SCIENTIFIC REPRESENTATION IN PRACTICE:
Models and Creative Similarity

PhD Thesis in
History and Philosophy of Science

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I, Julia Sánchez-Dorado, confirm that the work presented in this thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the thesis.

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Abstract

The thesis proposes an account of the *means* of scientific representation focused on similarity, or more specifically, on the notion of "creative similarity". I first distinguish between two different questions regarding the problem of representation: the question about the *constituents* and the question about the *means* of representation (following Suárez 2003; van Fraassen 2008). I argue that, although similarity is not a good candidate for *constituent* of representation, it can satisfactorily answer the question about the *means* of representation if adequately characterized. To motivate this position, I dispute the main arguments offered against similarity as *means* of representation, namely the arguments from variety, vagueness, and misrepresentation, and contend that similarity plays a central epistemic role in practices of representing in science. The study of the role of similarity in scientific practice, I argue, requires the analysis of the uses of judgments of similarity in the construction of scientific models. I examine the cases of the Mississippi Basin Model and the San Francisco Bay Model to illustrate how judgments of similarity are directly involved in the production of epistemically fruitful models. Informed by the practical investigation developed throughout the thesis, I finally outline what I call the *creative similarity account of the means of representation*. The notion of "creative similarity" helps to capture the way in which similarity, as means of representation, intervenes in actual practices of representing, namely, in the form of a productive interplay of judgments of similarity and distortions (i.e. idealizations, abstractions, simplifications), which is employed by resourceful agents with the aim of understanding aspects of the natural world.

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Introduction

> Parrhasius [...] entered into a pictorial contest with Zeuxis, who represented some grapes, painted so naturally that the birds flew towards the spot where the picture was exhibited. Parrhasius, on the other hand, exhibited a curtain, drawn with such singular truthfulness, that Zeuxis, elated with the judgment which had been passed upon his work by the birds, haughtily demanded that the curtain should be drawn aside to let the picture be seen. Upon finding his mistake, with a great degree of ingenuous candour he admitted that he had been surpassed, for that whereas he himself had only deceived the birds, Parrhasius had deceived him, an artist.
> (Pliny. *Natural History*. Book XXXV, Chap. 36)

The myth of Zeuxis and Parrahasius exemplifies an old and ingrained metaphor of representations as providing a mirror or perfect reflection of nature. Zeuxis's grapes were so lifelike that birds attempted to eat them. However, his artistic prowess was far surpassed by Parrhasius revealing that the curtain Zeuxis saw before him was itself a painting (Mansfield 2007: 26-7). Taking the copy for the real thing, the myth invites us to think, is irrefutable proof that a perfect representation has been achieved. Vestiges of this view on representation, philosophers such as Goodman (1968) and Rorty (1979) note, still permeate contemporary debates in epistemology and philosophy of science. For Goodman (1968), references to similarity in the debate of representation are evidence of the vigour of copy theories of representation. For Rorty (1979) the notion of representation itself should be rejected, as it is a remnant of the metaphor of the mirror of nature and the aspiration to achieve perfect knowledge of the world. This thesis is motivated by the rejection of the epistemological consequences that embracing the metaphor of the mirror of nature has. At the same time, it is motivated by the thought that the notions of representation and similarity are extremely valuable when addressing the problem of how humans comprehend the world around them, specifically through scientific representations.

In recent philosophy of science, the debate on representation has been focused largely on how scientific models represent natural phenomena. Models are far from being, or attempting to be, exact copies of the world. They idealize, simplify, abstract and distort the targets they attempt to represent. Nonetheless, many models succeed in providing epistemic access to numerous aspects of the world we are interested in. The central problem this thesis addresses is how epistemically successful representations are produced in science. I will adopt a framework of discussion that characterizes epistemic success in terms of understanding (Elgin 1996, 2017a). And I will defend that similarity, or more specifically "creative similarity", is fundamentally involved in the construction of epistemically successful scientific models. My account, the *creative similarity account of the means of representation*, should be taken as an attempt to rehabilitate the significance of similarity in the debate of representation by means of the study of its role in actual modelling practices in science, while refusing to identify similarity with the old metaphor of the mirror of nature.

The contents of the thesis are organized as follows. The starting point will be the distinction between two different questions about scientific representation: the question about the *constituents* and the question about the *means* of representation (following Suárez 2003, 2010). The question about the constituents asks 'in virtue of what is something a representation of a target in the world?', while the question about the means asks 'in virtue of what is something an epistemically successful representation of a target in the world?' There is agreement in philosophy of science that the question about the constituents demands a set of necessary and sufficient conditions for representation in response. Meanwhile, it has not been discussed sufficiently what exactly a response to the question about the means of representation requires. I will show throughout this thesis that proposing an account of the means of representation requires a systematic practical enquiry of the resources used in modelling practices. More specifically, it requires an analysis of the judgments scientists make during the design, construction, and evaluation of representations with regards to the targets they attempt to represent.

Chapters 1 and 2 are an investigation into the various accounts in contemporary philosophy of science that have debated the general problem of representation and the role of similarity in it. In Chapter 1 I will discuss the possibility of considering similarity, or structural similarity, a constituent of representation. That is, a necessary (and usually jointly with denotation) sufficient condition for representation. Works that advocate this view include French (2003), Bartels (2006), and French and Bueno (2011), among others. I will present two compelling arguments that should dissuade us from regarding similarity as a constituent of representation, namely Goodman's (1968) logical argument against similarity and the argument from misrepresentation. However, this conclusion will not be a reason to abandon similarity altogether.

In Chapter 2 I will examine the possibility of considering similarity the means (or a possible means) of representation. That is to say, I will discuss whether referring to the notion of similarity can help account for how epistemically successful representations are produced in science. Three arguments against similarity as a possible means of representation will be considered: the argument from variety, the argument from vagueness, and the argument from misrepresentation against the means. The conclusion this time will be that none of these arguments is conclusive enough to prevent us from offering an account of the means of representation focused on similarity. I will then consider some recent positions in the debate on representation that have been instrumental in rehabilitating similarity in an account of the means of representation, namely Suárez (2003, 2010), Contessa (2007a, 2011), Giere (2004, 2006), and van Fraassen (2008). One of the insights that will be gained from this analysis is that an account of similarity as means of representation should be framed in a triadic conception of representation that locates the *use* of representation by the relevant epistemic community at its centre. This approach stands in opposition to conceptions of similarity as two-term relations between a vehicle and a target, but also in opposition to triadic conceptions of representation that define similarity as an *intentional* notion, depending merely on individuals' purposes.

Chapter 3 will have a more methodological character. I will argue that an account of the means of representation requires both a practical investigation of the *use* of representation, and a normative component, as any genuine philosophical analysis requires. The fields of Philosophy of Science in Practice (PSP) and integrated History and Philosophy of Science (iHPS) will offer some useful methodological tools to help frame such an account. The main contribution of this chapter will be the claim that the study of 'judgments in scientific practice' in general, and 'judgments of similarity' in particular, is central for the advancement of an account of the means of representation, as it links the descriptive and the normative components of the account. Epistemic agents make constant judgments of similarity in the process of designing, constructing, and testing a broad range of scientific models. The source of the normativity of an account of the means of representation should be the description of the uses of these judgments, and how they are entrenched, regulated, and translated into norms and standards within the practice. The works of Weisberg (2013) and Sterrett (2009, 2017a) will help assess the role of judgments of similarity in scientific practices.

The analysis of actual practices of representing is fundamental to support the arguments in this thesis. Chapter 4 offers a detailed historical study of two cases of modelling in engineering: the Mississippi Basin Model (1943) and the San Francisco Bay Model (1953). I will explore the connections between the uses of judgments of similarity in these cases and the epistemic success of the models obtained. The selection of two scale models as case studies is motivated by the fact that traditional literature in philosophy of science has frequently underrated the epistemic value of scale models, and linked it to the erroneous assumption that scale models are superficially and naïvely similar to their targets. I will show that scale models have a genuine epistemic value way beyond illustrative functions; that judgments of similarity used in their construction are in great part responsible for the epistemic success they have; and that those judgments of similarity are in constant interplay with judgments of dissimilarity and distortions.

Finally, in Chapter 5, I will propose a specific account of the means of representation, the *creative similarity account of the means of representation.* This account advocates that 'creative similarity' is the means of representation in an epistemically relevant set of successful modelling practices in science, such as those I discuss throughout the thesis. The notion of 'creative similarity' is introduced to capture the way in which similarity intervenes in actual practices of representing, namely, in the form of a productive interplay of judgments of similarity and distortions that allows epistemic agents to produce fruitful representations that afford understanding of aspects of the world.

Chapter 1

The Constituents and the Means of Representation

The problem of representation has become a central topic of debate in contemporary philosophy of science. Although we can find isolated considerations of the notion of scientific representation since the late nineteenth century,[1] it only started to be systematically discussed in philosophy of science in the 1960s. At that time, the advancement of the semantic view motivated the study of scientific models, and with it the question of how scientific models represent natural phenomena gained centrality in the debate in philosophy of science. Finding a satisfactory response to this question has been considered a central endeavour of philosophers of science since then, given that it ultimately revolves around the crucial epistemological concern of how we learn about the world.

A 'vehicle', such as a scientific model, is used to represent a 'target system', such as the structure of DNA, the nucleus of the atom, the progression of a storm, the growth of carcinogenic cells, or the water flows in a bay, and make inferences about it. But what should a vehicle be like to perform such role? Watson and Crick's model of the DNA structure consists of a set of interconnected aluminium templates and rods; scale models of water flows are usually an ensemble of artificial pumps and water tanks; and mice are commonly used as model-organisms to represent cancer growth in humans. Scientific models always present us with

[1] Hertz's and Boltzmann's respective conceptions of scientific theories as images (*Bildtheorie*) are considered ancestors of account of representation in philosophy of science, particularly of structuralist views of representation (in Hughes 1997: 333; and van Fraassen 2008: 190-192).

idealized, schematic, distorted, and partial images of the aspects of the world they represent. So a central challenge philosophers of science face is to explain not only how artefacts like these models represent their targets, but more importantly, how they do so *successfully*, in a way that allows them to make fruitful inferences and predictions about the world. The subject of study of this thesis is the production of successful representations in science. I take successful representations to be those that, for the specific purposes at hand, are able to afford understanding of the world, despite, and frequently thanks to, the idealizations and distortions they involve (Elgin 2004, 2017a).

Scientific models are not the only vehicles used to make inferences about the world. Maps, pictures, photographs, narratives, and diagrams are also employed across the sciences, the arts, and other realms of everyday life to represent objects and states of affairs. Some philosophers have established comparisons between scientific representations and maps (Giere 1999; Contessa 2007a; Frigg and Nguyen 2017a), caricatures (van Fraassen 2008) and artworks (Suárez 2003, 2004; French 2003; Downes 2009; Elgin 2002, 2010, 2011, 2017b; Chakravartty 2010a; Ambrosio 2013) in an attempt to explore the specific ways in which scientific models work. Other philosophers of science have explicitly addressed the issue of whether or not there is special problem of *scientific* representation, different from artistic, mental, or linguistic representation. Callender and Cohen (2006) have, for instance, questioned the genuineness of the problem of scientific representation and urged philosophers to examine it in the context of wider debates in philosophy. This thesis does not directly address the problem of the demarcation between scientific and non-scientific representations. Nonetheless, I subscribe to the idea that philosophers of science have a good deal to learn from historical and contemporary debates in aesthetics, philosophy of language, and philosophy of mind, fields in which the problem of representation has a much longer tradition of discussion. In the last chapter of this thesis, I will specifically endorse the view that understanding is a central aim of both the practice of representing in science and in art (following Goodman 1968; Goodman and Elgin 1988), a reason for incorporating examples from artistic practices in an attempt to illustrate and support my own account of scientific representation.

In this first chapter, I begin (Section 1.1) by offering a brief overview of the changes that took place in contemporary philosophy of science regarding the debate on representation. The transition from the syntactic to the semantic view in the 1960s, and the flourishing of studies on scientific models defined how the problem of representation has been addressed until today. In Section 1.2, I start analysing the specific implications that the problem of scientific representation has. I particularly distinguish between two central questions about representation – both involved in the broader abovementioned question of how scientific models represent natural phenomena: namely, the question about the *constituents* and the question about the *means* of representation (Suárez 2003; 2010; van Fraassen 2008). The distinction between these two questions will be a guiding point of reference throughout this thesis. Without this distinction, I argue, the role of similarity in scientific representation would remain problematically opaque.

Returning to the concept of similarity, and its structural variants isomorphism, partial isomorphism, or homomorphism, has been a common way of addressing the problem of representation in philosophy of science, such as in the accounts of Giere (2004), French (2003), French and Bueno (2011), Bartels (2006), van Fraassen (2008), Contessa (2007a, 2011), and Weisberg (2013). The common intuition underlying all these varied accounts is that the fact that a scientific model represents a specific target in the world is *somehow* related to the fact that the model is question is similar to that target. Yet, positions regarding exactly how representation and similarity are related to each other can greatly vary. In Section 1.3, I will discuss a specific subgroup of accounts that hold a common view concerning the specific relation between representation and similarity, namely, accounts that consider similarity a *constituent* of representation. That is to say, I will examine accounts that claim that something (a vehicle) is a scientific representation of something else (a target system in the world) in virtue of a relation of similarity between them. I will then present two arguments against the view that similarity is a *constituent* of representation, namely Goodman's (1968; 1972) argument against similarity and the argument from misrepresentation. I will conclude that we should refrain from considering similarity, in any of its versions, a constituent of representation.

There are other subgroups of accounts that hold a different view of the specific relation between representation and similarity. Later, in Chapter 2, I will examine accounts that consider similarity a *means* of representation, that is, accounts that defend the central role of similarity in the production of accurate or successful scientific representations. In this case, I will also present three central arguments against similarity as *means* of representation, namely the argument from variety, the argument from vagueness, and the argument from misrepresentation against the means. But on this occasion, the conclusion will be different: if adequately characterized, similarity is a satisfactory concept to offer an account of the *means* of representation.

1.1. From syntactic to semantic to pragmatic: models in philosophy of science

In the syntactic view of theories, or "the received view", as it was called, reflecting its dominance during the first half of the twentieth century, the primary manifestation of scientific knowledge was scientific theories. Theories are defined in the received view as sets of axioms in a formal language that are truth-apt; that is, that can be individually evaluated as true or false. The role of scientific models in this conception was either completely disregarded or relegated to a merely illustrative or pedagogical one. Among the core group of logical positivists who endorsed this view, Carnap (1939: 68) famously claimed that "the discovery of a model has no more than an aesthetic or didactic or at best a heuristic value, but is not at all essential for a successful application of the physical theory". Maxwell's equations could, for instance, be presented in an "intuitive" manner, following Carnap (1939), by producing a model that involved visible known macro-processes. But such a model would be at best an illustration of the true axioms about electromagnetism that Maxwell's equations already contained (ibid.: 67). A similar view was previously held by Duhem ([1914] 1954: 32), who claimed that models are parasitic on the fully-formed theoretical units that carry genuine descriptions of physical phenomena. In short, models are in the syntactic view appendices to theories, mostly redundant, and with no crucial role in scientific development (in Bailer-Jones 1999: 24-25).

In the 1960s, the syntactic view experienced a phase of decay at the same time as the programme known as the semantic view of theories emerged. The origins of the semantic view can be traced back to Suppes (1960), with further developments by van Fraassen (1980), Giere (1988), and Suppe (1989). From the 1980s onward, the semantic view gained such wide acceptance in the philosophy of science that it became the orthodox view on models and theories, or "the new received view" (Frigg 2006: 51; Contessa 2006). In contrast to the syntactic view, advocates of the semantic view define scientific theories as families of models. Scientific models are objects and not linguistic entities, and as such they cannot be evaluated as true or false in the literal sense in which sentences can. Models can at best be expressible in statements about the empirical world whose truth or falsity can be then assessed (Bailer-Jones 2003: 60). It is precisely under this conception that the question of how models represent natural phenomena arises. To explain how models represent their targets, philosophers in the semantic view appeal to the set-theoretic structures of models: models represent certain phenomena if they share the same structure. Sharing the same structure has been described in terms of similarity, embedding, isomorphism, and partial isomorphism (van Fraassen 1980; French 2003; French and Bueno 2011).

The problem of scientific representation is hence historically linked to the shift from the syntactic to the semantic view as well as to the rise of the study of models in science. Despite the wide endorsement of the semantic view in contemporary philosophy of science, there have been some detractors. Halvorson (2012: 184) has, for instance, opposed the default definition of scientific theories that the semantic view provides, given that they are treated *only* as collections of models. His proposal can be seen as a partial rehabilitation of the syntactic view.[2] Frigg (2006: 62) argues along the same lines. He affirms that the development of the semantic view has evolved into a state where language *per se* seems to be irrelevant to the analysis of scientific theories and models, while actually scientific representations involve an intricate mixture of linguistic and non-linguistic elements.

From a different perspective, and with a stronger repercussion for the current debate on representation, there has been an increasing number of detractors of the semantic view who argue that, in fact, the syntactic and the semantic view are not so different from each other. A shortcoming that the two programmes share concerns how they address the use of models in scientific practice (Morgan and Morrison 1999; Cartwright et al. 1995; Cartwright and Suárez 2008; Contessa 2006). If models are dispensable tools in the syntactic view, they are just set-theoretic structures in the semantic view. None of these views allows us to account for the richness of roles that models, as objects that can be deployed, manipulated, and applied, perform in scientific research. For instance, the way in which the semantic view characterizes models cannot elucidate how many models are constructed with relative independence from a theory. Successful

[2] See Halvorson (2012: 184): "Regrettably, philosophers have been too quick to jump onto the semantic bandwagon, and they have failed to test the semantic view as severely as they tested the received view [...] In particular [...] the semantic view makes incorrect pronouncements about the identity of theories, as well as about relations between theories".

model building is frequently non-theory driven (Cartwright and Suárez 2008: 64). The London model of superconductivity is an example of that. It was constructed counting on the help of some background theory, but that doesn't mean that the model was already contained in the theory, waiting to be extracted from it, as the customary approach in the semantic view would claim (ibid.: 63). Additionally, when models are identified with set-theoretic structures that aim to be isomorphic to target systems in the world, the possibility of models misrepresenting those targets becomes a central challenge for philosophers in the semantic view. Yet, it is widely recognized among philosophers of science that the possibility of misrepresentation, via abstraction, approximation, and idealization, is a fundamental feature of how models are used by scientists (Suárez 2003: 233-235; Contessa 2006: 372-5; Frigg 2006: 51; van Fraassen 2008: 13-15). I discuss the problem of misrepresentation in more detail in Section 1.3.2.

These criticisms of the semantic view are connected to a relatively independent shift that also took place in the debate on representation in the 1960s and 1970s. A progressive transition from formal accounts to pragmatic and functional accounts of models took place at this time (Bailer-Jones 1999: 32-35). The exploration of actual modelling practices was an interest of some of the subscribers of the semantic view, but not of the view as a whole. This explains, following Bailer-Jones (1999), the varieties of accounts within the semantic view, some much more practice-oriented than other, as well as the numerous works on modelling practices that did not endorse the rationale of the semantic view. Suppes (1960), first and principal promoter of the semantic view, endorsed a formal type of account of models, his main concern being how to systematically accommodate the relation between models and theories (in Bailer-Jones 1999: 32). Giere's (1988, 2008) work is a case in point. He has always identified himself with the semantic view, but recognizes that it took him many years to realize that his understanding of models was not the same as Suppes's (in Giere 2008: 10n). While Suppes's models are "instantial" or "interpretational", for Giere (ibid.), models are primarily representational, as they provide above all tools for representing the world and not tools for interpreting formal systems.

Meanwhile, other philosophers, like Hesse (1966) and Morgan and Morrison (1999), more openly favoured characterizations of models supported by scientific practice, emphasizing their role in scientific discovery (in Bailer-Jones 1999: 34). A particularly influential functional type of account of models, that detached itself from the semantic view, was advanced in *Models as Mediators*

(Morgan and Morrison 1999). The central claim of the book was, as the title suggests, that models mediate between theory and phenomena, not because they are dependent on theory, but precisely because they function as autonomous objects (ibid.: 43). The tension between formal and functional accounts of models made evident that the more philosophers tried to grasp the varieties of roles models played in practice, the harder it was to provide a systematic and single definition of model (Bailer-Jones 1999: 35). And vice versa, the more the emphasis was on a general definition of model and on the formalization of the mapping between models and phenomena, the harder it was to account for the variety of formats and functions models in practice played.

Along with these debates, sociologists, anthropologists, and historians of science were also resolved to get into the muddy waters of everyday scientific practices in the 1960s and 1970s. A considerable number of scholars in these fields were committed to challenge the too *whig*, clean and theoretical accounts of science offered by the previous generations. Bloor (1976), Collins (1981), Knorr-Cetina (1981), and Lynch and Woolgar (1990), from the sociology of science; and Kuhn (1962; 1977), Shapin (1982), Shapin and Schaffer (1985), and Pickering (1984), from the new historiographies of science, influenced the debate on scientific representation with their various practice-sensitive accounts. Their studies stressed that an important part of everyday scientific work consists in the design and manipulation of representations, either as physical models, computer simulations, graphs, or diagrams, reinforcing the idea that the notion of representation should be first and foremost understood as 'activity of representing', instead of as 'finished product' obtained at the end of that activity. Some of these scholars even went inside laboratories to observe the progressive construction of representations, from the work in the field, to the collection of data, the design of experiments, and the final publication of papers in journals (see Latour and Woolgar 1979 for a classic account of the ethnographies of the laboratory). Philosophers of science like Giere (2006: 7-13) and van Fraassen (2008: 376 n17) have openly recognized the influence of these works in the pragmatic turn that their own accounts of representation experienced. It is worth drawing attention to the fact that the study of scientific practices was a project that went way beyond the interests of a group of philosophers in or outside the semantic view.

1.2. Two questions about representation

The increasing attention to scientific models and the transition to the semantic view brought to the fore of the debate the so-called problem of scientific representation. However, in more recent years, a group of philosophers started to suspect that the problem of representation involved slightly different, conflating issues (Suárez 2004, 2010, 2015; Frigg 2006; Hughes 1997; Contessa 2011). When asking how scientific models represent natural phenomena, we might be interested in elucidating the conditions for scientific representation. That is, we might want to address the question of "in virtue of what is something a scientific representation of something else?" I will abbreviate this first question about representation to *R1*. Alternatively, we might be interested in discussing the problem of the accuracy or success of scientific representations; that is, we might want to address the question of "in virtue of what is something an accurate or successful scientific representation of something else?" I will abbreviate this second question to *R2*. Some philosophers of science have conflated *R1* and *R2* in the past. Fortunately, Suárez (2010: 93) claims, this is no longer the case, and nowadays representation is carefully distinguished from true, accurate or faithful representation. One reason that it is important to differentiate the two questions is to be able to properly address the role of similarity in representation, as I will further explain in Section 1.3 and Chapter 2. Another reason is that the distinction between *R1* and *R2* helps make sense of the phenomenon of misrepresentation. If we want to explain why a certain model is, for instance, not a good representation of a target, we have to accept first that the model in question is a representation of that target. Only then we can discuss the reasons for its inaccuracy. In other words, misrepresentation is a species of representation after all (van Fraassen 2008: 13).

Regarding the first question, *R1*, we could say that it demands a close, universal type of answer in the form of necessary and sufficient conditions for representation. A standard answer to *R1* would have the form: "Something (a vehicle) is a scientific representation of something else (a target) if and only if_____" (Frigg and Nguyen: 2016). *R1* has also been characterized in other ways: as "the constitutional question", as it seeks to elucidate what constitutes the relation of representation (Suárez 2010: 92; Callender and Cohen 2006: 2); as the problem of "mere representation" (van Fraassen and Sigman 1993; Bolinska 2013); and as the problem of "representation *simpliciter*" (Contessa 2007b: 50). In addition, Suárez (2010: 94-95) characterizes all the accounts that affirm that it is possible to respond to *R1* with a close set of necessary and sufficient conditions as "substantive accounts" of representation. Defenders of substantive accounts agree that representation is a robust relation between a source and target that can be further analyzed in terms of something else.[3]

Meanwhile, *R2* has been defined as a question that concerns the accuracy or faithfulness of representation (Suárez 2004; Contessa 2007b), the "standards of accuracy" of representation (Frigg and Nguyen 2016), and the "normative issue of what it is for a representation to be correct" (Callender and Cohen 2006: 3). The answers to *R2* admit of degrees, as representations can be more or less accurate, more or less faithful, unlike *R1*, which demands conditions that are either satisfied or not satisfied. In addition, van Fraassen (2008: 15) identifies *R2* with the question of "*how* does a representation represent?", since only when we take for granted that something (a picture, a model) is a representation can we enquire *how* it can "correctly represent, misrepresent, caricature, flatter, or revile its subject". Frigg (2006: 50) points out a distinction between two variants of the *how*-question, or the *problem of style*, as he also calls it, the descriptive and the normative variants. There are different styles of representation in science, from mathematical models to physical models, images, and graphs. The descriptive variant would entail a taxonomy or portrayal of how different models represent in different ways, while the normative variant would involve the search for rules or standards of correctness, that is, of the acceptable and unacceptable styles of representing (ibid.).

The terminology I will follow more closely to identify the set of issues involved in *R1* and *R2* is Suárez's (2003: 229-230) distinction between the *constituents* and the *means* of representation. The study of the constituents requires a type of investigation that Suárez calls an 'analytical inquiry', and the study of the means requires a 'practical inquiry' (2010: 91-2). This is because the constituents are unique and universal relations between a target and a vehicle, while the means are sensitive to the particular context in which representations are actively employed by epistemic agents to draw inferences from a vehicle about a target (ibid: 93). We could apply Frigg's (2006) distinction here, and claim that there are two possible variants of an account of the means of representation. A descriptive

[3] More precisely, Suárez (2010: 95) specifies that substantive accounts of representation can be *primitivist* and *reductive*. But since substantive accounts are more naturally reductive, and that is the case among the contemporary proposals of representation I am going to refer too, I will take substantive accounts as being reductive as well.

variant of an account of the means of representation would typically entail a type of work done on a case-by-case basis, and it might advance some kind of taxonomy of possible means of representation (Suárez 2010: 93; Frigg 2006). A normative variant of the problem of the means of representation would alternatively, or additionally, judge how different means are able to provide more or less accurate representations. Later in this thesis, I will argue that the two variants are important for the development of a comprehensive account of the means of representation, and expand on the original definition of *means* that Suárez (2003; 2010) offered.

Even though I will follow the distinction between *R1* and *R2* (or between the *constituents* and the *means* of representation) throughout this thesis, it is worth taking into account two considerations. First, when we adopt a practice-based perspective on the problem of representation, separating issues concerning the mere conditions for representation and issues concerning the epistemic success of representation is somewhat an imposed, analytical type of exercise, as practices of representing do not divorce these matters, but typically address them together. In other words, practices of representing directly aim at constructing successful enough representations of the target under investigation. Still, Suárez (2010) stresses, when conceptual analysis of the problem of representation is required, to make sense (among other issues) of the phenomenon of misrepresentation, "puzzles regarding the notion of representation are *prior to* and *independent of* issues of accuracy" (2010: 93).

Second, if we incorporate the notion of *epistemic representation* into the debate, as for instance Contessa (2007b) and Bolinska (2013) do in their accounts, the distinction between *R1* and *R2* to some extent blurs, but, I believe, for justified reasons. Bolinska (2013) claims that "the concept of epistemic representation applies both to cases of mere and of faithful representation" (2013: 222). She seems to be conflating *R1* (or the problem of mere representation) and *R2* (or the problem of faithful representation), arguing that both are equally cases of epistemic representation. However, what is at stake here is that Bolinska (2013) is not really addressing the very general, abstract formulation of the constitutional question, or *R1*, in the terms in which Suárez (2003; 2010) defines it. She is concerned with actual uses of representations, which have to be in practice at least minimally accurate, or in her terms "minimally informative" about a target, to be able to allow scientists to perform inferences with them (2013: 226-7). She clarifies that we might perform false inferences using an epistemic representation. But it is difficult to believe that with an epistemic representation, which in her definition is at least minimally informative, all inferences made are wrong (ibid.). The distinction between *R1* and *R2* blurs in Bolinska's (2013) account because mere representations have to be at least minimally accurate or successful.

Contessa (2007a) seems to follow the same line of argument. He rephrases questions *R1* and *R2* as three possible questions about representation: one concerning *denotation*, one concerning *epistemic representation*, and one concerning *faithful epistemic representation* (2007a: 52). Leaving the particularities of denotation aside, Contessa identifies *epistemic representation* with the problem of *R1*, and *faithful epistemic representation* with the problem of *R2*, as formulated by Suárez (2003). However, it seems that for Contessa (2007a: 52-53) epistemic representations require some degree of accuracy, in the sense that epistemic representations have to have the right kind of features to allow epistemic agents to perform (actually or potentially) surrogative inferences about a target with them. Like Bolinska (2013), Contessa (2007a) is also blurring the distinction between *R1* and *R2*, due to the fact that he is not directly addressing the more abstract, constitutional question that seeks universal conditions for representation.

A closely related discussion in the debate over scientific representation is whether *R1* is in fact a relevant epistemological question that is worth addressing. We find, on the one hand, philosophers like Suárez (2015), van Fraassen (2008), and earlier on Hughes (1997), who explicitly reject the pertinence of searching for conditions of representation, and defend deflationary accounts of representation. On the other hand, philosophers like French (2003), French and Bueno (2011), and Frigg (2006) have argued in favour of the value of asking the constitutional question of representation.[4] Deflationary accounts argue that we shouldn't try to explain representation in terms of something more elementary than itself, such as denotation, similarity, or instantiation. At best, they claim, we can assert that a scientific representation is whatever is used as such by epistemic agents; and maybe also they would agree that scientific representations might typically have certain features in common.

Van Fraassen (2008), whose work has noticeably transitioned into pragmatics recently, offers a *Hauptsatz* of representation in strong deflationary terms: "there is no representation except in the sense that some things are used,

[4] In a more recent version of Frigg's (2006) account, Frigg and Nguyen (2016; 2017a) clarify that the problem they are addressing is in fact the problem of *epistemic representation*, and not exactly the problem of the constituents representation (*R1*) as formulated by Suárez (2003; 2010).

made, or taken, to represent some things as thus and so" (2008: 23). Introducing the term *Hauptsatz* in this context is precisely a way of avoiding the claim that he is offering a "definition" of representation, as definitions are normally close sets of necessary conditions, whereas van Fraassen is (merely) calling attention to the fact that the *use* made of representations is the only distinctive feature that they have in common. Suárez's (2003; 2004; 2015) deflationary view is very close to van Fraassen's (2008), especially in his version of deflationism, which he calls "use-based" (2015: 45). A use-based deflationary account of representation identifies a representation essentially by the features of its use in practice; that is to say, it identifies the constituents of a representation with its means (Suárez 2015: 45).[5] Previous to these accounts, Hughes's (1997) DDI account can be considered an early deflationary approach to the problem of representation.[6] For Hughes (1997), even if it is problematic to search for conditions of representation, it is still possible to point out some general symptoms of scientific representation. The DDI account stands for denotation, demonstration and interpretation, the three main symptoms of scientific representation that Hughes's (1997) identifies. He clarifies this point as follows:

> Let me forestall possible misunderstandings. I am not arguing that denotation, demonstration, and interpretation constitute a set of speech acts individually necessary and jointly sufficient for an act of theoretical representation to take place. I am making the more modest suggestion that, if we examine a theoretical model with these three activities in mind, we shall achieve some insight into *the kind* of representation that it provides (Hughes 1997: 329).

Hughes (1997) depicts a situation in which we already accept something as a "theoretical model". Hence, if we examine that model while having in mind the activities of denotation, demonstration, and interpretation, we can gain further insight into the kind of representation it is, namely a scientific representation. The three symptoms Hughes (1997) suggests should not be taken as jointly sufficient conditions for representation, but as the main activities involved in the production

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[5] Meanwhile, the "no-theory" version of Suárez's (2015) deflationary account claims that representation has no constituents, only means; that is, it accepts the possibility of pointing out general features of representation while refusing to offer necessary and sufficient conditions for it. Lastly, the "abstract minimalist" version of Suárez's deflationary account postulates that representation is *abstractly* constituted by representational force and inferential capacity (ibid.: 45).
[6] In particular, Hughes's (1997) deflationist position coincides with the "no-theory" version of deflationism that Suárez (2015) describes.

of scientific models. Notwithstanding, Suárez (2015: 43) is justified in arguing that, among the three activities that Hughes mentions, demonstration and interpretation are indeed terms commonly understood as activities performed by epistemic agents. This is not the case for denotation, which is usually taken as a substantive relation between a vehicle and a target, and not as an activity. Thus, we might want to question the strength of Hughes's (1997) commitment to deflationism, despite his explicit rejection to offering conditions of representation. Or alternatively, we could reinterpret denotation in this context, as Suárez (ibid.: 44) proposes, as "denotative function". In this way, denotation will more clearly describe an activity performed by epistemic agents trying to establish that a model denotes a target, or using a vehicle as a denotative tool even when it doesn't denote any specific target in the world (ibid.).

I will not explicitly commit to a particular view in the debate of whether it is convenient or not to adopt a deflationary account of representation. The main reason for that is that I will concentrate on advancing an account of the means of representation, and not of the constituents of representation. Moreover, the specific account of the means of representation I will propose in Chapter 5, which adopts a perspective in philosophy of science in practice (PSP), could be in principle adopted by deflationary and by substantive accounts of representation. At any rate, I am especially sympathetic to the perspectives on representation that the aforementioned deflationary accounts endorse (Hughes 1997; van Fraassen 2008; Suárez 2015), given that they have helped shift much of the attention in philosophy of science from the problem of the constituents of representation to the (possibly more enlightening in epistemological terms) problem of how accurate or successful scientific representations are produced. In the next section, I will start to examine the role of similarity in scientific representation. I will specifically argue that similarity is not a good candidate to be a constituent of representation, despite various attempts to defend this idea in the recent literature in philosophy of science. Later, in Chapter 2, I will further contend that this is not a reason to reject similarity altogether, and advocate the possibility of investigating its role within an account of the means of representation.

1.3. Constituents of representation and similarity

Considerations of similarity as a necessary and/or sufficient condition for representation concern, for reasons previously exposed, only substantive accounts of representation that attempt to respond to *R1*, and not deflationary accounts of representation. Some substantive accounts of representation, such as those of French (2003), French and Bueno (2011), and Bartels (2006), claim that similarity, or more specifically a version of structural similarity, is a constituent of representation. Other substantive accounts of representation, such as Frigg's (2006), reject similarity in any form as a constituent of representation. Still other substantive accounts, such as Contessa's (2007a; 2011), reject similarity as a constituent of representation but attempt to offer a general account of accurate representation based on similarity.

I treat the case of Giere's (2004, 2008, 2010) account as a special one. Giere has not always been completely clear about the exact role that similarity plays in his account of representation, although it visibly plays a fundamental role. He doesn't explicitly consider the distinction between what I call *R1* and *R2*, nor does he refer to the distinction between the constituents and the means of representation. Sometimes he seems to defend a substantive account of representation based on similarity, for which he has been repeatedly criticized (see Frigg 2006; Elgin 2010). An example of this is found when Giere claims that similarity is "the basic relationship between models and the world" (2010: 269). Some other times, however, Giere (2004) openly argues that similarity does not constitute the relation of representation: "I am not saying that the model itself represents an aspect of the world because it is similar to that aspect. There is no such representational relationship" (ibid: 747). Here, similarity is not taken as a condition of representation, but as a response to the problem of accurate representation or *R2*. I will assume from now on that the second position, that Giere (2004, 2008) more openly expresses, is the correct way of interpreting his account. Thus, I refer back to Giere's views in Chapter 2, when I focus on similarity-based accounts of the means of representation.

In the rest of this chapter, I address the various difficulties of attempting to respond to the constitutional question by alluding to the relation of similarity between a vehicle and a target. First, I discuss Goodman's (1968; 1972) logical argument against similarity. Although Goodman doesn't explicitly refer to morphisms or structural similarities, his argument applies to the project of responding to *R1* appealing to similarity in any form, including structural accounts like those of French (2003), French and Bueno (2011), and Bartels (2006). Second, I introduce the argument from misrepresentation to, again, cast doubts on substantive accounts of representation based on similarity or structural similarity. I conclude by claiming that the legitimate intuition that similarity is involved in representational practices should be addressed in the debate concerning the epistemic success or the means of representation.

1.3.1 Goodman's argument against similarity

The argument against similarity presented by Goodman in *Languages of Art* (1968) and "Seven strictures of similarity" (1972) has been followed by numerous contemporary philosophers of science, such as van Fraassen (2008), Suárez (2003), Hughes (1997), Contessa (2007a), Frigg (2006), and Toon (2012) as a starting point to situate their own accounts of scientific representation. Goodman (1968) opens *Languages of Art* with this assertion:

> The most naïve view of representation might perhaps be put something like this: 'A represents B if and only if A appreciably resembles B'. Vestiges of this view, with assorted refinements, persist in most writing on representation. Yet more error could hardly be compressed into so short a formula (Goodman 1968: 3-4).

The "most naïve view of representation" consists for Goodman in reducing representation to a relation of similarity. Which accounts is Goodman specifically reacting to here? There isn't to my knowledge any account in modern aesthetics or philosophy of science that supports a purely naïve view of representation as defined by Goodman. That is, there are no accounts that sustain that similarity on its own is the necessary and sufficient condition for representation. Since Goodman (1968, 1972) doesn't offer the names of his opponents, his argument should be taken more as a general criticism of all the watered-down or "vestiges of [the naïve view that], with assorted refinements, persist in most writing on representation" (1968: 3-4). A "watered-down or innocuous-looking" view of similarity would for instance be one that takes similarity to be the key to distinguish representation from other types of symbolization (1968: 5-6).[7] Goodman believes that both the naïve and the watered-down views are fundamentally mistaken. Vestiges of these views are found, among other places, in substantive accounts of representation based on similarity in philosophy of science, including those that sustain that structural similarity (homomorphism, partial isomorphism) is a necessary, and jointly with denotation, sufficient, condition for scientific representation (French 2003; Bartels 2006).

To reject these views, Goodman (1968) proposes the so-called 'logical argument' against accounts of representation based on similarity. The main claim of this argument is that similarity cannot constitute the relation of representation because while similarity entails symmetrical, reflexive and transitive relations, representation entails asymmetrical, non-reflexive and non-necessarily-transitive relations. A is as much like B as B is like A, Goodman (1968: 4) argues, but the fact that a painting represents the Duke of Wellington doesn't imply that the Duke of Wellington represents the painting. Similarity is symmetrical, while representation is not. Likewise, an object resembles itself to the maximum degree but rarely represents itself; resemblance, unlike representation, is reflexive (ibid.). Also, if object A is similar to object B, and object B is similar to object C, we would claim that A and C are similar to each other as well. In contrast, if A represents B and B represents C, there is not necessarily a representational relation between A and C (ibid.: 4-5). For instance, Diego Velázquez's *Portrait of Innocent X* (1650) was a representation of Pope Innocent X. There is also a series of canvases painted by Francis Bacon in 1953 under the title of *Study after Velázquez's Portrait of Pope Innocent X*, about which we would claim that they represented Velázquez's painting. However, it would be inadequate to claim that Bacon's painting represents Pope Innocent X (Suárez 2015: 449). Similarity entails transitivity relations while representation does not necessarily. The consequence of Goodman's logical argument is that representation cannot be explained in terms of similarity.

There are some accounts that have tried to undermine Goodman's logical argument by claiming that similarity actually does not always entail symmetrical, reflexive and transitive relations. Tversky (1977), from the perspective of

[7] Goodman (1968: 4 n1) understands the term representation as "pictorial representation, depiction, and the comparable representation that may occur in the other arts", such as paintings and sculptures. Symbolization is the term Goodman uses for all varieties of representation, including pictorial, but also verbal, and any other type of non-pictorial representation.

experimental psychology, argued that it is contentious to make such general claims about similarity without considering how individuals actually make judgments of similarity about everyday objects. To support this idea Tversky and Gati (1982) conducted various experiments in which a group of subjects judged similarity relations between countries, figures, letters and signals (Tversky 1977: 333-336; Tversky and Gati 1982). The results showed that experimental subjects would usually claim things like "an ellipse is similar to a circle", but not "a circle is similar to an ellipse", or they would say that "North Korea is like Red China" but not that "Red China is like North Korea" (Tversky 1977: 328). In other words, when looking at actual judgments, similarity does not necessarily involve symmetrical relations. Equivalent counterexamples were offered by Tversky (1977) to question the supposed transitivity of similarity. Experimental subjects would for instance claim that while Jamaica is similar to Cuba (because of geographical proximity) and Cuba is similar to Russia (because of their political affinity), Jamaica and Russia are not similar at all (1977: 329). Examples like this, Tversky argues, cast doubt on the general assertion that transitivity is a cornerstone of a definition of similarity (ibid.).

Goodman (1968) must have been aware of potential counterexamples like these regarding the symmetry and transitivity of similarity. That is probably why instead of giving specific examples of symmetrical similarity relations, he uses the much more abstract, undefined expression "if *A* is similar to *B*, then *B* is similar to *A*..." (1968: 4), where A and B appear to be interchangeable for almost anything. But then, when Goodman describes the asymmetry of representation immediately afterward, he offers a real, concrete, and unambiguous example: "while a painting may represent the Duke of Wellington, the Duke doesn't represent the painting" (1968: 4). If one substitutes the A and B in the first sentence for actual examples though, as Goodman does in the second sentence with the example of the Duke of Wellington, the claim that similarity is always symmetrical is much less convincing. That is, if he had claimed that if a painting is similar to the Duke of Wellington, then the Duke of Wellington is similar to the painting, we would have had at least suspicions about the claim that similarity always establishes symmetrical relation, as we usually recognise a portray as being similar to the person that is portrayed, but rarely do it the other way around.

Apart from symmetry and transitivity, Goodman (1968) argues that similarity establishes reflexive relations, while representation doesn't. It is more difficult to find counterexamples to this point based on how subjects make judgments of similarity. Something always resembles itself to a maximum degree, even if we shift the discussion to how judgments are made in practice. In some exceptional circumstances, we might want to claim that a picture represents itself, such as in the celebrated case of the "built-in reflexivity of *Las Meninas*", a painting that represents, among other things, the act of its being painted (Suárez 2003: 233; on the reflexivity of *Las Meninas* see Foucault 1973: 3-18). Still, we would have to admit that exceptions like this do not suspend the general claim that something is always similar to itself while it rarely represents itself. There have been, nonetheless, attempts to undermine Goodman's argument concerning reflexivity. Dipert (1996), for instance, argues that if we redefine the concept of similarity we could avoid the objection from reflexivity (ibid.: 382). His alternative definition of similarity, which he takes as synonym of resemblance in his argument, goes as follows:

> X resembles Y [to extent N with respect to P] iff:
> (1) X shares [N properties of type P] with Y, and
> (2) X is a distinct entity from Y: X is neither identical to Y, nor is X a constituent of Y, nor is Y a constituent of X (Dipert 1996: 382).

Adding (2) to the definition involves including a clause such that similarity –or resemblance – always concerns the relation between two different objects and not between an object and itself. In this way, the principle of reflexivity wouldn't apply to the definition of similarity. However, we might want to reply that this is too *ad hoc* a strategy to get around the objection of reflexivity. Dipert (1996: 382) argues that actually the most common meaning of similarity involves different objects, not objects being similar to themselves: "if asked as an exercise to go out into the world and remark upon some 'resemblances' in the world, I would not fixate upon, or even offer, the many cases in which things resemble themselves" (1996: 382n). Dipert's (1996) reply is again supported by common judgments of similarity subjects would make. But since his argument requires a redefinition of similarity, which is not firmly justified apart from helping to elude the problem of reflexivity, we shouldn't consider it a conclusive response to Goodman's logical argument with respect to reflexivity. The stronger discrepancy between similarity and representation concerning reflexivity has been pointed out by Suárez (2003). He claims that "any theory of similarity must concede this: similarity comprises identity; identity is a limiting case of similarity. Here representation and similarity definitely depart, for the vast majority of representations patently do not represent themselves" (ibid.: 239). Even if we are convinced by Tversky's (1977) argument on the asymmetry and intransitivity of similarity, the point about reflexivity should restrain us from endorsing the naïve view on representation that Goodman (1968) attacks.

In recent philosophy of science, some accounts of representation have tried to respond to Goodman's (1968) logical argument using a strategy similar to Tversky's (1977). French and Bueno (2011), for instance, defend a substantive account of representation based on partial isomorphism – their version of structural similarity. Their claim is that, indeed, partial isomorphism entails symmetrical relations between the structures of the vehicle and the target of the representation, but, French and Bueno (2011) argue, this is not a problem for defining scientific representation in terms of partial isomorphism because, if we consider the domain in which the representational process takes place as a whole, other factors such as the intended use of the representation break the symmetry of the relation vehicle-target (2011: 885). Their argument consists in stressing that epistemic agents eliminate the *in abstracto* symmetrical relationship of similarity between vehicle and target when they define which objects are similar.[8] There is, nonetheless, something suspicious with how French and Bueno (2011) insert the role of epistemic agents and the "domain in which the representational process takes places as a whole" into their account. Pragmatic elements such as the intended use of representation only break the symmetry of the isomorphism relation if they also constitute (or are built into) the relation of representation. However, for French and Bueno (2011) the partial isomorphism between a vehicle and a target is what really constitutes the relation of representation, while intentions, uses, and other pragmatic elements are only external factors surrounding the mechanism of representation (2011: 886). They explicitly affirm that "building particular intentions into the representational mechanism has disastrous consequences for an account of representation" (ibid.). Far from being disastrous, I will argue through Chapters 2 to 5 of this thesis that there are good reasons to think that an account of representation should be in agreement with actual representational practices, by seriously considering the purposes, uses,

[8] Giere (2008, 2010) presented a very similar argument, claiming that if we take into consideration the agents doing the representing and their intentions, the problem of the symmetry of similarity disappears (2008: 103; 2010: 274). The fact that Giere (2008, 2010) is responding to Goodman's (1968) logical argument is, at any rate, a sign that on certain occasions he seems to sustain a substantive account of representation based on similarity, since an account of the means of representation would not need to defend itself from the logical argument.

contexts, and pragmatic judgments involved in the stabilization of successful relations of representation. The difficulty with French and Bueno's (2011) account lies in the lack of clarity about how the symmetry of the isomorphism relation between vehicle and target can be broken if pragmatic elements are only accidental to the representational mechanism.

More successful is Bartels's (2006) account in this respect. He openly accepts Goodman's (1968) criticism of similarity, and agrees that similarity lacks the logical properties to explain representation. But his account of homomorphism, Bartels (2006: 10-11) adds, is immune to such criticism since it incorporates a distinction between the *reference* and the *content* of a representation. The *content* is what should be explained in terms of similarity (homomorphism in his view), while the reference is independent from, and not explainable in terms of, similarity (2006: 13-14). Both reference and content are requisites for representation: therefore Bartels is not adopting the naïve view that reduces the relation of representation to a relation of similarity. The important point is that homomorphism is a relation that occurs when the content of a vehicle entails a target, so homomorphism can only occur once the vehicle in question refers to the target thanks to a representational mechanism (2006: 13-14). That is to say, homomorphism does not involve a symmetrical relation between the structure of a vehicle and the structure of a target, because denotation, which goes only in one direction, from vehicle to target, is a prerequisite for homomorphism.

Some limitations in Bartels's (2006) account can be pointed out though. So far, I am treating accounts based on structural similarity (homomorphism, partial isomorphism) as particular cases of accounts based on similarity, and I argued that Goodman's logical argument applies to all of them. Yet, Bartels (2006: 17) explicitly differentiates between his characterization of the concept of homomorphism and the concept of similarity that Goodman (1968) uses and criticizes. However, there is a sense in which, even if homomorphism does not entail symmetrical relations as Bartels argues, if A is homomorphic to B, then A and B have some structure in common, and are thus similar in that respect (Chakravartty 2010b: 199n). So Bartels would be somehow missing the point with his categorical distinction between similarity and homomorphism (ibid.). In addition, Bartels (2006) is not convincing when responding to the problem of reflexivity. He tries to defend that if we distinguish between potential and actual representations, the problem of reflexivity is not fatal (2006: 11). Homomorphism would only be reflexive in an *in abstracto* or potential representation, whereas in actual representations, homomorphism would be non-reflexive, as it would concern the relation between two objects, a vehicle and a target. It remains to be seen if the distinction between potential and actual representation can offer a definitive solution to the problem, as the logical argument presented by Goodman (1968) principally concerns the analytic, conceptual definitions of representation and similarity in a potential or abstract sphere.

