view of scientific theories and scientific representation. The main focus is on two criticisms that are raised against the semantic view because of its focus on structures. I argue that the semantic view, at least in its early formulation, is immune to these criticisms. To this purpose, I first review what the early advocates of the semantic view posited about the relationship between theories and scientific representation, with particular emphasis on some aspects that are often overlooked – if not misunderstood. Then, I apply these posits to show that the semantic view is not a legitimate target for the criticisms considered in the chapter.

The semantic view has gained its orthodoxy status in contrast to the syntactic view. Such a contrast is then useful to understand the fundamental tenets of the semantic view. However, the contrast is often sketched in a way that, for sake of arguments and clarity, blurs some aspects of the semantic view which, I argue, turn out to be crucial to evaluate its contribution to the issue of representation. In my reconstruction of the semantic view (Section 2.2) I aim at recovering these aspects and, to this end, I proceed in three steps.

First, I examine the approach of the semantic view to the formalization of theories (subsection 2.2.1), showing that the view aims at providing a rational reconstruction which is fundamentally structural – i.e., based on models as structures and on the structural relationship between models – and "modest", in the sense that it does not claim that such formalization is the only possible one. Second, I illustrate the overall scope of the view, showing that the view acknowledges the relevance of actual scientific practice (subsection 2.2.2) and that it even looks at the actual practice to test the goodness-of-fit of the reconstruction thus provided. Third, building on the previous steps, I argue that the semantic view – at least in its early formulation – seems to be neutral with respect to both realism and antirealism and that it does not necessarily imply any particular ontological stance (subsection 2.2.3).

I then turn my attention to two criticisms against the semantic view which focus on its implications for scientific representation. The first criticism rules out the possibility for the semantic view to account for representation (Section 2.3). I focus on two recent contributions which I consider as instances of this criticism, i.e., Le Bihan (2012) (subsection 2.3.1) and Brading and Landry (2006) (subsection 2.3.2). In both these papers, representation is cashed out in terms of the hierarchy of models put forward by Suppes (1962), i.e., representation is intended as the relationship between the last layer of the hierarchy, at which we find data models, and the target system. The two contributions share the assumption that the semantic view is eminently a program of analysis of the structure of theories, and they both reach the conclusion that, within such a program, representation can not be accommodated. The second criticism is more subtle, as it concedes that the semantic view could be an account of representation, but only at the price of implying a form of structuralism about representation (Section 2.4). Such criticism is seldom made explicit and it seems to imply the idea that the semantic view is a hospitable framework for structural realism.

My arguments in defense of the semantic view follow from the analysis of the view that I have provided in the first part of the chapter. With respect to the first criticism I claim that, although I share with both Le Bihan (2012) and Brading and Landry (2006) the idea that the semantic view is eminently a rational reconstruction of scientific theories, we reach opposite conclusions about the possibility for the semantic view to account for representation. I argue that their conclusion, i.e., that the semantic view can not offer an account of representation, relies on assumptions which are unwarrantedly too strong.

Le Bihan (2012) assumes that a necessary condition for the semantic view to account for representation is that it must be able to tell us when the procedure followed by a scientist to construct a data model is good or bad for a given phenomenon (subsection 2.3.1). In the light of the discussion in subsection 2.2.2, I argue that the semantic view is capable of this kind of evaluation, but only in two weak senses. First, the semantic view can establish if the procedure is consistent with the theory of experimental design. Second, it can evaluate whether the procedure yields a data model that is potentially – but not necessarily – morphic to the theory's model. The semantic view can assess the procedure that leads to the construction of a data model in no more specific senses. However, this suffices to account for scientific representation in the sense expressed in Le Bihan (2012) by the concept of "functional adequacy".

Brading and Landry (2006) assume that representation can not be accounted for unless we somehow impose a structure on phenomena, either by identifying phenomena with their associated data models, or by directly assuming that phenomena come in structures. Since they (correctly) argue that the semantic view is silent on the structure of target phenomena, they conclude that the view can not account for scientific representation. My counter-argument is that an account of scientific representation need not be framed exclusively in structural terms – as Brading and Landry seem to believe – and that the semantic view does not appeal solely to a series of structural relationships to account for representation. More precisely, my point here is that a rational reconstruction is an account of representation that tells us how data models latch to their target phenomena, but it does so without dealing with any specific component of any particular target phenomenon. A rational reconstruction can be an account of scientific representation in Le Bihan's sense of functional-adequacy without any structuralist commitment.

Explaining why data models are built in the way they are actually built sheds light on how models latch to their target phenomena, at least in the weak sense of rationalizing the behavior of scientists and their actual practices.

With respect to the second criticism I argue that it is both historically misleading and unwarranted. The early advocates of the view, as clarified in the discussion of neutrality in subsection 2.2.3, did make it very explicit that the view should not aim at being a framework for developing epistemic stances. In subsections 2.2.1 and 2.2.2, I argue that the second criticism is unwarranted. In fact, the semantic view can account for representation – in the sense of a rational reconstruction of it – without requiring any ontological commitment. Indeed, to assume that reality is already in structural form (ready for the scientist to be translated into a data model) would mean to commit to structural realism. However, the semantic view does not make such an assumption. Instead, it assumes that the scientist is forced by the way in which theories are formulated (and, more precisely, by the hierarchy of models), to extract information from the phenomenon and to put it in the structural form which is typical of the data model. Finally, Section 2.5 sums up the main points of both the criticisms and my counterarguments.

Chapter 3 analyses an instance of the so-called structural approaches to scientific representation, namely Bartels' homomorphic theory of representation (Bartels, 2006). The hallmark of structural approaches is to present structural properties of models, such as morphisms, as necessary and sufficient condition to have representation. In this chapter, which is based on a joint work with Mauricio Suárez, we show that, contrary to Bartels' claim, his account can not accommodate *misrepresentation* and, hence, it can not account for representation *tout court*. This leads us to conclude that if the concept of structure is isolated from all the pragmatic aspects of representation, as structural approaches require, then any account which relies on such a concept can not accommodate representation.

In Section 3.2, we review the concept of misrepresentation in scientific modelling, in both the mistargetting and inaccuracy varieties. We consider the influential historical case of the billiard ball model of gases, which we use to identify the ways in which inaccuracy obtains. We then argue that Bartels' account fails to accommodate both the forms of misrepresentation.

In Section 3.3 we show that Bartels' homomorphism theory can not account for mistargetting, as it is presented in (Suárez, 2003). We consider Bartels' homomorphism theory of representation, pointing out the essential role played in Bartels' account by what he calls a "representational mechanism", whose role is to pick a target for a given model out of the set of its possible targets. Since a representational mechanisms is crucial for misrepresentation to occur in the form of mistargetting, but it is independent of any structural mapping, we argue that (mis)representation is not accounted fully in structural terms.

In Section 3.4, we show that Bartels' theory can not account for inaccuracy, that we define by introducing three ways in which models can inaccurately represent: abstracting, pretending, and simulating. We argue that all scientific models abstract, many pretend, and some simulate and that, despite their inaccuracy, models preserve their descriptive, predictive, and explanatory value. We show that even the weakest notion of structural morphism is too strong for jointly accommodating abstracting, pretending, and simulating. Therefore, we conclude that Bartels' account can not accommodate misrepresentation as inaccuracy.

Finally, in Section 3.5 we draw the moral that greater care should be taken with structural accounts of representation based on the notion of morphism: although morphisms may well be needed to assess the accuracy or faithfulness of a scientific model, neither scientific representation, nor actual theorizing, can be successfully reduced to any kind of morphism.

Chapter 1

Reinflating Scientific Representation

1.1 Presenting representation

The issue of scientific representation has arisen as a *philosophical* attempt to systematize the epistemic process of acquiring knowledge on a specific target, and to analyse thoroughly the devices available to carry out such a process. Among these vehicles, apparently, a pivotal role is ascribed by both scientists and philosophers to models. Many philosophers conceive science as a representational activity, that is, as depending on "the possibility of ignoring accidents, of isolating certain key features in a situation. These are captured by models, although in the very act of idealisation or approximation we convince ourselves that the model is indeed false." (Redhead, 1980, p.162). The act of explaining *notwithstanding*, or *in virtue of*, omission and isolation is what makes models crucial for science to be carried out. This is what Cartwright seems to suggest when she claims that "to explain a phenomenon is to construct a model which fits the phenomenon into a theory. The fundamental laws of the theory are true of the objects in the model, and they are used to derive a specific account of how these objects behave. But the objects of the model have only 'the form or appearance of things' and, in a very strong sense, not their 'substance or proper qualities'." (1983, p.17).[1]

As for many issue in philosophy, we can not fix a moment in time when *the* issue of scientific representation arose. We must be content to track down the context which allowed the issue to be set forth. This moment coincides with the rise of the "modeling attitude", a label recently coined by Teller (2008a) and Suárez (2015) to identify the tendency, both in physics and in philosophy, to consider models among the main means to pursue scientific theorizing.[2]

The modelling attitude finds its prehistory in the so called *Bildtheorie*, a view mainly developed throughout the work of Boltzmann ([1974] 1902,4), Hertz (1899) and usually extended to the work of Maxwell ([1990] 1856).[3] *Bildtheorie* mainly amounts to a claim about the aim of science, which is defined as "the ubiquitous

[1]Besides the works by Redhead and Cartwright just mentioned, other contributions subscribing this picture of science as representational are Cartwright (1999a), Giere (1988, 1999, 2004), Hughes (1997), Morgan and Morrison (1999), Redhead (2001), van Fraassen (1997, 2008). In particular, to stress the representational role of models are Hughes (1997), Giere (1999), Teller (2001), Suárez (2002), Bailer-Jones (2003). On the other hand, Hacking (1983) famously turns down this picture.

[2]An alternative account of the rise of the "modelling attitude" is presented by Bailer-Jones (1999). Bailer-Jones lets the modelling attitude begin with a phase that she labels "from disregard [of models] to popularity" (ibid, p.24). This is the phase when Duhem (1954), "against his own interest" (Bailer-Jones, 1999, p.25) brings to the fore the work of models in scientific theorizing. Indeed, in *The Aim and Structure of Physical Theory* (1954) Duhem draws the famous distinction between the 'Continental-schooled' and the 'English-schooled' physicists' view on theory construction. The French or German physicist will avoid any imaginative effort and she justifies her hypotheses by treading the path of the "algebraic development" *via* deduction which starts exactly from the attachment of physical attributes (magnitudes) to the objects of the theory (ibid., p.78). On the other hand, for the English physicist, understanding a phenomenon is "the same thing as designing a model imitating the phenomenon; whence the nature of the material things will be understood by imagining a mechanism whose performance will represent and simulate the properties of bodies" (p.72). In so doing, Bailer-Jones claims, Duhem implicitly and unintentionally frames the need for models to supplement theories for explanatory purposes.

[3]Blackmore (1999) and de Regt (1999) provide accurate analyses of Boltzmann's *Bildtheorie*, with a stress on his approach on explanation and understanding. They also draw interesting comparisons between Boltzmann's, Hertz's and Maxwell's views. A thorough analysis on the role of models, analogies and metaphors in Maxwell's work is provided by Peruzzi (2010). A reconstruction of the *Bildtheorie*'s development is provided by van Fraassen (2008, ch.8, pp. 191-204), with a stress on the *Bildtheorie*'s implications for the realism-antirealism debate. In this section, I do not need to go into details concerning the differences among *Bildtheorie*'s formulations and I only aim at providing an overview.

task [...] to explain the more complex in terms of the simpler; or, if preferred, to represent [*anschaulich darstellen*] the complex by means of clear pictures [*Bilder*] borrowed from the sphere of the simpler phenomena." (Boltzmann, [1974] 1905, p.149).

As is well known, *Bildtheorie* presents theories as mental constructions of models, "the working of which we make plain to ourselves by the analogy of mechanisms we hold in our hands and which have so much in common with natural phenomena as to help our comprehension of the latter." (Boltzmann, [1974] 1902, p.790). Thus, as Hertz (1899) stresses, on the basis of the analogy between our "inner pictures" and the "external objects", we are allowed to draw inferences from the former on the latter: "the necessary consequences of the pictures in thought [*Bilder*] are always the pictures [*Bilder*] of the necessary consequences in nature of the things pictured." (1899, p.2).

What emerges as relevant from this early stage of the modelling attitude is the fact that science is not conceived mainly as a *description*, capable of being true or false, of the system under inquiry. Science rather proceeds by building pictures of such systems. Secondly, the relationship between these pictures, *models*, and their target systems is representational. Boltzmann couches this relation as an ascription of (mental) pictures with a "definite content" to the thing we want them to stand for, although such an ascription does not imply *complete* similarity since "we can know but little of the resemblance of our thoughts to the things we attach them." (Boltzmann, [1974] 1902, p. 213).

What makes the *Bildtheorie* a candidate forerunner of the current philosophical analysis of the issue of scientific representation is the fact that, although cashing out the concept in terms of mental states, it puts models at the center of the theorizing activity, raising the puzzle of how something which does not mirror the *real* system under inquiry could be capable of providing knowledge about the latter. It is around this puzzle that the question '*in virtue of what do models represent ?*' becomes the core of the issue of scientific representation.

To grasp the origin of the philosophical qualm about representation, consider as an example the well-known model of a simple pendulum. The following equation describes the motion of a "grandfather clock" (Giere, 1988) as the period of a pendulum which is proportional to the square root of its length and independent of its mass:

$$T = 2\pi\sqrt{\frac{l}{g}} \tag{1.1}$$

where T is the time period for one oscillation, l is the pendulum length, and g is the acceleration due to gravity. To get to equation (1.1), we need to start by imagining the following idealization of a real pendulum. Our idealized pendulum consists of a point mass, m, which is attached to an infinitely light rigid rod whose length is l which, in turn, is attached to a frictionless pivot point. In accordance with Newton's Second Law, the equation of motion of the pendulum is the following:

$$ml\frac{\mathrm{d}^2\theta}{\mathrm{d}t^2} = -mg\sin\theta \tag{1.2}$$

where θ represents the angle of swing of the pendulum calculated from the vertical position. Equation (1.2) is further simplified by assuming that the angle of oscillation is very small, so that $\sin\theta \approx \theta$. The modified equation of motion has the following form:

$$\frac{\mathrm{d}^2\theta}{\mathrm{d}t^2} + \frac{g}{l}\theta = 0 \tag{1.3}$$

The solution to (1.3) is:

$$\theta = \theta_0 \sin\left(\sqrt{\frac{g}{l}}t + \phi_0\right) \tag{1.4}$$

where θ_0 is the angle of swing, and ϕ_0 is the initial phase angle. (The values of θ_0 and ϕ_0 depend on the initial conditions of the motion of the pendulum).

All these equations have the "unhappy feature [that they] do not represent facts" (Cartwright, 1983, p.58). Indeed, in the case of the motion of a pendulum, what is represented by the equation is only an idealized and approximate version of the motion of a grandfather clock. In fact, the latter has not just a point mass, and is not attached to a rod that is infinitely light, and the pivot point does have frictions. In other words, idealization imposes some "deliberate omissions" (Bailer-Jones, 2002) of parameters which are not considered as relevant.[4] Model-users are aware of what is represented by this equation and how – i.e., according to which parameters – it is represented. The selection of parameters is the feature which shows the "imaginative and intuitive element in theoretical physics" (Redhead, 1980, p.162) and makes models "not descriptions of facts, but an invitation to [...] imagine a particular situation" (Frigg, 2010, p.260).

The philosopher who aims at understanding the source of the explanatory power of models has nowadays the possibility to choose among, mainly, two conflicting stances, recently labelled by Chakravartty (2010) as the *functional* and the *informational* approaches to scientific representation. Informational approaches tend to present representation as a dyadic relation between the model and its target system, emphasizing the "objective relations" holding between the two, such as similarity, isomorphism or other kinds of morphisms. The relations are defined as objective in the sense of "mind-independency": what makes a model representational pertains to its 'ontology', i.e., to its features and properties.[5] Functional approaches rather emphasize the functional character of representation. As stressed by van Fraassen (2008), representation is a *function* of models and, as such, it is not operative unless it is ascribed or recognized by a model-user. The role played by model-users in such an ascription and recognition is then crucial to have representation.

[4]Idealization here is intended as "Aristotelian idealization" (Frigg and Hartmann, 2012), or "abstraction" (Cartwright, 1989), that is, as the act of "stripping away" those properties of a concrete object that are not considered as relevant for the problem at stake.

[5]The term "ontology" as applied to models is used here in the loose sense suggested by Frigg (2002, 2006), who defined as an "ontological puzzle" the question concerning 'what kinds of objects are models' (Frigg, 2006, p.50).

Informational approaches are also known as "structural" since they rely on the notion of morphism (such as isomorphism, homomorphism, partial isomorphism, etc.) to justify representation: a model represents its target system if and only if the former is isomorphic (or morphic to some degree) to the latter (Bartels, 2006, Bueno and French, 2011, Bueno et al., 2002, da Costa and French, 1990, 2003, French and Ladyman, 1999).[6] According to these approaches, the representational power of a model is a property of the system, and such a property determines the necessary and sufficient condition for representation to occur. As the condition (the morphism) has been identified, we do not need to appeal to model-users' intentions, purposes or representational conventions, to account for representation.

Functional approaches, on the other hand, aim "to provide a space for [scientists'] purposes" (Giere, 2004, p.743) in any account of representation. The function of something is defined by its use: something is used in order to fulfill a certain task and such fulfillment is its function. In the case of models, their task is to represent target systems. It is distinctive of the source of representation in general, and of scientific representation in particular, 'not to own' its functionality. In other words, a model does not represent *per se*: the representational function is always ascribed by and recognized by a model-user. Therefore the representational function of a model does not occur unless the users, with their intentions, purposes and representational practice, are part and parcel of the account. Bas van Fraassen couches the core claim of functional approaches in what he defines the *Hauptsatz* of a theory of representation: "There is no representation except in the sense that some things are used, made or taken, to represent a thing as thus or so." (van Fraassen, 2008, p.23)[7]

[6]The label "structural" is employed by Frigg (2002, 2006), and it includes, if not reduces to, the semantic view of scientific theories. Since the next chapter is devoted to give reasons to write the semantic view off Frigg's list, I limit myself here to mention those approaches which actually deserve such a label – and which use the label themselves to dub their accounts.

[7]The original formulation of van Fraassen's *Hauptsatz* is due to Giere (2004, p.743).

1.2 Twilight of the philosophical analysis?

Given its manifest relevance, the issue of scientific representation is nowadays a booming topic in philosophy of science. However, it is far from being settled. This is due to the fact that there is no agreement among philosophers either on the ontology of models, or on the possible relationship between models and their target system.[8]

This lack of agreement might be quite of an impasse for attempting a philosophical analysis of the issue. As stressed by McMullin (1968), the task for a philosophical analysis of models and, more in general, for the analysis of representation in science, should be to provide a reconceptualization, or a rational reconstruction, of the key terms of the issue (in this case: model, theory, representation). McMullin also emphasizes that this task is rather complicated given that we can not consider these key terms as they are employed by scientists, since scientists use them in a "vague and often inconsistent manner that would play havoc" (1968, p.386). On the other hand, McMullin argues, scientists' vagueness does not allow philosophers to opt for "purely stipulative definitions". What we have to find is rather a "half-way house between these lexical and stipulative extremes." (ibid.). So, McMullin points out that:

> [W]hen two philosophers disagree as to "what a model is", a rather frequent disagreement recently, this is not like two scientists disagreeing as to "what a meson is" or two lexicographers disagreeing as to how the word 'model' is actually used in some language group. *The criteria for setting this sort of disagreement are not purely empirical* – we cannot go out and find a model and start observing its behavior – nor are they purely lexical, since linguistic usage is not sufficiently definite nor sufficiently well correlated with actual scientific practice, as a rule, to make sharp decision of this sort possible. The criteria are [...] *partially empirical*, in that one must scrutinize the actual

[8] Weisberg (2007) and Godfrey-Smith (2006) even question whether a 'representational relationship' in fact holds between a model and a target system. In particular, Weisberg argues that modeling is a form of *indirect* representation: the target for a model is not a 'real' system but an idealized replica of the latter. A *direct* representational relation then holds only among the outcomes of what he defines as "direct abstract modelling" and the target system.

> epistemological structures inherent in current scientific practice and try to find out a conceptual system which will articulate as closely as possible with this practice. [...] In the last analysis, when two philosophers disagree as to "what a physical model is", it may be assumed that each has isolated some element in scientific practice that he considers important and has attached to it the label 'model', a label which in ordinary usage may be only loosely correlated with this element. (emphasis mine, 1968, pp.386-87)

So, according to McMullin the task of the philosophical analysis of "model" and "representation" is partly conceptual and partly empirical. The conceptual side has the aim of making the analysis as widely applicable as possible. The empirical side is a test-bed for the validity of such conceptual analysis.

However, as convincingly stressed by Hughes (1997), both the concept of representation and the concept of models – to which representation seems to be tightly tied – are "slippery" (ibid., p.325) and tricky to define. In the wake of Hughes' disillusion that the concepts of "model" and "representation" will ever be given a satisfactory definition, the general trend in current philosophy is, as puzzling as it may sound, to acknowledge the relevance of the issue of representation and, concurrently, to question its tractability. In other words, the trend is to *deflate* the issue of scientific representation.

Indeed, models can be either abstract or concrete entities and, consequently, the "problem of style" – i.e., the problem due to the different ways in which models represent according to their particular ontology – poses a conundrum for any account for representation (see Frigg, 2006, sect. 2).[9] An abstract model, such as the Bohr model or the simple harmonic oscillator, does *in practice* represent its

[9] For a taxonomy of the types of models proliferating in the philosophical literature, see Frigg and Hartmann (2012, sect.1). The following is a list with just a few of the models mentioned by the authors: phenomenological models, computational models, developmental models, explanatory models, impoverished models, testing models, idealized models, theoretical models, scale models, heuristic models, caricature models, didactic models, fantasy models, toy models, imaginary models, mathematical models, iconic models, analogue models. However, as Frigg and Hartmann points out: "Each of these notions is still somewhat vague, suffers from internal problems, and much work needs to be done to tighten them. [...] What we need is a systematic account of the different ways in which models can relate to reality and of how these ways compare to each other" (Sect. 1.1).

target system (respectively, an atom or the motion of, e.g., an oscillating spring) in a different way than a material model, such as Watson and Crick's metal model of DNA.

Moreover, models can be constructed either dependently on theories or independently of them. As Morrison (1999) and Frigg and Hartmann (2012) point out, the liquid drop model of the atomic nucleus ascribes to the nucleus properties (surface tension and charge) determined by appealing to different theories (respectively, hydrodynamics and electrodynamics). Analogously, Cartwright et al. (1995) argue that the London model of superconductivity, although grounded on classical electromagnetic theory, was not "contained" by the theory in a relevant sense, since the equation in the model had no justification in the theory and was motivated on the basis of purely phenomenological considerations. Many contributions in the volume by Morgan and Morrison (1999) aim at stressing the (at least partial) independence of models from theories and data, due to the weight of "additional 'outside' elements" (ibid., p. 11).[10]

In light of these differences, the relationship between models as *representans* and their targets as *representanda* can not be univocally presented. The possibility for philosophy of science to provide a rational reconstruction, that is, a neat and tidy account of what models are and in virtue of what they represent is increasingly dropped in favor of 'particularistic analyses' based on specific disciplines, where particular representational devices are used, and specific ways of gaining knowledge through such devices are thus laid bare (e.g., Godfrey-Smith, 2006, Knuuttila and Boon, 2011, Vorms, 2013).

The trends raised by 'particularistic analyses' certainly enriches the philosophical analysis. However, I wonder whether dropping the attempt of a rational reconstruction would not lead us to conceive philosophy of science as a mere description of the scientific activity and, in particular, of modelling in science, rather than as a proper elucidation of this very intricate practice. Just to avoid misunderstandings, I am not claiming here that we should opt for a mere rational reconstruction

[10]In Morrison and Morgan's volume, Boumans' essay (1999) – where Boumans examines business-cycle – provides an overview of these "outside ingredients" among which he counts mathematical techniques, analogies, metaphors, relevant policy views.

devoid of any link with actual scientific practice. Scientific practice is rather the constant referral to test our rational reconstruction. I am only skeptical that injecting as many details as possible into our philosophical analysis by looking at particular cases is the only way to better understand how modelling works and that such understanding would not be possible otherwise.

The challenge for me in this chapter is then to see if and to what extent this general tendency to deflate the issue of scientific representation can be accepted without renouncing to the kind of general philosophical analysis advocated by McMullin (1968). For this purpose, I consider two forms of deflationism concerning scientific representation, which have shaped the literature about the topic in the recent years. The first form of deflationism is quite detrimental for the issue insofar as it concerns the very *problem* of scientific representation. The assumption grounding deflationism about the problem of representation is that scientific representation has no special status and, as any other form of representation, it works mainly by stipulation. The second form of deflationism is more subtle and concerns the *concept* of scientific representation.

1.3 Deflationism about the *problem* of scientific representation

What philosophers find *special* about *scientific* representation is that the main tools of scientific theories, i.e., models, are informative about a target system *notwithstanding* the fact that they "commit sins of omission and commission by lacking and having features the world does and does not have" (Callender and Cohen, 2006, p.67). As in the example of the simple pendulum in Section 1.1, the omission of features actually characterizing the grandfather clock do not prevent from, rather they allow for, the former to represent the latter. So, if one wants to sort out *the* appropriate formulation of the problem of scientific representation, it would be the following: 'how do models represent despite the fact that they misrepresent?'.

The deflationism put forward by Callender and Cohen (2006) questions such a *special* status that philosophers tend to ascribe to *scientific* representation with respect to other forms of representation (e.g., artistic, literary, etc.). Callender and Cohen state this quite clearly from the very title of their famous paper: *There is no special problem about scientific representation*. In particular, they argue that the philosophical qualm about anything the problem of scientific representation could be reduced to is pretty much unjustified. So they claim that "while there may be outstanding issues about representation, there is no special problem about scientific representation" (ibid., p.77).

The deflation of the *problem* by Callender and Cohen is based on what they call *General Griceanism*, i.e., the assumption that all forms of representation – including scientific representation – can be reduced to just one fundamental form of representation, that is, stipulation allowed by individuals' mental states.[11] According to General Griceanism, the fundamental representation obtains by virtue of (i) the *mental state* of a given individual that is in a representational relation with a given object, together with (ii) a *stipulation* that confers upon the representant (whatever it is) the representational properties of that mental state.

By resorting to General Griceanism, Callender and Cohen assume that any form of representation is *derivative* with respect to the fundamental representation given by mental states. So, they can conclude that "once one has paid the admittedly hefty one-time fee of supplying a metaphysics of representation for mental states, further instances of representation become extremely cheap." (ibid., p.74). In other words, their proposal is to employ General Griceanism to "solve or dissolve" (ibid., p.67) the problem of scientific representation and to leave the remaining puzzles to philosophers of mind:

[11] The argument by Callender and Cohen actually proceeds in two steps. First, they argue that philosophers tend to merge into the question of 'how do models represent despite the fact that they misrepresent?' three different questions (constitution, demarcation and normative. See fn. 12) which should rather be sharply distinguished. Second, once the questions involved are singled out, they call upon *General Griceanism* and argue that all the three questions dissolve – and become pragmatic issues – and, consequently, the problem of scientific representation disappears.

> [S]cientific representation is just another species of derivative representation to which the General Gricean account is straightforwardly applicable. This means that, while there may be outstanding issues about *representation*, there is no special problem about *scientific* representation. (ibid., p.77)

Turning to scientific representation, Callender and Cohen claim that models are just the typical representational vehicles used in science, and that they represent their targets "by virtue of the mental states of their makers/users" together with a stipulation. More precisely, all that is required for a model to represent its target is that a model-user has the appropriate mental state (condition (i) of General Griceanism) and that the same user *stipulates* that the model does actually represent its target, trying to convey to the community (e.g., of fellow scientists) her belief (condition (ii) of general Griceanism). Given such posits, any general puzzle about (mis)representation is dissolved.[12,13]

It should be emphasized that the adoption of General Griceanism leads Callender and Cohen to reframe any distinction across the different forms of representation – which derive all from the fundamental form – exclusively in pragmatic terms. The use of one object or another as the representant in a given representational relationship is entirely based on utility, always intended in pragmatic terms. The choice of the representant made by the user depends only on the practical easiness of stipulation which, in turn, depends only on pragmatic (that is, cultural, social, historical, physical, etc.) elements.

[12] This framework also leads to the dissolution of the three questions that Callender and Cohen claim to be generally at stake when philosophers deal with the problem of scientific representation (and that often are conflated into the question 'how do models represent despite the fact that they misrepresent?'). The first question is on *constitution*: 'what does constitute the representational relation between the model and its target?' The second question is *normative*: 'what is for representation to be correct?', where the correctness is to be interpreted as 'being explanatory'. The third question is a *demarcation* question: 'how to distinguish scientific representations from non-scientific ones?'. See also footnote 13.

