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Scientific Representation And Human Action

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SCIENTIFIC REPRESENTATION AND HUMAN ACTION

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DEDICATION

I dedicate this dissertation to the memory of my father who would have been delighted to read it and even more delighted to see all that his children have accomplished.

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ABSTRACT

Increasingly, many philosophers of science agree that an account of representation in science must include an irreducible reference to the intentions, actions, and agency of a scientist. Though these pragmatic accounts of scientific representation have numerous advantages over alternatives, very little has been said about the reference to agency found within. My dissertation uses work from the philosophy of action to fill in some of the missing details, offering a better foundation and more complete picture of the nature of representation in science. I begin with an overview of the literature in an encyclopedia article. I then argue for the communal nature of representation in science, suggesting that we cannot understand scientific representation without first understanding how scientists license representational vehicles to be used for particular purposes. Next, I argue that an account of scientific representation reduced to mental states is mistaken precisely because it leaves out the communal element of licensing. The following paper offers the first ever account of the nature of scientific, representational actions. The Means-End Account of Scientific, Representational Actions relies on the work on the nature of intentional actions of G.E.M. Anscombe and suggests that representational actions in science can be demarcated from other forms of action in virtue of features that hold of their internal form. Relying on a similar point about the relationship between pragmatic accounts of scientific representation and the nature of intentional action, my fourth paper argues that the many pragmatic accounts of scientific representation are complementary with one another and allow a more complete understanding of scientific representation.

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CHAPTER 1

INTRODUCTION: SCIENTIFIC REPRESENTATION AND HUMAN ACTION

1. Introduction

If you were to enter a science department on any given university or accompany scientists as they collect data in the field, there are several things you would observe. Aside from measurement tools, notebooks for data collection, and some subject or phenomenon under investigation, you would observe a group of scientists performing actions: experimenting, theorizing, and, as I shall explore in this dissertation, *representing*. My work in this dissertation argues for, explores, and advances the view that representation in science exists in virtue of the activities of scientists. This view—the pragmatic view of scientific representation—has increased in popularity in recent years and has been defended in a few variations by a number of philosophers of science (e.g., Hughes 1997; Bailer-Jones 2003; Suárez 2004; Van Fraassen 2008; Giere 2004, 2010).

A natural way forward from the important work already conducted by these philosophers is to explore in greater detail the relationship between scientific representation and human activity. The task I take up in this dissertation is to begin this exploration, examining the relationships that hold between scientific representation and human action by drawing connections to the literature from analytic action theory. My work in this dissertation helps to show how it is that the relationship of representation is grounded in human action embedded in a practice. The substantive chapters of the dissertation thus make a useful contribution to the pragmatic perspective of scientific representation, by further developing and explaining the nature and role of actions within scientific representation. Apart from developing and defending an original analysis of the nature of scientific, representational actions (chapter 4), I also argue that the scientific community plays an irreducible role in the representational relationship (chapter 3) and offer an argument which uses the common groundwork among pragmatic accounts of scientific representation to show that they are complementary with one another (chapter 5). Before explaining these arguments in greater detail, I will briefly examine the background from both philosophy of science and philosophy of action.

1. Background

2.1 Philosophy of Science Background

Painting with broad strokes, we can trace some of the developments that lead to pragmatic accounts of scientific representation, paving the way for the insights coming from action theory to be applied to scientific representation. Early accounts of scientific representation were 'substantive' insofar as they offered an account of scientific representation in terms of the features which held between the vehicle (e.g. a model) and its target (e.g. a real-world system). These accounts, at least as they have been commonly interpreted, are "dyadic," (Knuuttila 2005, 1261) since they conceive of representation as a two-place relationship. The two main versions of substantive accounts are similarity-based and structuralist accounts.

Structuralist accounts explain the relationship between representational vehicle and its target in terms of shared structure between the vehicle and target, described in terms of a functional mapping relationship. For example, one common structuralist account says that the representational relationship between a vehicle and its target holds in virtue of an *isomorphic* relationship, namely the fact that we can draw a bijective (one-to-one and onto) function between the structure of the vehicle and the structure of the target. Such a view is most famously attributed to Bas van Fraassen (1980).[1] Of course, an account of representation as isomorphism faces certain problems, especially related to misrepresentation which is itself a common practice within science. As such, most philosophers interested in defending a structuralist account offer instead an account of *partial isomorphism* (e.g., French and Ladyman 1999; French 2003; Bueno and French 2011). The important distinction offered by accounts of partial isomorphism is that they leave room for misrepresentation and non-representational features of a vehicle and its target, since they do not require that every element of the structure be included in the isomorphism.

The second major type of substantive account of scientific representation says that the representational relationship between a vehicle and its target holds in virtue of some form of similarity. Similarity is a relationship which is "a weaker interpretation of the relationship between model and real system" than structural (partial) isomorphism, insofar as it incorporates isomorphism while also leaving room for additional forms of similarity (Giere 1988, 81). The most famous defender of a similarity account is Ronald Giere, who argues that appealing to similarity to explain representation requires an "implicit" (or perhaps in some cases explicit) "specification of relevant *respects* and *degrees*" (1988, 81).

[1] Though as he and Mauricio Suárez (Ladyman et al. 2011) point out, a purely dyadic interpretation of van Fraassen's understanding of scientific representation in his 1980 book constitutes at least a partial misinterpretation of his view, since his account leaves room for the agent. Nonetheless, the dyadic view of his account of representation, as it was commonly interpreted, was rather influential.

He means that when we rely on similarity to explain a representational relationship, we are not invoking just *any* similarities that happen to hold between vehicle and target, but rather specific similarities—taken in some respect, some vehicle is N-degree similar to its target.[2]

Recently, philosophers of science have (for the most part) begun to move away from substantive accounts of scientific representation in favor of pragmatic accounts of scientific representation. A major part of this movement seems to be due to a set of influential arguments against similarity and isomorphism presented and defended by Suárez (2003) among others (e.g. (Frigg 2006). Among the worries he raises are some which follow the work of Nelson Goodman (1976) by arguing that there is a mismatch between the formal properties of representation on the one hand and similarity or isomorphism on the other. Representation is commonly understood to be non-symmetric, non-reflexive, and non-transitive. Isomorphism has the opposite qualities for each of these: it is reflexive, transitive, and symmetric. Similarity *does* share non-transitivity with representation, but differs insofar as it is reflexive and symmetric. There is an essential, logical mismatch between the relationships of similarity and isomorphism on the one hand and representation on the other. As such, both are inadequate with regard to explaining the nature of representation in science.

Increasingly, and in light of these objections to substantive accounts, many philosophers of science agree that if we are to understand the nature of representation in science, we must analyze it as a particular sort of activity—as an activity carried out by agents working within a broader community, for the sake of particular aims and goals. Put

[2] The need to invoke similarities demonstrates, as Suárez suggested, that Giere's account also left room for the role of an agent, even if it was not interpreted that way (Ladyman et al. 2011).

otherwise, we must add another member to the relationship of scientific representation and offer "(at least) triadic" accounts of scientific representation (Knuuttila 2005, 1261). On pragmatic accounts of scientific representation, the relationship holds not only between the vehicle and its target, but also between the user of the vehicle—where the user can be understood as an individual scientist, a group of scientists, or the scientific community in general.

Pragmatic accounts of scientific representation, now quite numerous, offer significant insights into the nature of scientific representation (Hughes 1997; Teller 2001; Bailer-Jones 2003; Suárez 2004, 2010, 2015a, Giere 2004, 2010; Contessa 2007; Van Fraassen 2008; Mäki 2009; Frisch 2014). The details vary from account to account, but the unifying feature of these accounts is an irreducible place for agency: a vehicle represents its target because a scientist or group of scientists *uses* it that way. However, despite the insights offered by these accounts and their value in being better able to explain the heterogeneity of representational practice in science, there remains important work to be done on this approach to the issue of representation in science—including, as I will explore in this dissertation, on the nature of the human agency which undergirds representation in science.

While pragmatic accounts have made good use of the notions of action, agency, intention, and use, they have for the most part left these terms unanalyzed and unexplained. Given their importance for the pragmatic account of representational relationship (the scientist's action of using the representation is one of the three members of the 'triadic' account of representation), further analysis of scientific actions stands to deepen and advance a pragmatic approach to representation in science. It is for this reason that my dissertation turns to insights about the nature of human action drawn from the literature in analytic philosophy of action.

2.2 Philosophy of Action Background

Of course, there are a wide range of views and insights to be found within action theory. In order to draw insights, it is important to first narrow down the list of potential sources of insights in action theory by referring to ideas about the relevant form of agency found within pragmatic accounts of scientific representation. While very little has been said about what is meant by agency, there are two guiding points that I take. The two points, one negative and the other positive, are each related to the relationship between scientific representation and individual mental states.

First is the negative claim that scientific representation cannot be explained *only* through reference to individual mental states. Suárez, for example, is explicit in making this negative claim when he denies the view that "representation is necessarily the property of individuals: representation is 'in the head'" (Ladyman et al. 2011, 432; c.f. Suárez 2010, 98–99). On a pragmatic conception of scientific representation, Suárez argues that we must avoid "[a]n intentionality conception [of scientific representation which]… takes it that sources and targets are determined by some intentional state of some particular agent or agents, regardless of community, practice, and indeed any intended or unintended uses" (Ladyman et al. 2011, 432). As he says elsewhere, scientific representation is not "in the head" (Suárez 2010, 99). Thus, the relevant account of action which will be able to advance and deepen pragmatic accounts of scientific representation must not be such that it relies exclusively on the nature of mental states.

The second, positive claim offers an idea about what *should* be included in an account of action which is capable of playing such a role. What is important, according to the general ideas described by Suárez, is that we be able to understand the relevant actions as being communal and socially-based, embedded in the broader practice of science. As he puts it, scientific representation is to be found "'in the world', and more particularly in the social world – as a prominent activity or set of activities carried out by those communities of inquirers involved in the practice of scientific modelling" (Suárez 2010, 99). As such, a proper account of action for the purposes at hand will be such that it includes the broader scientific practice and the context in which the action is performed as essential components in the process of analysis.

In light of these considerations, it quickly becomes clear that the 'standard view' in analytic philosophy of action will not suffice. The 'standard view' or 'causal theory' argues that an action is intentional provided it has the right sort of causal relationship to an agent's mental states. That is to say that an action is intentional provided it is caused by intentions, understood as a particular form of mental state (e.g. a desire or a belief). In light of this central claim, we can see that it fails to meet the negative claim: that representation will not be sufficiently explained in terms of mental states. In addition to this basic problem, there are additional issues with the causal theory of action which makes it unsuitable to the task at hand.

One of the additional concerns with the causal theory for my purposes is explained by Harry Frankfurt (1998). The causal theory explains intentional actions in terms of the causal antecedents of an action (the mental states) and so "direct[s] attention exclusively away from the events whose natures are at issue, and away from the times at which they occur" (Frankfurt 1998, 70). Importantly, this means that the causal theory fails to take account of the fact that actions are temporally extended. It can explain only what happens before an action begins. After this point, "the subsequent 'effects'—i.e., the actual performance or doing, is simply a matter of nature taking its course" (Frey 2013, 4). As a philosopher of science who wants to draw our attention closer to scientific practice, this turns out to be no small failure. The causal theory turns our attention inward toward mental states, instead of outward towards the broader scientific community and the actual performances and doings of the scientist who represents.

For these reasons, I avoid the standard account and rely instead upon a different tradition within philosophy of action which takes the work of G.E.M. Anscombe, especially her book *Intention* (2000), as its inspiration. The Anscombean perspective on the nature of action is markedly different for several reasons, though I will focus on those which are relevant to the concerns at hand. For one, Anscombe meets the negative claim made by Suárez, since she is clear that an account of mental states will not suffice as an account of intentional action. As she says: "intention is never a performance in the mind" (2000, 49).

Furthermore, Anscombe offers an account of action which draws our attention to the actions itself. Indeed, she argues that "the only events to consider [in describing intentional actions] are intentional actions themselves…" (2000, 29). The analysis offered of intentional actions is made in terms of the descriptions of actions, identified by the application of a particular sort of "Why?" question. That is, if we ask someone who is engaged in an act of ϕ-ing, "Why are you ϕ-ing?" and they do not refuse this question, then the ϕ-ing is intentional (Anscombe 2000, 9). Intentional actions, on her analysis, are distinguished in virtue of the form which holds of the action descriptions (Anscombe 2000, 84). The form is seen in the teleological order within the characterizations of an action that is revealed by the 'Why?' question: that someone is A-ing because she's B-ing and B-ing because she's C-ing. It is this teleological, means-end order which reveals the formal characteristics of practical knowledge: it "shews what good, what use, the action is" (Anscombe 2005, 114). Importantly, such a conception of intentional actions leaves plenty of room for the role of the scientific community—in showing the use of an action, in framing the action descriptions, and so on.

Anscombe's account has proven to be useful for the purposes of my dissertation, precisely because it allows that we pay close attention to the scientist's actions, and their internal structure in a way which does not exclude or eliminate the role of the broader scientific community. The influence of Anscombe's work is most clearly present in chapter four, in which I offer an account of scientific, representational actions which explicitly draws upon her views. It is also clear in chapter five, in which I use her work on action descriptions to defend a view about the relationship between pragmatic accounts of scientific representation. Though it is less salient, there is a similar influence to be found in chapter three. In that chapter, I rely upon views presented by Wittgenstein in the *Philosophical Investigations* (2009), which influenced Anscombe.

2. Descriptive Overview of the Five Major Chapters

Below, I will offer a brief overview of each of the chapters of my dissertation, with an eye towards identifying some of the central themes of the chapters and showing how they connect to the central project of my dissertation.

Chapter two is entitled, "An Overview of Philosophical Accounts of Scientific Representation." The chapter provides a substantial and in-depth overview of some of the most influential philosophical and sociological accounts of scientific representation. The article offers an overview of influential views on scientific representation, arranged in a semi-historical narrative about the development of views. In addition to overviewing philosophical accounts of scientific representation, I also cover accounts of the nature of representation within the model literature and as it is discussed by sociologists of science. As can be seen, the semi-historical account of the development of the philosophical and sociological literature on scientific representation plays an important role throughout my dissertation, providing the more general theoretical context for the other chapters. Among other things, the second chapter demonstrates the importance of moving forward with pragmatic accounts by further developing the notions of agency and action which are central to these accounts.

Chapter three is entitled, "There *Is* a Special Problem of Scientific Representation." There are two primary objectives of the article. First is a response and argument against Craig Callender and Jonathan Cohen's influential article, "There Is No Special Problem about Scientific Representation" (2006). Callender and Cohen argue that the constitution of representation in science is no different from the constitution of representation in any other area, for example art or language. So, on their account, the explanation of how it is that the Lotka-Volterra model represents its target will be exactly the same as why Picasso's *Guernica* represents its target. They are open to what the relevant nature of representation is, but suggest that the answer will involve a more fundamental representational entity like mental states. Thus, they think that philosophers of science have been "concerned with non-issues" (Callender and Cohen 2006, 67).

I respond by suggesting that there is something unique about representation in science: the way in which representations are *licensed*. Licensing is the set of activities by which the scientific community constructs, develops, establishes, modifies, and uses a representational vehicle. Indeed, I argue, we cannot understand the nature of representation in science without being attentive to the element of licensing, which extends beyond mental states and includes an irreducible reference to the practice in which a representation is embedded. As such, I take it that there is still significant work to be done by philosophers of science: in understanding how it is that representational vehicles are licensed and used by scientists. Thus, one of the major roles of this chapter is clearing intellectual ground for continued work on the philosophy of scientific representation, by showing that it cannot be reduced to something more fundamental like mental states.