At any rate, Goodman (1968, 1972) would have not accepted the type of refinement of the naïve view of representation that philosophers like Bartels (2006) propose. The reason for this goes beyond his logical argument against similarity. Even if one acknowledges the distinction between the content and the reference of a representation, as Bartels claims, we still need to know in what respect the content of a representation would be similar to the target. And, Goodman (1972) argues,

> When, in general, are two things similar? [...] since every two things have some property in common, this will make similarity a universal hence useless relation. That a given two things are similar will hardly be notable news if there are no two things that are not similar. (Goodman 1972: 443)

Anything can be similar to anything else in at least some respects. Therefore, if true, it would be trivial to consider similarity a necessary condition for representation. This difficulty appears unsolvable for Goodman, and philosophers like Bartels (2006) don't offer explicit answers to it. Goodman (1972) is open to recognizing that some people may want to shift the discussion "from a categorical to a comparative formula" (Goodman 1972: 443). That is, they might want to say that even if it is useless to plainly say that two things are similar to each other, they can focus on the number of properties those things share, and on the importance of the properties they share, to claim that some objects are more similar to each other than other objects. This solution is for Goodman (1972) not satisfying either, insofar as determining the number of common properties and defining importance are "highly volatile matters" (ibid.: 443). I will come back to the issue of the *importance* of some similarities with respect to others, and to the problem of the "high volatility" of importance in Chapter 2, when I refer to the "argument from vagueness" against similarity and to Giere's (2004, 2008) account of "similarity in respects and degrees", which has been frequently criticized for its volatility or imprecision.

I have tried to show that Tversky (1977), Dipert (1996), French and Bueno (2011), and Bartels (2006), among others, have advanced some noteworthy ideas in response to Goodman's (1968, 1972) argument against similarity. Still, Goodman's criticisms remain strong enough to prevent us from proposing a substantive account of representation based on similarity in the naïve version, but probably also in its watered-down, refined versions. Goodman (1968) was primarily trying to dispute analytical attempts to offer universal explanations of representation based on similarity. So the fact that practical judgments of similarity are not always symmetrical or transitive doesn't really challenge the core of his criticism, since the move to judgments can be taken as a refocusing of the problems of representation and similarity more than as a solution. In addition, Goodman's (1968) argument concerning reflexivity hasn't been properly contested so far. We saw that Dipert's (1996) and Bartels's (2006) attempts to respond to it were not completely convincing, while French and Bueno (2011) didn't address it directly. In short, trying to reduce the relation of representation to a more basic relation of similarity or structural similarity between a vehicle and a target hasn't been very successful so far.

Before concluding this subsection, I would like to point out two further lessons that Goodman's work, especially *Languages of Art* (1968), can teach us about how to address the problem of representation, beyond his logical argument against similarity. The first is that we should reject the project of advancing a 'copy theory of representation' that assumes that representing is attempting to imitate reality. Copy theories involve the assumption that there is a clear, unambiguous reality to be imitated by our representations. To this, Goodman (1968) replies:

> The object before me is a man, a swarm of atoms, a complex of cells, a fiddler, a friend, a fool, and much more. [...] If all are ways the object is, then none is *the* way the object is [...] What I am to copy then, it seems, is one such aspect, one of the ways the object is or looks. (Goodman 1968: 6)

There is something suspicious with the very idea of copying, if copying an object can mean many different things. The copy theory of representation is undermined by its inability to specify what is to be copied (1968: 8). Moreover, there is no such a thing as an aseptic and innocent eye that can copy things objectively. How the eye sees and also what it sees is "regulated by need and prejudice. It selects, rejects, organizes, discriminates" (Goodman 1968: 7-8; see also Gombrich 1960). This first lesson does not only apply to substantive accounts of representation based on similarity, but also to accounts of the means of representation that, like the one I aim to develop throughout this thesis, resort to the notion of similarity to explicate characterize the production of a series of epistemically successful representations in science. In Chapter 5 I propose the *creative similarity account of the means of representation*, which explicitly rejects the association of the concepts of representation and similarity with an attempt to "copy" or "imitate" nature. The *creative similarity account* is much closer to the spirit of Goodman's account when he claims that representing an object is not copying but *achieving a construal* or interpretation (1968: 8).

The second important lesson that Goodman's (1968) work offers to the study of representation is the idea that representation always takes place in standardized systems of practices, which involve systems of symbols too. Goodman (1968) affirmed that "nothing is intrinsically a representation", insofar as the status of something as a representation is relative to a symbol system (1968: 226). The evaluation of how correct a representation is depends upon how it relates to other symbols in the system it belongs to and to how it conveys information according to the rules of the system. In turn, the level of standardization of a symbol system in a historical moment will affect how subjects read representations in the system (1968: 38). In this context Goodman emphasizes that "we must beware of supposing that similarity constitutes any firm, invariant criterion of realism; for similarity is relative, variable, culture-dependent" (1972: 438). Even if Goodman made this claim to question the objectivity of the notion of similarity, I believe we can take it as a positive instance to develop an account of representation. A good starting point for an account of the means of representation focused on similarity is the definition of similarity as variable, culture-dependent, and closely related to how norms, standards, and social and symbolic conventions are recognised within systems of practice. Although the specific account of the means of representation that I will propose is focused on similarity, or more specifically on "creative similarity", I will refer to the possibility of advancing various possible accounts of successful representation, focused for instance on convention, symbolization, or exemplification. These various accounts could complement and enrich each other regarding the general problem of how successful representations that afford understanding of the world are produced in science.

1.3.2 The argument from misrepresentation

The other main argument against substantive accounts of representation based on similarity concerns misrepresentation. Scientific models idealize, abstract, simplify, and generalize to some extent the target systems they aim to represent. For the sake of simplicity, I will refer to all these features of scientific models (idealization, abstraction, simplification, generalization) as different types of distortions, and use the term misrepresentation to describe how distorted models represent their target systems. The claim that all models misrepresent their target to some extent should not be at odds with the claim that they are representations of that target. In other words, there is "no reason for fearing that the merely approximate status of a model impugns its capacity to represent" (Callender and Cohen 2006: 79): a misrepresentation is not the same as a non-representation (Frigg and Nguyen 2016).

If this is correct, an account of representation must be able to cope with the possibility of models to misrepresent, as all (or at least many) representations are in practice misrepresentations. A theory that makes "the phenomenon of misrepresentation mysterious or impossible must be inadequate" (Frigg 2006: 51). Such a theory, I would add, must also be able to explain the valuable epistemic role that many distortions play in scientific models. That is, an account of representation should not only accommodate the fact that models represent *despite* distorting. It should also be able to elucidate why distortions, which are frequently intentional, systematic, purposeful, and not errors to be corrected, are important components of the epistemic success of models. I will mention issues concerning the epistemic value of distortions in Chapters 3 to 5. In the rest of this section, I focus on the more general challenge of accounting for the phenomenon of misrepresentation in substantive accounts of representation based on similarity or structural similarity (such as those of French 2003; French and Bueno 2011; Bartels 2006).

Isomorphism as a necessary condition for representation patently cannot accommodate the phenomenon of misrepresentation. Isomorphism requires a one-to-one matching of elements, properties, and relations between vehicle and target. If we include isomorphism as a condition for something to be a representation, the only possibility of obtaining a representation would be to obtain a perfectly accurate representation. But most (or all) representations are not perfectly accurate. Thus, an account of representation that includes isomorphism as a necessary condition will have to say that all cases of misrepresentation are cases of non-representation. No-one in the recent debate in philosophy of science seems to consider isomorphism the type of morphism that exists between models and target systems. The question is whether weaker versions (i.e. partial isomorphism, homomorphism) successfully respond to the argument from misrepresentation. French and Bueno (2011) have rejected the identification of their own proposal with isomorphism:

> [W]e are being tarred with the same brush as those who advocate isomorphisms despite our protests that our account is different in precisely this respect. With the introduction of partial isomorphism and homomorphism, no requirement is made that the structures that are used to represent other structures do so with perfect accuracy. (French and Bueno 2011: 888)

The concept of partial isomorphism is suggested as an alternative to isomorphism that could successfully address the problem of misrepresentation (see also French 2003; and French and Ladyman 1999). Partial isomorphism doesn't require the total matching of properties and relations between vehicle and target, but only some degree of morphism. The question that follows is: what degree of morphism is required in this account to obtain a representation (not an accurate representation)? In other words, how partial must the partial isomorphism be? French and Ladyman (1999), French (2003), or French and Bueno (2011) don't offer much guidance on how to respond to this questions. One could assume that we need a very partial isomorphism to consider something a representation. One could alternatively assume that we need a high degree of accuracy in the matching to say that there is partial isomorphism between a vehicle and a target and hence that the vehicle is a representation of a target. Or one could assume that the requirement is open, and that *any* partial isomorphism is enough to obtain a representation, no matter how partial it is. The response we give is important because the separation between non-representation and misrepresentation depends on it. The third response is not feasible, however. Anything could be isomorphic to anything else to a minimal degree, just as Goodman (1968) claimed that anything is similar to anything else in at least one respect (assuming that structural similarity is a form of similarity, being isomorphism the identity limit of similarity of structure). If any partial isomorphism is valid to establish representation, then the requirement of partial isomorphism is trivial, in the sense that anything would be at least minimally isomorphic to anything else. So there would be no way of distinguishing between a misrepresentation and a non-representation, all of them minimally isomorphic to a target in some way.

As pointed out earlier, French and Bueno (2011: 886) explicitly reject that intentions and other pragmatic issues could be built into the relation of representation. So appealing to the subjects and their intentions wouldn't be a way for them to define what a relevant partial isomorphism is to distinguish between a misrepresentation and a non-representation. What constitutes the relation of representation for them is the existing partial isomorphism between a vehicle and a target, which is already "there", in the two objects of the representation. This attitude is illustrated in the following example from French (2003):

> Consider the [...] case of the sea and wind carving the Lorentz transformations into the sand. Are we going to [...] insist that such markings do not represent relativistic phenomena of some sort because the relevant intention is absent? [...] we do not take the causal provenance of the markings themselves as having any bearing on the constitution of the theory as an object. The theory is "there" in the sand. (French 2003: 1473)

If the relation of partial isomorphism is "there" already, recognizable in vehicle and target, in the markings on the sand and the Lorentz transformations, then subjects and intentions do not help define what a relevant partial isomorphism is. To make sense of partial isomorphism in French's (2003) account, and to be able to claim that a representation of the Lorentz transformations is 'there' in the markings carved by the wind on the sand, we would expect the concept of partial isomorphism to demand a relatively high level of partiality of the morphism. That is, we would expect more than a minimal requirement of partial isomorphism. So the option has to be either the first (a low degree of partial isomorphism), the second (a high degree of accuracy in matching), or something else along the continuum. How do we decide where to we draw the line to distinguish between a non-representation and a misrepresentation?[9] The question is in itself conflating the problem of representation (*R1*) with the problem of accurate or successful representation (*R2*). The conflation of *R1* and *R2* does not benefit French's (2003) and French and Bueno's (2011) accounts, since accuracy comes in degrees while something is either a representation or it is not. They would need to account for

[9] Also, an equivalent question would apply to how we draw a line between misrepresentation and representation, but we can concede that this is a less problematic question.

the phenomenon of misrepresentation by drawing a line between the cases where the level of partial isomorphism is good enough to consider something a misrepresentation (or representation) of a target, and the cases where the level of partial isomorphism is too low to consider something a representation (or misrepresentation) of a target. The latter would be cases of non-representation. In the absence of that line, and given the rejection of subjects' intentions as constitutive of the relation of representation, it is not possible to claim that partial isomorphism is a good candidate for constituting representation.[10]

Substantive accounts of representation based on homomorphism (in Bartels's [2006] version) are more successful at facing the challenges from misrepresentation. Bartels distinguishes, as I already mentioned, between the *reference* and the *content* of a representation (2006: 13-14). This distinction, he claims, is key to accommodate misrepresentation. Homomorphism is a relation that occurs when the *content* of a vehicle entails a target, and *reference* is a relation established between vehicle and target via a representational mechanism previously established by a relation of homomorphism (2006: 13-14). In this way,

> The homomorphism theory does not have problems to allow for misrepresentation. If *B* represents *A*, then *B* *refers* to *A*. There is also a *content* of that representation which is not necessarily identical with its reference. *B* misrepresents *A* just in case *B* refers to *A* but the representational content does not entail *A*. Intuitively this means that *B* is about *A*, but does not match *A* in what it says about *A*. Problems with misrepresentation arise because some theories of representation do not have the resources to identify reference and content independently. (Bartels 2006: 13)

A case of misrepresentation is in Bartels's (2006) view simply a case in which a vehicle refers to a target, without homomorphism involved. Reference is the only necessary condition for misrepresentation. A case of representation, however, is one in which a vehicle refers to a target and the content of that vehicle entails the target. Bartels's (2006) strategy gives us more resources than accounts based on isomorphism or partial isomorphism to account for cases of scientific models that distort their targets. But it is still not successful though. I mentioned earlier that

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[10] More elaborate arguments against partial isomorphism, but compatible with that just presented, have been offered by Suárez (2003), Contessa (2007a), and Frigg (2006). A response to their critics regarding the problem of misrepresentation can be found in French and Bueno (2011: 889).

in different degrees, respects, and modes, all representations are misrepresentations. Even our most correct examples of scientific models entail a narrow selection of features of the target systems. In some cases, misrepresentation also implies significant inaccuracy of (that narrow selection of) features; in others, misrepresentation implies inclusion of undesired errors. There can be different types of distortions involved too. For Suárez and Pero (2016: 3), *abstraction* would be the type of distortion that involves neglecting some of the features of the target system; *pretence* is a distortion that involves ascribing to the target system features it does not possess; and *simulation* involves both abstraction and pretence. Bartels (2006) characterizes cases of misrepresentation as those where the only necessary condition is that a vehicle refers to the target, while in the rest of cases, which are cases of representation, the vehicle needs to both refer to and be homomorphic to the target (2006: 13). But if it is difficult to point to representations that do not distort their targets in one way or another, then Bartels's account would be saying that all cases of representation would only require reference (and not homomorphism) as a necessary condition. This is clearly not what Bartels is attempting to argue. Otherwise, what role would homomorphism be playing in his account?

Without making the strong claim that all representations are cases of misrepresentation, as I did above, Suárez and Pero (2016) offer an equivalent argument to Bartels's (2006) way of addressing the problem of misrepresentation:

> The issue here is [...] whether homomorphism is doing much work in this account, as it seems that the morphism is not a necessary condition for representation in cases of misrepresentation. *Reference* only is what is establishing the misrepresentation relation. (Suárez and Pero 2016: 17)

The best way to make sense of Bartels's (2006) argument is to assume that with misrepresentation he is thinking about cases of flagrant erroneous representations, and not what I assumed above when I claimed that all models are misrepresentations in one way or another. In other words, Bartels (2006) seems to be treating misrepresentation as exceptional cases among the majority of scientific representations. It is implicit in the way he presents his argument that significantly erroneous models would be described as misrepresentations and not non-representations, while other cases of relatively accurate models would be cases of representation (and not misrepresentation or non-representation).[11] What distinguishes a misrepresentation from a non-representation is that the former refers to a target, while the latter does not. Homomorphism is not involved in misrepresentation in Bartels's (2006) account, which is a somewhat unsatisfactory way of dealing with the problem of misrepresentation from the perspective of a homomorphism account of representation. To use Suárez and Pero's (2016) words: "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". In conclusion, following Bartels (2006), we can keep either homomorphism or misrepresentation in the very same account, but not both at the same time.

A broader note on how misrepresentation is understood in accounts of representation might be worth considering. Almost all philosophers of science seem to agree that a theory of representation must accommodate (if not explain) misrepresentation. However, the specific reasons for why this should be the case are not completely the same across different accounts. Is it because representing is in practice a synonym for misrepresenting? Is it because some models occasionally contain errors and shortcomings that we should account for? The first option has stronger epistemological implications than the second (and includes the second). We have seen that, in the case of Bartels's (2006) account, the adoption of one characterization of misrepresentation or another has consequences for the strength on the overall argument. Bartels's (2006) homomorphism account is more robust if we adopt a characterization of misrepresentation as models containing occasional mistakes, than if we adopt the conception that all models in practice misrepresent.

Another example in the literature where different characterizations of misrepresentation are at stake is found in Frigg (2006; 2010). In Frigg (2010), misrepresentation seems to be an issue that mainly concerns occasional (even unintentional) errors in representations. He gives the example of cartographers constructing a map and failing to correctly connect some of the dots and lines as a customary example of misrepresentation (2010: 129-130). Meanwhile, in Frigg (2006), the idea of misrepresentation has much broader implications. He claims: "misrepresentation is common in science. Some cases of misrepresentation are

[11] Suárez and Pero (2016) have analysed Bartels's (2006) proposal in detail to finally conclude that he is unsuccessful at accommodating misrepresentation, even if understood in this way. They claim that Bartels (2006) can cope with some forms of misrepresentation like *mistargeting* and *pretence*, but not with others like *abstraction*. The full argument can be found in Suárez and Pero (2016: 14-17), and earlier versions of it in Suárez (2003: 240).

plain mistakes (e.g. ether models). But not all misrepresentations involve error. Many models are based on idealising assumptions of which we know that they are false" (2006: 51). In this case, misrepresentation is understood as widespread in science, not necessarily as the consequence of errors or limitations, but also as facilitating the construction of more explanatory models (through the exploitation of idealizations, generalizations, abstractions). My view is closer to the idea that all scientific models both involuntarily and purposely misrepresent, as will become clearer in Chapters 3 to 5 when discussing actual practices of representing. For the particular goals pursued in representational practice, idealizing, generalizing, and including other forms of distortion can be important resources to improve the fruitfulness of a scientific model.

Analysis of some of the most well-known attempts to offer a substantive account of representation based on similarity, French's (2003) and French and Bueno's (2011) partial isomorphism accounts, and Bartels's (2006) homomorphism account, has shown that they are not successful at accommodating the phenomenon of misrepresentation in a satisfactory way. The proposals I have discussed specifically adopt structural conceptions of similarity (partial isomorphism, homomorphism), which I have treated as particular cases of similarity-based accounts. It remains to be seen if an alternative account, based on a non-structural understanding of similarity, would be more successful in the endeavour of addressing the argument from misrepresentation. Giere's (2004, 2008, 2010) account has been commonly treated as an example of this type of proposal, as he adopts an intuitive understanding of similarity, closer to how we use the concept of similarity in everyday language. However, as I pointed out at the beginning, Giere (2004, 2008, 2010) is not really proposing a substantive account of representation based on similarity. A closer look at his strategy reveals that the role of similarity has shifted in his account, from responding to *R1* to responding to *R2* (Frigg and Nguyen 2016). In the process of facing the challenge of misrepresentation, Giere (2010) endorses an intentional account of representation, where similarities are defined by the epistemic agents of the practice. Thus, I will discuss Giere's (2004, 2008, 2010) work in more detail in Chapter 2.

From the two arguments presented in this section, Goodman's (1968; 1972) logical argument against similarity, and the argument from misrepresentation, we should conclude that substantive accounts of representation should not be based on similarity. I remain neutral about what other elements or features would be good candidates to support a substantive account of representation, or whether it would be adequate to adopt a deflationary view on scientific representation. The main point to emphasize is that rejecting similarity as constituent of representation doesn't automatically mean that we should disregard the possible role of similarity in scientific representations. In the next chapter I will argue that the role of similarity can be satisfactorily incorporated into an account of the means of representation.

1. 4. Conclusions

In this first chapter, I distinguished between two questions contained in the so-called problem of scientific representation: the question about the *constituents* and the question about the *means* of representation (following Suárez 2003; 2010; van Fraassen 2008). The question about the constituents can be formulated as "in virtue of what is something, a vehicle, a scientific representation of something else, a target?", and it demands a set of necessary and sufficient conditions. Meanwhile, the question about the means can be formulated as "in virtue of what is something, a vehicle, an accurate or successful scientific representation of something else, a target?" and, following accounts like those of Suárez (2010) and Frigg (2006), it demands a more practical type of enquiry into the problem of representation. Without the distinction between the two questions, it would be difficult to make sense of the claim that something can be an "inaccurate" or "unsuccessful" representation.

Then, I developed a specific examination of the question concerning the constituents of representation, and the possibility of considering similarity a constituent of the relation of representation. My own view was located in opposition to substantive accounts of representation based on similarity, such as those of French (2006), French and Bueno (2011), and Bartels (2006), who specifically sustain structural version of similarity. The arguments presented to show why it is ultimately problematic to consider similarity a constituent of representation were Goodman's (1968, 1972) logical argument against similarity and the argument from misrepresentation. In Chapter 2, I will start building the premises to defend the idea that similarity can be, on the contrary, satisfactorily incorporated into an account of the means of representation.

Chapter 2

Similarity as Means of Representation

In Chapter 1, I presented some common criticisms of the idea of similarity as an answer to the question of what constitutes representation. I also suggested that these criticisms are not a reason to abandon similarity altogether. In this chapter, I explore some positions that have been instrumental in rehabilitating the role of similarity in connection to the problem of the epistemic success of representation (Suárez 2003, 2010; Contessa 2007a, 2011; Giere 2004, 2006; van Fraassen 2008). The starting point of these positions is the intuition that, despite not exhausting the relation of representation, similarity still plays a role in a broad range of modelling practices. Many philosophers and non-philosophers alike would probably agree that an orrery representing the solar system is similar in some relevant respects to the actual solar system; that a computer simulation of the development of a tornado is similar in a pertinent way to the advancement of a real tornado; that model organisms like NOD mice (non-obese diabetic mice) are relevantly similar to humans with respect to how type 1 diabetes develops; or that a hydraulic model of a river shares important similarities with the river represented (Maki 2009; Parker 2009; Pincock 2012; Ankeny 2001; Weisberg 2013). I believe that we should not disregard these intuitions, usually originating in the observation of practitioners' work with models, but consider them seriously when tackling the arguments against the involvement of similarity in the construction of successful scientific representation.

It might be the case that some philosophers of science claim that they don't actually have the intuition that relevant similarities are involved in an orrery representing the solar system, a computer simulation representing a tornado, or a model organism representing a human illness. There is not much that could convince them otherwise. Perhaps closer acquaintance with highly abstract or mathematical models prevents them from recognizing (literally or in a stronger epistemological sense) the visual, functional, and dynamic similarities that other philosophers patently see in many scientific representations. I will not focus on practices of representing that involve purely mathematical models in this thesis, but even in those cases, there are accounts that have defended the idea that the epistemic success of highly abstract and mathematical models can also be characterized appealing to the role of structural similarities (van Fraassen 2008; Contessa 2011; Pincock 2012). The methodology proposed in Chapter 3 and illustrated with a case study in Chapter 4 could be also extended to the analysis of cases where mathematical models are the object of study.

The position that Frigg and Nguyen (2017a) maintain, for instance, of rebutting the role of similarity in representation, is not meant to apply exclusively to cases of highly abstract or mathematical models. They also attempt to describe types of representation traditionally characterized as appealing to the role of similarity (such as maps or analogical models) in a way that principally invokes conventions and avoids references to similarity, structural or otherwise. I take accounts like this to be insightful to the extent that they help advance a better understanding of how systems of symbols participate in scientific practices, in the tradition of Goodman's (1968) work. However, avoiding references to similarity at all costs can be highly counterintuitive to explain how representations like maps or analogical models are in practice constructed and how they afford understanding of the targets represented. In Chapter 3 I argue that in numerous practices of representing, judgments of similarity indeed play a role in the construction of fruitful scientific models. Then, in Chapters 5, I defend that if similarity is adequately characterized to fit those actual uses of judgments of similarity in practice, it is then a useful notion to explicate the epistemic success of a relevant set of representational practices in science. If this argument is correct, I will be offering good reasons to resist the general rebuttal of the role of similarity in representation, and defining specific limits to characterize its exact role in modelling practices.

The previous chapter concluded with the claim that similarity should not be considered a constituent of the relation of representation. This conclusion didn't entail that similarity is irrelevant for representation, however. Even Goodman (1968; 1972) recognizes that similarity "has its place and its uses, but is more often found where it does not belong, professing powers it does not possess" (1972: 437). One place where "it does not belong" seems to be a substantive account of representation, but one of the "places and uses" where the value of similarity can be rehabilitated is as means of representation. I have claimed that the "means" concern the epistemic success or accuracy of scientific representations. Now a more precise characterization of the means of representation is required. In its original definition, Suárez (2003; 2010; 2015) defines the means as follows:

> *Means*: For any source–target pair (S, T) at a given time and in a given context: R' is the means of the representation of T by S if some user of the model employs R' (at that time and in that context) to draw inferences about T from S (Suárez 2010: 93).

In this definition of means, there is no mention of the accuracy or epistemic success of representation, but this is implied. The means are defined as the context-dependent relation that, at a given time, is actively employed by the users or epistemic agents of the representational practice to draw inferences about a target (ibid.: 93). The difference with the constituents is clear, as the constituents are unique and universal relations between two objects, a vehicle and a target, while the means are the relations *actively employed* in particular circumstances. The problem of the means concerns the accuracy or epistemic success of representation, because practices of representing are practices of trying to construct epistemically successful models with which to perform epistemically successful inferences. Possible means of representation are, following Suárez (2003: 229), similarity, isomorphism, exemplification, and convention. We need a practical type of enquiry to study the means of representation, since it is in particular practices that we can observe how agents actively employ relations between vehicles and targets to make inferences about the latter (ibid.). Moreover, a scientific model obtained at the end of a practice might not manifest its means of representation conspicuously. A careful study of representational practices is necessary to discern, and in some cases unravel, what the means of representation are and how they function. As Suárez (2003: 229) points out, "the means of representation are not exactly transparent: no source wears its means of representation 'on its sleeve'. In many cases the actual means of a representation may be opaque to the uninitiated". In Chapter 5, I will suggest that the definition of means, as originally proposed by Suárez (2003), should be expanded further: in order to identify the relations that agents actively employ to make inferences about a target, we have to pay attention to the *resources* used throughout the practice of representing that will define what particular relations are employed at the end of the practice to make inferences about that target.

The project of attempting to develop an account of the means of representation is probably messier and slipperier than characterizing the constituents of representation, precisely because studying the means requires thorough attention to actual practices where those means are employed. And practices are inherently intricate, complex, difficult to grasp from a single viewpoint. I take this to be an important challenge of the project of advancing specific accounts of the means of representation, including one focused on similarity like the one I attempt to advance. Nonetheless, this does not make the attempt worthless. Although messier, an account of the means of representation could be more insightful epistemologically than an account of the constituents of representation. There is a sense in which the conditions for representation "come cheap", as it doesn't take much for someone to adopt an interpretation of a vehicle in terms of the target (Contessa 2007a: 127).[12] What does not "come cheap" and deserves a well-developed enquiry is the problem of how accurate or epistemically successful models can be obtained (ibid.). Philosophers of science like Frigg (2006) and French (2003) would most probably disagree with this claim, as they engage with the task of identifying what the constituents of representation are. But others, especially proponents of deflationary views of representation, would certainly agree. For deflationary views, the main work that needs to be done with regards to the problem of representation is the characterization of the means that render representations epistemically successful. Chapters 3 and 4 will attempt to develop a practical type of enquiry on the role of similarity in representation. I specifically suggest that such an enquiry should be framed within the projects of PSP (Philosophy of Science in Practice) or iHPS (integrated History and Philosophy of Science). From there, I will extract some conclusions to give support to a particular account of the means of representation in Chapter 5.

The kind of general claim that an account of the means of representation focused on similarity would want to make could have the following forms: "the similarity between a vehicle and a target is what distinguishes a representation from a faithful representation" (this is approximately what Contessa 2007a argues);

[12] Interpretation of a vehicle in terms of a target, together with denotation, are for Contessa the necessary conditions for representation (2007a: 127).

or "the most important way scientists use models to successfully represent aspects of the world is by exploiting similarities between them" (approximately what Giere 2004 argues); or "similarity is the relation actively employed by epistemic agents to make inferences about a target from a vehicle" (this is Suárez's formulation in 2003, 2010).[13] In Chapter 5 I will discuss in detail what type of claim the account that I propose, the *creative similarity account of the means of representation*, specifically makes. But, to advance some important ideas, the *creative similarity account* seeks to have a restricted scope: that is, it neither makes a universal claim about similarity being the means of representation in all cases of successful representation, nor does it make a very limited claim about similarity being the means of representation in one or two specific cases of representation. It commits to some level of generality, as it tries to offer insight about various representational practices, and proposes to explore further practices using the *creative similarity account* as a framework. But it also recognizes that there might be a plurality of means of representation that allow us to construct successful representations in different modelling practices.

In any case, affirming that the notion of similarity can help address the problem of the epistemic success of representation has generated opposed responses in the debate in philosophy of science. The challenges that an account of successful representation focused on similarity faces are different from the challenges that a substantive account of representation based on similarity faces. In Section 2.1 I discuss the three main opposing responses to the idea that similarity is the means of representation: the argument from variety, the argument from vagueness, and the argument from misrepresentation against the means (that is, a second version of the argument from misrepresentation presented in Chapter 1). I conclude that none of these arguments is decisive enough to reject the possibility of advancing an account of the means of representation focused on similarity, provided that the notion of similarity is adequately characterized. Then, in Section 2.2, I explore the most constructive attempts in philosophy of science to discuss similarity in the context of advancing an explanation of how epistemically successful representations are produced. I describe the specific contributions of Contessa (2007a, 2011), Giere (2004, 2006), and van Fraassen (2008), and point out which elements from their accounts I incorporate into mine, presented in Chapter 5.

[13] In fact Suárez's (2003, 2010) claim is much weaker than this. He doesn't argue that similarity is *the* relation actively employed in general between vehicles and targets. He considers similarity one among other possible means of representation, as will become clearer in Section 2.1.

2.1. Three strictures on similarity as means of representation

The three main criticisms of the project of developing an account of the means of representation focused on similarity are the arguments from variety, vagueness, and misrepresentation against the means. I discuss and respond to them in turn. To clarify, the term "means" is proposed and exclusively used by Suárez (2003; 2010; 2015) in the debate of scientific representation.[14] However, for the sake of simplicity, I refer to all the accounts that have addressed versions of the question *R2* of representation, as defined in Chapter 1, as concerning the means of representation (i.e. accounts focused on the problem of accurate representation, faithful representation, epistemically successful representation, or the problem of the styles of representation). Later on, I include some clarifications about the different implications that these versions of the *R2* question have.

2.1.1 The argument from variety

The argument from variety against similarity as the means of representation has been formulated, among others, by Frigg (2006). He contends that similarity is not a good candidate to be the means of representation – or to respond to the *problem of style* in his version– because there is a huge variety of styles of scientific representation, and similarity is at best one among these (2006: 50). The only way we can approach the problem of style, Frigg adds, would be by offering a taxonomy of all the different styles used in scientific representation (ibid.).[15] I certainly agree that the practice of representing is complex and encompasses a variety of resources and techniques. However, the use of the term "style" in this context is rather ambiguous, and, I would like to claim, it doesn't give strength to the argument from variety against similarity. At some point, Frigg (2006: 5) affirms that mathematical models, physical models, and graphs are examples of different styles

[14] Later, I will specify that van Fraassen (2008) also uses the term "means" occasionally, but with slightly different implications.

[15] Earlier I claimed that Frigg (2006) also proposes a second variant of a response to *R2* or the problem of style, namely, a normative variant that establishes how scientific representations *must* be constructed to be successful. Since this second variant is not affected by the argument from variety as Frigg (2006) presents it, I focus on the descriptive variant here.

in which representations are constructed. At another point, he seems to endorse a slightly different meaning of the term style, and affirms that apart from similarity, isomorphism might be a style of representation, partial isomorphism another, embedding another, and he adds that there are "many other possibilities" (2006: 59-60). But there is no mention of what the many other possibilities are, and those he mentions seem very closely related to the general idea of similarity. So, so far, there is not enough reason to believe that the argument from variety seriously questions similarity as means of representation. Frigg then (2006: 50) gives concrete examples of scientific models constructed in different styles, and here the term style seems to refer to the very specific way in which individual models are uniquely constructed:

> An ink drawing, a wood cut, a pointillist painting, or a geometrical abstraction can represent the same scene in very different ways. This pluralism is not a prerogative of the fine arts. The representations used in the sciences are not all of the same kind either. Bill Phillips' hydraulic machine and Hicks' mathematical models both represent a Keynesian economy but they use very different devices to do so; and Weizsäcker's liquid drop model represents the nucleus of an atom in a manner that is very different from the one in the shell model. As in painting, there seems to be a variety of representational styles in science (Frigg 2006: 50).

From this quote, one style would be the use of a hydraulic machine to represent an economy; another style would be the use of a mathematical model to represent an economy; and another would be the use of an analogical model of a liquid drop. It is difficult to tell if similarity would be a style used in some of these cases (for instance, in the analogical model of the liquid drop), or if each of these cases entails a unique style. The comparison with the pictorial arts does not help much, since Frigg (2006) combines references to material techniques used in the visual arts, such as ink or wood cut, with what in the history of art is commonly considered a pictorial style, such as pointillism or abstraction. In a more recent work, the list of possible styles Frigg and Nguyen (2016) suggest gets even larger:

> An X-ray photograph represents an ankle joint in a different way than a biomechanical model, a mercury thermometer represents the temperature of gas in a different way than statistical mechanics does, and chemical theory represents a C60 fullerene in different way that an electron-microscope image of the molecule. Even when restricting attention to the same kind of representation, there are important differences: [...] an electric circuit model represents the brain function in a different way than a neural network

> model. In brief, there seem to be different representational styles. [...] There is no expectation that a complete list of styles be provided in response. Indeed, it is unlikely that such a list can ever be drawn up, and new styles will be invented as science progresses (Frigg and Nguyen 2016).

It is clear here that the concept of style refers to an unending list of particular possibilities when constructing models. If this is the case, the problem of style is ungraspable in its descriptive variant, always opened to further extension. In this context it is certainly problematic to consider similarity a solution to the problem of style, given the endless variety of styles. I believe, however, that even if similarity might not offer a solution to every possible case of successful representation, past and future, the argument from variety is not as fatal as Frigg (2006) and Frigg and Nguyen (2016) present it. In history of art, the term style is useful precisely because it groups individual instances into relatively unified clusters. Particularities of specific artworks fit within categories defined by historical periods, techniques, skills, and consolidated systems of practices. If one wanted to keep the discussion about styles of representing, the point would be to provide classifications that allowed philosophers to make comparisons and generalizations about the means commonly used in practice, as well as to define standards of successful ways of constructing models. I would like to argue that the concept of similarity, if adequately characterized, can offer that kind of generic insight about how a wide range of scientific practices are constructed and achieve successful results.

Along these lines, a narrower way of formulating the argument from variety is found in Suárez (2003: 231), who originally gave it that name. Suárez (2003) recognizes similarity and isomorphism as the two most common means of representation, but not the only ones (ibid.). Other means, "such as exemplification, instantiation, convention, truth" are also possibilities (ibid.: 229). In contrast to Frigg (2006), this is not a postulation of an endless variety of individual means or styles of representation. It is a more concise list that in principle would allow us to make generalizations and comparisons between common ways in which epistemic agents employ relations between models and target to make successful inferences. As I advanced, my aim in this thesis is not to argue that similarity is the only means of representation: in fact, what I will defend is that *creative similarity* is the means of representation in a relevant set of epistemically successful representations, not similarity in general. There might be a plurality of other means of representation, like the ones Suárez (2003) refers to, provided that alternative accounts of the means of representation can explain how those means enable the construction of successful representations. The idea I would like to defend is that, even in a situation where we have proposed and accepted the validity of various accounts of the means of representation, the argument from variety is not fatal. Each of those accounts can make insightful generalizations about recurrent ways of producing fruitful, adequate, useful scientific representations, maybe concerning specific disciplines, specific contexts, or throughout a diversity of modelling practices.

Suárez (2003) presents the argument from variety in more limited, manageable terms than Frigg (2006) and Frigg and Nguyen (2016), as for him it is not about an endless list of possibilities but a few common means that frequently appear in scientific practices. Still, Suárez (2003) advances the argument to criticize attempts to provide an account of the means of representation focused on similarity, probably assuming that advancing such an account would involve making universal claims about similarity. But if my reasons are justified, and we accept the value of providing insightful generalizations about the problem of successful representation, then trying to advance a similarity-based account of the means of representation that doesn't have a universal character or the form of necessary conditions is well motivated. The argument from variety has been advanced to obstruct the debate about similarity as responsible for the success of (a series of) scientific representations. However, it can be taken as motivation to continue investigating the concrete functions and *limits* of the role of similarity as means of representation.

Let us briefly discuss the other possible means of representation that Suárez (2003) mentions, and how they could in principle be complementary to each other. Although I will be focusing on similarity in the rest of the thesis, we should encourage the development of various other accounts of the means of representation, with the aim of enriching the general response to the problem of how epistemically successful representations are produced in science.

Concerning isomorphism, one of the alternative means of representation that Suárez (2003) refers to, we could claim that it is possible to integrate it into a similarity-based type of account of the means of representation, since isomorphism can be considered a structural type of similarity. Suárez himself recognizes that "it is possible in general to understand isomorphism as a form of similarity" (ibid.: 228). If A and B are isomorphic, then they are similar with respect to at least one property: their relational framework. Perhaps an advantage of taking similarity as a better option than isomorphism to develop a more insightful, broader, more encompassing, account of the means of representation is that similarity can apply to any sort of objects, while isomorphism (or homomorphism, partial isomorphism) only to objects that openly exhibit a mathematical structure. Being structurally similar is a way of being similar, while being similar cannot be reduced to being isomorphic. An account focused on isomorphism as means of representation would have to either be restricted to discussing the epistemic success of representations that concern mathematical objects or deal with the problem of explaining how non-mathematical objects can instantiate structures (see Frigg 2006 and Contessa 2007a for discussion of this point). Meanwhile, an account focused on similarity would be able to encompass the description of various ways in which similarities, including structural, are employed in modelling practices to produce successful results. Ideally, the contribution that this type of account should make is the identification of common dynamics generated by similarity as means of representation in different practices, whether it is defined as structural similarity or in other possible ways. In Chapter 3 I will argue that in order to identify those dynamics it is necessary to look at scientists' formulation of judgments of similarity (including judgments of structural similarity), and how they crystalize into community standards that allow them to construct successful representations.

Exemplification and instantiation are suggested by Suárez (2003) as other plausible means of representation. Exploring these means in specific accounts, complementing a similarity-based type account, seems to be beneficial when attempting to address the problem of the epistemic success of representation in a more comprehensive way. Instantiation is exhibition of the properties that an object possesses, so objects usually instantiate multiple properties. Exemplification requires instantiation, but also reference and interpretation (see Goodman 1968; and Elgin 2011).[16] It is not clear, though, unless further clarification is provided (and Suárez (2003) doesn't offer it) how instantiation could sustain an account of the means of representation. It is difficult to see how the mere exhibition of properties of an object can allow epistemic agents to construct successful representations and make inferences about a target with them. Exemplification seems a much more successful candidate: it consists in making manifest certain properties of an object that, via interpretation, epistemic agents relate to properties of a target. For instance, a swatch of herringbone tweed exemplifies herringbone tweed insofar as it instantiates herringbone tweed, refers to it, and relevant agents

[16] This is more specifically the terminology Elgin (2011) uses. Goodman (1968: 53-55) affirms that "exemplification is possession plus reference".

interpret what properties, among the many that the swatch instantiates (i.e. being square, small, frayed around the edges, having certain colours and patterns), are to be exemplified (Elgin 2011: 400). It is easier to see how exemplification could be employed by epistemic agents to produce adequate scientific representations that allow inferences about a target to be made.

A derived question that would be worth exploring is to what extent an account of the means of representation focused on similarity intersects with an account focused on exemplification. In some ways, similarity is closely connected to exemplification. Van Fraassen (2008: 17) has for instance argued that exemplification is the covert way in which Goodman (1968) inserts similarity or resemblance into the debate of representation: "One way in which Goodman did bring in resemblance was through the intricate notion of exemplification. [...] Obviously the use [of colour swatches] is to represent to you what your wall or floor will look like if you choose the corresponding paint or carpet". It is difficult not to see the participation of similarity in exemplification when we look at a swatch that exemplifies the colour of my proposed carpet. Certainly exemplification involves more than similarity. Relevant similarities must be selected and highlighted for an exemplification to be functioning (ibid.). So, especially if our conception of similarity as means of representation requires that similarity is taken as selective and highlighted by the relevant epistemic agents depending on the goals of the practice, then the connections between similarity and exemplification become stronger. This is the case for the account that I will advance in Chapter 5. There I characterize similarity as *creative similarity* in order to develop an account of the means of representation, where *creative similarity* is a notion that involves that similarity is selected, highlighted, and combined with relevant distortions for the purposes of the epistemic community. In any case, I do not try to argue that similarity should be taken as equivalent to exemplification. The development of separate accounts that analyse the functioning of these particular means must be stimulated. An interesting point would be to identify possible intersections of similarity and exemplification in practices of representing, and maybe offer some conclusions about the coexistence (or incompatibility) of different means in the same practices of representing. The source to sustain any of these accounts needs to be the empirical, historical study of practices of representing, where judgments of exemplification or similarity (among other possible judgments) are used by epistemic agents to produce successful representations.

Concerning convention, which Suárez (2003: 229) proposes as another possible means of representation, it would certainly be productive to explore the possibility of advancing an account of the means of representation focused on it. There are definitely conventional and symbolic elements involved in numerous representations, scientific or not. The London Underground Map, for instance, represents the transport system in London, and it includes among other things a blue line that conventionally stands for the Victoria Line, a red line that stands for the Central Line, and a black line that stands for the Northern Line (see Contessa 2007a; Bolinska 2016; and Nguyen 2016 for different readings of the use of conventions in the case of the London Underground Map). The San Francisco Bay Model, which I discuss extensively in Chapter 4, also contains elements that involve symbolic associations, like signs that linguistically associate points along the water tanks of the model with locations on the actual Bay. Yet, the possibility of employing these conventions in the construction of successful representations shouldn't be analysed independently from how the London Underground Map or the San Francisco Bay Model succeed as a whole in affording understanding of their targets. That is to say, the complete set of intersecting lines on the underground map is selectively, relevantly, manifestly similar to the London transport system with respect to the distribution of stations on the ground, and this fact played a fundamental role in the success of the map as a useful representation for its users. As I will show in Chapter 4, the San Francisco Bay Model was successfully constructed and calibrated thanks to scientists' formulation of various judgments of similarity throughout modelling practices. Perhaps in these cases we could say that convention works as a kind of expedient to facilitate the highlighting, constraining and recognition of relevant similarities in particular practice of representing. Studying the roles of conventions could therefore enrich similarity-based accounts of the means of representations like the one I attempt to advance. Still, there might be cases of representational practices where we should rather identify convention as the means of representation, instead of as a complementary element or expedient. For those cases, developing a separate accounts of the means of representation focused on conventions and symbolizations will be particularly helpful.

In reference to truth, the last alternative means of representation Suárez (2003) suggests, it is difficult to see how it can work as means of representation, strictly speaking. Can truth be actively employed by epistemic agents to make inferences about a target from a scientific model? The best way to make sense of this idea is to think that truth is understood as 'identity' here. It is indeed possible that a vehicle of representation have some identical features to a target. Then, agents might be able to employ that relation of identity to make inferences about that target from the vehicle. In Chapter 4 I will describe the case of the San Francisco Bay Model, where there are in fact some identical features in the model with respect to the target, such as equal salinity levels. Although the relation of identity in this case can in principle be employed to perform inferences about the (salinity levels of the) target, I will argue that since the model works as a whole, which is far from identical to the target, identical salinity levels become a distortive element for the model, in a way that makes it difficult to make successful inferences about the San Francisco Bay employing that identity relation. It is better in this case to claim that relevant similarity is the means of representation, which allowed epistemic agents to design, construct, and calibrate the model as a whole, in comparison to the target represented. Identity would be in this case a limiting case of similarity that appears in very particular features of the model with respect to the target, but that would demand the interaction with the rest of (non-identical) features of the model to allow us to perform successful inferences. In short, unless more clarification is provided, it is hard to see how truth can be the means of representation in modelling practices. And if truth is understood as identity, we could either develop an account of the means of representation focused on identity, or probably more satisfactorily, take it as a limiting case of similarity and integrate it into a similarity-based account of the means of representation.

The different means of representation that Suárez (2003) suggests, in addition to similarity, deserve careful study in the debate of representation in philosophy of science, probably with special attention to exemplification and convention. There is more to be said about how similarity could, for instance, be aided by conventions to perform an effective role as means of representation; or about how similarity works in the specific case of representation by exemplification. The argument from variety, in Suárez's (2003) version, is less critical than it appeared at the beginning. It does not offer strong reasons to prevent us from attempting to offer an account of the means of representation focused on similarity that might characterize numerous cases of representational practices. The argument from variety in Frigg's (2006) and Frigg and Nguyen's (2016) versions is a stronger attempt to reject not only the possibility of offering an account of the means of representation focused *solely* on similarity, but any effort to make generalizations about the role of similarity in modelling practices alluding to the singularities of individual practices. This version of the argument can be invalidated as soon as the development of a practical enquiry shows that similarity is commonly employed in the design, construction, and evaluation of models that we recognize for their epistemic achievements.

2.1.2. The argument from vagueness

In close connection to the argument from variety, the argument from vagueness points out that the concept of similarity is so poorly defined that it cannot characterize by itself an account of the means of representation. The problem here is not with the variety of scientific representations there are, but with the variety of meanings or connotations that the term similarity has. Chakravartty (2001: 336-7) has advanced this type of criticism when he claims that "similarity is by itself hopelessly vague [...] even when cashed out in terms of 'respects' and 'degrees'", which is the characterization of similarity that Giere (2004, 2005) offers. Frigg (2006) has also criticized the notion of similarity for its vagueness:

> An unqualified similarity claim is empty; relevant respects and degrees need to be specified to make a similarity claim meaningful. So what we need is an account of scientifically relevant kinds of similarity, the contexts in which they are used, and the cognitive claims they support. Before we have specifications of that sort at hand, we have not satisfactorily solved the problem of style. (Frigg 2006: 61)

The criticism in this quote concerns the need to specify respects, degrees and contexts to render the concept of similarity meaningful. Moreover, Frigg (2006) would expect those respects and degrees to be "scientifically relevant". What results from here, following Frigg, is not that searching for scientifically relevant applications of similarity is a worthy endeavour, but exactly the opposite. Since it is unfeasible to attain such standards or scientifically relevant specifications of similarity, Frigg (2006) argues, we should abandon the study of the problem of the means or styles of representation by reference to similarity. But would it be possible in this view to defend the value of similarity in cases where respects and degrees are precisely specified? Not really. Frigg and Nguyen (2016) have argued that even if we identify the relevant respects and degrees of similarity in particular situations, the notion of similarity is useless because what matters then are precisely those very specific respects in which similarity is playing a role. In other words, the concept of similarity is empty, since as soon as we say that a model and a target are similar to each other with respect to a particular property, behaviour, relation, causal structure, what matters is that particular respect, and not the idea of similarity anymore.

As advanced in Chapter 1, Goodman (1968) had also presented a version of the argument from vagueness. He claimed that, since anything is similar to anything else in some respect, important respects of similarity need to be identified to make sense of the concept. But, he follows, "*importance* is a highly volatile matter, varying with every shift of context and interest, and quite incapable of supporting the fixed distinctions that philosophers so often seek to rest upon it" (Goodman 1972: 443). The difficulty of defining the importance of certain similarities with precision appears both as an unavoidable and as a fatal fact here. However, the fact that similarity cannot offer the kind of "fixed distinctions that philosophers seek", I would like to argue, might be unavoidable, but doesn't need to be fatal. Studying the uses of similarity in practices of representing in science can provide useful generalizations about how similarity is frequently understood in practice, and how it regularly varies "with every shift of context and interest", to use Goodman's words (ibid.). The lack of a permanent, universal definition of similarity doesn't mean that some uses of similarities are not more entrenched than others. Those that are more entrenched are more prone to be employed in practices of representing, and eventually will be considered more 'important' or 'relevant' types of similarity.[17] In Chapter 3 I develop a discussion on judgments of similarity, the concrete manifestations of the use of similarity in practice of representing. The entrenchment of certain similarity judgments depends on the past frequency of their use, the usefulness that those judgments prove to have, and the fact that they do not conflict with other entrenched judgments (following Goodman 1983: 97). I then identify in Chapter 4 a particular type of judgment of similarity, which I call "standardized judgments of similarity" that exemplifies this point well. Standardized judgments of similarity are judgments that have evolved into the consolidation of norms and standards given their demonstrated efficacy to construct epistemically successful representations.