[13] As examples of answers to the constitution question, Callender and Cohen consider Giere's account of similarity (Giere, 1988), French's notion of partial isomorphism (French, 2003), and Suárez's inferential account (Suárez, 2004). According to Callender and Cohen, these accounts are attempts to identify necessary conditions for models to represent. However, while the identified conditions do serve the role of relating models to targets, as well as the role of "pragmatic aids" to recognize a representational relation, they are just "independent pragmatic constraints that may work together or separate to guide choices between scientific representations." (Callender and Cohen, 2006, p.78).

Hence, according to Callender and Cohen, virtually anything can serve as a vehicle for scientific representation.[14] The only distinction that can be made across potential representants is the degree of pragmatic utility that they entail for a particular representational purpose, i.e., how easily they allow stipulation. So, models are the typical vehicle of scientific representation only because they turn out to be more suitable for the stipulation to obtain (condition (ii) of General Griceanism). This idea is particularly evident in Callender and Cohen's analysis of the role played by similarity and morphisms within the representational practice:

> [W]e suggest that, while resemblance, isomorphism, partial isomorphism, and the like are unnecessary for scientific representation, they have important pragmatic roles to play; namely, they can (but need not) serve as pragmatic aids to communication about one's choice of representational vehicle. (ibid., p.76)

1.4 The pragmatics of structure ascription

The deflationism propounded by Callender and Cohen, if tenable, would be quite detrimental for the issue of scientific representation, in the trivial sense that it would make several contributions to the topic unwarranted. Nonetheless, few attempts have been made by philosophers interested in the problem of representation to resist this kind of deflationism. In this section I will briefly consider two explicit

[14] Teller could then be considered a deflationist à la Callender and Cohen, since in his (2001) paper he reaches exactly the same conclusion: "I take the stand that, in principle, anything can be a model, and that what makes a thing a model is the fact that it is regarded or used as a representation of something by the model users. Thus in saying what a model is the weight is shifted to the problem of understanding the nature of representation." (p. 397).

attempts in this direction: Toon (2012) and Liu (2015).[15] For reasons which will be made clear shortly, I claim that both the attempts point in the right direction, but are not ultimately effective. I then put forward a way to improve those attempts by resorting to a pragmatic use of the notion of structure.

Toon argues that assuming the General Gricean framework – according to which, just to recall Callender and Cohen's point, "scientific representation [...] is constituted in terms of a stipulation, together with an underlying theory of representation for mental states" (Callender and Cohen, 2006, p.78) – does not assure a univocal account of fundamental representation based on mental states, nor that stipulation is a sufficient condition for representation whenever an individual has the appropriate mental state.

Toon makes his point by focusing on *depiction*, namely a particular form of representation (i.e., "pictorial representation") which, on the basis of General Griceanism, should be as derivative as scientific representation. In order to show that it is not clear which mental states are to be considered responsible for condition (i) to hold, Toon considers two accounts of depiction, each of which justifies pictorial representation in terms of mental states.[16] On the first account, the mental state fulfilling condition (i) is the artist's intention to create a certain visual

[15] Suárez (2004) and Contessa (2007) provide, so to speak, *ante litteram* replies to Callender and Cohen's argument. Suárez, in particular, stresses that arbitrary stipulation by an agent is not sufficient for scientific representation. However, since Callender and Cohen's deflationism is not their target, I can not count Suárez and Contessa among the few attempts to resist the deflationism at stake. Thomson-Jones outlines a reply which he himself defines as the "*beginnings of* a response to Callender and Cohen", which amounts just to the remark that " it is nonetheless entirely possible that given the sorts of representational task we are attempting in the sciences, and given the various practical constraints at work, there are some particular ways of representing that *de facto* predominate in the sciences – some characteristic kinds of representational vehicle which are employed, for example. So there can be a special question about how representation works in the sciences. That question may well be worth asking, and answering, furthermore, for it may be that articulating an account of the specific ways of representing which predominate in the sciences will aid us in our attempts to understand scientific explanation, theory testing, modelling, and the various other aspects of scientific practice which concern us in the philosophy of science" (2011, p.138). The point I am making at the end of this section is sympathetic to Thomson-Jone's, and, I hope, a bit more detailed.

[16] The accounts considered by Toon are, respectively, Goldman's (2003) and Walton's (1990). It is not necessary to go into the details regarding the formulations of these accounts in order to illustrate Toon's argument.

experience of an object. On the second account, the mental state fulfilling condition (i) is given by the particular imaginative act in which the viewer of the picture is engaged. So, we can reasonably think of two distinct classes of mental states that could act as bases for the fundamental representation from which pictorial representation allegedly derives: the mental state of the painter and the mental state of the viewer of the painting. This shows that General Griceanism requires, at least, a more precise specification of who is the referent for the relevant mental state.[17]

The point about the untenability of condition (ii), i.e., of the insufficiency of stipulation, is made by Toon using the following example which, again, is about pictorial representation. Suppose we take a blank canvas, and suppose that condition (i) is satisfied by our current mental state. Then, suppose that we stipulate that the blank canvas represents Napoleon and that we manage to convince our audience that the canvas represents Napoleon. This situation perfectly fits Callender and Cohen's idea that an act of stipulation, as long as it is agreed upon, is sufficient to establish a particular form of representation.[18] However, even accepting that stipulation can be agreed upon for whatever object used as representant, if we want to use a blank canvas to represent Napoleon, then it seems fair to say that we can hardly talk of *pictorial* representation. So, there must be some characteristic which is proper of pictorial representation that goes beyond conditions (i) and (ii) of General Griceanism. Such additional characteristic can not be just the pragmatic utility of using a painting when we want to represent by means of visual images, as otherwise we lose any justification to call a form of representation 'pictorial' instead of, e.g., sculptural – not to mention the impossibility to justify the pictorial representation of Napoleon by means of a white canvas.

The reasoning applied to pictorial representation can be applied to scientific representation as well. This allows me to introduce the first steps of my argument.

[17] I do not push the consequences of this argument by Toon against condition (i) any further, since I mean to focus on Toon's argument about the untenability of condition (ii).

[18] The example that Callender and Cohen make is of a salt-shaker used to represent Madagascar: we pick up a salt shaker and stipulate (and inform our audience) that it represents Madagascar. As long as the audience agree with our stipulation, we have an occurrence of representation (Callender and Cohen, 2006, pp.7-10).

Consider the attempt to use the sign 'D.N.A', rather than the Crick and Watson's model of DNA itself, to represent the DNA molecules. Suppose again that condition (i) is satisfied and that we succeed in stipulating that 'D.N.A.' is a representation of DNA molecules. Is this scientific representation? I agree with Toon that it is not, even if an entire audience of biochemists could be persuaded that it is. Roughly speaking, an account of scientific representation should give us information on what is the *special* characteristic of models that makes them the vehicle mostly preferred by scientists to represent. Appealing to pragmatic utility without explaining *why* models are more useful than other vehicles for scientific representation is not a satisfactory *manoeuvre*:

> The particular derivative account assumed by Callender and Cohen, which claims that models represent simply in virtue of an act of stipulation, is clearly inadequate. In fact, their argument simply trades on the ambiguity of the term 'represent'. An act of stipulation may perhaps be sufficient to make it such that a model refers to or denotes some system, and so 'represents' it in some sense of the term. But just as theories of depiction aim to account for the particular forms of representation that pictures provide, so our theory of scientific representation should account for the particular form (or forms) of representation offered by scientific models. Stipulation is not sufficient to establish an instance of this relation. (Toon, 2012, p.33)

According to Toon, models are able to represent their target by "prescribing" us to imagine things in a certain way depending "upon the features of the models", which must be such that the community can imagine what a model prescribes about its target. In fact, the properties of an object chosen as a vehicle of representation by mere stipulation can be of no help to imagine the target's features. This argument, although sensible, is nonetheless wanting in the sense that it does not clarify *how* models should accomplish their prescriptive role in scientific representation.

Liu (2015) presents an argument which is very much in line with the one used by Toon, but which seems to go one step forward in developing a full criticism against Callender and Cohen's deflationism. Liu divides representational vehicles in two categories: *symbolic vehicles* and *epistemic vehicles* (Liu, 2015, p.10). According to Liu, the failure to recognize that scientific models are epistemic vehicles – rather than symbolic vehicles – has led to the misleading view that whatever distinguishes scientific models from other representational vehicles is merely a matter of pragmatics.

Liu explicitly recognizes that in order to argue that a model is an epistemic vehicle we should specify what a model does besides just referring to, or denoting, its target – that is, besides being a symbolic vehicles. Of course, in order to represent its target, a model has to be necessarily secured to it. In fact, this is a task that a model accomplishes by denoting or referring. As in maps the cities and towns are identified by names and labels, so in models the task of securing the representant to its target is usually done by those "symbolic elements" that are attached to the components of the model. Indeed, models typically contain both symbolic elements and structural elements, and there is a sort of division of labour between these two elements. The symbolic elements work to secure the *what* in the representation, while the "modelistic elements" work to show the *like-what*, i.e., what the target structure should be thought like.

So, the epistemic task of a model is to make us think what its targets is like. To accomplish this task, Liu argues, a model must be such that:

1. the connection to its targets is not essentially established by convention;
2. it does not work exclusively as a tag or name to pick out what it represents;
3. it is primarily used to have access to aspects of the target
4. if used to convey information, then its function is to induce the right belief among interlocutors without necessarily appealing to explicit conventions.

Condition (ii) of General Griceanism can not meet requirements 1-4 above. Hence, stipulation can only account for the securing task fulfilled by the symbolic elements of a scientific model, it does not account for the epistemic role of models. This argument is not as wanting as Toon's since it clarifies that the role ascribed to models in scientific representation is crucially epistemic, and that such role is carried out by the structural elements of the model – while the denotational task is accomplished by its symbolic elements. However, Liu's argument is not fully developed as well. Indeed, it does not explain *how* the structural elements actually carry out their epistemic role.

I will now try to take the final step missing in both Toon' and Liu's arguments, that is, to provide an answer to the question *how models pursue their epistemic role*. Preliminarily, the kind of epistemic role that models are meant to have should be specified. I claim that scientific models are built and used for two very specific epistemic tasks: *to explain* and *to predict* the behavior of target phenomena. Despite the wide variety of models used in scientific practice to accomplish these epistemic tasks, I think that it is nonetheless possible to identify a core feature that characterizes how such epistemic tasks can be accomplished – and which therefore characterizes the special status of scientific models. I call this core feature *epistemic structure ascription*, by which I refer to the capability of models to allow a model-user to ascribe a certain structure to a target phenomenon.

My proposal draws on Morrison's suggestion that "[t]he reason models are explanatory is that in representing these systems, they exhibit certain kinds of structural dependencies" (1999, p.63). Morrison does not flesh out her suggestion, but I think that much work in the direction she points to had already been done in the recent past. An important contribution in this regard is the work of McMullin (1978) about the "hypothetical-structural explanation" or, in short, "structural explanation". According to McMullin this kind of explanation is what we typically aim for when we use scientific models, and it obtains when the behavior of a complex entity is explained by alluding to its structure, which is understood as a set of constituent entities, processes and relationships between the entities.

According to McMullin, the structural explanation has a special role in the scientific enquiry since "it goes from effect to cause" (McMullin, 1978, p.145). To go from effect to cause means to ascribe a structure to a certain phenomenon that could explain its observed behavior. Indeed, in a structural explanation:

> [T]he structure is *postulated* to account for the observed properties or behavior of the entity under investigation. Here, the warrant for believing that the entity actually does have this structure is the success of the explanation it enables one to give. The explanation is an hypothetical one, since a different structure might also account for the features to be explained. (ibid, p.139)

The last sentence, in particular, clarifies that no ontological commitment is actually required for a structural explanation. What is required is to *ascribe* the structural elements of the model to the target phenomenon, but only instrumentally, in order to manage its complexity. Epistemic structure ascription hence seems to describe quite satisfactorily *how* the structural elements of a model carry out their epistemic role.

1.5 Deflationism about the *concept* of scientific representation

There is a form of deflationism that does not concern the problem of scientific representation. It is rather about the *concept* of scientific representation. More precisely, I consider here the deflationism propounded by Suárez (2004), which can be regarded to be about the concept of representation in the sense that it questions the possibility to give necessary and sufficient conditions valid for all instances of scientific representation. The concept of representation, it is argued, should be taken as primitive:

> I propose that we adopt from the start a deflationary attitude and strategy towards scientific representation, in analogy to a deflationary or minimalist conception of truth, or contextualist analyses of knowledge. [...] Representation is not the kind of notion that requires a theory to elucidate it: there are no necessary and sufficient conditions for it. We can at best aim to describe its more general features. (Suárez, 2004, pp.770-771)

Such a deflationary account has been named *inferential conception*, as it affirms that the possibility for the model-user to draw inferences about the target of the model is a necessary condition for scientific representation to obtain.

Besides the fact that the inferential conception does not attempt to give sufficient conditions for representation, there is another crucial aspect in which Suárez's deflationary account is different from the account of representation put forward by Callender and Cohen (2006), that is, the inferential conception does acknowledge a special status for *scientific* representation.

In order to place the inferential conception in the literature on scientific representation and to clarify why it is particularly relevant to my analysis, I need in the first place to consider the distinction drawn by Suárez between *substantive* and *deflationary* accounts, as well as the distinction between *reductive* and *non-reductive* accounts.[19]

A substantive account of scientific representation is an account that gives both necessary and sufficient conditions for representation, with such conditions being assumed to hold for all possible instances of representation. In other words, the well-specified set of conditions for representation to obtain are invariant with respect to the users of the model at stake. Suárez summarizes the tenets of a substantive account as follows:

> [A] source A is a representation of a target B *if and only if* A, or some of its parts or properties, constitute a mirror image of B, or some of its parts or properties. A and B are entities occurring in the world as described by science, so a thorough scientific investigation of all the facts about A and B and their relation should thus suffice to settle the matter. This is perhaps best summarized by means of a slogan: "scientific representation is a *factual relation* between entities in the world that can be studied by science". Since the relation of representation is factual it *cannot* involve essential or irreducible judgements on the part of agents.[20] (emphasis mine, 2003, p.226)

[19] Suárez (2015) uses the label *substantial* in place of *substantive*. I keep using the latter, following Suárez (2002, 2003, 2004, 2010).

[20] "Source" is the term chosen by Suárez to denote *any* vehicle of representation.

The substantive account is said to *naturalize* the concept of representation, in the sense that it disregards model-users' judgements and purposes, thus reducing the concept of representation to a dyadic relation holding between the source and the target of representation. Hence, the representational relation turns out to be justifiable only by appealing to intrinsic properties of both the source and the target (Suárez, 2004, p.768). In other words, for an account of representation to be substantive, the envisaged representational relationship must obtain universally between the source and the target, and "it does not in any way answer to the personal purposes, views or interests of the inquirer" (Suárez, 2002, p.4).

On the other hand, a deflationary account does not aim at establishing necessary and sufficient conditions that can be taken to hold for all occurrences of scientific representation. A deflationary account rather justifies the adequacy of the properties of the source to represent the target by appealing to the very fact that the source is used by someone to represent the target. So, the account does provide a criterion of functional adequacy to evaluate the source-target relation within scientific practice.

Suárez (2004, 2015) sharpens the contrast between substantive and deflationary account by appealing to its analogue for the theories of truth. Suárez claims that a substantive approach to truth aims at identifying a kind of relation (e.g., correspondence) between propositions and facts such that a proposition standing in that relation to facts *is* true. Substantive approaches thus allow to explain *why* a proposition is true. Such an explanation can not be attained if the account of truth at stake is deflationary. Indeed, the deflationary account determines the truth of a proposition by appealing to its functional role in the linguistic practice.

Now, it is worth noticing that deflationary accounts may differ in the degree of deflation (of the concept of scientific representation). Indeed, although all deflationary accounts renounce to provide conditions for representation that are both necessary and sufficient, a deflationary account can nonetheless provide necessary conditions. So, a deflationary account can give precise conditions for scientific representation *not* to obtain. The extent to which such conditions require the source, the target, or the relationship between them to satisfy some properties, fixes the degree of deflation which is implied by the account: the more demanding and restrictive the conditions are, the less deflationary – and more substantive – the account is.

Suárez also distinguishes *reductive* from *non-reductive* accounts. An account is reductive if it aims at reducing scientific representation to something else. This can be, for instance, a property of the source-target relationship – e.g., similarity – or some more general theory from which representation is derived – e.g., the theory of mind. As a result, a reductive account moves all crucial issues regarding scientific representation to whatever the latter has been reduced to.

A non-reductive account assumes that representation is irreducible in the sense that it is a primitive concept, hence it can not be understood by appealing to something that implies it, or from which it can derive.[21] With these two distinctions in place, I can now go back to Suárez's deflationary approach and examine the discrepancy between his account and Callender and Cohen's account.

Callender and Cohen's account of representation could be classified as reductive and mildly substantive. It is reductive as it resorts to General Griceanism to figure out a primitive concept to which representation can be reduced, and it is mildly substantive because it gives necessary and sufficient conditions for representation (conditions (i) and (ii) of General Griceanism, see Section 1.3). I dub this account as "mildly substantive" because, although the conditions given by Callender and Cohen are invariant with respect to model-users, they do take the users' judgements and purposes into account, therefore representation is not fully naturalized.

On the other hand, Suárez's *inferential conception* is both deflationary and non-reductive. The fundamental tenet of this account is presented in Suárez (2002, 2004) where it is assumed that a source S represents a target T *only if*:

(Inf1) the representational force of S points to T;

[21] It should be noted that many deflationary accounts are non-reductive and many substantial accounts are reductive. However, this is not the only possible combination. In fact, we can have a substantial account that is non-reductive and a deflationary account that is reductive. For a more detailed classification of existing accounts of scientific representation see Suárez (2015).

(Inf2) S allows an informed and competent agent to draw *specific* inferences on T

Condition (Inf1) secures the model to its target by appealing to the representational force of the source, i.e., to "the capacity of a source to lead a competent and informed user to a consideration of the target" (Suárez, 2004, p.768). Thus (Inf1) comprises denotation, although it is not limited to it. The "representational force" is not univocally interpretable, and aims at covering all the possible meanings of "S represents T". It is in this sense that the inferential conception is a non-reductive account.

Condition (Inf2) introduces the requirements that are specific to *scientific* representation. In so doing, (Inf2) confers to scientific representation the special status that is denied by Callender and Cohen (2006). Indeed, Suárez makes clear that:

> The inferential conception adds a second condition [(Inf2)], which is specifically required for scientific representation. The source must have the capacity to be employed by an informed and competent user to draw valid inferences regarding the target – what is known as "surrogative" reasoning or inference. (Suárez, 2010, p.98)

Inferences in (Inf2) are "specific" in the sense that they must not be the mere result of representation itself, rather they should be built upon the representational relation. So, to use a model of DNA to infer that a given nucleo-base's name is either guanine, adenine, thymine, or cytosine does not qualify the inference as specific, since the fact that the nucleo-base has been assigned one of the four names follows directly from the model denoting the nucleo-base by one of the names among guanine, adenine, thymine, or cytosine. On the other hand, to infer from the same model that the actual composition of two distinct pieces of DNA is the same is a *specific* inference, insofar as it goes beyond the mere information that we get by denotation.

Moreover, the inferences in (Inf2) are not required to lead to correct conclusions about the target. They only need to comply with some representational practice which allows to distinguish between a correct and an incorrect inference. In other words, the legitimacy of the inference does not rely on its being correct. An inference is legitimate if it is in line with the scientific representational practice. This allows Suárez's account to accommodate all kinds of representations.

I close this section by pointing out some relevant differences between the inferential conception advocated by Suárez and another deflationary and non-reductive account: Hughes' (1997) famous DDI account (the acronym is for *denotation*, *demonstration* and *interpretation*). According to Hughes, scientific representation is typically – yet not always – the result of three distinct activity. The first is an act of *denotation* of a target by a model. The second is an act of *demonstration* of some results by appealing to the internal dynamic of a model. The third is an act of *interpretation* of some features of the target as features of the model. Interpretation also involves the trasposition of the conclusions drawn *via* demonstration from the model to its target. The DDI account is a strongly deflationary account since it requires neither that any of the three actions is necessary for representation, nor that they are jointly sufficient for it.

A difference which is worth emphasizing between Hughes' DDI account and Suárez's inferential conception is that the latter is more user-based, in a sense that Suárez makes clear by resorting, again, to the analogy with the theory of truth:

> All deflationary theories of some concept X deny that there is a definition of the concept that explains its use. Redundancy theories [and analogously Hughes' DDI account] deny that X may be defined altogether; use-based theories [and analogously Suárez's inferential conception] admit that X may be defined, and may have possession conditions, but they deny that the use of X is thereby explained. (Suárez, 2015, p.17)

What I think to be a relevant difference between the two accounts is that the inferential conception sets out necessary conditions for representation, while the DDI account does not. Therefore, one could even be tempted to say that Suárez's account is slightly less deflationary than Hughes' account, as the latter gives neither sufficient nor necessary conditions for representation. For reasons that I will give in the next section, this difference makes the inferential account preferable to the DDI account for the sake of my argument.

1.6 A proposal for a combined account

In this section, I discuss the possibility to integrate the concept of *epistemic structure ascription* (that is, the idea that models accomplish their epistemic role by allowing a user to ascribe their structure to a target) into Suárez's inferential account.

As I have argued in Section 1.4, this concept is incompatible with the deflationism about the problem of representation propounded by Callender and Cohen (2006). On the other hand, the concept might fit well into the framework of the inferential conception. In particular, necessary condition (Inf2) is a good starting point for introducing epistemic structure ascription.

First of all, the inferential conception already acknowledges that (Inf2) requires models to have a structural component – although not necessarily expressed in a mathematical form:

> [(Inf2)] certainly requires unravelling, since it brings together several features of the practice of scientific representation. First of all, for the source to have this capacity [to be employed in order to draw valid inferences regarding the target] it needs to be endowed with some *internal structure*: it must be the case that the source can be divided into parts and the relations between the different parts can be outlined. (emphasis mine, Suárez, 2010, p.98)

Furthermore, the necessary "internal structure" of the model turns out to be a crucial ingredient for the surrogative reasoning at the core of the specific inferences indicated in (Inf2):

> [T]he source's parts and relations are in some way *interpreted in terms of* the target's own parts and relations. This is an implicit condition without which surrogative inference would be impossible. (emphasis mine, ibid, p.98)

The interplay between the internal structure of the model and the surrogative reasoning it allows for is nicely captured in the following overview of the inferential conception provided by Suárez:

> [A]ccording to the inferential conception, scientific representation is, unlike linguistic reference, not a matter of arbitrary stipulation by an agent, but requires the correct application of functional cognitive powers (valid reasoning) by means that are objectively appropriate for the tasks at hand (i.e., by models that are inferentially suited to their targets). (Suárez, 2004, p.778)

It is now helpful to introduce the distinction that Suárez makes between *means* and *constituents* of representation (Suárez, 2003, 2010). We say that R is the constituent of representation if, for any source–target pair (S, T), the occurrence of R is a necessary and sufficient condition for S to represent T. On the other hand, we say that R' is a means of the representation of T by S, for some source–target pair (S, T) at time t and in the context C, if some user of S employs R' at time t and in context C to draw inferences about T from S.

This distinction allows me to lay bare the reason why I find appealing the possibility to combine what I called the *epistemic structure ascription* with the inferential conception. Suárez (2003) presents several arguments to show that structural relationships as similarity and isomorphism are not the constituents of scientific representation.[22] I do agree on this point, and it is definitely not my purpose to

[22]These arguments will be illustrated in more details in Chapter 3. The following is just an overview of the arguments, and only the isomorphism case is considered here. The *logical* argument shows that isomorphism and representation do not share the logical properties: while isomorphism is reflexive, symmetric and transitive, representation is non-reflexive, non-symmetric and non-transitive. The *non- sufficiency* and *non-necessity* arguments show that representation may fail to obtain when isomorphism holds (non-sufficiency), and may obtain when isomorphism does not (non-necessity). Finally the *misrepresentation argument* appeals to fact that inaccuracy is intrinsic to scientific representation, while isomorphism seems to leave no room for either incomplete or incorrect representation.

argue that epistemic structure ascription is a constituent of scientific representation.

Suárez (2004) argues that (iso)morphism, similarity, instantiation, truth, and stipulation are among the most common means of scientific representation. However, Suárez stresses that none of them is universally employed and, therefore, none of them constitutes a necessary condition for representation. I agree on this point as well, for the vey same reasons put forward by Suárez and presented in the "argument from varieties" (Suárez, 2003). These reasons mainly concern the fact that, although those constituents are widely employed in science, "neither one, on its own, covers even nearly the whole range [of the possible means of representation]." (ibid., p. 231).

Once made explicit that I am not privileging any constituent of representation, nor am I holding that either (iso)morphism or similarity are universal means of representation, I shall illustrate the role of the epistemic structure ascription. In the inferential conception, condition (Inf2) is both specific to and necessary for scientific representation. A closer look at (Inf2) reveals that this condition sets at least two further requirements:

(Inf2.1) the model (source) has an internal structure, (i.e, "[it] can be divided into parts and the relations between the different parts can be outlined" (Suárez, 2010, p.98));

(Inf2.2) the internal structure of the model (source) is interpreted in terms of the target's internal structure, (i.e, "the source's parts and relations are in some way interpreted in terms of the target's own parts and relations" (Suárez, 2010, p.98))

Since (Inf2) is a necessary condition for scientific representation and both (Inf2.1) and (Inf2.2) are necessary conditions for (Inf2), it follows that conditions (Inf2.1) and (Inf2.2) are individually necessary conditions for scientific representation. Two issues emerge from this conclusion. The first concerns what does it mean to interpret "a structure in terms of another structure". The second issue concerns the fact that condition (Inf2.2) seems to require the target to possess its own internal structure, otherwise the internal structure of the model could not be interpreted in terms of the target's 'parts and relations'.

I suggest that, by resorting to epistemic structure ascription, both issues are solved. Such an accomplishment does not undermine the deflationary character of the inferential conception – although a special role for the concept of structure and for structural relationships is acknowledged. If we assume that the model-user takes the internal structure of the model – posited by (Inf2.1) – and ascribes it to the target, we do not need to assume that the target has its own internal structure and, moreover, we end up with a specification of what it means to interpret a structure in terms of another.

But what does it mean, in practice, to ascribe a structure to a target? Drawing on what argued so far, it means that a model-user considers the internal structure of a model that she (or a former competent agent) has identified as a good approximation of the target. Here the "goodness" of the approximation depends on the representational practice at stake – the very practice that determines whether an inference drawn from a model is correct.

This suggestion, I think, constitutes a development – maybe just a marginal one – of the inferential conception. As such, it is faithful to the deflationary spirit of the inferential conception and it strengthens its condition (Inf2) by explicitly combining it with a structural interpretation of the epistemic task that models accomplish in scientific representation.

1.7 Representation reinflated?

In this chapter I have presented the issue of scientific representation, with a focus on the recent tendency in the relevant literature to deflate the issue. The main aim of the chapter was to examine if and to what extent this tendency could be subscribed without renouncing the kind of general philosophical analysis advocated by McMullin (1968). In particular, I have examined whether the concept of structure could play any role in such an analysis. To this purpose, I have considered two different forms of deflationism about scientific representation.

First, I have considered the form of deflationism propounded by Callender and Cohen (2006), which questions the fact that *scientific* representation is a special form of representation worth of philosophical analysis. On this account, the so called *General Griceanism* is invoked to reduce all forms of representation to a fundamental form which is determined by mental states and fixed by stipulation.

Second, I have considered the form of deflationism propounded by Suárez (2004), which questions the possibility to give necessary and sufficient conditions valid for all instances of scientific representation. On this account, known as *inferential conception*, only necessary conditions for representation are given, which roughly amount to the possibility for the model-user to draw inferences about the target of the model – according to the actual norms of scientific practice.

Following Toon (2012) and Liu (2015), I have rejected the first form of deflationism as it does not account for the epistemic role of models. Furthermore, I have suggested that scientific models are built and used for two very specific epistemic tasks (i.e., *to explain* and *to predict* the behavior of target phenomena), and that it is possible to identify a core feature that characterizes how such epistemic tasks can be accomplished by a model-user, i.e., the capability of models to allow their users to ascribe a certain structure to a target phenomenon. I have dubbed this feature *epistemic structure ascription*. This proposal is a completion of, and consistent with, Toon's (2012) and Liu's (2015) contributions.

Finally, I have suggested that the idea of epistemic structure ascription could be fruitfully integrated into Suárez's inferential conception. By positing that the epistemic task of models is accomplished when a model-user ascribes the internal structure of the model to its target, we strengthen the necessary conditions for representation that the inferential conception sets forth, while remaining faithful to its deflationary spirit.