Chapter four is entitled "The Means-End Account of Scientific, Representational Actions." In this chapter, I take seriously the idea which lies at the heart of pragmatic account of scientific representation: that it is grounded in human actions embedded in the broader scientific practice. Though pragmatic accounts of scientific representation make it difficult to offer further substantial analysis of the representational relationship (Suárez 2015a, 47; Knuuttila 2009, 144), I begin my chapter by identifying a few ways to move forward to increase our philosophical understanding of representational practice in science. One of these ways, which has been and will continue to be explored, is to examine the nature and role of specific classes of vehicles, like models, diagrams, model organisms, and so on (as is suggested by Knuuttila 2011). Another way forward is to examine what

Suárez calls the "means" of scientific representation (2004, 229)—the relationships which scientists oftentimes utilize in representing. Exploring representational means can include exploring similarity, isomorphism, and other structural relationships of similarity. But there is a third way forward, I argue, which has not yet been thoroughly explored: to better understand the nature of the actions by which scientists represent.

In the chapter, I offer a novel analysis of the nature of scientific, representational actions. My account relies primarily on the work of G.E.M. Anscombe, taking her account of the nature of intentional actions as a starting point. After introducing her work, I turn to offer the Means-End Account of Scientific, Representational Actions, in which scientific, representational actions are demarcated from other forms of actions in virtue of three features that hold of their internal structure: I) the final description in the means-end ordering of descriptions is some scientific aim; (II) that interaction with a vehicle distinct from its target stands as an earlier description which is ordered toward the final description as means to end; and (III) the means-end structure is licensed by scientific practice, in the sense that I describe in chapter three.

After applying the account to an example involving the Lotka-Volterra model, I turn to examine some of the payoffs of the account. First, as a novel account of the nature of scientific, representational actions, the Means-End Account provides a means by which we can better understand representational practice in science. Instead of paying attention only to vehicles (like models) or other foundational relationships (like isomorphism), we can now be attentive to the actions themselves and see their role in representational practice. Attending to the actions is valuable not only because it helps to illuminate the foundation upon which pragmatic accounts of scientific representation are built, but also because it helps to explain the relationship that holds between the use of vehicles as means and the achievement of certain ends. Furthermore, the account suggests that every instance of representation in science is also an instance of some other form of scientific activity: an explanation, a prediction, an experimentation, and so on. While more work needs to be done to flesh out this connection, the Means-End Account offers fruitful material for further reflection on these issues.

The fifth chapter is entitled, "Pragmatic Accounts of Scientific Representation: A Tapestry of Explanation." It explores an additional payoff of attending to the foundational role of action in pragmatic accounts of scientific representation. There are a wide range of pragmatic accounts of scientific representation, each of which offers its own unique analysis of representation. They are all unified, however, in virtue of their reliance upon the actions of the representing scientist(s). In this chapter, I argue that the common foundation of human agency provides a means by which we can understand how the plurality of pragmatic accounts of scientific representation are complementary and indeed can offer a broader lens through which we can understand representational practice in science. I review Anscombe's account of intentional actions and suggest that what each account offers is a different set of action descriptions or characterizations. I argue that a single action can have a number of sets of descriptions, each of which is valid, though which might have different emphases. So, pragmatic accounts of scientific representation are describing one and the same representational action, though they draw our attention to different elements and features of the representational action. Most importantly, I argue, the plurality of pragmatic accounts of scientific representation is useful because it can help to better understand the complex and heterogeneous practices of scientific representation.

CHAPTER 2

AN OVERVIEW OF PHILOSOPHICAL ACCOUNTS OF SCIENTIFIC REPRESENTATION[3]

Scientific representation is the important and useful relationship that holds between scientific sources (e.g. models, theories, data models, etc.) and their targets (e.g. real-world systems, theoretical objects, etc.). There is a long history of describing the nature of the representational relationship between concepts and their objects, but the discussion on *scientific* representation is a relatively recent discussion within the philosophy of science. There are a number of different questions one can ask when thinking about scientific representation. The question which has received the most attention, and which will receive the most attention here, is what might be called, following Callendar and Cohen (2006, 68) the "constitution question" of scientific representation: 'In virtue of what is there representation between scientific sources and their targets?' This has been answered in a wide variety of ways, some arguing that it is a structural identity or similarity which ensures representation while others argue that there is only a pragmatic relationship. Other questions about scientific representation relate more specifically to the ways in which representations are used in science. These questions are more typically asked directly about certain sorts of representational objects, especially scientific models, as well as from the perspective of sociology of science.

[3]Boesch, B. (2015) Scientific representation. *Internet Encyclopedia of Philosophy*. http://www.iep.utm.edu/sci-repr/. Reprinted here with permission of publisher.

1. Substantive Accounts

Scientific representation became a rising topic of interest with the development of the semantic view of theories which was itself developed partly as a response to the syntactic view of theories. Briefly, on the syntactic view, theoretical terms are defined in virtue of relationships of equivalence with observational entities (Suppe 1977). This was done through the creation of a first-order predicate calculus which contained a number of logical operators as well as two sets of terms, one set filled with theoretical terms and the other with observational terms. Each theoretical term was defined in terms of a correspondence rule linking it directly to an observational term. The logical language also included a number of axioms, which were relations between theoretical terms. These axioms were understood as the scientific laws, since they showed relationships that held among the theoretical terms. Given this purely syntactic relationship between theory and observed phenomena, there was no need to give any more detailed account of the representation relationship that held between them. The correspondence rule syntactically related the theory with observations.

The details of the rejection of the syntactic view are beyond the scope of this article, but suffice it to say that this view of the structure of theories was widely rejected. With this rejection came a different account of the structure of theories, what is often called the semantic view. Since there was no longer any direct syntactic relationship between theory and observation, it became of interest to explain what relationship does hold between theories and observations, and ultimately the world.

Before examining the accounts of scientific representation that arose to explain this relationship, we should get a basic sense of the semantic view of theories. The common feature of the semantic approach to scientific theories was that they should not be thought of as a set of axioms and defined syntactic correspondence between theory and observation. Instead, theories are "extralinguistic entities which may be described or characterised by a number of different linguistic formulations" (Suppe 1977, 221). That is to say, theories are not tied to a single formulation or even to a particular logical language. Instead theories are thought of as being a set of related models. This is better understood through Bas van Fraassen's (1980) example.

Van Fraassen (1980, 41–43) asks us to consider a set of axioms which are constituents of a theory which will be called T_1:

A0 There is at least one line.

A1 For any two lines, there is at most one point that lies on both.

A2 For any two points, there is exactly one line that lies on both.

A3 On every line there lie at least two points.

A4 There are only finitely many points.

In Figure 2.1, we can see a model which shows that T_1 is consistent, since each of the axioms is satisfied by this model.

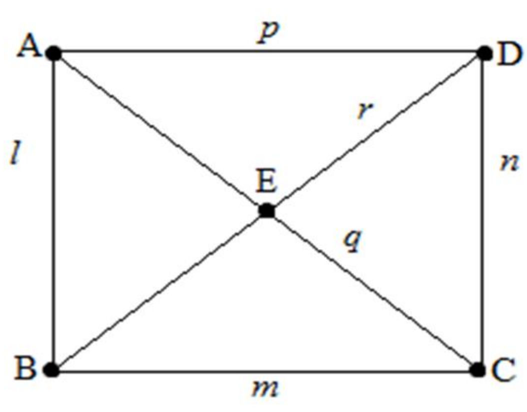

Figure 2.1 Model of Consistency of T_1

Notice this is just one model which shows the consistency of T_1, since there are other models which could be constructed to satisfy the axioms, like van Fraassen's "Seven Point Geometry" (1980, 42). Note that what is meant here by 'model' is whatever "satisfies the axioms of a theory" (Van Fraassen 1980, 43). Another, perhaps more intuitive, way of expressing this is that a model for any theory T is any model which would make T true iif the model were the entirety of the universe. For example, if Figure 1 were the entirety of the universe, then clearly T_1 would be true. Notice also that, on the semantic view, the axioms themselves are not central in understanding the theory. Instead, what is important in understanding a theory is understanding the set of models which are each truth-makers for that theory, insofar as they satisfy the theory.

This account of the structure of theories can be applied to an actual scientific theory, like classical mechanics. Here, following Ronald Giere (1988, 78–79), we can take up the example of the idealized simple systems in physics. These are, he argues, models for the theory of classical mechanics. For example, the simple harmonic oscillator is a model which is a truth-maker for (part of) classical mechanics. The simple harmonic oscillator can be described as a machine: "a linear oscillator with a linear restoring force and no others" (Giere 1988, 79); or mathematically: $F = -kx$. This model, were it the entirety of the universe, would make classical mechanics true.

The targets of theoretical models on the semantic view are not always real world systems. On some views, there is at least one other set of models which serve as the targets for theoretical models. These are variably called empirical substructures (Van Fraassen 1980) or data models. These are ways of structuring the empirical data, typically with some mathematical or algebraic method. When scientists gather and describe empirical data, they tend to think of and describe it in an already partially structured way. Part of this structure is the result of the way in which scientists measure the phenomena while being particularly attentive to certain features (and ignoring or downplaying others). Another part of this structuring is due to the patterns seen in the data which are in need of explanation. On some views, most notably van Fraassen's (1980), the empirical model *is* the phenomenon which is being represented. That is to say, there is no further representational relationship holding between data models and the world, at least as scientific practice is concerned (for a discussion of this, see (Brading and Landry 2006)). Others argue that the relationship between theoretical models and data models is only one of a number of interesting representational relationships to be described, which set themselves up in a hierarchical structure (French and Ladyman 1999, 112–14).

With this semantic account of the structure of scientific theories in place, there arose an interest to give an account of the representational relationship. The views which arose with the semantic view of theories are here called "substantive," because they all attempt to give an account of the representational relationship which looks to substantive features of the source and target. Another way of putting this (following Knuuttila (2005)) is to say that the substantive accounts of representation seek to explain representation as a dyadic relationship which holds between only the source and the target. As will be discussed below, this is different from the deflationary and pragmatic accounts which view scientific representation as at least a triadic relationship insofar as they add an agent to the relationship. There are two major classifications of substantive accounts of the representational relationship. The first are the structuralist views which are divided into three main types: isomorphism, partial isomorphism, and homomorphism. The second category is the similarity views.

1.2. Structuralist Views

Generically, the structuralist views claim that scientific representation occurs in virtue of what might be called "mapping" relationships that hold between the structure of the source and the structure of the target, i.e. the parts of the theoretical models point to the parts of the data models.

1.2.1. Isomorphism

Isomorphism holds between two objects provided that there is a bijective function—i.e., both injective (or one-to-one) and surjective (or onto)—between the source and the target. Formally, suppose there are two sets, set A and set B. Set A is isomorphic to set B (and vice versa) if and only if there is a function, call it *f*, which could be constructed between A and B which would take each member of set A and map it to one and only one member of set B such that each member of set B is mapped.

To make the point more clear, let us suppose that set A is full of the letters of the English alphabet and set B is full of the natural numbers 1 through 26. We could create a function which, when given a letter of the alphabet, will output a number. Let's make the function easy to understand and let *f*(A) = 1, *f*(B) = 2, and so on, according to typical alphabetical order. This function is bijective because each letter is mapped to one and only one number and every number (1 - 26) is being picked out by one and only one letter. Notice that since we can draw a bijective function from the letters to the numbers, we can also create one from the numbers to the letters: most simply, let *f'*(1) = A, *f'*(2) = B, and so on. Of course, there is nothing apart from the ease of our understanding which requires that we link A and 1, since we could have linked 1 with any letter and vice versa.

Isomorphism has frequently been used to explain representation (Van Fraassen 1980; Brading and Landry 2006). Since theories, on the semantic view, are a group of related models, there is a certain sort of structure that each of these models has. Most of the time, they are thought of as mathematical models though they need not be only mathematical as long as they have a structure. Van Fraassen also identifies what he calls "appearances," which he defines as "the structures which can be described in experimental and measurement reports" (1980, 64). So, the appearances are the measurable, observable structures which are being represented (the targets of the representation). On van Fraassen's account, a theory will be successfully representational provided that there is an isomorphic relationship between the empirical substructures (the sources) and the appearances (as targets), and an isomorphic relationship between the theoretical models (as sources) and the empirical substructures (as targets). (Or at any rate, this is how he has commonly been interpreted (see Ladyman et al. 2011). As described by Mauricio Suárez, this isomorphism between the models shows that there is an identity that holds between the "relational framework of the structures" of the source and the target (2003, 228). And it is this relational framework of structures which is being maintained.

So, on the isomorphism view of scientific representation, some scientific theory represents some target phenomena in virtue of a bijective mapping between the structures of a theory and data, and a bijective mapping between the data and the phenomena. Notice that on the isomorphic view, the bijections which account for representation are external to the theoretical language. That is to say that the relationship that holds between the theory and the phenomena is not internal to the language in which the theory is presented. This is an important feature of this account because it allows for a mapping between very different kinds of structures. Presumably the (mainly mathematical) structures of theories are quite different from the structures of data models and are certainly very different from the structures of the phenomena (because the phenomena are not themselves mathematical entities). However, since the functions are external, we can create a function which will map these very different types of structures to one another.

1.2.2. Partial Isomorphism

Isomorphism has much to suggest for it, especially when focusing in particular on those theories which are expressed mathematically. This is especially true in more mathematically-driven fields like physics. It seems that the mathematical models in physics are representing the structure that holds between various real world phenomena. For example, $F=ma$ represents the way in which certain features of an object (its mass, the rate at which it is being accelerated) correspond to other features (its force). However, many philosophers (e.g., Cartwright 1983; Cartwright, Shomar, and Suárez 1995) have pointed out that there are cases where a theory or model truly represents some phenomena, even though there are features of the phenomena which do not have any corresponding structure in the theory or model, due to abstraction or idealization.

Take a rather simple example, the billiard ball model of a gas (French and Ladyman 1999). Drawing on Mary Hesse’s (1966) important work on models, French and Ladyman argue that there are certain features of the model which are taken to be representative, e.g. the mass and the velocity of the billiard balls represent the mass and velocity of gas atoms. There are also certain features of the billiard balls which are non-representational, e.g. the colors of the balls. Most importantly, though, as a critique of isomorphism, there are typically also some undetermined features of the balls. That is to say, for some of the features of the model, it is unknown whether they are representational or not. For a more detailed scientific example, see Cartwright, Shomar, and Suárez (1995).

To respond to problems of this sort, many (Bueno 1997; French and Ladyman 1999; French 2003; Da Costa and French 2003) have argued for *partial* isomorphism. The basic idea is that there are partial structures of a theory for which we can define three sets of members for some relation. The first set will be those members which do have the relevant relation, the second set will be those members which do not have that relation, and the third will be those members for which it is unknown whether or not they have that relation. It is possible to think of each of these sets of individuals as being a relation itself (since a relation, semantically speaking, is extensionally defined), and so we could draw a bijective function between these relations. But, as long as the third relation (the third set of individuals for which it was unknown whether or not they had the relation) is not empty, then the isomorphism will be only partial because there are some relations for which we are unsure whether or not they hold in the target.

As a more concrete example, consider the billiard ball and atom example from above. In order for there to be a partial isomorphism between the two, we must be able to identify two partial structures of each system, i.e. a partial structure of the billiard ball model and a partial structure of the gas atoms. Between these partial structures, there must be a bijective function which maps relations of the model to relations of the gas-system. For example, the velocity of the billiard balls will be mapped to the velocity of respective atoms. There must be a second function which maps those non-representational relations of the model to features of the gas-system which are not being represented. For example, a non-representative feature of the model, like the color of the billiard balls, will be mapped to some feature of the system which is not being represented, like the non-color of the atoms. All the same, this will still remain partial because there will be certain relations that the model has which are unknown (or undefined) in relationship to the gas-system.