[17] Precisely Goodman (1972, 1983) offers a type of solution (or comfort) to the problem of induction based on the idea of *entrenchment*. Although it is not possible to know how exactly things in the future will be like the past, we refer to the future as similar to the past with respect to certain entrenched predicates. A predicate is better entrenched than another if we have projected more hypotheses with it and if it proves to be more useful (1983: 59-83).

A different type of counterargument can be offered to respond to the argument from vagueness. The goal of obtaining a "scientifically relevant" definition of similarity, as Frigg (2006) demands, or "fixed distinctions" between important and unimportant similarities, as Goodman (1968) demands, might not be that desirable after all, so we can abandon it. Giere (2004; 2005) and Teller (2001) have offered this type of reply to the argument from vagueness. Instead of attempting to articulate what exactly important or scientifically relevant similarity is, Giere (2004) has tried to convince philosophers of science that representing does not require the existence of an objective measure of similarity. Moreover, the lack of such a measure does not introduce an undesirable amount of relativity in claims of similarity between a model and a real system (2004: 748). It is sufficient in his view to have a reasonable, even if not unique, measure of similarity that can be successfully applied to particular cases (2006: 65). Giere replies to his critics:

> Commentators have criticized this characterization [of similarity in respects and degrees] as "hopelessly vague" and some have insisted that without an "objective" measure of similarity, the view is excessively relativistic (Chakravartty 2001). My view is that there is no such thing as an objective measure of similarity that is completely general, but this does not matter because no such measure is needed. Once the experimental set-up and the model have been specified, the context provides whatever measures of similarity are required. (Giere 2005: 8)

We should resign ourselves to a general, neutral criterion of similarity. The point is that the lack of context-independent parameters of similarity doesn't entail the elimination of the concept. If there is evidence that uses of similarity, in the form of judgments of similarity in scientific practice, are involved in the construction of epistemically successful representation, there are good reasons to keep it in the debate about the means of representation. Then, we should perhaps discuss if between the idea of an objective, context-independent definition of similarity, and the idea that similarity is defined in each individual circumstance, there are possibilities of arriving at informative, relatively general characterizations of similarities. Looking at common uses of certain judgments of similarity, and at how in some fields standards concerning relevant similarities are stabilized and even transferred to other fields, gives good evidence that it is possible to identify stable and "contextually-objective" definitions of similarity.

2.1.3. Argument from misrepresentation against the means

I argued in Chapter 1 that a substantive account of representation should be able to account for the fact that scientific models misrepresent the aspects of the world they represent, by idealizing, abstracting, simplifying, and otherwise distorting them. The proposals of French (2003), French and Bueno (2011) and Bartels (2006), I argued, have not successfully accommodated misrepresentation. Now, we are dealing with the problem of the means of representation, but a new version of the argument from misrepresentation ("argument from misrepresentation against the means") remains. The question in this case is how we can accommodate the claim that similarity is the means of representation, which allows us to make successful inferences about a target in the world, and the claim that distortions are fundamentally involved in successful representations. In other words, does the presence of distortions undermine the idea that similarity is a good candidate for means of representation?

Nguyen (2016) has presented an argument against similarity along these lines. He doubts that similarity can be the means (or a predominant style) of representation because distortions are an integral part of many scientific models.

> Additional problems for using the notion of similarity as a universal condition on accurate representation [...] occur when, as in some cases of epistemic representation, *distortions* play a vital role in allowing us to use representational vehicles to learn about their targets [...] In taking proposed similarity as a universal style of representation, and thereby actual similarity as a universal standard of accurate representation, we seem committed to the idea that models have to be copies of their targets (even if only in the relevant respects to the appropriate degree) in order to be accurate. (Nguyen 2016: 60)

There is no doubt that when we discuss the problem of the means (or styles) of representation, we need to consider the distortions and inaccuracies included in many scientific models. The problem, following Nguyen (2016), is that when we take similarity as the means of representation, models wouldn't admit the presence of inaccuracies or idealizations. Or, if one alternatively tries to accommodate distortions within a similarity framework, the notion of similarity that drops out becomes too flexible and unhelpful (ibid.: 60-1). I agree that an account of the means of representation should seriously consider the distortions involved in successful representations, as will become clearer in Chapter 5, when I present my own view. I also agree that similarity (including structural similarity) might not be the only means of representation. However, I disagree with the assumption that similarity as means of representation is incapable of accommodating distortions without losing its significance or common meaning. There are two possible ways of responding to this formulation of the argument from misrepresentation against the means, which I discuss in turn.

The first response would be to say that we can elude the problem by claiming that, despite the distortions scientific models involve, similarity could be still the means employed in a variety of models to make *some* accurate inferences about a target. So, similarity and distortion might be indeed incompatible with each other, each pulling in a different direction, but still similarity as means of representation might allow some inferences about aspects of the target that are not distorted by the model. This response tries to reject the clear-cut idea that if (relevant) similarity between a vehicle and target is obtained, the representation is accurate, and if distortions are involved in the representation, it is inaccurate (see also Frigg and Nguyen 2016 for an analogous argument focused on structural similarity). There are, so the argument would go, various degrees of similarity, so the more distorted a model is, the less possible it would be to obtain successful results from it and the more difficult it would be to consider similarity the means of representation in that case. This type of response to the argument from misrepresentation against the means is implicit in accounts like Contessa's (2007a, 2011), which I discuss in more detail in the next section. However, this is not the response to the argument that I would like to opt for.

The second possible way of responding to the argument from misrepresentation against the means is to claim that the assumption that similarity and distortion are incompatible is misguided. The most plausible reason for adopting this misguided idea of similarity is that not enough attention is paid to the actual connotations that the notion of similarity acquires in practices of representing. Similarity is only incompatible with distortion when one understands similarity as an ideal of "pure" or "bare similarity". In Chapter 5 I spell out these terms in detail, but briefly, pure or bare similarity is a conception of similarity that, in its limit version, could be identified with "perfect imitation", "exact copy", or "mirror image" of the target represented. Under this conception, which fits with the so-called "copy theories of representation" that Goodman (1968) referred to, saying that similarity is the means of representation is saying that models aspire to be as similar as possible to their targets, ideally perfect copies of them. This assumption is for instance articulated by Nguyen (2016: 60) in the quote presented above, when he says that similarity-based accounts are "committed to the idea that models have to be copies of their targets". So if for instance some models purposely idealize their target, even if with well-motivated epistemic purposes, we would have to claim that they are diverting from the aim of being perfectly similar to their target, and therefore diverting from being epistemically successful. I believe this is an inadequate understanding of what accounts of the means of representation focused on similarity, like those of Giere (2004, 2005) or van Fraassen (2008), attempt to do. These philosophers are well aware of the idealizations involved in scientific representation, and have defended the idea that similarity and distortion can be compatible components of the same practices of constructing successful models (van Fraassen 2008: 13-5). My own proposal tries to defend this point even more explicitly and with the support of the empirical study of the meaning of similarity that emerges from representational practices. The observation of actual uses of judgments of similarity in practices of representing shows that similarity doesn't involve "bare", "pure" similarity when it is employed by epistemic agents. It involves relevant approximations, useful limits, and a balanced combination with well-reasoned idealizations and simplifications. This approach opposes Nguyen's (2016) conclusions about similarity as means of representation, but also the first type of response to the argument from misrepresentation against the means. The first response entailed that similarity might be the means of representation in some cases *despite* the distortions involved in those representations, while this second response states that similarity as means of representation should not be understood as "bare" or "pure" similarity because in actual practices it does not involve an ideal of perfect copy of a target but is intertwined and compatible with distortions.

2.2. Compatible views of similarity as means of representation

The problem of the means of representation – or of accurate or epistemically successful representation – hasn't been systematically addressed in contemporary philosophy of science in the way the problem of the constituents has. There are nevertheless some notable proposals that have contributed to the advancement of the discussion about the means of representation. In the first place, Suárez (2003,

2010) proposed the use of the concept of means in contrast to the constituents of representation. Although he didn't advance any specific account of the means of representation, he suggested various possible means of representation, and emphasized the need to develop practical enquiries to study them (2010: 91-2). In this section, I would like to leave aside the project of advancing different possible accounts of the means of representation – focused on exemplification or convention among other possibilities – and concentrate on three proposals in contemporary philosophy of science that have specifically tried to address the problem of the accuracy of representation appealing to similarity, namely those of Contessa (2007a, 2011), Giere (2004, 2005), and van Fraassen (2008). I consider the most insightful and most critical aspects of their respective proposals, and how I believe we should incorporate some of those aspects into a satisfactory account of the means of representation focused on similarity. Later, in Chapters 3 and 4, I will also consider the recent proposal that Weisberg (2013) has advanced, as well as the work on the role of similarity in specialized practices of modelling that Sterrett (2009, 2017a) developed. These works have been particularly important in terms of discussing similarity from a practice-based perspective, with special attention on the role of judgments of similarity in modelling practices in science.

2.2.1. Contessa on structural similarity and faithful representation

Contessa (2007a, 2007b, 2011) is one of the few contemporary philosophers of science who explicitly advances a distinctive account of the means of representation, or in his terms, of *faithful* representation, separately from an account of the constituents of representation. To have a full understanding of how models represent, Contessa argues, we have to offer guidance about what distinguishes a faithful from an unfaithful (or less faithful) representation (2007a: 67-8). The constituents of representation are, for Contessa, denotation and the adoption of an interpretation of a vehicle in terms of a target (2007a: 176). Similarity is neither a necessary nor a sufficient condition for representation. It is a "further condition that needs to obtain in addition to the conditions that make a vehicle an epistemic representation of the target if the vehicle is to be a *faithful* epistemic representation of it" (2007a: 89). We don't have to adopt Contessa's view on the conditions for epistemic representation to accept that similarity is that "further condition" that needs to be met in order to obtain faithful epistemic representations. For instance, we could in principle defend a deflationary view on representation, and still follow Contessa's (2007a, 2011) description of how similarity needs to obtain to achieve faithful representations. I take this general project of attempting to advance a specific characterization of the means or of faithful representation as a central point to incorporate from Contessa's account.

There are three particularities of Contessa's (2007a, 2011) proposal that I would like to highlight. The first is the version of structural similarity that he defends. The second is the normative variant of an account of the means of representation that Contessa proposes, leaving the descriptive variant and the project of developing a practical enquiry as secondary. The third is the link Contessa establishes between similarity and faithfulness. I will not endorse Contessa's specific views with respect to any of these three particularities in my account, but they comprise fundamental points of reference – and criticism – that any account of the means of representation focused on similarity should consider.

Contessa (2007a, 2011) grounds his account of faithful representation in structural similarity. He believes that the notion of structural similarity is better than the broader or more intuitive notion of similarity to capture the specific sense in which a faithful scientific model is similar to its targets (2011: 127-8). The problem with the broader, more intuitive notion of similarity, Contessa argues, is that it is too permissive and could eventually include accidental similarities between vehicle and target that don't have an epistemic role in the faithfulness of a representation (2011: 128). For instance:

> I might happen to employ a model that, on this particular occasion, happens to predict [...] accurately, but does so in an entirely fortuitous manner (say, a model based on some wacky theory [...]). Would such an accidental similarity be sufficient to make the model into a faithful epistemic representation of the system for my purposes? This question, it would seem, should be answered negatively [...]. If accidental similarity was sufficient for faithfulness, then even tarots would sometimes be faithful epistemic representations of their targets. (Contessa 2011: 128)

A general concern about the intrusion of accidental similarities is justified. What doesn't seem justified is the claim that relying on structural similarity automatically resolves the problem of accidental similarities. Contessa (2011) is reacting to accounts like Giere's (2004), which endorse the idea of "similarity in respects and degrees". For Contessa, this concept of similarity is not systematic enough because we don't know "how similar is sufficiently similar" (2011: 128). However, his own idea of structural similarity is not without analogous difficulties. A representation is perfectly faithful in Contessa's account if all the inferences that can be made from a vehicle about a target are sound, that is, both valid and true, and partially faithful if only some of the inferences are sound (2007b: 55). The problem is that a specific morphism needs to be defined before proceeding to evaluate the soundness of the inferences we can make from a vehicle about a target. And to define a morphism, agents have to define what a relevant structure is and what exactly is going to count as better (or a higher degree of) structural similarity. Contessa (2007a) appeals to the role of interpretation to explain how the relevant structures of vehicle and target are reconstructed by epistemic agents (ibid.: 107). But this is not less ambiguous than the specification of relevant respects and degrees that Giere (2005) postulates. Invoking the notion of structural similarity might be after all a "case of misplaced precision", Giere (2005: 8) claims, as "similarity has just the flexibility required".

Another reason Contessa (2011) defends the notion of structural similarity is that it can capture (better than the notion of similarity) the more abstract sense in which a vehicle needs to be similar to a target (ibid.: 128). The similarity that commonly exists between vehicles and targets in scientific representation is not visually immediate, as in the case of a painted portrait, but more abstract, as in the case of the London Underground Map (2007a: 100-102). The London Underground Map and the actual transport network are, following Contessa (ibid.), not similar "in the *intuitive* sense of the word". One is a piece of glossy paper with names, small circles and coloured lines printed on it; the other is a system of trains, tracks, platforms, escalators, and so on (ibid.: 100-1). If the map and the network are similar, "they are similar at a very abstract level". Thus the notion of similarity does not afford the technical resources to capture this abstract sense of similarity (ibid.: 102).

In response, one could claim two things. One, that Contessa would have to recognize that for cases in which the intuitive or visual sense of similarity applies, the notion of similarity would be more explanatory than the notion of structural similarity. This is the case for portraits, but also for multiple examples of scientific representations in science that take the format of pictures, photographs, and material models. Two, it is not completely clear, except in the cases of purely mathematical models, why the best way to account for relatively abstract cases of similarity is by referring to the shared structures of models and phenomena. The

London Underground Map is similar to the transport network in some very specific respects, namely the interconnection between stations. Meanwhile, the map is not similar to its target in many other respects, such as the relative distance between stations on the ground, the size of the stations, etc. Adopting a structural account of similarity to explain the faithfulness of the London Underground Map doesn't resolve the problem of, using Contessa's words again, comparing "a piece of glossy paper with coloured lines" with an "intricate system of trains, tunnels, rails and platforms" (2011:123). The identification of the relevant respects is what really matters in the process of constructing a successful epistemic representation, and this applies also for structural similarity.

I do not attempt to reject the possibility of referring to structural similarity as the means of representation in particular practices of representing. In the previous section, I mentioned that structural similarity can be taken as a form of similarity. Morphisms are relations between set-theoretic structures, and the targets of representations in science are expected to be, at least in part, non-mathematical objects. So if we decide to appeal to the notion of structural similarity for a general account of the means of representation, as Contessa (2007a) does, we would have to endorse additional assumptions about how non-mathematical objects could instantiate structures (ibid.: 107). My suggestion in section 2.1 was that an account of the means of representation focused on similarity could in principle encompass cases where structural similarities are involved. In this way, the account would be able to provide broader conclusions and more enlightening comparisons between different practices where similarity is the means of representation, including those concerning mathematical objects that exhibit a structure. In summary, the first point that I extract from the discussion of Contessa's proposal is that an account of the means of representation focused on a broadly understood notion of similarity, that is concretized in particular cases, can help advance a more comprehensive account than one focused merely on structural similarity. Besides that, Contessa (2007a) attempts to offer a *universal* type of account of the means of representation based on structural similarity, a project that I explicitly reject. I have so far defended the idea that we should encourage the development of various co-existent accounts of the means of representation, each focused on a different means employed in a relevant set of practices of representing.

This conclusion connects with the second particularity of Contessa's (2007a, 2011) account that I would like to consider. A look into scientific practices could have offered Contessa fundamental insight about how similarity or structural similarity play a role in the construction of faithful representations. Empirical and historical work is key to support the idea that certain means of representation are those employed in a series of scientific models. Contessa (2007a) doesn't directly engage with the analysis of scientific practices, however. His account is more a rational reconstruction of scientific modelling than an account sustained, at least in part, on the development of a practical enquiry (in Frigg and Nguyen 2016). Quite commonly, accounts of the constituents of representation have dedicated little space to the study of actual scientific practices. For accounts of the means of representation, not relying on the study of practices is particularly problematic, as the means do not concern universal relations between a vehicle and target but the active employment of relations between vehicles and targets by the relevant epistemic community (Suárez 2010: 93).

In Chapter 1 I mentioned two variants of an account of the means of representation: a descriptive and a normative variant (Frigg 2006: 50). Contessa's (2007a) account has a predominant normative character, consisting in the postulation of *one way* in which faithful representation is to be obtained, namely structural similarity. My aim is not exactly to argue that, to the contrary, a purely descriptive variant is desirable to address the problem of the means of representation. A purely descriptive variant could only have the form of a taxonomy of types of models ever constructed, a project that I rejected above as epistemically uninteresting in terms of addressing the problem of the epistemic success of representation (Frigg 2006). The main claim that I would like to make is that an account of the means of representation needs to be both descriptive and normative. More specifically, the source of the normativity of an account of the means of representation should be the description of norms identified in actual practices of representing. It is in practices that standards and guiding criteria are established to direct the construction and the evaluation of subsequent successful representations. So the description of those norms and standards should support the normative component of an account of the means. I will show in Chapters 3 and 4 that it is possible to localize uses of judgments of similarity in practices of representing. So the normative component of an account of the means of representation should explain how to build a characterization of similarity as means of representation from the observation of the standardizations and regulations of judgments of similarity in practice. The second point I extract from Contessa's (2007a, 2011) proposal is that an account of the means of similarity should entail both a descriptive and a normative component, instead of being a pure rational reconstruction of practices. The source of the normativity of the account should be the description of regulated uses, established norms, and standards in actual practices.

The third particularity of Contessa's account that I would like to discuss concerns the association he establishes between structural similarity and the "*faithfulness*" of scientific representation. So far in this thesis I have taken the expressions "an account of the means of representation", "an account of accurate representation", "an account of epistemically successful representation", and "an account of faithful representation" as equivalent. But in fact, claiming that similarity as means of representation concerns the accuracy, the epistemic success, or the faithfulness of scientific representation has different implications.

Let us first see how Contessa (2011) situates his own account. He starts by acknowledging that faithfulness and successfulness should be separated: "faithfulness and successfulness need not go hand in hand, since a less faithful model of a certain system can be predictively or explanatorily as successful as a more faithful one" (2011: 130-1). Someone might be able to construct a very faithful model of, Contessa adds, "my daughters tobogganing down the hill" (ibid.). That model would take into consideration, among other things, the air friction and the gravitational pull of the Moon. Alternatively, one could use a basic inclined plane model to describe Contessa's daughters tobogganing down the hill. This one would be a less faithful model as far as it wouldn't consider the gravitational pull of the Moon or the air friction (ibid.). The point of the example is to stress that, for the purpose at hand (to roughly predict whether the toboggan will go too fast) the two models would be equally successful (ibid.). I think we can unproblematically accept this conclusion. Moreover, one would usually prefer the less faithful model because it is easier to produce, simpler, and serves the same goal.

However, there are some undesirable consequences derived from the distinction between faithfulness and success that Contessa (2007a, 2011) makes. The main issue is that what Contessa (2007a, 2011) is really interested in is the characterization of the faithfulness of a model, which is what he associates with the degree of structural similarity between the model and the target. The fact that Contessa also considers the success of representations is a way of introducing some pragmatic elements in the picture, so that the intentions of the epistemic agents can play a role in the selection of one scientific model over another (2011: 130-1). But those models from which epistemic agents select which one to use are already and objectively more faithful or less faithful to the target, depending on the degree of structural similarity they have with it. Even if Contessa (2007b: 53n) argues that his conception of representation is triadic, that is, that epistemic agents are a constitutive part of the relation of representation together with vehicle and target, it is difficult to see how his characterization of faithfulness, and accordingly of similarity, can include the epistemic agents in the representation in a fundamental way. He certainly refers to how the relevant structures of vehicle and target have to be previously reconstructed by epistemic agents (2007a: 107). But, once reconstructed, Contessa explicitly claims that similarity concerns the *overall* faithfulness of the representation, that is, the relation (of co-instantiation of structures) between a vehicle and a target, as opposed to a *specific* kind of faithfulness that would involve the particular purposes of the agents (2007a: 171). If this is correct, I believe that Contessa's (2007a) account is not really characterizing the relations epistemic agents actively employ to make successful inferences about a target from a model, which is the definition of means given by Suárez (2003, 2010). The epistemic agents decide whether they want to use the more faithful or less faithful model depending on their intentions. But structural similarity in Contessa's account simply exists between a vehicle and a target. Contrary to this view, I would like to defend the idea that similarity as means of representation should be explicitly framed in a triadic conception of representation. A consequence of this is that similarity should be more adequately related to the epistemic success of a scientific representation, which depends on the goals of the particular practice, rather than the overall faithfulness of a representation. I will return to this point in Chapter 5, where I defend that a similarity-based account of the means of representation should not take faithfulness (or truthfulness) as the measure of the epistemic success of representations, but adopt instead a view where *understanding* is taken as the overarching aim of scientific practices of representing.

2.2.2. Giere on similarity "in respects and degrees"

The work of Giere (2004, 2008, 2010) is a central point of reference when studying the role of similarity in scientific representation. Although his account has been occasionally treated as a substantive account of representation based on similarity, if carefully analysed, it contains insightful tools to help advance an account of the means of representation. Probably the two main contributions of Giere's (2004,

2008) work in this respect are his understanding of similarity as an *intentional* notion, instead of the sharing of properties between the vehicle and the target of the representation; and his understanding of similarity as an intuitive, common sense concept, which needs to be specified "in respects and degrees" depending on the particular scientific practice. These two points contrast with, and help amend, the three aspects of Contessa's (2007a, 2011) account that I pointed out above.

Giere (2010) defines his own view as an intentional conception of representation. This means that epistemic agents and their intentions are built into the definition of representation.

> I argue for an intentional conception of representation in science that requires bringing scientific agents and their intentions into the picture. So the formula is: Agents (1) intend (2) to use models, M; (3) to represent a part of the world, W; (4) for some purpose, P. This conception legitimates using similarity as the basic relationship between models and the world (Giere 2010: 269).

This presupposes at least a triadic conception of representation, where apart from a model and a target there are agents leading the activity of representing. An intentional conception of representation might well have the slogan: "no representation without representers" (Giere 2008: 102). This is not incompatible with the thought that there are similarities between vehicle and target of the representation, insofar as we understand that epistemic agents are those who use those similarity relations for particular purposes. The intentional character of similarity becomes particularly clear in the following quote:

> It is not the model that is doing the representing; it is the scientist using the model who is doing the representing. One way scientists do this is by picking out some specific features of the model that are then claimed to be similar to features of the designated real system to some (perhaps fairly loosely indicated) degree of fit. (Giere 2004: 747-48)

Similarity here is by definition dependent on scientists' actions and purposes. Epistemic agents are responsible for "doing the representing", "picking out specific features of the model", "claiming them to be similar to features of the real system", and "using the model". These actions are not secondary pragmatic additions to a substantive definition of representation, but built into the definition of representation. Almost any contemporary philosopher of science would recognize that scientists intervene in the construction of models, and that their actions are necessary for representation to happen at all. Giere (2004) is making a stronger pragmatic claim than that. He is saying that a model is what agents decide to use as a model; that representation is the action of representing performed by scientists; and that similarity is what scientists decide to pick out as similar features (or loosely similar features) between a vehicle and a target.[18] I take the intentional character of similarity to be a fundamental element in Giere's work to incorporate into an account of the means of representation. Yet, in the next subsection, I refer to a limitation of the idea of *intentional* similarity and suggest a more satisfactory way of addressing the role of epistemic agents in representation, drawing on Fraassen's (2008) account.

The second fundamental feature of Giere's proposal concerns his idea that similarity always comes "in respects and degrees" (2010: 273-4). When one claims that a vehicle and a target, or any two objects more broadly, are similar to each other in respects and degrees, we would probably want to ask "but in what respects and to what degree?" Let us consider the implications of the terms "in respects" and "in degrees" separately. The phrase "similarity in certain *respects*" suggests that a vehicle and a target are relevantly similar to each in relation to some very specific features that epistemic agents have determined. A map that represents a city is similar to the city only regarding very few specific respects that agents have selected for the purpose of the representational activity (Giere 1999: 4). A tourist map representing Mexico City would usually be similar to the city with respect to the distribution of the streets on the ground, but not with respect to the elevations of the terrain or the density of the population in the city. Of course we can find maps that are similar to the city with respect to these other aspects too. Moreover, the tourist map might only be similar to the distribution of some commonly considered historically interesting streets in the city centre – and not to the distribution of secondary streets, mews, or vehicular roads. The few respects in which a representation is similar to its target should not (or not only) be understood as a limitation of the representation but ultimately as an opportunity for its epistemic success. The fact that the tourist map of Mexico City is only similar to the city with respect to the distribution of some historical streets would benefit its success (i.e. its greater usefulness): the reduction of possibilities of where to go in the city would help tourists reduce the time consumed in planning their

[18] Here I am disregarding the ambiguities in Giere's (2004, 2010) account, to which I referred in Chapter 1, and assume that his (predominant) thesis is that similarity is a means and not a constituent of representation.

visit. Representations such as maps, but also scientific models, are not similar to their targets *in all respects*, nor do they attempt to be, but concerning certain aspects relevant for the case at hand.

The phrase "similarity to a certain *degree*" can more easily lead to misinterpretation. I take Giere's (2010) use of the term degree not to concern the overall degree of similarity between a vehicle and a target either, as Contessa for instance would claim (2007a: 171), up to perfect similarity, but to the specific respects previously identified. That is to say, the respects to which vehicle and target are similar to each other only need to be "loosely similar", and not identical or close to identical. Repeating Giere's words quoted above, "features of the model [...] are claimed to be similar to features of the designated real system to some (perhaps fairly loosely indicated) degree of fit" (2004: 748). The tourist map of Mexico City represents the city with respect to the distribution of the main historical streets. Even considering only that particular respect, we wouldn't say that the map and the city instantiate exactly the same features, but are only loosely similar features. The distribution of the historical streets on the map might be for instance idealized as to form a more regular grid than they actually form on the ground. The idealization of the grid, also in the degree that is adequate for the case at hand, might help tourists more efficiently visualize the general distribution of streets. An important point is that every time a decision is made about the degree of similarity that is adequate for a specific respect, it is at the expenses of the degree of similarity of other respects of the representation. The example Giere (2006: 78) offers makes the point clearer: Mercator's map of the world from 1569 was constructed with navigational purposes, while other maps such as the much more recent projection by Peters in 1973 were constructed with geographical purposes. The consequence of this is that Mercator's map is similar (or loosely similar) to the Earth with respect to the distance between points on the surface, while seriously distorting the areas occupied by the continents. Meanwhile, Peters's map is similar to the Earth with respect to the size of the different areas, but at the cost of not preserving distances between points on the surface (2006: 78). In this sense, the expression "similarity to a certain *degree*" does not really involve a higher or lower level of similarity of the whole representation regarding an ideal of complete similarity. What is at stake is the particular way in which different aspects of vehicle and target are specifically, selectively similar to each other as to be able to achieve a representation that is epistemically successful.

These two contributions from Giere's (2004, 2006, 2010) work can help amend the three aspects of Contessa's (2007a, 2011) account that I described above as not completely satisfactory when discussing similarity as means of representation. Giere's version of similarity "in respects and degrees" appears to be more suitable than Contessa's structural similarity account, as it can better characterize the many cases of representations that do not involve objects with a mathematical structure, such as maps and cities. Also, Giere's emphasis on similarity as specifically selected for the case at hand contrasts with Contessa's (2007a, 2011) assumed connection between similarity and overall faithfulness. An account of similarity as means of representation should connect similarity with the epistemic success of representation, not with overall faithfulness, because it is the search for epistemic success, specified in particular epistemic goals (and usually non-epistemic goals) in specific contexts, what defines the relevant aspects of similarity that are included in the representation at the expenses of other aspects. In addition, Giere's intentional notion of similarity truly implies a triadic conception of representation, differently from Contessa's (2007a, 2011), where similarity merely concerns the sharing of a structure between vehicle and a target. Understanding similarity both as intentional and in respects and degrees stimulates a move away from the abstract analysis of the notion of similarity to the development of a pragmatic account sustained on the study of uses of similarity in the practice of representing. This also contrasts with Contessa's account, which was restricted to a normative, rational reconstruction of modelling practices that didn't consider how similarity is actively employed to make inferences. Still, we can also say that Giere's proposal is incomplete regarding the development of a practical enquiry on the problem of representation. Now, I discuss some particularities of van Fraassen's (2008) account that help to expand and further adjust these points.

2.2.3. van Fraassen on distortive similarity and use

A central element of van Fraassen's recent philosophical project is the defence of a strong pragmatic view on representation, which he develops with the aid of notions like "perspective", "indexicality", and "use". In his book *Scientific Representation* (2008), van Fraassen offers an updated version of his antirealist position "constructive empiricism" (1980), which he now calls "empiricist structuralism"

(2008: 237) to make room for a more appropriate (i.e. pragmatic) concept of representation (in Suárez 2011: 428). The role of similarity in van Fraassen's proposal should be therefore framed in the context of advancing both a pragmatic and a structural type of account of representation.

It is important to emphasize that although van Fraassen specifies similarity in terms of the structural properties of models and targets, or as "embedding" (2008: 29; earlier in 1980), he is not claiming that structural similarity is a constituent of the relation of representation (differently to French 2003; Bartels 2006). He defends a deflationary view on representation that draws on the idea that "there is no representation except in the sense that some things are used, made, or taken, to represent some things as thus or so" (2008: 23). Endorsing this general view on representation implies assuming that the question about the constituents of representation is irrelevant or even misleading. The question about the means, though, remains a legitimate problem that demands the study of the products of science and, more importantly, the study of the intellectual and material processes that lead to those products (ibid.: 18). Also, van Fraassen's idea of structural similarity openly rejects any kind of metaphysical realism about structures (2008: 190), differently to French and Ladyman (1999), and French and Bueno (2011). His idea of structure is not about what nature is but about what science is, and all science is "at heart mathematical" (2008: 238-9). Structuralism in van Fraassen's account is strongly supported by pragmatics: "I shall advocate a version, an empiricist version, of structuralism. Once again, the redeeming clues are to be found in pragmatics" (ibid.: 190).

Throughout this thesis I have been treating structural similarity as a type of similarity, isomorphism being the identity limit of similarity of structure. Certainly, we should be careful not to conflate accounts where similarity specifically concerns the structures of objects with an account where it concerns the properties of objects in other various ways, since this distinction has been an object of disagreement between philosophers like Giere (1990, 2004, 2006) and van Fraassen (1980, 2008) for some time. However, it is worth noting that van Fraassen's most recent proposal draws on a discussion about a common, everyday, more intuitive notion of similarity to then build up an account focused on structural similarity. I would like to refer to two important assumptions about similarity – understood in the broader, more intuitive sense – that van Fraassen incorporates into his account, as they can enrich the previous points discussed regarding Giere's (2004, 2006) and Contessa's (2007a, 2011) accounts. One, van Fraassen (2008: 23) doesn't refers to scientists' *intentions* to characterize the role of similarity in representation, but to the *use* of representations. Two, he does not only refer to the selective character of similarity, as Giere (2004, 2006) mainly does with the idea of "similarity in respects and degrees", but also to the possible *distorted* character of similarity.

The centrality of the concepts of "use" and "usefulness" in van Fraassen's account should not be taken as a reference to the practical applications of scientific representations, but more fundamentally to the "indexicality" of representations (2008: 59). That is to say, scientific models and theories, van Fraassen claims, cannot do anything for us unless we can locate ourselves with respect to their content (ibid.: 235). So it is indispensable that representations include an indexical judgment of the kind "from here we stand" in them, in the same way maps have a legend indicating that "you are here" (ibid.: 81). There might be circumstances where scientific models even lack content, as in the Cartesian coordinate system, but as scientific representations, van Fraassen argues, they cannot lack indexicality (2008: 66-69). Locating the "use" at the core of representation therefore clashes with two-term conceptions of representation that do not consider where subjects stand. But it also clashes with triadic conceptions of representation grounded merely in subjects' intentions.

We saw that Giere (2004, 2006) advances an intentional conception of representation, in which epistemic agents are responsible for selecting relevant similarities in each scientific context depending on the purposes at hand. The idea of intentions is too heavily loaded with an individualistic view on representation though, probably inherited from classic accounts of representation in the philosophy of mind (van Fraassen 2008: 23-27). In contrast, van Fraassen's urge to understand representation in direct connection with the idea of "use" can be taken as an invitation to further explore communal activities and social systems of practice in which representational activities take place. The idea of "use" might concern intentions and goals of the relevant epistemic agents, but encompasses also the coding conventions extant in the community, the reception of an audience or users, the particular display of the objects doing the representing, among other things (ibid.: 23). Van Fraassen doesn't elaborate much on the communal implications that bringing the idea of use to the core of an account of representation has. But his acknowledgment of this point (in a footnote) invites to explore the consequences of his view in this direction:

> The emphasis on use, as here understood, implies community: there is no such thing as essentially private representation any more than private language, except in the sense in which private uses can exist as derived from or parasitic on communal practices. (van Fraassen 2008: 348n24)

Representation is possible only in a context where certain systems of practice, developed skills, agreed standards, depicting conventions, symbol systems, "languages of art", and other modes and activities concerning representation are already shared in a community. A significant difference between Giere's (2004, 2006) and van Fraassen's (2008) views is that, although both endorse triadic conceptions of representation, they could be respectively characterized as "intentionality" and "intended-use" accounts of representation (Suárez 2011: 430). In the first case, representation is necessarily individual; it is located "in the head" so to speak, and sources and targets are determined by some intentional state of particular agents, regardless of community or practices (ibid.: 432). Meanwhile, an "intended-use" conception of representation, of the kind van Fraassen (2008) and Suárez (2011: 432) advocate, grounds representation in the social world.

The move from an "intentional" to an "intended-use" conception of representation has consequences for the characterization of the role of similarity in representation. If for Giere (2004) similarities are selected by agents, depending on their intentions, in an "intended-use" conception, the role of similarity as means of representation should be at least partly explained by reference to the historical and social agreements of the scientific and extra-scientific communities. For instance, from a conception based on "use", one could enquire about how certain similarities become relevant in specific historical moments and specific epistemic groups. One could also enquire about the processes of systematization of certain similarities that might help regulate practices of representing. This will be relevant for the case of the methodologies of physical similarity that I will discuss in Chapter 4. One could also ask about the role of similarity in the formation of so-called "realistic styles" of representation, as for instance they have traditionally existed in the pictorial arts but also concerning the use of images in science (see Goodman 1968: 38). Or one could ask about the extent to which human perception plays a role in establishing agreements about relevant respects of similarity or not. I don't attempt to respond to all these stimulating questions in the present thesis. Further research would be required to advance an exhaustive practice-based, historically and socially-engaged account of representation. For the more limited goals of this thesis, I would like to defend the notion that we should implement a framework to study the problem of representation in direct connection to its "use", precisely because this framework allows us to ask all those questions about similarity and take the responses seriously. I will make the specific suggestion in Chapter 3 that examining scientists' use of various judgments concerning model-target comparisons during modelling practices is a valuable source to study the agreements that conduct to the production of successful representations, including those concerning similarity.

It might be true that van Fraassen (2008) doesn't say much about how different systems of representation arise and eventually allow communities to produce fruitful types of representations. In Suárez's words, Fraassen's *Hauptsatz* is probably "very thin and provides little by way of understanding the relevant practices" involved in representation (2011: 240).[19] Van Fraassen is aware of this limitation. He recognizes that philosophers of science have usually a vision of science "from above" (2008: 91). Their views focus on what is achieved when it has been achieved; that is, the products of science such as models or theories, instead of on the long journey from the initial research steps to the attainment of temporarily stable representations (ibid.). Still, we can agree that his move to a view based primarily on "use" is a step towards a thoroughly pragmatic, social, and historically-sensitive conception of scientific representation (Suárez 2011: 433).

The second point in van Fraassen's account that I would like to discuss is his understanding of similarity not only as *selective* but, more importantly, as compatible with the introduction of *distortions* in representational practices. We have seen that Giere (2004) emphasized the selective character of similarity in claims such as "using a model to represent some aspect of the world is being able to pick out the relevantly similar features" (2004: 747). Also van Fraassen (2008) refers to the selective character of similarity when he describes the activity of representing partly as a "technique for rendering a systematically selective likeness" (2008: 8), or when he claims that "likeness in contextually selective fashion is important to scientific practice" (ibid.: 9). However, van Fraassen (2008: 13) points out that whenever there is selection of similarities, there is inclusion of distortions, infidelities, and lack of resemblance, all of them possibly crucial to the

[19] In a recent paper, Boesch (2017) suggests that the agential concepts – such as "use" – frequently incorporated into accounts of representation remain mostly unanalysed. To advance a comprehensive account of representation, something has to be said about the nature of the actions and agency embedded in the use of representation. And this is something that still remains to be done (Boesch 2017).

success of the same representations. The "use" determines the selection of likenesses that are involved in a representation; at the same time, that representation "trades equally on *unlikeness*, distortion, addition" (ibid.: 7). If we take similarity as the core of representation, as substantive accounts of representation like those of French (2003) or Bartels (2006) do, it would be puzzling to admit that distortion might be needed for effective representation. But, and this is the key of van Fraassen's position, if resemblances are "means to an end", and not the core of representation, there is no puzzle at all (2008: 15). Similarity is a means to achieve the epistemic goals of a practice of representing, and as such it can work in combination with distortion if doing so facilitates the attainment to those epistemic goals.

Figure 1. "Enrique Peña Nieto and Justin Trudeau bridge gap over Donald Trump". By Graeme MacKay. National Newswatch (June 29 2016).

An illustrative example of this view on similarity is found in caricatures (van Fraassen 2008: 15). A caricature is a picture that contains identifiable relevant similarities with the target of the representation. For example, in Figure 1, there is a figure that bears identifiable similarities with Donald Trump. At the same time, the same figure considerably distorts that very same target, Donald Trump, as he is represented as something he is not, namely a creature made out of flames and smoke that hides under the ground. A caricature is a specific type of representation characterized usually by its pictorial simplicity and the goal of conveying a critical message about a public figure or current affair. In this context, we would usually agree that the caricature in Figure 1 is a successful representation of Trump, in the very specific way in which caricatures are considered successful. Or more precisely, it is a successful representation of a specific event: the encounter of the heads of state of Canada and Mexico under Trump's scrutiny. The aim of this particular caricature was to manifest the tension of the political encounter between Enrique Peña Nieto and Justin Trudeau, possibly given the threatening promises made by Trump about breaking the NAFTA.

Selective similarities are introduced in the caricature to help achieve the particular goal of the representation. For instance, similarities with respects to the physiognomies of Trump, Peña Nieto, and Trudeau, as well as with respect to the shape of the geographical limits of the U.S. territory, are included. Van Fraassen (2008: 14) mentions two possible functions of these selected similarities. On the one hand, similarity may play "the function of being a vehicle of reference": we recognize one of the characters in the caricature as Trump because of the resemblance in certain respects to Trump (shape of the face, hair, etc.). This doesn't mean that reference needs similarity, as reference can occur through linguistic or other symbolic means. But it means that reference can be "*effected* by selective uses of resemblance and non-resemblance" (van Fraassen 2008: 14).[20] On the other hand, similarity "may also be the means of attribution or misattribution of some characteristic" (ibid.). Similarities help us recognize Trump in the caricature, but also identify specific qualities attributed to Trump. Here it becomes evident that similarity and distortion go hand in hand. The physiognomic features that make Trump recognizable in the picture are the same features that are seriously distorted: his body is shapeless or has the diluted shape of flames, and his face and body have the colours of fire. These are commonly considered features iconographically associated with figures of demons or the devil.[21] So the features that distort Trump in the caricature are the same features that make Trump similar to the devil in some respects. To put it in Goodmanian words, we know that this is a representation *of Trump*, not of the devil, but we also know, as pertinent

[20] The term "effected" should be understood as 'permitting effective reference to occur' in the representation.

[21] There is an additional level of discussion concerning the connection between similarities and symbolisms involved here. Fire and references to the centre of the Earth are traditional attributions to the devil.

audience of the image, that it represents Trump *as thus or so*, that is, *as* the devil (van Fraassen 2008: 14; following Goodman 1968: 27-31)[22]. In short, similarities work together with distortions in the task of achieving the goal of the representation. The likeness to the devil, which are unlikenesses to Trump, help highlight the hostile character of Trump's policies and discourses. I shall refer to the distortive character of relevant similarities in more detail in Chapter 5, when I propose the notion of "creative similarity" precisely to address this features of similarity.

The contributions that I extracted from van Fraassen's (2008) account help complement the characterizations of similarity that Giere (2004, 2006) and Contessa (2007a, 2011) offered. The notion of similarity broadly understood is conceived by van Fraassen as compatible with distortion, the two working together towards the same epistemic end of representational practice. This opposes the thought that there are levels of similarity that correspond to an overall faithfulness of a representation (Contessa 2007a). The example of the caricature shows how it is not so pertinent to refer to the higher or lower levels of similarity of the image. Neither the inclusion of more similarities with respect to the target of the representation (more details about the political event, more features of the politicians represented), nor the use of a more realistic pictorial style of representing, would have necessarily rendered the caricature more successful. The specific goals of the representation are what determines the adequate combination of similarities and distortions employed. As van Fraassen (2008: 15) remarks, in a certain political context "a caricature may rightly be judged to be accurate, [while] in another misleading or blatantly false" (2008: 15); "much hinges here on what the criteria of success were when [a representation] was made" (ibid.: 13). It is possible to imagine a different context in which the caricature above would be unsuccessful (or bad, false, incomprehensible); for instance, one in which Trump was recognized for his ability to advance conciliatory foreign policies. A list of likenesses separated from the understanding of the use of the representation is not going to tell us how successful the caricature is. The main lessons I take from van Fraassen's proposal are his strong pragmatic view on representation focused on the idea of "use", which invites us to study the community agreements and social systems of practice in

[22] Of course Goodman (1968) would reject this description of the image in terms of similarity. In any case, van Fraassen (2008: 11) adopts Goodman's (1968) distinction between "representation *of*" and "representation *as*" in the context of characterizing the role of similarity in representation in a way that does not contradict but is compatible with the introduction of distortions.

which representational practices take place; and his general characterization of the role of similarity in representation as compatible with the role of distortion.

2.3. Conclusions

This chapter attempted to establish some important premises to discuss what an account of the means of representation is, and what an account of the means of representation focused on similarity would sustain. I started arguing that there is a patent intuition among many philosophers of science that similarities are involved in numerous scientific models. But explaining what exactly that involvement is remains an open object of debate. The thought that similarity might be an effective means of representation seems to be a promising way of addressing the issue. The general claim that an account of the means of representation focused on similarity, like the one I intend to advance, would sustain could be expressed as follows: similarity is one of the common relations (or *the* relation, in a stronger version of it, which I rejected) actively employed by epistemic agents to perform successful inferences from a model about a target in the world (Suárez 2003, 2010). In other words, proposing an account of the means of representation focused on similarity can help describe how an epistemically relevant set of successful representations are produced in science.

There are, however, a few arguments that should make us doubt whether similarity is actually a good candidate for means of representation. The argument from variety claims that, given the huge diversity of models there are, and the different ways in which epistemic agents can potentially employ relations between vehicle and target to make inferences, similarity could be at best one among many other means. To this I replied that similarity might not be the only means of representation. But this should not stop the project of attempting to develop a comprehensive and generic to some extent (although not universal) type of account focused on similarity, if there is evidence that in a plurality of modelling practices similarity works as effective means of representation. The argument from vagueness claims that similarity is an empty or utterly vague concept, given that its meaning has to be specified in every particular case. To this argument I replied that having an absolute, scientifically objective definition of similarity doesn't seem achievable, but that it should not be an aspiration either. What we can do is study how similarity works in practices of representing, where standards and general agreement about definitions of similarity are established. Lastly, the argument from misrepresentation against the means claims that, since scientific models distort, idealize, and abstract the targets they represent, similarity doesn't seem a good candidate for means of representation. I replied to this argument that similarity and distortion, as they actually intervene in practices of representing, are not incompatible with each other but intertwined resources. Therefore, similarity, if it is not characterized as "bare similarity" (as I will define in Chapter 5 in detail) can be still considered a means of representation in multiple cases of scientific models that contain distortions.

Then, I discussed some constructive proposals in recent philosophy of science that can help to advance an account of the means of representation focused on similarity. Referring to Contessa's (2007a, 2011) work helped me highlight that, first, the more intuitive and flexible notion of "similarity" seems more promising than a structural conception of similarity to advance a comprehensive account of the means of representation that encompasses a variety of practices. Second, developing an account of the means requires a practical enquiry into the uses of similarity in modelling practice, not only a purely normative approach to it. Third, the role of similarity should be more directly connected to epistemic success than to the overall faithfulness of representations. Looking at Giere's (2004, 2006) account was particularly useful to emphasize that similarity as effective means of representation always comes in respects and degrees; and that within a triadic conception of representation, similarity is not a relation between vehicle and target, but it includes the epistemic agents and their intentions as well. Finally, from van Fraassen's (2008) account I explicitly endorsed the idea that similarity is defined by the "use" of the representation, which concerns systems of practice and communal agreements. Also, the idea that similarities and distortions are compatible and mutually enriching in the practice of representing that van Fraassen (2008) defends will be fundamental to propose the notion of "creative similarity" later on in the thesis.

The discussion in the following two chapters incorporates the various conclusions achieved in this chapter, both concerning the constraints to develop an account of the means of representation and the positive components that such an account should include. The direction that needs to be followed now is the more direct study of practices of representing, including the actual role of similarity in them. The development of a practical enquiry involves paying attention to particular case studies and to scientists' use of judgments concerning model-word comparisons during the construction of scientific models.

Chapter 3

Judgments of Similarity in Scientific Practice

In the previous chapter I examined some key proposals in recent philosophy of science that have contributed to the discussion about the means of representation. I discussed some accounts that have taken similarity as a possible means of representation (Suárez 2003, 2010; Contessa 2007a, 2011; Giere 2004, 2005; and van Fraassen 2008). An important conclusion was that advancing an account of the means of representation requires developing a practical type of enquiry, focused on the use of representation. A practical enquiry makes it possible to observe how certain relations are actively employed by epistemic agents to make inferences about a target in the world from the use of a model, which is the definition of means offered by Suárez (2003; 2010). There is, however, an important element that has not been sufficiently spelt out in accounts that address the problem of the means of representation: how do we specifically observe and reach conclusions about the relations actively employed by epistemic agents to make inferences about certain targets in the world? The goal of this chapter is to argue that the study of scientists' judgments concerning model-target comparisons during modelling practices is crucial to develop a philosophical account of the means of representation supported on a systematic practical enquiry. I will focus on the formulation of judgments of similarity in modelling practices, as this will be the basis from which to advance a similarity-based account of the means of representation later on in the thesis.

With the term "judgments of similarity" I try to encompass the various expressions containing the idea of similarity (and associated concepts like resemblance and similitude) that scientists make use of in representational practices concerning model-target comparisons. In the process of designing, constructing, and evaluating scientific models, scientists and engineers use judgments of similarity regarding the features, behaviours, and relations involved in a particular model in comparison with features, behaviours and relations involved in the target of the representation. The attention to the uses of judgments of similarity in scientific practice offers valuable insight about how similarity plays a role in the production of epistemically successful representations. Records of the uses of judgments of similarity might be explicitly displayed in scientific reports, technical manuals, interviews, and published articles. In those cases, it is easier to track the involvement of particular judgments of similarity in the decisions and agreements relating to the construction of specific models. In other cases, there might not be explicit records of the judgments made. Still, if a comprehensive practice-based analysis of particular case studies is developed, it should be possible to identify some of the judgments involved in the actions scientists perform during representational practices, including judgments of similarity. In the present chapter I introduce several examples that illustrate the presence of judgments of similarity in a variety of practices of representing in science. Later, in Chapter 4, I develop a detailed analysis of a case study (of scale models in engineering) to show how specific judgments of similarity participate in the construction of epistemically successful representations. These analyses support the idea that similarity is a good candidate to develop an account of the means of representation, since it plays a significant role in the production of a variety of modelling practices that we recognize as epistemically successful. Moreover, these practice-based analyses of the actual role of judgments of similarity help resist the arguments in recent philosophy of science that claim similarity is unimportant for representation in general.