Chapter 2

Semantic View and Scientific Representation

2.1 The relevance of the relationship

References to the semantic view are ubiquitous in the literature about scientific representation after the Sixties. The connection between the semantic view and representation is nicely outlined in this remark by Frigg: "if we wish to learn from a model about the world [...] we are committed to the claim that the model involves some sort of representation. This raises the question of how models manage to represent and of what kind of things they are. The currently most influential answer to this question has been given within the context of the so-called semantic [...] view of theories" (2002, p.2).

There are important criticisms against the semantic view whose goal is to write off the import of the view for the issue of scientific representation. In this chapter I consider two of these criticisms. The first criticism questions the possibility for the analysis provided by the semantic view to account for representation. The second criticism allows the semantic view to be an account of representation only at the price of implying a form of structuralism about representation.[1]

The semantic view, as is well known, gained its orthodoxy status on theories and models in contrast to the syntactic view. The contrast is usually drawn in the literature in a way that could lead to a misleading picture of the two views and, in particular, of those aspects of the semantic view which are essential to understand its implications for the issue of representation. In the first part of this chapter, I attempt to recover those aspects and to show that they are essential to an adequate understanding of the contribution of the semantic view to both the issues of theory structure and of representation. Such aspects also set the basis for my defense of the semantic view from the two criticisms mentioned above, which are examined in the second part of the chapter.

Before I proceed, some disclaimers are due. The literature about the semantic view, as well as the literature about representation, is vast. In this chapter the formulations of the semantic view that are considered are those which belong to the early stages of the semantic view, and which offer a formal account of models and of their representational task. These are the formulations provided by Suppe, Suppes, and van Fraassen. The reason why I focus on the early stages of the semantic view is that the objections considered in the second part of the chapter mainly apply to these formulations. For this very reason, formulations of the view such as Giere's (1979, 1999), as well as the non-statement view formulated by the German structuralism (e.g., Moulines, 1996, Sneed, 1994, Stegmüller, 1979), are not considered here.

Finally, a last and crucial remark concerning what is meant by the term "representation" throughout this chapter. The notion of representation, as intended both by the semantic view and within the charges against it considered here, is interpreted as the relationship, allegedly of explanatory kind, holding between models and their target system. I do agree with the deflationary claim about the concept

[1]The criticism of the second kind would be conceived as such only by those who, like myself, consider ontological commitment detrimental for an analysis of scientific representation. Reasons for this viewpoint are provided in Section 2.2.3.

of representation in general, and with the remarks by Morrison (1999) and Knuuttila (2011) in particular, that representation amounts to the entire process of model construction, of which model application is only the final stage. However, as I hope to make clear in the following analysis, a rational reconstruction of scientific theories, such as the semantic view, does not aim at a *literal* description of model construction and application. Many aspects related to the epistemic process of model construction and application are just conflated into the model-target relationship.

2.2 The semantic view as a 'program of analysis'

The semantic view arose in the Sixties as an analysis of the structure of scientific theories and, since then, it has gradually gained the status of orthodoxy on the issue. The view was recognized as such at the Illinois symposium on the structure of scientific theories in 1969 where, after a state of "acute intellectual disarray" (Suppe, 1977, p.4) due to the failure of the syntactic view of theories, alternatives to the latter were considered.[2]

The semantic view is presented as a *program* of philosophical analysis of theories. As such, it is not a univocal view: it rather comprises different formulations with a common core of assumptions.[3] The first assumption concerns the aim of this program, which is to provide a "format" for scientific theories (van Fraassen, 1987, p.109), i.e., a possible way to present the structure of a theory. The second assumption concerns the "nature" of the theory structure: while the format

[2]Before that symposium, individual contributions were given by Suppes (1957, 1960, 1967), Suppe (1967) and van Fraassen (1970).

[3]An accurate analysis of the reasons which led advocates of the semantic view to choose one formulation of the view over another has been long overdue. However, it is not among the goals of this chapter to provide such analysis. A conjecture has been put forward by Suppe: "How significant these differences [within the semantic view formulations] are is a matter worth investigating; my conjecture is that they reflect the mathematical preferences of the authors or decisions as to which mathematical approach is most suitable for making progress on other philosophical problems the author is interested in [...]. Despite their mathematical differences, there is a general agreement among the above authors that the theory structures are equivalent to state spaces or the homomorphic images of state spaces." (1989, p.420).

of theories may slightly vary according to the mathematics employed by the supporters of the view, the theory structure is generally assumed to be *extralinguistic*. The argument in favor of the extralinguistic nature of theories comes in two steps. First, the concept of theory is claimed not to be reducible to its formulation in a formal language. Second, it is argued that the formulation of a theory comprises also the class of its models. Note that the same class of models could be described by different linguistic formulations, therefore models are conceived as – at least to some extent – *independent* of descriptions in a particular language (see van Fraassen, 1980a, p.44, and Suppe, 1989, p.82). The following quote from Suppe concisely presents the semantic view as a program of analysis:

> The Semantic Conception gets its name from the fact that it construes theories as what their formulations refer to when the formulations are given a (formal) *semantic* interpretation. Thus 'semantic' is used here in the sense of formal semantics or model theory in mathematical logic. On the semantic conception, the heart of a theory is an *extralinguistic theory structure*. Theory structures variously are characterized as set-theoretic predicates (Suppes and Sneed), state spaces (Beth and van Fraassen) and relational systems (Suppe). Regardless which sort of mathematical entity the theory structures are identified with, they do pretty much the same thing – they specify the admissible behaviors of state transition systems. (1989, p.4)

Given that the semantic view arises as an alternative to the syntactic view, it is ordinary practice in the literature to outline the former by contrast with the latter. The present work is not an exception to such practice. However, two preliminary remarks about the contrast I draw need to be made.

The first remark concerns the version of the syntactic view which I take into account. As for the semantic view, also for the syntactic view we can not identify a univocal school of thought. What I refer to as the "syntactic view" here is its "final version", as it is presented and critically analysed by the advocates of the semantic view (see Suppe, 1977, Suppes, 1967, Thompson, 1989, van Fraassen, 1980a).[4] The final version of the syntactic view presents theories as axiomatized systems comprising a set T of theoretical postulates, or fundamental laws of the theory expressed in formal language, and a set of sentences C (correspondence rules) which provide T with a semantic interpretation.

The second remark concerns the guidelines I appeal to for drawing the contrast. In particular, I follow the advice by Hughes (2010), Suppe (2000) and van Fraassen (2014) to avoid the oversimplified sketches of the semantic and syntactic views often found in the literature. There exists indeed a tendency to reduce the analysis of the theory structure provided by these views to "identity statements" of the form "a theory is ...", where the dots are to be filled in by, respectively, "a collection of models" and "an axiomatic calculus".[5] As the semantic view does not opt for a total rejection of the linguistic component of theories, so the final sketch of the syntactic view heavily relies upon (Tarskian) semantics. On the one hand, as Thompson (1988) and Suppe (2000) emphasize, correspondence rules can be considered as semantic models for the syntactic view.[6] On the other hand, by defining theories as "extralinguistic entities", the advocates of the semantic view do not reject any role for language in the formulation of theories. They rather aim

[4]This version of the syntactic view is defined as "final" by Suppe (1977, p.50). By "final version" Suppe explicitly refers to Carnap (1956, 1966) and Hempel (1958, 1963) (cf. Suppe, 1977, p.52, fn.107). It might seem at first sight odd to analyse a view employing the version which is provided by its very critics. However, the most accurate overview of the syntactic view to date has been provided by the very advocates of the semantic account and, in particular, in the mentioned work by Suppe. Moreover, it is with respect to this sketch of the syntactic view that the semantic view has been formulated by its advocates. So it will be useful to consider the "final version" also in the light of its role of "opposite manifesto" for the semantic view. A critical analysis of this sketch of the syntactic view has been provided by Lutz (2014).

[5]Examples of standard sketches are Craver (2002), French (2008), Godfrey-Smith (2006), Halvorson (2012), Hendry and Psillos (2007).

[6]The influence of Tarski's work on Carnap is actually earlier than his paper *Meaning and Necessity* (1947). In *Introduction to semantics* (1942), Carnap reveals Tarski's influence for the development of his idea of a syntactical formal methods "supplemented by semantical concepts" (ibid., p. vi). He nonetheless acknowledges that their conception of semantics differs on *how to draw* the distinction between syntax and semantics. For Carnap, semantical systems are interpreted languages, while syntactic systems are uninterpreted calculi. Moreover, Carnap puts forward another distinction – upon which Tarski would not agree – between factual and logical truth, the former being dependent on the actual state of things, and the latter relying solely on the meaning of terms determined semantically.

at *deflating* the role which is ascribed to it by the syntactic view.[7] In fact, the view supports the weaker claim that the language of a theory is not the appropriate "individuation property" for the theory structure (Suppe, 1989). According to Suppe, the features of the language in which a theory is formulated can not be used in the philosophical analysis to construe the theory structure.[8] On the other hand, the fact that the semantic view invites to focus on models does not imply that the view amounts to an identity statement about theory structure of the form: "a theory *is* a collection of its models". In other words, the focus on models still leaves some room for language in the formalization of theories.[9]

In the following, to avoid the oversimplifications led by identity statements, I draw the contrast between the semantic and syntactic views on the grounds of three elements, each of which is a crucial aspect of an analysis of scientific theories: the formalization employed, its relationship to actual scientific practice, and the neutrality with respect to other issues that can be raised concerning scientific theories. By fleshing out these aspects, I set the basis for the defense of the semantic view that I present in the second part of the chapter.

2.2.1 Formalization

In this subsection I mean to identify the semantic view's take on formalization. First of all, I focus on the role that the semantic view ascribes to formalization. Secondly, I focus on the concept of model as presented by the semantic view in its formalization of theories.

Contrasting the semantic view with the syntactic view turns out to be useful for at least two reasons. First, it helps to stress that the semantic view, just as the

[7]As stressed by van Frassen, the semantic view's insistence on the extralinguistic character is to be understood as an attempt to orient the philosophical analysis "towards models rather than language" (van Fraassen, 1980a, p.217).

[8]I think that, among the advocates of the semantic view, only van Fraassen explicitly claims the irrelevance of language (1989, p.222). However, he has recently amended the claim (see van Fraassen, 2014, p.279).

[9]Indeed, the very advocates of the semantic view have made their aversion for identity claims explicit in several works (see van Fraassen, 1989, p.222, Suppe, 1989, p.17 Suppe, 2000, p.S112, Suppes, 2002, p.2).

syntactic view, is eminently a *theory of theories*. As such, it preserves the *prescriptive character* of the syntactic view. Thus, the semantic view is *not* a faithful description of scientific theories as employed in actual practice. Second, the contrast allows to show that, while the prescription by the syntactic view to conceive theories in a certain way was in fact an attempt to satisfy the wider philosophical program of logical positivism, the prescriptive character of the semantic view is not intertwined with any philosophical agenda other than the mere analysis of theory structure.

It is fair to say that the semantic view, at least in its early formulation, shares the positivists' idea of philosophy as the *Theory of Science* (Carnap, [1991] 1955, p.393). More precisely, the semantic view conceives philosophy as an analysis which, as Carnap points out, has among its major tasks that of clarifying issues concerning science. Formalization is among the main tools to accomplish such a task. In this vein, Suppes claims that:

> The role of philosophy in science is to clarify conceptual problems [...]. The clarification of conceptual problems or the building of an explicit logical foundation are tasks that are neither intensely empirical nor mathematical in character. They may be regarded as proper philosophical tasks directly relevant to science. In the context of such clarification and construction, a primary method of philosophical analysis is that of formalizing and axiomatizing the concepts and theories of fundamental importance in a given domain of science. (Suppes, 1968, p.653)

The same view is held by Suppe, who claims that: "It is only a slight exaggeration to claim that a philosophy of science is little more than an analysis of theories and their role in scientific enterprise" (Suppe, 1977, p.3), and it is reiterated by Suppes (1954) and van Fraassen (1980b).[10]

[10]The fact that the semantic view, as the syntactic view, conceives philosophy of science *mainly* as an analysis of theories has led Cartwright, Shomar and Suárez to count the semantic view, together with the syntactic view, among the "theory dominated views of science" (Cartwright et al., 1995). It is interesting to note that Suppe has recently deflated the tone of the claim quoted in the text, thus reaching conclusions which are quite sympathetic to the view held by Cartwright, Shomar and Suárez in their paper. Indeed, Suppe acknowledges that nowadays:

So, the semantic view does not reject the task of philosophy of science set by the syntactic view, nor its claim that formalization is the chief device to accomplish such a task. The semantic view rather rejects the syntactic view's reliance on axiomatization to characterize scientific theories (Suppe, 1977, 113). In particular, the discontent is on the dyad to which the syntactic view reduces theories, i.e., axiomatic calculus *plus* correspondence rules.[11]

The semantic view, on the other hand, opts for a more liberal approach to formalization. First, formalization is not necessarily restricted to the axiomatic method, but it can also be extended to encompass semantic techniques, such as those of model-theory. Second, the chosen formalization is *modest* in two senses: neither it is the only possible one that a philosophical analysis can favor, nor it is applicable to all the empirical sciences. Both the points have been clearly stressed by Suppes. In his 1968 paper, Suppes claims that: "To argue that such formalization is one important method of clarification is not in any sense to claim that it is *the only* method of philosophical analysis" (emphasis mine, 1968, p.653). Recently, Suppes has extensively argued against the idea that a philosophical analysis of theories could ever be universally applicable, that is, that it could apply to all scientific theories:[12]

> My view of science has moved increasingly from that of a foundationalist to the viewpoint that the conceptual content of science is best analysed in terms of a diverse set of methods for solving a wide variety of problems. [...] For those raised in traditional philosophy or even traditional philosophy

"much of science is atheoretical [...] . The business of most experimental and observational science is modeling data. [...] Today, models are the main vehicle of scientific knowledge." (Suppe, 2000, p.S109).

[11]Standard criticisms of the kind of formalism employed by the syntactic view in its "final version" are examined in Suppe (1977), Suppe (2000), Suppes (1967) and van Fraassen (1980a). We can group these criticisms as follows: (i) the individuation properties of a theory identified by the syntactic view do not correspond to those of theories as employed in actual scientific practice (Suppe, 1989, p.4); (ii) the Löwenhein-Skolem theorem implies the existence of unintended models for a theory in first-order language, that is, the language of theories as presented by the syntactic view (see Suppe, 2000, p.S104); (iii) given its formulation of theories, the syntactic view would allow for the case that a change in the syntax of a theory leads to change the theory *tout court*.

[12]In the same vein, Suppe (1989, p.199) specifies that the goal of the semantic view is a tentative account of *what is to be* a scientific theory, and not of *what a scientific theory is.*

> of science, with the search for generality and universality of conceptual schemes so dominant, it is not easy to accept or even be sympathetic with a view that is skeptical of the success of any of the general schemes aimed at providing a traditional philosophy of science. [...]. It is only a myth engendered by philosophers – even in the past to some extent by myself – that the deductive organization of physics in nice set-theoretical form is an achievable goal. [...] This does not mean that set-theoretical work cannot be done, it is just that its severe limitations must be recognized. (Suppes' reply to Sneed, in Sneed, 1994, pp.213-214)

In other words, the semantic view does not aim at laying bare the deep structure of theories, nor at using such structure as a canonical formulation valid for *all* the empirical sciences. What it aims at is to provide a formal framework to formulate and solve problems of specific disciplines which can be formalized as the view suggests.[13]

This point helps to correctly interpret the claims made in the seminal paper by Suppes (1960) about the *fundamental* character of the Tarskian concept of models.[14] According to Suppes, the Tarskian concept of models is fundamental in the sense that it can be employed as a "technical meaning" shared by different sciences (empirical and mathematical), as well as in the sense that it can be employed to deal with different issues internal to a specific science. Despite claims to the contrary (see Landry, 2007), in this paper Suppes is *not* setting the basis for reducing all the different concepts of models to the Tarskian one.[15]

[13] This is one of the main features distinguishing the semantic view from German Structuralism (see Moulines, 1996, p.5) which claims that issues concerning methodology do have ontological implications, that is, they play a role in revealing the deep structure of theories. Suppes makes explicit his aversion for this view in his published reply to Sneed (1994).

[14] Tarski defines models as "A possible realization in which all valid sentences of a theory T are satisfied" (1953, p.11).

[15] To be precise, Landry appeals to Suppes' commitment to "the set-theoretical foundationalist program" to ground her interpretation of the role that Suppes assigns to the Tarskian concept of models (Landry, 2007, p.5). Indeed, the goal of the program is to reduce all the branches of mathematics to set-theory (see Suppes, 1972, p.1). However, as the quote from Sneed (1994) suggests, Suppes gave up the idea of applying the set-theoretical foundationalist program to the philosophical analysis of theories at least twenty years ago. I will come back to this point in subsection 2.2.3.

Therefore, the concept of formalization employed within the semantic view is *contingent* in the sense that there is no intention, nor an explicit claim, by its advocates to reduce *all* scientific theories to the format provided by the formalization. Indeed, the formalization of theories that the semantic view puts forward is presented just as one among the *possible* methods of analysis. The adequacy of the formalization is assessed only with respect to those theories which *can* be formulated as the semantic view prescribes. In other words, the semantic view turns out to be adequate with respect to a particular discipline if the issues which are internal to that discipline can be tackled, or even solved, once the theory is formulated as the semantic view prescribes. Contrary to the syntactic view, the format of a theory given by its formulation is neither canonical (i.e., to be conceived as universally applicable), nor used to provide a demarcation criterion to figure out whether or not a theory is scientific. Suppe means to stress exactly the contingent nature of the formalization provided by the semantic view when he claims that:

> I have not offered *necessary* or *sufficient* conditions for being a theoretical explanation. [...] any conclusive account of what a theoretical explanation is must be parasitic upon an account of what is to be a *scientific* theory. While I believe the Semantic Conception provides a defensible account of what it is to be *a* theory, I do not believe it is, or potentially can become, and adequate account of what a *scientific* theory is. The issue of what theories are scientific ultimately is based upon the domain a science deals with and the science's evolving standards as to what is scientific, and thus *ultimately it is not a matter for philosophical fiat or decision.* However, one can present, as I have tried to here, a philosophical account of what it is about theories that *enables* them to provide scientific explanations, and to explain how – and what sorts – of explanation they afford. I believe that doing so can be philosophically or scientifically illuminating – but I do not believe that either I or philosophers of science can have the last word on the subject.[16] (Suppe, 1989, pp.198-199)

Along with the criticisms of the syntactic view based on the *kind of* formalism employed by the syntactic view, there is a less explicit but equally interesting line

[16]This point is reiterated by Suppe later in the text (see Suppe, 1989, p.420,p.432 fn.6, p.420 and p.432, fn. 6).

of argument pursued by the advocates of the semantic view against the syntactic view, and it concerns the *origin of* such formalism. This line of argument can be summarized as follows: the programmatic goal of the syntactic view was not to give a philosophical analysis of scientific theories *tout court*, rather to provide an analysis of scientific theories which could fit well the main posits of logical positivist. On the other hand, the semantic view only aims at being philosophically illuminating about some aspects of theories as employed in actual scientific practice.

That the positivistic epistemology and the syntactic view are deeply intertwined is suggested by Suppe (2000), who dubs the syntactic view the "epistemic heart" of logical positivism. Conflating the analysis of the structure of scientific theories with issues merely pertaining to the positivistic epistemology has been, according to Suppe, "one of their [i.e., the positivists'] more serious philosophical errors" (1989, p.432, fn.7).[17] The same discontent about the reliability of the analysis provided by the syntactic view is expressed by Suppes:

> [P]ositivistic philosophers have written a great deal about the structure of scientific theories [...]. [T]hey have been particularly concerned to give a general account of empirical meaningfulness *which jibes with their account*

[17]The following is a schema which sums up the posits of logical positivism. (i) The *meaning criterion*, or *criterion of cognitive significance* (Carnap, 1947, Hempel, 1965): The meaningful statements are either analytic or synthetic. The condition for synthetic statements to be true is the verifiabilty of their meaning. The *demarcation criterion* follows from the meaning criterion: a theory which does not comply with the meaning criterion is not scientific. (ii) The *role of logic*: Logic has two *denotata*. Logic as Logic of Science denotes Philosophy and, in particular, its aim: to assure that the statements forming science are meaningful. Logic as first-order logic is the form in which the axioms of a theory are given. With the exception of the axioms of mathematics and logic, which are both non-empirical sciences, the axioms of an empirical theory are meaningful only if their observational consequences are verifiable. (iii) The distinction between the *context of discovery* and the *context of justification* of theories (Reichenbach, 1938). Through this distinction, philosophy, as defined in (ii), is sharply separated from those inquiries which take into account the psychological and sociological aspects pertaining to the context of discovery. Hence, the aim of philosophy is to provide the "logical substitute" (Reichenbach, 1938) for the occurrence of actual scientific thinking. The logical substitute of actual scientific thinking is the analysis of the relation of theory to facts (its justification), disregarding who presents the theory and the process of its discovery (Carnap, [1991] 1955). The relation between the theory and the facts is defined by Carnap as *denotational* (ibid.). Therefore, according to the syntactic view, the semantic relation between the theory and the world that it describes is provided by the denotational function of language.

> *of this structure*. Although many precise and interesting philosophical distinctions have been made in the course of these investigations, there has been *little if any* attempt by philosophers working in the positivistic tradition to give a detailed analysis of particular scientific theories. (emphasis mine, 1954, p.242)

The controversy over the *origin of* the formalism chosen by the syntactic view also sheds light on the choice of models as the relevant element for the formalization of theories by the advocates of the semantic view. Indeed, among the reasons given by the advocates of the view for their focus on models there is the need to "regain contact" with theories as employed in *actual* scientific practice.[18] According to Suppe, the "dominant thrust" of contemporary philosophy of science, within which the semantic view took shape, requires the analysis of theories *not* to be jibed with any particular epistemology. In fact, the analysis should be guided solely by the "close examination of actual scientific practice and products" (1989, p.415). It is the "direct examination of theories" (ibid.) which reveals that the syntactic view is a deceptive analysis of theories. In particular, the analysis provided by the syntactic view is deceptive in two senses. First, it assumes that the explanatory role of theories pertains to the (formal) language in which theories are formulated. Second, the analysis assumes that the language of the theory refers to the target system of the theory directly. On the contrary, the direct examination of scientific theories reveals that the language used for their formulation is not itself explanatory. The formal language of theories should be rather conceived as a 'description' of possible models for the theory. It is to models, not to language, that the explanatory power pertains. Hence, if the analysis of theories wants to regain contact with the actual scientific practice, models should be recognized as the main tool in the formalization of theories.

Therefore, the philosophical reconceptualization of scientific theories pursued by the semantic view relies heavily on formalization, whose models are the main device. In so doing, this philosophical analysis reflects the tension that its advocates

[18]A famous formulation of this point is van Fraassen's claim that: "the scholastically logistical distinctions that the logical positivist produced [...] had moved us *mille milles de toute abitation scientifique, isolated in our abstract dreams.*" (1989, p.225).

accept and seek for between, on the one side, the idea of philosophy as a *theory of science* and, on the other side, the demand for the formalization presented by the view to be *in line with* actual scientific practice.

2.2.2 Actual scientific practice

Carnap ([1991] 1955, p.393) claims that philosophy of science is essentially a rational reconstruction of scientific theories and, as such, it differs from its subject matter. The semantic view acknowledges such a difference. However, as stressed in the quote from Suppes in the former subsection, such a difference does not justify a full discrepancy between the philosophical reconstruction of the structure of theories and the fundamental aspects of scientific practice.[19] For this reason, the semantic view can not be conceived as merely prescriptive, i.e., as merely providing a format for theories.[20] The semantic view also aims at being descriptive to some extent, that is, at capturing relevant elements of scientific practice. This subsection is an attempt to sketch how the concept of model presented by the view is affected by its twofold nature, that is, prescriptive *and* descriptive.

In *Experience and Prediction*, where the famous distinction between the content of discovery and of justification is introduced, Reichenbach argues that epistemology is not a description of actual scientific practice, but a *logical substitute* for it:

> It would be, [...], a vain attempt to construct a theory of knowledge which is *at the same time* logically complete and in strict correspondence with the psychological processes of thought. [...] What epistemology intends is to construct thinking processes in a way in which *they ought to occur* if they are to be ranged in a consistent system; [...] For this logical substitute the term *rational reconstruction* has been introduced; it seems an appropriate

[19] Evert Willem Beth who, together with von Neumann, is considered among the pioneer of the semantic view, provided an early version of this stance, claiming that the discrepancy between philosophy and science can not be solved unless the logical analysis of science is freed from "philosophical speculations" (Beth, 1949).

[20] By "prescriptive" I do not refer to anything related to the demarcation criterion between science and non-science. I only refer to the identification of a format which a theory formulation should comply with.

> phrase to indicate the task of epistemology in its specific difference from the task of psychology. [...]; it will, therefore, *never be a permissible objection* to an epistemological construction that actual thinking does *not* conform to it.[21] (emphasis mine, 1938, pp.5-6)

Carnap ([1991] 1955) applies Reichenbach's analysis of epistemology in general to the logical analysis of theories in particular:

> [W]e come to a theory of science [...] if we study not the actions of scientists but their results, namely, science as a body of ordered knowledge. [...] We mean by 'results' certain linguistic expressions, viz., the statements asserted by scientists. The task of the theory of science in this sense will be to analyze such statements, study their kinds and relations, and analyze terms as components of those statements and theories as ordered systems of those statements. [...] it is possible to abstract in an analysis of the statements of science from the persons asserting the statements and from the psychological and sociological conditions of such assertions. The analysis of the linguistic expressions of science under such an abstraction is *logic of science*." (ibid., p.393).

The analysis of scientific theories as presented within the syntactic view is then a rational reconstruction in the following sense: (i) its aim is to provide understanding of theories independently of the elements that pertain to the pragmatics of theory construction (see Carnap, 1942, p.viii), and – most importantly for what I want to discuss here – abstraction from scientific practice guarantees such an outcome;[22] (ii) the logical image of theories thus obtained provides the canonical form that the theory *ought to* mirror in order to be deemed scientific. In other words, the clarifying task of an analysis of theories has to sacrifice scientific practice in order to reach a final image of *the* scientific theory. It is probably on these

[21] The term *rational reconstruction* which Reichenbach employs is the one introduced by Carnap in *Der Logische Aufbau der Welt* (1928). Carnap defines 'rational reconstruction' as an explicit definition for those concepts which take shape in the psychological processes of cognition.

[22] This is what Reichenbach suggests when he claims that logical completeness obtains only at the *context of discovery*'s costs.

grounds that Suppe concludes: "the less it was the positivistic influence, the more it was the concern with 'actual science'." (Suppe, 1989, p.16).

The rational reconstruction obtained within the semantic view is much less detached from the context of discovery than the rational reconstruction provided by the syntactic view. The semantic view does not avoid the reference to actual scientific theorizing, it rather seeks for it. Yet, as a rational reconstruction, it can not open up to the subjective or psychological aspects of scientific theorizing. So Suppe (1977) rejects *Weltanshauungen* analyses exactly because giving too much emphasis to psychological and sociological factors.[23] These analyses, Suppe claims, make science too subjective, whereas "What is needed is a philosophical analysis of the activity of scientific theorizing which does not take recourse either to psychological or sociological factors or their reifications as Weltanschauungen" (1977, p.236). On the other hand, Suppes (1960, p.66) dismisses behaviorist approaches, which focus eminently on users and consumers of theories, insofar as lacking the scientific clarity and definiteness to be compelling alternatives to formal accounts.

According to this picture, the semantic view turns out to be a rational reconstruction of scientific theories which: (i) should be proved able to encode in its formulation some elements of actual scientific theorizing, and (ii) should avoid the possible interference of subjective or sociological factors into the analysis. But how can an analysis of scientific theories be all such things at the same time? An answer come from Suppe's reflections upon the role of the "historically oriented philosophy of science" (1977, p.655). The method of a historically oriented philosopher of science is to abstract patterns of scientific reasoning from the history of science, to examine whether they are good patterns and, in case they were, to extract the structure of the pattern and eventually formulate claims of the form 'if elements of a good pattern of reasoning feature in the theory, then the theory is likely to be successful'.[24] So, actual scientific practice (past and current)

[23]The main claim of *Weltanshauungen* analyses is that the work of philosophers should overlap with the work of historians and sociologists of science. Suppe counts among the *Weltanshauungen* analyses the work by Toulmin (1953), Kuhn (1962), Hanson (1958) and Feyerabend (1970).

[24]The pattern identified is, of course, only a fallible conjecture but, as Suppes stresses by appealing to Lakatos' (1971) idea of a 'system of rules for scientific games', without such conjecture the scientific game is just a game devoid of epistemological relevance.

provides an "evidential warrant" for the conjectures of the historically oriented philosopher of science, and it may or may not confirm the reasoning pattern that the philosopher superimposes upon actual scientific theorizing.