1.1.3. Homomorphism

Homomorphism, defended by Bartels (2006), is more general than isomorphism insofar as all isomorphisms are homomorphisms, but not all homomorphisms are isomorphisms. Homomorphisms still rely on a function being drawn between two sets, but they do not require that the function be bijective; i.e. the function need not be one-to-one or onto. So, this means that not every relation and part of the theory must map on to one and only one relation or part of the target systems. Additionally, this permits that there be parts and relations in the target system which are unmapped. Homomorphisms allow for a great deal of flexibility with regard to misrepresentations.

1.2. Similarity

Isomorphism (and the other -morphisms) places a fairly strict requirement on the relevant constitutive features of representation, which are on these views structural. But, as Giere points out (1988, 80–81), this is often not the relevant relationship. Oftentimes, scientists are working with theories or models which are valuable not for their salient *structural* features, but rather for some other reason. For example, when modeling the behavior of water flowing through pipes, scientists often model the water as a continuous fluid, even though it is actually a collection of discrete molecules (Giere 2004). Here, the representational value of the model is not between the structure of the model and the structure of the world (since water is structurally not continuous, but rather a collection of discrete molecules). Instead, the relevant representational value comes from a more general relationship which holds between the *behavior* of the modeled and real world systems. Giere suggests that what is needed is “a weaker interpretation of the relationship between model and real system” (1988, 81). His suggestion is that we explain representation in virtue of similarity. On his account a model will represent some real world system insofar as it is similar to the real world system. Notice that this is a much weaker account of representation than the structural accounts, since similarity includes structural similarities, and so encompasses isomorphism, partial isomorphism, and homomorphism.

Of course, if we try hard enough, we can notice similarities between any two objects. For example, any two material objects are similar at least insofar as they are each material. Thus, Giere suggests that an account of scientific representation which appeals to similarity requires an “implicit” (or explicit) “specification of relevant *respects* and *degrees”* (Giere 1988, 81). Respects indicate the relevant parts and ways in which the model is taken to be representative. Perhaps it is some dynamical relationship expressed in an equation; perhaps it is some physical similarity that exists between some tangible model and some target object (e.g. a plastic model of a benzene ring); perhaps it is the way in which two parts of a model are able to interact with one another, which shows how two objects in the target system might interact (like the relevant behavior of the model of water flowing through pipes). The limitations with regard to claims of the respects of similarity are limited only by what scientists know or take to be the case about the model and the target system. For example, a scientist could not claim that there was a similarity between the color of a benzene model and a benzene ring since benzene rings have no color.

Similarly, a scientist could not claim that there is similarity between the color of a mathematical model and the color of a species of bacteria since a mathematical model does not have any color. Notice that it is insufficient to merely specify the respects in which a model is similar since similarity can come in degrees. Of course, there is a whole spectrum of degrees of similarity on which any particular similarity can fall. A source can be anywhere from an extremely vague approximation of its target to being nearly identical to its target (what Giere calls "exact" (1988, 93)) and everywhere in between.

Giere's own example is that, "The positions and velocities of the earth and moon in the earth-moon system are very close to those of a two-particle Newtonian model with an inverse square central force" (1988, 80). Here, the relevant respects are the position and velocity of the earth and moon. The relevant degree is that the positions and velocities in the earth-moon system are "very close" to the two-particle Newtonian model. These respects and degrees thus give us an account of how we should think of the similarity between the model and the target system.

Giere uses similarity to describe the relationship between models and the real-world systems they represent, and sometimes between different models (one model may be a generalization of another, and so on). Theories themselves are constituted by a set of these models as well as some hypotheses that link the models to the real world which define the respect and degree of the similarity between the models and their targets.

More recently, Weisberg (2013) has argued for a similarity account of representation. In brief, his view argues that two sets of things be distinguished in both source and target: the attributes and the mechanisms. In distinguishing these sets, an equation can be written in which the common attributes and mechanisms can be thought of as the intersection of the attributes of the model and of the target system, and the intersection of the mechanisms of the model and target system. The dissimilarities can also be identified in a similar fashion. He adds some terms to these sets which are weighting terms and functions. These allow the users to indicate which similarities are more important than others. Rewriting the equation as a ratio between similarities and dissimilarities will result in a method by which we can make comparative judgments about different models. In this way, we will be able to say, for example, that one model is more or less similar than another.

1.3. Critiques of Substantive Accounts

While similarity and isomorphism continue to have some support in the contemporary literature (especially in modified versions, see below, section 3c), the versions described above have faced serious criticisms. One of the most common arguments against the substantive views is that they are unable to handle misrepresentations (Suárez 2003, 233–35; Frigg 2006, 51). Many models in science do not accurately reflect the world, and, in fact, the model is often viewed as particularly useful because of (not in spite of) the misrepresentations. Nancy Cartwright (1983) has famously argued for a fictional account of modelling and made this case for the laws of physics. Others have shown that similar things are true in other scientific domains (Weisberg 2007a). When the theories are intentionally inaccurate, there will be difficulty in explaining the way in which these theories *are* representational (as scientists and philosophers often take them to be), with reference to isomorphism or similarity.

Suárez (2003, 235–37) has also argued that both similarity and isomorphism are each neither necessary nor sufficient for representation. Consider first isomorphism. It must be the case that it is not necessary for representation, given that scientists often take certain theories to be representative of their real-world targets even though there is no isomorphic relationship between the theory and the target system. The same is true of similarity. Using his example, suppose that there is an artist painting an ocean view, using some blue and green paints. This painting has all sorts of similarities to the ocean view she is representing, one of which is that both the painting and the ocean are on the same relative side of the moon, are both in her line of vision at time *t*, share certain colors, etc. But which ones are relevant to its being representative and which are more contingent is up to the discretion of the agent who takes it to be representative of the ocean view in certain respects (as Giere argued). But if this is the case, then it turns out that A represents B if and only if A and B are similar in those respects in which A represents B. This ultimately leaves representation unexplained.

Supposing we can give some account of salience or attention or some other socially-based response to this first problem (which seems possible), we are left with the problem that plenty of salient similarities are non-representational. Suárez makes this point with Picasso's *Guernica* (2003, 236). The bull, crying mother, eye, knife, etc. are all similar to certain real-world objects. But the painting is *not* a representation of these other things. It is representing some of the horrible atrocities of Franco.

Suárez also argues that both similarity and isomorphism are insufficient for representation. Consider the first, similarity. Take any given manufactured item, for example, an Acer C720 Chromebook, a computer which is similar to many other computers (hundreds of thousands). Notice that the fact of its similarity is insufficient to make it represent any of the other computers. Even if we add in Giere's requirement that there be hypotheses which define the respects and degrees of the similarity, the insufficiency will remain. In fact, it seems as though there are hypotheses which define the relevant respects and degrees of similarity between the computers: Acer's engineers and quality control have made sure that the production of these computers will result in similar computers. All the same, even with these hypotheses which give respects and degrees, we would not want to say that any given computer represents the others.

The non-sufficiency problem holds for isomorphism as well. Suppose someone were to write down some equation which had various constants and variables, and expressed certain relationships that held between the parts of the equation. Suppose now that, against all odds, this equation turns out to be isomorphic to some real-world system, say, that it describes the relationship between rising water temperatures and the reproduction rate of some species of fish which is native to mountain streams in the Colorado Rockies. To many, it appears to be counterintuitive to think that representations could happen accidentally. However, if isomorphism is sufficient for representation, then we would have to admit that the randomly composed equation does represent this fish species, even if no one ever uses or even recognizes the isomorphic relationship.

There are other arguments against these views in general, an important one being that they lack the right logical properties. Drawing on the work of Goodman (1976), both Suárez (2003, 232–33) and Roman Frigg (2006, 54) argue that representation has certain logical properties which are not shared by similarity or isomorphism. Representation is non-symmetric, so when some A represents B, it does not follow that B represents A. Representation is non-transitive: if A represents B and B represents C, it does not follow that A represents C. It's also non-reflexive: A does not represent itself. Since isomorphism is reflexive, transitive, and symmetric, and similarity is reflexive and symmetric, they do not have the properties required to account for representation.

There are replies to these arguments on behalf of the substantive views. First, there is a general question about whether or not we are justified in making inferences from representation in art to representation in science. As was discussed above, many of the criticisms against substantive views draw examples from the domain of art (e.g., Suárez's (2003) uses many examples of paintings and is drawing upon Goodman's (1976) which discusses representation in art). But, it should not be taken as given that what holds in art must translate to science. In fact, in many cases, the practices in art seem to be quite different from the practices in science. As Bueno and French say, "After all, what do paintings—in particular those that are given as counter-examples to our approach, which are drawn from abstract art—really have to do with scientific representation?" (2011, 879).

Following Anjan Chakravartty (2010), Bueno and French (2011) argue that something like similarity or partial isomorphism *is*, in fact, necessary for successful representation in science. If there were no similarity or isomorphism at all, the successful use of models "would be nothing short of a miracle" (2011, 885). That is to say, while similarity or partial isomorphism might not be the whole story, they are at least part of the story. Using the aforementioned example of Picasso's *Guernica*, they note that "there has to be some partial isomorphism between the marks on the canvass and specific objects in the world in order for our understanding of what *Guernica* represents to get off the ground" (2011, 885).

Replies have been made to the other arguments as well. Bueno and French (2011) argue that their account of partial isomorphism can meet all of the criticisms raised by

Suárez (2003) and Frigg (2006). Adam Toon (2012)discusses some of the ways in which supporters of a similarity account of representation might respond to criticisms. Bartels (2006) defends the homomorphism account against these criticisms.

2. Deflationary and Pragmatic Views

If, as these scholars have argued, these substantive views will not work to explain scientific representation, what will? Suárez (Suárez 2015a) argues that what is needed instead is a deflationary account. A deflationary account claims "that there is no substantive property or relation at stake" (Suárez 2015a, 37) in debates about scientific representation. Deflationary accounts are typically marked by a couple of features. First, a deflationary account will deny that there are any necessary and sufficient conditions of scientific representation, or if there are, they will lack any explanatory value with regard to the nature of scientific representation. Second, these accounts will typically view representation as a relationship which is deeply tied to scientific practice. As Suárez puts it, "it is impossible, on a deflationary account, for the concept of representation in any area of science to be at variance with the norms that govern representational practice in that area...representation in that area, if anything at all, is *nothing but* that practice" (Suárez 2015a, 38).

Already we can see that these views will be quite different from the substantive views. Each of these views was substantive in the sense that they gave necessary and sufficient conditions for representation. There was also a distinct way in which these views were detached from scientific practice, since whether something was representational had little to do with whether or not it was accepted by scientists as representational and more to do with the features of the source and target. In each case, it was a relationship that was entirely accounted for by features of the theory or model and the target system. As

Knuuttila (2005) describes it, these were all dyadic (two-place) accounts insofar as the relationship held between only two things. The deflationary accounts take a markedly different direction by moving to at least a triadic (three-place) account of representation.

In some cases, the views that have developed have followed the general lead of many deflationary views in giving a central role to the work of an agent in representation. These views do not classify as deflationary, given that they still give necessary and sufficient conditions of representation. Given the importance of the role of agents and aims, we might call these views pragmatic. Although pragmatic and deflationary views are importantly distinct in their aims, they share many common threads and in many cases, the views could be reinterpreted as deflationary or pragmatic with little effort. As such, they will be grouped together in this section.

2.1. DDI

The earliest deflationary account of representation was RIG Hughes' DDI Account (1997). The DDI Account consists of three parts: denotation, demonstration, and interpretation. Denotation is the way in which a model or theory can reference, symbolize, or otherwise act as a stand-in for the target system. The sort of denotation being invoked by Hughes is broad enough to include the denotation of concrete particulars (e.g. a model of the solar system will denote particular planets), the denotation of specific types (e.g. Bohr's theory models not just this hydrogen atom, but all hydrogen atoms), and the denotation of a model of some global theory (e.g. this particular model is "represented as a *quantum system*" (Hughes 1997, S331)). In each case, the model denotes something else; it stands in for some particular concrete object, some type of theoretical object, or some type of dynamical system.

We might think this relationship sufficient for representation, since the fact that scientists treat certain objects or parts of models as being stand-ins or symbols for some target system seems to answer the question of the relationship between a model and the world. Hughes, though, thinks that in order to understand scientific representation, we need to examine how it is actually used in scientific practice. This requires additional steps of analysis. The second part of Hughes' DDI Account is demonstration. This is a feature by which models "contain resources which enable us to demonstrate the results we are interested in" (Hughes 1997, S332). That is, models are typically "representations-as," meaning not only do scientists represent some target object or system, but they also represent it in a certain way with certain features made to be salient. The nature of this salience is such that it allows users to draw certain types of conclusions and make certain predictions, both novel and not. This is demonstration in the sense that the models are the vehicles through which (or in which) these insights can be drawn or demonstrated, physically, geometrically, mathematically, etc. This requires that they be workable or used in certain ways.

The final part of the DDI Account is interpretation. It is insufficient that the models demonstrate some particular insight. The insight must be interpreted in terms of the target system. That is to say, scientists can use the models as vehicles of the demonstration, but in doing so, part of the representational process as defended in the DDI Account is that scientists interpret the demonstrated insights or results not as features of the *model*, but rather as features which apply to the target system (or at least, the way scientists are thinking of the target system).

In summary, with denotation, we are moving in thought from some target system to a model. We take a model or its parts to stand in or symbolize some target system or object. In demonstration, we use the model as a vehicle to come to certain insights, predictions, or results with regard to the relationship that holds internal to the model. It is in interpretation that we move from the model back to the world, taking the results or insights gained through use of the model to be about the target system or object in the world.

2.2.Inferential

2.2.1. Suárez

After criticizing the substantive accounts in his (2003), Suárez (2004) developed his own account of representation which focused centrally on inference and inferential capacities, what he calls an inferential conception of representation. As he describes it there, this account involves two parts. The first part is what he calls representational force. Representational force is defined as "the capacity of a source to lead a competent and informed user to a consideration of the target" (Suárez 2004, 768). Representational force can exist for a number of reasons. One way to get representational force is to repeatedly use the source as a representation of the target. Another way is in virtue of intended representational uses, that is, in virtue of the intention of the creator or author of some source viewed within the context of a broader scientific community. Oftentimes, the representational force will occur as a combination of the two. It is also a contextual property, insofar as it requires that the agent using the source has the relevant contextual knowledge to be able to go from the source to the (correct/intended) target.

So, for example, in the upper left-hand corner of my word processor is a little blueish square with a smaller white square and a small dark circle inside of it (it is supposed to be an image of a floppy disk). This has representational force insofar as it allows me to go from the source (the image of the floppy disk) to the target (a means of saving the document which I am currently writing). In this case, the representational force exists in virtue of both the intended representational uses (the creators of this word processor surely intend this symbol to stand in for this activity) as well as repeated uses (I am part of a society which has, in the past, repeatedly used an image of a floppy disk to get to this target, not only in this program but in many others as well). It is also contextual: someone who had never used computers would not have the requisite knowledge to be able to use the icon correctly.

This is part of the story for Suárez, but in order to have scientific representation there must be something more than mere representational force. On his view, scientific representations are subject to a sort of objectivity which does not necessarily exist for other representations, e.g. the example above of the save icon. The objectivity is *not* meant to indicate that there is somehow an independent representational relationship that exists in the world when scientists are engaged in scientific representation. Instead, the objectivity is present insofar as representations are constrained in various ways by the relevant features of the targets system which is being represented. That is, because there is some real feature which scientists are intentionally trying to represent in their scientific models and theories, the representation cannot be arbitrary but must respond to these relevant features. So the constraints are themselves objective, but this does not commit Suárez to identifying some reified relationship that holds between sources and targets.