First, this chapter begins by introducing some reflections on the projects of "Philosophy of Science in Practice" (PSP) and "integrated History and Philosophy of Science" (iHPS) in Section 3.1. These projects, or fields of study, offer ideal frameworks with which to develop a practical enquiry on the problem of the means of representation (Chang 2012; Schickore 2011; Dear 2012). An account of the means of representation would especially benefit from endorsing the rationale of PSP and iHPS because these fields offer methodological tools to reconcile, in a single account, the description of scientific practices and a normative view of philosophical problems like representation. In Section 3.2, I argue that studying the uses of judgments in scientific practice is fundamental to develop an account of the means of representation framed within the projects of PSP or iHPS. I briefly discuss the implications of the notion of "judgments" and their role in science (Elgin 1996; Brown 1988; 2006; Siegel 2004; Tversky 1977; Giere 1988; Wimsatt 2007). Lastly, in Section 3.3, I introduce the expression "judgments of similarity", and characterize it with the help of some important contributions in recent philosophy of science (Weisberg 2013; Sterrett 2009, 2017a, 2017b); finally, I present some examples that illustrate the presence of judgments of similarity in a variety of modelling practices (Bengoetxea et al. 2014; Morgan 2012; Ankeny et al. 2014).

3.1. Methodological lessons from PSP and iHPS

The fields of Philosophy of Science in Practice (PSP) and integrated History and Philosophy of Science (iHPS) can be taken as particular manifestations of the wider project to naturalize philosophy of science, which in turn is a product of the wider project to naturalize epistemology that took place during the second half of the twentieth century (see Kuhn 1962; Quine 1969; Goldman 1986; Giere 1988; and Kornblith 1988, for influential works on the naturalization of epistemology and philosophy of science). Broadly speaking, naturalized epistemology aims to study the processes by which beliefs are produced, on the assumption that this would help determine which of those beliefs constitute knowledge (Eraña 2007: 101). In philosophy of science, naturalized approaches claim, with similar motivations, that scientific practices ought to be taken seriously into consideration as sources with which to evaluate scientific knowledge (Ankeny et al. 2011: 304). Various aspects of scientific practices are emphasized in naturalized philosophy of science, depending on the particular approach, from the psychological components of practices, to the biological, neurological, sociological, or historical aspects they entail. I would like to focus on the projects of iHPS and PSP within this broader trend, as they address particularly relevant methodological issues that can help in the endeavour of advancing an account of the means of representation. Some well-known examples among the varied works in iHPS and PSP are found in Shapin (1994), Hacking (1999), Chang (2004), Schickore and Steinle (2006), Daston and Galison (2007), and Dear (2012).

The most explicit characterization of the project of philosophy of science in practice has been formulated in the mission statement of the Society for Philosophy of Science in Practice (SPSP),[23] and by some of its founding members in Ankeny et al. (2011). The SPSP was created in 2006 to advocate "a philosophy of scientific practice, based on an analytic framework that takes into consideration theory, practice and the world simultaneously" (the SPSP Mission Statement). To develop such a project, one of the first things its advocates had to do was to define scientific practices:

> Practice consists of organized or regulated activities aimed at the achievement of certain goals. Therefore, the epistemology of practice must elucidate what kinds of activities are required in generating knowledge. Traditional debates in epistemology (concerning truth, fact, belief, certainty, observation, explanation, justification, evidence, etc.) may be re-framed with benefit in terms of activities. In a similar vein, practice-based treatments will also shed further light on questions about models, measurement, experimentation, etc., which have arisen with prominence in recent decades from considerations of actual scientific work. (SPSP Mission Statement)

Practices here are not taken as the sum of individual actions, but as organized and regulated systems of actions that concern a community and their epistemic (and non-epistemic) goals. The mission statement also points out that the philosophy of science in practice is "concerned with not only the acquisition and validation of knowledge, but its use" (SPSP Mission Statement). I take this to imply that the study of practices is neither limited to the analysis of what has traditional been called the context of discovery – acquisition of knowledge – nor exclusively the context of justification – validation of knowledge – (distinction proposed by Reichenbach (1938). Studying practices concerns an analysis of the "use" of scientific knowledge, an idea that blurs the sharp distinction between the two contexts. The use, as I claimed in Chapter 2 when discussing van Fraassen's (2008) proposal, involves not merely how pre-existing knowledge gets applied to practical ends but more importantly "how knowledge itself is fundamentally shaped by its intended use" (SPSP Mission Statement). An account of the means of representation fits with the rationale of PSP to the extent that it should assume that the study of the use of scientific representations will offer insights on how

[23] SPSP Website. Consulted 12 January 2018. http://www.philosophy-science-practice.org/about/mission-statement

representations are fundamentally shaped, as well as how they are validated as successful or unsuccessful by the relevant epistemic communities.

A suggestive distinction concerning the project of PSP can be helpful here. Dupré (2012: 9) differentiates between an account of "philosophy of science-in-practice" and "philosophy-of-science in practice". While an account of philosophy-of-science in practice implies advancing a philosophical work that engages with scientists' conceptions of philosophical problems, philosophy of science-in-practice is philosophy that analyses science in the making, engaging with the communities producing science (i.e. their various goals, tools and social structures). Suárez (2015) uses this distinction to suggest two possible approaches to the study of scientific representation from a deflationary perspective. An account of representation from the point of view of the philosophy of science-in-practice would take the study of the use of representation as indispensable to determine its content (ibid.: 47). Meanwhile, an account of representation from the point of view of philosophy-of-science in practice would imply that the analysis of the constitutive question on representation, even where feasible, cannot explain its use and conditions of application (ibid.). Whether one endorses a deflationary perspective on representation or not, the distinction between the two approaches of PSP is especially relevant as background against which to articulate an analysis of the problem of the means of representation.

I would like to claim that both approaches to PSP should ideally be incorporated into a comprehensive account of the means of representation. Such an account should incorporate an approach in terms of philosophy of science-in-practice because an investigation of the means of representation demands more than the study of the final products of science (theories, models, simulations). It requires the study of science-in-practice, of the use of scientific representation, and the paths to successful models and fruitful inferences. More specifically, the study of the means of representation should be primarily a study of the *resources* used in the practice of representing that, at the end of that practice, permit epistemic agents to actively employ relations between vehicle and target to make inferences, instead of mainly a study of those *relations* employed at the end of the practices of representing. The twofold understanding of the idea of means of representation as *relations* employed at the end of the practice and as *resources* used in the construction of representations will be reconsidered in Chapter 5.

An account of the means of representation should also incorporate an approach to philosophy-of-science in practice, in the sense that it should adopt a pragmatic or practice-based methodology in philosophy-of-science. This pragmatic methodology can be interpreted as one that facilitates the integration of a descriptive and a normative component within the same account of the means of representation. I would like to argue that the particular methodological tool that would allow the integration of the descriptive and the normative components is the study of the judgments formulated by epistemic agents concerning model-target comparisons throughout representational practices. For instance, the systematic study of the use of judgments of similarity in modelling practices would allow us to support a similarity-based account of the means of representation. In other words, the description of actual uses of judgments of similarity in representational practices should be a source of normativity in an account of the means of representation focused on similarity. An alternative account of the means of representation, for instance focused on exemplification or convention, would have to be sustained on the uses of judgments of convention or exemplification in representational practices.

The project of iHPS has similar motivations to PSP, although it pays particular attention to the idea that attention to scientific practices inevitably involves historical aspects of practices, and the communities, contexts, and rules that govern them at particular historical moments. Work in iHPS initiated in the 1960s, stimulated by Kuhn's (1962) work, and was profuse over the following decades.[24] The field of iHPS faces particularly interesting methodological dilemmas, the main one of which is, of course, how to integrate philosophical and historical perspectives on science into a single account. More specifically, how do we gather historical evidence about particular circumstances to justify philosophical claims, which have a universal (and even ahistorical) character? How do we support general philosophical claims with a handful of singular events?[25] These problems resonate with the broader issue of how to develop an account that descriptive of the historically contingent scientific enterprise and, at the same time, keeps its normative character as a genuine philosophical discourse. I would like to refer to the responses that Schickore (2011) and Chang (2012), among others, have

[24] The volume *Integrating History and Philosophy of Science. Problems and Prospects* (Mauskopf and Schmaltz 2012) offers a good overview of the state of the field of iHPS after fifty years of debate.
[25] Whether philosophical claims have an ahistorical character or not is itself a point of discussion within iHPS. Authors like Kuukkanen (2015: 1) argue that philosophy and history entail incompatible metaphysics, i.e. essentialist versus historicist metaphysics, while others like Chang (2012) argue that it is possible to conceive philosophy as a historically-engaged endeavour.

offered to this challenge. From there I extract general methodological guidelines for the project of developing an account of the means of representation.

The assumption that iHPS faces the problem of combining two perspectives, one historical and one philosophical, involves, following Schickore (2011) and Chang (2012), a confrontational model. A confrontational model is one in which frameworks from different fields are supposed to be contrasted with one another. If we accept the confrontational model of iHPS, Schickore (2011: 469) argues, there is the worry that the frameworks of philosophy and history of science appear so fundamentally different that we conclude that generalizations cannot be drawn from isolated instances or case studies. However, both Schickore (2011) and Chang (2012) reject the confrontational model. Philosophical reflection is above all an interpretive endeavour, consisting in an attempt to understand scientific concepts and practices by understanding how they came about (Schickore 2011: 455). In other words, the idea of "combining" history and philosophy is misguided from the outset, since the analysis of science involves "interpretation, clarification, and explication of scientific concepts and arguments" (ibid.: 471). In equivalent terms, Chang (2012: 110) vehemently rejects what he calls "the inductive view of the history-philosophy relation, which takes history as particular and philosophy as general". His argument is that, when we extract abstract insights from concrete cases, that is not so much a process of generalization as an articulation of what was already put into the cases that we have somehow produced ourselves. That is why, instead of the notion of "case studies", Chang (ibid.) prefers to use the term "historical episodes". The idea of an episode makes more evident the abstract and analytical tools required to select and characterize what a relevant case study (or episode) is in the first place.

From the proposals of Schickore (2011) and Chang (2012) we can conclude that the work in iHPS doesn't have to proceed either "bottom-up" or "top-down", as traditional discussions in the confrontational model claim (Schickore 2011: 472). An account in iHPS does not strictly proceed bottom-up because it is not merely a generalization from actual events, insofar as one needs preliminary concepts to examine historical records and distinguish between events (ibid.). It does not proceed strictly top-down either, because it is not an illustration or test of preconceived ideas about science. Examination of the historical record will affect the initial preconceived ideas as well (ibid.). This shouldn't be taken as a shortcoming of the methodology of iHPS but as an acknowledgement of the stance that, as epistemic agents, we always act *in medias res* (Elgin 1996: 15). Preliminary concepts are adjusted in the light of particular cases until a cogent interpretation is obtained, in such a way that all the elements we have are kept in a relation of "reflective equilibrium" (Schickore 2011: 472; Elgin 1996: 14; originally in Rawls 1971). A system in reflective equilibrium maintains its elements in balance and coherence with each other. But more than coherence between the elements of a system is required. The system is also "reflective" because its components have to be reasonable in light of one another, and the system as a whole has to be reasonable in light of our previous tenable commitments (Elgin 1996: 107).

Developing a practical enquiry on the problem of the means of representation requires examining particular case studies, which are typically historical cases. So the methodological reflections in iHPS just pointed out can assist in defining a framework to develop it. A case-study-based, historically-engaged account of the means of representation would not work in a top-down or a bottom-up direction strictly speaking. The process is, as for any account in iHPS, interpretive. Preliminary ideas about what the means of representation are and how successful scientific representations are produced help the search for concrete examples and historical case studies. From the detailed examination of those examples and case studies we extract some conclusions that might lead to the revision of preliminary ideas. When an adequate equilibrium is achieved, even if it is a revisable one, we might be able to make some insightful generalizations about the functioning of certain means of representation and the dynamics of constructing successful representation.

My own proposal exemplifies this methodological strategy. I draw on initial assumptions about similarity as a possible effective and prevalent means of representation in science. The selection of cases I introduce – of scale models in Chapter 4, as well as various examples of modelling practices later on in this chapter – is motivated by those initial assumptions. The analysis of particular cases helped me observe that similarity indeed plays a role in the construction of multiple successful representations, through the formulation of a variety of judgments of similarity, even beyond the initial expectations. Also, the observations made in the case studies led to the amendment of the initial assumptions: similarity is present in practices of representing under the form of judgments of similarity that are in constant combination with judgments of distortion, depending on the goals of the practice. This fact prevented me from proposing an account of the means of representation focused on similarity understood as "pure" or "bare similarity". I will explain in Chapter 5 what precisely I mean with "bare similarity", and defend the idea that an account of the means of representation should be focused instead on the idea of "creative similarity", which captures much better the dynamics of the interplay of judgments of similarity and distortion in representational practices.

3.2. Judgments in scientific practice

In this section, I discuss the idea of "judgments in scientific practice", to then focus on "judgments of similarity" in the next. Some contemporary philosophers of science like Weisberg (2013: 138) have argued that an analysis of the problem of representation should reflect judgments that scientists actually make in practice. This is, I believe, an adequate starting point from which to develop an account of the means of representation. Yet, Weisberg (2013) does not specify what the idea of judgments exactly comprises, nor is it sufficiently spelt out to what extent his account relies on actual judgments that scientists make. My aim here is to articulate, in more detail than Weisberg (2013) does, what the idea of "judgments" would entail in the context of discussing scientific practices.

Two clarifications, coming from debates outside philosophy of science, can help elucidate the implications of incorporating the term "judgments" in an account of the means of representation framed in PSP or iHPS. The first is that the term judgment can be used both to refer to an ability humans exercise (i.e. a process) and to the result of such an ability (i.e. a product with specific content). The second clarification is that the notion of judgment has been profusely discussed both in the fields of epistemology and experimental psychology in recent times, with slightly different implications in each field. I expand on these points, and specify what I take from each of them when using the notion of judgments in scientific practice.

There are two possible meanings of the term 'judgment', as a process and as a product. The first entry of the Cambridge Dictionary for judgment defines it as an uncountable noun, the "ability to form valuable opinions and make good decisions"; the second defines it as a countable noun, the "decision or opinion about someone or something that you form after thinking carefully". The distinction between the two meanings is relevant because some disagreements about what judgments are maybe caused by an emphasis on one or other meaning of the term. For instance, in debates in epistemology, Brown (1988) has prioritised the idea of judgment as a process, while Siegel (2004) rejects Brown's proposal by arguing that he does not address the specific content of judgments (judgment as product). I take the term "judgments in scientific practice" to involve the two senses of the term to some extent. Scientists employ their ability to make judgments concerning model-target comparisons throughout practices of representing in science. There are, at the same time, specific analysable contents in the judgments scientists formulate concerning the construction of representations. We should be wary of not reducing the ability to make judgments (and the processes and actions connected to that performed ability) to the propositional content of the judgments. One must avoid the "mistake of only paying attention to the propositional aspects of the scientific actions" (Chang 2014: 67). Scientific work consists of organized systems of physical, mental, material, and social actions. With the practice-turn in philosophy of science – which coincides with the booming of iHPS and later on PSP – the focus of attention changed from propositions to the study of those actions (ibid.). There is certainly propositional content in judgments formulated in scientific practices (including judgments of similarity). But our study of judgments concerning model-target comparisons should go beyond propositional analysis of them. It should also be a study of how and when scientists formulate those judgments in connection to different activities in representational practices (design, construction, evaluation of models), and how those judgments trigger certain actions, decisions and agreements, in an epistemic community.

Moving to the second clarification, it is worth noting that the idea of judgment has been debated both by epistemologists and experimental psychologists in recent times. In epistemology, judgments are examined in relation to debates on rationality (Brown 1988; Govier 1999; Siegel 2004), while in psychology, the term judgment is used in the context of investigating individuals' decision-making ability (Simon 1955; Tversky 1977; Gigerenzer and Gaissmaier 2011). Some particularities from each of these fields relevant to my argument are pointed out.

In epistemology, recent views on the problem of rationality, like those of Brown (1988) and Govier (1999), defend the idea that rational claims should be understood in connection to the role of judgments. This is contrary to classical conceptions on rationality in epistemology, in which rational claims are a matter of conformity to explicit rules, such as logical, inductive, or evidential rules (Siegel 2004: 598). The problem with the classical conception of rationality is that, even if rules are desirable operating principles, the process of deciding which rules to adopt, or when rules should be relaxed or reconsidered, takes us inevitably to subjects' judgments (Brown 2006: 649). So if we want to make sense of actual human exercises of rationality, we cannot exclude the role of judgments from our account. This position would move us closer to the definition of judgment as a process in which an ability is exercised. Notwithstanding this, the validity of specific judgments seems to be a matter of satisfying relevant criteria. And what are relevant criteria if not rules that are followed, consciously or not, by a relevant epistemic community? (Siegel 2004: 608-9). A *good* or normatively appropriate judgment is not independent of rules. This position puts us closer to (or at least not completely separate from) a definition of judgment in terms of the specific rational content of particular claims. There is ultimately agreement in the contemporary debate on rationality that both rules and judgments are part of what grounds rationality. But exactly how they are entangled is still an open debate. There is also a firm agreement, following from the idea that judgments are connected to some degree to rules, that judgments are not "mere opinions". To put it Brown's (2006) words:

> Exercising judgment is not the same as having an opinion. On my account, exercise of judgment is a skill that is subject-matter specific and one that we develop by mastering available information and techniques in a field. An individual may develop the ability to exercise, say, engineering judgment or medical judgment in much the same way as one develops the ability to write computer programs or construct proofs in logic (Brown 2006: 647).

Judgments are not mere opinions because they are assessed by established rules, which in turn might be the product of previous tenable judgments. Brown (1988) in particular thinks that judgments need to be "submitted to the community of competent individuals for evaluation and criticism" (ibid.: viii; in Siegel 2004: 605). This might not require that I accept the consensus of the community or experts, only that I take their responses seriously as a means of enhancing the basis for my judgment (Brown 2006: 650). When we refer to judgments in scientific practice, it is patent that epistemic agents attempt to make tenable judgments according to the particular situation they are in. And, at the same time, they are following the rules governing their epistemic practice and relying on experts' evaluations. I take the link between rules and judgments as essential when discussing what judgments in scientific practice are, and in particular what judgments of similarity are.

Probably one of the most insightful accounts in contemporary epistemology that encompasses the discussions presented above is Elgin's (1996, 2017a) study of the role of judgments within complex epistemic systems. I take her views in *Considered Judgment* (1996) and the recent *True Enough* (2017a) as central points of reference when using the term "judgments in scientific practice". The idea of "considered judgment" points to the commitments that are held within networks of interrelated values, standards, and norms, whether in science, art, or other social systems of practice. A key feature of judgments is that they must be revisable in the light of one another (1996: 12-3). We can sustain some initially tenable judgments if there are not strong reasons to discard them, but that is very different from claiming that some judgments are foundationally basic beliefs (ibid.: 110). If our considered judgments prove inadequate, we round them out by additional judgments; if they lead to untenable conclusions — for instance if they generate inadequate predictions — we "retrench, retool, and try again" (2017a: 12-13). This method of relying on considered judgments while rejecting foundationalism is risky, Elgin argues, as judgments can be the repository of ancient error (1996: 13). Still, it is a bearable risk, as judgments are not kept come what may. They are sanctioned by the intersubjective agreements of the community, which are authoritative and usually difficult to satisfy (ibid.: 14-15, and 82). Applying Elgin's (1996, 2017a) view to the case of representation in science, we can also claim that judgments are formulated in accordance with networks of values, standards, and norms that have been agreed within the epistemic community. During modelling practices, some judgments might prove inadequate to construct a representation that accommodates the behaviour of the target represented, while other judgments might be disputed among scientists, and all of these have to be tested against the rules and standards of the scientific community.

Moving to the approach in experimental psychology, the study of judgments in this field has been closely related to the analysis of human decision-making ability. In the 1950s, Simon (1955), and in the 1970s Tversky (1977) and Tversky and Kahneman (1976), famously examined and even conducted experiments on how individuals make everyday judgments, in particular judgments of similarity, concerning objects, shapes, situations and places. One of their conclusions was that agents employ certain strategies, which they call *heuristics*, to make judgments in circumstances of uncertainty (Tversky & Kahneman 1976: 1131). Heuristic strategies are considered rules of thumb used in problem-solving that improve the capacity of subjects to evaluate possible scenarios (see Simon 1955; in Hey 2006: 472-3). Heuristics might eventually lead to errors due to the biases they involve (as pointed out by Tversky and Kahneman

1976: 1131),[26] but they are usually effective for their capacity to simplify complex processes such as those involving probabilities (see Gigerenzer 2000; Gigerenzer and Gaissmaier 2011; Hey 2006).

These works in psychology, together with other research in the cognitive sciences, have had an impact in debates in philosophy of science, as part of the trend to naturalize philosophy of science referred to in Section 3.1. When philosophers of science like Giere (1988), Wimsatt (2007), and Weisberg (2013) refer to the judgments scientists make in modelling practices, they are usually endorsing some of the ideas proposed in psychology by Simon (1955) or Tversky (1977). For instance, Giere (1988) draws on Simon's (1955) work when he argues that scientists do not follow a Bayesian model of decision-making when deciding what models to use and explore, but a model of bounded rationality in which agents have limited ability to gather and process information (Giere 1988: 158). Wimsatt (2007: 76-7) has thoroughly investigated the heuristics strategies used in science, following both Simon (1955) and Tversky and Kahneman (1976). Heuristic strategies in science are cost-effective, well-adapted to specific contexts of research, checkable, and improvable (Wimsatt 2007: 76-7). They work very differently from truth-preserving algorithms, as they do not guarantee correct solutions to a problem if correctly applied (ibid.: 346). Still, scientists' judgments are guided by heuristics because they help them calibrate, correct, and increase the robustness of their scientific products (ibid.: 345). Scientists need to "use what comes readily to hand, as quicker, cheaper, more convenient" given the complexities of their objects of study (ibid.: 354). Lastly, Weisberg (2013) has recently applied Tversky's (1977) *contrast account of similarity* to formalize the judgments of similarity scientists make use of in practices of representing. I will refer back to Weisberg's (2013) account in Section 3.3.

From the research done in experimental psychology, we can incorporate some indications that help refining the notion of judgments in scientific practice. Judgments in the practice of constructing scientific models are not always the product of perfectly rational, truth-preserving, methodologies of decision-making. They are sometimes the result of applying rules of thumb and heuristic strategies. This is important when considering the role of judgments of similarity in achieving successful models in science. In Chapter 4 I will identify the use of various types of

[26] Tversky and Kahneman (1976, 1983) refer to three types of heuristic strategies that respectively involve three types of biases: representativeness, availability, and anchoring.

judgments of similarity in the case of the construction of scale models in engineering. Some of those judgments, I will argue, are less systematic or standardized, and have a more "intuitive" character. Still, they are grounded in previous skills and shared experiences of members of the epistemic community, and play a role in the phases of design and calibration of the models under construction.

As the result of bringing the previous ideas together, we can now arrive at a positive characterization of the value of "judgments in scientific practice" for an account of the means of representation. Judgments formulated in representational practices have, on the one hand, a valuable propositional content because that content concerns model-target comparisons that are assumed, tested, and stabilized by a relevant epistemic community. On the other hand, a pragmatic perspective on the problem of representation reminds us that we should not study the propositional content of those judgments independently from consideration of the processes and actions that take place in the same representational practices. Studying judgments in scientific practices is also very valuable because we can observe that the formulation of those judgments triggers actions such as decision-making regarding the design of a model, the application of a methodology to construct certain parts of a model, the inclusion or exclusion of certain features in a model, the resolution of a disagreement about the most adequate model-target comparison, or the evaluation of the predictive results of a model. In addition, we can observe that both actions and judgments are partly determined by rules, standards, and values pre-existent in the community. Rules and standards should be understood as desirable operating principles that regulate epistemic systems of practice. Judgments might, in turn, lead to the consolidation of new rules if they prove to be tenable. For instance, if a particular judgment triggers a certain way of resolving a disagreement in a representational practice, which eventually proves to be an effective way of resolving it, that judgment might become a rule or recommendation that regulates how to resolve similar disagreements in the future. Additionally, new values (epistemic and non-epistemic) could motivate the adjustment of both rules and judgments existent in a system of practices.

A study of judgments in scientific practice might comprise a variety of empirical and historical sources, such as the analysis of historical reports and memoirs of the construction of models, the direct observation of modelling practices (in the tradition of ethnographical work), interviews with scientists and engineers, textual analysis of scientific papers and manuals, etc. Philosophers of science interested in practices have certainly looked at the reasoning behind some of the decisions scientists make during modelling activities (see Nersessian 1992; Magnani et al. 1999; Knuuttila 2009). Few of them, though, introduce explicit considerations to the uses of judgments of similarity, or to other types of judgments in representational practices. I believe that attending to recurrent uses of judgments regarding model-target comparisons during the construction of models is essential to shed light on the problem of the means of representation. The case studies in Chapter 4 show how identifying, classifying, and analysing a particular set of judgments in scientific practice, namely judgments of similarity in scale modelling, helps obtain illuminating conclusions about the means employed to construct successful representations. In the next section, before moving to these historical case studies, I describe some issues concerning judgments of similarity in particular.

3.3. Judgments of similarity

Following the previous characterization of judgments in scientific practice, we can now claim that the study of judgments of similarity is a valuable source of information to support an account of the means of representation focused on similarity, for two reasons. One, because judgments of similarity have a propositional content that concerns model-target comparison (in terms of similarity) that are assumed, tested and established by the relevant epistemic community; two, because the formulation of judgments of similarity triggers, motivates, and is connected to a variety of processes and actions taking place in representational practices. Sometimes judgments of similarity are introduced when discussing the design of a model in comparison with the target to be represented; sometimes they are used when deciding the materials, scales, and features to be used when building a model, considering relevant materials, dimensions, and features of the target; sometimes judgments of similarity are introduced when calibrating or correcting the functioning of a model; and sometimes they are used when assessing the adequacy of the results obtained from a model. I do not aim to offer a complete analysis of possible judgments of similarity in practices of representing. The aim here is more limited, to suggest, with the help of some examples and a case study in Chapter 4, that judgments of similarity are present in various forms in a plurality of representational practices, performing an active epistemic role in the attainment of representations that we consider epistemically successful. Now, I discuss two proposals in recent philosophy of science that have explicitly referred to the formulation of judgments of similarity in particular modelling practices, namely Weisberg (2013) and Sterrett (2009, 2017a, 2017b). Then, I present various examples of practices of representing in which judgments of similarity play an effective role.

3.3.1. Weisberg and Sterrett on judgments of similarity

Weisberg (2013) and Sterrett (2009, 2017a, 2017b) have probably advanced the most noteworthy proposals in recent philosophy of science concerning the practical involvement of similarity in the construction of scientific representations. Earlier I mentioned that Weisberg (2013) introduces the idea that an analysis of the problem of representation should consider judgments that scientists actually make in practice, with special emphasis on judgments of similarity. In doing so, Weisberg (2013) situates his work in contrast to other contemporary literature that "has not fully explored the role of theorists' intentions in all aspects of modeling, including the individuation of models, the coordination of models to real-world systems, and the evaluation of the goodness of fit between models and the world" (ibid.: 5). His central aim is to develop, in response to this literature, a comprehensive, practice-based account of scientific representation that considers all these aspects of modelling. In Chapter 4 I will refer to some of the particularities of Weisberg's (2013) *weighted feature-matching account of similarity* when it is applied to the analysis of particular modelling practices. For now, I consider the role that judgments of similarity play in his proposal.

Specific examples like Schelling's model of segregation, the Lotka-Volterra model, and the San Francisco Bay Model help Weisberg (2013) argue for the importance of judgments of similarity when approaching the problem of representation. He claims:

> I think that an analysis of the model–world relationship should reflect judgments that scientists can actually make, as opposed to asserting that the relation holds between inaccessible, hidden features of models and targets. [...] In many cases theorists won't necessarily articulate the

> grounds for the judgments of similarity – the judgments are just made. Nevertheless, when it matters, such as in cases of disagreement, theorists should be able to work out the grounds for their similarity judgment. (Weisberg 2013: 138)

Phenomena have myriad properties, and modellers are not interested in studying the total states of phenomena, but rather some considered important subset of those properties related to the goal of the research (Weisberg 2013: 91-2). Judgments of similarity are, for Weisberg (2013), the way in which epistemic agents articulate the relevant elements of the model-world relation in particular representational practices. This idea stands in direct opposition to the assumption that similarity is an "inaccessible", "hidden" relation between a model and a target (ibid.: 138). Even if Weisberg formalizes similarity as the sharing of features by vehicle and target of the representation, he makes explicit that those features are selected and weighted by the epistemic agents, not fixed before the representational activity. Moreover, judgments of similarity have to be "cognitively accessible" (ibid.: 301). So even when scientists do not reveal the reasons for their judgments, if disagreements arise it should be possible to track their grounds back to the practice of representing.

I believe this last point is a noteworthy claim that we can expand further. Disagreements in science are made manifest through the articulation of opposed judgments in practices of representing. Sometimes the origin of a disagreement might be the existence of other competing judgments of similarity about an aspect of the representation. This idea is relevant for an account of the means of representation because exploring why some judgments acquire prevalence before others carries a good deal of information about the epistemic success (or failure) of particular models. If we for instance observe that certain judgments of similarity become entrenched and give rise to rules or standards, we have good reasons to believe that those judgments played a relevant role in the epistemic success of the model obtained. If we observe that a judgment of similarity relates to a particular action in the construction of a model that turns out to be irrelevant or counterproductive for the result, we should probably consider that particular judgment of similarity inadequate for the goals pursued in the practice. The cases of the San Francisco Bay Model and the Mississippi Basin Model, which I will describe in Chapter 4, illustrate this point.

I take one of the main contributions of Weisberg's *Simulation and Similarity* (2013) to be the general thought that a debate on the role of similarity in scientific representation should seriously consider judgments of similarity that scientists make. There are nonetheless two controversies around the use of the notion of judgments of similarity in his account. The first is that it is not completely clear what role judgments of similarity are playing in his account, as it is not completely clear what type of account he is proposing. Parker (2015: 271) has advanced this type of criticism. She points out that it is not obvious which of the three kinds of accounts Weisberg (2013) is advancing: (1) an account of successful representation; (2) an account of the general conditions for representation; (3) an account of what underlies scientists' judgments concerning the extent to which models and targets are similar to each other. Parker (2015) seems to imply that considering the role of judgments of similarity would tie Weisberg to the type of account in (3), while he claims to be offering an account of (1) and (2) as well (2015: 300-1).

I believe Parker (2015) is well justified in questioning the conflation of (1) and (2) in Weisberg's account. I argued in the previous chapters that, while an account of (2) (equivalent to what I called *R1* or the question about the constituents of representation) should not be based on similarity, an account of (1) (equivalent to an account of the means of representation) could be in principle focused on similarity, even if perhaps not conceiving similarity as the only, exclusive means of representation. Although Weisberg (2015: 300) insists that he is addressing the two issues, I take his account to be more comprehensive if understood as dealing with the problem in (1), the epistemic success of scientific representations. Concerning the role of judgments of similarity, I have defended at various points throughout this thesis that the types of accounts (1) and (3) do not need to be incompatible with each other, but can and should be complementary. That is to say, an account of successful representation would highly benefit from being supported on a practical enquiry that studies the uses of judgments in scientific practices. Thus, I do not believe that conflating (1) and (3) is a shortcoming of Weisberg's (2013) account, as Parker (2015) seems to claim, but a fruitful move to approach the problem of the means of representation from a practice-based perspective.

The second closely related controversy around Weisberg's (2013) use of the notion of judgments of similarity concerns the psychological connotations that the term "judgments" has. This point has been also presented by Parker (2015), in close relation to the previous criticism, and in slightly different terms by Sterrett (2006, 2009, 2017b). Parker (2015: 271) argues that trying to make sense of what underlies scientists' judgments when they evaluate models as being more or less similar to their targets is a valid psychological type of endeavour, but not what an account of representation is meant to be. Weisberg (2013) indeed takes the work in cognitive psychology that Tversky (1977) developed in the 1970s as a starting point for the formulation of his proposal.[27] But he disagrees with Parker's (2015) observation in the following way:

> Bring these psychological ideas to my account might look like a confusion, but I think it makes sense [...as] even the most strictly metaphysical parts of my account have a substantial pragmatic element. This is because on my view, the relation between a model and a target depends, in part, on the scientific context (Weisberg 2015: 300-301).

This reply makes evident Weisberg's commitment to some degree or another with a practice-based approach to the problem of scientific representation. An account in naturalized philosophy of science would aim to incorporate insights from fields like psychology, sociology, and biology to illuminate problems traditionally addressed in philosophical accounts. Adopting a "psychologized" version of the idea of similarity, through consideration of scientists' judgments, can help support a philosophical proposal on the problem of representation. Parker (2015: 271) seems to be concerned about the elimination of the normative component of a philosophical account on representation if the same account tries to "make sense of what underlies scientists' judgments when they evaluate models as being more or less similar to their target". I have argued though, endorsing the rationale of PSP and iHPS, that the systematic study of judgments in scientific practice is a tool to reconcile, in a single account, the description of practices and a normative view of the problem of scientific representation.

At any rate, the criticism presented by Parker (2015) remains fairly valid, in that Weisberg (2013) never clarifies how exactly the study of what underlies scientists' judgments helps sustain his account of successful representation. There is for instance no reference to the important methodological point that the study of judgments of similarity should be the empirical source of the normative component of an account of successful representation, given that agents already stabilize uses and define norms concerning similarity within the practice of representing. The characterization of the notion of judgments I have offered in this

[27] Also, Weisberg (2013: 144-5) follows other works in contemporary psychology, such as Goldstone, Medin, and Gentner (1991), to motivate aspects of his *weighted feature-matching account of similarity*, such as the distinction between the *attributes* and the *mechanisms* of a model.

chapter can help unpack the motivations that seem to underlie Weisberg's (2013) recent work when he refers to the importance of judgments of similarity, but which are far from fleshed out in his account.

At this point we can add the criticism offered by Sterrett (2006, 2009, 2017b). She argues that the "psychologized" notion of judgments of similarity that Weisberg (2013) uses renders the concept of similarity subjective and dependent on scientists' personal opinions.[28] If we adopt Weisberg's (2013) notion of judgments of similarity, what remains is a plurality of different, unreliable definitions of similarity that scientists and engineers could capriciously apply. This conception of similarity contrasts with the work that Sterrett (2002, 2006, 2009, 2017b) herself has developed for years, where she studies the rigorous, mathematically-founded uses of similarity in scientific research. Her work examines practices of modelling in engineering and the physical sciences, where the methods of physical similarity have been established since at least the beginning of the twentieth century (2009: 5). It is indeed crucial to recognize that in specific modelling practices, such as the construction of scale models in engineering, the term similarity has precise definitions, in the form of geometric similarity, dynamic similarity, and methodologies of dimensional analysis. These definitions might be adapted to fit individual practices, but they can usually be transferred from practice to practice, and even from field to field, as it is the case when principles of dynamic similarity circulate from the engineering sciences to the geosciences (2017b). Against the argument from vagueness discussed in Chapter 2 that similarity is an empty and utterly vague notion, we should contend with Sterrett (2017a, 2017b) that, when applied in actual practices, similarity has explicit, unambiguous meanings. Furthermore, we should also contend that philosophers of science ought to pay more attention to the history of the methods of physical similarity, since the notion of similarity they introduce in their accounts of representation should not be at odds with the uses of the term in modelling practices (2009: 6-7).

Nevertheless, I believe that it does not follow from Sterrett's arguments that we should eliminate the notion of judgments of similarity from the discussion. I started claiming that both Weisberg (2013) and Sterrett (2009, 2017b) have advanced the most noteworthy proposals in recent philosophy of science regarding

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[28] Thanks to Susan Sterrett for pointing out this argument against Weisberg's (2013) notion of "judgments of similarity" in private correspondence, 8 September 2017. Cited with the author's permission.

the role of judgments of similarity in representational practices. But then, I argued that Sterrett (2017) emphatically rejects the notion of judgments of similarity for its subjectivity, as I believe that Sterrett's (2009, 2017b) work is compatible with, and even reinforces, the characterization of judgments of similarity that I have tried to offer so far. There are two reasons for this. One, the concept of judgment does not necessarily have those individualistic, subjective implications that Sterrett claims. There is agreement in epistemology that judgments are not mere opinions, but abilities that at least in part are regulated by norms and the agreement of the community. If we endorse the characterization of judgments in scientific practice sketched in Section 3.2, the problems with the aspects of subjectivity infiltrating the idea judgments of similarity largely vanish. Two, the type of proposal that Sterrett (2009, 2017b) has advanced is an exploration of the functioning, historical consolidation, and disagreements around a fundamental set of judgments of similarity employed in science. Her work has contributed to make the processes of standardization of the notion of similarity visible to philosophers of science. For that reason, I believe that this type of empirical and analytic work, in combination with other possible work done in fields where judgments of similarity have different standardized meanings, is fundamental to advance an informed account of epistemically successful modelling practices focused on similarity as means of representation.

3.3.2. Some examples

There is a variety of scientific practices in which judgments of similarity concerning model-target comparisons have an important presence, since epistemic agents employ them during the phases of design, construction, and evaluation of models. This should be, I have argued, an element in support of the advancement of an account of the means of representation focused on similarity. Philosophers of science have occasionally discussed case studies of modelling practices, making implicit or explicit references to the role that similarity plays in them. However, these works in philosophy of science are diverse, seldom in dialogue with each other, and not always directly concerning the problem of representation. By bringing some of them together here, I try to show that there is acknowledgement in philosophy of science of the presence and role of judgments of similarity in multiple scientific fields and cases. By illustrating how these works can be in dialogue with each other more explicitly, I also show that they could influence the general debate on representation positively, by pointing to the possibility of a common, more encompassing account of the means of representation focused on similarity. I briefly refer to the accounts of Quine (1969), Bengoetxea et al. (2014), Morgan (2012), Ankeny and Leonelli (2011), and Ankeny et al. (2014), among others.

Even Quine (1969: 117-135), who was an open detractor of the concept of similarity, along the same lines as Goodman (1968), recognized that scientists make constant judgments of similarity in the context of scientific research. He acknowledged that judgments of similarity are in practice crucial for learning, inductive generalization, and prediction, even if they do not involve objective measures of similarity but greatly depend on scientists' theories (1969: 133-135). More specifically, Quine (1969: 121) postulated the existence of epistemic paths of scientific research, divided into phases, in all of which judgments of similarity would play a role some way or another, depending on the characteristics of the phase, and generally retaining some similarity standards along the whole process. This idea seems so far compatible with the study of judgments of similarity in scientific practice that I am discussing. However, the epistemic paths Quine describes are supposed to finish in the dissolution of all judgments of similarity (or all judgments really) as they would be substituted by formally sound notions when disciplines achieve maturity (ibid.: 121. In Bengoetxea et al. 2014: 219). In response to this idea, we could argue that, in mature disciplines, judgments of similarity seem to be usually employed. It would be inappropriate to "categorically state that scientists have to refrain from judgments of similarity" if they prove to be valuable in their research (Bengoetxea et al. 2014: 224). In organic chemistry, Bengoetxea et al. (2014) have for instance examined the uses of judgments of similarity and concluded that they are an integral part of representational practices in various ways and at different stages. Rouvray (1992) enumerates some of the ways in which similarity is fundamental in research in chemistry (in Figure 2), and Bengoetxea et al. (2014) discuss various functions of similarity in modelling in toxicology and synthetic chemistry:

> [E]xamples from chemistry and toxicology demonstrate how the similarity notion is used in scientific practice and why in many cases such practice would in fact be difficult to imagine without scientists' recurring to similarity. […] The first example, from synthetic chemistry, shows that

the scientific analysis of organic molecules, as well as the generation of new molecules invariably recurs to similarity judgments about functional groups. The second example, from regulatory science [i.e. toxicology], demonstrates that the notion of similarity and its application is also crucially relevant in scientific practices oriented towards the taking of regulatory decisions that have direct consequences for people's lives. (Bengoetxea et al. 2014: 226)

Judgments of similarity are used in synthetic chemistry to analyse molecules, aiding scientists to satisfactorily represent their properties and chemical relations, while in toxicology judgments of similarity help assess whether different chemical substances are harmful or innocuous by comparing their biochemical activity (Bengoetxea et al. 2014: 225-9). If judgments of similarity can help scientists orient complex practices of representing, there are good reasons to think that they are worthy of serious philosophical analysis.

Table II. Listing of Many Applications of Similarity Concepts in the Chemical Domain

author(s)	year	application of similarity concepts
Richter	1793	chemical equivalence and stoichiometry
Proust	1804	law of constant proportions
Dalton	1808	similarity of atoms
Gay-Lussac	1808	combining volumes of gases
Berzelius	1810	law of multiple proportions
Avogadro	1811	Avogadro's Law
Mitscherlich	1811	isomorphism in crystals
Petit and Dulong	1819	law on specific heats
Wöhler	1828	behavior of bioactive molecules
Berzelius	1830	concept of isomerism
Dumas	1839	similarity of inorganic and organic species
Berzelius	1840	phenomenon of allotropy
Gerhardt	1845	concept of homologous series
Hofmann	1849	theory of molecular types
Mendeleev	1869	periodic classification of the elements
Le Bel and van't Hoff	1874	concept of stereoisomerism
Fischer	1894	complementarity of drug and receptor
Soddy	1913	concept of isotopy
Ruzicka	1921	isoprene rule for polyterpenes
Franck	1925	Franck–Condon Principle
Hückel	1934	principle of minimum structural change
Rice and Teller	1938	Rice–Teller Principle
Fox	1953	molecular sequence comparison studies
Hammond	1955	transition-state postulate
Hansch and Fujita	1964	structure–activity correlations
Woodward and Hoffmann	1964	Woodward–Hoffmann rules
Hine	1977	principle of least nuclear motion
Ugi et al.	1980	principle of minimum chemical distance
Carbó et al.	1980	molecular charge similarity measure
Mezey	1985	shape group molecular similarity measure

Figure 2. Applications of similarity concepts in the chemical domain. Extracted from Rouvray (1992: 581)

There are also works in philosophy of science that have emphasized the importance of using judgments of similarity in the process of model-making in economics. Morgan (2012) has, for instance, pointed out the importance of subjects' cognitive and practical abilities to recognize and exploit what they believe are relevant similarities between a model and a target. She offers the examples of Fisher's (1911) model of exchange between money and goods:

> Model-making, or giving form to a model, depends upon our cognitive abilities to recognise similarities and our creativity in exploring those similarities. Scientists choose models on the basis of similarities seen in the form, structure, content or properties between two fields and investigate these similarities in a systematic way. For example, Fisher (1911) chose a mechanical balance as a model for his economic "equation of exchange" between money and goods because he recognised the similarity between the elements and their relations (Morgan 2012: 23).

Scientists can decide to pursue an investigation on certain type of model on the basis of the effectiveness of the judgments of similarity they can make about it in comparison with the target to be represented. Fisher (1911) exploited the consequences of making judgments of similarity concerning the design of a visual model of economic exchange that had the appearance of a mechanical balance (see Figure 3; in Morgan 2012: 22). This mechanical balance weighing loaves of bread was an everyday object that came readily to hand for Fisher (1911), and was judged to be relevantly similar in some key respects to a complex, difficult-to-grasp phenomenon like economic exchange. Another good example of practice of representing in economics that Morgan (2012) mentions is the construction of Phillips's (1954) hydraulic model of a Keynesian economy. There were decisive judgments of similarity involved in Phillips's activity of designing the hydraulic model through consecutive sketches (like the early sketch shown in Figure 3; in Morgan 2012: 22), and later on, in the construction of several physical models. Phillips (1954) himself acknowledges the use of central judgments of similarity in the process of designing and building the model:

> There are certain formal similarities between the problems of devising policies for economic stabilisation and those of designing automatic control systems in engineering. [...] Methods have recently been developed by engineers for analysing the dynamic properties of quite complex models [...] These methods and procedures can also be used for the analysis of dynamic process models in economics. (Phillips 1954 [2000]: 184)

The judgments of similarity mentioned here by Phillips (1954) concern the relevant functional, formal similarities between two activities, the activity of "devising policies for economic stabilisation" and the activity of "designing automatic control systems in engineering". From there, Phillips (1954) resolves that the use of the method employed in the design of an engineering system might bring fruitful results if employed in the study of economic stabilisation. The method in question is the construction of a physical hydraulic model, and the object resulting from the process was a plumbing system with valves, tanks, and water flows representing the dynamics of market demands. Phillips's model was conceived and built thanks in great part to the help of the formulation of judgments of similarity. The result was a model that allowed him to draw successful inferences from the specific behaviours of the hydraulic device (like the transfer of water from one tank to another, the closing and opening of valves, etc.) about specific properties of an economy system (like the income of foreign purchases into a central bank, the interruption of exports in a country, etc.) (see also Frigg and Nguyen 2017a; Morgan 2012; Vines 2000). We could claim, following Vines's (2000) historical reconstruction of the case, that the use of judgments of similarities was decisive for the design of a model that represented an economic system "*vividly*", in a way that was "immensely visible" (2000: 58). This allowed scientists to reason about money transactions at the same time that they were looking at water tanks rise (ibid.: 46-9), which triggered the exploration of new aspects of the target system investigated and stimulated further thoughts and conjectures about the functioning of economic systems.

This observation can cast doubt on some recent accounts in philosophy of science that have strongly rejected the involvement of similarity in the achievement of successful representations, such as Frigg and Nguyen (2017b), who use the case of the Phillips-Newlyn machine in support of their idea. Frigg and Nguyen (2017b) argue that their DEKI account, which stands for Denotation-Exemplification-Keying-Imputation, captures what the necessary conditions for representation are. Among them, the "keys", which are codes that define how to translate properties of the model into properties of the target, are crucial to determine whether we would obtain successful representations (2017b: 49-51). Although Frigg and Nguyen (2017b) eventually acknowledge that similarity might be one of the many ways in which the "keys" could be specified, they would not recognise the importance of formulating judgments of similarity in the process of designing and constructing models like the Phillips-Newlyn machine, despite the explicit references by Phillips in the quote above. Moreover, they argue that "introducing keys does not amount to smuggling in a mimetic conception of representation via the back door. On the contrary, keys can be as conventional as they like" (Nguyen 2016: 173).

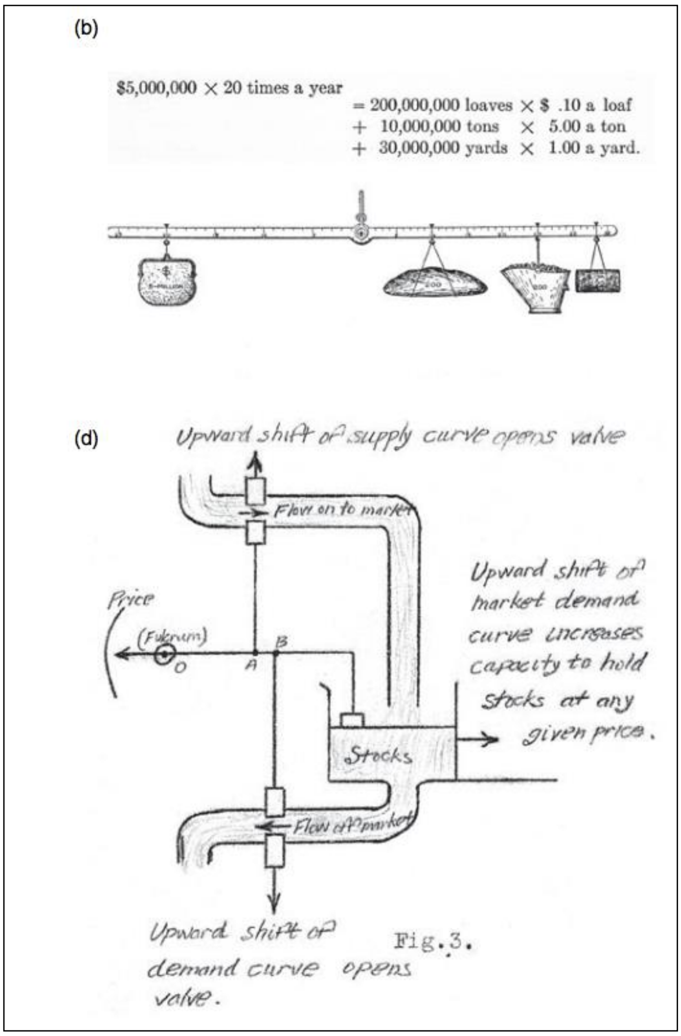

Figure 3. Fisher's Arithmetical and Mechanical Monetary Balance; and Phillips's Plumbing Diagram. Extracted from Morgan (2012: 22).