According to Suppe, the semantic view is an analysis carried out within a historically oriented philosophy of science and its main posits have been achieved through the following steps:

1. After having examined several uses of the term 'theory' in alternative scientific contexts, the historically oriented philosopher notices that "one very central use of 'theory' " that could be abstracted is that of characterizing the change of an isolated system's behavior.
2. Examining scientific attempts to formulate and employ this particular notion of 'theory', the philosopher discovers "invariant features" of its uses in actual practice.[25]
3. After the invariant features have been identified, the philosopher can then attempt a precise analysis of those theories.

Therefore, Suppe concludes:

> The semantic conception of theories [...] is offered as such an analysis, and its adequacy depends in parts on the extent to which it provides a precise characterization of this kind of theories and the uses to which such theories are put in science. Such adequacy does not depend on whether its characterization is one the scientist would give or recognize – any more than the philosopher's reconstruction of reasoning patterns found in science need be accounts the scientist would offer. Rather, what counts is that the analysis be an accurate reconstruction of the devices actually employed, and that it can explain how theories bear the epistemic burdens they are called upon to bear in actual usage. (Suppe, 1977, p.658)

[25] Among these features Suppe (1977) includes the identification of several items: the system's class of states used to characterize the behavior of physical systems, the laws describing the system's change over time, the possible formulations (linguistic descriptions) of the system, etc.

Now that we have made plain the intention of the semantic view to accommodate actual scientific practice – within the limits imposed by rational reconstruction – the next question in line is: does the semantic view proves to be adequate not only in theory formalization but also with respect to theory application? According to its advocates, it is the very issue of theory application that makes the choice of models *natural*: models allow to deal with the complexity of theory application which was obscured by the syntactic view and its appeal to correspondence rules (Suppe, 1967, 1972, 1977, Suppes, 1967, van Fraassen, 1980b, 1989):[26]

> When one reflect that the reliance of the theory on the results and procedures of related branches of science, the design of experiments, the interpretation of theories, calibration procedures, etc., are all being lumped into the correspondence rules, there seems to be reason to suspect that, by doing so, a number of epistemologically important and revealing aspects of scientific theorizing are being obscured. (Suppe, 1972, p.11)

The most relevant aspect obscured by correspondence rules is "the concrete experience that scientists label as experiment" (Suppes, 1967, p.62). This aspect is crucial – and the semantic view advocates all agree on this point (Suppe, 1977, Suppes, 1962, van Fraassen, 1980b) – since it sheds light on how theories and phenomena are 'hooked-up' (Suppe, 1977, p.108). On the other hand, the advocates of the semantic view argue, correspondence rules offer a misleading picture of how we apply theory to phenomena. This is mainly for two reasons. First, what justifies theory application is not a bare correspondence of theories (theoretical laws) to facts. Second, what allows to connect theory to phenomena is not to be searched within theories, but it is external to them, and it corresponds to the atual practice of theory construction and testing. In order to grasp this fundamental epistemological aspect, we need to look at actual practice and provide a reconstruction of

[26]The following is a sketch of how correspondence rules are supposed to accomplish the task of theory application, which I borrow from Suppe (1977) and Thompson (1989). The task is fulfilled by providing theories with empirical interpretation. Let's take the classic example of a correspondence rule for a disposition term such as "soluble in water" (Carnap, 1936). A correspondence sentence would then be the formula: $(x)(t)(Sxt \rightarrow (Fx \leftrightarrow Bxt))$ which claims that if x is placed into water at any time t, then x is soluble in water if and only if x dissolves at t. So S is an observation term for a test condition, F is the theoretical term to which we want to ascribe an empirical meaning, and B is the observation term.

how theories latch to phenomena. And it is here that we encounter the concept of representation as entertained by the semantic view, as well as the crucial role that models play within the semantic view – not only in theory formulation, but also in the rational reconstruction of scientific practice.

In order to understand these claims we need to look a bit deeper into the reconstruction of theory application according to the semantic view. A detailed sketch of the reconstruction is provided by Suppe:

> In applying a theory (or law) to phenomena, what we do is collect data about phenomena; the process of collecting the data often involves recourse to rather sophisticated bodies of theory. If accepted standards of experimental design, control, instrumentation – and possibly involved reliability checks – are carried out, a body of "hard" data is obtained from experimentation and is taken to be relatively non problematic, sometimes generally accepted laws of theories are also employed in obtaining these "hard" data. It is to this body of "hard" data that the theory is applied. If the purpose of the application of a theory is explanation, then the theory explains an event under the description provided by this "hard" data by relating it to other "hard" data which function as descriptions of other features which were the cause of the event so described.[27] (1989, pp.63-64)

Throughout the developments of their formulations of the semantic view, Suppe, Suppes and van Fraassen have all deal with the issue of theory application (see Suppe, 1974, 1977, 1989 , Suppes, 1962, 1967 van Fraassen, 1985, 2008). Among these contributions, Suppes' (1962) is particularly relevant. Indeed, in this paper Suppes introduces the idea of a *hierarchy of models* standing between theories and

[27]According to Suppe (1989), the concept of "hard data" is particularly helpful to bracket within the analysis of scientific theory the issue concerning the distinction between observables and unobservables which, on the other hand, the syntactic view forces into its analysis of theories. As presented in the quote above, hard data are *descriptions of* raw data and of (causal or of other sort) interactions among the latter. Hard data might be theory-laden, in the sense that the process of their collection involves sophisticated bodies of theories. If this is the case, hard data are not given by direct observation. Hard data might obtain by direct observation as well . Therefore, theoretical statements are not necessarily correlated to direct-observation statements, as the final sketch of the syntactic view suggests. Theoretical statements are correlated to hard data, which may or may not obtain by direct observation.

their target systems. The idea of a hierarchy of models has then been borrowed both by Suppe and van Fraassen to account for theory application. Within Suppes' reconstruction, the hierarchy goes from the high level theory to the "hard data" obtained through the body of theories mentioned by Suppe. In the following, I focus mainly on Suppes' reconstruction.[28]

Suppes famously claims that an "exact analysis of the relation between empirical theories and relevant data call for a hierarchy of models of different logical type" (1962, p.253) to bridge the gap between "the model of the basic theory and the complete experimental experience" (ibid., p. 260).[29] Models are of different logical types in the sense that at each layer of the hierarchy there is a "theory in its own right" (ibid.) of which the model is a realization:

> Given an axiomatised theory of measurement of some empirical quantity such as mass, distance, or force, the mathematical task is to prove a representation theorem for models of the theory which establishes, roughly speaking, that any empirical model is isomorphic to some numerical model of the theory. The existence of this isomorphism between models justifies the application of numbers to things. [...] What we can do is to show that the structure of a set of phenomena under certain empirical operations is the same as the structure of some set of numbers under arithmetical operations and relations. (1967, p.57)

So the glue holding the hierarchy together is the notion of isomorphism between two structures, which Suppes defines as the "most general and useful set-theoretical

[28]Van Fraassen's reconstruction of how theories latch to phenomena will be examined in subsection 2.2.3. On the other hand, Suppe's analysis, and his concept of *empirical design* in particular, is openly parasitic to Suppes' formulation (cf. Suppe, 1977, p.108 and cf Suppe, 1989, p.135). So, the analysis that follows implicitly concerns Suppe's formulation as well. For an overview of Suppe's formulation, see Appendix B.

[29]Behind Suppes' attempt to reconstruct actual scientific practice within the framework of the semantic view, there is a more ambitious program that Suppes presented at the very early stages of his work on scientific theories (see Scott and Suppes, 1958, Suppes, 1954). Suppes holds that in foundational studies of philosophy of science we need to distinguish between theory and experiment, since the reconstruction of the experimental practice is more problematic than the reconstruction of the theoretical work which can be axiomatised using set-theoretic predicates. The final goal to be reached is then a "kind of algebra of experimentally realisable operations and relations" (1954, p.246), that is, a systematic theory of measurement which could be axiomatised as easily as the fundamental theory.

[notion]" to make precise the relation holding between the different models making up the hierarchy (Suppes, 2002, p.54). It should be noted that the notion of isomorphism between models of a theory is defined by Suppes as "sufficiently independent" (ibid.) of the axioms of a theory. Indeed, the definition of isomorphism depends only on the set-theoretical properties of the models of a theory. Although models are realization of the axioms of a theory, the latter plays no role in determining their set-theoretical properties. Therefore, isomorphism can be said to be "axiom free". [30]

Back to the hierarchy, it ideally comprises the high level theory, the theory of experiment, the theory of data, the theory of experimental design, and the so called *ceteris paribus* conditions. At the top of the hierarchy we find the fundamental theory, whose realization are models of the theory (theoretical models) in the set-theoretical sense presented by Suppes (1954) (see Appendix A). In case the models of the hierarchy are isomorphic, we can have theories whose models of different logical types (satisfying different axioms) have the *same* structure.

For instance, suppose that the theory at stake is classical particle mechanics and that the we want to predict the behavior of a metal ball on a plane with a certain inclination γ, which is our target system. The equations of classical particle mechanics describe the behavior of the physically possible systems of a finite number of point masses. However, not all of these physical systems represent the target system. It is the theory of experiment which identifies the physical system that represents the target system, that is, the physical system that represents the behavior of a metal ball on an inclined plane with inclination equal to γ. Indeed, the theory of experiment (together with some auxiliary hypotheses) specifies which discrete ordered sets of data (e.g., positions and momenta coordinates) correspond to sequences of states of inclined plane systems. The theory of experiment thus

[30] Downes (1992), Suárez (2005) and Thomson-Jones (2006) extensively argue about the ambiguity of the semantic view concerning the *actual* role it ascribes to models, whether that of semantic interpretations, or of abstract mathematical structures. In particular, they argue that only the second sort of role could let models be representational. On the contrary, as stressed by Suárez, leaving room to the notion of models as semantic interpretations would be a risky choice which would make the view fall prey of Friedman's (1982) and Worrall's (1984) objection that the semantic and syntactic view are just equivalent.

describes all the possible sets of data which can be obtained by experiments on an inclined plane. In order for the gathered data to be compared with the outcomes from the theory of experiment, we need a theory of data. The theory of data specifies how the data from the theory of experimental design can be put in a canonical form, that is, it specifies how data can be expressed in terms of the parameters specified in the physical theory. Indeed, raw data can be collected from planes with friction, while the theory of experiment considers frictionless planes only. (The theory of data also employs auxiliary hypotheses and statistical theories). Finally, a theory of experimental design comprises all the rules that should be followed to run the experiment (consistently with the theory of data and with the theory of experiment). The theory of experimental design comprises *ceteris paribus* conditions as well, which are statements describing the laboratory set-up to design the experiment and how the latter is performed.

To have a clearer overview of the role of models within the hierarchy, we can follow Suppes' suggestion (1962) and imagine the hierarchy in terms of sample-spaces. The model of the experiment, which is a realization of the theory of experiment, identifies all the possible outcomes of the experiment that can be compared with the theoretical model, which is a realization of the fundamental theory. The next layer of the hierarchy comprises the model of data, which is a realization of the theory of data. A model of data restricts to those aspects of the experiment that have variables in the theory. Moreover, this models undergoes tests of goodness-of-fit which establish whether the model can fit the model of experiment. The last layer – the closest to the concrete target system – comprises the model of experimental design, which is a realization of the theory of experimental design. The model of experimental design provides the experimenter with the necessary rules to be followed in order to perform the experiment in a controlled environment. The outcome of the experiment will then be compared with the model of data and with the model of the experiment.

As for the formalization provided, so for the analysis of theory application, the advocates of the semantic view call for a modest interpretation. The analysis of how theories latch to their target system has to be conceived merely as a rational reconstruction . As Suppes (2011) has recently emphasized, a rational reconstruction of scientific theorizing crucially depends on the selection of relevant aspects of theorizing which characterize *particular* sciences (e.g. physics, biology, chemistry, etc). In the selection process, several details actually characterizing scientific theorizing, and which are distinctive of the different disciplines, are missed. What we end up with is an analysis which aims at being accurate with respect to scientific theorizing as actually carried out, although it is not required to be universally applicable. In fact, what Suppes claims to provide by means of the hierarchy of models is a "conceptual grinder that in many cases is excessively coarse" through which scientists' experience can be put (Suppes, 2002, p.7).

In the light of the analysis of the semantic view presented in this subsection and in subsection 2.2.1, we can draw the following conclusion on the role of models within the semantic view. The role of models is twofold. On the one side, models are the main tool for the formalization of theories. On the other side, models are also part and parcel of the reconstruction of actual scientific practice. Suppes gives exactly this twofold role to the set-theoretical structures that he employs in his analysis of theories. Indeed, Suppes argues that set-theoretical structures provide a framework which is "powerful enough easily to express any of the systematic results in any branch of empirical science." (2002, p.2). This framework also works as s "formal machinery" which, by relating theory to data, provides a more elaborate account of the actual practice of testing scientific theories (ibid.).[31]

2.2.3 Neutrality and ontological commitment

In this subsection I analyze a feature of the semantic view which, despite its relevance, is seldom taken into account to draw the contrast between the syntactic and the semantic view. The feature at stake is the semantic view's neutrality

[31] In his most recent paper on the semantic view (2011), Suppes introduces a new stance on the possible tasks of an analysis of scientific theories. Suppes argues that any rational reconstruction of scientific theorizing, including the set-theoretical formulation of the semantic view, leads to a level of abstraction which is "far removed from what is going on in actual experiments" (2011, p.116). Therefore, a rational reconstruction hardly can deal with the complexity of the experimental practice.

with respect to other philosophical agendas not directly pertaining to the issue of theory structure and application. In subsection 2.2.1, I have stressed that the advocates of the semantic view dismiss the formalization of theories propounded by the syntactic view *also* as an attempt to accommodate the positivistic epistemology within the analysis of the theory structure. In subsection 2.2.2, I have pointed out that the inadequacy of the syntactic view to accommodate essential aspects of actual scientific theorizing was partly due to the influence of positivist epistemology. Drawing a contrast between the syntactic and the semantic view, then, also implies to see whether the semantic view is tied or not to any epistemic stance.

In this section I analyse whether the semantic view in fact manages to preserve its neutrality with respect to epistemic attitudes – whether realist or antirealist – and with respect to the ontological commitment that such attitudes prescribe. We could assume that a failure in this respect might undermine the tenability of the semantic view as an analysis of theories and representation, just as the influence of positivism has jeopardized the tenability of the syntactic view.

Overall, one of the features of the semantic view is that it aims at neutrality. What I mean to highlight in the following is that while Suppe's and Suppes' formulation of the view complies with neutrality, van Fraassen's formulation faces some problems in this respect. This is because, beginning with *The Scientific Image*, and in subsequent works (1987, 1989, 1991), van Fraassen employs the semantic view as a framework to develop his famous antirealist stance known as *constructive empiricism*. Despite his claims to the contrary (1980a, 1987, 1989), van Fraassen can not help but conflate his formulation of the semantic view with the epistemic stance that he advocates, thus compromising the neutrality of the semantic view.

Before proceeding, let me briefly introduce the concept of epistemic attitude. An epistemic attitude is generally cashed out in terms of the belief that is implied by the acceptance of a (successful) theory.[32] According to van Fraassen (1980a), for

[32]For sake of argument, I am casting antirealism solely in terms of van Fraassen's *constructive empiricism* (van Fraassen, 1980a). For an overview of the different forms of antirealism in

a realist to accept a theory implies to believe that what the theory says about the world – whether the entities, relations, etc. at stake are observable or unobservable – is literally true. The epistemic attitude of the form of antirealism known as constructive empiricism (van Fraassen, 1980a) is substantially more modest: to accept a theory implies only to believe that it is empirically adequate, i.e., that the theory has "at least one model that all the actual phenomena fit inside" (van Fraassen, 1980a, p.12) – or, using van Fraassen's terminology, that it "saves the phenomena".[33] In order to grasp the kind of ontological commitment involved by these two different epistemic attitudes, we could employ Fine's analysis of the relation between epistemic attitudes and commitment (1986, p.130): the acceptance of a theory as true (or empirically adequate) implies the commitment to the existence of the individuals, properties, relations, etc. which are referred to by the theory. More precisely, while the realist is ontologically committed to both the observable and the unobservable posits of successful theories, the constructive empiricist will remain agnostic towards the unobservables and commit solely to the observables.[34]

The neutrality here invoked for the semantic view depends crucially on its capacity *not* to privilege any epistemic attitude, *or* the ontological commitment that an epistemic attitude would imply. The negative consequences, were the semantic view *not* casted neutrally, have been clearly specified by Chakravartty:

> The moment the [semantic view advocate] opts for any sort of commitment, be it instrumentalist or realist, she opens the door to the very difficulties the development of the semantic approach was in part intended to leave behind:

philosophy of science, see Chakravartty (2014, Sect. 4). Also worth mentioning is that Psillos (1999, p.xix) argues that scientific realism incorporates three theses: metaphysical, semantic, and epistemic. Although I do agree with this tripartition, given that my focus is on van Fraassen's antirealism, I do not strictly stick to it.

[33] Massimi (2007) argues that the "practice of saving phenomena" as presented within van Fraassen's formulation of the semantic view can be read as including unobservable phenomena as well as observable ones, since also unobservable phenomena manifest themselves in data models.

[34] Although not relevant for our topic, it is worth noticing that van Frassen's empiricism differentiates from traditional forms of empiricism since, although it is agnostic about unosbervables, he does not conceive statements about unobservables as not capable of having truth value. They can therefore be taken literally, although this does not force us to believe in the existence of theoretical entities featuring in these statements.

> namely, issues concerning the correspondence between language and world. As soon as we give not merely a prediction, but a description of ontological commitments associated with that prediction – concerning which elements of our model are meant to correspond to reality and which are not – the traditional challenge to the realist of giving a satisfactory account of such correspondence returns. (Chakravartty, 2001, p.330)

The quote above illustrates where the risk (or the temptation) to undertake ontological commitment lies for an advocate of the semantic view and, generally, for a philosopher engaged in the analysis of the theory structure and its application. The risk lies in justifying the application of models to their target system: given that models are abstract entities, how can the semantic view justify the fact that models are informative about non-abstract reality? Ontological commitment could be a quick route to reality for the semantic view: an advocate of the semantic view could first commit herself to the truth (or the empirical adequacy) of the theory whose models are structures, and then assume that (either the observable, or both observable and unobservable parts of) the actual world is in some mapping relation with one of these models. Under these assumptions the model would represent because it just 'mathematically resembles' an actual state of affairs. So the issue of neutrality becomes a matter concerning the possibility that advocates of the semantic view employ any epistemic stance to justify the fact that models represent their target systems.

According to van Fraassen, the semantic view does not run any risk in terms of neutrality. Van Fraassen (1980b) makes this point clear by drawing a schema of the arguments we can deal with within philosophy of science and claiming that those pertaining to the semantic view are all tackled neutrally with respect to realism (or to its alternatives). So, in his paper, van Fraassen sharply distinguishes the kind of issues pertaining to philosophy of science into two broad categories: *internal* questions and *external* questions.

Internal questions concern the "theory taken *by itself*" (ibid., p.664). These are issues whose analysis "does not depend on the way the world actually is, nor on how we use or regard the theory, but only on the way the theory is" (ibid.).

Among the internal issues van Fraassen counts the issue of the theory structure and the theory content. We have already met the issue of the theory structure in subsection 2.2.1: it is the issue of how to provide an adequate formalization of theories in order to answer the question 'what is a scientific theory'. As for the issue of content, van Fraassen vaguely defines it. The characterization which emerges from this and other works by van Fraassen connects the issue of content with the renown problem of interpretation, that is, the analysis of how we equip theories with content (see van Fraassen, 1987, 1989, 1994). I will say more on this shortly. External questions, on the other hand, concern the elements "external to the theory" (ibid.), such as the epistemic attitudes of the users of a theory towards the content of that theory. Internal issues are then conceived as prior to and independent of the choice of realism – or any of its alternatives – as well as of any other element which is external to theories.

Within the schema, van Fraassen (1980b) places a third "topic" which is claimed to be neither internal nor external. This is the relation of the theory with the data, which van Fraassen considers "independent of the truth of the data" (ibid.). The argument is – in accordance with Suppes' idea of a hierarchy of models – that a theory is not confronted with raw data, but with models of data ("hard data", in Suppe's jargon) which are "the dress in which the debutante phenomena make their debut" when a theory is construed or applied (ibid., p. 666). Hence, we can infer, notions such as truth – or empirical adequacy – are not applicable to evaluate such a relationship either.

This schema provides a tidy reconstruction of the main issues of the philosophy of science and the domains of analysis they belong to, with a stress on the independence of the semantic view's domain of analysis with respect to realism or to any of its alternatives. However, this schema also reveals a crucial difference of perspective between van Fraassen and the other advocates of the semantic view. This difference – as I argue below – consists in how to handle the 'internal' issue of content.

There is a general agreement on the fact that the issue of content can be identified with the problem of interpretation (see van Fraassen, 1989, p.226, and Suppe, 1989, p.422). Indeed, without an analysis of the relationship between models and their target systems, the theory structure would be just an ensable of abstract symbols devoid of cognitive significance.[35] To avoid the risk envisaged by Chakravartty – and confirmed to be avoided within the semantic view by van Fraassen's schema – we need to equip these abstract symbols with content in a way that does not commit to the existence of any element of the theory.

While Suppe and Suppes stick to the schema drawn by van Fraassen and handle the issue of content neutrally, the very van Fraassen fails in this respect. The main reason for van Fraassen's failure is the fact that he sharply distinguishes the issue of content from the 'third' issue of theory-data relationship, while Suppe and Suppes conflate the two. Indeed, as we have seen in subsection 2.2.2, in order to explain how theories can be latched to phenomena – thus accomplishing their explanatory function – we do not need to get outside the hierarchy of models provided by Suppes. The hierarchy provides a rational reconstruction that helps understanding how theories latch to their target systems in virtue of the very practice of theory construction and application.[36] The neutrality of Suppes' account in the justification of such a relationship is widely acnowledged (Stegmüller, 1979, p.11,, Suppe, 1989, p.22,, Ruttkamp, 2002, p.92). Suppes himself explicitly lays it bare: "[My view] says nothing about actual beings. [...]. The actual events I am referring to [within his formulation of the semantic view] are also in some sense abstract. In fact, [...], I think that much of our experience with actual objects and processes is indescribable in its full concreteness. Any description, in informal or formal language, is some kind of abstraction." (Suppes, 2011, pp.115-116).

[35] I am borrowing the expression from Reichenbach (1965, p.36) who poses exactly the same problem, though of course cashed in 'syntactic' terms. He stresses that the fundamental equations of physics only pose a system of mathematical relations, and nothing of this system of relations hints to the fact that it is cognitively significant: "although the equations, that is, the conceptual side of the coordination [between a mathematical and an empirical entity] are uniquely defines, the 'real' is not" (ibid.37). Reichenbach addresses the problem by the famous coordinative definitions.

[36] This interpretation of the role of rational reconstruction of actual practice brings the semantic view, and Suppe's formulation in particular, much closer to the view held by detractors of the semantic view, such as Morrison (1999), who stresses that it is the very practice of model construction and applications which links models to their target system.

In what follows I restrict the focus to Suppe's and van Fraassen's formulation only. The fact that their formulations of the view are similar might help to easily grasp their different approaches to the issue of content and neutrality. The similarity of their formulations of the semantic view will allow to better grasp where the main difference with respect to the issue of content and neutrality actually lies. As is well known, Suppe and van Fraassen both develop their formulation of the semantic view using the concept of state space (cf. Appendix B).[37] On their accounts, the interpretation of a theory is a two-step process.

The first step is to specify the theory structure by means of which to represent the target-system. In order to do that, one needs to specify the theory's state space.[38] One then specifies the physical magnitudes (observables) considered in the theory. Finally, one needs to specify the theory dynamics: the development in time of states and observables. The possible dynamical evolution of the system can be described as trajectories through the state space, i.e., as sequences of states in the state space. The structures needed to represent the target system are provided by the so called phase space: the state space equipped with rules describing how the system goes from one state to another – i.e., trajectories – which are derived according to the laws of the theory. Each trajectory in the state space corresponds to a "possible world" according to the theory, namely to a sequence of events that describes one possible behavior of the system.

The second step is to identify the possible worlds according to the theory and 'to claim' that one of the possible worlds correctly represents the *actual* one. The representational claim is then that the model is *true* of this world. The accounts of Suppe and van Fraassen differ in the way they deal with this second step. More precisely, there are important differences in both the kind of 'truth' involved in the representational claim and the notion of actuality implied by the claim that

[37] I am not singling out here the specificities of each account. Suppe (1977, pp.227-228, fn.565) however claims the main difference to be that, while van Fraassen (1980a) identifies theories with configurations imposed on the phase space, he instead prefers to view configurations imposed on phase space as canonical mathematical replicas of theories. For the sake of argument and space I will merge the two account with, I hope, no loss of content.

[38] In the case the theory is classical particle mechanics and the system at stake is a particle moving in one dimension, the state space comprises all possible ordered pairs of the form (x, p), where x describes position and p describes momentum. See Appendix B.

the represented target system is the 'actual' one. Below, I contrast Suppe's and van Fraassen's accounts on these points.

Suppe (1989) holds that the possible worlds identified by the theory are "causally possible physical systems". Members of the set of causally possible physical systems correctly describe actual systems in situations where parameters *not* incorporated into the models exert negligible influence. Call S a model (*physical system*) corresponding to a target system (*phenomenal system*) P and let S be characterized in terms of various parameters abstracted from P. The values of the parameters characterizing S (more precisely, characterizing the states in which S is in at any given time t) are *not* actual parameter values characterizing P (at the same given time t). Rather they stand in a replicating relation to the values in P of the form:

> If P were an isolated phenomenal system in which all other parameters exerted a negligible influence, then the physical quantities characteristic of those parameters abstracted from P would be identical with those values characteristic of the state at t of the physical system corresponding to P. (Suppe, 1977, p.95)

If the relation between a model and its target system is of *replication*, any description of the model S can only be "counterfactually true" of P. So the model is only *counterfactually* true of the target system.

To see whether realism is at any level implied here, we need to briefly consider the distinction by Suppe between abstraction and idealization (Suppe, 1989, pp.95-96). If the parameters of P are 'simply' *abstracted*, then it would be possible for P to replicate S's behavior in reality. This is because we can recreate (e.g., in laboratory) a situation where the behavior of P as described by S becomes causally possible. If the parameters of P are *idealized*, then the conditions set for the phenomenon as described by S to occur are impossible to realize.

The fact that scientific theories tend to both abstract and idealize makes classical realism (as well as empirical adequacy) untenable to unravel the theory-world relation: "there is no guarantee that any of the theory's model [...] will be isomorphic to any actual phenomenal systems" (ibid.). Therefore theories as presented within the semantic view are not literally true since they do not literally describe how the world behaves, rather how it would behave were certain conditions met ("if the world *were* 'nice and clean' ", Suppe, 1977, p. 348). For this reason, Suppe prefers to talk of *quasi-realism* which, he argues, does not presupposes ontological commitment. In other words, theories are only *counterfactually* (non-literally) true of phenomena, and the representational relation between the model and the phenomenon at stake is only of replication, not of correspondence. There is then no need to commit to the existence of idealized objects in *actual phenomena* to have the relationship justified. So, Suppe concludes:

> [R]ealism is descriptively false of the theories science actually uses [...] Realists' mistake is to conceive theoretical entities as particulars. [...] idealizations typically are theoretical entities used nonexistentially. Thus idealizations can be employed in theories without committing oneself to their existence. When a physical system S corresponding to a phenomenal system P is an idealized replica of P, one need not commit oneself to the idealized values of parameters being properties possessed by particulars in causally possible phenomenal systems. And using such idealized values does not preclude the theory from being empirically true or false, for by the empirical truth conditions [...] theories are *counterfactually* true or false of the phenomena within their intended scope, regardless whether causally possible physical systems are purely abstractive or idealized replicas of phenomena. The realists are correct, then, in supposing that theories are empirically true or false and may commit one to the existence of nonobservable particulars or attributes, but they are wrong in identifying empirical truth or falsity with factual truth or falsity and in supposing that to invoke theoretical entities in a theory *always* is to commit oneself to their existence as particulars. (1989, pp.100-101)[39]

[39] Another way to illustrate Suppe's and Suppes' stance on the impossibility to deal with actual beings within the semantic view is to parallel their stance to a possible analogue in philosophy of mathematics. An example in this field is Resnik (1975) who develops the idea that the notion of object is never *absolute*, but always *relative* to a theory. More recently, Rizza (2011) has provided a view of mathematics as non-ontologically committing. Mathematics rather offers means to "conceptualize" empirical phenomena, that is, to provide them with a structure which sets up the formal conditions from which the explanation of the phenomena at stake derives.