According to Suárez, if we are going to get this objectivity in representations, we must turn to a second feature: the capacity of a source to allow for surrogate reasoning. This second feature requires that informed and competent agents be led to draw specific inferences regarding the target. These inferences can be the result of "any type of reasoning…as long as [the source] is the vehicle of the reasoning that leads an agent to draw inferences regarding [the target]" (Suárez 2004, 773). Suárez's point here is that not only does the source lead the agent to the target, but also it leads the agent to think about the target in a particular way, coming to particular insights and inferences with respect to the source.

More recently, Suárez (2010) has argued that this second feature, the capacity for surrogate reasoning, typically requires that three things be in place. First, the source must have internal structure such that certain relations between parts can be identified and examined. Secondly, when examining the parts of the source, scientists must do so in terms of the target's parts. Finally, there must be a set of norms defined by the scientific practice which define and limit which inferences are "correct" or intended. It is in virtue of these norms of the practice that an agent will be able to draw the relevant and intended inferences, making the representation a part of that particular scientific practice. Of course, he takes his view to be deflationary, so these are not to be understood as necessary and sufficient conditions of the capacity for surrogate reasoning, but rather features which are frequently in place.

Consider an example of a mathematical model, for example the Lotka-Volterra equation. The model is supposed to be representational of predator-prey relationships. Part of this is Suárez's representational force—the fact that competent agents will be lead to consider predator-prey relationships when considering the source. However, as Suárez notes, this is insufficient for scientific representation because in science the terms interact in a non-arbitrary way. To account for this, he argues that there is another feature of the model, which is the capacity to allow for surrogate reasoning. In this case, that means that individuals who examine or manipulate the model in terms of its parts (the multiple variables) will be able to draw certain inferences about the nature of real-world interactions between predators and prey (the parts of the target system). These insights will occur in part due to the nature of the model as well as the norms of scientific practice, which means that the inferences will be non-arbitrarily related to the real-world phenomena and will afford us to recognize certain specified inferences of scientific interest.

2.2.2. Contessa

Suárez's inferential account has been further developed by Gabriele Contessa (Contessa 2007, 2011). He is explicit in his claim that the interpretational view he is defending is not a deflationary account, but is rather a substantive version of the inferential account insofar as he takes the account to give necessary and sufficient conditions of representation. All the same, the account he defends is clearly pragmatic in nature. Contessa begins by noting an important distinction he has drawn from Suárez's work, that of the difference between three types of representation. The first is mere denotation, in which some (arbitrarily) chosen sign is taken to stand for some object. He gives the example of the logo of the London Underground denoting the actual system of trains and tracks.

The second sort of representation is what Contessa calls "epistemic representation" (2007, 52). An epistemic representation is one which allows surrogate reasoning of the sort described by Suárez. The London Underground logo does not have this feature since no one would be able to use it to figure out how to navigate. A map of the London Underground, on the other hand, would have this feature insofar as it could be used by an agent to draw these sorts of inferences.

The final sort of representation is what he calls "faithful epistemic representation" (Contessa 2007, 54–55). Whether or not a representation is faithful is a matter of degree, so something will be a *completely* faithful epistemic representation provided all of the valid inferences which can be drawn about the target using the source as a vehicle will also be sound. Notice this does not require that a model user be able to draw every possible inference about the target, but rather that the inferences licensed by the map that *are* drawn will be sound inferences (both following from the source and true of the target). In this sense, a map of the London Underground produced yesterday will be more faithful than one produced in the 1930s.

Using this framework, Contessa goes on to describe a scientific model as an epistemic representation of features of particular target systems (2007, 56). The scientific model will be representational for a user when she interprets the source in terms of the target. He remains open to there being multiple sorts of interpretation which are relevant, but suggests that the most common sort of interpretation is "analytic," which functions quite similarly to an isomorphism in which every part and relation of the source is interpreted as denoting one and only one part and relation in the target (and all of the target's parts and relations are denoted by some part or relation from the source).

Of course, given that this is determined by the agent's use, it is not necessary that the agent believe that her interpretation is actually the case about the system. Here is where

Contessa draws on the distinction of faithfulness. Since models are often misrepresentations and idealizations as has been discussed above, they need not be completely faithful in order to be useful. This is not the end of the story, though, because the circumstances also play an important role in understanding whether or not something is a scientific representation.

2.3. Agent-Based Versions of Substantive Accounts

In light of some of the insights of Suárez and others, many of the views described above as substantive views were altered and updated to more explicitly and centrally make reference to the role of an agent, making them what could be called agent-centered approaches. Of most importance, given their role in the substantive views as described above are recent advances made by van Fraassen and Giere.

2.3.1. Agent-Based Isomorphism

The view of isomorphism commonly attributed to van Fraassen, which was described above, was the one drawn from his book, *The Scientific Image* (1980). More recently, van Fraassen has presented an altered account of representation, which places much more emphasis on the role of an agent (2008). Van Fraassen notes that while some reference to an agent was a part of his earlier views (Ladyman et al. 2011), Suárez's important work on deflationary accounts was influential in the development of the view he defends (Van Fraassen 2008, 7).

He begins his account by looking primarily to the way in which a representation is used, saying that a source's being representative of some target "depends largely, and sometimes only" on the way in which the source is being used (2008, 23). Though he does not take himself to be offering any substantive theory of representation, he does call this the *Hauptsatz* or primary claim of his account of representation: “There is no representation except in the sense that some things are used, made, or taken, to represent things thus and so” (Van Fraassen 2008, 23). Van Fraassen notices that this places some restrictions on what can possibly be representational. Mental images are limited, because they are not made or used in some way. That is to say, we do not give our mental states representational roles. Similarly, there is no such thing as a representation produced naturally. What it is to be a representation is to be taken or used as a representation, and this is not something that happens spontaneously without the influence of an agent.

Van Fraassen also notices an important distinction in two ways of representing: representation *of* and representation *as*. When scientists take or use some source to be representational, they take it to be a representation of some target. This target can change based on context, and sometimes scientists might not even use the source to be a representation at all. Consider van Fraassen’s example: we can use a graph to represent the growth of bacterial colonies under certain conditions, and so the graph will be a representation of bacterial growth (2008, 27). But we could also use that graph to represent other phenomena, perhaps the acceleration of an object as it is dropped from some height. Part of what this captures is the way in which our perspectives can change the way in which we are representing a particular appearance. Thus, by using a source in some distinct way, we can represent some particular appearance of some particular phenomena.

In intentionally using a source as a representation, scientists do not only make it a representation of something, but they also represent it in a certain light, making certain features salient. This is what van Fraassen calls representation *as*. Two representations can be *of* the same target, but might represent that target *as* something different. Van Fraassen offers an example: everything that has a heart also has a kidney, but representing some organism as having a heart does not mean the same thing as representing it as has having kidneys (2008, 27). Similarly, we might represent the growth of bacteria mentioned above as an example of a certain sort of growth model or as the worsening of some infection as it is seen as part of a disease process.

Of course, all of this is very general, which van Fraassen acknowledges. However, in a true deflationary attitude, he notices that there is no good way of getting more specific about scientific representation since it has "variable polyadicity: for every such specification we add there will be another one" (2008, 29). Nonetheless, he still maintains that the link between a good or useful representation and phenomena requires a similarity in structure. As it stands, then, there is still an appeal to isomorphism present in his account: "A model can (be used to) represent a given phenomenon accurately only if it has a substructure isomorphic to that phenomenon" (2008, 309). Just as before, we have an account of representation which relies on isomorphism between the structure of the theoretical models and the (structure of) the phenomena. All the same, this is still a markedly different view from his earlier view described above. No longer is it the isomorphism or structural relationship alone which is representational. Now, on van Fraassen's views, it is the fact that a scientific community uses it or takes it to be representational.

2.3.2. Agent-Based Similarity

Ian Hacking (1983) has famously argued that, in philosophical discussions of the role and activity of science, too much emphasis is put on representation. Instead, he suggests that much of what is done in science is intervening, and this concept of intervention is key to understanding the reality with which science is engaged. All the same, he still thinks that science can and does represent. Representation, on his account, is a human activity which exhibits itself in a number of different styles. It is people who make representations, and typically, they do so in terms of a likeness, which he takes to be a basic concept. Representation in terms of likeness, he thinks, is essential to being human, and he even speculates that it may have played a role in development like many think language did. In creating a likeness, though, he argues that there is no analyzable relation being made. Instead, "[likeness] creates the terms in a relation…First there is representation, and then there is 'real'" (Hacking 1983, 139). Representation on his view is not interested in being true or false, since the representation precedes the real.

Giere (2004, 2010) has also made pragmatics more central and explicit to his account of scientific representation. He claims that in attempting to understand representation in science we should not begin with some independent two-place relationship, which substantially exists in the world. Instead, we should begin with the activity of representing. If we are going to view this activity as a relationship, it will have more than two places. He proposes a four-place relation: "*S* uses *X* to represent *W* for purposes *P*" (2004, 743). Here, *S* will be some agent broadly construed, such that it could be some individual scientist, or less specifically some group of scientists. *X* is any representational object, including models, graphs, words, photographs, computational models, and theories. *W* is some aspect or feature of the world and *P* are the aims and goals of the representational activity; i.e. the reasons why the scientist is using the source to represent the target. Giere identifies a number of different potential purposes of representation. These include things like learning what something is actually like, but are fairly contextual and depend upon the question being asked. So the way in which something is modeled might change depending on the purposes of the representation (2004, 749–50).

Giere is still working from what should be considered a semantic conception of theories, in which a theory is a set of models which are created according to a set of principles and certain specific conditions. The principles are what we might otherwise think of as being empirical laws, but he does not conceive of them as having empirical truth. Instead, by thinking of them as principles by which scientists can form models, it is these scientists who construct and use the models who make particular the otherwise general and idealized principles. On this view, then, it is the models which are representational and will link up to the empirical world.

There are many ways a scientist can use a model to represent the world, on Giere's view, but the most important way remains similarity. Giere is quick to note that this does not mean that we need to think of the representational relationship as some objective or substantive relationship in the world. Instead, the scientist who uses the model does the representing and she will often do this in virtue of picking out certain salient features of a model which are similar to the target system. In doing so, the scientist specifies the relevant aspects and degrees of similarity which she is using in her act of representation.

One of the advantages of this updated version of the similarity view is the wide range of models which can be effectively representational on this account (Giere 2010). Giere gives an example of a time when he saw a nuclear physicist treat a pencil as a model of a beam of protons, explaining how the beam could be polarized. It is in virtue of the *invoked* similarity between the pencil and a beam of photons, i.e. the fact that the physicist specifically used a relevant similarity, that he was able to use it to represent the beam of photons. By noting the importance of the role of the agent, Giere is better able to explain the scientific representation which occurs in the whole range of scientific representations.

A similar yet importantly distinct account of representation as similarity is defended by Paul Teller (2001). Teller argues that we should abandon what he calls the ‘perfect model’ model, in which we take scientists to model in a way that is perfectly correspondent with the real world targets. Instead, he thinks that models are rarely, if ever, perfect matches for their targets. This does not mean that models are not representations. He argues that models represent their targets in virtue of similarity, though he denies that any general account of similarity can be given. What makes something a similarity depends deeply upon the circumstances at hand including the interests of the model user.

2.4. Gricean

One way to ‘deflate’ the problem of scientific representation is to claim that there is no special problem for scientific representation, and instead argue that we should understand the question of scientific representation as part of the already widely discussed literature on representation in general. This is the project taken up by Craig Callender and Jonathan Cohen (2006). According to their view, representation in many different fields (art, science, language, etc.) can be explained by more fundamental representations, which are common to each of the fields.

To explain this, they appeal to what they call “General Griceanism”, which takes its general framework from the insights of Paul Grice. On their General Gricean view, the representational nature of scientific objects will be explained in terms of something more fundamentally representational. The more fundamentally representational objects in this case are mental states. This, in effect, pushes the hard philosophical problem back a stage, since some account must be given with regard to the representational nature of mental states. They remain uncommitted to any particular account of the representational nature of mental states, leaving that something to be argued about in philosophy of mind. All the same, they mention a few popular candidates: functional role theories, informational theories, and teleological theories.

There are, on their view, significant advantages to taking this General Gricean viewpoint. For one, it has a certain sort of simplicity to it. By explaining all representation in terms of the fundamental representations of mental states, we do not need to give wildly different explanations as to why a scientific model represents its target and why, for example, a green light represents ‘go’ to a driver. Each occurs because, in virtue of what the scientist or “hearer” knows, a certain mental state will be activated which contains with it the relevant representational content.

They can also explain the reasons why similarity or isomorphism will be commonly used (though non-necessary) since these are strong pragmatic tools in helping to better bring about the relevant mental state with its representational content. This is, as they argue, clearly one of the reasons why people from Michigan use an upturned left hand to help them explain the relative location of their hometown--because the upturned left hand is similar in shape to the shape of Michigan. The reasons why similarity is a useful tool here are identical to the reasons why similarity would be useful in scientific contexts, because it will make the relative instance of communication more effective--meaning that the hearer (or user of a model or scientific representation) will be better able to arrive at the relevant mental states which represent the target system.

In short, their view is that while there might be a general philosophical problem of representation, there is not anything special about scientific practice that makes its stake in this problem any different from any other field or the general problem. Of course, as they amusingly note, this passes the buck to the more fundamental question: “Once one has paid the admittedly hefty one-time fee of supplying a metaphysics of representation for mental states, further instances of representation become extremely cheap” (Callender and Cohen 2006, 71).

2.5.Critiques of Deflationary/Pragmatic Accounts

These deflationary and pragmatic accounts of representation have not avoided criticisms of their own. Many of these criticisms are presented as part of the defense of one of the views over another. For example, Contessa (2011) argues against a purely denotational account of scientific representation, such as the one seen in Callendar and Cohen’s (2006). As he says, “Whereas denotation seems to be a necessary condition for epistemic representation, it does not, however, seem to be a sufficient condition” (Contessa 2011, 125). As Contessa argues, it is insufficient merely to be able to stipulate a denotational relationship to have the sort of representation which is useful to scientists. For example, we might use any given equation (e.g. $F=ma$) to denote the relationship which holds between the size of predator and prey populations. But, while this equation could successfully denote this relationship, it will not be of much use to scientists because they will not be able to draw many insights about the predator-prey relationship. Therefore, Contessa argues, while denotation is a necessary condition of representation, it cannot alone be the whole story. In addition, he suggests the need for interpretation in terms of the target, as described above.

Matthias Frisch (Frisch 2015, 296–304) has raised a worry which he addressed specifically to van Fraassen's (2008) account, but which is applicable to many of the pragmatic and deflationary accounts described above. The worry is that if we take van Fraassen's *Hauptsatz* ("There is no representation except in the sense that some things are used, made, or taken, to represent things thus and so" (Van Fraassen 2008, 23)) literally, then it seems to be impossible that some models can represent. Taking Frisch's example, say we wanted to construct a quantum mechanical model of a macroscopic body of water. To do this, "we would have to solve the Schrödinger equation for on the order of 10^{25} variables—something that is simply impossible to do in practice" (Frisch 2015, 297). But if this is so, then it turns out that that the Schrödinger equation cannot be used to represent a macroscopic body of water—since we could never use the equation in this way, it is not representational in this way. Notice that this concern applies to other pragmatic and deflationary accounts: if we are unable to make inferences or interpret the source in terms of the target (which, given the complexity here, it seems we would not be able to do), then it will also fail to be representational on these other accounts. But this leads to a fairly strong conclusion that we can only use a model to represent a system once we have actually applied the model to that system. For example, the Lotka-Volterra model seems to only represent those systems for which scientists have used it; it does not represent all predator-prey relationships, in general.