Moving to a different field, research with model organisms and animal-based models is one of the areas where the use of judgments of similarity in representation has been more widely discussed. Ankeny and Leonelli (2011) define "model organisms" as non-human species that are studied in the hope that data and theories generated through their use will be applicable to other organisms (2011: 313). Common examples of model organisms are mice, rats, zebrafish, fruitflies, nematode worms, and thale cress. Of the assumptions underlying research with model organisms for the investigation of the human genome, it is claimed: "many genetic and biological similarities exist between various model organisms and humans; the model organisms will provide information that will aid in the interpretation of human genomic sequences" (Ankeny 2001: 255). It is important to stress that the reference to similarities in this context does not concern pre-existent, objective measures of similarity between humans and model organisms. The issue at stake is the use scientists make of judgments of similarity concerning model-target relations for the goals pursued in the practice. For instance, scientists would perform certain judgments of similarity when comparing the genome of single-celled model organisms and the human genome, which might bring positive results at the end of the practice of representing in the form of fruitful inferences. But the huge difference in complexity between single-celled organisms and humans might in other circumstances not allow scientists to obtain particularly fruitful results at the end of the practice (Ankeny 2001: 258-9). Community agreement about what the relevant formulations of judgments of similarities are for the case at hand are without doubt fundamental, although that doesn't prevent more than one judgment of similarity existing and disagreeing with others in the same representational practice. Disagreements between different judgments of similarity are not a reason to discard their epistemic significance in representation, but an additional motivation to argue that reaching agreements about them is important for the achievement of the epistemic goals of the community of researchers.

The last point connects with an aspect of judgments of similarity that should be emphasized: relevant judgments of similarity exist in scientific practice in parallel to the formulation of relevant judgments of dissimilarity and distortion. Good illustrations of this can be found in Huber (2015),[29] and Huber and Keuck (2013: 389) about research on the complex symptoms of Alzheimer's Disease; in Nelson (2012) about research on anxiety; and in Ankeny et al. (2014) about research on alcohol addiction. In all these cases, mice are used as animal-based models. The case of research on human alcoholism described by Ankeny et al. (2014), for instance, starts from the idea that "the similarity between addiction

[29] From Huber (2015). Talk "From Mice to Men: Homogeneity, Similarity and Relevance in Model-Based Reasoning" at the SPSP Conference, Aarhus (Denmark), June 2015.

behaviours in humans and in non-humans is particularly difficult to assess" (ibid.: 489). Precisely for that reason, the weighting of different judgments of similarity and dissimilarity is fundamental in the process of deciding where to focus the attention of the modelling practice. The most striking question that this research has to face is: how can mice be adequate models of human alcoholism if they are reluctant to drink alcohol? One of the possible responses to this question has consisted in making judgments of similarity concerning the biological reactions that alcohol respectively provokes in humans and mice. If those reactions are judged relevantly similar to each other in important respects, then the aspect of investigation that needs to be further explored is the biological response of alcohol in the target and in the model (mice). This would also imply that it doesn't matter if mice don't like drinking alcohol, because they would be artificially induced and their biological response to alcohol analysed (ibid.: 495). Another possible response to the question has consisted in making judgments of similarity about the behaviour of humans and mice. If scientists agree that the behaviour is the key of the study of alcoholism, and that artificially induced mice would behave too dissimilarly from mice that voluntarily consume alcohol via oral self-administration, then the modelling should focus on the comparison of human behaviour and mice that voluntarily drink alcohol (see Cicero 1980; in Ankeny et al. 2014: 495). A third possible response to the question has consisted in introducing judgments of similarity about the alcohol that humans and mice consume. In order to make mice self-administer alcohol, something has to be added to the alcohol – sugar usually – to attract them to consume it. In this case, scientists might judge that there is a risk of finding a prohibitively strong dissimilarity between the substances consumed respectively by humans and mice, therefore generating behaviours of addiction that are not considered relevantly similar to those of human addiction (see dispute between Gauvin et al. (1993) and Cicero (1980); in Ankeny et al. 2014: 494-5).

The conclusion that Ankeny et al. (2014) reach from the complex case study of alcoholism is that representing is a fallible practice. So the question as to whether animal-based models "can reliably stand for human beings, and whether the experimental setting bears any relation to the social situations in which a putatively *similar* human behavior would be instantiated, remain open-ended" (ibid.: 501). Still, the practice of modelling alcohol addiction with mice has become more homogenous and consistent over time. Research communities reach some (temporary) agreements about the most fruitful ways of studying a phenomenon like this, standardizing parameters for future research. And this is, importantly, due to the exploitation of various judgments of similarity and dissimilarities concerning the models and targets of the representation.

Additional cases of scientific practices that philosophers of science have described, pointing out the role of judgments of similarity, are, for instance: Eigner (2009), who focuses on the history of psychology and refers to how scientists make judgments about relevant similarities supported by practical skills and previous theoretical knowledge; Parker (2009), who highlights the importance of relevant similarity judgments in the inferences scientists make from computer simulations of meteorological phenomena; Steel (2010), who uses examples like the study of the carcinogen aflatoxin B1 to refer to scientists' similarity judgments, specifically in terms of mechanisms and causes involved in models and targets; and Downer (2007), who studies the similarity judgments engineers need to make to design airplane engines that adequately pass "birdstrike tests", that is, tests in which birds (or artificial birds, rubber gelatine, or other materials considered relevantly similar to birds in different aspects) are launched into the engines to assess whether they will explode or not.

After mentioning a range of examples, I hope to have made a reasonable case for the manifest presence of judgments of similarity in a broad variety of scientific practices. Putting together some of these works helps make more explicit the commonalities between them. The debate on scientific representation would benefit from being directly informed by works that analyse the role of judgments in concrete scientific practices like these. I have mostly referred to examples where the construction of physical models is involved (hydraulic machines, animal-based models), but it may be possible to explore the presence of judgments of similarity in practices that involve constructing diagrammatic, mathematical, or computational models too. Probably in those cases we would find judgments of similarity concerning the formal properties of models in comparison to the formal properties of the targets represented (as argued by Parker 2009: 493). Lastly, I hope to have shown that disagreements between various judgments of similarity within a practice, or the fact that judgments of similarity go together with judgments of dissimilarity and distortion, should not prevent us from thinking that they are important resources for the achievement of fruitful representations. The presence of disagreements precisely corroborates that finding a solution to them, in the form of an agreed relevant combination of similarities and distortions, is essential for the progress of the practice of representing. Some judgments of similarity might prove to be ineffective or carry negative consequences for the construction of a model. But by then, we would have learnt which paths not to pursue: we abandon the use of those judgments, and exploit others that correct the negative consequences of the previous ones. Representational practices are processes of trial and error, where norms, skills, judgments, and values are in dynamic interaction with each other.

3.4. Conclusions

In this chapter I have argued that the study of judgments of similarity in representational practices is crucial to develop an account of the means of representation. An account of the means of representation requires a practical type of enquiry that allows us to observe how epistemic agents make successful inferences from models about the targets of the representation (Suárez 2003). The rationale of the projects of PSP (Philosophy of Science in Practice) and iHPS (integrated History and Philosophy of Science) offer an adequate methodological framework to develop the type of practical enquiry that an account of the means of representation requires. I referred to three particular methodological lessons to be incorporated from PSP and iHPS. One, an account of the means of representation should mainly focus on the study of science-in-practice, that is, the *activities* and *resources* involved in the production of successful representations, instead of on the final products obtained at the end of them. Two, an account of the means of representation should incorporate both a descriptive and a normative component. The analysis of judgments in scientific practice is the methodological tool that facilitates the integration of the two components, given that the consolidation of norms derived from the use of certain judgments should be the source of normativity of a philosophical account. Three, the advancement of an account of the means of representation doesn't strictly proceed bottom-up or top-down, but is an interpretive endeavour by which general claims are adjusted to particular cases in the same way as cases are shaped by general claims.

Afterwards, I briefly discussed the notion of "judgments in scientific practice", and more specifically the implications of incorporating the study of judgments of similarity into an account of the means of representation. From the debates in epistemology and psychology, I resolved that judgments in scientific practice are not mere opinions, but commitments regulated by standards and the agreements of the competent epistemic community. Judgments are fallible, and do not work as truth-preserving algorithms, but their importance in epistemic practices is demonstrated by their usefulness at contrasting, testing, calibrating, and correcting models in comparison to their targets. The specific role of judgments of similarity in practices of modelling has been addressed by Weisberg (2013) and Sterrett (2009, 2017a, 2017b) in recent philosophy of science. Their respective accounts are well motivated, but could be further developed as to explicitly address the issue of how to integrate descriptive and normative components in an account of the means of representation that studies judgments of similarity. I concluded by mentioning a series of examples from various scientific disciplines in which judgments of similarity concerning model-target comparisons visibly participate in the construction of epistemically successful representations. The debate on the means of representation demands to be directly informed by the study of examples and cases like these. The next chapter is an attempt to describe some historical case studies regarding the construction of scale models in engineering. I will particularly explore the connections between the uses of judgments of similarity and the epistemic success of the models. The conclusions obtained will motivate the outlining of a particular account of the means of representation in Chapter 5.

Chapter 4

Not only size matters: Scale models and judgments of similarity

In this chapter I develop a case study in support of the main arguments presented so far in the thesis, and more specifically in Chapter 3. I use the cases of the construction of the Mississippi Basin Model (1943) and the San Francisco Bay Model (1953) to illustrate how judgments of similarity pervasively and effectively intervene in practices of modelling. I concluded Chapter 3 by arguing that judgments of similarity (and dissimilarity) are present in numerous practices of representing, performing an epistemic role in the construction of successful models. Now I draw on some primary sources (reports, interview, technical manuals) to localize the various uses of judgments of similarity in the design, construction, and testing of the San Francisco Bay Model (SFBM) and the Mississippi Basin Model (MBM). From there I hope to show that a satisfactory account of the means of representation should emerge from the study of the actual representational resources employed in practices of representing. Later, in Chapter 5, I will more specifically argue that the notion of "creative similarity" is suitable to support an account of the means of representation, as it captures the generative interplay of judgments of similarity and distortions that takes place in practices of representing. The case studies in the present chapter help support that account.

The fact that I have selected two scale models in engineering as case studies is not accidental. Contemporary literature in philosophy of science has frequently undervalued the epistemic role of scale models, and linked it to the erroneous assumption that scale models are superficially and naïvely similar to their targets. The cases of the SFBM and the MBM examined here help to show that scale models can have an extraordinary epistemic value, and that the judgments of similarity that scientists formulate in the process of constructing such models do not involve merely superficial or apparent similarities. To argue for the role of judgments of similarity in the construction of successful scale models in engineering, I first need to defend the genuine value of scale models beyond illustrative functions. I start Section 4.1 by discussing some of the literature in philosophy of science concerning the epistemic value of scale models, as well as some historical debates within the engineering sciences on the functions of scale models for research. Then, I focus in Section 4.2 on the role of judgments of similarity in the cases of the MBM and the SFBM. I show that judgments of similarity were present in the construction of these models in three different forms, as "standardized judgments of similarity", "context-dependent judgments of similarity", and "intuitive judgments of similarity". In Section 4.3, I further characterize these judgments of similarity as being in constant interplay with distortions (idealizations, simplifications, abstractions). Lastly, in Section 4.4, I present a discussion on how the analysis of these case studies can inform specific aspects of the debate on representation in philosophy of science.

4.1. The epistemic value of scale models

Traditional accounts in philosophy of science have commonly attributed a low epistemic value to scale models, especially compared to how they have regarded mathematical and other highly idealized models. This idea might not be articulated in an explicit way very frequently, but it is manifest, among other things, in the little attention given to scale models in the literature on scientific modelling. Important exceptions to this are found in Sterrett (2002; 2006; 2009; 2017a), Pincock (2012), Weisberg (2013), Zwart (2009), and Parker (2009). Scale models have been usually recognized as one type among others of "material", "concrete" or "physical" models[30], but not many works were devoted to arguing for the

[30] I use the terms "concrete model", "physical model", and "material model" indistinctly. In the field of hydraulic engineering, to which I mostly refer to in the chapter, "physical model" is the term commonly used to refer to "scale models" (in contrast to numerical models and computer simulations). Except in the cases where I quote original work in hydraulic engineering, I use the term "scale model" to refer to a particular type of physical model that involves explicit processes of scaling.

possibilities of prediction and understanding they could afford until recently (Sterrett 2017a: 859-60). Moreover, when the value of scale models is recognized, it is often defined as *illustrative* of the physical phenomena represented, or *pedagogical*, in a similar way to toys and collectors' miniatures. A classic definition of scale models in these terms was offered by Max Black (1962), who said that good examples of scale models were "the ship displayed in the showcase of a travel agency, the airplane that emerges from a small boy's construction kit, the Stone Age village in the museum of natural history" (Black 1962: 219). A more recent example is found in the entry of the SEP for "Models in Science", where the authors mention "wooden cars" and other "down-sized or enlarged copies of their target" as typical cases of scale models (Frigg & Hartmann 2018). The problem with these definitions is that, even without underplaying the pedagogical value that miniatures and toys might have, they presume a very narrow conception of the practice of scale modelling in science and engineering.

Susan Sterrett (2002; 2006; 2009; 2017a) has first highlighted that philosophers of science have underestimated the role that scale models play and have played in modern science (2017a: 860). Not only have scale models been treated as mere didactic tools, but also architectural models have been frequently taken as a paradigmatic case of scale model (ibid.: 859a). The association with architectural layouts interferes with the understanding of how broadly the concept of scale model applies.

> There are of course some rather straightforward uses of scale modeling, such as the use of geometrical scale models in the architectural layout process, where the only purpose of the model is to represent spatial relationships. These are such special cases of scale modeling that they hide the extremely interesting and sophisticated methods involved in more general cases of scale modeling, in which fluid phenomena, mechanical phenomena that depend upon stress-strain or other material properties, and heat transfer phenomena are modeled by small objects whose behavior can be used to predict the behavior of larger machines and situations. (Sterrett 2002: 57)

The practice of scale modelling does not mean "simply built to the same geometrical proportions"; other quantities can be scaled too. There are many good historical examples of the use of the methodologies of scale modelling in engineering and geological sciences. Hydraulic physical models have been constructed from the times of Leonardo da Vinci and his experiments with currents for the canalisation of the River Arno (around 1509)[31] to Galileo's tests with small vessels in water tanks around 1612,[32] Froude's experiments with small scale ships representing the H.M.S "Greyhound" in 1874,[33] Monsieur Fague's innovative river models in France in 1875,[34] Reynolds's tidal model of the Mersey estuary near Liverpool in 1885,[35] up to the twentieth century, when modern techniques of scale modelling were consolidated. Sterrett (2017a: 860-1) mentions the physical model of Lake Superior in the 1970s as a fruitful contemporary case (Lien & Hoopes 1978)[36], and I will describe the well-known cases of the Mississippi Basin Model (MBM) (1943) and the San Francisco Bay Model (SFBM) (1953) throughout this chapter.

Geology has been another field in which physical models have been employed for the purpose of prediction. For example, Koeningsberger & Morath (1913) and Hubbert (1937) applied the method of dimensional analysis to their models of tectonic structures; and the so-called "sandbox experiments", which involve the deformation of piles of layered materials representing sedimentary rock, have been in use since the late nineteenth century (Cadell 1888) until very recently (McClay et al. 1991; Souloumiac et al. 2012).[37] In volcanology and other subfields of geology that concern risk assessment, physical models have been particularly beneficial. Merle & Borgia (1996), Renschler (2005), Maceda et al. (2009), Norini & Acocella (2011), and Gabuchian et al. (2014) are examples of

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[31] In Rouse and Ince (1963: 44-49). See more in: da Vinci, Leonardo (1924) *Del Moto e Misura dell'Acqua*, edited by Carusi, E., and Favaro, A. Bologna, 1924; and Duhem, P. (1913) *Etudes sure Léonard de Vinci*, Ser. 1, 2, and 3, Paris, 1906, 1909, and 1913.

[32] Van Fraassen (2008: 50-51). See more in: Galilei, G. (1974) *Two New Sciences*. Tr. Stillman Drake. Milwaukee: University of Wisconsin Press; and Galilei, G. (2005) *Discourse on Bodies in Water*. Tr. Thomas Salusbury. New York: Dover Publications.

[33] Rouse and Ince (1963: 182-187). See more in: Froude, W. (1874) "On Experiments with H.M.S. Greyhound". *Transactions of the Institution of Naval Architects*, Vol. 16, 1874.

[34] Price (1978: 26).

[35] Rouse and Ince (1963: 182, 206-212). See more in: Reynolds, O. (1888) "On Certain Laws Relating to the Regimen of Rivers and Estuaries, and on the Possibility of Experiments on a Small Scale". *Third International Navigation Congress*, 1888.

[36] Lien, S. L., and Hoopes, J. A (1978) "Wind-driven, steady flows in Lake Superior" *Limnology and Oceanography* 23, 91-103.

[37] See: Koenigsberger & Morath (1913) "Theoretische Grundlagen der experimentellen Tektonik". *Zeitschr. Deutsch. Geol. Gesellsch.* Monatshefte, Vol 65, 1913, pp. 65-86; Hubbert, M. K. (1937) "Theory of scale models as applied to the study of geologic structures", *The Geological Society of American Bulletin*, 48, pp. 1459 – 1520; Cadell, H. (1888) "Experimental researches in Mountain building". *Transactions of the Royal Society of Edinburgh*, 35, 337–357; McClay, K., et al. (1991) "Physical and seismic modelling of listric normal fault geometries". *Geological Society, London, Special Publications* 1991; v. 56; p. 231-239; Souloumiac, P. et al. (2012) "Bias due to side wall friction in sand box experiments". *Journal of Structural Geology* 35 (2012) 90-101.

recent constructions of scale models complementarily (or alternatively) to the use of computer simulations.[38] Even in physics, Sterrett (2017a) mentions Unruh's (1981) scale model to study black holes in general relativity (ibid.: 870).[39]

Sterrett's criticism of philosophers of science for disregarding the importance of scale models as in the aforementioned examples is fully justified. However, there is some recent literature on the topic that should not be overlooked. Philosophers of science like Winsberg (2009) and Guala (2002) have discussed the epistemic value of scale models in experimentation with laboratory water tanks. Oreskes (2007), Schaffer (2004), and Bokulich and Oreskes (2017) have offered remarkable historical insights about the various uses of scale models in geosciences and civil engineering. And Weisberg (2013), Pincock (2012), and van Fraassen (2008) have thoroughly engaged with cases of scale modelling to advance their accounts of scientific representation.[40] Furthermore, it is worth noting that the lack of serious consideration to scale models is not something exclusive to philosophers of science. Disagreements about the epistemic value of scale models have actually occurred within the engineering sciences first. We can for instance find disputes in the history of twentieth-century engineering research about whether scale models are indispensable tools for prediction or only illustrative tools. The case of the construction of the MBM and the origins of the creation of the WES (Waterways Experimental Station) in the US illustrate well the disputes within the community of engineers about the functions of scale modes, which were ultimately resolved in favour of attributing a high value to scale models.

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[38] See: Merle, O. and Borgia, A. (1996) "Scaled experiments of volcanic spreading", *Journal of Geophys. Res. Solid Earth*, 101, 13805-13817; Renschler, C. S. (2005) "Scales and uncertainties in using models and GIS for volcano hazard prediction". *Journal of Volcanology and Geothermal Research*, 139, 73-87; Maceda et al. (2009) "Experimental use of Models in Disaster Risk Management". *Shima: The International Journal of Research into Island Cultures.* Volume 3, Number 1; Norini, G., and Acocella, V. (2011) "Analogue modeling of flank instability at Mount Etna: Understanding the driving factors", *J. Geophys. Res.*, 116, 1-21; Gabuchian et al. (2014) "Experimental investigation of strong ground motion due to thrust fault earthquakes", *J. of Geophys. Res. Solid Earth*, 119, 1316-1336.

[39] Unruh, W. G. (1981) "Experimental Black-Hole Evaporation?", Phys. Rev. Lett., 46, 1351-1353.

[40] Other philosophers of science who have addressed the methodologies of scale modelling, and whose work Sterrett acknowledges are: Mattingly and Warwick (2009); Zwart (2009); Rothbart (2004); Kroes (1989); and Layton (1992).

Figure 4. Section of the Mississippi Basin Model. US Army Corps of Engineers (1970)

Figure 5. Detail of the Mississippi Basin Model. US Army Corps of Engineers (1970).

Figure 6. Water tanks at the Mississippi Basin Model. US Army Corps of Engineers (1970).

The MBM is considered the largest small-scale model constructed in the world, occupying a surface of 200 acres near Clinton (Mississippi). It is intended to reproduce the entire drainage basin of the Mississippi River and its tributary system, from the Rocky Mountains to the Appalachian Mountains. The model had the appearance of a gigantic relief map with streams and floodplains moulded in concrete. It was built with the help of three thousand German and Italian prisoners of war during World War II, and was continued by civilian personnel of the US Army Corps of Engineers after the prisoners were transferred in 1946. It was completed in 1966, although individual sections were in operation by 1949 (Foster 1971: 1; Robinson 1992: 290). In light of the consequences of terrible floods along the Mississippi River for centuries, the model was expected to help improve the coordination of flood control operations.

After years of demands from engineers inside and outside the US Army Corps of Engineers, such as John R. Freeman in the twenties and General Reybold in the thirties, the plan for the construction of the ambitious MBM was finally approved in 1943 (Robinson 1992: 278; 281). This was an important year for the confirmation of the importance of small-scale in hydraulics within the Corps' civil works. Following the narration of M. C. Robinson (1992), historian for the US Army WES and the Mississippi River Commission,[41] the approval of the

[41] See biographical note about Robinson in Fowle (1992: 480); and about the "Michael Robinson Award" in the history of public works in his honour, see: Public Works History Society (Consulted April 2017): http://www2.apwa.net/About/SIG/PWHS/socact.asp#robinson

construction of the MBM was remarkable when one considers that, as late as 1930, many officers and civilian engineers at the Corps regarded models "as mere toys for youngsters of the profession" (Robinson 1992: 277).

> Most of the Corps was openly sceptical of the value of models and actually feared they posed a threat to traditional engineering practices based on field data and experience. Until the founding of WES (1929), rivers and harbours work was largely an empirical process and little attention was given to conducting model experiments using the fundamental principles of hydraulic similitude (Robinson 1992: 277).

Civil engineer John Freedman was a prominent American structural engineer, who toured Europe in 1913. He was impressed by the contributions of European hydraulic laboratories in technical universities, especially those in Germany (Brown 2013: 42). Upon his return, he began to campaign for the creation of a national hydraulics laboratory (WES). However, the Corps was in clear disagreement with Freedman's ideas. Their official position was clearly stated in a letter prepared by Secretary of War Dwight W. Davis in 1926 (Robinson 1992: 278-9):

> The art of river regulation and control has heretofore been developed principally by practical experience in the solution of problems on a large scale [...] Field experience in the solution of problems of this nature is undoubtedly of much greater value than laboratory experiments could possibly be, and the application of principles evolved in the laboratory to the solution of practical problems in the field must be difficult and uncertain... (Signed by Secretary of War Dwight W. Davis in 1926) (Robinson 1992: 278).

The argument is directed against laboratory experiments, as they are presented as incompatible with, and of less valuable than, field work on a large scale. A year later, Major General Edgar Jadwin, Chief of Engineers of the Corps at that moment, also wrote to Freeman:

> I do not believe that many of the larger problems involved in the work of the Corps of Engineers, as, for example, flood control on the Mississippi, can be solved in a laboratory (in Robinson 1992: 278).

More than substituting the empirical work traditionally done, the aim of a laboratory, of the kind Freeman had seen in German universities, was to build scale models that could reproduce the dynamics of, for instance, the Mississippi River and its reservoirs in a way that the data collected from the full-scale physical system could not allow. In a scale model, extreme conditions of currents and flows can be tested, and future plans of dams and barriers could be evaluated. In 1929, and after a terrible flood on the Mississippi River, the plan for the foundation of a Waterways Experimental Station (WES) was finally enabled. Nonetheless, Major General Lytle Brown, Chief of the Corps after Jadwin, still thought that the potential benefits of the laboratory were going to be largely tangential: "specific in character… not for general informative purposes" (in Robinson 1992: 2).

Years later, the scale models constructed by the WES proved to be extremely fruitful for prediction. This was demonstrated dramatically in April 1952, in a field for which the MBM was not originally planned: the forecasting of the progression of a flood in the Missouri River section (Robinson 1992: 291-2). During fifteen crucial days that month, constant communication was maintained between engineers at the WES (based in Vickburg, Mississippi) and the Missouri River Division. Information furnished by the Missouri River section of the MBM "was of incalculable value in aiding evacuations and supporting flood-fighting activities" (Robinson 1992: 292). The model anticipated the progression of the flood days before it happened in the Missouri River, which allowed the identification of critical locations where levee raises were necessary. In Robinson's chronicle, the achievements of the MBM in flood control "exceeded the expectations of its inventors", and since it was built during World War II, partly by prisoners of war, he considered the model itself "a good and reliable soldier" (ibid.: 292).

Civil engineer Margaret Petersen, who participated in the construction of the MBM in the late forties, also describes the benefits of doing research with physical models instead of using exclusively numerical data (US Army. Interview with Petersen 1997). Petersen recalls the experiments she and her colleagues carried out at the MBM station: they analysed the effects of levees on flood heights, studied the consequences of building dams in various locations, surveyed the variations in roughness of the bed, simulated the density of vegetation in the model with screen wire, etc. (ibid.: 7). Petersen affirms that these experiments involved conditions that could have been "routed theoretically for individual streams", but for complex systems like the Mississippi River "it was more reliable to do it on a model where we could change variables one at a time and note the results" (ibid.: 7). From Petersen's account, it would be erroneous to conclude that the model was a mere illustration of what the data was already showing, as it seems to have played an important role in obtaining successful predictive results about the Mississippi River.

Since 1949 the epistemic value of hydraulic scale models was "officially" acknowledged by the community of engineers in the US. On that year the first conference specialized in Engineering Hydraulics took place at University of Iowa. One of the results of the conference was the publication of the principles of hydraulic similitude and flow measurements in the form of a manual, which became the basic reference book in the field for many years (Rouse 1950; in US Army: Interview with Petersen 1997: 10).[42] In that manual, there are various allusions to the value of scale models in much more convinced terms than in previous years:

> Model-prototype comparisons have clearly demonstrated that, almost without exception, there is a correspondence of behaviour within and usually well beyond the expected limitations. [...This] attests the real value of this modern tool of the hydraulic engineer. [...] River-improvement plans of tremendous magnitude have worked out successfully according to predictions based on model tests [...] The improvements indicated by the models were invariably found to be real when the prototypes were constructed. (Rouse 1950: 138)

The use of scale models for prediction is presented here as *successful*, *clearly demonstrated*, and *of real value*, contrary to the idea of scale models "as mere toys for youngsters of the profession" defended years before (Robinson 1992: 277). Scale models have potential for prediction and understanding, beyond the illustrative or pedagogical roles they might play. In fact the MBM was *also* a visual illustration of the material properties of the Mississippi River, and a persuasive tool for educational and political purposes. All these functions were complementary to each other. General Reybold, Chief of Engineers at the Corps from 1941 to 1945, was

[42] Margaret Peterson (1997: 10), in her interview with the US Army Corps, claims: "The 1949 conference was very special. It was attended by 425 people, and the 1000-page proceedings was published as a hardbound book by Wiley entitled Engineering Hydraulics edited by Hunter Rouse. The proceedings covered fundamental principles, hydraulic similitude, flow measurement, hydrology, ground water, steady flow in conduits, water hammer, channel transitions, gradually varied flow, flood routing, wave motion, sediment transportation, and hydraulic machinery. That book was the basic reference book for hydraulic engineering for many, many years". The reference book in question is: Rouse, H. (1950): *Engineering Hydraulics. Proceedings of the Fourth Hydraulics Conference.* Iowa Institute of Hydraulic Research. June 12-15, 1949. Decades later, other manuals on hydraulic engineering were widely used for general consultation as well: Yalin, M. S. (1971) *Theory of Hydraulic Models*; Dalrymple, R. A. (Ed.) (1985) *Physical Modelling in Coastal Engineering*; Hughes, S. (1993) *Physical Models and Laboratory Techniques in Coastal Engineering.*

aware of the possible effects among public personalities that the construction of a model like the MBM could have:

> GEN Reybold stated that such a model would have great potential value for demonstrating flood-control measures to Government officials and laymen as well as to engineers. He said the model could be a means of convincing those responsible for flood-control legislation of the necessity for a central control of all reservoir operation during flood emergencies in the Mississippi River Basin. (Foster 1971: 2)

The functions of "demonstrating" and "convincing" public authorities and lay people go hand in hand with the epistemic benefits for research that the model had. The capacity of persuasion of the MBM was largely due to the direct visual access it afforded, in comparison to numeral models of data. In a manual on *Ocean Engineering Sciences* (1990), coastal engineer Bernand Le Méhauté explains the "convincing" properties of scale models in relation to their materiality:

> Physical contact with the fluid element remains the best guide for intuitive discovery. It demonstrates visually, with credibility, what will happen. It stimulates the imagination and guides creative engineering solutions at a level that cannot be reached by theory and computer printouts, not even by computer graphics. (Le Méhauté 1990: 957)

The work with scale models awakens our senses of vision and touch, what for Le Méhauté (1990) stimulates our intuitions in ways that differ from, but could be complementary to, mathematical and computational models. The physicality of scale models is linked to their credibility.

Someone could perhaps affirm that the MBM indeed played a vital role in hydraulic research in the forties and fifties. But today, with the development of mechanisms for the computerization of data bases, the role of scale models is not crucial anymore. Moreover, one could say that the high costs and long periods of time that the construction of scale models entail make them rather dispensable. This is an argument offered among others by Oreskes (2007), who identifies scale models with "instruments of a past era", when resources such as computers did not exist: nowadays "a computer simulation can be used in precisely the same manner than a mimetic physical model" (Oreskes 2007: 113.). In response, Susan Sterrett (2017a: 861) has tried to show that in many circumstances the type of information and understanding about physical phenomena that scale models afford are irreplaceable.

Experimentation on physical models has not been supplanted by computer simulations for at least two reasons. The first is that experimental scale models often reveal phenomena that a computer simulation built using current knowledge cannot. For instance in aerodynamics, the demise of the wind tunnel – designed to study the effects of air moving past solid objects – has been predicted several times over the last century, but wind tunnels are still considered indispensable today and most research institutes in engineering have one in use (ibid.: 866). This idea connects with the thought that physical models can be surprising in a way computer simulations cannot (Morgan 2012: 294-296).[43] The second reason is that most computer simulations "rely upon information gained by observation and experimentation, especially experimentation on analogue models" (Sterrett 2017am: 866). To be able to design computer simulations, empirical data is necessary, and frequently scale models are required to obtain those data.

I would like to add a third reason: computer simulations are less transparent than scale models regarding the distortions they contain.[44] Civil engineer Petersen points out, concerning the work with the MBM and other physical models, that "everything changed completely with the inclusion of computers" (US Army Corps. Interview with Petersen 1997: 48), because "computers had a tremendous effect on hydrology, on forecasting, and hydrologic studies [because they…] greatly simplified sediment transport and backwater computations, which were terribly tedious" (ibid.). However, computers could not perform some of the most important work that needed to be done in research:

> Hydraulics is more an art than a science. Of course, it's what today's generation wants to put into equations and into the computer. I have reservations […] I think the biggest problem with computers today is that so many people don't write the programs they use. They learn to use the programs, and they really don't recognize all the approximations that go into them. (US Army Corps. Interview with Petersen 1997: 48)

[43] Morgan (2012: 34) distinguishes between *surprise*, that physical models, mathematical models, and computer simulation can afford, and *confoundment*, that only physical models can afford. *Confoundment* is a stronger term because it implies not only the production of unexpected results but also of potentially unexplainable results given the existing knowledge.
[44] The use of the term "transparent" here is inspired by a slightly different discussion about the differences between computer simulations and thought experiments. It is argued (di Paolo et al. 2000) that while thought experiments are transparent with respect to the steps that lead to a certain result, computer simulations are usually epistemically opaque.

"Approximations" is the key word here, referring to the different idealizations, simplifications, and distortions included in all scientific models. If the implications of the inclusion of approximations are not fully and openly recognized, it is hard to evaluate the real epistemic value that models can have. Being aware of the whole process of construction of a representation is fundamental to understanding its potentials and limitations. I do not deny the possibility of being aware of the whole process of designing and programing computer simulations, nor do I deny the advantages of introducing computers in engineering sciences. Yet, I agree with Peterson's observation that computer simulations are in some respect a way of distancing ourselves from the physical properties of the object studied, as well as from the manipulations involved in the modelling practice. Approximations and distortions are brought to the fore in scale modelling, while they remain partially occult in the programs' codes in computer simulations. In conclusion, computer simulations might be preferred for various reasons, including costs, adaptability, and the ease with which they can be modified (Sterrett 2017a: 866). But that is different than claiming that they are satisfactory substitutes for scale models *in general*. The epistemic value of scale models should not be underestimated, neither by attributing them only illustrative functions nor by considering them replaceable by computer simulations.

The founding of the WES and the subsequent construction of the MBM give us a glimpse of the development of the practices of scale modelling in the engineering sciences throughout the twentieth century. The broader epistemic potential of the MBM was not obvious, however, from the time of its construction. Instead, it unfolded through time, when the model continued to produce predictions and solutions to flooding problems that would carry over even to phenomena and geographical areas at which it was not initially directed. The model as a whole stretched to produce novel results beyond its immediate range of applications. In Chapter 5, I will describe this and other models that allow to perform novel research as fruitful, and explain how fruitfulness concerns the broader aim of gaining understanding from scientific models.

Figure 7. Postcard of the site. Visitors' centre. At the back: "WES, Corps of Engineers, US Army, Vicksburg. Showing a portion of the Mississippi River scale model used for flood control purposes. Open daily to the public Monday through Friday, with free guide.

4.2. Judgments of similarity in scale modelling

After challenging the assumption that scale models have a low epistemic value, I argue in this section that philosophers of science who usually undervalue the role of scale models in science also recognize the presence of obvious visual similarities between scale models and the targets they represent. However, they frequently consider those similarities superficial, of mere appearance, and not playing a particularly relevant epistemic function. The miniature replica of an airplane, of the kind a collector would keep on her shelf, could easily be judged as *similar to* a full-sized airplane, as they probably share some of the same shapes and colours. My point is that, first, collectors' miniatures should not be considered paradigmatic cases of scale models (as in Black 1962; Frigg & Hartmann 2018). Instead, the fields of engineering and life sciences provide abundant examples of fruitful scale models for the purposes of prediction and understanding, like the MBM. Second, similarities that play a role in fruitful scale models are not merely similarities of appearance or superficial. I introduce the case of the San Francisco Bay Model (SFBM) in this section to illustrate how judgments of similarity are constantly present in the practice of scale modelling, but also how these judgments involve much more than superficial similarities. In Section 4.3 I show that they also involve dynamic similarities and various forms of distortion and idealization.

To illustrate the attitude towards scale models and similarity that I try to dispute, consider this quote from Suárez:

> …an engineer's toy bridge may be similar to the bridge that it represents in the proportions and weights of the different parts […] There are also important dissimilarities, such as size, which make the representation only a partially successful one. (Suárez 2003: 231)

Similarity intervenes here, but only as a superficial feature of a "toy" that looks like a real bridge in appearance. The mere idea of an "engineer's toy bridge" is rather unclear: it can be understood as a toy that works merely as an illustration of the modelling practice, or as an explanatory tool that models the behaviour of the real bridge. Suárez (2003) adds that the dissimilarities in size of the toy bridge makes it "only a partially successful" representation. I contend that the difference in size doesn't render the model of the bridge a less successful representation, but allows the possibility of investigating the target with the help of a model. There is an implicit assumption here that if similarity has anything to do with scale models it is because scale models are supposed to be "naturalistic replicas" or "mirror images of the target" (following Achinstein's [1968] characterization of scale models). Contrary to this conception of scale models as naturalistic replicas of their targets, similarity, I argue, is manifest in practice through a plurality of judgments of similarity that work as resources for the construction of models that can be used for prediction and explanation.

There is a substantial record of the plans of design of both the MBM and the SFBM thanks to digitalized reports of the US Army Corps of Engineers (see US Army Corps 1963 and 1997; Foster 1971). The reports allow us to localize constant references to the notions of "similarity", "similitude", "analog", and "resemblance" in the practice of constructing these models. Some of the judgments of similarity employed in the practice are very specific and context-dependent. Other judgments of similarity entail more standardized conceptions of the principles of physical similarity; still other judgments of similarity rely on previous experiences, and involve more intuitive, unspecified ideas about the similarity relation between the model and the target of the representation. I will illustrate the presence of these three types of judgments of similarity in turns, using the case of the SFBM.

In 1957 the U.S. Army Corps of Engineers decided to construct a model that could reproduce the water flows of the San Francisco Bay. Initially the main goal was to study the ambitious plan to dam the Bay proposed by John Reber in the late 1940s (Weisberg 2013: 2). Reber believed that damming the Bay would supply San Francisco with nearly unlimited drinking water, and could also revolutionize the area's transportation and industrial infrastructure.[45] There were, however, opponents to the Reber Plan within the Corps. They worried that it might render the south of the Bay a cesspool, creating problems for the ports and having other unintended environmental consequences (ibid.: 2). The model finally built was an immense structure of around 1.4 acres (although much smaller than the MBM), located in a warehouse in Sausalito. It represented the San Francisco Bay, the confluence of the Sacramento and San Joaquin rivers, and seventeen miles of the Pacific Ocean beyond the Golden Gate Bridge. It was constructed out of precast and lightweight concrete, and the bottom surface of the model was embedded with thousands of copper strips, which simulate the roughness of the sea bed (ibid.: 9-10). When the SFBM was finished and calibrated, a scaled version of the barriers of the Reber Plan was located in the model and its effects were evaluated. The final report of the Corps described the effects of the Reber Plan in the following terms: "the Reber Plan practically eliminated tidal current movements and associated turbulence, and thus changed the Bay oceanward of the barrier from a rapid to a slow dispersion and flushing system" (US Army Corps 1963: 267).

The potential changes in the currents and tides that the model predicted could indeed have had disastrous consequences for the Bay, as the movement of the flows would have been significantly reduced by the barriers, and the report finally discouraged the construction of the barriers proposed by Reber. The SFBM was also used to study other projects of barriers in the Bay, such as the Chipps Island Barrier, the Dillon Point Barrier, the Point San Pablo Barrier, and to test the quality of the water and waste diversion conditions of the Bay (Huggins and Schultz 1967).

When looking at the technical reports of the US Army Corps on the construction of the SFBM, we observe the abundant presence of judgments of similarity in the phases of design, building, and calibration of the model (US Army

[45] See Reber, J. (1959) "Our perpetual gift to California and the nation: A master plan for the vast San Francisco region". National Archives and Records Administration. Pacific Region, Papers of John Reber, Location 2126E-G, Accn. 77- 94-09. In Weisberg (2013: 2-3).

Corps 1963 and 1981). The most prominent type of judgments is what I have called "standardized judgments of similarity". The use of standardized judgments of similarity concerns the laws or criteria of physical similarity, well-known in modern physics and engineering and commonly employed in the construction of hydraulic models. They are "standardized" because they have been systematically described in manuals and textbooks in hydraulic engineering and fluid mechanics, and are applied following very specific rules and standards (Rouse 1950; Rouse & Ince 1963; Hughes 1993) concerning how to obtain adequate "geometric similarity", "kinematic similarity", and "dynamic similarity" for the purposes of the models under construction. Geometric similarity is similarity in form; kinematic similarity is similarity in motion; and dynamic similarity refers to the correspondence of all homologous forces in model and prototype (Warnock 1950: 138-9). Standardized judgments of similarity are easily transferred from one practice of modelling to another, contrary to what happens with the other types of judgments of similarity – intuitive and context-dependent, described below– where the concrete practice defines the specific implications of the use of the notion of similarity.

Figure 8. San Francisco Bay Model. Visitors' Centre. Sausalito, CA.

Figure 9. San Francisco Bay Model. Sausalito, CA. John Easterby, engineering specialist (left), and Col. John A. Graf check readings on June 12 (1957). The Chronicle.

Figure 10. SFBM. Copper strips simulate the roughness of the sea bed (1986). The Chronicle.

Figure 11. Gen. A. Frye checks Dillon Point Barrier at the SFBM (1963).

Figure 12. Park ranger Griggs pours oil into the SFBM while oil company representatives and legislators look on (1989).

The study of the principles or laws of similarity has a very long history, derived from the analysis of ratios. Sterrett (2009) explains how the use of ratios in reasoning from similarity was already recognized by the Pythagoreans (2009: 1). Later, many proofs in Euclid's *Geometry* employed the strategy of establishing the similarity between two figures in order to draw inferences about other geometrical entities (ibid.: 1). The notion of a "similar system" goes back to at least Newton, and involves the possibility of generalizing from similarity of geometrical figures to similarity of physical systems. So instead of ratios involving only lengths, similarity of physical systems can involve other quantities such as time, mass, and force (ibid.: 3-4). Methodologies to establish physical similarity were not developed in detail and formalized until the late nineteenth and early twentieth century, however. Buckingham's (1914) article "On Physically Similar Systems: Illustration of the Use of Dimensional Equations" set out the methodologies engineers still use today to reduce the relation between physical quantities to a relation between dimensionless parameters, which in turn could be used to establish physical similarity (Buckingham's theorem) (ibid.: 5). Dimensionless parameters go from very simple (such as Mach number, which is a ratio of two velocities), to very complex (such as Reynolds number, which contains density, velocity, length and kinematic viscosity) and are incredibly useful in hydraulics and coastal engineering (ibid.: 4).

Some examples of the explicit application of standardized judgments of similarity in the construction of the SFBM are expressed in the reports as follows:

> In accordance with the *laws of similitude*, the measurements made on the model indicate the corresponding prototype quantities (U. S. Army Corps 1963: 41).
> [Under the rubric "Model Design – Similitude Relations"]: Model *laws* commonly used for establishing the *similarity* of model and prototype are known as Froude's, Reynolds's, Weber's, and Cauchy's laws (U. S. Army Corps 1963: 43).
> The establishment of the *similarity* between model and prototype commonly makes use of *laws of similitude* expressed by [...] the Froude, Reynolds, Weber, and Cauchy numbers (U. S. Army Corps 1981: 6-1. In Weisberg 2013: 154).

The Froude, Reynolds, Weber, and Cauchy numbers are dimensionless parameters regarding the most relevant forces intervening in fluid dynamics. The Froude number expresses the correlation of inertial forces with the force of gravity; the Reynolds number the correlation of inertial forces with viscosity; the Weber number the correlation of inertial forces with surface tension; and the Cauchy number the correlation of inertial forces with elasticity (Rouse 1950: 142-4). The construction of adequate hydraulic models in principle requires that each of these numbers be the same in model and prototype, as above. However, it is also clear in the reports that, in practice, some parameters are neglected and others distorted –

for instance, in the construction of the SFBM, the Cauchy and Weber numbers were disregarded.

Two clarification notes. In the literature on scale modelling there are mentions of "principles of similarity" (Rouse 1950; Robinson 1992; Ettema et al. 2000; Sterrett 2006; 2009), "laws of similarity" (Rouse 1950; U. S. Army Corps 1963), "criteria of similarity" (Rouse 1950; Hughes 1993; Ettema et al. 2000; Sterrett 2006; 2009) and "requirements of similarity" (Rouse 1950; Hughes 1993; Ettema et al. 2000; Sterrett 2009). Although it could seem that referring to "laws" has stronger implications than referring to "requirements" of similarity, they should be considered synonymous. All these terms refer to the regulated set of norms to achieve geometric, kinematic, and dynamic similarity. Also, there is a slightly different use of terminology in the literature when using the terms "similitude" and "similarity". Some manuals only use the term "similarity"; others prefer "similitude"; others use either interchangeably (see Heller 2011); and others like Hughes (1993) assign different meanings to the terms similarity and similitude. Similitude refers (for Hughes) to the formal mathematical conditions that must be met by the scale ratios between prototype and model, whereas similarity is a condition that exists when a model gives a similar response to the prototype (Hughes 1993: 14). In Hughes's account, it is possible to have model similarity without meeting similitude criteria. Conditions of similarity are chosen by the experimenter in order to make the physical model reproduce certain effects satisfactorily (ibid.: 52). Despite being an interesting distinction, there is no need to stress it in an account that focuses on "judgments of similarity". Similitude as Hughes describes it would correspond to the "standardized type of judgments of similarity" in my account, before being applied to specific practical situation. I keep the mentions of similitude and similarity as in the original quotes, but will understand them as equivalent.

Apart from standardized judgments of similarity, other types of judgments of similarity are also identifiable in the construction of the SFBM. In the process of operating and adjusting the model, specific, context-dependent uses of judgments of similarity appear too. The levels of salinity of the water tank of the model had to be adjusted to those of the Bay. It is affirmed in the report that "the model was operated until stable salinity regimens were obtained *similar to* that existing in the prototype" (U. S. Army Corps 1963: 56; also 68. My emphasis). What the use of a judgment of similarity here involves is quite specific: enough agreement between the salinity levels of the water in the tanks of the model and those registered in the prototype on 21st September 1956 (ibid.: 68). There was a constant inflow of fresh water into the Bay that mixed with salt water from the sea, so the salinity levels varied a lot across the Bay. The calibration of the model regarding its salinity levels was a complex process that had to consider different locations and variables. Introducing judgments of similarity and dissimilarity happened to be useful in this situation because it allowed comparison of very specific aspects of the behaviours of model and prototype. When it is affirmed that "salinity regimens were obtained *similar to* that existing in the prototype" it is clear that "complete similarity", "identity", or "sharing of the exact same properties" is not what was pursued in the practice; rather, relevant features of model and prototype were stabilized so as to permit the pursuit of the goals of the practice.

In a different section of the report, it is also claimed that "profiles were measured in the flume for comparison with *similar* profiles measured in the Bay model" (US Army Corps 1963: 165. My emphasis). On this occasion, the attention was on the study of the water-surface elevations of the model. A board of engineers had the task of collecting data from the profiles of the real flume and comparing them with the profiles of the model. The comparison did not involve an attempt to achieve perfect identity or duplication of the actual profiles of the Bay, but rather the use of judgments of similarity to determine which profiles in the flume to compare with which profiles in the model. Context-dependent judgments of similarity served as resources to direct and focalize the practice of scale modelling.

Additionally, the use of some other judgments of similarity that we can identify as more "intuitive" and "unspecified" is also found in the reports of the construction of scale models. In the first phases of the construction of the MBM, for instance, engineers of the Corps had to provide a justification for building the MBM on a particular terrain near Clinton, Mississippi. They allude to a particular feature of the terrain in Clinton: it had "as close a resemblance to the natural features of the Mississippi Basin as could be found" in the area (Foster 1971: 10). "Resemblance" is a rather imprecise term here. There is almost no further clarification of what the requirements for the terrain exactly were, but the report still documents the selection of this terrain based on an imprecise judgment of similarity, as it informally intervened in the decisions made in the first stages of the construction of the model. My use of the term "intuition" in this context has no further implications regarding the rationality of the decisions made appealing to them. I call this type of judgment intuitive in the sense that they are more directly supported by the previous skills and background knowledge of engineers, without detailed formulation of the justifications for them. In fact, there are relatively frequent references to the term intuition in manuals on scale modelling, when they refer to decisions engineers make based on their expertise and previous experiences (see Warnock 1950: 137; Le Méhauté 1990: 937; Hughes 1993: 52). Scientists and engineers master certain techniques and skills thanks to previous experiences. This allows them to adjust and apply model results in a non-formalized, but usually satisfactory, manner.