As for van Fraassen, the possible worlds identified by the theory are empirical substructures to be used "as candidates for the direct representation of observable phenomena" (1980a, p.64). More precisely, the representational claim in van Fraassen's formulation of the view coincides with the *theoretical hypothesis* that models are isomorphic to "concrete observable entities" (van Fraassen, 2008, p.386) or, equivalently, that the world is isomorphic to one of these models.[40] Claims concerning the model are then true about the observables of the target system (that is, they are claims of empirical adequacy of the model). Consequently, to hold such claims entails to commit oneself to the existence of those observables which can be *embedded* in the empirical substructures:

> Thus we see that the empirical structures in the world are the parts which are at once *actual* and *observable*; and empirical adequacy consists in the embeddability of all these parts in some single model of the world allowed by the theory. (van Fraassen, 1991, p.228)

In conclusion, van Fraassen requires the representational claim to concern *actual* phenomena and, by doing so, he ends up asking to go beyond the theory-data relationship to which both Suppe and Suppes restrict any representational claim.[41] Hence, van Fraassen resolves the representational relationship within constructive empiricism, rather than within the semantic view itself. Van Fraassen has recently acknowledged this 'misuse' of representational claims. In his more recent contributions on the topic (see van Fraassen, 2006, p.536, and van Fraassen, 2008, fn.8, p. 386), van Fraassen makes a "*mea culpa*", holding that in the former works mentioned above the claim of empirical adequacy "uses unquestioningly the idea that concrete observable entities [...] can be isomorphic to abstract ones". In other words, van Fraassen recognizes that the theoretical hypothesis above lays

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Analogously, within the semantic view the objects we deal with are not *particulars*, but rather objects of a theory, that is, the objects that we would obtain if the formal conditions provided by the theory were satisfied.

[40]Van Fraassen borrows from Giere (1979) the idea of a 'theoretical hypothesis' to express the representational relation between models and their target systems.

[41]An analogue analysis could be carried out taking into account van Fraassen's development of *semi-interpreted language* to give semantic interpretation of theoretical language (see van Fraassen, 1967).

itself open to metaphysical readings. How van Fraassen turns down such possibility in his more recent contributions is part of my arguments in sections 2.3.2 and 2.4. (See also Appendix C for a thorough analysis of van Fraassen's neutral formulation of the semantic view before and after the 'bewilderment' due to the *Scientific Image*).

2.3 First charge: The semantic view does not account for representation...

There are two ways for criticizing the semantic view as an account of representation. First, we could question whether the semantic view is a *good* account of representation.[42] Second, we could question whether *any* account of representation can be given within the semantic view.

The charge I am going to consider in this section is of the second sort. More precisely, I will examine as illustrative examples of the second charge two recent paper by Le Bihan (2012) and Brading and Landry (2006). These papers share a common assumption and a common conclusion (reached by means of different arguments): respectively, that the semantic view is eminently a program of analysis of the structure of theories, and that within such a program representation can not be accommodated.

What makes these contributions relevant for my analysis is that, although appealing to the same assumption (the semantic view is eminently a rational reconstruction of scientific theories), we reach opposite conclusions about the possibility for the semantic view to account for representation. I will scrutinize the reasons that these authors provide for such a conclusion, and I will attempt a counterproposal.

My argument takes the criticisms put forward by Le Bihan (2012) and Brading and Landry (2006) to be based on two assumptions which, I claim, are unwarrantedly

[42]Philosophers criticizing the semantic view on these grounds are Bailer-Jones (1999), Cartwright (1983, 1989, 1999a), Cartwright, Shomar and Suárez (1995) Frigg (2006), Morrison (1999, 2007), Suárez and Cartwright (2008).

too strong. The first assumption (held by Le Bihan) is that a necessary condition for the semantic view to account for representation is that it must be able to tell us when the procedure followed by a scientist to construct a (data) model is good or bad for a given phenomenon. The second assumption (held by Brading and Landry) is that representation *via* models demands for "actual beings" – i.e., a structure on phenomena must be imposed either by identifying phenomena with their associated data models, or by directly assuming that phenomena come in structures and, they argue, the semantic view can not meet such a demand.

2.3.1 Le Bihan

Le Bihan (2012) begins her analysis with the claim that there are two possible interpretations, one strong and one modest, of the semantic view.

According to the strong interpretation of the semantic view, the view aims at providing a *complete* account of scientific theories and scientific practice. The account could be complete in two senses: (i) as an account of theories, the semantic view *fully* exhausts the concept of theory by *identifying* it with the class of its models; (ii) with respect to actual scientific practice, it is assumed that the semantic view can *fully* accommodate scientific practice.[43] Hence, the semantic view aims at being a *faithful* description of models as employed in actual practice and of the scientific practice *tout court*.

The modest interpretation of the semantic view is rather a "methodological prescription to use model theory as a tool for the rigorous analysis of the structure of what scientists typically use to represent the world in actual practice" (ibid., 251). In this weaker interpretation of the view, scientific models and scientific practice can be at most *partially* characterized by the view. That is, the semantic view has not among its goals to provide a *complete* view of scientific theories.

[43]As example of the strong reading, Le Bihan considers mainly Morrison (2007) and Maudlin (2007). Suppe (2000, pp.S111-S112), on the other hand, claims that examples of strong interpretation are to be found in the work of Cartwright et al. (1995) and Morrison (1999).

Le Bihan claims that it is a matter of survival for the semantic view to drop the strong version in favor of the modest one. In particular, she argues that only the modest interpretation of the semantic view can survive the criticisms usually raised against the strong version.[44]

This naturally leads Le Bihan to ask the following crucial question: *what can be accomplished by the semantic view as a methodological prescription?* Such a question, Le Bihan goes, pertains to the issue of the *adequacy* of the Semantic View as an account of scientific models, "where scientific models are taken to be 'what scientists typically use' to represent the world" (ibid., p.261). Le Bihan identifies three criteria of adequacy for any account intended to describe an item highlighting the use for which it has been built for. Following Le Bihan, I illustrate the criteria by means of an example related to an ordinary object, such as a sailboat. Consider a sailboat which has been built for the purpose of sailing. With respect to such a sailboat an account can be:

> (i) *structural-adequate*, in the sense that the described structure of the sailboat (e.g., length, height, weight, etc.) is the one actually possessed by the sailboat;
> (ii) *functional-adequate*, in the sense that it explains how the structure of the sailboat is employed for sailing (e.g., why that specific length, height, weight, etc. are needed), that is, how it accomplishes its function;
> (iii) *pragmatics-adequate*, in the sense that it explains how the structure and function of the sailboat are employed for attaining certain purposes of the sailors (e.g., sailing comfortably, sailing fast, etc.).[45]

[44]An example of criticism to be conceived as directed against a strong interpretation of the view is that of Morgan and Morrison, claiming that the semantic view provides only a narrow perspective on the role of models in scientific practice and that "there is much more to be said concerning the dynamics involved in model construction, function and use" (1999, p.10). Analogously, Cartwright questions whether the semantic view is able to capture the "incredibly difficult and creative activity" of model production (1999b, p,247), and Suárez argues that "the semantic view lacks the resources to provide us with an understanding of how, in practice, models mediate between theory and the world" (1999a, p. 172).

[45]More precisely: "a pragmatic account is expected to account for the choices made in explaining how these choices result from how the structural features of the boat, along with how

On the grounds of the above classification of adequacy, we can consider whether and to what extent the semantic view is an adequate account of scientific models, casting the adequacy as in (i), (ii), and (iii) above. Accordingly, the semantic view could be an account:

> (i)' *structural-adequate:* the model structure described by the semantic view is the one possessed by the models of interest;
> (ii)' *functional-adequate:* the structure of models described by the semantic view explains how models accomplish the representational function (assuming that the function of models is to represent their target systems);
> (iii)' *pragmatics-adequate:* both the model structure described by the semantic view and the model function explained by the semantic view help to understand pragmatic choices made by the scientists using the models.

Two things are worth noting at this point. First, as emphasized by Le Bihan, looking at the sailboat example we can infer that: structural features can be analysed independently of functional or pragmatic considerations; functional features can be analysed independently of pragmatic considerations yet they crucially depend on structural features; whereas pragmatic features depend on both structural and functional features. In other words: "the way sailboats are used depends on the internal make up of sailboats, and our choices regarding sailboats depend on both the internal make up of sailboats and how this affects their possible usage" (ibid., p.262). Second, note that what is at stake when assessing functional-adequacy is the issue of representation. More precisely, Le Bihan's definition of representation is the following: "it is a relation between a *representans* and a *representandum.* [...] the representans is a scientific model, while the representandum is a phenomenon in the world" (ibid., p. 266).

Le Bihan claims that within the modest interpretation of the semantic view only structural-adequacy can be attained – while the strong interpretation would also

these features result in specific functions, relate to the specific goals and means that the sailors have." (ibid., 262).

allow for the attainment of functional-adequacy.[46] In other words, the semantic view's modesty asks for dropping the representation issue and admitting that: "[T]he Modest Semantic View commits only to the claim that studying scientific theories from the point of view of scientific models, and studying scientific models from a model-theoretic point of view, is sufficient to give a structural-adequate account of scientific models" (ibid. p, 263). The representation issue has to be dropped since the semantic view, being merely a formal analysis, can not account for the representational function of models as defined above, that is, it can not account for how models are put to use in order to represent the targeted phenomenon.[47] Hence, Le Bihan concludes:

> If the Semantic View is supposed to be a comprehensive view of science, then it had better say something about the functional features and the pragmatics of scientific models. [The Modest Semantic View] does not pretend to be a complete account of scientific theories and scientific practice. (ibid., p.269)

For the reasons examined in subsection 2.2.1 and 2.2.2, I do agree with Le Bihan that the *correct* interpretation of the semantic view is the modest one, and that the modest semantic view attains structural-adequacy. However, if the analysis of the view provided in the former sections is correct, Le Bihan's conclusion that the semantic view can not attain functional-adequacy is too strong. In order to argue that the semantic view is capable to *rationally reconstruct* how models accomplish their representational function, let me consider an example provided by Le Bihan.

Suppose that I want to "compare" a tile with a square given that they might share structural features (e.g., the angular separation between the edges). In order to do that, I need first to construct a data model for the tile (e.g., by measuring the tile's angles) which then I can compare with the theoretical model (viz., the mathematical square presented as a structure). So, Le Bihan argues, while formal

[46]Le Bihan is not clear whether the strong interpretation would also achieve pragmatics-adequacy. This is not an issue here, however, as I want to focus on functional-adequacy.

[47]Also, failing the functional-adequacy, the semantic view consequently fails the pragmatic-adequacy as well. I will not purse this line of reasoning further as my interest here in on the account of representation, and hence on the functional-adequacy only.

methods, such as model theory, are a possible tool for analysing the comparison between the two structures at stake, such methods *do not* provide "any way to say whether the procedure that you used to construct your model is a good or a bad one" (ibid., p.265). Now, there are two possible interpretations for this last claim by Le Bihan.

The first interpretation is that the rational reconstruction provided by the semantic view lacks the resources to assess whether the procedure to construct the (data) model for a tile is good or not in *general*. In section 2.2.2, we have seen that the rational reconstruction of the representational function of models is modest in the sense that it provides no more than a "conceptual grinder", sometimes even too coarse, through which to put scientists' experience. Nonetheless, in certain cases, such a grinder is able to be quite specific concerning the procedure to build models.[48] So, just consider the final stages of the theory application within Suppes' hierarchy that we saw in section 2.2.2. The data model is not just a free floating abstract structure, but a structure built according to the theory of experimental design. The theory of experimental design provides us with an ensemble of rules for the test of a specific scientific theory. Such ensemble then tells us whether the procedure we use to construct the (data) model is "a good or a bad one". Moreover, the semantic view provides an *internal* criterion of functional adequacy, that is, a criterion which holds within the very rational reconstruction: the data model has to be construed according to a procedure that does not rule out the possibility to put it in a structural relationship with the theoretical model. If the procedure does not rule out such possibility, then it is *good*. Otherwise, the procedure is *bad*. In this regard it is worth emphasizing that what is required to be a *good* procedure is just the possibility – not the necessity – that it produces a data model that is morphic to the theory model.

The second interpretation of Le Bihan's claim is that the rational reconstruction provided by the semantic view lacks the resources to assess whether a procedure to build the data model for a tile is good or not in a *particular* case. For this sort

[48]There are even cases where the reconstruction successfully captures most part of the scientific practice involved in model construction and application, as shown in the analysis of evolutionary biology models and confirmation by Thompson (1988) and Lloyd (1994).

of assessment we need to appeal to pragmatic elements, such as the specific goals and means sought for by the scientist. Evidently, this implies taking into account elements pertaining to the pragmatic account of scientific models. Accounting for scientists' pragmatic considerations means, according to Le Bihan, to explain scientists' choices as determined by sociological or material factors (the potential interest aroused by the tackled problem, the instrumentation available, the costs, etc.). The possibility for the semantic view to cover these aspects is ruled out by its very advocates (see section 2.2.2). However, as held by Le Bihan herself, functional features can be analysed independently of pragmatic considerations. Therefore the fact that the semantic view is intrinsically unable to account for pragmatic considerations does not undermine its capability of being functional-adequate.

The question as to *whether* a philosophical analysis of scientific theories should cover also the pragmatic considerations influencing the particular case of model construction is worth to be considered. Here I will limit the analysis to what could be a possible reaction by advocates of the semantic view to this question. Lloyd (1994, p.27), for example, holds that an analysis dealing with pragmatic considerations such as how to build "the most appropriate" model to represent the behavior of a particular phenomenon is an *empirical question*. One needs to fully enter the level of actual practice, thus abandoning the perspective of the philosophical analysis in the sense of the "historically oriented philosopher of science" introduced by Suppe in subsection 2.2.2. On the other hand, the determination of "types or categories" of models which are *used* in a certain discipline, the analysis of how they relate in order to form the structure of a theory as well as in its confirmation are *philosophical issues*.

In conclusion, if we mean to show that the semantic view can not provide any account of how models represent, there are two possible ways to go. We can hold a 'strong interpretation' of the semantic view, claiming that the format that it provides for theories and actual practice is set forth as a *faithful description* of theories and of actual practice. Otherwise, we can opt for the modest interpretation of the view – the only possible according to the analysis provided in Section

2.2. Contrary to what held by Le Bihan, the issue related to the use of models can find room within a rational reconstruction, even if it is cashed out mainly in formal terms. What is left out is pragmatic adequacy. However, pragmatic choices as identified by Le Bihan are not within the province of the analysis provided by a rational reconstruction for the reasons examined in subsection 2.2.2. This has been clearly expressed by Suppe: "philosophical problems are *not* scientific problems; thus there is no guarantee that what is adequate for doing science is adequate for doing philosophy of science. To provide an adequate philosophical analysis of science – to explain and provide an understanding of what science does and why it has the epistemic or other philosophical attributes claimed for it – may require philosophical theorizing about things that science itself *need not* to countenance. [...]. To think otherwise is simply to confuse philosophy of science with science" (1989, p.31).

2.3.2 Brading and Landry

In their 2006 paper, Brading and Landry reach a conclusion about the semantic view that is very similar to the one held by Le Bihan: the semantic view is a "methodological stance" (ibid., p.580), which falls short of saying anything about representation. Also in this context, representation is intended as a model-target relationship.

However, in order to justify their claim, Brading and Landry do not appeal to the functional character of representation which is assumed by Le Bihan. Rather, they call for the *ontological significance* of representation: the objects that empirical theories talk about are *particular* objects. So the semantic view, in order to account for representation, should have *access* to particular objects and, according to the authors, it fails in this respect.

Brading and Landry claim that the conception of scientific theorizing as a hierarchy of models which are related to each other through morphism allows us to access only *kinds of objects*, that is: "objects that can be individuated only up to isomorphism as positions in a structure system of a given kind" (p.572). Drawing on the analogy with mathematical structuralism[49], Brading and Landry claim that:

> [A]ccording to the semantic view of scientific theories, theories (regardless of how, or whether, they are formally framed) are to be *characterized as* a collection of models that *share the same kind of structure*, and the *kinds of objects* that the *theory* talks about can be *presented* as positions in such models. (ibid., p. 573)

When we identify kinds of objects via morphisms, we are *presenting* objects. The hierarchy of models put forward by the semantic view surely achieve the presentation-level and, in so doing, it connects theoretical models to data models.

However, the semantic view is a formal analysis provided for empirical theories and objects of empirical theories, contrary to those of mathematics as presented within mathematical structuralism, are *particular* objects. In order for what we say about kinds of objects to be true of particular objects, we need to justify not only the applicability of high level models to data models, but also the *applicability of data models to phenomena*. The problem in this regard is that the semantic view does not provide any justification for the fact that particular objects can be structured as in data models. That is, what we lack in the semantic view is a *theory of phenomena*:

> [W]ithout a theory of the phenomena one cannot formalize (again, by model-theoretic methods) the treatment of the structure of the phenomena in terms

[49]Mathematical structuralism is the philosophical position according to which the subject matter of mathematics is not the intrinsic nature of its objects, but the relations among such objects. Hence the slogan "mathematics is the science of structure" (see Shapiro, 1983, sect.III). So, in arithmetics the number 2 is not a particular object, it is rather a "position" in a structured system which exemplifies the natural-number structure, such as von Neumann's ordinals or Zermelo's numerals. The morphism holding between structure systems of this sort, assuring that the number 2 has the same position in every structure, identifies 2 as a *kind of* object. The properties of the objects will not be that identifying number 2 as a particular object, but those identifying 2 with respect to other objects in the structured system. To use Shapiro's words: "There is no more to being the natural number 2 than being the successor of the successor of 0, the predecessor of 3, the first prime, and so on." (Shapiro, 1983, p.6).

> of data models alone, and so one cannot use the semantic view's account of shared structure between models to fully account for the applicability of a theory to the phenomena and, thereby, to establish a theory-world connection. (ibid., p.575)

A theory of phenomena then would allow to talk about the "structure of the phenomena" which, in turn, allows the theory not only to talk about, but also to *be about* its objects. As soon we get the "structure of phenomena" we then have an account of *representation*, that is: "an account of how a physical theory, that *talks about* kinds of objects, comes to *be about* particular objects" (ibid., 576). Hence the conclusion:

> The question of the reality of a particular physical object and/or the truth of physical propositions cannot be settled semantically, that is, cannot be settled merely by appeal to a Tarskian notion of a model and/or a Tarskian notion of truth: it depends crucially on some *extrasemantic process* whereby the connection between *what we say* and *what there is* is both established and justified. *This is what we mean when an account of representation is required.* (emphasis mine, ibid., p.576)

Since the semantic view relies on structures and structural relations among structures it should be understood as a methodological stance which *only* commits to the claim that:

> [T]he kinds of objects the theory talks about are presented through the shared structure of its theoretical models and that the theories apply to phenomena just in case the theoretical models and the data models share the same kind of structure. No ontological commitment – nothing about the nature, individuality, or modality of particular objects – is entailed. Viewed methodologically, to establish the connection between the theoretical and the data models, [the semantic view] considers only the appropriateness of the kind of structure and owes us no story connecting data models to the phenomena. In adopting a *methodological stance*, we forgo talk of 'the structure of phenomena' and simply begins with data models. (ibid., p. 577)

Brading and Landry then offer two alternative steps beyond this methodological stance, claiming that one of the two must be taken to attempt to account for scientific representation. The fundamental aim of such additional step is to account for what the semantic view can not, i.e., how the data model latches to its target phenomenon.

A first possible step is the one made by van Fraassen (2008) with his *empiricist structuralism*, which entails the acceptance of the famous *pragmatic tautology* that basically collapses the phenomenon with the data model. Indeed, this empirical stance asserts a sort of identity between the data model and its target phenomenon by assuming that a theory is adequate to the phenomenon if and only if the theory "is adequate to the phenomenon as represented, i.e., as represented by *us*" (van Fraassen, 2008, p.259).

The second possible step beyond the methodological stance is offered by *structural realism*. French (2000) and Ladyman (1998) defend this stance by resorting to a 'no miracles' argument: if there was no shared structure between the data models and their target phenomena, then the success of science would be a miracle. Although different variants of structural realism exist, all assume that the kinds of objects presented by the successful theory adequately represent the structure of the particular objects of which the target phenomenon consists – the various forms of structural realism being distinguished on the basis of how they claim that representation is obtained.

So, Brading and Landry end up claiming that unless we somehow impose a structure on phenomena – either by identifying phenomena with their associated data models or by directly assuming that phenomena come in structures – we cannot give an account of scientific representation, and that therefore the semantic view cannot account for scientific representation. I contend that both these implications are unwarranted. My argument is that scientific representation need not be framed exclusively in structural terms and, in addition, that there is more in the semantic view than just a series of structural relationships. Let me better clarify these two points.

As discussed in subsection 2.2.3, both Suppe and Suppes have been quite clear about the impossibility for the semantic view to deal with actual beings – i.e., with particular objects, in Brading' and Landry's terminology. This, however, does not prevent the view from providing a rational reconstruction of how theories are used to represent phenomena. More precisely, my point here is that a rational reconstruction is an account of representation that tells us how data models latch to their target phenomena, but it does so without dealing with any particular object of any particular target phenomenon. To put it differently, a rational reconstruction can be an account of scientific representation in Le Bihan's sense of functional-adequacy (see subsection 2.3.1) without any structuralist commitment.

A data model is not a randomly chosen abstract structure. It is built according to a precise theory of experimental design and with the specific aim of making it potentially morphic with the theory model. The information gathered from the target phenomenon by means of experimental procedures is always collected having a precise theory model in mind. Can we explain this procedure of building data models? My answer is yes. The explanation comes from the formalization of theories provided by the semantic view given in subsection 2.2.1 and their actual use discussed in subsection 2.2.2. A data model is an instrument for a precise aim: to test whether the theory model successfully represents its target phenomenon. Such an aim, which has to be supposed for all data models in general and not just for some data models in particular, naturally gives rise to an internal criterion for evaluating whether a given procedure followed to build a data model is bad or not. Such internal criterion is directly implied by the structural nature of the formalization of theories given by the semantic view: a procedure is definitely bad when independently of the data collected it necessarily produces a data model that can not be embedded in the theory model.

So, the semantic view can explain why data models are built as they actually are, at least in general terms. Is this enough to account for scientific representation? My answer here is a *weak* yes, which requires some specifications. If I required, as done by Brading and Landry, an account of scientific representation to be based only on structural relationships, then I should have agreed with them that the semantic view can not account for representation at all. Also, I would have reached a similar conclusion if I had restricted the scope of the semantic view to the formalization of theories in structural terms, as there would have been no room in the view to say anything on the relationship between the data model and its target phenomenon. However, according to my view, both antecedents of these claims are false. It is true that to account for scientific representation is to explain how (data) models latch to their target phenomena, but this need not be done exclusively in structural terms. In fact, explaining why data models are built how they actually are improves our knowledge of how models latch to their target phenomena, at least in the weak sense of rationalizing the behavior of scientists and their actual practices. Further, as discussed in subsection 2.2.2, the semantic view does consider scientific practices, and it even uses actual practice as a criterion to evaluate the goodness-of-fit of its own reconstruction of scientific theorizing. In particular, the view can help to explain why scientists are forced to extract information from the phenomenon and to put it in the structural form of the data model.

2.4 Second charge: ...And if it does, it implies structuralism

There is another objection that can be put forward against the semantic view when it comes to scientific representation. The objection does not amount to an explicit charge. It rather stems from the critical remark made by several authors in the literature that, unless the semantic view assumes a "privileged relationship" between the (hierarchy of) models and their tagets, the applicability of the former to the latter is left unjustified (Frigg, 2006, Giere, 1999, Knuuttila, 2014, Suárez, 1999b).[50] Drawing on some remarks by van Fraassen about the relationship between the semantic view and the general concept of structuralism, I argue that the critical remark above could be interpreted as a charge against the semantic view

[50]Evidently, this criticism can be conceived as such only by those who, like myself, consider ontological commitment as detrimental for an analysis of scientific representation.

to imply a form of (ontic) structural realism about representation. Assuming that the above observation could be so interpreted, my purpose in this section is to examine whether the semantic view, in its original formulation, is the right target for this charge.

Structuralism in the philosophy of science is a term with as many meanings as the subject matter at stake. Here, I will consider the general meaning of structuralism as nicely formulated in a recent paper by Frigg and Votsis (2011): "Generally, a structuralist perspective is one that sees the investigation of the structural features of a domain of interest as the primary goal of enquiry".

The semantic view, as an analysis of theories which privileges the formulation of theories as model-theoretic structures, can easily be interpreted as a structural perspective on theories.[51] Van Fraassen explicitly recognizes the *possibility* of such an interpretation, claiming that:

> By common, if often tacit, consent among [advocates of the semantic view], the semantic approach is the current form of the general idea of structuralism. [...] According to the semantic approach, to present a scientific theory is, in the first instance, to present a family of models – that is, mathematical structures offered for the representation of the theory's subject matter. Within mathematics, isomorphic objects are not relevantly different; so it is especially appropriate to refer to mathematical objects as "structures". Given that the models used in science are mathematical objects, therefore, scientific theoretical descriptions are structural; they do not "cut through" isomorphism. So the semantic approach implies a structuralist position: science's description of its subject matter is solely of structure. (1997, pp. 522-523)

When the semantic view is assessed *as* an account of scientific representation, the possibility of its structural reading turns out to be potentially problematic. Even

[51]A former case of a rational reconstruction of theory structure which has turned out to be fruitful for structural considerations is the Ramsey-sentence approach to theories (see Worrall, 2007, Worrall and Zahar, 2001).

assuming a deflationist attitude and avoiding a definition of representation, it is clear that in the context of philosophy of science we interpret representation as a way of gaining knowledge via the medium at stake (Morrison, 2008, Suárez, 1999a, van Fraassen, 2007). So, as long as representation is assumed to be the relationship between (a hierarchy of) models and a target system, representation-as-application is also loaded with an epistemic value: it is by (construing and) applying models that we gain knowledge of the phenomenon, or kinds of phenomena, of interest.[52] Borrowing an example from Lloyd (1994, p.145), we can say that when a population geneticist claims that a Mendelian model is applicable to, conforms to, or represents a natural or a laboratory population, she is claiming that aspects of the population system (e.g., the distribution of genes frequencies) are explained because the system is isomorphic in certain respects to the model.

This view of representation which can be drawn from the semantic account has undergone several criticisms, most of them focusing on the following reading of the claim above: the model applicability requires a form of morphism which is a relation proper of mathematical entities, such as structures. So, either we claim that it holds among models and *structured versions of* targets (supposedly, models of data), or we assume, using van Fraassen's words: "that nature has itself a relational structure in precisely the same way that a mathematical object has a structure" (2006, p.539). In the first case, we are simply claiming that we can gain knowledge of reality via morphism between mathematical structures and it is in this context that objections such as Brading and Landry's arise. In the second case, we are considering whether the semantic view, in order to gain any epistemic access to reality, needs to resort to a form of realism about structure.

This second interpretation is not fair of the semantic view in its original formulation. As we have seen in Section 2.2 (and, in particular, in subsection 2.2.3), all advocates of the view are fundamentally neutral with respect to realism. More precisely, none of the advocates of the semantic view in its original formulation

[52]This is the view that representation is among the primary aim of science, see van Fraassen (1980a, 1987), Friedman (1982, Ch.6), Kitcher (1983), Giere (1988, 1999), Morgan and Morrison (1999), Cartwright (1999a), and Morrison (2007, Ch.2). On the other hand, authors such as Hacking (1983), Peschard (2011), Kennedy-Graham (2012) and Knuuttila (2011) resist the representational interpretation of science.

would assume a realist commitment to reality. So, the second interpretation seems to be quite misleading.