Frisch does not think that this argument is ultimately fatal to the pragmatic accounts since he argues that because there are constraints on the use of models which are part of the scientific practice, there is a sense in which the Lotka-Volterra model, for example, represents all predator-prey relationships (even though it has not yet been used in this way)

(Frisch 2015, 301–4). There is no problem in extending models "horizontally," i.e. to other instances which are in the same domain of validity of the model. There is, Frisch argues, a problem in extending models "vertically," i.e. using a model to represent some phenomena which is outside the domain of validity. This can be seen in the quantum mechanics example from above since we do not have any practice in place to use Schrödinger equations to describe macroscopic bodies of water. So, he claims, van Fraassen's view (and, by extension, the other pragmatic and deflationary views) must be committed to an anti-foundationalism (a view that the sciences cannot be reduced to one foundational theory) that denies that the models of quantum mechanics can adequately represent macroscopic phenomena. Of course, the anti-foundationalist commitments might be viewed as a desirable feature of these views, rather than a flaw, depending upon other commitments.

Another important critique which applies more generically to a number of these deflationary and pragmatic views comes from Chakravartty (2010). As described above, many of those who argue for a deflationary or pragmatic account of representation offer their view as an alternative to the substantive accounts. That is to say, they deny that scientific representation is adequately described by the substantive accounts and do not merely add to these accounts, but rather reject them and offer their deflationary or pragmatic account instead. Chakravartty argues that this is a mistaken move. We should not think of deflationary or pragmatic accounts as alternatives to the substantive accounts, but rather as compliments. On the deflationary or pragmatic accounts, representation occurs when inferences can be made about the target in virtue of the source. But, "how, one might wonder, could such practices be facilitated successfully, were it not for some sort of similarity between the representation and the thing it represents—is it a miracle?" (Chakravartty 2010, 201). That is to say, the very function which proponents of the deflationary or pragmatic accounts take to be the central explainer of scientific representation seems to require some sort of similarity or isomorphism (Bueno and French 2011). On Chakravartty's view, the pragmatic or deflationary accounts go too far in eliminating the role for some substantive feature. In doing so, they leave an important part of scientific representation behind.

3. Model-Based Representation

The question of scientific representation has received important attention in the context of scientific modeling. There is a vast literature on models, and much of it is at least tangentially related to the questions of representation. An examination of this literature provides an opportunity to see other sorts of insights with regard to representation and the relationship between the world and representational objects.

3.1. Models as Representations (and More)

Much of the literature on models focuses on the various roles of models within scientific practice, both representational and others. In an influential volume on models, Margaret Morrison and Mary Morgan (Morgan and Morrison 1999b) use a number of examples of models to defend the view that models are partially independent from theories and data and function as instruments of scientific investigation. We can learn from models due to their representational features. Morrison and Morgan start out by focusing on the construction of models. Models, on their account, are constructed by combining and mixing a range of disparate elements. Some of the elements will be theoretical and some will be empirical, that is, from the data or phenomena. Thus far, this view is mostly in line with what has been discussed in the above sections. What makes models unique in their construction is that they often involve other outside elements. These can be stories (ways of explaining some unexpected data which are not part of a theory), other times it is a sort of structure which is imposed onto the data. These other elements, they argue, give models a sort of partial independence or autonomy. This is true even when the outside elements are not as obviously present, for example when a model is an idealized, simplified, or approximated version of a theory. This independence is crucial if we are to use them to help understand both theories and data as we often use them to do.

According to Morrison and Morgan, models function like tools or instruments for a number of purposes. There are three main classifications of the uses of models. The first is in interacting with theories: models can be used to explore a theory or to make usable a theory which is otherwise unusable. They can also be used to help understand and explore areas for which we do not yet have a theory. Other times, the models are themselves the objects of experimentation. The second classification of the use of models is in measurement: not only as a way of structuring and presenting measurements, but they can also function directly as instruments of measurement. Finally, models are useful when designing and creating technology.

Models are not valuable only insofar as they have these functions. Models, Morrison and Morgan argue, are also importantly representational. Their representational value relies in part on the way in which they are constructed with both the theory and the data or phenomena. Models can represent theories, data, or can be representational instruments which mediate between data and theory. Whatever the case, representation, on their view, is not taken to be some mirroring or direct correspondence between the model and its representational target. Instead, “a representation is seen as a kind of rendering--a partial representation that either abstracts from, or translates into another form, the real nature of the system or theory, or one that is capable of embodying on a portion of a system” (Morgan and Morrison 1999b, 27). Sometimes models can be used to represent non-existent or otherwise inaccessible theories, as they claim is the case with simulations.

The final role of models described by Morrison and Morgan is the way that models afford the possibility of learning. Sometimes the learning comes in the construction of the model. Most frequently, though, we can learn using models by using and manipulating them. In doing so, we can learn because the models have the other features already described: the wide range of sources for construction, the functions, and their status as representations. Oftentimes, the learning takes place internal to the model. In these cases, the model serves as what they call a representative rather than a representation. With representatives, the insights we can gain from manipulating the model are all about the model itself. But in doing so, we come to a place from which we can better understand other systems, both real-world systems and other systems. Other times, we take the world into the model and then manipulate the world inside the model, as a sort of experiment.

Daniela Bailer-Jones (Bailer-Jones 2003, 2009) defends a slightly different but related account of the representational nature of models. On her account, models entail certain propositions about the target of the model. As propositions, they are subject to being true or false. One way of thinking about the representation of models is to say that models are representational insofar as their entailed propositions are true. However, this cannot be exactly right, since, as was mentioned above, models oftentimes intentionally entail false propositions. Since models are about those aspects of a phenomenon which are selected, they will fail to say things about other aspects of a phenomenon. In some cases, the propositions entailed may be true for one aspect but false for another. This calls for the role of model users who decide what function the model has, the ways in which and degree to which the model can be inaccurate, and which aspects of the phenomenon are actually representing. In sum, on her view, models are representational in part due to their entailed propositions, but also due to the role of the model users.

Tarja Knuuttila (2005, 2011) has argued that in thinking about models, too much emphasis has been placed on their representational features – even in accounting for their epistemic value. Following and expanding on Morrison and Morgan (Morgan and Morrison 1999b), she argues that we should think of models as being material epistemic artefacts, i.e. "intentionally constructed things that are materialized in some medium and used in our epistemic endeavors in a multitude of ways" (Knuuttila 2005, 1266). The key to their epistemic functioning is to be found from their constrained and experimental nature. Models, according to this account, are constrained by their construction in such a way that they make certain scientific problems more accessible, and amenable to a systematic treatment. This is one of the main roles of idealizations, simplifications, and approximations. On the other hand, the representational means used also impose their own constraints on modeling. The representational modes and media through which models are constructed (e.g. diagrams, pictures, scale models, symbols, language) all *afford* and *limit* scientific reasoning in their different ways. When considered in this respect, Knuuttila argues, we can see that models have far more than mere representational capacities including that they are themselves the targets of experimentation and can be thought of as creating a sort of conceptual parallel reality.

3.2. Model-Building

In addressing model-building Weisberg (2007b) and Godfrey-Smith (2006) both take up the idea that the characteristic way in which models are constructed is indirect. This comes about in a three step process in which a scientist first constructs a model, then analyzes and refines the model, and finally examines the relationship between the model and the world (Weisberg 2007b, 209). Models are used and understood by scientists with "construals" of the model (Godfrey-Smith 2006). The construal, on Weisberg's account, is made of four parts. The first is an assignment which identifies various parts of the model to the phenomena being investigated. The second part of a construal is the scope, which tells us which aspects of the phenomena are being modeled. The final two parts of the construal are each fidelity criteria. One of these is the dynamical fidelity criteria, which identifies a sort of error tolerance of the predictions of the model. The other is the representational fidelity criteria, which give standards for understanding whether the model gives the right predictions for the right reasons, i.e. whether or not the model is linking up to the causal structure which explains the aspects of the phenomenon being modeled.

This strategy of model-based science is contrasted with a different sort of strategy, what Weisberg calls abstract direct representation. Abstract direct representation is the strategy of science in which study of the world is unmediated by models. He gives the example of Mendelev's development of the periodic table of elements. This process did not begin with a hypothetical abstract model which is refined and then used representationally (as Weisberg thinks the process of model-based science proceeds). Instead, this process starts with the phenomena and abstracts away to more general features.

Such distinction between modelling and abstract direct representation underlines the possibility that not all scientific representations need to achieved in same ways.

There are some worries about Weisberg's understanding of the process of model-making Through a close examination of the development of the Lotka-Volterra model, Knuuttila and Loettgers argue that the process of model-building often begins with certain sorts of templates, or characteristic ways of modeling some phenomena, typically adopted from other fields. Such already familiar modeling methods and forms offer the modeler a sort of scaffolding upon which they can imagine and describe the target system. They also argue that another distinct feature of model-making is its outcome-orientation. That is, in developing a model, a scientist will typically do so with an eye to the anticipated insights or features of the target system that they wish to represent. Thus, on their view, the modeler pays close attention to the target system or empirical questions in all stages of the development of the model (not just at the end, as Weisberg suggests).

3.3. Idealization

One of the important discussions that has developed primarily in the literature on models concerns idealization. Weisberg argues that there are three different kinds of idealization, which he generically describes as "the intentional introduction of distortion into scientific theories" (2007a, 639). The first kind of idealization he calls Galilean idealization. This is the sort of idealization in which a theory or model is intentionally distorted so as to make the theory or model simpler, in order to render it computationally tractable. This sort of idealization occurs when scientists ignore certain features of a system or theory, not because they are playing no role in what actually happens, but rather because including them makes the application of the theory or model so complex that they cannot gain traction on the problem. By removing these complexities, scientists distort their model (because it lacks complexities which reflect the target). But after gaining some initial computational tractability, they can slowly reintroduce the complexities and thus remove the distortions.

The second type of idealization is what Weisberg calls minimalist idealization. In a minimalist idealization, the only features that are carried into the model or theory are those causal features which make a difference to the outcomes. So, if some feature of a target can be left behind without losing predictive power, a minimalist idealization will leave that feature behind. As an example, Weisberg notes that when explaining Boyle's law, it is often assumed that there are no collisions between gas molecules. This is, in fact, false since collisions between gas molecules are known to take place in low-pressure gasses. But, "low pressure gases behave as if there were no collisions" (2007a, 643). So, since these collisions do not make any difference to our understanding of this system, scientists can (and do) leave this fact behind.

Notice that this is distinct from Galilean idealization insofar as minimalist idealizations leave certain features out of their theories or models because they make no difference to the relevant tasks or goals at hand. Galilean idealization, on the other hand, leaves certain features out even when they do make a difference, simply because leaving them in would make the model more complex and less tractable.

The final sort of idealization described by Weisberg is what he calls multiple-models idealization. This is the practice of using a number of different, often incompatible models to represent or understand some phenomenon. In this case, none of the models by itself is capable of accurately modeling the relevant target system. All the same, each of the models is good at representing certain features of the target system. Thus, by using not just a single model but rather this group of models, each of which is distorted, scientists can get a better sense of the target system. Weisberg offers a helpful example: the National Weather Service uses a number of different models in making its weather forecasts. Each of the models used represents the target in a different way, each being inaccurate in some way or another. It is the use of all of these models that permits forecasts of higher accuracy since attempts to make a single model have resulted in less accurate predictions.

4. Sociology of Science

4.1. Representation and Scientific Practice

Many important insights on the nature of scientific representation have come not from the philosophy of science but rather from thinkers who would typically be considered part of the field of sociology of science. The insights from this field can serve as both a source of insight on the nature of representation in scientific practice, as well as a challenge to the primarily epistemically-oriented insights from the philosophy of science. Michael Lynch and Steve Woolgar (1990) edited an important collection of papers on scientific representation in practice written from the perspective of sociology of science. More recently, Lynch and Woolgar edited another collection with Catelijne Coopmans and Janet Vertesi (Coopmans et al. 2014). Treating representation from the perspective of sociology of science involves asking a different sort of question than the one so far addressed in this article. Instead of asking about the constitution of scientific representation, sociologists of science are more interested in a different question, "What do the participants, *in this case*, treat as representation?" (Lynch and Woolgar 1990, 11). In the introduction to this volume, Lynch and Woolgar provide a general overview of some of the important insights from this perspective.

Since sociology of science treats scientific practice as its object of inquiry, it is keen to describe precisely how representations are actually used by scientists. They note the importance of "the heterogeneity of representational order" (Lynch and Woolgar 1990, 2). That is, there is a wide range of devices which are representational as well as a wide range of ways in which the representations are used and in which they are useful. Importantly, sociologists are often interested in discussing more than merely the epistemic or informational role and use of representations, viewing them as significantly social, contextualized, and otherwise embedded in a complex set of activities and practices. Sociologists of science attempt to pay attention to the whole gamut of representations and representational uses to better understand precisely the role they play within scientific investigation.

Another important insight, Lynch and Woolgar note, is that the relation between representations is not to be thought of as directional in the sense that the representations move from or towards some "originary reality" (Lynch and Woolgar 1990, 8). Instead, any directionality of representations is to be thought of as "movement of an assembly line" (Lynch and Woolgar 1990, 8). That is to say, representational practice must be seen as constructing not only a representation, but also (re)constructing a phenomenon in a way so that it can be represented. This is something that can be seen in much of the literature from sociologists of science, including the work of Latour as I describe in greater detail below.

In paying close attention to the way representations are actually used, some sociologists of science note settings in which "discrepancies between representations of practice and the practices using (and composing) such representations" (Lynch and Woolgar 1990, 9). These discrepancies and other problems encountered in the actual practice of science allow for improvisation and creativity which can help advance the particular domains of which they are a part. Sociologists of science are interested in studying this creativity, not only for its productivity in science, but also as an interesting phenomenon in its own right.

4.2. Circulating Reference

A particularly telling example of these insights, especially from the philosophical point of view, is provided in Bruno Latour's "Circulating Reference" (Latour 1999). Latour's photo-philosophical case study is based on the work of a group of scientists who were examining the relationship between a savannah and forest ecosystem. At the end of their project, they collectively published a paper on their findings which included a figure of the interaction between the ecosystems, detailing the change in soil composition among other features. Latour asks how it is that this abstract drawing, which takes a perspective no individual could possibly have had and which ignores so many of the features of the ecosystem, can be about that stretch of land. That is to say, here we have a drawing, something made by ink and paper, and there we have the forest-savannah ecosystem, how is it that the former can be about the latter?

Latour's method of answer, which takes the form of a strikingly well-written case study in which he presents pictures from the expedition which he uses to structure and represent the process he describes, is to look carefully at all the details and steps by which the scientists got from the expedition to the figure in the paper. What happens, says Latour, is that there are a series of steps through which the scientists abstract from the world in some intentional fashion. In doing so, they maintain some relevant feature of the world, but they are simultaneously constructing the phenomena they are studying. In the process the representations produced are also getting more abstract.

An example will make this clearer. At one stage in the process, soil samples are collected from a vertical stretch of ground. These samples are transferred into a device which allows the whole vertical stretch of earth to be viewed synoptically. In taking the sample, the scientist has already begun to construct--already this particular bit of dirt is taken to be representative of the dirt for a much wider area of land. Once the soil has been collected, various features of the soil are maintained through intentional actions on the part of the scientist. For example, the scientists will label the soil as being of a certain sort of consistency. The scientists then use a clever device in which there are pinholes in a tool which has the various Munsell colors and numbers, which is itself a construction with a long history. In looking through the pinholes, the scientist can abstract away from the dirt sample itself, taking, in some sense, only the color (which is done in virtue of a construction of numbers associated with particular colors). Something has clearly been lost, namely, the full materiality of the dirt. But something has also been gained, in this case, a number which corresponds to the color of the dirt; some usable, manipulatable data.