The classification into three types of judgments of similarity is supported by a classification of "kinds of similitude" that the classic manual in hydraulic engineering edited by Rouse (1950) offers. Warnock writes a chapter in the manual on "Hydraulic Similitude", in which he refers to the existence of three pertinent similitude criteria employed in the practice of scale modelling (Warnock 1950: 137). The first kind of similitude has a more "rational basis" and has been developed by "theoretical means". That is, it is a standardized, systematic criterion of similitude, by which a set of transferences ratios can be applied to different model findings to predict prototype behaviours (ibid.: 137). The second type of similitude criteria is presented by Warnock as follows:

> In addition to those having a rational basis, there are other kinds of similitude which are developed by experiment in the model to be applied *in special cases* [...] Although attempts are being made to develop a more rational basis for transference of experimental results in such cases, the current procedure is based primarily on empirical relations. (Warnock 1950: 137. My emphasis)

This second type of similitude criteria can be associated with the notion of "specific or context-dependent judgments of similarity" I proposed. They cannot be easily formulated in mathematical or theoretical terms, but they apply in special cases and play an important role in the adjustments of the experiments with specific models. Third, Warnock recognizes that "in still other cases the only basis for predicting prototype behaviours from model experiments is one compounded of experience and intuition" (Warnock 1950: 137). This description of how certain decisions are made based on experience with other scale modes fits well with the notion of "intuitive, unprecise, judgments of similarity". Having the ability to effectively formulate convenient judgments of similarity appears as an important part of the practice of scale modelling. The agreement between Warnock's (1950) characterization of different similitude criteria and the classification of judgments

I proposed based on the analysis of the reports of the SFBM and the MBM can be taken as a sign that this is a satisfactory way of studying the presence of similarity in the construction of representations like these.

This classification does not intend to be rigorous or definitive, however. The aim was mainly to emphasize the plurality of forms in which similarity is actively involved in practices of constructing models like the MBM and the SFBM. Few philosophers of science have focused on the study of judgments of similarity in practice, and some who have done so, like Sterrett (2009, 2017a) have exclusively analysed the use of "standardized judgments of similarity". I aimed to offer a more flexible way of thinking about the various applications of similarity in modelling practices. If we aim to advance an account of the means of representation, we are addressing a pragmatic question that needs to be informed by specific uses and definitions in manuals and handbooks like Rouse's (1950), which connects practitioners' work with the standardization of practices within a community.

4.3. Not only size matters. The role of distortion

The uses of judgments of similarity identified in the previous section can be characterized further. As pointed out in Chapter 3, making judgments of similarities usually involves the formulation of judgments of dissimilarity and distortion as well. The cases of the construction of the MBM and the SFBM exemplify this particularly well, as idealizations, simplifications, and other forms of distortion are directly involved in the uses of the three types of judgments of similarity. When engineers for example made standardized judgments of similarity based on the well-established principles of physical similarity, they also judged the deviations from the standards that they were willing to accept, as well as the potential benefit that inserting distortions could have for the modelling practice. Geometric similarity, for instance, usually goes hand in hand with geometric distortions in scale models. Robinson (1992) captures how the application of the principles of geometric similarity in the SFBM involved considerable distortions:

> To be whimsical, if one were a Lilliputian resident in this model conforming to its laws, he or she would be about ¾-inch tall, and because of the distorted scale, as thin as tissue paper. The normal 8 hours sleep would last but 2.4 minutes, while lunch hour would be only 18 seconds. (Robinson 1992: 287)

One would be “as thin as a tissue paper” because some dimensions of the San Francisco Bay were reduced much more than others. The SFBM combines a scale of 1:1000 for the horizontal dimensions (length and width of the surface of the Bay) with a scale of 1:100 for the vertical (depth of the Bay); while in the case of the MBM, the model combines a scale of 1:2000 for the horizontal with 1:100 for the vertical. The distortion of the geometric proportions is clear in both cases, and also quite high (of x10 in a case and x20 in the other case). The application of the criteria of geometric similarity is a process of judging what dimensions are going to be scaled (enlarged or reduced), and how exactly each one is going to be scaled in relation to the others. In other words, when applying the criteria of geometric similarity to concrete scale models, not only size matters.

Distortions should not be strictly understood as limitations of the practice of scale modelling. There are good reasons for the introduction of distortions:

> Distorted scales are used when a departure from geometric similarity serves some definite objective and the results are limited to this objective. Distortion is usually required in models of river channels, floodways, harbors, and estuaries, for which the horizontal dimensions are large in proportion to the vertical ones [...] Many valuable studies have been made with distorted models. (Warnock 1950: 146)

Here we see that the idea of geometric similarity can be traded off against the possibility of introducing well-reasoned, efficient distortions. For the case of the SFBM, geometric distortions were justified as a “compromise between two conflicting demands” for the scale model, to be “large enough to give faithful reproduction of flows [...] and as small as is permissible in order to reduce cost, construction time, water supply, and space requirements” (US Army Corps 1963: 41). Engineers know from experience that the vertical geometric scale for rivers and estuaries should not be reduced by more than 1:100, as the behaviour of the flows would otherwise be substantially different to that of the prototype. Meanwhile, there is more flexibility with reducing the horizontal scale, insofar as other important parameters concerning dynamic similarity are met. Warnock (1950) gives specific recommendations about the selection of scales in his manual:

> The following ranges of scales have been used successfully in many model studies and are recommended for use as a point of departure in the determination of scales. [...] Spillways for large dams may be studied on

> models with scale ratios in the range from 1:30 to 1:1000. […] Most river models vary in scale ratios between 1:100 and 1:1000. There is a minimum size for each type of model. […] When models smaller than these sizes are used, it is difficult to build them, to say nothing of the possibility of introducing similitude defects. (Warnock 1950: 144)

Previous experiences are fundamental to decide what the most convenient scale is and what degree of deviation from the standard should be accepted. Warnock (1950: 144) adds that "it should be realized that the experienced investigator can venture beyond the above-stated limits and obtain satisfactory results, but this is not recommended as a general practice". Engineers decide on the most convenient combination of judgments of similarity and distortion for the case at hand, taking into account the rules, standards, recommendations, and shared skills that are entrenched in the system of practices.

In addition to geometric similarity, we find dynamic similarities accompanied by dynamic distortions in the practice of scale modelling too.[46] Scientists require empirical understanding of the physical systems to make appropriate idealizations and generalizations about the behaviour of those systems (Sterrett 2009: 25). I previously referred to the Reynolds, Froude, Cauchy, and Weber numbers. In theory, dynamic similarity requires that each of these numbers be the same for model and prototype, but in practice engineers do not aim to do so. This is because, for instance in the case of the SFBM, "the forces of surface tension and elasticity, represented by the Weber and Cauchy numbers, do not significantly affect conditions in the Bay, so it is not necessary to simulate their effects" (US Army Corps 1981: 6-1. In Weisberg 2013: 154). Instead of aiming at complete, perfect dynamic similarity between model and prototype, what is advantageous for the practice is to disregard the forces that only have a slight effect in the flow phenomenon, while stressing the effects of the more significant forces. In an estuary such as the San Francisco Bay, the features of flow are mostly controlled by the joint effect of inertial and gravitational force; thus, hydraulic quantities mainly vary according to the Froude number (ibid.). However, focusing on the variations of the Froude number and not considering the Reynolds number could generate some undesirable effects in the model. "Fortunately [the report of the

[46] There is also "kinematic similarity", but since it is included in the notion of "dynamic similarity" I will only refer to the latter. Additionally, Sterrett (2009: 4) points out other kinds of physical similarity employed in experimental model testing outside hydrodynamics, such as elastic similarity, similarity of mass distribution, electrical similarity, magnetic similarity, thermal similarity (following Pankhurst 1964: 18 and 77-80).

SFBM reads] it is not necessary to duplicate the Reynolds Number, for the only important result of such distortions [can be] [...] checked" and compensated by taking into consideration other parameters (U. S. Army Corps 1963: E-3).

In a more recent paper, Heller (2011) suggests three common practices among engineers to deal with the undesirable consequences of scale effects or privileging certain adjustment of geometric and dynamic similarities over others: "avoidance", "compensation", and "correction" (2011: 299-203). Avoidance refers to the practice of avoiding scale effects as much as possible, by applying rules of thumb based on past experience (ibid.: 299). For instance, one could avoid distortions generated by kinematic viscosity by replacing the fluids used in the model (i.e. air for water). The idea of engineers using rules of thumb connects well with the thought that intuitive judgments based on previous experiences are occasionally employed in the practice of modelling. The second practice Heller (2011) mentions, compensation, refers to the inclusion of explicit distortions of parameters (i.e. distortion on the grain diameter, length scale) from the beginning of the process of designing a model, "in favour of an improved model-prototype similarity" (ibid.: 302). That is, significant distortions of the parts of the model are compensating in such a way that the whole results in a model that generally fulfils the requirements for adequate dynamic similarity. Nevertheless, in both avoidance and compensation, practical considerations more often than not limit the modeller's choices (in Hughes 1993: 251). The third resource, correction, refers to the posterior adjustments of the model results by using available data from the prototype. When it is well-known that for instance waves would decay faster in the model than in the prototype, the results are corrected in the phase of evaluation, without necessarily modifying the physical model (Heller 2011: 303).

Figure 13 systematizes the scales used in the SFBM to convert important features of the prototype into parameters in the model. The parameters included in the list are only a small selection of parameters that engineers considered relevant, among the many possible features of the San Francisco Bay that could have been included. The viscosity of the water, the colour and density of water and soil are for instance parameters not considered in the construction of the SFBM. Each of the parameters included was converted from prototype to model according to a specific scale. Apart from the various geometric distortions manifest in the chart (when looking at the depth and length of the model), we can see how the scale of the slope, instead of being reduced from prototype to model, was amplified on a scale of 10:1, while the time of the tidal cycles is reduced to a scale of 1:100, and the salinity levels of the Bay are kept to a scale 1:1. The key to the production of a fruitful model is found in adequate integrations of the various parameters in the whole. This, I have tried to argue, is done with the help of the interplay of various judgments of similarity and distortion. The use of a scale 1:1 for the salinity levels means very little if we do not consider other parameters to which salinity is related, such as the tidal cycles, the vertical distribution of water and suspended particles in the tank, the roughness of the sea bed, etc. It could be claimed that perfect similarity or identity is the resource (of the means) used to translate the level of salinity in the Bay into the level of salinity in the water tank of the model. But once we consider the interaction between different parameters, the salinity level scaled at 1:1 becomes a distortive element for the whole, since it hasn't been scaled as other parameters have. Judgments about how the different parts of the model work individually are always connected to how those parts are integrated.

CONVERSION OF MODEL DATA TO PROTOTYPE EQUIVALENTS

Model	Factor	Prototype
1 foot	Depth	100 feet
1 foot	Length or Width	1,000 feet
10	Slope	1
1 cubic foot	Volume	100,000,000 cubic feet
1 cfs	Discharge	1,000,000 cfs
1 ft. per sec.	Velocity	10 ft. per sec.
14.9 minutes	Time*	24 hours and 50 minutes
1	Salinity**	1

* The time scale is 1:100.

** The salinity scale ratio required for an investigation of this type is 1:1.

Figure 13. Conversion of model data to prototype equivalents. San Francisco Bay Model. From US Army Corps (1963: 42).

The idea that scale models are mere replicas of the objects represented, in the sense of miniature copies of their targets, must be rejected in favour of a conception that highlights the various judgments of similarity and distortions at stake in the process of constructing scale models. This is the basic idea behind the account of the means of representation, supported on the notion of "creative similarity", advanced in Chapter 5. Philosophers of science should seriously consider how concepts like similarity are defined and employed by practitioners who intervene in the construction of models, and who participate in the development of technical manuals that help direct a wide range of practices. Definitions of similarity like the following, offered by Warnock (1950), support the idea that similarity in the practice of scale modelling does not consist in achieving complete correspondence or copy images of the target.

> Similitude, as applied to hydraulic models, goes considerably beyond the superficial aspects of geometric similarity with which it is sometimes erroneously identified. Similitude can be defined as a known and usually limited correspondence between the behavior of a model and that of its prototype, with or without geometric similarity. *The correspondence is seldom perfect*, because it is generally impossible to satisfy all the conditions required for complete similitude; however, these conditions are known, as will be shown subsequently. *The term similitude should hence be qualified to indicate the general limits of correspondence*, or one might speak of various types of similitude, each of which has a definite set of limitations (Warnock 1950: 136. My emphasis).

Two key ideas are implied here. The role of similarity in scale modelling goes beyond geometric similarity: dynamic similarity is fundamental too. Also, similarity is identified with *the limits of correspondence* between a model and a prototype, and not with perfect correspondence. This idea supports the claim that similarity in the practice of representing is about the effective combination of judgments of similarity and distortions, and not imitating or copying the target. Accounts in philosophy of science, and more specifically in PSP and iHPS, should take definitions from manuals like this one more seriously. A characterization of similarity based on practitioners' own definitions and recommendations on how the concept is applied in practice should work as the source of normativity of the discussion on similarity in the philosophical debate on representation.

4.4. Implications for the debate of scientific representation

The cases of the MBM and the SFBM can inform the debate of representation in philosophy of science in interesting ways. In this concluding section, I discuss two particular ways in which the analysis in the preceding sections complements or challenges certain positions in the debate of representation. One, my own examination of the case of the SFBM can cast doubts on some of Weisberg's (2013) assumptions in his *weighted feature-matching account of similarity*. Two, the study of various judgments of similarity in the construction of the SFBM and the MBM can bring to the debate on scientific representation the thought that similarity adopts a variety of forms in the practice of representing. Yet, this shall not involve the thought that similarity is a "problematically vague" notion, as for instance Goodman (1972) and Frigg (2006) have argued.

As mentioned in Chapter 3, the *weighted feature-matching account of similarity* that Weisberg (2013) proposes is partly sustained on the idea that an analysis of representation should reflect judgments that scientists actually make (ibid.: 138). This differentiates his account from others in which the relation of representation "holds between inaccessible, hidden features of models and targets" (ibid.: 138). The *weighted feature-matching account* can be understood as an attempt to formalize the idea that a model is similar to its target when they share many , and do not fail to share too many, features that are thought to be salient by the scientific community (ibid.: 136). Weisberg offers a formula, derived from Amos Tversky's (1977) *contrast account of similarity* in cognitive psychology, which assigns a 'similarity score' to the relation between particular models and targets.[47] First, the relevant attributes (i.e. properties or patterns) and mechanisms (i.e. generating processes) of model and target have to be identified and included in the formula (Weisberg 2013: 146). Then, a weighting function that ascribes a level of relevance to the various members of the feature set needs to be assigned to each of the terms in the equation (ibid.: 148). For instance, θf is assigned to the attributes shared by

[47] Amos Tversky proposed a *contrast account of similarity* to capture everyday judgments of similarity made by his experimental subjects (Tversky 1977). His account assumes that the similarity between objects *a* and *b* depends on the features they share and the features they do not share. To measure this, Tversky begins with a set of features Δ. We call A the set of features in Δ possessed by *a*, and B the set of features in Δ possessed by *b*. Then, a weighting function *f*, which is context-sensitive, is chosen to assign a degree of relevance to the features selected, and to obtain a score that can be used in comparative judgments of similarity. Weisberg substitutes the *a* and *b* in Tversky's account for model and target, and attributes the ability to identify relevant features and weighting functions to the scientific community (Weisberg 2013: 146-150).

model and target, αf to the mechanisms shared by model and target, βf to the attributes that model has but target lacks, γf to the mechanism that target has but model lacks, etc. Weisberg (2013) makes explicit that there are no context-free definition of functions and selection of properties. They are the product of the deliberation of the scientific community (ibid.: 145). The following formula is obtained:

$$S\ (m,\ t) = \frac{\theta f\,(M_a \cap T_a) + \rho f\,(M_m \cap T_m)}{\theta f\,(M_a \cap T_a) + \rho f\,(M_m \cap T_m) + \alpha f\,(M_a - T_a) + \beta f\,(M_m - T_m) + \gamma f\,(T_a - M_a) + \delta f\,(T_m - M_m)}$$

S indicates the final similarity score, M and T are the sets of features possessed by model and target, f are the weighting functions, and the different Greek letters correspond to the weights attributed to each of them. The similarity between a model and a target increases when they share many features (attributes and mechanisms), when the model is not penalized for containing extraneous details, and when the model does not fail to capture important features of the target (ibid.: 146). The score obtained ranges from 0 to 1. When model and target share many relevant features, S approaches 1 (ibid.: 148).

Thus stated, the account seems to be able to successfully systematize the degree of similarity between a model and a target in terms of the number of properties, and the relative importance of the properties, that model and target share. Some difficulties arise though when Weisberg tries to fit the analysis of particular cases like the SFBM into his formula. It is not particularly clear how each feature of the SFBM is going to be classified in the *weighted feature-matching account*, either as a feature that model and target share, as an extraneous feature in the model, or as a feature of the target that the model fails to capture. This is how Weisberg proposes to do so:

> At this point, we can consider how the different terms would get filled for the San Francisco Bay model. We know that, within reasonable tolerance, the model is physically scaled to the Bay's features, and the tidal cycle and salinity gradient are represented accurately (attributes shared by model and target, or Ma ∩ Ta). Mechanistically, the model has little in common with the forces producing tides (the moon) and salinity gradients (the ocean) in

> the Bay (mechanisms possessed by the model but not the target or Mm−Tm). However, the ratios of physical forces as represented, for example, by the Froude number (ratio of body's inertia to gravitational forces) are correctly represented (mechanisms possessed by the model and the target, or Mm ∩ Tm). (Weisberg 2013: 148-9)

From this description it is not evident why some features of the SFBM are classified as shared, while others are considered non-shared. Weisberg (2013) seems to assume that, for instance, the salinity gradient should be classified as a shared feature of model and target, insofar as the scale used for the property of salinity is 1:1, and the salinity fluctuations in the Bay are adequately reproduced. However, I pointed out in Section 4.3 that the salinity gradient scaled at 1:1 wouldn't be exactly a shared feature if we consider that all the parameters are in interaction with each other, and that the rest of parameters that affect the geometric and dynamic qualities of the model have been substantially scaled. If we decide to consider the salinity gradient a shared feature of model and target (leaving aside the fact that it is in interaction with other features), then we have to acknowledge that it might be a shared feature that doesn't necessarily augment the final similarity score of the SFBM. It seems to be the case that not scaling the salinity level of the water tanks, thus maintaining it identical in model and prototype, generated certain undesirable effects in the behaviour of the model that had to be compensated for with adjustments in some other parameters (US Army Corps 1963: 56-70). Accordingly, it would be more consistent to include the "shared feature of the salinity gradient" in the part of the equation that Weisberg describes as subtracting from the similarity score instead of in the one augmenting it.

Also in the quote above, Weisberg classifies the mechanism that generates the tides in the SFBM as a non-shared feature of model and target; or more precisely, he considers the artificial system of pumps a mechanism possessed by the model but not by the target (Weisberg 2013: 148-9). There are two considerations here. First, it is not completely clear why comparing the mechanism that generates the tides in the Bay (the Moon) and in the model (artificial system of pumps) is relevant to evaluating the accuracy of the SFBM, as the SFBM, like many other models, is in itself an artificial tool for the study of a natural system we don't have direct access to. Second, it is hard to see why the artificial mechanism in the model would reduce the final similarity score of the whole, when the goal of the SFBM is to reproduce the behaviour of the tides in the Bay and that artificial mechanism is collaborating in reproducing such behaviour.

To the first point, Weisberg would have a more straightforward response: if the SFBM is used for purposes other than the study of the causal mechanisms that generate tides, the comparison of the mechanism in model (artificial system) and target (Moon) would be indeed irrelevant. In that case, we would assign a function to the feature "sharing of mechanisms" and "non-sharing of mechanisms" that reduces the importance of these features to a minimum, so they don't affect the similarity score of the whole (ibid.: 151-2). I believe Weisberg's account would have more difficulty responding to the second point. The artificial system of pumps plays a key role in the adequate reproduction of the fluctuations of tides in the Bay. In a hypothetical case where engineers were interested in using the SFBM to study the mechanism of tides and their effects in the Bay – that is, a case of engineers using the SFBM as a "mechanistic model" (ibid.: 151-2) – they would certainly want to assign a high positive value to the artificial system of pumps that reproduces the tides. However, Weisberg's formula would have problems in assigning a function that ascribes a high positive value to a feature that is non-shared, such as the feature concerning mechanisms. Weisberg claims that when building mechanistic models, scientists are interested in generating a model that shares many mechanistic features with its target; that has few mechanisms in the model not in the target; and that has few mechanisms in the target not in the model (ibid.: 151-2). But it seems that scientists would want to say that a model like the SFBM, that "mechanistically has little in common with the forces producing tides in the Bay" (ibid.: 148), can nevertheless be adequate for the study of the causal mechanisms and effects of tides in the Bay. In other words, it looks like it would be difficult in some cases to accommodate Weisberg's account with consideration of "judgments that scientists would actually make" in practice (ibid.: 138). This brings back the point discussed in Chapter 3 on the role that judgments of similarity play in Weisberg's (2013) account. He explicitly acknowledges that scientists select and weigh the properties of a model depending on the epistemic goals of the practice. But it remains uncertain how his account can characterize cases in which features that are not really shared can nevertheless be judged as highly relevant for the epistemic goals at hand.

With the difficulties just pointed out, I do not try to merely question the particular way in which Weisberg (2013) classifies the features of the SFBM. Weisberg (2013) concedes that his account works as a general framework that needs to be filled in in particular cases, and that disagreements between scientists about how to weigh and select different properties arise all the time. The main issue with how Weisberg's (2013) account applies to particular cases is, I believe, that weighing individual properties independently from each other does not offer unequivocal conclusions about the epistemic success of a representation as a whole. Following Parker (2015: 273-4), the perceived significance of a feature shared by a model and a target sometimes depends on which other features are shared. Following Fang (2017), Weisberg's account is "atomistic" instead of "heuristic". An atomistic account considers the number of features shared by model and target, and the individual importance of each of the features. In a holistic conception, it is not possible to weigh the value of individual features without considering the function they fulfil in the whole: "the essence of testing [...] is not based on weighting each feature independently and then adding them together, but on the *holistic* relationship between the predicted data and the observed data *as a whole*" (Fang 2017: 1752. Original emphasis). The salinity gradient in the San Francisco Bay might be, strictly speaking, a shared feature if considered individually. But it would be counterintuitive to say that it can by itself increase the similarity score of the whole without considering how the salinity gradient interacts with other geometric and dynamic properties in the model. The importance of the interaction of features in the San Francisco Bay was acknowledged by the engineers of the US Army Corps that worked on the construction of the SFBM:

> None of these problems [the intrusion of salinity into the Sacramento-San Joaquin Delta, the fluctuation of tides, and the currents and salinity levels of the Bay] can be studied separately, for each affects the others. And in an estuary so complex as San Francisco Bay, no formula could be found to encompass all the subtle changes that might occur in each of these areas were physical changes to be made in the regimen of the Bay. (Huggins & Schultz 1967: 12)

A holistic approach that considers the interaction of parameters in complex systems like the San Francisco Bay is required to fairly evaluate the function of individual properties in a model like the SFBM. Further questions about Weisberg's account arise from this observation. In particular, how does Weisberg (2013) understand the similarity score that is obtained from his formula? He seems to identify the similarity score with the level of accuracy of a model, so the higher the similarity score, the more accurate the model (2013: 148-150). But if that is the case, it is still unclear why some properties, such as the mechanism of tides in the

SFBM, might help improve the accuracy of a mechanistic model while being a feature that subtracts from (or is at best irrelevant to) the similarity score of the model. In Chapter 2 I argued that there are difficulties when identifying degrees of similarity with degrees of accuracy or overall faithfulness of a representation (as in Contessa 2007a, 2011). In Chapter 5, I will defend that we should adopt a framework where *understanding*, instead of knowledge (taken as the sum of true beliefs), is the measure of the epistemic success of scientific representations. Within that framework, values like fruitfulness – instead of accuracy, faithfulness, or truthfulness – would be more adequate when characterizing the epistemic achievements of models like these where similarity is the means of representation.

The second way in which the analysis of the SFBM and the MBM in the previous sections can inform the debate of scientific representation concerns precisely the idea of "judgments of similarity". After examining the reports of the construction of these models and different manuals used to guide the practices, one significant element stands out: similarity, manifested in various types of judgments, happens to be a central resource in the practice of constructing scale models. The analysis here, using historical and empirical sources, was limited to describing two particular scale models, in an attempt to illustrate the type of detailed analysis that an investigation of the role of similarity in practices of representing requires. I hope to have shown that judgments of similarity might play a key role as representational resources concerning model-target comparisons in a variety of other practices of representing as well, such as those described in Chapter 3: experimentation with model organisms (Ankeny et al. 2014; Ankeny and Leonelli 2011; Huber and Keuck 2013), research in synthetic chemistry (Bengoetxea et al. 2014), or the construction of physical models in economics (Morgan 2012; Vines 2000), among others. The classification into three types of judgments of similarity I suggested aimed at offering a flexible framework to think about the various participations of similarity as representational resource in modelling practices. Some of these judgments concerned the application of standardized principles of physical similarity, while others involved engineers' decisions about the features of specific models and prototypes, and still others rely on intuitions based on previous experiences and skills. When discussing the general problem of representation in philosophy of science, we usually refer to the concept of similarity (as a *means* or as a *constituent* of representation). But in the practice of representing, similarity is only manifest through a range of different judgments concerning model-target comparisons that epistemic agents employ with the aim of constructing successful models.

From the idea that there are various forms of judgments of similarity in practices of representing, we should not conclude though, as Goodman (1972) and more recently Frigg (2006) do, that if similarity is context-sensitive and plural, then it is an overly vague or empty notion. In Chapter 3 I disputed the so-called "argument from vagueness" against similarity. Here, we have seen that some judgments of similarity actually have a very standardized meaning, as formal principles of physical similarity that are transferable across a variety of representational practices. Some other judgments of similarity might acquire their meaning in specific practices and contexts, but this does not mean that their meaning is empty or "that anything is similar to anything else" in particular situations (Goodman 1972: 443). Even intuitive judgments of similarity are sustained on agreements within the community of scientists about what works and what doesn't in practice, according to their shared skills and experiences. As Giere (2004) and Teller (2001) have defended, representing does not require an objective measure of similarity, and the lack of such a measure does not introduce an undesirable amount of relativity to claims about the similarity of model and target (Giere 2004: 748). Engineers working at the SFBM and the MBM referred to "similar levels of salinity", "similar profiles," and "similar results in model and prototype" because making these judgments of similarity helped them capture the comparisons, approximations, and margins within which they were operating. These margins are rough, unprovable, and, more importantly, not aiming to an ideal complete correspondence between model and target.

I hope to have shown that practices of representing can be better understood if philosophers of science attend to the use of judgments of similarity in the construction of scientific models. In the next and final chapter, I suggest a way to advance an account of scientific representation that is directly informed by the study of judgments of similarity in specific practices of representing like the one developed in the present chapter. More specifically, I will ground an account of the means of representation in the notion of "creative similarity", as this term can help capture the dynamic interplay of various judgments of similarity and distortion that occurs during the construction of a relevant set of modelling practices in science.

Chapter 5

The Creative Similarity Account of the Means of Representation

In this final chapter I propose a specific account of the means of representation, the *creative similarity account of the means of representation.* To do so, I bring together central points discussed in Chapters 1 and 2 about the general problem of scientific representation, as well as the methodological considerations and examples examined in Chapters 3 and 4. Proposing a specific account of the means of representation has two purposes. It is firstly an attempt to elucidate what exactly an account of the means of representation is. There is general agreement in philosophy of science that the question about the constituents of representation is different from the question about the means. There is also certain agreement that studying the means of representation requires adopting some kind of practical enquiry or practice-based perspective. Very few other indications are found in the debate on representation on what it is required to advance an account of the means of representation though. Proposing the *creative similarity account of the means of representation* was in itself an exploration of what an adequate account of the means of representation requires. As outcome of this exploration and as a way of recapitulating the main arguments of the thesis, I offer in Section 5.2 four desiderata for any attempt to develop an adequate account of the means of representation.

The second purpose of developing a specific account of the means of representation is to accommodate the role of similarity in scientific representation in a satisfactory way. The concept of similarity has been a constant object of dispute in the debate of representation since the semantic turn and the shift of the attention to the study of scientific models. I discussed in Chapter 1 how some philosophers of science attempted to respond to *R1*, or the question about the constituents of representation, referring to a relation of similarity between a model and a target (French 2003; French and Bueno 2011; Bartels 2006). This proved to be problematic after considering Goodman's (1968) logical argument against similarity and the argument from misrepresentation. In Chapter 2 I then claimed that, on the contrary, similarity has the potential to be, an adequate notion with which to respond to *R2* or the question about the means of representation. However, not all characterizations of similarity are adequate to develop an account of the means of representation. An adequate characterization has to emerge from actual uses of judgments of similarity in scientific practices of representing. In Section 5.3 I will characterize similarity as *creative similarity* and explain why this is a suitable notion to help advance an account of the means of representation. The main reason suggested will be that *creative similarity* is a concept capable of capturing the very particular way in which judgments of similarity effectively intervene in the construction of a plurality of successful models in science: that is, they intervene in generative interplay with judgments of distortions towards the establishment of norms and uses that help epistemic agents pursue their representational aims. Finally, I will describe how the *creative similarity account* fulfils the previously mentioned desiderata, and therefore is an adequate account of the means of representation that advances insight on the problem of the epistemic success of representation.

First, I open the chapter by arguing in Section 5.1 that *understanding* should be considered the overarching aim of scientific practices, and fruitfulness a fundamental value when characterizing epistemically successful representations. The *creative similarity account of the means of representation* and the view of understanding as the overarching aim of science support and strengthen each other.

5.1. The quest for understanding

Scientific models often idealize, simplify, generalize, or otherwise distort aspects of the world they represent. I have argued throughout this thesis that the question about the means of representation concerns the epistemic success of representations. But how can scientific models be epistemically successful if they systematically idealize, simplify, or distort their targets? One response has been to claim that, despite the distortions they entail, epistemically successful models contain true descriptions of the world (da Costa and French 2003 refer to "partial truth"; French and Bueno 2011 to "quasi-truth"; and Chakravartty 2010a to "approximate truth"). Models might not be truth-apt in themselves but they are expressible in statements about the empirical world whose truth or falsity can be evaluated. Under this conception, a model *entails* or can be transcribed into truth-apt propositions (see Bailer-Jones 2003: 60).

This response is, as it stands, not entirely satisfactory. If truth is the measure of the epistemic success of a model, it is hard to explain why models that contain strong idealizations and entail few true propositions are kept when *truer* and less idealized alternatives are available. The ideal-gas law is an integral part of thermodynamics, even though it describes gases as comprised of dimensionless, spherical molecules that exhibit no mutual attraction, and even though there are *truer* alternatives available, such as the van der Waals equation (Elgin 2017a: 15; Woody 2015: 4). Likewise, making a model truer, that is, adding more elements to it that can be transcribed into true propositions, doesn't automatically render it more successful at providing epistemic access to a target in the world. Some truths might be trivial and not worth knowing, while some falsehoods might be illuminating approximations and useful idealization that are not worth dismissing (Elgin 2002: 22). It would be unwise to say that a scientific model that reveals illuminating idealizations is epistemically idle, and it would be gratuitous to say that a model that only produces trivially true propositions is epistemically successful.

In some cases, adding more true content to a model can even be detrimental to its results. We might want to construct a model that helps identify core causal factors that give rise to a phenomenon. In that case, we would have to build a minimalist model (following Weisberg and Elliott-Graves 2014). Schelling's (1971) model of segregation isolates one essential aspect of racial segregation, namely, its link to individual perceptions of difference. If we add more true assumptions to the model, such as the heterogeneity in the perception of difference among individuals or socio-economic conditions of the city, we are definitely adding *relevantly true* elements to the model, as individuals perceive in heterogeneous and complex ways, and socio-economic conditions do determine segregation. However, these assumptions would render Schelling's (1971) model incapable of identifying a fundamental key factor that gives rise to segregation and that the original model managed to uncover, namely, individual perceptions of difference that are not linked to explicit preferences for segregated neighbourhoods. Truths, even if not trivial, can obscure the main aspects that a model tries to highlight. If we want to maintain the idea that some true propositions are involved in the success of scientific modelling, we have to recognize that it requires much more than that. Using Elgin's (2017a) expression, models might have to be "true enough", but not more than enough. She uses the term "felicitous falsehoods" to denote the set of literally false propositions, and the set of non-truth-apt instances, such as pictures, diagrams, physical models, that can contribute to expand our comprehension of the world despite not being literally true (Elgin 2002; 2004; 2017a).

Following from here, an alternative way of addressing the question of how scientific models can be epistemically successful if they distort, idealize, simplify their targets is to characterize epistemic success in terms that do not mainly involve the increase or approximation to truth. I would like to suggest, following Elgin's (1996, 2004, 2017a) non-factive view, that the epistemic success of scientific models should be primarily described in terms of the *understanding* they afford (see also de Regt 2017, Potochnik 2015, Grimm 2006, Kvanvig 2003, and Zagzebski 2001 for alternative accounts of understanding). This idea has consequences for the characterization of an account of the means of representation, as I will spell out below. Understanding involves grasping together states of affairs, revealing connections and patterns, providing insight about facts that had remained unnoticed until then (Elgin 2004; Cooper 1995). To understand an aspect of the world, or of a domain, is to make sense of it, to ask questions about it, to connect it and apply it to other cases. Understanding is also a cognitive achievement, as it produces a sense of grasping, of mastery, of awareness of the object understood in the agents that possess it (Potochnik 2015: 72; see also Grimm 2012; Pritchard 2010; Zagzebski 2001).[48] Knowledge, rather than understanding, has been the

[48] It is not possible to go into the details of the numerous positions on understanding in contemporary epistemology here. I am mainly following Elgin's (1996; 2004; 2017a) characterization here. But, briefly, Grimm (2012) emphasizes the sense of grasping that understanding produces; Pritchard (2010) claims that understanding is a cognitive achievement that is worth pursuing for its own sake, differently to propositional knowledge; Cooper (1995) argues that essential to understanding is *epistemic ascent*; and Zagzebski (2001) claims that understanding produces a state of *conscious transparency*: that is to say, it is impossible to

main focus of epistemology for a very long time. And knowledge, in its traditional definition, consists in amassing discrete pieces of true propositions that describe individual, separate facts (Elgin 2017a: 13).

An advantage of defining epistemic success in term of understanding is that it can better account for the achievements of actual scientific research. Science has succeeded in the past and still succeeds today in producing effective representations that we can use, apply, further explore, and employ to generate new questions. If we focus on knowledge and the truthfulness of representations as the ultimate goal of science, we are forced to see the accomplishments of scientific practices as more limited. Scientific representations of the past are considered largely false today, and models in use today idealize and distort to a great extent. As Elgin (2017a) puts it:

> Actual science is cognitively reputable –indeed, estimable. So an adequate epistemology should explain what makes good science cognitively good. Too strict a commitment to truth stands in the way. Nor is science the only casualty. In other disciplines such as philosophy, history, political science, and economics, as well as in everyday discourse, we often convey information and advance understanding by means of sentences and other representations that are not literally true. An adequate epistemology should account for these as well. (Elgin 2017a: 15)

The task of epistemologists and philosophers of science should be to bring light into actual epistemic achievements, into what real scientists, real practices, real tools have attained and continue to attain, instead of focusing on describing ideal states of enquiry in which complete knowledge is reached (Wimsatt 2007: 5; Elgin 2017a: 15-6). Shifting the attention to understanding as the overarching goal of science and relaxing our commitment to truth can help accomplish this task.

An additional advantage that Potochnik (2015: 72-4) identifies in considering understanding to be the overarching aim of scientific practices is that we don't have to include an intermediate step to explain the epistemic success of representations that involve idealizations or other forms of distortion. That is to say, it is traditionally assumed that there is gap that needs to be bridged between the recognition of the idealizations that scientific representations contain and the successful pursuit of the aims of science (ibid.: 71). A traditional way of dealing

understand without being aware that one understand, while one can know without knowing that one knows.

with this gap consists in affirming that science aims for truth, and that idealized models must be de-idealized in order to fulfil their epistemic aim. Another way of dealing with it does not hold that de-idealizing is necessary but it still affirms that there is a gap to be bridged to be able to use "false" models to achieve "truer" theories (Wimsatt 2007; in Potochnik 2015: 72). On Potochnik's approach, though, "nothing has gone wrong with or is lacking from idealized models, and no intermediary step is needed for idealized models to achieve the aims of science" (ibid.: 72). What we need is to rethink what the aims of science are, other than truth, so that idealizations can directly contribute to science's epistemic success (ibid.). There might be an open-ended list of particular scientific aims, Potochnik adds (2015), but considering understanding its central epistemic aim is a satisfactory way of not making models that contain falsehoods intellectually suspect. As Elgin argues, there is no contradiction in saying that felicitous falsehoods advance understanding (2010: 1).

How do considerations of understanding as the central goal of science affect the debate on the means of representation? At the beginning of this thesis, I identified the question about the means of representation, or *R2*, with the problem of the accuracy or faithfulness of representation, following common characterizations by Contessa (2007, 2011), Frigg (2006), and Suárez (2003, 2010, 2015). Then, I clarified in Chapter 2 that identifying the problem of the means of representation with the accuracy, faithfulness, usefulness, or fruitfulness of representations, among other values, has not exactly the same implications. What all these values attributable to representations have in common is that they involve some kind of epistemic success, characterized in one way or another. So for most of this thesis, I remained neutral about how I precisely understood the epistemic success of representations, and referred to the problem of the means of representation as broadly concerning the production of "epistemically successful representations". I advanced a criticism of Contessa's (2007a, 2011) account, as he identifies similarity as means of representation with the *overall faithfulness* of representation. I rejected this because it is attached to a conception of similarity as a relation between two terms, the vehicle and the target of the representation, and conceals an aspiration to perfectly copy the world. This view hinders the possibility of truly considering the role of epistemic agents in representational practices.

Now I would like to argue that a broader difficulty with associating the means of representation with the faithfulness of representation is that faithfulness (also truthfulness and accuracy) are values too closely connected to knowledge –

taken as the sum of true propositions – as the overarching aim of science. If we adopt a perspective where understanding is the overarching aim of science instead, we should re-evaluate the specific way of conceiving successful representations, especially if we attempt to advance a similarity-based type of account of the means of representation. We just saw that models involve multiple falsehoods and that augmenting the amount of true assumptions entailed by a model does not necessarily augment the positive results that the model produces. If this is correct, *truthfulness*, *faithfulness*, and *accuracy* are not the main values that we are after when constructing models. I would like to argue that the epistemic success of scientific models should be more adequately characterized in terms of the *fruitfulness*, *effectiveness*, or *usefulness* of models. It is easier to see how fruitful, effective, and useful models can be epistemically successful, affording understanding of their target, despite the falsehoods and non-truth-apt elements they might contain.[49]

The *creative similarity account of the means of representation*, described in more detail in section 5.3, takes fruitfulness as a central value of epistemically successful representations. The notion of fruitfulness fits particularly well with the idea that understanding is the overarching aim of science. Fruitful representations usually produce a sense of grasping of the targets represented, of mastery of the models used to represent, in a way that allows epistemic agents to use those models to ask new question, open new paths of enquiry, and further apply the model results to other situations. The case studies I discussed provide some evidence in support of this claim. The MBM can be considered a fruitful model that advanced a great deal of understanding of its target, the Mississippi tributary system. It allowed engineers to make new predictions about the progression of floods in a way that even "exceeded the expectations of its inventors" (Robinson 1992: 292). Also, the MBM helped develop new modelling techniques in hydraulics that were later applied to the construction of other models. The SFBM was a fruitful model as well, that permitted the exploration of new engineering possibilities in the San

[49] Alternatively, we could argue that terms like *accuracy* or *faithfulness* do not necessarily have to implicate approximation to truth, but could be understood as the adequacy of models with respect to the particular epistemic goals of a practice of representing. However, this is not the common use of the terms faithfulness and accuracy in the debate of representation. For instance, Frigg (2010: 130) describes a faithful representation as one that produces true claims about a target; and Contessa (2011) defines a faithful representation as one capable of providing sound inferences about a target, that is, *valid* and *true* conclusions about it. The same is the case for *accuracy*. Toon (2010: 87) takes the accuracy and the realism of a model to be equivalent, and Suárez (2004; 2010) identifies "accurate, complete, and true representations". I shall hence take the terms accuracy and faithfulness as equivalents to truthfulness, and focus the attention on other values more closely linked to the attainment of understanding to define the epistemic success of representation.

Francisco Bay. Apart from testing the Reber's plan, the SFBM was employed to investigate the construction of barriers at Dillon Point and Chipps Island, among others, and opened new directions of research, such as investigation of pollution and shoring patterns, in the Bay (Huggins and Schultz 1967). This is not just a characteristic of scale models, or of models in engineering. The case of research with model organisms to investigate alcohol addiction is another good example of the close connection between fruitful models and understanding. Unexplored relevant features of human behaviour were disclosed in the practice of modelling alcoholism with mice, and further enquiries about the relation between conduct and biological response in organisms were developed as consequence of it.

Some might want to argue that these and other examples of scientific models also entailed some truths, or can be transcribed into true propositions. I am not denying that. Following Elgin (2017a), these models have to be "true enough" to be effective. The argument that understanding should be favoured over knowledge as the overarching aim of science is that, to be able to make sense of the epistemic role that strong idealizations and rough approximations play in scientific models, paying attention to the true propositions they entail is not sufficient. In some sense, understanding is more demanding than knowledge, as it requires a broader appreciation of the phenomenon that is the object of study, as well as integration, organization of a topic, a domain, a network of instances (see Cooper 1995). For this reason, an account of the means of representation that attempts to offer insight on how epistemically successful representations are achieved should take epistemic success as the enlargement of understanding, and successful models mainly as fruitful, effective, useful, among other possible values that are not exclusively attached to the approximation of truth. The *creative similarity account of the means of representation* conceives understanding as the overarching aim of science, and fruitfulness as the main value involved in the characterization of successful models that are have been produced with creative similarity as means of representation.

There is an important additional consequence of taking understanding as the overarching goal of science and measure of epistemic success. The distinction between the cognitive gain obtained from science and from other domains such as the arts to some extent blurs. Fictions, metaphors, depictions, and exemplars are commonly used in the arts to help afford understanding of aspects of the world, in analogous ways as models and felicitous falsehoods in science. Goodman and Elgin (1988) have openly defended the idea that the arts function cognitively. Works of art can be epistemically rewarding, as they reorient us, enabling us to see things differently from the way we saw them before. Encounters with artworks frequently leave us with the feeling that we have learnt something, not only about the artwork or the author but about the extra-aesthetic world (Elgin 2017b: 29-30). And this is not that different from what a laboratory experiment or thought experiment can do. They isolate some elements from their environment, modify them, even create new items that are nowhere to be found in nature. Then, when scientists look back at the natural environment, they identify new elements and connections (ibid.: 35). Putting it in Goodman's (1968) words, "the arts must be taken no less seriously than the sciences as modes of discovery, creation, and enlargement of knowledge in the broad sense of advancement of the understanding" (ibid.: 102).

Recognizing the parallels between the epistemic achievement of scientific and artistic representations presents some challenges for traditional theories of knowledge, as it demands a more flexible conception of the boundaries of epistemology. Placing understanding at the centre of epistemology's interest can provide the flexible boundaries that knowledge cannot. In a different paper (Sánchez-Dorado 2017), I maintained that the integration of debates in philosophy of science and aesthetics, especially concerning the problem of representation, can be incredibly insightful. Yet, methodological clarity is required to justify the integration of concepts from the arts and the sciences into a single account, something that is not always accomplished in the literature (ibid.: 22). In the next section, I introduce some examples from the arts to help address the problem of the means of representation in science, in particular to characterize the notion of *creative similarity*. To justify the integration of elements from the arts into an account in philosophy of science, I adopt the methodological framework proposed by Goodman and Elgin (1988), which assumes that epistemology, as a normative discipline, should be equally able to account for the varied, and often non-propositional, vehicles through which scientific and artistic representations succeed in affording understanding of the world. Understanding provides the foil against which a range of practices that have occupied philosophers of science and philosophers of art in separate debates can sensibly and productively come together. That doesn't mean that artistic and scientific representations are equivalent, or have the same results and uses, but that "the difference between the arts and the sciences is more practical than epistemic" (Elgin 2017b: 40).[50] Fruitful representations, both in science and in art, allow us to perceive previously unnoticed connections in the world, address new problems and perform innovative actions as a result of the encounter with them. *Creative similarity*, I contend, is a central means of constructing such fruitful representations.

5.2. Desiderata for an account of the means of representation

As a way of summarizing the important ideas I have presented so far, and before presenting the *creative similarity account*, I would like to return to one of the initial questions addressed in the thesis: what is an account of the means of representation? In Chapter 1, I distinguished between the constituents and the means of representation. Then, in Chapter 2 I disputed the three main arguments against similarity as means of representation. And in Chapter 3 I argued that the projects of PSP and iHPS offer adequate frameworks with which to advance an account of the means of representation. Now I would like to re-examine these issues in more detail, with the additional aid of the conclusions achieved from the case studies investigated. There is little guidance in the contemporary debate in philosophy of science about what is required to address the problem of the epistemic success of representation in a systematic way. So I will propose four desiderata to develop an adequate account of the means of representation, either focused on similarity or other possible means. Then, in section 5.3, I show how the *creative similarity account of the means of representation* that I propose fulfils these desiderata, and can be therefore taken as a step forward in the pursuit of a solution to the problem of how epistemically successful representations are produced in science.

A first important aspect that must be re-examined is Suárez's (2003, 2010) original definition of the term "means":

> *Means*: For any source–target pair (S, T) at a given time and in a given context: R' is the means of the representation of T by S if some user of the

[50] See Elgin (2011) for a more nuanced specification of the differences between scientific and artistic representation, concerning the specific case of representation by exemplification. While *density* and *repleteness* are symptoms of aesthetic representation, they are not of scientific representation.

> model employs R' (at that time and in that context) to draw inferences about T from S. (Suárez 2010: 93)

Here the means of representation are defined as the relation actively employed at a given time by epistemic agents to draw inferences about a target from a model. I would like to argue, in view of the practice-based approach to the problem of representation I developed throughout the thesis, that this definition is too narrow to characterize what the means of representation are, insofar as it only considers the *relations* established by agents at the end of a practice of representing. If the means are actively-employed relations between a model and a target, to be able to say something about what these relations are and how they allow epistemic agents to make fruitful inferences about aspects of the world, we have to examine how they came about. Therefore, an account of the means of representation should primarily concern the means of representation understood as the representational *resources* used in the construction of successful models. Models are themselves the product of employing those representational resources during their production. To put it in other words, to be able to offer a constructive account of the means of representation that tells us something about the *relations* employed by epistemic agents to make inferences from models about targets, we have to consider the representational *resources* used to construct such models in the first place. Moreover, a scientific model obtained at the end of a practice might not manifest its means of representation conspicuously (Suárez 2003: 229). A careful study of practices is necessary to discern what the means of representation (as *relations*) are by paying attention to the representational *resources* used in the process.

Under this expanded definition of "means", offering a similarity-based type of account of the means of representation would entail claiming that similarity – in any form described – is primarily a *resource* of the construction of a series of scientific models. And, only as a result of that, such an account would also claim that epistemic agents employ similarity *relations* between certain representations and certain target systems to make inferences about the latter. Other possible accounts of the means of representation would for instance claim that convention or exemplification are *resources* used in scientific practice to produce successful representations. And, as a consequence of that, they would claim that exemplification or convention are *relations* employed at the end of the practice of representing between certain vehicles and targets to make inferences about the latter.

This revised characterization of the notion of means of representation, that captures and expands Suárez's (2003) original definition, fits with van Fraassen's (2008) occasional use of the term "means" in his proposal. Van Fraassen (2008) argues that similarity and distortion are not the "core" of representation but the "means to an end" used in the practice of representing (2008: 14-5). I take this to mean that similarity and distortion are for van Fraassen principally *resources* used throughout practice of representing with the aim of constructing accurate scientific models, and only derivatively *relations* employed between a model and a target. From this interpretation of the implications of van Fraassen's claim, I argue that the weight of an account of the means of the representation needs to be moved to the study of the resources that allow epistemic agents to implement productive representational practices that eventually generate fully-formed models with which it is possible to make inferences. In the next section, it will become clear how these two elements, *resources* and *relations*, are interrelated in a specific account of the means of representation, namely the *creative similarity account* that I propose, but also how the focus of study should be the representational *resources* used to construct successful models.