The possibility for such a misinterpretation might be traced back to two sources. The first is van Fraassen's 'bewilderment' that we analyse in section 2.2.3 and which led him to formulate the model-target relation in metaphysical terms. The second is the interest shown by supporters of *structural* realism for the semantic view, conceived as an hospitable environment for their stance. In particular, the semantic view is presented by Ladyman as a "natural framework" (Ladyman, 1998, p.411) for the ontic structural realism[53], which in the mentioned paper makes its debut:

> I will argue that structural realism gains no advantage over traditional scientific realism if it is understood as merely an epistemological refinement of it, and that instead it ought to be developed as a metaphysical position. I explain why the semantic approach to scientific theories offers the natural framework for this, and what a metaphysical structural realism must involve if it is to do justice to the intuition behind the no-miracles argument. (Ladyman, 1998, p.411)

This viewpoint has then been further developed by French and Ladyman (1999) and French and Saatsi (2006). Even more recently, Ladyman (2014) presents ontic structural realism as a form of structural realism motivated by several problems, among which, he counts scientific representation and, in particular, the role of

[53]As it is well known, Ladyman (1998) shapes his ontic structural realism in contrast to the pre-existing structural realism put forward by Worrall (1989), which he dubbed *epistemic*. For a thorough analysis of the relationship between these two forms of structural realism, with attention whether the ontic form should be actually be preferred to the 'traditional' epistemic one, see Morganti (2011). In his (1998) paper and in the entry on structural realism (2014), Ladyman distinguishes the two forms of structural realism as follows. *Epistemic structural realism* (*ESR*) states that structural properties identify our epistemic boundaries. The scientific realists' commitment towards theoretical entities slides into the commitment towards the relations instantiated by (observable and unobservable) entities - whilst remaining agnostic about the nature of theoretical entities. Roughly speaking, the *ESR* main thesis is that we know solely the relational structure of things and not the things themselves. Instead, a "crude statement of [*Ontic structural realism* (*OSR*)] is the claim that there are no 'things' and that structure is all there is" (Ladyman, 2014).

models in physics. So my conjecture is that the semantic view and its "inherent structuralism" (French and Saatsi, 2006, p.549) have been used not only as a good framework for ontic structural realism, but as a proper tool to justify its metaphysical stance. The "Poincaré Manoeuvre" described by French makes the intention clear: "we begin with the standard presumption that theories are committed to objects, at least as the subjects of property instantiation; we then reconceptualise or [...] eliminate those objects in structural terms." (French, 2012, p.23). If the semantic view is conceived as the tool allowing for such a manoeuvre, this would turn the semantic view into the 'epistemic heart of structural realism', in the same way as the syntactic view was for the Positivism. In other words, the step from "being particularly appropriate for a structural realist stance" to "implying a structural realist stance" is somehow taken by structural realists.

However, according to the analysis of the early formulations of the semantic view provided in Section 2.2 (subsections 2.2.1 and 2.2.2), the implication above is unwarranted: the view can account for representation – in the sense of a rational reconstruction of it – without requiring any ontological commitment. Let me clarify this point in further details.

The semantic view accounts for representation by providing a rational reconstruction of how scientists actually use models to represent. Such an account relies on two crucial elements. The first element is the development of a characterization of theories that is fundamentally structural, and which is based on the idea of a hierarchy of models going from the most abstract to the most concrete. Admittedly, the very structure of the hierarchy of models is built by means of structural relationships among its constituent parts. But the hierarchy of models also provides the boundaries for the application of structural relationships. No structure or structural relationship is assumed outside the hierarchy of models. And here is where the second element crucially comes in: the semantic view is not indifferent to what scientists do in practice with models and, moreover, the view tries to explain scientists' behavior on the basis of its own formalization of theories. Although the view does not aim at accounting for the intentions or aims of any single scientist, it provides good reasons why a data model exists, and why it comes in a structural form. It is because any scientist who attempts to relate a theory to a phenomenon must, first of all, construct a data model of the phenomenon, basically creating a structure that he can then put in a structural relation with the theory model. To assume that reality is already in structural form, ready for the scientist to be translated into the data model, would indeed mean to commit to structural realism. But the semantic view does not assume this. It assumes that the scientist is forced by the very way in which theories are formulated, to extract information from the phenomenon and to put it in the structural form of the data model. Note that this does not happen by chance: the way in which the data model is structured crucially depends on the models that occupy a higher level in the hierarchy.

2.5 Keeping the semantic view alive

In this chapter I have examined two charges against the semantic view of scientific theories. Both the charges are based on the fact that the semantic view is an analysis of theories focusing mainly on models as structure. The first criticism questions the possibility for the analysis provided by the semantic view to account for representation. The second criticism questions whether the semantic view could be an account of representation without implying any form of structural realism about representation. Drawing on an analysis of some features of the semantic view put froward in the first part of the chapter, I have tried to give some reasons to resist both the criticisms.

I have considered as illustrative examples of the first charge the recent papers by Le Bihan (2012) and Brading and Landry (2006). What made their contributions particularly relevant for my analysis is that I share with the three authors the conviction that the semantic view is eminently a rational reconstruction and that we use this very conviction to reach opposite conclusions. While Le Bihan, Brading and Landry conclude that this character of the view prevents it from being able to accommodate representation, I argue that, within the boundaries of rational reconstruction (which I have traced out in the first part of the chapter), the view does provide an account of representation and of the scientific practice by means of which it is carried out.

Le Bihan assumes that a necessary condition for the view to account for representation is that it must be able to tell us when the procedure followed by a scientist to construct a data model is "good" or "bad" for a given phenomenon. As pointed out in my analysis of the rational reconstruction of scientific practice as provided by the semantic view, data models are not free floating structures, rather they are built according to the so-called theory of experimental design. Therefore, contrary to Le Bihan, I argue that the semantic view is capable of assessing the procedure followed by a scientist to construct data models. This is indeed possible in two weak senses. First, in the sense that it can be established if the followed procedure is consistent with the theory of experimental design. Second, in the sense that the procedure can yield a data model that is potentially – but not necessarily – morphic to the theory's model. This is enough to fulfill the necessary condition for a (functional) adequate account set by Le Bihan.

Brading and Landry, on the other hand, assume that representation can not be accounted for unless we impose a structure on phenomena, either by identifying phenomena with their associated data models, or by directly assuming that phenomena come in structures. Since they (correctly) argue that the semantic view is silent on the structure of target phenomena, they conclude that the view cannot account for scientific representation. My counter-argument runs as follows: a rational reconstruction is an account of representation that tells us how data models latch to their target phenomena, but it does so *without* dealing with any specific component of any particular target phenomenon. What we might expect from a rational reconstruction of scientific representation is to rationalize scientist's behavior in scientific practice, and this presupposes in a weak sense an explanation of how models latch to their target systems.

The second charge is more subtle, as it allows for the semantic view to be an account of representation, but only at the price of implying a form of structural realism about representation. Such criticism seems to be potentially supported by the idea that the semantic view is a hospitable framework for structural realism. I have argued that this conjecture is both historically misleading and unwarranted. By appealing to the neutrality of the semantic view, which I have defended in the first part of the chapter, I have argued that the view programmatically avoids to 'create an environment' which could be hospitable for any epistemic stance to be developed. Moreover, such charge is unwarranted. The view can account for representation – always within the boundaries of a rational reconstruction – without requiring any ontological commitment. Indeed, to assume that reality is already given as structure (ready for the scientist to be translated into a data model) would mean to commit to structural realism. But the semantic view does not opt for this assumption. Instead, it assumes that the scientist is forced by the very way in which theories are formulated (and, more precisely, by the hierarchy of models), to extract information from the phenomenon and to put it in the structural form which is typical of the data model.

Chapter 3

A Misleading Use of Structure: An Example

3.1 Structural approaches

There is by now a long tradition of structural approaches to scientific representation, starting in da Costa and French (1990), French and Ladyman (1999), Bueno et al. (2002), da Costa and French (2003), Bueno and French (2011) to the most sophisticated recent accounts by Bartels (2006) and Pincock (2012). The tradition's critics (Contessa, 2011, Frigg, 2006, Giere, 1999, Suárez, 2003, van Fraassen, 2008) have invoked putative counterexamples to structural notions, displaying instances of scientific modeling where a model $\mathbb{B}$ is accepted as a representation of some object, system or process $\mathbb{A}$, while failing to hold the required structural morphism relation to $\mathbb{A}$. As a response, defenders of structural accounts have progressively weakened their constraints, from full isomorphism to embedding, partial isomorphism and, most recently, to homomorphism. (Van Fraassen was both an early proponent, and is nowadays a critic, at least in the terms defended here.)

It is unclear in these papers what precise claims are being made on behalf of structural mapping or morphisms, and what exactly is the work that structures are supposed to perform. More worryingly, perhaps, the notion of structure itself remains imprecise and elusive. But whatever else is claimed on behalf of structural morphism, it is clear that the point of providing a structural account of representation is to provide some elucidation, however partial, of the central notion of scientific representation. Hence we shall take it that any structural account of representation is minimally committed to the claim that representation in science is a relation that is appropriately characterized or described as a kind of structural mapping or morphism. And indeed most authors in the tradition have invoked structural isomorphism and its variants as part of an *analysis* of representation. Thus for instance, it is claimed that:

> [T]o understand how an organism performs well using a certain representational system we have to consider the specific contents of the representation and how they relate to its reference objects. Content is a necessary component of representation, and homomorphisms are necessary to explain this necessary component. (Bartels, 2006, p.17)

The evidence for these claims and their reach remains nonetheless surprisingly unclear. It is in particular often unclear, as we shall point out in this article, whether isomorphism and its cousins are intended to provide an analysis of the notion of representation itself, or whether they are merely intended to describe some of the ways in which representation in science achieves some of its characteristic ends, such as for instance, the aim of accuracy. In other words, it is unclear whether structural mappings or morphisms are constitutive of representation in science, or merely some efficient means for representation to achieve its ends. Defenders of the structural accounts are often irritatingly imprecise in shifting from evidence for the weaker case to claims in favor of the stronger constitutive claim. But the inference from the former to the latter claim is invalid, since the problem of representation and the problem of accurate representation are by now well-known to be distinct (Callender and Cohen, 2006, Contessa, 2007, Frigg, 2006). We believe that there is so far no good argument to the effect that the evidence for the weaker claim (that structural morphisms are typically involved in the assessment of the accuracy of many mathematical representations in science) is also evidence for the stronger claim (that structural morphisms are constitutive of the nature of scientific representation, i.e., that a structural account of representation is correct). There are powerful independent arguments against the stronger claim (see Frigg, 2006, Suárez, 2003) that recommend a skeptical attitude to structural accounts of scientific representation in general.

In this paper we analyze the most sophisticated and plausible structuralist account of representation to date, namely Andreas Bartels' (2006) homomorphism account. The account's main virtue is the alleged capacity of homomorphism to account for the phenomena of misrepresentation, and indeed we believe this to be one of the greatest stumbling blocks for structural accounts. Hence we begin in Section 3.2 by reviewing the problem of misrepresentation in scientific modelling, in both the mistargetting and inaccuracy varieties. As an illustration of the latter, we briefly discuss the essential features of an elementary yet influential historical case of scientific modeling: the billiard ball model of gases. We argue that there are three ways in which scientific models typically misrepresent, and we refer to them as abstraction, pretence and simulation. We provide bare structural characterizations for all of them in terms of simple structural renditions of their representational sources and targets. We argue on the basis of the billiard ball model that scientific models abstract, many pretend, and some simulate; but that this does not take away any of their descriptive, predictive and explanatory value. Then in Section 3.3 we summarize Bartels' homomorphism theory of representation and review his claim that this theory accounts for misrepresentation. We point out the essential role adjudicated by Bartels to what he calls the "representational mechanism". Representational mechanisms have a crucial role for representation (and misrepresentation) to occur and, being these mechanisms independent of any structural mapping, we argue that misrepresentation is not accounted fully in structural terms. This particularly holds for mistargetting as presented in (Suárez, 2003). In Section 3.4 we dispute the claim that misrepresentation as inaccuracy is accommodated within Bartels' structural account. We first argue that what Bartels calls *homomorphism* is in fact a stronger notion, namely *epimorphism*. We then show that epimorphism can not account for either abstraction, pretence or simulation. Turning to homomorphism proper, which is an extremely weak structural constraint, amounting to the relation technically known as *completeness*, we show that it can accommodate pretence kinds of misrepresentation, but not abstraction. Since we argue that most if not all scientific models abstract, it follows that even the weakest notion of structural morphism is too strong for scientific representation. The formal result is summarized in Table 3.2. In Section 3.5, we admonish philosophers to take greater care with structural accounts of representation – while structural morphisms may provide good and valuable resources to assess the accuracy of many mathematical models in science, they can not actually account for the very relation of representation.

3.2 Misrepresentation: Mistargetting and inaccuracy

"Scientific representations misrepresent": This is one of the main points of agreement in the recent literature on scientific representation. Any philosophical theory or account of scientific representation must not only accommodate but also explain minimally how representations fail to accurately characterize or describe their entire subject. Representations always simplify to some degree: this is at the heart of why they are useful in practice.[1] Thus it would be a major objection to any philosophical account of representation that it does not account for misrepresentation. This has often been an issue for structural accounts – since on such accounts the conditions for the accuracy of a representation (the 'matching' of relations and properties at the source and target end) are also the very conditions for establishing the relationship of representation in the first place. This is most evident an objection to isomorphism accounts, and the relevant question for us is the extent to which the objection can be answered by means of suitable weakenings of the isomorphism relation. Yet, there is a stronger form that the objection may take, which we would like to consider in this section.

[1]Jorge Luis Borges' wonderful discussion of the one-to-one map is an exemplary parody of how a perfectly accurate representation is also perfectly useless (Borges, 1954).

The stronger objection begins with the observation that there are distinct forms of misrepresentation and that these pose significantly different challenges for structural accounts. Two main kinds were already identified in Suárez (2003) and referred to there as *mistargetting* and *inaccuracy*. A model may misrepresent by being applied to the wrong target, perhaps as a result of having been mistakenly taken to be a different model in some particular context. The model's target is selected as part of the normative practice of model building that gives rise to it, but a particular agent may, perhaps out of lack of information or competence, apply it to the wrong target. The model is in that very context misrepresenting in a rather strong sense: it is used as a representation of a system or object that it was not intended for. We return to the issue later on in addressing whether Bartels' account actually provides necessary and sufficient conditions for representation, and whether these conditions can in some sense be thought to be 'structural'. For now, we focus only on the varieties of inaccurate misrepresentation. More specifically we discuss three forms that inaccuracy can take, and which we refer to as abstraction, pretence, and simulation.

The rough and ready definition of these terms is as follows: An abstraction essentially neglects some of the features of the target system it is about; a pretence ascribes to the target system features that this does not possess; a simulation both abstracts and pretends: it both neglects some of the actual features of the system and ascribes features to the system that it does not possess. We discuss these distinctions in relation to one of the best known and most widely discussed examples of an analogical model in the history of philosophy of science, namely the so-called 'billiard ball model' (Hesse, 1970). Hesse presents this model as consisting of a negative, positive and neutral analogy between macroscopic billiard balls and gas molecules in a container. Thus in her famous dialogue between the Duhemist and the Campbellian, the Campbellian lists the properties of billiard balls and classifies them in three groups in relation with the analogy with gas molecules. In the negative analogy (the properties that pertain to billiard balls but not gas molecules) there are color, hardness, brightness; in the positive analogy (the properties that billiard balls and gas molecules share) there are motion and impact. But there is a third group of properties that constitute what Hesse calls the neutral analogy.

These are in Hesse's Campbellian words, "the properties of the model about which we do not yet know whether they are positive or negative analogies: These are the interesting properties, because [...] they allow us to make new predictions." (Hesse, 1970, p.8).

Now, Hesse does not describe them, but there is a further group of properties of interest in the analogical relationship between billiard balls and gases; these are the properties of the gas that are most definitely not properties of billiard balls. For instance, the billiard ball model captures microscopic features of elastic collisions between gas molecules to some extent, but it does not say anything informative regarding the macroscopic features of the gas, such as volume, density and pressure. We find ways to draw inferences to those macroscopic properties from the fully developed kinematical theory of gases, but there are no correlates in a system of billiard balls for such properties. What's more, the billiard ball model is positively misleading as a guide for such properties, since there is no relation in a system of billiard balls between average speed of the balls and the pressure exerted outwards by the system. Obviously the missing ingredient is free expansion, which is a thermodynamically irreversible property of any system of gas molecules, but has no equivalent or corresponding property in any dynamical feature of elastic collisions between classical particles or massive bodies, such as billiard balls. We could call this the 'inverse negative analogy' (or negative analogy 'by denial'): they are the properties that pertain to gas molecules but not billiard balls. They may even be explicitly denied for billiard balls (as indeed is the case with free expansion).

In fact, as some careful reading will reveal, the inverse negative analogy is of particular relevance in Campbell's original discussion of the example (see Campbell, 1957). And there is some sense to this. Hesse had her own reasons to suppress the discussion of the inverse negative analogy which could only take away from the neutral analogy which she deemed fundamental. It is well known that her chief aim was to defend the thesis that the neutral analogy was key to the heuristics of research, and fully informed its logic. Campbell, however, was mainly preoccupied with the relation between theory and measurement, and more particularly with the theoretical presuppositions underlying measurement procedures. In this context the inverse negative analogy is relevant, for the macroscopic thermodynamic properties in question are measurable in the laboratory, while the internal microscopic properties of the gas can only by hypothesized or inferred from observation via the model.

There are further interesting differences between Hesse's discussion of Campbell's example and Campbell's original discussion. Perhaps the most striking is that Campbell never actually employs the term "billiard ball model". In fact, he does not refer to billiard balls once! His analogy is more generally with a system of perfectly elastic macroscopic balls – and, of course, billard balls are an approximate instance of these, even though they are not in reality perfectly elastic. But the analogy is fit for most relevant purposes, since it captures some essential aspects of the relationship between the laws that apply to both gas molecules and macroscopic yet point-size elastic balls. As Campbell writes: "The propositions of the hypothesis of the dynamical theory of gases display an analogy [...] to the laws which would describe the motion of a large number of infinitely small and highly elastic bodies contained in a cubical box." (1957, p.128). There are however some important points of difference where the model most definitely goes astray, and they can not be understood to be part of Hesse's negative analogy, since they comprise properties of the gas molecules that the model fails to describe correctly altogether. These properties, which comprise what we refer to as the inverse negative analogy, include free expansion, but also thermal conductivity, and viscosity. As Campbell puts it: "The relation predicted [between pressure, density, and temperature of the gas and its viscosity] does not accord with that determined experimentally; in particular it is found that the theory predicts that the coefficient of viscosity will be be determined by the size and shape of the containing vessel, whereas experiment shows that it depends, in a given gas, only on the density and temperature." (ibid., p.134).

While there is no space here to discuss the details fully, the considerations above already suggest the following distinctions with respect to the ways in which the elastic macroscopic balls model misrepresents gases. First of all, there are all the properties of the model elements which are missing in the gases: they constitute the negative analogy in Hesse's terms. Thus billiard balls are shiny and hard, but gas molecules are not (they are neither hard nor soft; neither shiny nor opaque). We may then say that the model *pretends* with respect to its target system. It may seem easy to discharge these properties by simply redefining the model to include only the positive and neutral analogies. Thus, one may insist, the analogy is not meant with billiard balls per se but with constructs that are like billiard balls except in those respects in which billiard balls are positively unlike gas molecules. But there are a number of problems with this strategy, some of which were already discussed by Hesse. For a start, the move is of course circular as a definition of the function of the analogy – since it requires us to already have a hang on those properties that are not actually analogous. And things get even worse when we notice that there are also properties of gases that the system of elastic balls – whether or not billiard balls – can not possibly be said to have, including thermal conductivity, viscosity and free expansion. This is the inverse negative analogy we are emphasizing here and we may say that the model *abstracts* in this case. The analogy as based upon the model denies that the gas has these properties. In some cases the model even positively misleads regarding the character of such properties in the gas. If we consider viscosity in the example above, we see that the fact that the model fails to describe it correctly depends on the fact that it abstracts from density and temperature on which viscosity actually depends. Instead, according to the model, viscosity rather depends on properties the billiard ball model pretend about, such as the size and shape of the containing vessel. In these cases we concurrently abstract and pretend about a property of the target system, thus lying about it. We then say that a model *simulate* its target whenever it is deceptive in this sense about it. As Campbell insists, the model analogy is not to be considered a mere heuristics in the development of a new theory, but must be understood to be part of the theory itself: "It is often suggested that the analogy leads to the formulation of the theory, but that once the theory is formulated the analogy has served its purpose and may be removed and forgotten. Such a suggestion is absolutely false and perniciously misleading" (1957, p.129).

Thus we must take seriously that models misrepresent by abstracting away, and thus ignoring, certain properties of the target system (escape velocity), by pretending that certain properties of the target system do obtain which actually do not (hardness and shine) and by simulating, that is, by misleadingly denying that some properties obtain which in fact do (viscosity, thermal conductivity). What's more for some and the very same elements, a model will typically both abstract with respect to some property, and pretend with respect to some other. In other words, the representation by models will typically involve both ignoring certain properties that do obtain and postulating other properties that do not obtain even for the very same sets of elements in the domain of the model.

Now, let us attempt to represent these distinctions somewhat more formally, in what we regard as a hospitable framework for structuralism, which assumes that there are uncontroversial structural representations of both source and target. This is a strong assumption, but without which the structuralist conception of representation does not even get off the ground. Thus consider a model and its target as two *relational structures*, $\mathbb{B} = \langle B, (R^B)\rangle$ and $\mathbb{A} = \langle A, (R^A)\rangle$, with their own domains of individuals, A and B, and the sets of relations defined over the domains: respectively (R^A) and (R^B). $\mathbb{A}$ and $\mathbb{B}$ are assumed to be *similar* structures: while the elements of A and B may be different, the corresponding relations in R^A and in R^B have the same number of arguments (Dunn and Hardegree, 2001, p.10). We use the bar symbol for tuples of elements of A and B: $\bar{a} = (a_1, ..., a_n) \in A^n$ and $\bar{b} = (b_1, ..., b_n) \in B^n$.

We say that a model $\mathbb{B}$ abstracts some property $R^A_j \subseteq A^n$, $j \in \{1, \ldots, m\}$, of a target system $\mathbb{A}$ if and only if there exists $\bar{a} \in A^n$ such that $R^A_j(\bar{a}) \wedge \neg R^B_j(f(\bar{a}))$, with $R^B_j \subseteq B^n$ being the corresponding relation of R^A_j in $\mathbb{B}$ and f being a mapping from A to B. The abstracted properties are in the inverse negative analogy, or negative analogy by denial. We then say that the model $\mathbb{B}$ pretends some property $R^B_k \in B^n$, $k \in \{1, \ldots, m\}$, of the target system $\mathbb{A}$ if and only if there exists $\bar{b} \in B^n$ such that $f^{-1}(\bar{b}) = \bar{a} \in A^n$ and $\neg R^A_k(\bar{a}) \wedge R^B_k(f(\bar{a}))$, with $R^A_k \subseteq A^n$ being the corresponding relation of R^B_k in $\mathbb{A}$. The pretended properties are typically in the negative analogy as originally discussed by Hesse. Finally, we say that a model $\mathbb{B}$ simulates a target $\mathbb{A}$ when it both abstracts and pretends some properties of the same elements of A and of their images in B; formally, for some tuple $\bar{a} \in A^n$ with $\bar{b} = f(\bar{a}) \in B^n$, some $R_j^A, R_k^A \subseteq A^n$ and $R_j^B, R_k^B \subseteq B^n$, it is true that $R_j^A(\bar{a}) \wedge \neg R_j^B(f(\bar{a}))$ and $\neg R_k^A(\bar{a}) \wedge R_k^B(f(\bar{a}))$.

We have argued in this section, by appeal to a well-known foundational example in the literature, that models typically simulate their targets, by both abstracting some of their properties away and misleading asserting some of the properties they do not actually possess. We next turn to the best candidate we know for a structuralist conception of representation, namely Bartels' homomorphism theory, and argue that it can not accommodate these features.

3.3 Bartels' homomorphism theory and the 'representational mechanism'

The main tenets of a structural account of scientific representation can be summarized as follows: (i) model sources and their targets exemplify, instantiate, possess or at any rate may be described as relational structures in the sense of mathematical logic, or set-theory; (ii) a model represents a target system only if the relations in the target are partially or totally transferred to the model via some sort of morphism.

We have provided a definition for relational structure in the previous section. The transfer required by condition (ii) is accomplished by some function $f : A \rightarrow B$. In model theory a twofold role is ascribed to f. As a *mapping*, f assures that each individual in A has one, and only one, corresponding element (an *image*) in B. But in addition, as a *morphism*, f is a structure preserving mapping and it assures that related objects possess related properties. The existence of a morphism between the model and its target is what the advocates of the structural approach take to be the condition for representation: *a model* $\mathbb{B}$ *represents a target system* $\mathbb{A}$ (*if and*) *only if* $\mathbb{A}$ *and* $\mathbb{B}$ *are morphic structures.*

Full isomorphism is sometimes advocated as the basic morphism between structures. For $f : A \to B$ to be an isomorphism, several conditions need to be met. First, $f : A \to B$ must be a bijective function, that is, for every $b \in B$ there exists an $a \in A$ such that $f(a) = b$ (also known as surjectivity) and, for every $a, a' \in A$, if $a \neq a'$ then $f(a) \neq f(a')$ (injectivity). Second, for all j and all elements a_i of A: $R_j^A(a_1, ..., a_n)$ if and only if $R_j^B(f(a_1), ..., f(a_n))$. In other words, all relations in $\mathbb{A}$ are transferred to $\mathbb{B}$ so that the two structures are relationally identical, in the sense that the properties they define have identical features. (The structures themselves are obviously not identical since their domains contain different elements).

The idea that isomorphism may constitute representation has been criticized on several grounds. There are first of all urgent questions regarding the fundamental assumption that model sources and targets are or may be said to possess structures. For instance, van Fraassen (2008) suggests that isomorphism alone cannot serve as a condition of representation because, he argues, the structure $\mathbb{A}$ is a "relevant mathematical representation" (ibid, p.243) of the target system to be represented only by construction. That is, we must first of all "choose" a domain of elements A and a set R^A of relations for it. The claim that a model $\mathbb{B}$ is isomorphic to $\mathbb{A}$, which allows to use $\mathbb{B}$ as a representation of $\mathbb{A}$, thus depends on the former act of construction of $\mathbb{A}$ which is essentially a conventional and pragmatic act.

Another class of objections, raised by Suárez (2003) and reiterated by Frigg (2006), undermine the attempt to reduce representation to the relation of isomorphism, irrespective of whether the fundamental assumption that model sources and targets are structures or may be described as such. Thus the *logical argument* shows that isomorphism and representation do not share logical properties: while isomorphism is reflexive, symmetric and transitive, representation is non-reflexive, non-symmetric and non-transitive. The *non-sufficiency* and *non-necessity* arguments show that representation may fail to obtain when isomorphism holds (non-sufficiency), and may obtain when isomorphism does not (non-necessity). Finally the *misrepresentation argument* appeals to the already mentioned fact that *inaccuracy* is intrinsic to all scientific representation, while isomorphism seems to leave no room for either incomplete or incorrect representation.

In response to these objections the advocates of the structuralist account have proposed weakenings of the isomorphism relation. Following this direction, Andreas Bartels (2006) suggests that homomorphism will serve to overcome at least the misrepresentation objection. Roughly speaking, what allegedly makes homomorphism immune to the criticisms undermining isomorphism is the fact that homomorphism allows some parts of the model not to have any counterparts in the target, thus leaving the necessary room to account for inaccurate representation.

Bartels explicitly endorses the structural account of representation when he claims that homomorphism is a necessary condition for representation: "something, $\mathbb{B}$, can represent something, $\mathbb{A}$, only if some structure of the represented domain $\mathbb{A}$ is transferred to its image $\mathbb{B}$" (ibid., p.7) and that: "$\mathbb{B}$ represents $\mathbb{A}$ only if $\mathbb{B}$ is a homomorphic image of $\mathbb{A}$" (ibid., p.8). The homomorphism account of representation advocated by Bartels in fact comprises two parts. One part is purely formal, and it treats homomorphism model-theoretically. The other part concerns the application of the concept 'being homomorphic to' and claims that this concept is extensionally equivalent to 'to represent *potentially*'. Both the formal and the extensional analyses of homomorphism provided by Bartels play a role in his attempt to show that homomorphism accounts for misrepresentation, so let us look at them in turn.

According to Bartels' definitions, the following three conditions must obtain for two similar structures $\mathbb{A} = \langle A, (R^A)\rangle$ and $\mathbb{B} = \langle B, (R^B)\rangle$ to be homomorphically related: for all j, all $(a_1, ..., a_n)$ in A^n, and all $(f(a_1), ..., f(a_n))$ in B^n:

$$\begin{array}{lcr} \text{Completeness:} & \text{if } R_j^A(a_1, ..., a_n), \text{ then } R_j^B(f(a_1), ..., f(a_n)) & (1) \\ \text{Faithfulness:} & \text{if } R_j^B(f(a_1), ..., f(a_n)) \text{ then } R_j^A(a_1, ..., a_n) & (2) \\ \text{Surjectivity:} & \text{for every } b \in B, \text{ there exist } a \in A \text{ such that } f(a) = b & (3) \end{array}$$

The condition of surjectivity on f assures that all the elements in B are images of one or more element in A. Completeness rules out the possibility that there is a relation in $\mathbb{A}$ which has not a counterpart in $\mathbb{B}$, so that the information that $\mathbb{B}$ provides about $\mathbb{A}$ is complete. On the other hand, faithfulness rules out that there is a relation in $\mathbb{B}$ which has not a counterpart in $\mathbb{A}$, so that $\mathbb{B}$ provides a faithful snapshot of the relational framework in $\mathbb{A}$. We then say that $\mathbb{B}$ is homomorphic to $\mathbb{A}$.