Latour's essay carefully describes many of these transitions from the savannah-forest system to the published figure. As he claims, it is this series of transitions (which each involve abstraction and construction due to the intentional decisions of a scientist) which ensures that the figure at the end references or represents the savannah-forest system. There is not a single gap between the figure and the world which must be accounted for by some representational relation. Instead, on his account, there is a large series of gaps, each of which is crossed by a scientist's actions in abstracting and maintaining, constructing and discovering. This series, he thinks, can be extended infinitely in either direction. By abstracting further from the already quite-abstract figure, certain hypotheses might be suggested, which would result in a return to the savannah-forest system, to gather data which might be more basic than the data already gathered. On his view, there is no such thing as "the world" which is the most basic thing-in-itself; nor is there any most-abstracted element.

4.3. Critiques of Sociology of Science

While these insights from the sociology of science literature have been both sources of support and criticism for the philosophical literature, they have also been subject to criticisms. One important criticism comes from Giere's (1994) review of Lynch and Woolgar's (1990) *Representation in Scientific Practice.* Giere's primary target is the extremely constructivist nature of the sociology of science literature. The constructivist approach claims that science is socially constructed: i.e., science is filled with socially-dependent knowledge and aimed at understanding socially-constructed objects. There is no such thing, on this view, of a non-constructed world to be understood by scientists, and therefore, no such world to be represented. The attempt to explain representation in this framework results in a "no representation theory of representation" (Giere 1994, 115). But, Giere thinks there is a straightforward counter-slogan to a view of this sort: "no representation without representation" (1994, 115). That is to say that if there is nothing 'out there' in the world being represented, it cannot be that this is an instance of representation. This is not to reject the importance of paying attention to the role of the practices and representational devices in particular case studies. All the same, Giere argues that if we want a general account of scientific representation, "we must also go beyond the historical cases" (1994, 119). Put otherwise, the sociology of science perspective is an important part of explaining scientific representation, but this work by itself leaves representation unexplained.

Knuuttila (2014) takes up a similar line of criticism. While she places great importance on the insights of sociologists of science, she thinks that many of their views have developed with a false target in mind. Many sociologists of science place their views as a contrast to a traditional philosophical view of science as something which perfectly represents the world. The alternative, they suggest, is their constructivist approach, as described above. However, Knuuttila argues, this motivation runs into a few problems. First, when they select certain practices to investigate rather than others, by what criterion are they distinguishing this practice as representational? In doing so, they seem to be relying on some traditional account of representation to delineate the cases of interest. Further, it seems that these studies do not show that representation is a defunct concept and that we are bound to a purely constructivist account of science. Instead, "these cases actually reveal…what a complicated phenomenon scientific representation is…and give us clues as to how, through the laborious art of representing, scientists are seeking and gaining new knowledge" (Knuuttila 2014, 304). We need not think that just because there is not *perfect* representation of the world, that there is therefore no world to be represented. Their insights could equally contribute to an intermediate view in which we reject this perfect-representation view of science, but still maintain that science is giving us knowledge of the real world. That is, we can simultaneously deny that representations "are some kind of transparent imprints of reality with a single determinable relationship to their targets" while still affirming that the "artificial features of scientific representations…result from well-motivated epistemic strategies that in fact enable scientists to know more about their objects" (Knuuttila 2014, 304).

CHAPTER 3

THERE IS A SPECIAL PROBLEM OF SCIENTIFIC REPRESENTATION[4]

1. Introduction

According to many philosophers of science, representation in scientific practice is different from representation in other disciplines, like art and language. This claim is denied by Craig Callender and Jonathan Cohen (2006), who argue that representation is the same across disciplines. In this paper, I will argue that their view leaves the communal nature of scientific representation unexplained. To explain how scientific representation is dependent upon practice, I will introduce the concept of licensing, in which the targets of representational vehicles are determined through various activities performed by scientists in accord with broader scientific practice. I will argue that licensure is a constitutive feature of representation in science, indicating that there *is* a special problem of scientific representation.

2. Callender and Cohen's View

On Callender and Cohen's evaluation, much of the literature on scientific representation has been "concerned with non-issues" (2006, 27).[5] Specifically, they think there is no reason for philosophers of science to give a special account of the "constitution

[4] Boesch, B. (2017) There is a special problem of scientific representation. *Philosophy of Science*, 84(December 2017): 970-981. DOI: 10.1086/693989. Reprinted here with permission of publisher.
[5] Romina Zuppone (2014) argues in favor of Callender and Cohen's evaluation, suggesting that there are normative but not substantive or constitutive differences in representation between art and science.

question:" "What constitutes the representational relation between a model and the world?" (Callender and Cohen 2006, 68). In response to this question, they make a few observations. One is that it is "economical and natural to explain some types of representation in terms of other, more basic types of representation" (Callender and Cohen 2006, 70). They also identify a general desire to have a consistent account of how "entities other than models—language, pictures, mental states, and so on—…represent the very same targets that models represent" (Callender and Cohen 2006, 71). For these reasons, they suggest that "scientific representation is just one more special case of derivative representation" (Callender and Cohen 2006, 75). That is to say that the representational nature of scientific vehicles is explained in the same way that the representational nature of linguistic entities, artwork, etc. is explained. In each case, and in every practice, the representational nature in question will be reduced to a more fundamental representational entity. So, e.g., the representational nature of a word, a painting, and a scientific model will each be explained in terms of the representational nature of mental states.

On Callender and Cohen's view, representation is purely stipulative: "virtually anything can be stipulated to be a representational vehicle for the representation of virtually anything…" (2006, 74). Of course, it is not the case that *any* stipulated representation will actually be useful for scientific aims. Thus, they identify pragmatic constraints which delimit scientific representation. However, they make it quite clear that these constraints are delimiting *already-existing* representations. As such, the pragmatic constraints are not a part of an account of the constitution of representation itself: "the questions about the utility of these representational vehicles are questions about the pragmatics of things that are representational vehicles, not questions about their representational status per se" (Callender and Cohen 2006, 75).

If Callender and Cohen are correct, then we are left rethinking a rather extensive literature on scientific representation which typically begins with the assumption that there *is* something special about representation in science.[6] As one example among many, Mauricio Suárez (2004) defends an inferential conception of scientific representation. His account takes careful notice of the aims of scientific practice, noting that mere stipulation (what he calls "representational force") is insufficient for representation in science. To be a *scientific* representation, a vehicle must also permit surrogate reasoning which "allows competent and informed agents to draw specific inferences regarding [a target]" (Suárez 2004, 773). If we accept Callender and Cohen's view, then Suárez's account and the many others like it do nothing more than identify some of the typical pragmatic strategies employed in delimiting representations for scientific uses (Callender and Cohen 2006, 78).

3. Private Reminiscence and Communal Representation

In order to show that the extensive literature on scientific representation has not been addressing a non-issue, I will need to show that there is a special problem of scientific representation, a feature unexplained by Callender and Cohen's account. I submit that the relevant feature in need of special explanation is the communal nature of scientific representation, that it inherently involves reference to the practice. To see why Callender and Cohen's view is unable to account for the communal nature of scientific representation,

[6] For more accounts which answer the constitution question in a distinctive way, see the work of, e.g., Ronald Giere (1988, 2004, 2010), Bas van Fraassen (1980, 2008), R.I.G. Hughes (1997), Steven French, James Ladyman, and Otávio Bueno (Bueno 1997; French and Ladyman 1999; French 2003; Bueno and French 2011), and Gabriele Contessa (Contessa 2007, 2011). For an overview of these accounts of scientific representation among others, see Brandon Boesch (2015), Mauricio Suárez (Suárez 2015b), and Roman Frigg and James Nguyen (2016).

consider what I call ‘reminiscence’, a representational relationship which lacks the same communal feature. It is defined schematically as the following:[7]

> Some X is reminiscent of some Y for some agent A provided that when A thinks about or experiences X, she thinks about or experiences Y and attributes some connection between X and Y.

So, for example, a drawing can be reminiscent of my nephew, the smell of jasmine can be reminiscent of golfing, and so on.

There are three noteworthy features of reminiscence. First, the representational nature of reminiscence can be reduced to the representational nature of more fundamental entities. For example, I can explain the drawing’s reminiscence of my nephew in virtue of the mental state produced by the drawing (which is about my nephew, who created it). Second, stipulation is sufficient to create an instance of reminiscence. For example, I could draw a symbol on my hand which I create for the sake of reminding me to call my nephew. The reminiscent relationship between the symbol and my nephew exists because of my stipulative act. Finally, any limitations of reminiscent relationships will be made for pragmatic reasons. For example, it would be for pragmatic reasons that I make the symbol on my hand look like the ball from his favorite sport since it will more easily remind me of him.

These three features of reminiscence are noteworthy because they are shared by Callender and Cohen’s view of scientific representation. In fact, from Callender and Cohen’s perspective, the only major difference between the two concepts would be the particular aims for which each relationship is utilized. While important, these different

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[7] I should note that the account of reminiscence here is not meant as a detailed explanation of this concept, but only as an analogy to draw a point about representation.

aims alone are insufficient to explain a key dissimilarity between scientific representation and reminiscence: while reminiscence can be private, scientific representation is necessarily communal. That reminiscence can be private can be seen from the fact that discussions of reminiscence can terminate in disagreement. For example, no one is ultimately 'correct' about whether or not a drawing is reminiscent of my nephew. This is because reminiscence is agent-relative and so depends only upon some particular agent and her mental states.

Scientific representation relies on much more. As Suárez has argued, "representation is not at all 'in the mind' of any particular agent. It is rather 'in the world', and more particularly in the social world – as a prominent activity or set of activities carried out by those communities of inquirers involved in the practice of scientific modelling" (2010, 99). Scientific representation is not isolated from the practice in which it is embedded. It is necessarily communal.[8] The communal nature is demonstrated from the fact that representational vehicles demonstrate autonomy from individual scientists and their mental states.[9] For example, a scientist's rogue stipulation that the Lotka-Volterra model (which represents predator-prey relations) represents population change due to genetic drift does not count as an instance of scientific representation. This is not only because it does not (pragmatically) allow for meaningful insights, but also because it ignores and discounts the autonomous elements of the model as understood and developed

[8] The view of representation argued for in this paper echoes many of the points made by Ludwig Wittgenstein's in his 'Private Language Argument' where he argues that meaning is necessarily communal (Wittgenstein 2009, 95[e]-111[e]).

[9] A similar point about the autonomy of models (from both theory and the world) has been made by Morrison and Morgan (Morgan and Morrison 1999b). Here, I am extending a related point to other representational vehicles, including things like diagrams and figures, arguing that they are autonomous from individual scientists' mental states.

by the broader scientific community.[10] The autonomous elements are seen in the materiality or historicity of the representational vehicle; in its development, reception, and contemporary use. Understanding how and why the scientific object represents its target requires paying attention to these communal features.[11] That is to say that the communal features are partially *constitutive* of the representational relationship. Callender and Cohen's account of scientific representation does not sufficiently account for these constitutive communal elements, as will be shown more explicitly below.

4. Licensing

Explaining the communal nature of scientific representation requires that attention be given to the material, autonomous dimensions of the representational vehicle in terms of its development, reception, and use. All of these features partially establish a scientific representation, through an activity I call *licensing*. Licensing is the set of activities of scientific practice by which scientists establish the representational relationship between a vehicle and its target. It is through licensing that scientists and the broader scientific community establish what Suárez calls the "intended representational uses" of a vehicle (2004, 768). Licensing is itself a constitutive element of the representational relationship: it plays a critical role in explaining how and why some vehicle represents its target. Seeing the sorts of activities involved in licensing and how they partially constitute the representational relationship will require that we pay close attention to the historical development, reception, and use of actual instances of scientific representations.

[10] Of course, there may be disagreements and developments internal to the practice about how to use some representation, but these disagreements and developments are *part of the practice*.
[11] According to Bruno Latour, the peculiarities of scientific image-making (which include the communal features I describe below) "offer[] an excellent way to define what is 'scientific,' after all, in science" (2014, 347).

4.1 Licensing in Artistic Representation

A similar sort of licensing is present in representation in art, and so an initial pass on the concept as it applies to artistic practice will be of use in drawing an analogy to licensing in science.[12] To see the role of licensing in artistic representation, consider an example. The mere stipulation that Pablo Picasso's *Guernica* should represent the pain of cyberbullying is clearly insufficient to make it represent this target. Understanding how *Guernica* is representational involves an awareness of communal features: Picasso's intentions within the environment in which he created the painting, how the painting was received by viewers in the years following its creation, and how it is understood today. With these features in mind, it is clear that *Guernica* represents the pain and suffering of the people of Guernica who had been bombed by axis forces at the request of Francisco Franco and the Spanish Nationalists. The licensing here is a constitutive element of *Guernica*'s representational nature: without these features, it is not clear whether or how the painting would manage to represent anything at all.

Licensing also occurs outside of the scope of authorial intent, when the artistic community comes to accept that a piece of art is representational in a way that was not intended by the author. A good example can be taken from an anecdote related by the author Flannery O'Connor: [A] student asked me…: "Miss O'Connor, what is the significance of the Misfit's hat?" Of course, I had no idea the Misfit's hat was significant, but finally I managed to say, "Its significance is to cover his head" (1988, 853). The Misfit is a key character in O'Connor's famous short story, "A Good Man is Hard to Find," and,

[12] It is somewhat contentious to draw conclusions about the nature of representation in science by appeal to art; see e.g. Bueno and French (2011). Nonetheless, it is a common technique in discussions of scientific representation; see e.g. Suárez (2004).

as such, it would not be surprising for his wardrobe to be importantly representational. Her answer indicates that while she did not intend any representational target for the hat, there may yet be one. If the hat is representational, it will not be due to her authorial intent, but rather due to the views of the broader artistic community.

Let me make it very clear that the licensure so far described is not already accounted for by elements of Callender and Cohen's account. First, notice that none of these means of licensing is a mere pragmatic limitation of already existing representations. It is not as if *Guernica* represents anything and everything, but is then *limited* by the contexts of Picasso, audiences, and art historians. These contexts are a crucial part of understanding why it represents at all. Nor is the licensing mere stipulation. O'Connor leaves it open that there may be a representational target for the Misfit's hat, even though she did not stipulate one. A single reader's stipulation alone is insufficient to make it a representation, since the target must also fit well with the Misfit's characteristics, with O'Connor's general themes as understood by literary critics and audiences alike, and so on. Once again, these contexts are a critical part of establishing the representational nature of the hat.

4.2 Licensing in Scientific Representation: A Case Study

The unique aims of science indicate that the licensing of scientific representation is of a different kind than the licensing in art. All the same, licensing similarly plays a critical role in establishing scientific representation. According to Tarja Knuuttila, case studies of scientific representation have revealed that it is "a complicated phenomenon" and "a laborious art" (2014, 304). Understanding the nature of licensing and its role in the complexities of scientific representation will be best accomplished by examining the complicated features seen in the context of a case study. Examples could be made of any type of representational vehicle, like the masterful case study of a scientific figure made by Bruno Latour (1999). I will take as my example the Lotka-Volterra model, since its development exhibits interesting features, many of which have already been widely discussed by other philosophers (e.g. Knuuttila and Loettgers 2012, 2016).

As mentioned above, the Lotka-Volterra model is used by ecologists to represent predator-prey relations. It had its beginnings in the independent work of two different scientists, Vito Volterra and Alfred Lotka. In understanding the representational nature of this model, it is important to pay attention to its licensing through its historical development. This attention includes noticing things like the way that the construction of the model by Lotka, Volterra, and others has been responsive to certain theoretical and empirical aims. These historical and practice-centered features of the model's development reveal the partial autonomy of its representational nature. These features make up the licensing which is itself partially constitutive of the representational nature of the model since understanding how and why the model represents its targets requires attending to these features. Let us now turn to examine these features in more detail.