An additional aspect that should be clarified regarding an account of the means of representation concerns its scope. I have only partially addressed the issue of the level of generality that an account of the means of representation is supposed to have. In Chapter 2 I discussed the argument from variety, and claimed that the fact that there might be a plurality of means of representation was not a reason to abandon the project of advancing a specific (or various specific) account(s) of the means of representation. If there is for instance evidence, as I showed in Chapters 3 and 4, that judgments of similarity are used as representational resources in a variety of modelling practices in science, and thus advancing a similarity-based type of account of the means of representation is well motivated. Here, I would like to resist the idea that an account of the means of representation must aim to *resolve the problem* of the epistemic success of representation *in general*. What I tried to achieve with the practical investigation developed in Chapters 3 and 4, and the advancement of the *creative similarity account* in this chapter, was to offer insight, with some degree of generality, on a problem that is complex and difficult to encapsulate under a single view, namely the problem of the epistemic success of representation in scientific contexts.

Certainly, philosophers like Giere (2004) have maintained that similarity is *in general* the way or "the most important way" in which scientists produce models that successfully represent the world, while others like van Fraassen (2008) and Contessa (2007a) have appealed to some form of structural similarity to explain *the way* in which representations produce faithful results. Although I have treated these proposals as accounts of the means of representation, and extracted some helpful lessons from them, these philosophers focus on similarity as the central if not the only way of resolving the problem of successful representation. My approach is more plural than that. The *creative similarity account of the means representation* that I summarize in the next section is meant to be one among other possible accounts of the means of representation, which can co-exist and complement each other. Although I do not expand on the characterization of other possible accounts of the means of representation in this thesis, the advancement of for instance an account focused on convention as means of representation, sustained on a practical enquiry on how scientists formulate judgments of conventions and stabilize the use of certain symbols and codes in a relevant set of modelling practices, could complement and enrich the conclusions achieved by the *creative similarity account.*

It is important to stress that the fact that there might be a plurality of means of representation does not entail that that plurality is endless or impossible to tackle. This assumption was implicit in the formulation of the "problem of style" that Frigg (2006) and Frigg and Nguyen (2016) offered, especially when they argued that ultimately each scientific model might have been construed in a very specific, unique style. To this argument I replied in Chapter 2 that, even if it is right that in some sense every representation is unique and every practice unrepeatable, to bring light into the problem of the epistemic success of representation it is essential to highlight commonalities between different practices, establish comparisons, identify recurrent resources, and describe dynamics and patterns that appear in a variety of circumstances. It is indeed problematic to sustain a universal type of account of the means of representation, but it is equally problematic to give up on the attempt of offering any type of generalization about how successful representations are frequently produced in science.

A further reason why an account of the means of representation has to be restricted in scope is that it deals with the problem of the epistemic success of representation, and success needs to be defined in relation to particular goals and values. Even if we take understanding as the overarching aim of science, epistemic success regarding scientific representations can be specified as fruitfulness, precision, generality, capacity to predict, effectiveness. Also, we must consider non-epistemic goals and values that directly affect the characterization of what a successful representation is. It is neither possible, nor desirable, to offer a universal response to the problem of the epistemic success of representation, as the problem itself has to be narrowly framed to be meaningful. Moreover, there are trade-offs among desirable virtues of models, such as between generality and precision (Potochnik 2015). The scope of an account of the means of representation, I contend, must be that of offering some conclusions, even if partial, about how epistemic communities produce successful representations, by focusing on the functioning of a specific means of representation, and on the achievement of a particular kind of successful representation – that is, representations that are either predictively accurate, fruitful at opening new paths of investigation, useful at fulfilling some non-epistemic goals, etc.

After clarifying these two important points about what an account of the means of representation is, I would like to suggest four desiderata for any attempt to develop an account of the means of representation. There is a certain agreement in philosophy of science that responding to the question about the epistemic success of representation requires adopting some kind of practice-based perspective (Suárez 2010; van Fraassen 2008; Weisberg 2013). But beyond that, it has not been sufficiently spelt out in the debate of representation what exactly advancing a satisfactory account of the means of representation comprises. These desiderata are not general conditions for representation or for accurate representation. They are instead important methodological or epistemological aspects that should be considered to approach the study of the means of representation in a satisfactory way. The investigation developed throughout the present thesis was itself an exploration of the requirements to advance such an account.

1. An account of the means of representation requires the development of a **practical enquiry**. A practical enquiry would usually have the form of a case-by-case type of study (Suárez 2010: 91-3), but could also include the study of different examples in less detail and comparisons between practices. Developing a practical enquiry is crucial because it allows us to identify the representational resources used to construct successful representations, and, as a consequence, the relations actively employed by epistemic agents to make inferences about a target from a model. The conclusions achieved

> from the practical enquiry will comprise the descriptive component of an account of the means of representation. Chapter 4 of this thesis works as an illustration of the type of study that needs to be done when developing a practical enquiry. Examining two scale models in hydraulic engineering afforded a great amount of information about the vehicles, targets, users, purposes, and contexts intervening in concrete modelling practices. More importantly, it allowed us to observe that the use of certain judgments in scientific practice – judgments of similarity and distortion – aided decision-making about the design of the models, the features to be included and excluded, and the calibration and evaluation of the process concerning the model-prototype relation. Complementary to these case studies, in Chapter 3 I pointed out various examples of modelling practices in which judgments of similarity and distortion played analogous functions in representing, such as in the use of models in synthetic chemistry, the construction of physical models in economics, and research with model organisms. These other cases would need to be analysed in further detail, but together with the case studies in Chapter 4 provide empirical evidence to advance some illuminating conclusions on the problem of the epistemic success of representation. In other words, the practical enquiry developed on actual examples from different scientific disciplines is the descriptive basis for the development of the *creative similarity account of the means of representation*. An alternative account of the means of representation, focused on exemplification or convention, for instance, would also have to develop a practical enquiry that allows its proponents to identify the representational resources used in modelling practices to construct successful representations.

Presenting this point as the first desideratum is not trivial, given that for at least some philosophers of science in the debate of representation developing a practical enquiry is not treated as indispensable when addressing the question of the epistemic success of representations. I pointed out the limitations of Contessa's (2011) proposal in Chapter 2 for not engaging with actual practices. He developed instead a rational reconstruction of scientific modelling, which concluded in a similarity-based account that, I argued, did not match some relevant observations of how similarity participates in actual practices.

2. A practical enquiry, as described in the first desideratum, is not enough to develop an account of the means of representation. An openly **normative component** should also be integrated in the account. Describing the particularities of individual models does not establish an account of the means of representation if a normative element is missing in it. Representational practices are more than the sum of individual actions. They are part of regulated systems that concern a community and their epistemic (and non-epistemic) goals, values, standards, skills. Any account of the means of representation, focused either on similarity, convention, exemplification, or other possible means suggested, would have to contain some normative claims about how those specific means participate in the production of successful representation through the entrenchment of norms and common uses in practice. The study of judgments in scientific practice is key to connect the descriptive and the normative components of an account of the means of representation. Judgments in scientific practice are not mere opinions, but commitments regulated by the agreement of the competent epistemic community. Offering some generic insight about the dynamics generated by the use of certain kinds of judgments – like the combination of judgments of similarity and distortion that I describe in Chapters 3 and 4 – is what constitutes the normative part of the account.

Including this point as a second desideratum is not trivial either. It is frequently assumed in the debate of representation, more implicitly than explicitly, that there is a tension between a descriptive and a normative way of approaching the problem of representation. In Chapter 1 I mentioned that philosophers like Frigg (2006: 50) helpfully point out that there can be two variants of an account of epistemically successful representation: a descriptive or factual variant, which would typically offer a taxonomy of possible means of representation; and a normative variant, which would judge how scientific representations *must* be constructed to be successful (ibid.: 59). Yet, I claimed that the two variants should be rather taken as two important components of a comprehensive account of the means of representation. The descriptive part would not be a mere enumeration of models or styles, but a case-by-case type of study as well, as described in the first desideratum. And the normative part would not establish a single rule on how representations *must* be constructed, but rather advance some conclusions about how certain representational dynamics have allowed in the past (and there are good reasons to believe that they could allow future practices) to stabilize and promote successful ways of constructing representations. The projects of PSP and iHPS discussed in Chapter 3 can help define an adequate methodological framework that facilitates the integration of the descriptive and the normative components in an account of the means of representation, since these projects do not view the integration of particular cases and generalizations as a confrontational process but as an interpretive endeavour.

3. An account of the means of representation would have to specify how the **epistemic success** of representations is specifically understood, and how the particular means studied help achieve that form of epistemic success. I argued earlier that an adequate account of the means of representation should focus on the functioning of a specific means of representation in relation to the achievement of a specific form of successful representation: that is, representations that are predictively accurate, fruitful, useful, general, precise, etc. The reason for this is that success needs to be defined in relation to particular goals and values, even if we take understanding as the overarching aim of science. Also, there are frequently tensions and trade-offs between different forms of epistemic success. The *creative similarity account of the means of representation* that I propose is an example of an account that recast epistemic success precisely in terms of fruitfulness, and takes the pursuit of understanding as the overarching aim of science. An alternative account of the means of representation could also recast epistemic success as usefulness, faithfulness, or accuracy in generating predictions. In any case, these different accounts should be able to explain how the means of representation studied – similarity, convention, exemplification – help achieve epistemically successful representations, in accordance with the specific characterization of epistemic success we decide to embrace.

Making this third desideratum explicit is fundamental because proposals in the debate of scientific representation frequently avoid making open claims about how they understand epistemic success, or just assume it concerns the faithfulness (increase of knowledge) of representations. However, since there are different ways of characterizing epistemic success, some of which might be incompatible with others (i.e. precision vs. generalization), it is important to limit the scope of the conclusions a specific account of the means of representation makes regarding epistemic success.

4. An account of the means of representation should be able to accommodate the potential epistemic value of **idealizations** and other forms of **distortions**. This fourth desideratum follows from the previous point. Scientific models that we usually recognize as highly successful contain abstractions, idealizations, and simplifications. I argued above that defining the overarching aim of scientific practices in terms of understanding or in terms of the increase of knowledge has different consequences for an account of the means of representation. In the former case, an account of the means of representation would be able to accommodate the value of felicitous falsehoods more easily. Meanwhile, when truth or the increase of knowledge is considered the central aim of science, there are more difficulties in accommodating them, since felicitous falsehoods do not provide true statements about the target. In Chapter 1 I presented the argument from misrepresentation against similarity as constituent of representation, and in Chapter 2 I discussed the argument from misrepresentation against similarity as means of representation. Now, I would like to reiterate that an account of the means of representation, whether it is focused on similarity, convention, exemplification or other possible means, should be able to do more than claim that some representations are epistemically successful *despite* including distortions. It should also be able to elucidate how, in some cases, including more or stronger distortions can improve the epistemic success of models. Valuable distortions do epistemic work, so accounting for the phenomenon of misrepresentation includes describing how distortions can be highly valuable in certain scientific contexts.

This desideratum has direct consequences for the characterization of, for instance, the concept of similarity to sustain an account of the means of representation. When epistemic success is identified with understanding, the idea of similarity as means of representation can be compatible with the acknowledgement of the value of distortions, whereas when epistemic success is identified with the increase of knowledge, similarity would have to help produce truthful statements. My own account, sustained on the notion of *creative similarity*, is much closer to the former view, while accounts like Contessa's (2007a) that connect similarity with epistemic success understood as the overall faithfulness of a representation are closer to the latter.

This list of desiderata attempted to make explicit various important elements that should be considered to develop a comprehensive account of the means of representation. In the next session, I discuss the motivations to propose the *creative similarity account of the means of representation*, and specify how it fulfils the desiderata just listed.

5.3. The creative similarity account of the means of representation

The remainder of this chapter is dedicated to discussing a specific account of the means of representation, the *creative similarity account of the means of representation*, which I would like to propose as the result of the investigations developed throughout the thesis. Some details of this account would need to be spelt out fully in future work, especially regarding how the analysis of other case studies would complement or adjust the ideas presented here. Also, the *creative similarity account* is suggested as one specific account of the means of representation, which aims to illuminate part of the debate concerning the production of successful representations in science. But addressing the problem of the epistemic success of representation completely is an endeavour that requires the development of possibly various accounts focused on different means of representation, and on various ways of recasting epistemic success.

In few words, the *creative similarity account of the means of representation* states that *creative similarity* is the means of representation in a significant set of modelling practices that we acknowledge for their epistemic achievements. Among those modelling practices, we have to include the case studies of scale modelling analysed in Chapter 4, as well as other analogous practices of constructing physical models in engineering or the geosciences; the various examples of representational practices suggested in Chapter 3, such as research with model organisms to investigate human conditions; and potentially other cases that I haven't explored in this thesis, but that could be further investigated with the *creative similarity account* as framework of reference. The *creative similarity account of the means of representation* identifies the epistemic success of representation, broadly conceived, with the growth of understanding, and more specifically defined, with the production of fruitful representations that afford understanding by opening new paths of enquiry, asking new questions and pursuing new strategies of investigation and methodologies. I have argued that an account of the means of representation primarily characterizes the representational *resources* used in the production of successful representations, and derivatively the *relations* epistemic agents actively employ between certain models and targets to make inferences. Accordingly, the *creative similarity account of the means of representation* claims that the formulation of a variety of judgments of similarity, in interplay with judgments of distortion, are the representational *resources* of the construction of successful representations in a range of practices of modelling like the ones studied in this thesis. Thus, concerning that range of practices of modelling, we can claim that the *relation* actively employed by epistemic agents to make inferences from a model about a target is a relation of creative similarity.

But why "creative similarity"? In *The Dappled World*, Cartwright (1999) argued that "the whole point of the tradition that generates both the syntactic and the semantic views is the elimination of *creativity* – or whim – in the use of a scientific theory to treat the world" (1999: 184-5, my emphasis). The syntactic and (at least part of) the semantic views are equally cases of the "vending machine" approach, by which a scientific theory is fed with some input and drops out the desired output in the form of a fully formed model (ibid.). There is no place for genuine creativity in this approach because scientific models seem to be already contained in the theory, waiting to be extracted from it. On the contrary, Cartwright contends, producing epistemically successful models is a *great creative achievement* that goes well beyond the principles of any of the theories involved in their construction (ibid.). An adequate account of the means of representation should capture the hard work, creative imagination, and human fallibility that goes into the construction of successful scientific models, contrary to what many proposals in the syntactic and semantic view accomplish, following Cartwright's criticism. Practice-based approaches to the study of scientific models certainly recognize some of these fallible, creative aspects of the process of modelling, but they frequently assume that similarity-based accounts of representation are somehow incompatible with practice-based approaches (e.g. Knuuttila 2009). What I would like to argue is that if similarity plays a role in the construction of epistemically successful scientific models, then that similarity has to be understood as humanly shaped and genuinely creative. And, since I showed that similarity indeed plays a role, through the formulation of a plurality of judgments of similarity, in the process of design, construction, and evaluation of at least a relevant set of scientific models, the notion of *creative similarity* seems suitable to capture the nature of the similarity that emerges from the study of practices. Cartwright's criticism of a well-known limitation of the syntactic and great part of the semantic view is one important motivation to use the concept of creative similarity (see also Cartwright et al. 1995; Cartwright and Suárez 2008).

A second motivation to use the concept of creative similarity in the context of advancing an account of the means of representation is found in a recent paper by Marcos (2011: 195), in which he contends that, in both science and art, the "creative discovery of similarity" is fundamental for a variety of cognitive practices, including representation. Skilful interpreters are required for creative discoveries of similarity to take place, since "we are not talking about a given similarity and nothing else, like the one Goodman rightly criticizes, but one drawn up by the subject, the fruit of his creativity" (ibid.: 207). Marcos's (2011) emphasis on the triadic relation of similarity, which is established between two objects (vehicle and target) and an active and skilful subject, resonates with the analysis of actual representational practices offered in this thesis. I described how resourceful epistemic communities employ a variety of judgments of similarity in representational practices, in combination with judgments of distortion, in a dynamic process from which uses, norms, standards, concerning model-world comparisons emerge and are stabilized. In that particular sense, we could talk about a creative discovery of similarity, or use the term *creative similarity*, adopting it from Marcos (2011), to describe the character of similarity as it emerges from actual practices of representing.

A more careful examination of debates on creativity, and in particular on the function of creativity in scientific practice, could help advance a more refined characterization of the notion of creative similarity. I am not developing that line of investigation here, but there is a growing literature on creativity that combines problems traditionally discussed in aesthetics with approaches in philosophy of mind and philosophy of science that would complement the account I present in insightful ways. For instance, looking at these debates could help to spell out where creativity precisely lies (subjects, actions, products), what its value exactly is, and what different forms creative practices have (combinatorial, exploratory, transformational) (see Boden 2004; Gaut 2010; Kieran 2014; Hills and Bird 2018; Pérez-Ransanz and Ponce 2018). For now, I would like argue, drawing on the common understanding of the notion of creativity, as well as on the aforementioned works that motivate the use of this term, that adding the adjective 'creative' to the concept of similarity can help emphasize three important ideas. One, it stresses that similarity, as it effectively intervenes in the production of successful representations, concerns not a relation of sharing of properties between two terms (vehicle and target), but a three-term relation where resourceful epistemic agents define, through the formulation of various judgments of similarity, what relevant similarities are for the purposes of the practice. I identify conceptions of similarity as a two-term relations with accounts of "bare similarity", and contrast them with the idea of "creative similarity" that I endorse. Two, the idea of "creative similarity" can help make sense of the fact that similarities are intertwined and compatible with distortions (idealizations, simplifications, abstractions) in the practice of representing in science. Three, the adjective creative helps emphasize that similarity as means of representation is generative. That is to say, when employed as resource (and consequently as relation model-target) in modelling practices, creative similarity can help reveal previously unnoticed features of the objects in the world under investigation. For this reason, creative similarity and the production of fruitful representations that afford understanding of the world are closely related. I discuss these three ideas implied by the notion of creative similarity in subsequent subsections.

5.3.1. Creative similarity vs. bare similarity

In contemporary philosophy of science the notion of similarity has been considered "repugnant" (Quine 1969: 117), "empty" (Frigg 2006: 61), "useless", "a pretender, an impostor, a quack" (Goodman 1972: 443 and 437). Most of these critiques of similarity, I would like to argue, actually invoke a conception of similarity as "bare" or "pure" similarity. The idea of "bare similarity", as I am describing it, recalls a two-term relation between an object and a representation-as-copy of that object that attempts to imitate it. I showed in previous chapters that, when we approach actual practices, we observe that representing does not consist in copying the object of the representation. The project of imitating reality is doomed from the start, since there is no objective way of doing so, no aseptic and innocent eye with which to look at things (Goodman 1968: 6-8). In representing the world, we need to select, reject, organize, discriminate, depending on the needs and prejudices that regulate our use (ibid.: 8). In other words, copy theories of representation should be entirely rejected. Yet, the rejection of copy theories of representation should not impose the elimination of the notion of similarity from the debate on representation: only the idea of "bare" or "pure" similarity needs to be rejected.

It is certainly true that many philosophers of science accept that the relation of representation is at least a three-term relation. However, fewer take seriously what it means to say that *agency* and the *uses* of a epistemic community are, together with vehicle and target, ingrained in what a representation is. In Chapter 1 I argued that, for instance, French and Bueno (2011), despite recognizing the existence of agents in the practice of representing (with intentions and goals), consider them mere external factors surrounding the relation of representation. They believe that it would be disastrous to build subjects' intentions into the representational mechanism (ibid.: 886). One of the implications of this view is that French and Bueno (2011) implicitly conceive similarity as "bare similarity", that is, a two-term relation between a vehicle and target, independently from the uses of the epistemic community. In addition, the conception of similarity as bare similarity hides an aspiration to mirror the world. That is, it conceals the ideal that representational vehicles should be as similar as possible to their targets.

Probably Giere (2004), van Fraassen (2008), and Weisberg (2013) are, in opposition to this view, the clearest cases in recent philosophy of science that refuse to understand similarity as "bare similarity". As discussed in Chapters 2 and 3, Giere (2004: 747) defends an intentional conception of representation that conveys also an intentional conception of similarity. Agents are, for Giere, responsible for selecting and employing relevant similarities in the practice of representing. For van Fraassen (2008), the *use* of representation by epistemic communities establishes the representational relation and defines similarity. And for Weisberg (2013), when studying scientific representation we have to direct our attention at actual judgments of similarity that scientists make in practice and not at inaccessible, two-term relations of similarity between vehicle and target. I believe that the idea of *creative similarity* is partially compatible with the views on similarity that these philosophers endorse. But it is proposed as an attempt to stress points that they don't explicitly make. Namely, the idea of *creative similarity* is chosen, rather than "intentional similarity", a notion that Giere would employ, or "use-based similarity", a notion that van Fraassen could use, because creative similarity is more than the selection of relevant similarities by epistemic agents. It embraces the idea that similarities are combined with insightful dissimilarities and distortions by epistemic communities, in such a way that a productive interplay occurs. The result of the interplay is that unnoticed associations and relevant features are brought to the fore of the representational practice. In the next two subsections I develop these points in detail.

An example from the engineering sciences can illustrate the difference between bare similarity and creative similarity. Elgin (2010: 11-12) describes the tragic explosion of the space shuttle Challenger in 1986, because of a failure in its O-rings due to cold weather. The day before the accident, a report was sent to NASA containing data that pointed to the possible danger of launching the space shuttle. However, the evidence of the vulnerability of the O-rings in cold weather was obscured by a melange of other information that was also included in the report. The launch took place, the shuttled exploded, and the astronauts died (ibid; following Tufte 1997: 17-31). Elgin reads this example as a case of a representation that contained "the requisite resemblance between model and target", but nevertheless did not allow scientists to produce a fruitful representation that afforded the required understanding about the target (ibid.). This case is therefore used to reject the role of similarity (or resemblance in Elgin's terms) in the construction of epistemically successful representations, since even the existence of relevant similarity between the data in the report and the target was insufficient to produce adequate results. Representation, Elgin claims, also "must make the resemblance manifest" (ibid.).

Yet, I believe that similarity (or resemblance in this example) is plainly understood as "bare similarity" here. It concerns only a vehicle and a target, that is, the data displayed in the report and the O-rings of the shuttle. The resemblance was not manifest in the representation as it was not even noticed or investigated by the agents involved in the modelling. Very differently, the notion of creative similarity, in the description that I am offering, has to be selected, tested, and manipulated by the relevant epistemic community throughout the practices of representing. In the case of the SFBM, we can observe that scientists capitalized on multiple judgments of similarity and dissimilarity to construct a model of the San Francisco Bay that could test the effects of the Reber's plan. Tides were simulated by a mechanical system of pumps, the roughness of the sea bed by thousands of copper strips, and the Reber's plan reproduced by the inclusion of concrete barriers throughout the water tanks. And in this process, the selection and integration of the various elements composing the model was in great part facilitated by the formulation of those judgments of similarity concerning model-prototype relations, in the phases of design, construction, and evaluation. The process resulted in a remarkably fruitful representation, which allowed engineers to foresee the elimination of current movements in the Bay if the Reber's plan was implemented, and helped them to investigate additional engineering plans they hadn't planned to study before (US Army Corps 1963; Huggins and Schultz 1967). If this is correct, creative similarity is the means of representation of the practice of constructing the SFBM, whereas it was not involved in the very unsuccessful representational practice concerning the functioning of the O-rings in the case of the Challenger.

Bare similarity and creative similarity distance themselves with respect to the epistemic goals they can help attain too. There is a close link between knowledge – increase of true propositions – as the overarching aim of science, and faithfulness or truthfulness as the measures of the epistemic success of scientific models. I have doubts that creative similarity can be taken as the means of representation in an account of the means that recasts epistemic success in terms of truthfulness, as I discussed in the previous section. If adding new true beliefs to our corpus of knowledge is what we are after, it is difficult to see how creative similarity, which involves employing as resource a selective combination of judgments of similarity and distortions, can contribute to that endeavour. Bare similarity, mirroring, isomorphism, or identity would be in this framework more appropriate notions to sustain an account of the means of representation. However, I argue that the notion of similarity that emerges from the observation of actual practices of representation does not correspond to a conception of bare similarity, where a model ideally aspires to be as similar as possible – by sharing as many properties as possible – to the target. It is a conception of similarity that is intertwined with relevant distortions, to work as resources of epistemic communities with specific goals to pursue. Moreover, I have argued above that the conception of knowledge as the overarching goal of science, and consequently faithfulness as the measure of the epistemic success of representation, does not offer the most adequate framework to explain the achievements of actual scientific practices and the numerous felicitous falsehoods that models contain. A conception that takes understanding as the central goal of science, and fruitfulness, effectiveness, usefulness as ways of defining the epistemic success of representations would be better to account for the actual cognitive achievement of most scientific models. To use Elgin's (2010) words:

> Neither art nor science is, can be, or ought to be, a mirror of nature. Rather, […] effective representations in both disciplines embody and convey an *understanding* of their subjects. Since understanding is not mirroring, failures of mirroring need not be failures of understanding. (Elgin 2010: 2)

Failures of mirroring don't need to be failures of understanding because representations that afford understanding of targets in the world don't need to be highly truthful to those targets. Some scientific models might contain strong idealizations and be highly fruitful insofar as they allow scientists to connect that phenomenon with others, and apply the model to other situations and targets. Similarity as means of representation in this conception is quite different from mirroring or bare similarity: it is creative similarity, that is, it is selective, distortive, and generative.

5.3.2. Creative similarity is distortive

The claim that "similarity is distortive" might seem contradictory. However, this is only the case if we interpret similarity as "pure" or "bare" similarity. The term *creative similarity* can help capture the apparent tension of the assertion that similarity and distortion can be intertwined resources of the same practices of representing. In Chapter 2 I claimed that van Fraassen (2008) has been one of the few contemporary philosophers of science who has openly argued that the existence of distortions in the practice of representing is not a reason to undermine the role of similarity in it (2008: 15). It shouldn't be puzzling, he claims, that adding distortions to a representation, such as a caricature, is needed for the success of the representation, since both selective similarities and distortions can be "means to an end" of the practice. In any case, van Fraassen (2008) ultimately maintains his preference for a structural version of similarity to offer a general response to the problem of successful representation, and does not explicitly incorporate the potential epistemic value of distortions in it. Introducing the idea of creative similarity to advance an account of the means of representation can help emphasize this point. In Chapters 3 and 4, I suggested that whenever we localize the use of judgments of similarity in scientific practices of representing, like the construction of scale models or research with model organisms, we also find judgments of dissimilarity or distortion. The presence of well-reasoned distortions motivates the thought that similarity is not bare but *creative* when it functions as the means of representation. Defending the idea that creative similarity is the means of representation in a plurality of practices of representing does not automatically support any account of the means of representation based on similarity. On the contrary, proposing the *creative similarity account* is a way of offering some restrictions on the functions that similarity can play in representational practices, by emphasizing that it is an effective means when it appears in the right balance with insightful distortions.

An example from the pictorial arts can help illustrate how practitioners deal with the constant interplay of similarities and distortions, as much in artistic as in scientific practices of representing. Earlier I said that the focus on understanding as a central epistemic goal blurs some of the traditional distinctions between the cognitive gain that can be obtained from scientific and artistic representations (Goodman and Elgin 1988; Elgin 2017b). Incorporating examples from the pictorial arts into the debate in philosophy of science can help bring together fertile points in common regarding the role of similarity in representational practices.

The period of the artistic avant-gardes, in the last years of the nineteenth century and beginning of the twentieth century, was notorious for questioning traditional conceptions of representation. Avant-garde movements motivated aesthetic debates around the concepts of depiction and similarity, and new ideas (such as 'abstraction') challenged the status of realistic styles of representing. Wassily Kandinsky's treatise *Concerning the Spiritual in Art* (1911) is a fundamental theoretical essay from that period, which emerges directly from the artistic practices of depicting of its author. Kandinsky believed in the genuineness of abstract art. However, he had doubts about the need to reject the use of visual similarities in artistic creation. He asked himself: "must we then abandon utterly all material objects and paint solely in abstractions?" (Kandinsky 1911: 100). Trying to give a solution to this question, he elaborated a response that I reconstruct in two parts. In the first part, Kandinsky presents a strong rejection of similarity understood as 'conventional imitation of nature' or mirroring: "Nature has her own language, and a powerful one; this language cannot be imitated. [...] The *Stimmung* of nature can be imparted by every art, not, however, by imitation" (Kandinsky 1911: 123).[51] This is an open refusal of the idea of mirroring, imitation,

[51] In the original version of *Concerning the Spiritual in Art* this point is even clearer. While in the translation we read that Kandinsky rejected "mere representation" (1911: 54), the original states that he rejected the "Nachahmung der Naturerscheinungen", that is, the mere "imitation of the

or merely copying when explaining what the practice of representing in art consists in.[52] Then, the second part of Kandinsky's response to the question of whether artists should reject similarity altogether consists in a vindication of the role of similarity, although understanding it as something very close to the idea of creative similarity that I am proposing. Visual similarities with respect to objects and shapes in the world, Kandinsky (1911: 71-2) claims, could help artists reach the final purposes of their creations. So if we deprive art of the possibility of using similarities, we would be limiting its power of expression.[53] Nonetheless, fertile similarities that can contribute to achieve the purposes of the artist's creations always go, for artistic reasons, hand in hand with distortion and intentional manipulation: "features or limbs are for artistic reasons changed or distorted [...]. These apparently irresponsible, but really well-reasoned alterations in form provide one of the storehouses of artistic possibilities" (Kandinsky 1911: 73-4).

A good example of the use of well-reasoned alternations for artistic reasons is found, following Kandinsky (1911), in Cezanne's painting "Bathing Women" (1900). The depicted group of women in the painting was distorted in such a way that the geometric shapes of the bodies represented stood out and integrated into the triangular pattern formed by the arranged natural elements (trees and river) of the whole. Distortions had "perfect justification" in this case, Kandinsky said, given the "purely artistic purposes" motivating them (1911: 127, n38). Well-reasoned distortions and alterations are as fundamental as well-reasoned similarities in the attainment of the purposes of the artist's work. The result of the combination of selected similarities and distortions was a painting that enabled the viewer to perceive the integration of geometrical shapes forming individual figures and their natural backgrounds saliently, in a way that had not been perceived or appreciated before.

appearances of nature". The difference between the translation and the original is not trivial when part of the argument consists in claiming that representation is not equivalent to conventional or superficial imitation.

[52] In a similar vein, artists like Piet Mondrian (1919: 285) had also claimed that "the appearance of nature is far stronger and much more beautiful than any imitation of it can ever be", or Rebecca West (1928: 131) affirmed that "a copy of the universe is not what is required of art; one of the damn things is ample". The latter quote was erroneously attributed to Virginia Woolf by Goodman (1968). See Elgin (2017a: 250) for clarification.

[53] Kandinsky identified the ultimate goal of his own work with the provocation of the "inner vibrations of the spirit" (1911: 71-2). And given that the visual identification of objects could provoke intense emotions on the spectator, similarities could have a fundamental value in artistic representation (ibid.).

Although I contended, following Elgin (2011; 2017b), that scientific and artistic representations might not be equivalent in some critical ways (such as in terms of uses and practical outcomes), the debate in philosophy of science will especially benefit from reflections in aesthetics and artistic practice on the role of similarity in representation. Philosophers of science have repeatedly debated whether they should advocate or reject the notion of similarity, which they almost invariably understand as "bare similarity". Little has been done, though, to investigate the interplay of judgments of similarity and dissimilarity in practice, in a way that could give rise to a better characterization of the actual role of similarity in the production of representations. Examples like Kandinsky's attempt to explain the fertile interplay of similarities and distortions, from the perspective of practitioners' work of depicting, can be stimulating for philosophers of science who usually present similarity and distortion as terms pulling in opposite directions.

The case studies presented in Chapter 4 show significant parallels with the way in which the fertile integration of similarities and distortions happens in artistic practices. Concerning the construction of the SFBM and the MBM, I suggested that the use of geometric similarities was perfectly compatible with the inclusion of geometric distortions. Similarities and distortions were neither pulling in opposite direction nor diminishing each other, but showing that more than superficial spatial similarity was required to construct functioning scale models. Engineers know from experience that they can combine certain geometric similarities with certain distortions to build more tractable models. So they formulate judgments to find the right balance for the purposes of the practice. Furthermore, if we look into scientists' definitions of the notion of similarity in manuals and technical reports, which are closely connected to their modelling practices, we will find stronger support to the idea that similarities and distortions can enrich each other. We saw that for instance the classic manual *Engineering Hydraulics* (Warnock 1950) claims that "distorted scales" and "departures from geometric similarity serve some definite objective", so they should not be taken, in general, as shortcomings of models (Warnock 1950: 146). Similarity is a useful concept for the practice of constructing scale models, precisely because it is not defined according to an ideal of exact imitation, but as indicating "the *general limits of correspondence*, or one might speak of various types of similitude, each of which has a definite set of limitations" (ibid.: 136).

Definitions like these should be taken seriously by philosophers of science. I suggest that the notion of creative similarity is satisfactory to capture the idea that perfect matching is not what similarity is about in actual practices of modelling. It is about the *limits of correspondence* between a vehicle and a target, which are exploited by resourceful epistemic agents through the formulation of a balanced combination of judgments of similarity and distortion.

5.3.3. Creative similarity is generative

So far I have claimed that the notion of creative similarity helps capture the idea that epistemic communities are essential in determining what relevant similarities are, as well as the idea that in scientific practice similarities are effectively combined with distortions. But what ultimately characterizes the notion of creative similarity that I am using to advance an account of the means of representation is that it is generative, that is, it produces new associations that didn't exist or hadn't been noticed before. In other words, the notion of creative similarity attempts to encapsulate the productive interplay of judgments of similarity and dissimilarity that occurs in the process of constructing fruitful representations in science. The process in which new associations are generated is not automatic, but iterative, dynamic and shaped by the resourceful agents taking part in the practices of representing. An example from the pictorial arts can again help illustrate the generative character of creative similarity.

Gombrich (1972) tells an anecdote of Picasso, who was commissioned to paint a portrait of Françoise Gilot. Picasso drew a series of sketches, some of them using realistic styles of depiction, others closer to abstraction. At some point he felt he had found the "equivalence for him", the right balance of shapes, expressions and associations. The result was a portrait of Gilot with the contours, colours, and shapes of a flower (he titled it *Femme Fleur*, 1949) (Gombrich 1972: 30). Gombrich (1972) concluded that similarities evolve during artistic practices: "likeness needs the method of trial and error, of match-mismatch to trap this elusive prey. [...It] must be tested and criticized, it cannot be easily analysed step by step and therefore predicted" (ibid.). Likeness is not fixed in advance of the practice of representing. It needs to be tried, challenged and reconfigured when needed, so that it eventually generates new insightful associations and features regarding the objects intervening in the representational practice. The adjective "creative" emphasizes the centrality of the *use* of the representation, the actions and manipulations involved, and the resourceful epistemic agents that progressively stabilize fertile connections.

Examples like this from the arts help us to see the dynamic and ultimately creative character of similarity in the construction of scientific representations as well. The results of the use of judgments of similarity in scientific practices is the place to look at if we want to observe this generative process. I briefly referred to the construction of Phillips's hydraulic machine (Morgan 2012; Frigg and Nguyen 2017a; Vines 2000). Various judgments of similarity regarding the comparison of economic systems and hydraulic engineering systems were exploited in this case. Combinations with judgments of dissimilarity and distortion were tested, adjusted, modified, discarded when inadequate, and eventually stabilized when they proved to work. As a result, Phillips managed to construct a machine that uncovered patterns of the behaviour of economic systems, and displayed them saliently, "immensely visibly" (Vines 2000: 46-9). Economists could reason about money transactions while looking at the water tanks rise, in such a way that new, vivid, and even surprising understanding of economic policies was advanced.

Disagreements between different judgments of similarity and dissimilarity are evidence of this dynamic, generative character of creative similarity. In the example of the research on alcohol addiction using mice as model organism, we saw the importance of disagreements concerning what the relevantly similar aspects of humans and mice behaviours were (Ankeny et al. 2014). There was, for instance, a dispute about the significance of giving sweetened alcoholic drinks to mice – so that they would voluntarily consume alcohol and showed a (considered relevant) similar behaviour with respect to human alcohol consumption. Eventually judgments were balanced between those who argued that the dissimilarities between human addiction to alcohol and mouse addiction to sweetened alcoholic drinks was too significant, and those who argued that the key similarities between mice and human behaviours were captured, given that, for instance, human addiction usually starts with the consumption of sugary drinks (Ankeny et al. 2014). The importance of resolving disagreements like this, about the right combination of the most promising similarities and enlightening distortions, reinforces the idea that similarity is creative, the product of a process led by resourceful epistemic communities.

Representational practices are processes of trial and error, so certain judgments of similarity might eventually lead to erroneous actions in practice. Still, scientists seem to keep employing judgments of similarity in many cases, like the practices described throughout this thesis, modifying those that lead to undesirable results. Errors usually provide "incentives and resources for serious, focused, effective inquiry", as they reveal not only that we have got something wrong, but also where we got it wrong, therefore pointing us in the direction of advancing our understanding (Elgin 2017a: 305-6). Research communities, after balancing and weighing different judgments, frequently reach agreements about the most suitable way of modelling certain phenomena. This requires training, skill and awareness of the crucial questions asked in the practice. If there is a functioning epistemic community, we can assume that there is an attempt to stabilize an adequate balance to produce the most fruitful models for the case at hand. The norms or criteria of physical similarity established in practices of scale modelling for instance were consolidated and formalized when they proved their efficiency for the construction of physical models. From there, engineers in subsequent practices were able to use "standardized judgments of similarity" applying the norms previously formalized.

Earlier I mentioned that van Fraassen (2008) explicitly considers distortions compatible with similarities in the practice of representing. This was, I argued, an adequate starting point from which to develop a similarity-based type of account of the means of representation. But a limitation of van Fraassen's (2008) account is that he doesn't explicitly consider the generative, creative consequences that combining similarities and distortions has. In the light of the examples from the arts and sciences discussed, we can press this point further. Returning to the example of the caricatures, I said in Chapter 2, regarding Figure 1, that van Fraassen would argue that this caricature bears some resemblance to Trump (2008: 13-5).[54] At the same time, he would say, the caricature also distorts Trump, as he is represented as something he is not, namely a creature made out of flames. It is no puzzle that distortions can accompany similarity, since similarity is not the core of representation but "the means to an end" (ibid.: 14-5). This description can be further developed as follows: the combination of similarities and distortions in the caricature triggers a particular way of seeing Trump (as an evil creature, a demon) from the moment the caricature is encountered by the relevant community to which it is directed.[55] When looking at Trump after seeing the caricature, those

[54] In the original, van Fraassen (2008: 13-5) refers to a caricature of Margaret Thatcher represented as a dragon and of Otto Bismarck represented as a peacock.
[55] This applies even if one might want to argue that this is a metaphorical or ironical way of seeing Trump more than a literal one.

who have seen it may perceive evil in his attitudes, malevolence in his speeches and even a physical reminiscence of the devil. The community would even gain criteria for what it takes Trump-alike attributes to be, and appreciate those attributes in other subjects (in Elgin 2017a: 256-7).[56] Creative similarity is more than the selection of some relevant similarities. It demands a combination of relevant similarities and distortions from which insightful connections arise. The definitive feature of creative similarity is its capacity to generate or reveal new associations or features in the world.[57]

A possible objection to the idea that new associations or features are actually *created* in the world can be posed. Metaphysicians might want to claim that nothing really *arises* or is *created* in the practice (Elgin 2017a: 257). The attributes (of Trump in my example, Thatcher in van Fraassen's, or Gertrude Stein in Elgin's) existed all along: they were not brought into existence by the caricatures or the painting. This is true but, as Elgin responds, trivial, since there is little reason to care about many potential associations, extensions, that don't have significant things in common (ibid.). What the caricatures and the painting bring out is that some particular attributes are associated with a subject – or some attributes with other attributes, or subjects with other subjects – in a meaningful, non-arbitrary way (ibid.). The associations and attributes are *created* in the most epistemologically relevant sense. Although Elgin (2017a), following Goodman (1968; 1978), rejects referring to the role of similarity in any form in the process of producing fruitful representation, the notion of creative similarity fits well with a great part of their project. Creative similarity, I have tried to argue, is a central means of representation in science, present in a significant set of modelling practices that produce epistemically successful results, as it allows us to bring out non-arbitrary and illuminating associations concerning the target investigated.

[56] This example is adapted from Elgin (2017a: 256-7), who follows Schwarz (1985) to describe the case of Picasso's portrait of Gertrude Stein. Picasso's portrait allowed us to see, following Elgin (2017a), attributes in Stein that hadn't been appreciated before: in the painting she was magisterial, imposing, someone to be reckoned with. Moreover, Elgin (2017a) claims, we gained criteria for what it takes to *look* and *be* like Stein.

[57] The discussion of caricatures can be further enriched by another view from the arts. Ernst Gombrich (1960; 1972) argued that "caricature had been defined [...] as a method of making portraits which aims at the greatest likeness of *the whole* of a physiognomy while all *the component parts* are changed". For this reason, he adds, "it could serve me for a demonstration of equivalence, the proof that the images of art can be convincing without being objectively realistic" (Gombrich 1972: 1). Gombrich offers an alternative way of explaining how the integration of similarities and distortions is possible, by considering the relation between the whole and the parts of a representation: none of the components of a caricature intend to be a copy of the target, and nevertheless there is a similarity of great epistemic value associated with the entire representation.

Not far removed from this idea is Goodman's (1968: 7-8) metaphor of *worldmaking*, which links understanding with creation (see Ammon 2017: 94-5; Goodman 1978). In representing an object, we do not copy: we *achieve a construal or interpretation* (1968: 7-8). Achieving a fruitful, rewarding, illuminating construal of an aspect of the world is precisely what the resource of creative similarity can facilitate.

5.3.4. The Creative Similarity Account is a satisfactory account of the means of representation

Using the notion of creative similarity to develop a specific account of the means of representation was an attempt to rehabilitate the value of similarity in the debate of scientific representation, arguing that it plays a central role in practices of representing. Further, adding the adjective "creative" to the notion of similarity was an attempt to explicitly reject the associations of similarity with copy theories of representation and the idea of "bare similarity". Not any characterization of similarity could satisfactorily capture the dynamics of the uses of judgments of similarity as representational resources in practice. Based on the examples and case studies analysed, I believe the term *creative similarity* can do so. The *creative similarity account of the means of representation* therefore advocates that creative similarity is the means of scientific representation in a relevant set of scientific modelling practices. Following the characterization of the notion of creative similarity offered above, I now spell out how the *creative similarity account* fulfils the desiderata to advance a satisfactory account of the means of representation.

First of all, the *creative similarity account of the means of representation* is largely supported by a practical investigation of the construction of successful scientific representations. The historical analysis of the cases of the SFBM and the MBM presented in Chapter 4, as well as the more sketchy description of various examples of modelling practices in Chapter 3, was precisely a practical enquiry focused on particular cases that set the descriptive base to propose the *creative similarity account of the means of representation*. Adopting a practical type of enquiry allowed me to observe, among other things, how epistemic agents employ judgments of similarity and distortions concerning model-target comparisons as representational resources to construct fruitful representations. The study of judgments of similarity helps me connect the descriptive component of the *creative similarity account* with its normative component.

The normative component in an account of the means of representation is responsible for offering insight, with some degree of generality, about how certain representational dynamics have allowed in the past (and there are good reasons to believe they could allow in future) to promote successful ways of constructing representations. We saw that judgments of similarity in the cases of the SFBM and the MBM were of various types: some were standardized judgments, which proved to be efficient for the construction of models and got formalized in manuals; others were not systematically regulated, such as the case-specific judgments of similarity, but still were considered important by the epistemic community in the phases of calibration and testing of the models; and still other judgments of similarity were qualified as intuitive, that is, entrenched in practices of representing through common skills and experiences shared by members of the epistemic community. With the classification into different types of judgments of similarity, I aimed to offer a more flexible, plural way of thinking about the uses of similarity concerning model-target comparisons in modelling practices. Further studies of other modelling practices in different fields of research will help refine this classification, in such a way as to perhaps we attain more precise conclusions about pattern in the standardization of judgments of similarity. On this point, the *creative similarity account* claims that certain interplays of judgments of similarity and distortion historically proved to be productive and crystalized into norms, uses, or recommendations that helped guide a range of practices of modelling, sometimes beyond the fields where they were originally formulated, such as when criteria of physical similarity in engineering were transferred to the geosciences. Conclusions concerning the standardization of judgments within epistemic communities constitute the normative component of the *creative similarity account of the means of representation*. An alternative account of the means of representation, which for instance focuses on convention, should also be able to provide insightful generalizations about how the formulation of judgments of convention in a series of practices give rise to standards that guide the construction of successful models.

Concerning the third desideratum for a satisfactory account of the means of representation, we can also claim that the *creative similarity account* fulfils it. The third desideratum states that an account of the means of representation has to specify how the epistemic success of representations is precisely understood, and how the specific means proposed help achieve that form of epistemic success. In the *creative similarity account of the means of representation*, epistemic success is broadly identified with the growth of understanding, as particularly identified with the production of fruitful models. Recasting success into fruitfulness was a crucial step in my account to depart from "bare" similarity and focus instead on the relationship between creative similarity, which is generative and productive, and understanding. The account demonstrates that creative similarity as means of representation helps achieve fruitful scientific models by revealing paths that take us from the formulation of judgments of similarity and distortion to the production of new associations and fertile inferences through the use of models. More specifically, the *creative similarity account of the means of representation* offers at least two indicators of the path from creative similarity (or more precisely from the combination of judgments of similarity and distortion, which are the means understood as *resources*) to the production of fruitful models.

One, the interplay of judgments of similarity and distortion usually consolidates norms, common uses, and recommendations. This is an indicator that creative similarity is considered an effective means of representation, because it becomes entrenched in epistemic systems to guide future practices of modelling. As in the cases of the SFBM and the MBM, those practices can be considered fruitful, as they gave rise to models that "exceeded the expectations of [their] inventors" (Robinson 1992: 292), allowing them to explore new technologies and methodologies, study some hydraulic projects that were not planned in advance, and open new research questions about the targets investigated. Two, a less direct but equally significant indicator that creative similarity leads to the production of fruitful representations is the persistent presence of disagreements about the importance of different judgments of similarity and dissimilarity in practices of representing. In the case of modelling human alcoholism with mice that I discussed in Chapter 3, we saw that there were disagreements about which exact combinations or balances between relevant similarities and dissimilarities were the most likely to produce successful models of alcoholism. The importance of historically resolving disagreements like this, making certain lines of research move forward, reinforces the idea that creative similarity is generative, that is, in an epistemologically relevant sense it *creates* new associations and features that were previously unseen (Elgin 2017a: 257). For that reason, it is employed in practices of representing to achieve fruitful epistemic results.

If one proposes an alternative account of the means of representation focused for instance on convention or exemplification, these accounts would also have to explain how these means help achieve fruitful representations (or useful, predictably adequate, general, precise representations, depending on how epistemic success is specified). At any rate, I contended in section 5.1 that conceiving epistemic success in general in terms of the increase of knowledge (and in particular conceiving successful representations as accurate, truthful, or faithful) entails some important shortcomings. Mainly, it is difficult to directly accommodate the value of idealizations, abstractions and simplifications in the epistemic success of representations, since these elements do not provide truthful statements about the world (Potochnik 2015; Elgin 2017a). For that reason, I argue that alternative accounts of the means should also be framed within a view of understanding as the overarching goal of scientific representation.

Connected to this point, the *creative similarity account of the means of representation* satisfies the fourth desideratum for an adequate account of the means of representation because it can accommodate the potential role of misrepresentations in the achievement of successful models. This is because creative similarity is explicitly differentiated from "bare" or "pure" similarity. Replication is not what creative similarity is about; nor is it about obtaining the highest degree of similarity between a vehicle and a target, following an ideal of a perfectly mimetic copy. Instead, it is about the integration of appropriate similarities and distortions in productive interplay, in such a way that significant relations that were previously concealed are revealed.