The relation of homomorphism thus defined identifies the set or class of structures to which any structure $\mathbb{B}$ is homomorphic, what we may call its *homomorphism class*. According to Bartels, these structures constitute the *representational content* of $\mathbb{B}$, that is, they are all *potential* representational targets of $\mathbb{B}$. In order for any of these potential targets to turn into the *actual* target of $\mathbb{B}$, a *representational mechanism* must pick it out from the homomorphism class as *the* target for $\mathbb{B}$. A representational mechanism can be of two kinds: it may arise from an agent's intentions and purposes (an intentional representational mechanism), or it may be the result of naturally occurring *causal relations* (a causal representational mechanism). In the first case, the selection of the actual target from the homomorphism class is arbitrary, depending entirely on an agent's purposes, while in the second case the selection is driven by some causal facts that are independent of the agent. In either case, the representational mechanism has in effect the absolutely ineliminable role of picking out the actual representational target of a particular model $\mathbb{B}$. In spite of this, Bartels claims that his theory retains its structural character, since homomorphism is nonetheless "the necessary condition of correct actual representation" (ibid., p.12). Let us inspect this claim a little closer.

Two forms of misrepresentation are generally considered in the literature: inaccuracy and mistargetting.[2] The three kinds of misrepresentation presented in Section 3.1 all lead to inaccuracy, which is misrepresentation in the broad sense. As for mistargetting, it is "the phenomenon of mistaking the target of a representation" (Suárez, 2003, p.233).

[2]While misrepresentation as inaccuracy is taken into account in Cartwright (1983), Contessa (2011), Frigg (2006), Giere (1988), Pincock (2011), Suárez (2003, 2004), Teller (2001, 2008b), van Fraassen (2008), misrepresentation as mistargetting is presented in Suárez (2003, 2004).

Now, homomorphism theory is claimed by Bartels to be *conceptually adequate*, that is, it sharply distinguishes cases where $\mathbb{B}$ represents, $\mathbb{B}$ does not represent, and $\mathbb{B}$ misrepresents. This claim is relevant insofar it is usually on the grounds of conceptual adequacy that the literature dooms to failure structural accounts. Indeed, it is argued, the fact that structural accounts treat morphisms as necessary conditions for representation leaves no room for the intermediate condition 'there is representation and it is *incorrect*': either there exists a morphism from $\mathbb{A}$ to $\mathbb{B}$, hence representation, or there exist not morphism and representation does not obtain.

According to Bartels, such a charge would be unfair to his homomorphism theory. Indeed, the distinction between the *representational content* of $\mathbb{B}$ and its *target* allows the theory to account for the following situation:

> If a reference object for B [$\mathbb{B}$] is chosen by a representational mechanism out of the set of objects potentially represented by B [$\mathbb{B}$], then B [$\mathbb{B}$] will correctly represent this object. If a reference object for B [$\mathbb{B}$] is chosen which does not belong to this set, then this reference object will be *misrepresented* by B [$\mathbb{B}$]. Thus, the case in which something A [$\mathbb{A}$] is misrepresented by B [$\mathbb{B}$] and the case in which A [$\mathbb{A}$] is not represented by B [$\mathbb{B}$] (i.e. A [$\mathbb{A}$] is not a reference object of B [$\mathbb{B}$]) are clearly distinct. (2006, p.14)

The distinction between *target* and *content* of $\mathbb{B}$ plays then a crucial role in accommodating those intermediate cases where representation occurs, *and* it is not correct. In order to illustrate misrepresentation thus conceived, let's consider a universe of discourse which allows the following five structures $\{\mathbb{B}, \mathbb{A}_1, \mathbb{A}_2, \mathbb{A}_3, \mathbb{A}_4\}$. Among the five structures, only $\mathbb{A}_1$ and $\mathbb{A}_2$ are homomorphic to $\mathbb{B}$. We call $\mathbb{H}$ the set containing $\mathbb{A}_1$ and $\mathbb{A}_2$, which then constitute the representational content of $\mathbb{B}$. Now suppose that a representational mechanism picks $\mathbb{A}_3$ as the target of $\mathbb{B}$, thus misrepresenting $\mathbb{A}_3$. Consequently, structure $\mathbb{A}_4$ is neither a potential target of $\mathbb{B}$, nor misrepresented by $\mathbb{B}$. Providing a sharp distinction between representing (picking a target within $\mathbb{H}$), non-representing (having a structure neither belonging to $\mathbb{H}$ nor picked by a representational mechanism), and misrepresenting (having a structure not belonging to $\mathbb{H}$ and nonetheless picked as a target), the homomorphism theory has the resources to satisfy conceptual adequacy, thus explaining misrepresentation. In particular, this notion of misrepresentation may be seen to be addressing directly the concerns raised by Suárez (2003) about mistargetting: the act of ascribing a target outside the representational content of $\mathbb{B}$ may be thought to make his notion of mistargetting precise.

However, Bartels' homomorphism is only allegedly conceptually adequate. Fact is, we can not differentiate representation, non-representation and misrepresentation on the grounds of homomorphism alone. Indeed, Bartels claims that we have misrepresentation if a representational mechanism picks a target for $\mathbb{B}$ *outside* the set $\mathbb{H}$ of all the structures $\mathbb{B}$ is homomorphic to. Misrepresentation is then the act performed by a representational mechanism to choose as a target for $\mathbb{B}$ a structure which $\mathbb{B}$ is not homomorphic to. Of course, homomorphism is necessary to identify the set $\mathbb{H}$ of structures over which neither non-representation nor misrepresentation can occur. However, before a representational mechanism choses a target for $\mathbb{B}$ among the structures outside $\mathbb{H}$, any of these structures could be either misrepresented or non-represented at all. Therefore, it is the choice made by a representational mechanism to actually determine which structure is misrepresented and, consequently, which one is not represented. In other words, homomorphism alone can not help in sharply distinguishing representation, non-representation and misrepresentation. Consequently, Bartels' homomorphism theory is still in violation of conceptual adequacy. What is also noteworthy is the fact that Bartels' attempt to accommodate the conceptual adequacy seems to resolve in a form of deflationary, or functional, account: the crucial role is played by the choice made by a representational mechanism.[3] Bartel's homomorphism theory falls short also as an account of misrepresentation as mistargetting. The fact is that it does not actually characterize misrepresentation by mistargetting in full structural terms. For the original objection raised by Suárez was not reliant on the possibility of

[3]Deflationary (Suárez, 2004) or functional (Chakravartty, 2010) approaches treat representation as a function of models which allows model users to gain information about the target at stake *via* the model. The ascription, or recognition, of the representational function of a model by a user is then essential to have representation.

ascribing a target that lies outside of the homomorphism class. To pursue the example above, the objection does not trade on the actual representational target $\mathbb{A}_3$ lying outside the homomorphism class at all. The objection can be entirely run within the homomorphism class, and in fact it properly belongs there. For Suárez's point is that the mistaken target is assumed wrongly to be the target precisely because it holds the required structural relation, and merely on account of this fact. The point of misrepresentation by mistargetting is rather that no structural characterization can distinguish structures within the homomorphism class *regardless of* whether they are or not picked out as the actual target. In other words, suppose that the representational mechanism above picked out $\mathbb{A}_1$ as the representational target of $\mathbb{B}$ and that someone mistakenly identifies $\mathbb{A}_2$ as the target for $\mathbb{B}$. Then there is no structural characterization available of this mistake since both structures are on equal terms in the homomorphism class of $\mathbb{B}$. It should be clear that this point survives Bartels' disquisition in the quote above entirely.

Homomorphism theory seems then to fall short of what would be required for an adequate account of scientific representation even by Bartels' own standards. What we need to see now is whether the homomorphism theory fares any better in dealing with misrepresentation as inaccuracy.

3.4 Structural morphisms and representational inaccuracy

We need to see now if the formal analysis fares any better than the extensional analysis and enables the homomorphism account to accommodate the inaccuracy kinds of misrepresentation. We have seen that Bartels identifies three conditions for homomorphism (Sect.3): completeness, faithfulness and the condition that the $f : A \to B$ be surjective. These conditions, if weakened, might "fit the cases in which representations do not work perfectly" (Bartels, 2006, p.9). In such cases, Bartels argues, representation may either "lead to false expectations concerning facts in the represented domain" or "blur some of the fine grained differences existing in the represented domain" (ibid.). These are precisely cases of misrepresentation as inaccuracy. In particular, they do recall the formulation we put forward for, respectively, pretending and abstracting. This is why in what follows we treat Bartels' formal analysis of homomorphism as an attempt to accommodate misrepresentation as inaccuracy.

3.4.1 Homomorphism *versus* epimorphism

Before proceeding, we need to point out a technical issue about the notion of homomorphism advocated by Bartels. In the literature, the only condition required for homomorphism is completeness, i.e., the condition which assures that every fact in $\mathbb{A}$ has a corresponding (atomic or relational) fact in $\mathbb{B}$.[4] On the other hand, a *surjective* homomorphism is the condition for $\mathbb{B}$ to be the *homomorphic image* of $\mathbb{A}$.[5] Therefore, the notion of homomorphism that Bartels is appealing to does not coincide with the standard notion of homomorphism nor with homomorphic image. Indeed, besides completeness and surjectivity of f, Bartels requires an additional condition, namely faithfulness:

> If (i) [faithfulness] and (ii) [completeness] are fulfilled, f is a homomorphism from $\mathbb{A}$ onto $\mathbb{B}$, and $\mathbb{B}$, by virtue of the existence of f, can be said to be an *homomorphic image*. (Bartels, 2006, p.8)

[4]See Chang and Keisler (1973), Dunn and Hardegree (2001), Hodges (1997), Hodges and Scanlon (2013).

[5]"A relational structure $\mathbf{B}$ is said to be a *homomorphic image* of $\mathbf{A}$ if there exist a homomorphism from $\mathbf{A}$ to $\mathbf{B}$ that is onto B (in symbols, $\mathbf{B} = h^*(\mathbf{A})$). (A function f maps $\mathbf{A}$ onto $\mathbf{B}$ [it should be A onto B] if for every $b \in B$ there is an $a \in A$ such that $h(a) = b$)." (Dunn and Hardegree, 2001, p.15). Read the bold character in the quote as our $\mathbb{A}$ and $\mathbb{B}$.

It should be noted, however, that a homomorphic image is not necessarily also a faithful one. Indeed, the structure $\mathbb{B}$ can be a homomorphic image of $\mathbb{A}$ and yet bear a relation R_j^B which has *no* counterpart in $\mathbb{A}$.[6]

Our claim is that the morphism on which Bartels grounds his structural account is not really homomorphism, but what is technically known as epimorphism. Indeed, as demonstrated by Rothmaler (2005, Sect.2, p.474), epimorphism requires the surjectivity of the $f : A \to B$ mapping and, in addition, both faithfulness and completeness.

We can now consider the weakenings which, according to Bartels, allow epimorphism to accommodate misrepresentation as inaccuracy. The first form of weakening is on faithfulness and it leads to the notion of *minimal fidelity* (Bartels, 2006, p.9). While faithfulness in its original formulation (2) requires that the implication $R_j^B(f(\bar{a})) \to R_j^A(\bar{a})$ holds *for all* the counterimages of $f(\bar{a}) \in B^n$, all $j, R_j^A \in A^n$, and $R_j^B \in B^n$, minimal fidelity allows the implication to hold for *some* of the counterimages only. In other words, minimal fidelity admits the following case:

$$\text{Minimal Fidelity:} \quad \text{there exists } f^{-1}(\bar{b}) = \bar{a} \in A^n\text{: } R_j^B(f(\bar{a})) \text{ and } \neg R_j^A(\bar{a}) \tag{4}$$

The fact that epimorphism is not necessarily injective is crucial here since a one–to–one correspondence between the arguments in A and their images in B would make the conditions of faithfulness and minimal fidelity equivalent: given that each $b_i \in B$ in the range of f has only one counterimage $a_i = f^{-}1(b_i)$, it is just

[6]Consider two similar structures, $\mathbb{A} = \langle A, (R_1^A, R_2^A)\rangle$ and $\mathbb{A} = \langle B, (R_1^B, R_2^B)\rangle$, with $A \in \mathbb{A} = \{a_1, a_2, a_3, a_4\}$, $B \in \mathbb{B} = \{b_1, b_2, b_3\}$. The mapping $f : A \to B$ is surjective, and the condition of completeness holds. Therefore, $\mathbb{B}$ is a homomorphic image of $\mathbb{A}$. To find a case where the conditions of completeness and the surjectivity of f (and $\mathbb{A}$ and $\mathbb{B}$ are similar structures) are satisfied, but $\mathbb{B}$ is *not* faithful, we need a relation $R_j^B \in \mathbb{B}$ which has no counterpart $R_j^A \in \mathbb{A}$ and, at the same time, we need to assure that all the relations in $\mathbb{A}$ have their counterparts in $\mathbb{B}$. The function $f : A \to B$ is surjective (and *not* injective) and ascribes to each argument the following images: $f(a_1) = b_1, f(a_2) = b_2, f(a_3) = b_3, f(a_4) = b_3$. Consider now the case that $\mathbb{A}$ has the following family of relations: $R_1^A \subseteq A^2 = \{(a_1, a_2), (a_1, a_3)\}$ and $R_2^A \subseteq A^2 = \{(a_1, a_2), (a_3, a_4)\}$. As for $\mathbb{B}$: $R_1^B \subseteq B^2 = \{(b_1, b_2), (b_1, b_3)\}$ and $R_2^B \subseteq B^2 = \{(b_2, b_1), (b_3, b_2)\}$. The relation R_1^B in $\mathbb{B}$ thus corresponds to *both* the relation R_1^A and R_2^A in $\mathbb{A}$, while the relation R_2^B has *no* counterpart in $\mathbb{A}$. Therefore, $\mathbb{B}$ is a homomorphic image of $\mathbb{A}$ while faithfulness is violated.

equivalent to claim that the conditional $R_j^B(\bar{b} = f(\bar{a})) \to R_j^A(\bar{a})$ holds for *all* the tuples of counterimages of $\bar{b} = f(\bar{a}) \in B^n$, or that it holds for *at least* one tuple.

The second form of weakening is on completeness, and it admits the case where *some*, or even *all* the relations in $\mathbb{A}$ are not preserved in $\mathbb{B}$. Weakening on completeness can take two forms: either some relations in $\mathbb{A}$ are not represented at all in $\mathbb{B}$, or some $n-$tuples in $\mathbb{A}$ are not represented at all in $\mathbb{B}$ (which is to say, some $n-$tuples of images in $\mathbb{B}$ do not stand in any relation of $\langle R^B \rangle$ although their counterimages stand in the corresponding relations $\langle R^A \rangle$).

It is worth noticing at this point the major difference between these two types of weakenings. The weakened form of faithfulness is a proper condition, in the sense that it does impose some restrictions on the transfer of structure: it cannot be the case that a relation in $\mathbb{A}$ does not have a corresponding relation representing it in $\mathbb{B}$. The weakened form of completeness, on the other hand, is not a condition *at all*, it rather consists in allowing any possible scenario, which is forecasted by Bartels himself: "The fewer relations for which the transfer of structure holds, and the fewer the number of elements of A to which the transfer is restricted, the poorer the representation will be with respect to content. In an extreme case, no content will be left" (ibid., p.11). Another, more astonishing, fact about weakened completeness is that it is a violation of the very minimal condition required for the transfer of structure (i.e. completeness). Thus no attempt to ground the representational relation on weakened completeness may be interpreted as providing a meaningful structural account of representation. The relevant weakenings must be of a different kind. Let us see what Bartels proposes in order to accommodate inaccurate representations.

3.4.2 Morphisms and misrepresentation (as inaccuracy)

In the previous section we have introduced the weakenings which, according to Bartels, allow to accommodate misrepresentation as inaccuracy. In order to see whether they actually accomplish the task, here we confront each morphism, both

Morphism	Characteristic Conditions
Homomorphism	completeness
Epimorphism	surjectivity of f, completeness, faithfulness
Epimorphism^{*c}	surjectivity of f, weak completeness, faithfulness
Epimorphism^{*f}	surjectivity of f, completeness, weak faithfulness
Isomorphism	surjectivity *and* injectivity of f, completeness, faithfulness
Isomorphism^{*c}	surjectivity *and* injectivity of f, weak completeness, faithfulness
Isomorphism^{*f}	surjectivity *and* injectivity of f, completeness, weak faithfulness
Epimorphism^{*c,f}	surjectivity of f, weak completeness, weak faithfulness
Isomorphism^{*c,f}	surjectivity *and* injectivity of f, weak completeness, weak faithfulness

TABLE 3.1: Morphisms

in its standard and weakened version, with the formalized versions of abstraction, pretence and simulation that we introduced in Section 3.2. For the sake of completeness, our analysis will include also isomorphism which, as mentioned in the previous sections, is the morphism employed in other structural accounts. Isomorphism demands the following conditions to be satisfied: completeness, faithfulness, and that the mapping $f : A \to B$ be both injective and surjective.[7] Our goal is then to verify that for every morphism there exists *at least* one form of misrepresentation which is not accommodated, thus showing that none of the three morphisms account for misrepresentation as inaccuracy. For the sake of clarity, we recapitulate in Table 3.1 the conditions for each morphism, marking with a star the weakened morphisms that we have discussed.

Two things need to be noted before proceeding. First, cases where the morphisms are weakened on completeness are not to be considered since, for the reasons presented in the previous section, they are not morphisms at all. Second, in Table 3.1 the following two cases are not listed: the case of a surjective homomorphism, and the case of a faithful homomorphism (without surjectivity). The first case

[7] Dunn and Hardegree (2001, p.17) consider the injectivity and surjectivity of f only as a condition for isomorphism. Chang and Keisler (1973, p.21), Hodges (1997, p.5) and Robinson (1963, p.25) consider also faithfulness as a condition for isomorphism.

satisfies the conditions for $\mathbb{B}$ to be a homomorphic image of $\mathbb{A}$. The surjectivity of $f : A \to B$, however, is neither a sufficient nor a necessary condition for homomorphism, so it can be omitted for the sake of argument without any loss of generality. On the other hand, a faithful homomorphism f which is not surjective is not an interesting case to consider, since faithfulness holds for the elements in $\mathbb{B}$ which are in the range of $f : A \to B$ only. And so in what follows, these quantifiers will be omitted whenever redundant.

We consider abstraction first, which we have formalized as follows:

$$\exists j, R_j^A \subseteq A^n, R_j^B \subseteq B^n, \exists \bar{a} \in A^n : R_j^A(\bar{a}) \wedge \neg R_j^B(f(\bar{a})) \tag{3.5}$$

Let's start with homomorphism. The formula (1) describing the completeness condition is logically equivalent to the following formula: $\neg R_j^A(\bar{a}) \vee R_j^B(f(\bar{a}))$ whose logical contradiction $\neg(\neg R_j^A(\bar{a}) \vee R_j^B(f(\bar{a})))$ is, in turn, equivalent to the formula for abstracting $R_j^A(\bar{a}) \wedge \neg R_j^B(f(\bar{a}))$. In other words, the condition of completeness is logically incompatible with abstraction. Yet, epimorphism and isomorphism, both in their standard version and in the version where only faithfulness is weakened, all satisfy completeness. Therefore epimorphism, epimorphism^f, isomorphism and isomorphism^f are logically unsuited to accommodate abstraction.

The second form of misrepresentation is pretence, which we have formalized as follows:

$$\exists j, R_j^A \subseteq A^n, R_j^B \subseteq B^n, \ \ \exists \bar{b} \in B^n, f^{-1}(\bar{b}) = \bar{a} \in A^n : \neg R_j^A(\bar{a}) \wedge R_j^B(f(\bar{a})) \tag{3.6}$$

We have just seen that homomorphism and, more precisely, the condition of completeness, is logically equivalent to the formula: $\neg R_j^A(\bar{a}) \vee R_j^B(f(\bar{a}))$. Hence, homomorphism allows for pretence as a logical possibility. On the other hand, pretence logically contradicts faithfulness. Indeed, the formula (2) for faithfulness is equivalent to $R_j^A(\bar{a}) \vee \neg R_j^B(f(\bar{a}))$ whose logical contradiction is exactly $\neg R_j^A(\bar{a}) \wedge R_j^B(f(\bar{a}))$. Therefore, any morphism that satisfies faithfulness can not accommodate pretence. This evidently holds for epimorphism and isomorphism. What about the weakened version of faithfulness ? We have seen that weakening faithfulness admits of a tuple $\bar{a} \in A$ which does not stand in relation $R_j^A \subseteq A^n$ even though its image $\bar{b} \in B$ stands in the the corresponding relation $R_j^B \subseteq B^n$. Weakened faithfulness, then, allows pretence in principle. However, for weakened faithfulness to actually accommodate pretence, it is crucial that the function f is *not* injective, otherwise weakened faithfulness can not accommodate pretence. Therefore, epimorphism^{*f} accommodates pretence, but epimorphism, isomorphism and isomorphism^{*f} do not accommodate this form of misrepresentation.

The third form of misrepresentation is simulation, which we have formalized as follows:

$$\begin{aligned}\exists j, k, R_j^A, R_k^A \subseteq A^n, R_j^B, R_k^B \subseteq B^n,\ \ \exists \bar{a} \in A^n, \bar{b} \in B^n, \bar{a} = f^{-1}(\bar{b}) :\\ \underbrace{(R_j^A(\bar{a}) \wedge \neg R_j^B(f(\bar{a})))}_{\text{abstracting on } \bar{a},\bar{b}} \wedge \underbrace{(\neg R_k^A(\bar{a}) \wedge R_k^B(f(\bar{a})))}_{\text{pretending on } \bar{a},\bar{b}}\end{aligned} \tag{3.7}$$

Simulation is what obtains from both abstracting and pretending on *the same* tuple, which is a common phenomenon in modeling (as stressed by Cartwright, 1989, Frigg and Hartmann, 2012). In this case, it is much easier to verify which form of misrepresentation is accommodated by which kind of morphism, given that we just need to jointly consider what abstracting and pretending allow for. It is then the case that only homomorphism^{*c} and epimorphism^{*f,c} accommodate simulation, and neither of them are proper morphisms that can transfer structure.

In Table 3.2 we summarize the results of our analysis, which leads us to conclude that no morphism that can be said to transfer structure from a source to a target is actually able to accommodate all forms of inaccurate misrepresentation. The structural mappings that merely satisfy weakened versions of completeness can not be said to transfer structure, and the rest are unable to accommodate at least one main form of misrepresentation as inaccuracy. Therefore we conclude

	Abstraction	Pretence	Simulation
Homomorphism	NO	YES	NO
Epimorphism	NO	NO	NO
Epimorphism^{*f}	NO	YES	NO
Isomorphism	NO	NO	NO
Isomorphism^{*f}	YES	NO	NO

TABLE 3.2: Morphisms and Inaccuracy

that isomorphism, epimorphism and homomorphism all fail to account for the phenomenon of misrepresentation.

It is in particular startling that most of the structural accounts proposed so far fail to accommodate the one form of misrepresentation as abstraction that philosophers of science have entertained ever since the times of Cambpell's influential discussion of the kinetic theory of gases. While structural mappings can be very helpful in establishing the accuracy of certain mathematical representations in physics, they are unable to characterize the very relation of representation in general.

3.5 Final remarks

We have examined Bartels' homomorphism theory of scientific representation. We have examined it in relation to two typical kinds of misrepresentation in scientific models, which we may refer to as 'mistargetting' and 'inaccuracy'. The former involves choosing the wrong target for a modeling source on account of perceived similarities or structural matches, and shows representation to be an essentially intentional notion (in a broad sense that encompasses intended use). The latter involves at least three different kinds of distortion of model targets by model sources, which we have distinguished as abstraction, pretence and simulation. We have illustrated these distinctions by means of a careful study of the historical case of the billiard ball model. This model was notoriously invoked by Mary Hesse in her rightly influential work on analogy. Nevertheless Hesse's treatment of the model is itself highly idealized. We claim that there is more to the actual case study than just positive and negative analogies in the sense discussed by Hesse. In particular there are inverse negative analogies, or analogies 'by denial', as well as negative analogies by 'abstraction': there are properties of gas molecules that billiard balls lack, as well as properties of billiard balls that gas molecules lack. We then endeavored to provide formal characterizations for all these distinctions in a form that is suitable to the homomorphism account of representation. The taxonomy thus obtained proves useful to determine whether homomorphism – or indeed any other kind of morphism – accommodates misrepresentation.

We share with Bartels the thought that the adequacy of any account of scientific representation demands such accommodation. Any adequate account must at least accommodate, if not explain, mistargetting and the three kinds of inaccuracy we have discussed. Now, as for mistargetting, we have examined whether Bartels' account successfully cope with it. A closer analysis has revealed some issues remain regarding how much work effectively homomorphism is doing in the account. We have argued that the representational mechanism that Bartels appeals to is crucial in determining representation,misrepresentation or non-representation. Thus, there does not seem to be much work left for homomorphism to do. Bartels does claim that homomorphism is necessary for representation or misrepresentation alike, yet his actual discussion of the role played by the representational mechanism seems prima facie to belie this claim. As for the three forms of inaccuracy that we have discussed, we have provided arguments to the effect that while homomorphism may account for pretence – although not in the form of epimorphism actually defended by Bartels – it can not provide for abstraction. We thus concluded that, contrary to Bartels' claim, the homomorphism account can not provide for any of the two typical kinds of misrepresentation by scientific models.

A structural account may well be needed to assess the accuracy or faithfulness of a scientific model, particularly in those cases where the model source and target can both be given appropriate structural descriptions. Nonetheless, even in such cases, it does not seem to be the case that the representational relation, or activity, is constituted by any structural morphism. It is rather what Bartels refers to as the “representational mechanism” that does all the conceptually required work at this stage. Once this basic mechanism is in place, it becomes appropriate to ask questions regarding the structural match of sources and targets. Representation does not essentially consist in transfer of structure from target system to source object. And while the homomorphism account may describe the means whereby some mathematical representations operate in science, it can not fully describe representation per se.

Conclusions

To draw the discussion to a close, let me see whether I can now attempt a reply to the question I began the dissertation with: *if* and *to what extent* the concept of structure can be integrated into the philosophical analysis of scientific representation.

Let me first answer the *if*-question. My answer is 'yes', the concept of structure can be integrated into the analysis. Chapters 1 and 2 are meant to give evidences in favor of such an answer.

In Chapter 1 the concept of *epistemic structure ascription* has been presented as a possible way to go in order to resist the deflationism about the problem of representation. Turning down this form of deflationism is, trivially, a first step to carry on the debate over scientific representation without giving ground to any metaphysics of representation for mental states, as Callender and Cohen (2006) suggest. So, the first – I think – significant advantage that we gain from appealing to the concept of structure is to keep the debate on representation alive. The concept of epistemic structure ascription is just in its embryo stage. Surely its tenability has to be further verified and there are also possibilities for it to be enriched by the comparison with analogue concepts presented, e.g., in the philosophy of mathematics (Resnik, 1975, Steiner, 1978), or in more recent works in the philosophy of science (see, e.g., Bokulich, 2011 and Debs and Redhead, 2007, ch. 1). Passed these tests and comparisons, this concept could turn out to be a prerequisite for posing and tackling the problem of representation, as many philosophers are keep doing, *notwithstanding* just few of them have picked out the gauntlet thrown by Callender and Cohen's deflationism. Moreover, the concept of epistemic structure ascription could be integrated into the inferential account advocated by Suárez (2004, 2015). And this might be a non-trivial result. Indeed, Suárez's account is among the few attempts to date that does provide an account of representation general enough to contravene the tendency, to some extent detrimental for the issue of representation, to reduce its analysis mainly to a description of particular instances of scientific representations, each with their own representational formats (in Chapter 1, I have termed this trend of inquiry "particularistic analysis"). This tendency, I think, is leading us far from the original scope of the *philosophical* analysis of scientific representation, that is, to provide a rational reconstruction which is broad and consistent enough to successfully grasp the general features of representation.

In Chapter 2, whose first part aims at presenting the semantic view through the analysis of its early formulations (those provided by Suppe, Suppes and van Fraassen), I have singled out those features that make the semantic view a rational reconstruction of scientific theories and scientific theorizing. In particular, I have argued that the semantic view offers a *contingent*, *modest*, and *neutral* rational reconstruction built on the notion of models as structures. These, I think, are essential features that an account of scientific representation should display. The work done within the semantic view also allows to understand the usefulness of a rational reconstruction for an otherwise too complex enterprise, such as scientific theorizing and experimental practice. To deal with such a complexity, we should *a priori* give up the possibility of providing a faithful description of the scientific practice. So, to mention just one formulation of the semantic view considered in this chapter, the hierarchy of structures (models) put forward by Suppes (1962) is a huge step forward in this direction. It is indeed presented with no demand to provide necessary and sufficient conditions for having either representation or, more generally, scientific explanation. The reconstruction of scientific theorizing provided by the semantic view rather aims at setting a formal framework within which the general features of model construction and application can be identified, thus achieving an account which is a close approximation of actual scientific practice. The success of the semantic view in this respect might be interpreted as a confirmation that a rational reconstruction built on the notion of structure could shed some light on scientific representation. In the second part of Chapter 2, I have presented a critical analyses of the charges against the semantic view *as* an account of scientific representation.