Consider first the development of the model by Volterra, who was "motivated by the goal of reproducing the kind of oscillating behavior that was observed empirically in fishery statistics" (Knuuttila and Loettgers 2016, 19). His aim to address a theoretical question with an empirically useful model is central not only to understanding how the model historically came about, but in understanding how it represents its targets. Consider how Volterra described his project and the aims which permeate his description: "Let us seek to express in words the way the phenomenon proceeds roughly: afterwards let us translate these words into mathematical language. This leads to the formulation of differential equations. If then we allow ourselves to be guided by the methods of analysis we are led much farther than the language and ordinary reasoning would be able to carry us and can formulate precise mathematical laws. These do not contradict the results of observation. Rather the most important of these seems in perfect accord with the statistical results" (Volterra 1928, 5).

Volterra's actual process of moving from words, to equation, to application of results (for both theoretical and empirical purposes) first involved creating an equation to account for the population change of a single species. He then added additional species and modelled interactions under different conditions, including, notably, contending for the same food and the predation of one species upon the other. Using these models, he demonstrated "three fundamental laws of the fluctuations of the two species living together" (1928, 20). He then applied these theoretical laws of predator-prey relations to the empirical case which had prompted his analysis, the peculiar rise in predator populations during the decrease of fishing of prey populations in the Adriatic Sea during World War I (1928, 21).

Why does Volterra's model represent these theoretical features of predator-prey relations? Why does it represent the populations of fish in the Adriatic during World War I? It represents these targets because, through a series of steps of analysis, revision, and development, each of which was responsive to certain theoretical and empirical aims understood and described in his account, Volterra *established* this representational nature. Indeed, as explained by Knuuttila and Loettgers (2016), the historical development of this model has a much more extended history than the one Volterra described in the two papers in which he first introduced it (1926, 1928). The model is a representation of its target not by mere stipulation and pragmatic constraint, but through careful and attentive construction of equations which ensure that the model functions in the wider theoretical contexts and can explain the relevant empirical aims. In short, the model represents its targets because Volterra so *licensed* it by building into the model these external, autonomous representational features. Without these features, how or what would it represent?

Consider another instance of licensing in the development of the Lotka-Volterra model, this time by Lotka. His development proceeded with a different aim than Volterra: "instead of starting from the different simple cases and generalizing from them, he developed a highly abstract and general model template that could be applied in modelling various kinds of systems" (Knuuttila and Loettgers 2016, 13). He began by creating a very general equation which described "evolution as a process of redistribution of matter among the several components…of the system" (Knuuttila and Loettgers 2016, 15). In two papers (1920a, 1920b), Lotka applied this general equation to particular cases in biology and chemistry, in each case coming to theoretical conclusions about the systems in question. For example, in applying the equation to a predator-prey system, he concluded that there would be "undamped oscillation continuing indefinitely" among the two populations (Lotka 1920a, 414). Lotka did not specifically apply the results to any empirical data, but instead used his results to come to theoretical conclusions about these relationships which he then connected to theoretical ecological principles drawn from Herbert Spencer's *First Principles* (1920a, 414).

Why does Lotka's model represent its theoretical target? What constitutes this representational relationship? Any attempt to explain the representational relationship must reference the way in which Lotka derived his general equation and the way in which he applies it to the specific cases. That is to say, the representational nature of the model is *constructed* through the scientific activities performed by Lotka during the development of the model. Lotka does not merely stipulate that his model targets predator-prey relationships. Instead, he builds this ability into the model during the development of the general equation and further constructs this ability in his application of the question to specific targets. In so doing, he partially constructs the representational nature of the model—he licenses it as a representation through activities in accord with the broader practice.

The Lotka-Volterra model's history since its initial development is long and complex. As such, the licensing of the model goes beyond the initial work of Lotka and Volterra—similar to how the licensing of O'Connor's story goes beyond her initial work. As described by Alan Berryman (1992), one development was a shift in the 1940s to the use of a logistic formulation which allowed for attention to be placed on predator-prey ratios rather than products. Another development, which occurred around the same time, was the use of a predator functional response which introduced a nonlinear rate of death for the prey. Each of these developments license new representational targets by expanding and altering the model to make it responsive to different theoretical or empirical aims, by removing idealizations, or otherwise by allowing for different theoretical conclusions. Many other variations of the Lotka-Volterra model exist, licensed by similar developments. As just one example, Richard Goodwin (1967) modified the model to apply to questions in economics. Additionally, the original formulation of the model is still used in introductory textbooks on ecology (see, e.g., Cain, Bowman, and Hacker 2008). The representational nature of the model in each of these cases is partially established by these features of the model which stand independent of any mental states of scientists and students alike. Scientists do not merely start using a model however they would like, without recourse to the history of the use of the model. There are autonomous elements of the model which are carried with it when it changes contexts in virtue of how it was originally developed and how the broader scientific practice has come to use and understand it over time (Knuuttila and Loettgers 2014). In short, the constitution of the representational nature of the Lotka-Volterra model relies deeply upon these historical features of licensing as understood by the broader scientific community.

Let me briefly underscore the importance of these activities of licensing to the representational nature of the Lotka-Volterra model by imagining a scenario in which these features are absent. Suppose that Volterra and Lotka had proceeded differently. Suppose that they began, for no particular reason, by drawing a five-pointed star and stipulated that it represented predator-prey relations. What is the status of this star, qua representation? It is not as if the star *really is* a scientific representation, albeit a bad one, of predator-prey relations. Rather, the star plainly fails to be a scientific representation at all. Indeed, Yann Giraud's (2014) study of the Laffer Curve in economics reveals that bad scientific representations still include significant elements of what I have called licensing. Scientific representations, good and bad, are all constructed to assist in answering certain questions, explaining certain phenomena, and understanding certain target systems. It is through licensing that scientists and the broader scientific community build into and around the vehicle the features and interpretations capable of achieving these aims. A vehicle without licensing lacks these features and interpretations. As such, it is not just a bad representation; indeed, it is not a representation *at all*. A discussion of the representational nature of vehicles which lack these features is either infelicitous or involves an equivocation of the word 'representation.' A view of scientific representation which equally counts both the star and the Lotka-Volterra model as full scientific representations, even if it specifies one as good and one as bad, underestimates the role of these historical features of the model. They are not external to the representational nature of the vehicle, but are themselves an essential constitutive element of its representational nature: without these features, the vehicle is not a scientific representation at all.

5. The Special Problem of Scientific Representation

If I am right that licensing is a necessary constitutive feature of scientific representation which explains its communal nature, then contrary to Callender and Cohen's suggestion, we cannot pull the question of the constitution of representation away from questions of practice. A scientific object represents its target not (only) because there is some stipulation and pragmatic constraint, but also in virtue of licensing: the context in which it was created, the application of theoretical and empirical constraints, the awareness of and management of idealizations, and the history of its reception and use. Accounting for whether and how a scientific object represents its target will always require reference to these features which partially establish the representational nature. Thus, there *is* a special problem of scientific representation.

I should note that I am not here arguing for a stronger counter claim to Callender and Cohen which says that accounts of the representational nature of mental states are without *any* value to the constitution question of scientific representation. But my argument does indicate that an account of the representational nature of mental states *alone* is insufficient to account for scientific representation. Put otherwise: even if tomorrow we had a solid, universally accepted account of the representational nature of mental states, we would not yet have a complete account of scientific representation. We would still need an account of the deep reliance that it has upon the practice in which it is embedded. Thus, while our discussion of the constitution of scientific representation might include reference to the representational nature of mental states, it must also include reference to what I have described here as the licensing by the practice.

A different concern is that the use of the word ‘special’ is a bit deceptive. What I have identified here as the ‘special’ problem of scientific representation turns out to be a common feature of representation across disciplines, since, for example, I have suggested that it holds of artistic representation as well. While it is true that, according to my argument, an account of artistic representation will likely take account of licensing as well, it does not indicate that it is the *same type* of licensing in both practices. Indeed, given the unique aims that mark off scientific practice, its licensing can reasonably be expected to be correspondingly unique. That is to say that understanding, knowing, or explaining the empirical world are special aims, and therefore subject to special sorts of licensing. Scientific representation remains special because these features merit special attention.

We might also wonder whether it is right to continue to discuss scientific representation as a whole. If understanding representation in science requires in part that we understand the way in which scientists of a practice develop, utilize, and adapt these representational devices, then it is at least possible that these activities will be different within different domains. For example, the licensure of representations in physics might be rather different from that of economics. My suspicion is that, given the common broad scale aims of the various domains, we can still say some general things about representation in science as a whole. Nonetheless, we would do well to pay attention to representation as it occurs in these more localized contexts. Moving forward from this conclusion to develop further insights about the nature of scientific representation will involve analyzing specific representational objects or strategies as they occur in scientific practice, perhaps taking hints and clues from in-the-field investigations like those conducted by sociologists of science, e.g. those in Lynch and Woolgar (1990), Latour (1999), and Coopmans et al. (2014).

6. Conclusion

Though Callender and Cohen's view remains a formidable approach to the constitution question of scientific representation, I have endeavored in this paper to show why their account is insufficient, and thus why this question merits continued attention by philosophers of science. Representation in science is deeply tied up with the practice in which it is embedded. The communal nature of scientific representation can be seen in the way that science, as a practice, partially constructs its representations through the activities of licensing. The licensing is not the pragmatic limitation of some already existing representations, but is itself a constitutive element of the representational relationship. Any account of what it is for a scientific object to represent its target will necessarily involve reference to licensing. Thus, there *is* a special problem of scientific representation.

CHAPTER 4

THE MEANS-END ACCOUNT OF SCIENTIFIC, REPRESENTATIONAL ACTIONS

1. Introduction

In the past fifteen years, there has been an increased interest among philosophers of science in discussing the nature of representation as it is used within scientific practice (Boesch 2015; Suárez 2015b; Frigg and Nguyen 2016a). Over time, a significant number of philosophers have shifted their focus away from substantive accounts of scientific representation which refer to the *features* of representational vehicles and their targets and instead offer pragmatic accounts of scientific representation which focus more heavily upon the *actions* of *users* of representational vehicles. Apart from avoiding criticisms raised against substantive accounts, pragmatic accounts of scientific representation can explain a wide range of representational uses while drawing our attention closer to the actual practice of science.

Despite these advantages of pragmatic accounts, there has been little said about the notions of action, intention, and agency being employed by these accounts. Just as one example among many, Ronald Giere argues that representation occurs when “Agents (1) intend; (2) to use model, M; (3) to represent a part of world, W; (4) for some purpose, P” (2010, 269). Though he uses work in the philosophy of language to help explain the communicative nature of representation, he does not analyze the notions of intention or use which he employs. Since these agential concepts are playing an important role in his account (and, indeed, in all pragmatic accounts), the lack of analysis of these concepts constitutes a significant gap in our understanding of the representational practices of science. For this reason, I will turn in this paper to some work within the philosophy of action, and in particular to an account of the nature of intentional actions, to better understand the nature of scientific, representational actions.

Given the broad commitments of pragmatic accounts of scientific representation alongside some guiding suggestions offered by the proponents of these accounts, I will argue that a suitable account of action can be found in the work of G.E.M. Anscombe (Anscombe 2000). She argues that intentional actions are marked off from other actions in virtue of the form of the internal means-end structure of the descriptions of the agent's intentional action, which is itself revealed by a particular sort of 'Why?' question. After briefly describing Anscombe's account of intentional action, I will argue that representational actions in science can be understood and analyzed in virtue of their internal means-end structure. What I shall call the Means-End Account of Scientific, Representational Actions offers three central features: (I) the final description in the means-end ordering of descriptions is some scientific aim; (II) that interaction with a vehicle distinct from its target stands as an earlier description which is ordered toward the final description as means to end; and (III) the means-end structure is *licensed* by scientific practice, in the sense I have previously described (Boesch 2017). After describing each of these features in greater detail through an example, I show how the Means-End Account can demarcate scientific, representational actions from representational actions in other disciplines and from other types of scientific actions. I close by identifying some payoffs of the Means-End Account: that it offers the first account of the scientific, representational actions which ground pragmatic accounts of scientific representation and that it identifies representation as a *form* of action which is of use when exploring how representation is intertwined with other scientific activities.

1. From Dyadic to Triadic Accounts of Scientific Representation: A (Brief) History

To show explicitly the value of turning to the philosophy of action for this topic, it will be of use to first offer a brief characterization of the history of accounts of scientific representation. Early accounts of scientific representation were "dyadic" (Knuuttila 2005, 1261), in which scientific representation was understood as a two-place relationship that holds between some vehicle (e.g. a model) and some target system (e.g. a theoretical or empirical mechanism). The representational relationship, on dyadic accounts, is explained in virtue of some more fundamental relationship that holds between the vehicle and target—most commonly offered in terms of a structure-preserving mapping relationship, e.g. isomorphism, partial isomorphism, isomorphic embedding, and homomorphism (see, e.g., Bartels 2006; French and Ladyman 1999; French 2003; Van Fraassen 1980), or a more general relationship of similarity (e.g., Giere 1988)[13] Dyadic accounts of scientific representation have been subject to several criticisms. As just one example, a prominent set of criticisms (Suárez 2003; Frigg 2006) follows the work of Nelson Goodman (1976) and argues that both similarity and isomorphism (and other forms of structure-preserving mapping relationships) are insufficient accounts of scientific representation, since they do not have the same logical features as representation.

[13] While van Fraassen and Giere have often been interpreted as holding dyadic accounts of scientific representation in the form of isomorphism and similarity in their early work, Suárez (2004, 768) has pointed out that they each give importance to the role of a user even in their early work. Van Fraassen agreed with Suárez's assessment, arguing that he always left room for pragmatics, and only makes it more explicit later on (Ladyman et al. 2011, 443–44).

Partially due to these criticisms, philosophers of science later began to offer pragmatic accounts of scientific representation.[14] Pragmatic accounts are "(at least) triadic" (Knuuttila 2005, 1261) because their proponents argue that there is an irreducible place for the agent in the representational relationship. They argue that "what representations are depends on how we *use* them" (Knuuttila 2011, 266). I have already noted the central role for the scientist's agency, intentions, and use in Giere's (2010) account. Suárez's inferential conception of scientific representation similarly makes an essential reference to "the presence of agents and the purposes of inquiry" (2004, 773). These accounts are far from the only ones to give prominence to the agency and use of scientists in offering an account of scientific representation (Hughes 1997; Teller 2001; Bailer-Jones 2003; Suárez 2010, 2015a; Giere 2004; Contessa 2007, 2011; Van Fraassen 2008; Mäki 2009).

2. Three Ways Forward from Pragmatic Accounts

In some ways, the shift toward pragmatic accounts of scientific representation makes it difficult to offer any further insights about scientific representation. At least some pragmatic accounts of scientific representation deflate the concept of representation entirely to use. Deflationary accounts of representation imply that "the analysis of the concept of representation, even where feasible, cannot determine its conditions of application, and therefore cannot explain its use" (Suárez 2015a, 47). It is unclear what further insights can be offered by an account which cannot describe when and how representations will be used within scientific practice. Even for those pragmatic accounts

[14] Other alternatives to a substantive account include denying that there is any such thing as representation in science, suggesting that instead there is a mere family of resemblance of a set of practices (e.g., Lynch and Woolgar 1990). Giere (1994) and Knuuttila (2014) criticize this 'no-representation' approach. Another alternative is to offer an account of representation which applies to all disciplines in which it is found. Craig Callender and Jonathan Cohen (Callender and Cohen 2006) make such an argument in terms of internal mental states. For a criticism of this account, see my response to their work (Boesch 2017).

which maintain substantive elements in addition to a foundation in human actions embedded in a practice, there remains a concern that little more can be said about the pragmatic grounding of these accounts. As Tarja Knuuttila has put it: "if representation is grounded primarily in the specific goals and the representing activity of humans as opposed to the properties of the representative vehicle and its target, nothing very substantial can be said about it in general" (2009, 144). Nonetheless, there are still several ways forward which jointly help to make general progress in better understanding the practice of representation within science.