In summary, the *creative similarity account of the means of representation* advances a proposal with the aim of addressing the complex (insufficiently discussed in a systematic way) problem of how epistemically successful representations are produced in science. More specifically, the *creative similarity account* claims that in a significant set of modelling practices, including the construction of scale models in engineering and research done with model organisms in the biological and psychological sciences, *creative similarity* is the means of representation. Claiming that creative similarity is the means of representation in these, and possibly other, practices of modelling entails that a generative interplay between judgments of similarity and distortion has been employed as a representational resource by the relevant epistemic community to produce fruitful models. If the desiderata that I proposed earlier are accepted as a fair set of important requirements to guide the advancement of an account of the means of representation, then the *creative similarity account* can be taken as a satisfactory account of the means of representation.

5.4 Conclusions

This final chapter has proposed a specific account of the means of representation, the *creative similarity account of the means of representation*. I framed this account within a broader discussion of the general aims of scientific practice. I argued, following Elgin (1996, 2017a) and Potochnik (2015), that understanding should be considered the overarching aim of science, and the measure of the epistemic success of scientific representations. This view contrasts with the conception of knowledge (understood as collecting true beliefs) as the overarching aim of science. An important limitation of views focused on knowledge is that they make models that contain falsehoods suspect and necessarily deficient with respect to their epistemic achievements. A view focused on understanding should be preferred because collecting true propositions doesn't guarantee that we grasp the phenomenon represented, that we are able to make sense of it, or ask new questions about it, whereas understanding does. The *creative similarity account* is an attempt to elucidate how epistemically successful representations are produced, in the sense of how a series of fruitful models that afford understanding of aspects of the world are obtained.

Then, I presented four desiderata for any project that attempts to develop an adequate account of the means of representation. One, an account of the means of representation should incorporate a practical enquiry that investigates the epistemic resources used in the production of a series of scientific representations. Two, such an account should include a normative component that offers some kind of generic insight on how certain representations achieve epistemically successful results, grounded in the descriptive work done on how certain judgments in scientific practice are regulated and entrenched within the relevant epistemic community. Three, such an account should specify how 'epistemic success' is understood (as fruitfulness, usefulness, precision), since, depending on the overarching conception of epistemic success adopted, the characterization of the resources to achieve that success could vary. Accordingly, an account of the means of representation should specify how the use of certain resources leads to the achievement of epistemically successful representations. And four, an account of the means of representation should explicitly accommodate the phenomenon of misrepresentation, in the sense of being able to explain the valuable role that idealizations, abstractions, and other forms of distortions might play in the achievement of the successful representations.

These desiderata were proposed as a guide for those attempting to address the problem of the epistemic success of representation from a philosophical perspective. It is implied in the desiderata that an approach in philosophy of science in practice (PSP) is desirable to advance such an account. Only when a practical enquiry that pays attention to particular case studies is developed, and when empirical evidence on how certain epistemic resources lead to the production of successful models is gathered, is it possible to offer insight on how epistemic success is achieved in a series of representational practices. The *creative similarity account* fulfils the desiderata. Therefore, it should be taken as an example of how a satisfactory account of the means of representation can be advanced.

Then, I defined the notion of creative similarity, explaining why I believe it is a satisfactory notion with which to support an account of the means of representation. The notion of creative similarity emerges from actual practices of representing, and helps capture scientists' uses of judgments of similarity when constructing representations. I first characterized creative similarity in opposition to "bare" or "pure" similarity, which is a conception of similarity that recalls a two-term relation of representation and an aspiration to obtain perfect copies of the object represented. I then characterized creative similarity as compatible and combinable with distortions, based on the empirical observation that when judgments of similarity are used in modelling practices, they go hand in hand with judgments of dissimilarity and distortion. Lastly, I argued that what ultimately makes similarity creative is that, from combination with relevant distortions, new, insightful associations arise. The use of various examples from artistic practices helped illustrate the implications of the notion of creative similarity, and opened some grounds for further exploration on the commonalities between these two fields concerning shared goals and resources in their respective practices of representing.

Additional accounts of the means of representation would be required to complement and further expand the insight offered by the *creative similarity account.* Other accounts of the means of representation might be able to highlight the role that conventions or exemplifications – through the formulation of judgments and the stabilization of norms – play in certain representational practices. These alternative accounts would not disprove the importance of creative similarity as the means of representation in a variety of modelling practices, but will help achieve more nuanced comparisons and generalizations about the various means employed in the production of epistemically successful representations.

Conclusions

This thesis tackled the role of similarity in the production of epistemically successful representations in science. Focusing on this problem differentiated my account from others in recent philosophy of science revolving around the identification of the constituents (necessary and sufficient conditions) of scientific representation (French 2003; French and Bueno 2011; Bartels 2006; Frigg & Nguyen 2017a). I started Chapter 1 by distancing my approach from accounts that have privileged questions about the constituents of representation, and focused instead on the problem of the means of representation from a practice-based perspective (following Suárez's [2003, 2010] terminology). I regarded the problem of the means of representation (or the epistemic success of representations) as particularly relevant, since offering a response may advance insights on how we learn, make sense of, and understand aspects of the world through the construction of scientific models.

I proposed to address the problem of the means of representation by appealing to the role of similarity in scientific practices of modelling. The role of similarity in representation has been recurrently discussed in contemporary philosophy of science, usually polarized between detractors and advocates of similarity. My goal was to show that there are good reasons to support an account of the epistemic success of scientific representation focused on similarity. However, I took criticisms of similarity seriously, and acknowledged that similarity has to be adequately characterized to address the problem of the means of representation.

In Chapter 1, I agreed with detractors of similarity that it is indeed problematic to consider similarity a constituent of the relation of representation. The argument that Goodman (1968, 1972) offered, together with the argument from misrepresentation, should dissuade us from attempting to advance a substantive account of representation based on similarity. Then, Chapter 2 recognized three central arguments against similarity as means of representation: the argument from variety, the argument from vagueness, and the argument from misrepresentation against the means. Although these arguments advance important points that an account of the means of representation focused on similarity should consider, none is conclusive enough to prevent us from trying to offer such an account.

The argument from variety pointed out the diversity of models and representational practices in science. This fact does not hinder developing an account of the means of representation focused on similarity however, if there is evidence that in a range of modelling practices similarity works as an effective means of representation. Still, the argument from variety reminds us that similarity might not be the only means of representation. Alternative accounts of the means of representation, for instance focused on convention or exemplification, could enrich and further expand the insight offered by the *creative similarity account*, and together help develop a more comprehensive approach to the problem of the epistemic success of representation. In Chapter 5, I suggested four desiderata that any attempt to develop an account of the means of representation should fulfil. This list of desiderata allows for the possibility of alternative accounts of the means of representation, provided they fulfil them as satisfactorily as the *creative similarity account of the means of representation* does. The first desideratum is that an account of the means of representation should develop a practical enquiry on particular modelling practices. The second is that the account should incorporate a normative component, as any genuine philosophical proposal, in connection to the descriptive component that results from the practical enquiry. The study of judgments concerning model-target comparisons in scientific practices in the key to connect the two components. The third desideratum is that an account of the means of representation should specify how 'epistemic success' is understood (as fruitfulness, usefulness, precision), and accordingly how certain means lead to the achievement of successful representations characterized in that particular way. Finally, the fourth desideratum is that an account of the means of representation should explicitly accommodate the potential value that idealizations and other forms of distortions might play in the epistemic successful of scientific representations.

The argument from vagueness stated that similarity is an empty or utterly vague concept, because its meaning ought to be specified in every particular representational practice. I replied that a context-independent, objective definition of similarity is neither attainable nor desirable. An investigation of how similarity is actually defined in particular practices of representing can bring light to this problem. I observed, with the help of the case studies in Chapter 4, that some definitions of similarity might indeed depend on very specific contexts of modelling. Other characterizations of similarity, though, are widely shared by epistemic communities, as they are supported by common skills and experiences. Still others are deeply entrenched and consolidated in the form of standards and norms that regulate representational practices. The classification into three types of judgments of similarity (standardized, context-dependent and intuitive) helped me offer a more flexible characterization of the role of similarity concerning model-target comparisons in representational practices, while rejecting the idea that similarity is a problematically vague notion. Further empirical and historical work needs to be done to complement these claims. The continued study of specific practices of representing in different fields of research will help confirm, and maybe adjust, the classification into these three types of judgments of similarity that I identified for the case of scale modelling in engineering, as well as providing more general and robust conclusions about the plural character of similarity and the agreements of the epistemic communities that shape it.

The argument from misrepresentation against the means stressed that scientific models distort, idealize, and abstract the targets they represent. If an account of the means of representation focused on similarity cannot accommodate this fact or explain why highly idealized models can be epistemically successful, such an account must be inadequate. I replied that an account of the means of representation focused on "bare similarity" is certainly incapable of accommodating misrepresentation. Bare similarity is a conception of similarity as a two-term relation between a vehicle and a target, where the former aspires to be a copy of the latter. In contrast to this conception, which I spelt out in Chapter 5, I proposed an account of the means of representation supported on the notion of *creative similarity*. The idea of creative similarity has to be framed in a triadic conception of representation, where resourceful epistemic agents, and more broadly the epistemic community that determines the use of representations, seek an adequate balance between relevant similarities and distortions according to the goals of the practice. Proposing a definition of similarity as creative similarity should not be understood as a strategy to evade the argument from misrepresentation against the means. This notion is deeply ingrained in the observation that, in actual practices of representing, judgments of similarity go hand in hand with judgments of dissimilarity and distortion. The generative interplay between them allows epistemic agents to construct fruitful representation, as in the case of the SFBM and the MBM that I examined in Chapter 4.

The *creative similarity account of the means of representation* is the outcome of bringing together central points discussed in Chapters 1 and 2 about the general problem of scientific representation, as well as the methodological considerations and case studies examined in Chapters 3 and 4. This account advocates that *creative similarity* is the means of representation in a relevant set of representational practices that we recognized for their epistemic achievements. It is supported by the study of concrete modelling practices, and specifically the analysis of judgments of similarity concerning model-target comparisons used in the design, construction, and testing of scientific models. The presence of judgments of similarity in modelling practices has been addressed by Weisberg (2013) and Sterrett (2009, 2017a, 2017b) in recent philosophy of science. My research helps to further develop the conclusions of these works by showing that an account of the epistemic success of representation requires a descriptive and a normative component, and that the study of judgments of similarity permits the integration of the two. The source of the normativity of an account of the means of representation should be the description of norms, standards, and common uses consolidated in representational practices.

An additional claim that this thesis has defended is that *understanding* should be taken as the overarching goal of science, and fruitfulness as a central value to describe the epistemic success of representations. A good reason to support a definition of epistemic success in terms of understanding is that it can better account for the achievements of actual scientific practices, where idealizations and other felicitous falsehoods are frequently involved (Elgin 2004, 2017a). Understanding concerns grasping the phenomena that are our object of study, revealing connections and patterns, generating new questions about them and providing insight about facts that had remained unnoticed before. The *creative similarity account of the means of representation* and the view of understanding as the overarching goal of science support and strengthen each other. A related subject that needs to be explored in more detail is the parallel between scientific and artistic representations. Many representations in the arts also seem to have understanding as a central goal (Elgin 2011, 2017b; Goodman and Elgin 1988), while creative similarity could be considered an efficient means to reach that goal in a variety of artistic practices, such as the pictorial works mentioned in Chapter 5. Ultimately, bringing together important aspects of scientific and artistic practices of representing could help expand the scope and strength of the *creative similarity account of the means of representation*.

The investigation developed in this thesis brings us closer to an elucidation of the problem of scientific representation because it helps characterize the actual role that similarity plays in the production of epistemically successful representational. It shows that similarity, through the formulation of a plurality of judgments of similarity in generative interplay with judgments of distortions, allows us to construct fruitful scientific models that afford understanding of aspects of the world.

Bibliography

ACHINSTEIN, P. (1968) *Concepts of Science. A Philosophical Analysis*. Baltimore: Johns Hopkins Press.

AMBROSIO, C. (2013) "Iconic Representations, Creativity and Discovery in Art and Science". In GONZÁLEZ, W. (ed.) *Creativity, Innovation, and Complexity in Science*. A Coruña: Netbiblio.

AMMON, S. (2017) "Explaining understanding, understanding knowledge". In GRIMM, S., BAUMBERGER, C., and AMMON, S. (eds.) *Explaining Understanding. New Perspectives from Epistemology and Philosophy of Science*. New York: Routledge, pp.92-110.

ANKENY, R. (2001) "Model organisms as models: Understanding the 'lingua franca' of the Human Genome Project". *Philosophy of Science*, 68 (Suppl.), 251–261.

ANKENY, R. and LEONELLI, S. (2011) "What's so special about model organisms?". *Studies in History and Philosophy of Science*, 42, 313–323.

ANKENY, R., CHANG, H., BOUMANS, M. and BOON, M. (2011) "Introduction: philosophy of science in practice". *European Journal for Philosophy of Science* 1 (3), 303.

ANKENY, R., LEONELLI, S., NELSON, N., and RAMSDEN, E. (2014) "Making Organisms Model Human Behavior: Situated Models in North-American Alcohol Research, since 1950". *Science in Context*, 27 (3).

BAILER-JONES, D. (1999) "Tracing the Development of Models in the Philosophy of Science". In MAGNANI, N., NERSESSIAN, N., and THAGARD, P. (eds.) *Model-Based Reasoning in Scientific Discovery*. New York: Kluwer/Plenum, pp.23-40.

_________(2003) "When scientific models represent". *International Studies in the Philosophy of Science*, 17 (1), 59-74.

BARTELS, A. (2006) "Defending the Structural Concept of Representation". *Theoria*, 21, 7–19.

BENGOETXEA, J. B., TODT, O. and LUJÁN, J. L. (2014) "Similarity and representation in chemical knowledge practices". *Found Chem*, 16, 215–233.

BLACK, M. (1962) *Models and Metaphors. Studies in Language and Philosophy.* Ithaca, NY: Cornell University Press.

BLOOR, D. (1976) *Knowledge and Social Imagery*. Chicago: University of Chicago Press.

BODEN, M. (2004) *The Creative Mind: Myths and Mechanisms.* 2nd ed. London: Routledge.

BOESCH, B. (2017) "The means-ends account of scientific, representational actions". *Synthese.* https://doi.org/10.1007/s11229-017-1537-2

BOKULICH, A. and ORESKES, N. (2017), "Models in Geosciences". In MAGNANI, L. and BERTOLOTTI, T. (eds.), *Springer Handbook of Model-Based Science.* New York: Springer, pp.891-911.

BOLINSKA, A. (2013) "Epistemic representation, informativeness and the aim of faithful representation". *Synthese*, 190(2), 219–234.

_________(2015) *Epistemic Representation in Science and Beyond.* PhD Thesis: University of Toronto.

_________(2016) "Successful Visual Epistemic Representation", *Studies in History and Philosophy of Science*, 56, 153-60.

BROWN, H. (1988) *Rationality*. London: Routledge.

_________(2006) "More about judgment and reason". *Metaphilosophy*, 37(5), 646-651.

BROWN, J. (2013) "Supermodel: The Mississippi Basin in Miniature". *Civil Engineering Magazine*, 83 (9).

BUCKINGHAM, E. (1914) "Physically similar systems: Illustrations of the use of dimensional equations". *Phys. Rev.* 4, 345–376.

CADELL, H. (1888) "Experimental researches in Mountain building". *Transactions of the Royal Society of Edinburgh*, 35, 337–357.

CALLENDER, C. and COHEN, J. (2006) "There is no Special Problem of Scientific Representation". *Theoria* 55, 67–85.

CARNAP, R. (1939) "Foundations of logic and mathematics". In NEURATH, O., MORRIS, C., and CARNAP, R. (eds.) *International Encyclopaedia of Unified Science.* Vol. 1. Chicago: University of Chicago Press.

CARTWRIGHT, N., SHOMAR, T. and SUÁREZ, M. (1995) "The Tool-Box of Science: Tools for the Building of Models with a Superconductivity Example". *Poznan Studies in the Philosophy of the Sciences and the Humanities*, 44, 137–149.

CARTWRIGHT, N. and SUÁREZ, M. (2008) "Theories: Tools vs. Models". *Studies in History and Philosophy of Modern Physics*, 39, 62–81.

CHAKRAVARTTY, A. (2001) "The semantic or model-theoretic view of theories and scientific realism". *Synthese*, 127, 325–345.

_________(2010a) "Truth and Representation in Science: Two Inspirations from Art". In FRIGG, R. and HUNTER, M. (eds.) *Beyond Mimesis and Convention: Representation in Art and Science.* Berlin and New York: Springer, pp.33-50.

_________(2010b) "Informational versus functional theories of scientific representation". *Synthese*,172, 197–213.

CHANG, H. (2004) *Inventing Temperature: Measurement and Scientific Progress.* Oxford: Oxford University Press.

_________(2012) "Beyond case-studies: History as philosophy". In MAUSKOPF, S., and SCHMALTZ, T. (eds.) *Integrating History and Philosophy of Science: Problems* *and Prospects*. Boston Studies in the Philosophy of Science. London: Springer, pp.109-124.

_________(2014) "Epistemic Activities and Systems of Practice. Units of Analysis in Philosophy of Science After the Practice Turn". In SOLER, L., ZWART, S., LYNCH, M. and ISRAEL-JOST, V. (eds.) *Science After the Practice Turn in the Philosophy, History, and Social Studies of Science*. London and Abingdon: Routledge, pp.67-79.

CICERO, T. (1980) "Animal Models of Alcoholism?" In ERIKSSON, K., SINCLAIR, J. D., and KIIANMAA, K. (eds.) *Animal Models in Alcohol Research.* London: Academic Press, pp.99-118.

COLLINS, H. (1981) "Stages in the empirical program of relativism". *Social Studies of Science*, 11, 3-10.

CONTESSA, G. (2006) "Scientific models, partial structures and the new received view of theories". *Studies in History and Philosophy of Science*, 37, 370–377.

_________(2007a) Representing Reality: The Ontology of Scientific Models and Their Representational Function. PhD Thesis. London: London School of Economics.

_________(2007b) "Scientific representation, interpretation, and surrogative reasoning". *Philosophy of Science*, 74, 48–68.

_________(2011) "Scientific models and representation". In FRENCH, S. and SAATSI, J. (eds.) *The Continuum Companion to the Philosophy of Science*. London: Continuum Press.

COOPER, N. (1995) "The epistemology of understanding". *Inquiry*, 38, 3.

DA COSTA, N. and FRENCH, S. (2003) *Science and Partial Truth: A Unitary Approach to Models and Scientific Reasoning*. Oxford: Oxford University Press.

DALRYMPLE, R. A. (ed.) (1985) *Physical Modelling in Coastal Engineering.* Rotterdam: A. A. Balkema.

DASTON, L. and GALISON, P. (2007) *Objectivity.* New York: Zone Books.

DA VINCI, L. *Del Moto e Misura dell'Acqua*, edited by CARUSI, E., and FAVARO A. Bologna, 1924.

DEAR, P. (2012) "Philosophy of Science and Its Historical Reconstructions". In MAUSKOPF, S., and SCHMALTZ, T. (eds.) *Integrating History and Philosophy of Science: Problems and Prospects*. Boston Studies in the Philosophy of Science. London: Springer, pp.67-82.

DE REGT, H. (2017) *Understanding Scientific Understanding*. Oxford: Oxford University Press.

DI PAOLO, A., EZEQUIEL A., J. NOBLE and S. BULLOCK (2000) "Simulation Models as Opaque Thought Experiments." In BEDAU, M. et al. (eds.). *Artificial Life VII: Proceedings of the Seventh International Conference on Artificial Life*, Cambridge, MA: MIT Press.

DIPERT, R. (1996) "Reflections on Iconicity, Representation, and Resemblance: Peirce's theory of signs, Goodman on resemblance, and modern philosophies of language and mind". *Synthese*, 106, 373-397.

DOWNER, J. (2007) "When the Chick Hits the Fan: Representativeness and Reproducibility in Technological Tests". *Social Studies of Science*, 37(1), 7-26.

DOWNES, S. (2009) "Models, pictures, and unified accounts of representation: lessons from aesthetics for philosophy of science". *Perspectives on Science*, 17(4), 417–428.

DUHEM, P. (1913) *Études sur Léonard de Vinci*, Ser. 1, 2, and 3, Paris, 1906, 1909, and 1913.

__________([1914] 1954). *The Aim and Structure of Physical Theory*. Translated from the French 2nd edition. Princeton: Princeton University Press.

DUPRÉ, J. (2012) "Philosophy-of-Science in Practice vs. Philosophy of Science-in-Practice". *Society for the Philosophy of Science in Practice Newsletter*. Autumn, 9-11.

EIGNER, K. (2009). "Understanding in Psychology: Is Understanding a Surplus?" In H. W. REGT, S. LEONELLI, and K. EIGNER (eds.), *Scientific Understanding: Philosophical Perspectives*. Pittsburgh: University of Pittsburgh Press, pp.271-297.

ELGIN, C. (1996) *Considered Judgment*. Princeton: Princeton University Press, 1996.

__________(2002) "Creation as reconfiguration: art in the advancement of science". *International Studies in the Philosophy of Science*, 16 (1).

__________(2004) "True enough". *Philosophical Issues*, 14(1), 113–131.

__________(2010). 'Telling Instances'. In FRIGG, R. and HUNTER, M. (eds.) *Beyond Mimesis and Convention: Representation in Art and Science*. Berlin and New York: Springer, pp.1-18.

__________(2011) "Making manifest: Exemplification in the sciences and the arts". *Principia*, 15, 399– 413.

__________(2017a) *True Enough*. Cambridge, MA: MIT Press.

__________(2017b) "Nature's handmade, art". In BUENO, O., DARBY, G., FRENCH, S., and RICKLES, D. (eds.) *Thinking about science, reflecting on art. Bringing Aesthetics and Philosophy of Science Together*. London and New York: Routledge, pp.27-40.

ERAÑA, A. (2007) "Sobre la viabilidad de una epistemología empírica y normativa". *Signos Filosóficos*, IX, núm. 17, January-June, 101-137.

ETTEMA, R., ARNDT, R., ROBERTS, P., and WAHL, T. (2000). *Hydraulic modelling. Concepts and practice*. ASCE manuals and reports on engineering practice, No. 97.

FANG, W. (2017) "Holistic modelling: an objection to Weisberg's weighted feature-matching account". *Synthese*, 194, 1743–1764.

FISHER, I. (1911) *Purchasing Power of Money*. New York: Macmillan.

FOSTER, J. E. (1971) Mississippi Basin Model Report 1-6. *History and Description of the Mississippi Basin Model.* US Army Engineer Waterways Experiment Station, Vicksburg, Mississippi.

FOUCAULT, M. (1973) *The Order* of *Things.* English translation. New York: Random House, Vintage Books.

FOWLE, B. (Ed.) *Builders and fighters: U.S. Army Engineers in World War II.* Fort Belvoir, VA: Office of History, United States Army Corps of Engineers.

FRENCH, S. (2003) "A Model Theoretic Account of Representation (or, I don't know much about art, but I know it involves isomorphism)". *Philosophy of Science*, 70, 1472–83.

FRENCH, S. and BUENO, O. (2011) "How Theories Represent". *British Journal for the Philosophy of Science*, 62, 857–94.

FRENCH, S. and LADYMAN, J. (1999) "Reinflating the semantic approach". *International Studies in the Philosophy of Science*, 13, 103–121.

FRIGG, R. (2006) "Scientific Representation and the Semantic View of Theories." *Theoria*, 55, 37–53.

__________(2010) "Fiction and scientific representation". In FRIGG, R. and HUNTER, M. (eds.) *Beyond Mimesis and Convention: Representation in Art and Science.* Berlin and New York: Springer, pp.97-138.

FRIGG, R. and HARTMANN, S. (2018) "Models in Science", *The Stanford Encyclopedia of Philosophy* (Summer 2018 Edition). Edward N. Zalta (ed.), forthcoming URL https://plato.stanford.edu/archives/sum2018/entries/models-science/

FRIGG, R. and NGUYEN, J. (2016) "Scientific Representation". *The Stanford Encyclopedia of Philosophy*. Winter 2016 Edition, Edward N. Zalta (ed.), URL https://plato.stanford.edu/archives/win2016/entries/scientific-representation/

__________(2017a) "The turn of the valve: representing with material models". *European Journal for the Philosophy of Science.* https://doi.org/10.1007/s13194-017-0182-4.
__________(2017b) "On barrels and pipes: Representation-as in art and science". In BUENO, O., DARBY, G., FRENCH, S., and RICKLES, D. (eds.) *Thinking about science, reflecting on art. Bringing Aesthetics and Philosophy of Science Together*. London and New York: Routledge, pp.41-61.

FROUDE, W. (1874) "On Experiments with H.M.S. Greyhound". *Transactions of the Institution of Naval Architects*, Vol. 16.

GABUCHIAN, V., ROSAKIS, A. J., LAPUSTA, N., and OGLESBY, D. D. (2014) "Experimental investigation of strong ground motion due to thrust fault earthquakes", *J. of Geophys. Res. Solid Earth*, 119, 1316-1336.

GALILEI, G. (1974) *Two New Sciences.* Tr. Stillman Drake. Milwaukee: University of Wisconsin Press.
__________(2005) *Discourse on Bodies in Water.* Tr. Thomas Salisbury. New York: Dover Publications.

GAUT, B. (2010) "The Philosophy of Creativity". *Philosophy Compass* 5/12, 1034–46.

GAUVIN, D., MOORE, K. and HOLLOWAY, F. (1993) "Do Rat Strain Differences in Ethanol Consumption Reflect Differences in Ethanol Sensitivity or the Preparedness to Learn?". *Alcohol* 10, 37–43.

GIERE, R. (1988) *Explaining Science. A Cognitive Approach.* Chicago: University of Chicago Press.
__________(1999) "Using Models to Represent Reality". In MAGNANI, N., NERSESSIAN, N., and THAGARD, P. (eds.) *Model-Based Reasoning in Scientific Discovery.* New York: Kluwer/Plenum, pp.41-57.
__________(2004) "How Models Are Used to Represent Reality". *Philosophy of Science*, 71(5), 742-752.
__________(2005) "Scientific Realism: Old and New Problems". *Erkenntnis*, 63(2), 149-165.

__________(2006) *Scientific Perspectivism.* Chicago and London: Chicago University Press.
__________(2008) "Models, Metaphysics and Methodology". In HARTMANN, S. and BOVENS, L. (eds.) *Nancy Cartwright's Philosophy of Science.* New York: Routledge.
__________(2010) "An agent-based conception of models and scientific representation". *Synthese*, 172(1), 269–281.

GIGERENZER, G. (2000) *Adaptive Thinking: Rationality in the Real World.* New York: Oxford University Press.

GIGERENZER, G., and GAISSMAIER, W. (2011) "Heuristic Decision Making", *Annual Review of Psychology*, 62, 451–82.

GOLDMAN, A. (1986). *Epistemology and Cognition.* Cambridge, MA: Harvard University Press.

GOLDSTONE, R., MEDIN, D., and GENTNER, D. (1991) "Relational similarity and the nonindependence of features in similarity judgments". *Cognitive Psychology*, 23, 222–262.

GOMBRICH, E. (1960) *Art and Illusion: A Study in the Psychology of Pictorial Representation.* London: Phaidon.
__________(1972) "The mask and the face: the perception of physiognomic likeness in life and in art". In GOMBRICH, E., HOCHBERG, J., and BLACK, M. (eds.) *Art, Perception, and Reality.* Baltimore: Johns Hopkins University Press, pp.1-46.

GOODMAN, N. (1968) *Languages of Art: An Approach to a Theory of Symbols.* Indianapolis: The Bobbs-Merrill Company.
__________(1972) "Seven strictures on similarity". In GOODMAN, N. (ed.) *Problems and projects.* Indianapolis/New York: Bobbs-Merrill, pp.437-447.
__________(1983) *Fact, Fiction, and Forecast.* Cambridge, MA: Harvard University Press, 4th edition, 1955.

GOODMAN, N. and ELGIN, C. (1988) *Reconceptions in Philosophy and other Arts and Sciences.* Indianapolis: Hackett; London: Routledge.

GOVIER, T. (1999) "Rosebuds, Judgment and Critical Thinking." In GOVIER, T. *The Philosophy of Argumen*t. Newport News: Vale Press, pp. 123–36.

GRIMM, S. (2006) "Is understanding a species of knowledge?" *British Journal for the Philosophy of Science*, 57, 515-535.
__________(2012) "The value of understanding". *Philosophy Compass*, 7, 103-117.

GUALA, F. (2002) "Models, Simulations, and Experiments". In MAGNANI, L., and NERSESSIAN, N. (eds.), *Model-Based Reasoning: Science, Technology, Values.* New York: Kluwer, pp.59-74.

HACKING, I. (1999) *The Social Construction of What?* Cambridge, MA: Harvard University Press.

HALVORSON, H. (2012) "What Scientific Theories Could Not Be". *Philosophy of Science*, 79, 183–206.

HELLER, V. (2011) "Scale effects in physical hydraulic engineering models". *Journal of Hydraulic Research*, 49(3), 293-306.

HESSE, M. (1966) *Models and Analogies in Science.* Notre Dame: Notre Dame University Press.

HEY, S. (2006) "Heuristics and Meta-heuristics in Scientific Judgement". *British Journal for the Philosophy of Science*, 67, 471–495.

HILLS, A. and BIRD, A. (2018) "Creativity without value". In GAUT, B., and KIERAN, M. (eds.) *Creativity and Philosophy.* Abingdon: Routledge, pp.95-107.

HUBBERT, M. K. (1937) "Theory of scale models as applied to the study of geologic structures". *The Geological Society of American Bulletin*, 48, 1459–1520.

HUBER, L. (2015) "From Mice to Men: Homogeneity, Similarity and Relevance in Model-Based Reasoning". Talk at the Conference of the Society for the Philosophy of Science in Practice. Aarhus (Denmark), June 2015.

HUBER, L. and KEUCK, (2013) "Mutant mice: Experimental organisms as materialised models in biomedicine". *Studies in History and Philosophy of Biological and Biomedical Sciences* 44, 385–391.

HUGGINS, E., and SCHULTZ, E. (1967) "San Francisco Bay in a warehouse". *Journal of the IEST*, 10(5), 9–16.

HUGHES, R. I. G. (1997) "Models and Representation", *Philosophy of Science*, 64, 325–336.

HUGHES, S. (1993) *Physical Models and Laboratory Techniques in Coastal Engineering*. Singapore: World Scientific Publishing, 1993.

KANDINSKY, W. (1911) *Über das Geistige in der Kunst. Insbesondere in der Malerei.* Bern-Bümplitz: Benteli-Verlag, 1952. Translation by M. H. Sanders. *Concerning the Spiritual in Art.* The Floating Press, 2008.

KIERAN, M. (2014) "Creativity as a Virtue of Character" in PAUL, E. and KAUFFMAN, S. (eds.), *The Philosophy of Creativity: New Essays.* Oxford: Oxford University Press, pp.

KNORR-CETINA, K. (1981) *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science.* Oxford: Pergamon.

KNUUTTILA, T. (2009) "Some Consequences of the Pragmatist Approach to Representation: Decoupling the Model-Target Dyad and Indirect Reasoning". In SUÁREZ, M., DORATO, M., and RÉDEI, M. (eds.) *EPSA Epistemology and Methodology: Launch of the European Philosophy of Science Association.* New York and Berlin: Springer.

KOENIGSBERGER, J., and MORATH, O. (1913) "Theoretische Grundlagen der experimentellen Tektonik". *Zeitschr. Deutsch. Geol. Gesellsch*, 65.

KORNBLITH, H. (1988) "What is naturalistic epistemology?". In KORNBLITH, H. (ed.) *Naturalizing Epistemology*. Cambridge: MIT Press, pp.1-14.

KROES, P. (1989) "Structural analogies between physical systems", *The British Journal for the Philosophy of Science*, 40(2), 145-154.

KUHN, T. (1962) *The Structure of Scientific Revolutions.* Chicago: Chicago University Press.

__________(1977) *The Essential Tension: Selected Studies in Scientific Tradition and Change.* Chicago and London: University of Chicago Press.

KUUKKANEN, J. (2015). "Historicism and the failure of HPS". *Studies in History and Philosophy of Science* xxx, 1-9.

KVANVIG, J. (2003) *The value of knowledge and the pursuit of understanding*. New York: Cambridge University Press.

LATOUR, B. and WOOLGAR, S. (1979) *Laboratory Life: The Social Construction of Scientific Facts.* Beverley Hills: Sage Publications.

LAYTON, E. (1992) "Escape from the Jail of Shape: Dimensionality and Engineering Science". In KROES, P., and BAKKER, M. (eds.) *Technological development and science in the Industrial Age: new perspectives on the science-technology relationship.* Dordrecht, Boston: Kluwer Academic Publishers, pp.35-68.

Le MÉHAUTÉ, B. (1990) "Similitude". In HANES, M., and Le MÉHAUTÉ (eds.) *Ocean Engineering Science.* Vol. 9, Part B in *The Sea.* New York: John Wiley and Sons.

LIEN, S. L., and HOOPES, J. A (1978) "Wind-driven, steady flows in Lake Superior". *Limnology and Oceanography* 23, 91–103.

LYNCH, M., and WOOLGAR, S. (Eds.) (1990) *Representation in Scientific Practice.* London: MIT Press.

MACEDA, E. et al. (2009) “Experimental use of Models in Disaster Risk Management”. *Shima: The International Journal of Research into Island Cultures*, 3(1).

MAGNANI, L., NERSESSIAN, N., and THAGARD, P. (Eds.) (1999) *Model-Based Reasoning in Scientific Discovery*. New York: Kluwer Academic.

MAKI, U. (2009) “Missing the world. models as isolations and credible surrogate systems”. *Erkenntnis*, 70(1), 29–43.

MANSFIELD, E. (2007) *Too beautiful to picture: Zeuxis, myth, and mimesis*. Minneapolis: University of Minnesota Press.

MARCOS, A. (2011) “The Creative Discovery of Similarity”. In MARCOS, A., and CASTRO, S. (eds.) *The Paths of Creation: Creativity in Science and Art*. Bern: Peter Lang, pp.195-211.

MATTINGLY, J., and WARWICK, W. (2009) “Projectible predicates in analogue and simulated systems”. *Synthese*, 169, 465-483.

MAUSKOPF, S., and SCHMALTZ, T. (eds.) (2012) *Integrating History and Philosophy of Science: Problems and Prospects*. Boston Studies in the Philosophy of Science. London: Springer.

McCLAY, K., et al. (1991) “Physical and seismic modelling of listric normal fault geometries”. *Geological Society, London, Special Publications*, 56, 231-239.

MERLE, O., and BORGIA, A. (1996) “Scaled experiments of volcanic spreading”, *Journal of Geophys. Res. Solid Earth*, 101, 13805-17.

MONDRIAN, P. (1919) “Dialogue on the New Plastic”. In HARRISON, C. and WOOD, P. (eds.) *Art in Theory. 1900-1990*. Oxford: Blackwell, 1999.

MORGAN, M. (2012) *The world in the model: how economists work and think*. Cambridge: Cambridge University Press.

MORGAN, M. and M. MORRISON (eds.) (1999) *Models as Mediators.* Cambridge: Cambridge University Press.

NELSON, N. (2012) “Modeling mouse, human, and discipline: Epistemic scaffolds in animal behavior genetics”. *Social Studies of Science*, 43(1) 3–29.

NERSESSIAN, N. (1992) “How do scientists think? Capturing the dynamics of conceptual change in science”. In GIERE, R. (ed.), *Cognitive models of science.* Minneapolis: University of Minnesota Press, pp.3-44.

NGUYEN, J. (2016) *How models represent.* PhD Thesis. London: London School of Economics.

NORINI, G., and ACOCELLA, V. (2011) “Analogue modeling of flank instability at Mount Etna: Understanding the driving factors”, *J. Geophys. Res.*, 116, 1-21.

ORESKES, N. (2007) “From scaling to simulation: Changing meanings and ambitions of models in geology”. In CREAGER, A., LUNBECK, E., and NORTON WISE, M. (eds.) *Science Without Laws.* Durham, London: Duke Univ. Press, pp.93-124.

PANKHURST, R. C. (1964) *Dimensional Analysis and Scale Factors.* The Institute of Physics and The Physical Society Monographs for Students. London: Chapman & Hall Ltd.

PARKER, W. (2009) “Does matter really matter? Computer simulations, experiments, and materiality”. *Synthese,*169, 483–496.
__________(2015) “Getting (even) more serious about similarity”. *Biol. Philos.*, 30, 267–276.

PÉREZ RANSANZ, A., and PONCE, A. (eds.) (2018) *Creatividad e Innovación en Ciencia y Tecnología*. Mexico: Dirección Gral. de Publicaciones UNAM.

PHILLIPS, A. W. (1954) “Arnold Tustin’s *The Mechanism of Economic Systems:* A Review”. In LEESONA, R. (ed.), *W. H. Phillips: Collected Works in Contemporary Perspective.* Cambridge: Cambridge University Press.

PIKE, D. (2002) "Modernist Space and the Transformation of Underground London". In GILBERT, P. (ed.) *Imagined Londons.* Albany: State of University of New York Press, pp.101-119.

PINCOCK, C. (2012) *Mathematics and Scientific Representation.* New York: Oxford University Press.

PLINY the Elder. *The Natural History.* John Bostock, M.D., F.R.S. H.T. Riley, Esq., B.A. London. Taylor and Francis. Book XXXV, Chap. 36. http://www.perseus.tufts.edu/hopper/text?doc=Perseus%3Atext%3A1999.02.0137%3Abook%3D35%3Achapter%3D36

POTOCHNIK, A. (2015) "The diverse aims of science". *Studies in History and Philosophy of Science* 53, 71-80.

PRICE, W. A. (1978) "Theme Speech: Models – Can we learn from the past?" *Proceedings of the 16th Coastal Engineering Conference*, Hamburg. ASCE, 1, 25-36.

PRITCHARD, (2010) "Knowledge and understanding". In HADDOCK, A., MILLAR, A., and PRITCHARD, D. (eds.) *The nature and value of knowledge: Three investigations.* New York: Oxford University Press, ch.1-4.

QUINE, W. V. (1969) *Ontological Relativity and Other Essays.* New York: Columbia University Press.

RAWLS, J. (1971) *A Theory of Justice*, 2nd Edition 1999, Cambridge, MA: Harvard University Press.

REBER, J. (1959) "Our perpetual gift to California and the nation: A master plan for the vast San Francisco region". National Archives and Records Administration. Pacific Region, Papers of John Reber, Location 2126E-G, Accn. 77- 94-09.

REICHENBACH, H. (1938) *Experience and prediction: An analysis of the foundations and the structure of knowledge.* Chicago: University of Chicago Press.

RENSCHLER, C. S. (2005) “Scales and uncertainties in using models and GIS for volcano hazard prediction”. *Journal of Volcanology and Geothermal Research*, 139, 73–87.

REYNOLDS, O. (1888) “On Certain Laws Relating to the Regimen of Rivers and Estuaries, and on the Possibility of Experiments on a Small Scale”. *Third International Navigation Congress*, 1888.

ROBINSON, M. C. (1992) “Rivers in miniature: The Mississippi Basin Model”. In B. W. FOWLE (ed.) *Builders and fighters: U.S. Army Engineers in World War II*. Fort Belvoir, VA: Office of History, United States Army Corps of Engineers.

RORTY, R. (1979) *Philosophy and the Mirror of Nature*. Princeton, New Jersey: Princeton University Press.

ROTHBART, D. (2004) (ed.) *Modeling: gateway to the unknown: a work by Rom Harre*. Studies in Multidisciplinarity. Amsterdam: Elsevier Science.

ROUSE, H. (1950) *Engineering Hydraulics. Proceedings of the Fourth Hydraulics Conference*. Iowa Institute of Hydraulic Research. New York: Wiley.

ROUSE, H. and INCE, S. (1963) *History of Hydraulics*. Iowa Institute of Hydraulic Research. New York: Dover Publications, 1957.

ROUVRAY, D. (1992) “Definition and Role of Similarity Concepts in the Chemical and Physical Sciences”. *Journal of Chemical Information and Computer Sciences*, 32, 580-586.

SÁNCHEZ-DORADO, J. (2017) “Methodological lessons for the integration of philosophy of science and aesthetics: the case of representation”. In BUENO, O., DARBY, G., FRENCH, S., and RICKLES, D. (eds.) *Thinking about science, reflecting on art. Bringing Aesthetics and Philosophy of Science Together*. London and New York: Routledge, pp.10-26.

SCHAFFER, S. (2004) "Fish and Ships: models in the age of reason". In DE CHADAREVIAN, S. and HOPWOOD, N. *Models: the third dimension of science.* Stanford, Calif: Stanford University Press, pp.71-105.

SCHELLING, T. (1971) "Dynamic models of segregation". *The Journal of Mathematical Sociology*, 1 (2), 143-186.

SCHICKORE, J. (2011) "More Thoughts on HPS: Another 20 Years Later". *Perspectives on Science*, 19 (4), 453-481.

SCHICKORE, J. and STEINLE, F. (2006) *Revisiting Discovery and Justification: Historical and philosophical perspectives on the context distinction*. Dordrecht: Springer Netherlands.

SCHWARZ, R. (1985) "The power of pictures". *Journal of Philosophy*, 82, 189-198.

SHAPIN, S. (1982) "History of science and its sociological reconstructions". *History of Science*, 20, 157-211.

__________(1994) *A social history of truth. Civility and Science in Seventeenth-Century England.* Chicago: Chicago University Press.

SHAPIN, S. and SCHAFFER, S. (1985) *Leviathan and the Air Pump: Hobbes, Boyle and the Experimental Life.* Princeton: Princeton University Press.

SIEGEL, H. (2004) "Rationality and Judgment". *Metaphilosophy*, 35 (5), 597-613.

SIMON, H. (1955) "A Behavioral Model of Rational Choice". *Quarterly Journal of Economics*, 69, 99-118.

SOULOUMIAC, P., et al. (2012) "Bias due to side wall friction in sand box experiments". *Journal of Structural Geology*, 35, 90-101.

SPSP. Mission Statement of the *Society for the Philosophy of Science in Practice.* Consulted 12 January 2018. SPSP Website. http://www.philosophy-science-practice.org/about/mission-statement

STEEL, D. (2010) “A New Approach to Argument by Analogy: Extrapolation and Chain Graphs”. *Philosophy of Science*, 77 (5), 1058-69.

STERRETT, S. (2002) “Physical Models and Fundamental Laws: Using One Piece of the World to Tell about Another”. *Mind and Society* 3 (1), 51-66.

__________(2006) “Models of Machines and Models of Phenomena”. *International Studies in the Philosophy of Science*, 20 (01), 69-80.

__________(2009) “Similarity and Dimensional Analysis”. In MEIJERS, A. (ed.) *Philosophy of technology and engineering sciences*, vol. 9. Amsterdam: North Holland/Elsevier, pp.799-823.

__________(2017a) “Experimentation on analogue models”. In MAGNANI, L. and BERTOLOTTI, T. (eds.), *Springer Handbook of Model-Based Science*. Dordrecht: Springer, pp.857-878.

__________(2017b) “Physically Similar Systems: a History of the Concept”. In MAGNANI, L. and BERTOLOTTI, T. (eds.), *Springer Handbook of Model-Based Science*. Dordrecht: Springer, pp.377-411.

SUÁREZ, M. (2003) “Scientific representation: against similarity and isomorphism”. *International Studies in the Philosophy of Science*, 17 (3), 225-244.

__________(2004) “An Inferential Conception of Scientific Representation”. *Philosophy of Science* 71, 767–79.

__________(2010) “Scientific representation”. *Philosophy Compass* 5/1, 91–101.

__________(2015) Deflationary representation, inference, and practice. *Studies in History and Philosophy of Science*, 49, 36–47.

SUÁREZ, M. and PERO, F. (2016) “Varieties of misrepresentation and homomorphism”. *European Journal for Philosophy of Science*, 6(1), 71-90.

SUPPE, F. (1989) *The Semantic Conception of Theories and Scientific Realism*. Chicago: University of Illinois Press.

SUPPES, P. (1960) “A Comparison of the Meaning and Uses of Models in Mathematics and the Empirical Sciences”. *Synthese*, 12, 287–301.

TELLER, P. (2001) "Twilight of the Perfect Model Model". *Erkenntnis* 55, 393-415.

TOON, A. (2010) "Models as make-believe". In FRIGG, R. and HUNTER, M. (eds.) *Beyond Mimesis and Convention: Representation in Art and Science*. Berlin and New York: Springer, pp.71-96.

__________(2012) "Similarity and Scientific Representation". *International Studies in the Philosophy of Science* 26 (3), 241-257.

TUFTE, E. R. (1997) *Visual and Statistical Thinking: Displays of Evidence for Making Decisions*. Cheshire, Connecticut: Graphics Press.

TVERSKY, A. (1977) "Features of Similarity". *Psychological Review*, 84, 327-352.

TVERSKY, A. and GATI, (1982) "Similarity, Separability, and the Triangle Inequality". *Psychological Review*, 89(2), 123-154.

TVERSKY, A. and KAHNEMAN, D. (1976) "Judgment under Uncertainty: Heuristics and Biases", *Science*, 185, 1124–31.

__________(1983). "Probability, representativeness, and the conjunction fallacy". *Psychological Review*, 90(A), 293-315

UNRUH, W. G. (1981) "Experimental Black-Hole Evaporation?" *Phys. Rev. Lett.*, 46, 1351-53.

US Army Corps of Engineers (1963). *Technical report on barriers: A part of the comprehensive survey of San Francisco Bay and tributaries*. California (Main report). San Francisco: Army Corps of Engineers.

__________(1981). *San Francisco Bay – delta tidal hydraulic model: User's manual.* San Francisco: Army Corps of Engineers.

__________(1997) "Mississippi Basin Model at Jackson." *Water Resources: Hydraulics and Hydrology: Interview with Margaret S. Petersen*. USACE Engineering Pamphlets EP 870-1-60.

van FRAASSEN, B. (1980) *The Scientific Image*. Oxford: Oxford University Press.

__________(2008) *Scientific Representation. Paradoxes of Perspective.* Oxford and New York: Oxford University Press, 2008.

van FRAASSEN, B. and SIGMAN, J. (1993) "Interpretation in Science and in the Arts". In LEVINE, G. (ed.) *Realism and Representation.* Madison, WI: University of Wisconsin Press, pp.73-99.

VERTESI, J. (2008) "Mind the Gap: The London Underground Map and Users' Representations of Urban Space". *Social Studies of Science*, 38 (1), 7-33.

VINES, D. (2000) "The Phillips machine as a 'progressive' model". In LEESONA, R. (ed.), *W. H. Phillips: Collected Works in Contemporary Perspective*. Cambridge: Cambridge University Press, pp.39-67.

WARNOCK, J. E. (1950) "Hydraulic Similitude". In ROUSE, H., *Engineering Hydraulics. Proceedings of the Fourth Hydraulics Conference.* Iowa Institute of Hydraulic Research. New York: Wiley, pp.136-176.

WEISBERG, M. (2013) *Simulation and Similarity.* Oxford: Oxford University Press.

__________(2015) "Response to critics. Biology and Philosophy symposium on Simulation and Similarity: Using Models to Understand the World". *Biol Philos,* 30, 299–310.

WEISBERG, M., and ELLIOTT-GRAVES, A. (2014) "Idealization". *Philosophy Compass* 9/3, 176–185.

WEST, R. (1928) *The Strange Necessity.* Garden City: Doubleday.

WIMSATT, W. (2007) *Re-Engineering Philosophy for Limited Beings. Piecewise approximations to reality.* Cambridge, MA: Harvard University Press.

WINSBERG, E. (2009) "A tale of two methods", *Synthese,* 169 (3), 575-592.

WOODY, A. (2015) "Re-orienting discussions of scientific explanation: A functional perspective". *Studies in History and Philosophy of Science* xxx, 1-9.

YALIN, M. S. (1971) *Theory of Hydraulic Models*. London: Macmillan.

ZAGZEBSKI, L. (2001) "Recovering understanding". In STEUP, M. (ed.), *Knowledge, truth, and duty: Essays on epistemic justification, responsibility, and virtue*. New York: Oxford University Press, pp.236-252.

ZWART (2009) "Scale modelling in engineering: Froude's case". In MEIJERS, A. (ed.) *Philosophy of technology and engineering sciences*, vol. 9, North Holland/Elsevier, Amsterdam, pp.759-798.