In Chapter 3, I have considered a case where the integration of the concept of structure into the philosophical analysis of scientific representation turns out to be unsuccessful. This is Bartels' structural account of scientific representation. Bartels' account is an instance of the so-called structural approaches to representation. The hallmark of these approaches is, so to speak, to let the formal concept of structure make all the representational work – thus relying on the objectivity of the mathematics employed to cash out the relation between a model and its target. This way of conceiving models and their representational task has been widely criticized in the past by renown arguments such as Newman's problem (1928) or Putnam's model-theoretic argument (1976) and, more recently, further arguments have been provided against structural approaches by Frigg (2002, 2006), Giere (2004), Suárez (2002, 2003), van Fraassen (1997, 2006, 2008). The argument presented in this chapter against Bartel's account draws on the criticisms put forward by the recent literature and it leads to the same conclusions. Conceiving the structural properties of a model – such as 'being morphic to' a structure taken to represent the target system – as (necessary and) sufficient conditions turns out to be problematic. Indeed, structural approaches compel the philosopher to deal either with the distinct, and equally problematic, issue of the applicability of mathematics, or with an account based on the mind-independency of representation, which is hard to justify. The solution that the recent literature offers to avoid the pitfalls of the structural approaches is to acknowledge the relevance of the pragmatic aspects characterizing scientific representation (such as the role of model-users, the practice of model-building, the epistemic goals to achieve, etc.) and to provide accounts which consider such aspects as well. Both the accounts of representation which I have defended in the previous chapters have the happy feature of taking into account the pragmatic aspects of representation while providing its rational reconstruction in structural terms.

If the overall answer to the *if* question is 'yes', then the *to what extent*-question can be reformulated as follows: *whither structuralism for scientific representation?* My answer is that there exists a form of structuralism which can be fruitfully used to settle issues pertaining to scientific representation. This form of structuralism appeals to a notion of structure as *epistemic device* which is employed by a competent model-user to get information about the target system. Therefore, the notion at stake is that of structure as a representational vehicle. However, this form of structuralism can settle only *general* issues about scientific representation, that is, it may not work for particular instances of representation, which vary according to the discipline at stake. With respect to other form of structuralism for scientific representation, such as that held by the advocates of structural accounts, the one which I advocate does *not* exceed its scope as a rational reconstruction of scientific representation. This could happen if the notion of structure is employed either to justify the representational practice independently of pragmatic aspects, or to provide both necessary and sufficient conditions for *all* occurrences of scientific representation, or when it is framed within wider epistemic programs – thus compromising the neutrality of the reconstruction.

Of course, there is still much work to be done to flesh out this conception. I would be satisfied if I managed to make this form of structuralism for scientific representation only conceivable as an option.

Appendix A

Suppes' Formulation of the Semantic View

A.1 Models and set-theoretic predicates

In this appendix I illustrate Suppes' attempt to formalize classical particle mechanics by employing set-theory. Suppes' goal is to show that "even a relatively complicated theory can be given a clear and exact formulation within set theory" (Suppes, 1957, p.291). The formulation of the theory begins with its *axiomatic characterization*, in line with what is typically done in mathematics. A set-theoretic structure is said to be *model* of a theory thus formulated *if and only if* it satisfies the set of axioms of the theory.

Let $\mathbb{S} = \langle P, T, s, m, f, g \rangle$ be a system of particle mechanics, where P and T are sets, s and g are binary functions, m is a unary function, and f is a ternary function. The intended physical interpretation of the axioms of the theory is the following. The elements of P are particles. The elements of T are time indices. The function $s(p, t)$ denotes the position of particle $p \in P$ at time $t \in T$. The function $m(p)$ denotes the mass of particle $p \in P$. The function $f(p, q, t)$ denotes the force that a particle $q \in P$ exerts on another particle $p \in P$ at time $t \in T$. The function $g(p, t)$ denotes the resultant external force acting on particle $p \in P$ at time $t \in T$.

A set-theoretical predicate which axiomatizes particle mechanics within set-theory has the following form:

Definition A.1. $\mathbb{S}$ *is a system of particle mechanics* if and only if the following axioms are satisfied:

- *Kinematical axioms:*

  M1. P is finite and $P \neq \emptyset$;

  M2. T is an interval of the real numbers $\mathbb{R}$ and $T \neq \emptyset$;

  M3. $s(p,t) \in \mathbb{R}^3$ and is twice differentiable on T, for all $p \in P$;

- *Dynamical axioms:*

  M4. $m(p) \in \mathbb{R}$ and $m(p) > 0$, for all $p \in P$;

  M5. $f(p,q,t) = -f(q,p,t) \in \mathbb{R}^3$, for all $p, q \in P$ and $t \in T$;

  M6. $s(p,t) \times f(p,q,t) = -s(q,t) \times f(q,p,t)$, for all $p, q \in P$ and $t \in T$;

  M7. $m(p)\dfrac{\partial^2 s(p,t)}{\partial t^2} = \sum_{q \in P} f(p,q,t) + g(p,t)$, for all $p, q \in P$ and $t \in T$.

Axiom M1 is required to have well defined mass and kinetic energy of the whole system. Axioms M2 and M3 are considered essentially for the sake of convenience, in particular, for tractability reasons. For instance, axiom M2 could be replaced by the requirement that T is a subset of the rationals – empirically, this is to say that observations take place in discrete time – but this would not allow to use differential analysis, which makes calculation simpler. Analogue reasons can be given for axiom M3, which requires differentiability.

Axiom M4 requires that the mass of every particle is strictly positive and independent of time. Although an object could have a time-dependent mass, such possibility is ruled out within classical particle mechanics. Similarly, were a particle mass null, then, according to M7, we could not determine the acceleration of the particle by determining the forces acting on it. Together, axiom M5 and axiom M6 require Newton's Third Law of motion to hold. Finally, axiom M7 requires Newton's Second Law of motion to hold.

P, T, s, m, f, and g are the "primitive" notions of the theory. In other words, they are the main 'components' of the theory. Kinematic axioms M1, M2, and M3 define these components set-theoretically. Furthermore, additional structure is imposed on the primitives which should not be inconsistent with respect to the theory. Dynamical axioms M4, M5, M6, and M7 serve this purpose. A model $\mathbb{M}$ is a set-theoretical entity which satisfies the set-theoretical predicate. That Suppes conceives models as set-theoretic structures is essential to understand in what sense the model $\mathbb{M}$ can be a realization of the theory of particle mechanics (Suppes, 1967, p.252). Sentences about the model $\mathbb{M}$ are valid only if they are logical consequences of the axioms.

A model is a 'possible realization of the theory' if all valid sentences of the theory are satisfied. For a model $\mathbb{M}$ to be a realization of the theory of particle mechanics, it should be the case that all sentences which are valid for the theory of particle mechanics are also satisfied in $\mathbb{M}$. In this example from classical particle mechanics, the satisfaction of the kinematical and dynamical axioms above guarantees that this is the case.

Another hallmark of Suppes' formulation of the semantic view, besides the set-theoretical formalization of theories, is the idea that for a given theory we do not have just one "logical type" of model which relates the theory to phenomena. We rather have a "hierarchy of models" of different logical types. Suppes illustrates in details what of a hierarchy of models is in his paper *Models of data* (1962). In the next section, I integrate Suppes' illustration of the hierarchy view of models with his work on both set-theoretical formalization, the theory of measurement and linear models.

A.2 Hierarchy of models

To illustrate Suppes' idea of the hierarchy of models, I resort to an example from the *theory of statistical learning* presented in Suppes (1962). Suppes chooses the theory of statistical learning to couch the hierarchy view of models since this theory is simple, mathematically non-trivial, and it involves stochastic elements. I will not stick precisely to Suppes' presentation of the hierarchy, partly because now the jargon of statistical learning theory has been slightly changed and, more importantly, because some small changes will help to shed light on those aspects of the hierarchy view which I find particularly relevant for the purpose of this section.

A typical realization of the theory of statistical learning is a *model of learning through experiments* that describes a situation where an agent is uncertain about the characteristics of an environment which are assumed to be persistent over time. To learn about the environment, the agent can repeatedly run an experiment whose outcome is stochastic but to some extent informative about the environment.

A possible specification of the statistical learning theory is the *linear response theory* (see Estes and Suppes, 1959). Roughly speaking, the linear response theory is a particular specification of how the agent interprets the evidence provided by the experiment, that is, of how she updates her beliefs about the characteristics of the environment. The linearity lies in the fact that, after each trial of the experiment, the agent updates her beliefs by combining linearly her previous beliefs and the experimental evidence.

To fix ideas, consider the following case. A researcher wants to understand the efficacy of a drug. The experiment is a sequence (potentially infinite) of trials of the drug on a sample of selected subjects. Each trial can provide some information in the form of an observable response from the subjects – e.g., a particular reaction to the drug – and a reinforcement of such a response – e.g., the intensity or the degree of identifiability of the reaction. In order to keep things as simple as possible, let's assume that there are only two possible responses: yes or no, and only two possible reinforcement: strong or weak.

Formally, the outcome of a single trial of the experiment is a pair $\omega = (a, e)$, where $a \in A = \{a_1, a_2\}$ is the observed response of the experiment, and $e \in E = \{e_1, e_2\}$ is the reinforcement. A possible experimental outcome is a sequence $\{\omega_i\}_{i=0}^{\infty} =$

$\{\omega_0, \dots, \omega_n, \dots\}$ where ω_n stands for the pair of response-reinforcement observed in the n-th trial.

A linear response theory that is particularized to the described case can be formalized as follows: (Ω, p, θ), where $\Omega = 2^{(A \times E)^\infty}$ is the set of all possible infinite sequences of outcomes of single experimental trials (that is, all possible experimental outcomes), $p : \Omega \rightarrow [0, 1]$ is a probability measure describing the likelihood of each event in Ω, and $\theta \in (0, 1]$ is a parameter describing how much the new evidence provided by the experiment affects learning – i.e., how much the researcher weights the last observation with respect to her previous beliefs. A realization of such a particularized linear response theory is a *model of linear response* which is consistent with (Ω, p, θ) and, in addition, that satisfies the following axioms:

A1. if $w_n = (A_i, E_i)$ is observed at trial n, then at trial $n+1$ we have:
$p(A_i|\omega_0, \dots, \omega_n) = (1 - \theta)p(A_i|\omega_0, \dots, \omega_{n-1}) + \theta \ \ ;$

A2. if $w_n = (A_i, E_j)$ is observed at trial n with $i \neq j$, then at trial $n+1$ we have:
$p(A_i|\omega_0, \dots, \omega_n) = (1 - \theta)p(A_i|\omega_0, \dots, \omega_{n-1}) \ .$

Axiom A1 prescribes that a response which is observed to be reinforced at trial n leads to a higher probability of observing the same response in trial $n+1$, and the adjustment is obtained by a linear combination of the previous expectations and probability 1, where θ is the weight given to probability 1. Similarly, A2 prescribes that a response that is observed not to be reinforced at trial n leads to a lower probability of observing the same response in trial $n+1$, and the adjustment is obtained by a linear combination of the previous expectations and probability 0, where θ is the weight given to probability 0.

From this formulation it follows that a realization of the theory can not be an actual realization of the experiment in at least two respects. First, trials can not be actually run an infinite number of times. Second, θ is never observed in the experiment, as a trial provides only information on A and E.

So, one should move down one level of the hierarchy from the abstract linear response theory. In so doing, the observations provided by the experiment can be related to the linear response theory.

The *theory of the experiment* allows for such a move. A theory of the experiment which is particularized to the described experiment can be formalized by the following pair $(\hat{\Omega}(k), q)$, where $\hat{\Omega}(k) = 2^{(A \times E)^k}$ is the set of all possible finite sequences of length k, where k is the number of trials fixed by the researcher, and $q : \hat{\Omega}(k) \to [0, 1]$ is a probability measure describing the likelihood of each event in $\hat{\Omega}(k)$ and such that it agrees with the restriction of p from Ω to $\hat{\Omega}(k)$. A realization of such a particularized theory is a *model of the experiment* that is consistent with $(\hat{\Omega}(k), q)$ and that satisfies, for instance, the following contingent reinforcement rules:

CR1. $q(w_n = (A_1, E_1)|w_n = (A_1, \cdot)) = \pi_1 = 1 - q(w_n = (A_1, E_2)|w_n = (A_1, \cdot))$

CR2. $q(w_n = (A_2, E_2)|w_n = (A_2, \cdot)) = \pi_1 = 1 - q(w_n = (A_2, E_1)|w_n = (A_2, \cdot))$

Rule CR1 requires that, at any trial n of the experiment, the probability of observing E_1, given the outcome A_1, is independent of any previous trial. The probability of observing E_1 is the complement to one of the probability of observing E_2. The same goes for the probability in CR2.

Note that, once the researcher has chosen the number of trials k, the finite sequences contained in $\hat{\Omega}$ describe all the possible outcomes of the experiment. However, as k is small with respect to the cardinality of $\hat{\Omega}$ the researcher can still learn very little from his experiment without imposing further structure. We then need to go down another level of the hierarchy.

The *theory of data* allows for this further move. The theory of data is a theory of statistical inference applied to experimental data. With the help of auxiliary assumptions about the distributions of relevant variables and measurement errors, the researcher can arrange the evidences provided by the k experimental trials into a coherent picture. A theory of data which is particularized to the described experiment can be formalized by the following tuple $(\hat{\Omega}(k), \tau_0(\alpha, \cdot), \dots, \tau_s(\alpha, \cdot))$, where $\tau_\ell : [0, 1] \times \hat{\Omega}(k) \to \{reject, accept\}$ is a function providing an answer regarding the inference $\ell = 0, \dots, s$ for a selected level of statistical significance $\alpha \in (0, 1)$ and the given observed data $(\omega_0, \dots, \omega_k) \in \hat{\Omega}(k)$. It is worth to stress that all the additional structure that allows for statistical inference is embedded in the functions $\{\tau_\ell\}_{\ell=0}^s$. A realization of such a particularized theory of data is a *model of data* that is consistent with $(\hat{\Omega}(k), \tau_0(\alpha, \cdot), \dots, \tau_s(\alpha, \cdot))$ and that assumes a particular value of α, as well as a subset of relevant inferences denoted by the indices in $I \subset \{1, \dots, s\}$.

However, the model of data does not consider by default the possible confounding factors that typically arise in experiments and that can substantially affect the reliability of statistical inference. In other words, the "input" of models of data is not guaranteed to be of the kind supposed by the theory of data, since the model of data builds on codified inferences and therefore abstracts from the details of the experimental design. So, to get to a layer which is closer to the concrete target system at stake, we need to go down another level in the hierarchy

This final step is taken through the *theory of experimental design*. This is a theory of controlled design by means of which we can read off confounding factors, that can be eventually eliminated. In so doing, the data collected can be used within the *model of data* in order to fit the *model of the experiment*. A realization of the theory of experimental design is a *model of experimental design* that sets the rules to be followed for running a particular experiment.

Table A.1 represents the hierarchy of models (and theories) described so far. At the top of the hierarchy we find the theory of statistical learning which, being at the highest layer of the hierarchy, is ideally the most far-off from the actual phenomenon which is inquired. Going downward through the hierarchy, both theories and models get more 'concrete' as they become particularized to fit the needs of the researcher – namely to test the linear response theory in a controlled environment. So, the model of statistical learning is the most abstract and theoretical, while the model of experimental design is the most concrete and applied. In between the highest and the lowest layers, there are the structures which allow the model of

Theory	**Model**	**Hierarchy position**
of statistical learning	abstract model of an agent learning by experimenting	1st
of linear response	model consistent with (Ω, p, θ) and satisfying A1/A2	2nd
of experiments	model consistent with $(\hat{\Omega}, q)$ and satisfying CR1/CR2	3rd
of data	model consistent with $(\hat{\Omega}(k), \tau_0(\alpha, \cdot), \ldots, \tau_s(\alpha, \cdot))$ plus a choice of significance level α and inferences $\{1, \ldots, s\}$	4th
of experimental design	model producing data to be used in the model of data taking into account contingent experimental conditions	5th

Table A.1: Hierarchy of models according to Suppes

statistical learning to relate to the model of experimental design. Roughly speaking, at each layer of the resulting hierarchy, the "output" of a model crucially depends on the "input" given by the model at the former layer.

Appendix B

Suppe's Formulation of the Semantic View

B.1 The intended scope of a theory

Suppe (1977) bases his analysis of scientific theories on the intuitive concept of the *intended scope* of a theory, i.e., the class of phenomena that a theory aims at describing in a manner that is both sufficiently precise and abstract enough to allow for predictions and explanations. In particular Suppe emphasizes that, in order to deal with the complexity of a target system, theories abstract from those features of the system which are not taken to be relevant for explanatory purposes.

As a consequence, theories never provide an accurate description of their target system. What theories can provide is an accurate description of the *system in isolation*, i.e., of a system as it would behave were it isolated from the elements which are abstracted away. Within Suppe's formulation of the semantic view, the term "physical systems" refers to such systems in isolation, which are also defined as "idealized replicas of phenomena" (Suppe, 1977, p.224). Physical systems are then "counterfactually true" of the target system they represent (Suppe, 1989, p.95).

B.2 The state space

More precisely, Suppe characterizes physical systems as idealized replicas of those phenomena that can be fully described by the set of *selected parameters*. A particular configuration of the systems - i.e., a particular set of values of the selected parameters – is called a *state* of the system. The behavior of the system over time is then described by a sequence of states – each of which is fully characterized by a particular set of values of the selected parameters. A sequence of states is a trajectory in a physical system.

The following is an example from Suppe (1977). Consider the theory of classical particle mechanics and a phenomenon within the theory's intended scope, such as a free falling object in a viscous fluid. Classical particle mechanics assumes objects to be point masses and to move in a vacuum. Motion depends on momentum and mass. So, the only parameters that turn out to be relevant are momenta and coordinates of point masses.

More in detail, an object in free fall through a viscous fluid is described by two point masses, say A for the object and B for the earth, their positions, and their momenta (both position and momenta are given by three coordinate variables). The set of potential states of this system – i.e., the state space – is characterized by the values of the twelve position and momentum coordinates that A and B can have. So, the state of this system at a given time index t is described by the numerical values of these twelve parameters, and the behavior over time $t \in [t_0, t_1]$ of this system is described by a sequence of such states, one for each $t \in [t_0, t_1]$.

Note that, since each state is characterized by twelve "numbers", i.e., the twelve values of the relevant parameters, the state space can be described as a subset of a twelve-dimensional coordinate space – where the coordinates are the values that the parameters can take – and the behavior of the system can be described by a trajectory in the coordinate space.

Now, consider a scientist who wants to predict at time t the behavior of a small rock (A) in free fall towards the earth (B) at time $t' > t$. First of all, the scientist has to measure both A' and B's positions and momenta at time t. The measurements thus obtained should be converted into *data*, that is, into information about the state of the system "would have been had the idealized conditions presupposed by the theory been met" (Suppe, 1977, p.225). For instance, data must be corrected for the presence of the air medium. These data provide the twelve values of the parameters at time t that identify the state of the system at time t, as if it were in isolation. The scientist can then apply the theory and, from the knowledge of the state of the physical system at time t, she can derive the state of the system at time $t' > t$.

Note that, even if the theory is correctly specified, the state so predicted for time $t' > t$ is not the state that the scientist will be likely to observe at t', but it is the state that she would observe if the system was in isolation. However, the scientist can reverse the procedure applied to derive the twelve values of the parameters at time t from the initial measurements relative to A and B: she can use the twelve values of the parameters at time t' to derive the values of the actual A' and B's positions and momenta – namely, the values of the actual phenomenon which does not take place in isolation. If the theory is correctly specified, then these actual values will match the positions and momenta of A and B which are measured at t'.

B.3 The phase space

According to Suppe (1977), a physical system characterized by n relevant parameters is well described by a n-dimensional *phase space*. *Laws of coexistence* determine which subset of the phase space are physically possible. That is to say that only the points of the phase space whose coordinates satisfy the law will be physically possible. *Laws of succession* determine the possible trajectories in the phase space. *Laws of interaction* describe the effect of interacting systems and the deriving composite configurations on the phase space. A physical system is then defined by Suppe as what obtains when "all the configurations save one are removed from phase space" (ibid., p.227) and scientific theories are defined as *structures*, "these structures being phase spaces with configurations imposed on them in accordance with the laws of the theory" (ibid).

According to Suppe, this formalization sheds some light on the relationship between theories and their formulations. A formulation of a theory consists in a physical system which employs *elementary statements*. Elementary statements are made of propositions expressing the value r of a certain physical magnitude m at a certain time t. These statements can be true or false. Since, according to Suppe, "the theory is a model of any of its possible formulations" (ibid., p.228), the truth of an elementary statement (not to be confused with the empirical truth of the theory) depends on the phase space which is configured.

Let H denote the phase space, $\mathbb{E}$ the set of all elementary statement, and $e \in \mathbb{E}$ the generic elementary statement. The *satisfaction function* $h : \mathbb{E} \to 2^H$ is defined as the mapping that assigns to each elementary statement $e \in \mathbb{E}$ the region of H where e is true. The triplet $\langle \mathbb{E}, H, h \rangle$ provides the formal language for a theory. Elementary statements can be combined together according to logical rules which the language of the theory complies with (e.g., those of the Boolean algebra). The truth of the compound statements will be determined by the truth of the elementary statements. The structure of the phase space is a "major factor" in determining how elementary statements in $\mathbb{E}$ can be combined together and form statements which are true or false of the phase space. This, in turn, implies that the theory imposes restrictions on the possible languages to be employed for its formulation.

Appendix C

Van Fraassen's *Neutral* Formulation of the Semantic View

C.1 Before *The Scientific Image*: Semi-interpreted languages

In his paper on Beth's semantics of physical theories (1970), van Fraassen sets the basis for his formulation of the semantic view. In this paper, van Fraassen presents his analysis of the structure of scientific theories as an extension of Beth's contribution on the issue. More precisely, van Fraassen extends Beth's analysis by providing a framework for it which is built on the theory of semi-interpreted language (see van Fraassen, 1967 and van Fraassen, 1969).[1]

Within this formulation, theories are formalized in terms of *meaning* relations among predicates. This formulation can be formally presented by means of a set S of meanings, a set P of predicates, and a function $f : P \to 2^S$ which assign to each predicate $p \in P$ a set of meanings $f(p) \subset S$. A similar formulation is provided

[1]The reasons given by van Fraassen for focussing on Beth's analysis are, first of all, that Beth's analysis is "a much more deep-going analysis of the structure of physical theories" (1970, p.325) and, secondly, that it is more faithful to actual practice than the analysis put forward by the syntactic view.

for the relations of *intent*. By "relations of intent" van Fraassen (1967) refers to "intensive relations" which establish whether two predicates are intensionally equivalent. Thus, we can replace the set of meanings S with the set of intents T, and the function f is replaced by a function $g : P \to 2^T$ which assigns to each predicate $p \in P$ a set of intents $f(p) \subset T$.

The specification of a logical space of a language, such as P, and the specification of an interpretation function, such as f or g, jointly provide a semi-interpretation for an uninterpreted structure. In particular, according to van Fraassen (1967), in a semi-interpreted language four elements should be specified:

1. the *syntax* of the language, i.e., its vocabulary and grammar, that provides the set of possible sentences;
2. the *logical space* of the language, given as a set;
3. the *interpretation* of the language, given as a function from the set of possible sentences to the logical space;

More precisely, van Fraassen (1970) claims that the formalization of physical theories by means of a semi-interpreted language requires the following elements (see also van Fraassen, 1972):

- a set of (measurable) physical magnitudes M, together with a set of elementary statements E about the physical magnitudes. Each elementary statement assigns a value to a certain magnitude (the *syntax*);
- a state space S for the physical system under consideration that specifies all possible states in which the physical system can be, that is, all the possible values of the physical magnitudes according to the theory at stake (the *logical space*);
- a satisfaction function σ which assigns to each elementary statement a region of the state space (the *interpretation*).

A semi-interpreted language for a physical theory is the ordered triple $\mathbb{L} = \langle E, S, \sigma \rangle$. A *model* for such language can be formalized as a pair $\mathbb{M} = \langle X, \xi \rangle$, where X is some physical system which can be represented using the magnitudes in M, and ξ is a function which assigns to X a state in S. Therefore, an elementary statement $e \in E$ about the model $\mathbb{M}$ is true if and only if $\xi(X) \in \sigma(e)$, i.e., if and only if the system actually is in the region of the state space which is specified by the satisfaction function. An elementary statement $e \in E$ is a valid sentence in $\mathbb{L}$ if and only if e is true for every model of $\mathbb{L}$. Finally, a set of statements $E' \subseteq E$ semantically entails an elementary statement $e \in E$ if and only if e is true in all models of the theory in which every $e' \in E'$ is true.

To sum up, van Fraassen suggests that the meaning structure of a theory can be formally reconstructed by means of a semi-interpreted language which, in turn, becomes the language of the theory. For this reason, van Fraassen claims that a theory defines the kinds of systems to which it applies.

C.2 After *The Scientific Image*: Data and surface models

In *Scientific Representation: Paradoxes of Perspective* (2008), van Fraassen presents an analysis of the structure of scientific theories which differs from the one illustrated in the previous section. The common hallmark of these two analyses is that, contrary to the analysis presented in *The Scientific Image* (1980a) and strongly influenced by constructive empiricism, they are both *neutral* formulation of scientific theories (see discussion in Chapter 2, subsection 2.2.3).

The analysis carried out by van Fraassen in *Scientific Representation* appeals to the idea of a hierarchy of models introduced by Suppes (1962) (see Appendix A) and, in particular, it focuses on the work done at the lowest layers of the hierarchy by the data model and by the so-called *surface* model.

The data model provides a first "policed summary" of the raw data which are collected by means of experiments and observations. This summary is obtained through a series of repeated measurements. More precisely, the data model summarizes the relative frequencies of the measured magnitudes which are taken to be relevant for the phenomenon at stake. For example, a graph representing the temperature in a certain region at a certain time t is a data model. It provides a "smoothed-out summary" of the information collected form the raw data which have been gathered at various stations in the region of interest.

The surface model is a summary which "smoothes still further" the information in the data model. The relative frequencies summarized by the data model are replaced in the surface model by measures with a continuous range of values. The main task of the surface model is to arrange information from the data model in such a way that the empirical evidence can be put in relation with more abstract structures at the higher layers of the hierarchy of models and, in particular, with the theoretical model. This is a crucial step in order for a scientist to evaluate the explanatory adequacy of the theory with respect to the phenomenon at stake. In this regard, van Fraassen explicitly states that surface models should *ideally* be considered as isomorphically embeddable into theoretical models. Otherwise, it would not be possible to show whether theoretical models fit observed phenomena, which is indeed the task they have been construed for.

A precise formalization of the surface models is provided in van Fraassen (2008, p.169). A surface model mainly comprises the following elements:

- *observable condition PRG:* a set A of possible measurement choices;
- *observable condition PRS:* a set B of possible measurement outcomes;
- *surface state:* a probability measure π on B which is conditional on A.

The probability given by $\pi(b|a)$ for some $a \in A$ and $b \in B$ is called *surface probability*, and it describes the probability that, given the measurement choice a, the outcome which will be observed is b.

In order to see in what sense a surface model is to be embedded into a theoretical model, it is useful to give a brief and formal description of the theoretical model as well. In general, a theoretical model specifies:

- a set M of *observables* (physical magnitudes), each of which has its own *range of possible values* $X(m)$, with $m \in M$;
- a set S of *states*, where each $s \in S$ specifies a value for each observable, i.e., $s \in \prod_{m \in M} X(m)$ for all $s \in S$;
- a *stochastic response function* μ^m which is a probability measure on $X(m)$, $m \in M$, that is conditional on S.

The probability given by $\mu^m(x|s)$ for some $m \in M$, $x \in X(m)$, and $s \in S$ has to be interpreted as the model's specification of the probability that a measurement of m gives value x when the system is in state s.

Hence, the theoretical model fits the surface model – i.e., the surface model is successfully embedded in the theoretical model – if and only if there exists some $s \in S$ such that the probability measures $\{\mu^m(\cdot|s)\}_{m \in M}$ all agree with the probability $\pi(\cdot|a)$ for every $a \in A$ which allows the measurement of m. Thus, embedding obtains if and only if one can find at least one state of the theoretical model that can account for all measurement occurrences, as well as for their frequencies, as they are described by the surface model.

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