One way forward is to begin by acknowledging the essentially pragmatic nature of scientific representation, but then turn to examine and describe the more fundamental features and relationships which are frequently utilized when scientists represent. These more fundamental relationships are what Suárez (2003, 229) calls the "means" of representation—while representing scientists take advantage of these features, they are alone insufficient to explain the representational relationship. These include things like similarity (e.g., Giere 2004, 2010; Weisberg 2013), as well as other structure-preserving mapping relationships (Bartels 2006; Bueno and French 2011; Frisch 2015; Van Fraassen 2008). These studies are insightful and useful in understanding some instances and means of representational use since they can help draw our attention to some of the strategies and features that scientists use.

Another way forward, suggested by Suárez (Suárez 2015a, 47), is to pay more attention to the uses of representational vehicles in practice. Practical studies of classes of representational vehicles attend not to the more fundamental relationships being utilized in representational activities, but to the classes of representational vehicles which scientists construct, revise, and employ. Interesting and insightful studies have been made about various classes of vehicles, including models (Morgan and Morrison 1999a; Knuuttila 2005, 2011; Godfrey-Smith 2009; Weisberg 2007b; Knuuttila and Loettgers 2012, 2014, 2016) and other representational vehicles like diagrams and figures (e.g. Woody 2004; Perini 2005a, 2005b; Sheredos et al. 2013). Similarly, sociologists of science have done many useful studies examining the development and employment of some particular representational vehicle (e.g., Lynch and Woolgar 1990; Latour 1999; Coopmans et al. 2014). Studies of representational vehicles are of obvious value to deepening our understanding of the uses of scientific representation since they draw our attention to the ways in which classes of vehicles or specific instances of representation are utilized within practice.

There is a third way forward which, so far, has not yet been discussed by philosophers of science. It is to pay greater attention to the nature of agency which is central to pragmatic accounts of scientific representation and offer an account of scientific, representational actions. Many accounts indeed do offer some insight into what sorts of actions they have in mind. Van Fraassen, for example, speaks of using, making, and taking something as a representation in his famous *Hauptsatz* or central point about the nature of representation (2008, 23). Suárez (2004)(2004) speaks of the surrogate inferences that are made with regard to the target system being represented. And Giere (2004, 2010) speaks, as I mentioned above, about an agent's intentions and use. Despite the insights which have arisen from these accounts of using, making, taking, inference-making, intending, and so on, nothing has been said about the nature of the actions themselves with regard to their internal structure. Put differently, what is the nature of the action being performed when a scientist uses, makes, takes (etc.) a vehicle as a representation? What are the features and elements that make up the representational use of a vehicle? Taking up such an inquiry requires offering and working through an account of the nature of actions, working to apply the account to the representational practices in science so as to analyze the nature of scientific, representational actions.

In this paper, I will follow this third way forward, getting behind the references to using, making, taking, and so on to examine the internal structure of scientific, representational actions. My argument will speak generally about scientific, representational actions as a whole. The generality of my account will leave several elements unspecified, since as Knuuttila notes, we must proceed forward from pragmatic accounts in a way which leaves room for the "specific goals" taken up in the activity of the scientists (2009, 144). The account defended here will leave room for the specific goals a scientist might take up by offering a general account of some of the formal, internal features of the intentional actions of representing in science. Before I offer and defend an account of the nature of scientific, representational actions it will be of use to turn first to some work on the nature of intentional actions drawn from philosophers of action.

3. Desiderata of an Account of Scientific, Representational Actions

There are numerous accounts of the nature of agency and intentional actions within the philosophy of action. Before diving into the literature in greater detail, it is of use to first identify general features of the notion of agency as it is being utilized in pragmatic accounts of scientific representation. The most helpful insights are two related claims, one negative and one positive, about the relationship between representational use and mental states: (1) that representational actions are not explained in terms of internal, individual mental states and (2) that representational actions are to be explained in terms of the wider scientific practice.

Let us examine these in greater detail. Suárez is explicit in denying that "representation is necessarily the property of individuals… [or that] representation is 'in the head'" (Ladyman et al. 2011, 432; c.f. Suárez 2010, 98–99). If we are to have a truly pragmatic account of representation in science, Suárez argues that we must avoid "[a]n intentionality conception [of scientific representation which] … takes it that sources and targets are determined by some intentional state of some particular agent or agents, regardless of community, practice, and indeed any intended or unintended uses" (Ladyman et al. 2011, 432). He argues that an account which reduces representation to the singular intentions or mental states of some individual misses the important role that the broader scientific practice and community plays in scientific representation. Indeed, on Suárez's view, scientific representation is not 'in the head' but rather "'in the world', and more particularly in the social world – as a prominent activity or set of activities carried out by those communities of inquirers involved in the practice of scientific modelling" (Suárez 2010, 99).

Following Suárez's work, I have recently argued that scientific representation is "necessarily communal" insofar as it "is not isolated from the practice in which it is embedded" (Boesch 2017, §3). Scientific representations, I argue, are 'licensed', i.e. established as representational by the broader scientific practice. Licensing is a complex "set of activities of scientific practice by which scientists establish the representational relationship between a vehicle and its target" (Boesch 2017, §4) and includes elements like "the context in which [the vehicle] was created, the application of theoretical and empirical constraints, the awareness of and management of idealizations, and the history of its reception and use" (Boesch 2017, §5). My point is that if you are going to understand how, why, and in virtue of what some vehicle is a representation in science, you cannot ignore its history, its development, and how it is understood and used by the broader scientific practice. Put more directly to the concerns at hand, an account of scientific representation must move beyond individual mental states and consider the broader practice in which representational actions are embedded.

4. Anscombe's Account of Action

In selecting an account of action for the purposes at hand, it will be important that we follow the suggestions above to identify an account which pays attention to the broader circumstances and does not terminate in discussions about mental states. There are therefore reasons to shy away from the standard causal account which argues that actions are intentional in virtue of being caused by particular sorts of mental states (see, e.g., Davidson 2001). Such an account turns our attention inward to mental states, rather than outward to the circumstances and society in which an action occurs. Furthermore, the relevant mental states occur before the action actually takes place, meaning that "the subsequent 'effects'—i.e., the actual performance or doing, is simply a matter of nature taking its course" (Frey 2013, 4). As Harry Frankfurt argues, the causal account thereby "direct[s] attention exclusively away from the events whose natures are at issue, and away from the times at which they occur" (Frankfurt 1998, 70). Frankfurt's criticism is particularly relevant given that we are attempting to offer an account of scientific, representational actions as they occur within the context of scientific practice, not of the mental states which precede these actions. As such, we will need a different account of action which pays closer attention to the actual action itself and which helps to show the way in which an action is embedded in a wider practice.

G.E.M. Anscombe (Anscombe 2000) developed an account of intentional action which agrees with the basic point that actions are not intentional because of private, internal mental states. She is quite clear on this point, arguing that "intention is never a performance in the mind" (Anscombe 2000, 49). Instead, she argues that an investigation into intention must begin with the actions themselves: "The only events to consider are intentional actions themselves, and to call an action intentional is to say it is intentional under some description that we give (or could give) of it" (Anscombe 2000, 49). Put otherwise, the term 'intentional' does not have reference to a connection to internal mental states, but instead, "the term 'intentional' has reference to a *form* of description of events" (Anscombe 2000, 84). There is not room to discuss her account in much detail, but a brief overview of a few features will be of use in what follows.

To understand what Anscombe means by the 'form of description of events', we must first get clear on a few key concepts. An action, she argues, can be described in several accurate ways. Consider a simple action, like when I get a refill on my cup of coffee as a reward and encouragement to finish a paper. It can be described as 'getting a refill,' 'tipping a coffee pot,' 'activating certain neurons, neural regions, and nerves,' 'rewarding myself for finishing a section on my paper,' (if there is a crack in my mug) 'making a mess,' and so on. We can distinguish between these descriptions through the application or non-application of "a certain sense of the question 'Why?'" (Anscombe 2000, 84). The application or non-application of the 'Why?' question can be seen in the coffee refill example. Some of these descriptions are such that when asked why I am doing the thing described by that description, I will offer an answer—they are such that I will *not* refuse the 'Why?' question. So, for example, if you ask me 'Why are you getting a refill?,' I will accept the application of the question when I respond, e.g., that I am rewarding myself for finishing a section on my paper. However, you may also ask about descriptions to which I refuse application of the 'Why?' question (Anscombe 2000, 84). For example, if you ask, 'Why are you making a mess?" I may deny the application of the question by saying that I did not know that I was doing so. Genuine lack of awareness is one way that someone can deny the application of the 'Why?' question. Others include descriptions of something involuntary (e.g. activating certain neurons, since we cannot voluntarily activate specific neurons) as well as things which are 'downstream' from our action such that we cannot say that the agent is doing them now (e.g. getting tenure, since at the end of writing the paper I will not have tenure, even though the paper may very well ultimately contribute to my getting tenure).

Suppose that, in the coffee example, there are three descriptions to which the 'Why?' question applies: (A) 'Tipping the coffee pot;' (B) 'Getting a refill;' and (C) 'Rewarding myself for finishing a section.' Anscombe argues that these descriptions have a unique formal connection to one another which is central to explaining the intentional nature of the action. Consider the following series of 'Why?' questions and answers:

Why are you (A) tipping the coffee pot?

(B) To get a refill.

Why are you (B) getting a refill?

(C) To reward myself for finishing a section.[15]

[15] You could of course ask why I am doing (C). But at a certain point, the answer to this question is too far 'downstream,' as the example of 'getting tenure' shows (Anscombe 2000, 38–40).

There are two important things to notice about this example. First, Anscombe thinks that it is important that (A) is a means to the end of (B), and (B) is a means to the end of (C). That is to say that the reason or purpose in tipping the coffee pot is to get a refill, and the reason or purpose of getting a refill is to reward my hard work. Second, it is important to notice that (A) - (C) are not *separate actions,* but rather *separate descriptions* of one and the same action. In these contexts and circumstances, with the mug under the coffee pot (which is itself full of coffee), tipping the coffee pot *is* getting a refill, not a step toward getting a refill. Similarly, getting a refill *is* rewarding myself, not a step toward rewarding myself.[16]

We are now in a position to see what Anscombe means when she says that "the term 'intentional' has reference to a *form* of description of events" (Anscombe 2000, 84). The form is an internal means-end structure, in which the multiple accepted descriptions of one and the same action are ordered as means to ends. So, the intentional nature of an action has very little to do with internal mental states and instead has everything to do with the *structure* of the descriptions of the actions—which is itself understood within the broader context of the action since the contexts and circumstances (that the mug is under the coffee pot, that the coffee pot is full, etc.) are key in making it such that an earlier description (e.g. (A) tipping the coffee pot) is the same action as a later description (e.g. (B) getting a refill).

Furthermore, Anscombe argues that the broader context plays a central role in the agent's practical reasoning which itself reveals "what good, what use, the action is" (Anscombe 2005, 114). So, to understand how and why an action is intentional, we must

[16] This depends a bit on what I assume the reward to be. I am assuming it is the refill of the mug (and not drinking the coffee) because I often count a refill a reward and do not then drink from it. If you find this contentious, you can consider Anscombe's more detailed water pump example (Anscombe 2000, 37) which is not subject to the same ambiguities.

understand the aims of the action and how the contexts and circumstances connect certain descriptions accepted by the agent. So, for example, understanding how refilling coffee was intentional involves understanding my aims (rewarding myself), our society (that coffee is a desirable drink) and how the circumstances connect the descriptions—that the coffee pot is full, that the mug is under the coffee pot, that it is not leaking, and so on, are essential conditions to making it such that tipping the coffee pot (in this case) *is* an instance of refilling my mug.

So, if we are going to understand the nature of scientific, representational actions as intentional actions in the Anscombean sense, we will need to pay careful attention to the *form* of the scientist's action (the way in which descriptions are ordered as means to ends), the aims of the scientist (why she is representing—"what good, what use, the action is" (Anscombe 2005, 114)) as well as the circumstances in which she is acting, since these play a central role in structuring the form of the action.

5. The Means-End Account of Scientific, Representational Actions

There are two important demarcation projects in offering an account of scientific, representational actions. The first is to explain in virtue of what the actions are *scientific*, representational actions, i.e. how they are different from representational actions in other contexts. The second demarcation project is to offer an account of how the actions differ from other scientific actions, i.e. how the actions are scientific, *representational* actions. Each of these demarcation tasks will be conducted in terms of the "*form* of description of events" (Anscombe 2000, 84). Here, I will offer what I call the Means-End Account of Scientific, Representational Actions in which there are three features which hold of the form of the action which can jointly address the two demarcation questions and offer insights into the nature of the representational practices in science: (I) the final description in the means-end ordering of descriptions is some scientific aim (e.g. scientific explanation, prediction, theorizing, etc.); (II) that interaction with a vehicle distinct from its target (e.g. a mathematical model, diagram, figure, etc.) stands as an earlier description which is ordered toward the final description as means to end; and (III) the means-end structure is *licensed* by scientific practice, as I have previously described (Boesch 2017).

6.1 Three Features of Scientific, Representational Actions

Before describing how these features which hold of the internal means-end structure of scientific, representational actions can answer the two demarcation questions and offer insights about scientific, representational actions, it will be useful to spend some time explaining each of the features and describing them in greater detail. A running example will be of use during this stage, so let us consider a scientist who is using a mathematical model as a representation of some target system, e.g. a scientist who uses the Lotka-Volterra equations to represent the population dynamics of a predator-prey system of foxes and rabbits. What is going on when the scientist uses the equations in this way? Like the example of getting a coffee refill above, there are several ways to describe her action. She could be described as ‘writing down numbers,’ ‘manipulating an equation,’ ‘analyzing dynamical features of the equation,’ ‘coming to an understanding of the dynamics of populations resulting from predator-prey interactions,’ ‘shaking the table,’ and so on. As before, some of these descriptions will be denied the application of Anscombe’s ‘Why?’ question: for example, perhaps she does not know she is shaking the table and so would deny this description of her action. Let us suppose that she accepts three of these descriptions of her action, offered in the following imagined series of ‘Why?’ questions and her replies to those questions. As before, the use of the ‘Why?’ question helps to reveal how these descriptions are all of one and the same action and ordered as means to ends.

‘Why are you writing down numbers?’

‘I am writing the equation in different ways.’

‘Why are you writing the equation in different ways?’

‘I want to analyze its dynamical features.’

‘Why are you analyzing the equation’s dynamical features?’

‘I want to understand the population dynamics which result from the foxes’ predation upon rabbits.’

Each representational action will have its own set of descriptions which may vary due to user, aim, or vehicle. But, assuming that each case of representational action is an intentional action, it will be subject to the same sort of analysis as above. The Means-End Account of Scientific, Representational Actions identifies the general features that will hold of the action’s internal structure.

Using the example, consider the first part of the Means-End Account: (I) the final description in the means-end ordering of descriptions is some scientific aim (e.g. scientific explanation, prediction, theorizing, etc.).[17] In this example, the final description (understanding the fox-rabbit population) clearly counts as a scientific aim. Other scientific aims include, but are not limited to, things like scientific explanation, experimentation, theorization, prediction, alongside nonepistemic aims, including things like mitigation or practical implementation (Elliott and McKaughan 2014). I do not mean to offer a full and

[17] Note carefully that the multiple describability of actions, on Anscombe’s account means that there are many ways to describe any given action, but this does not further imply that either (1) every action has multiple aims in its descriptions nor that (2) the descriptions are purely arbitrary.

complete list of the potential scientific aims that can be present in scientific, representational actions. While the aims are in most cases easily identifiable, the only requirement is that the aim be recognized or accepted by the broader scientific community as a scientific aim.[18] Thus, for example, suppose that the scientist in the example above was writing the equation in different ways because she realized that the pencil scratches matched the beat of her favorite song. Such an aim is patently non-scientific. The same is true if she is writing the equation in different ways as part of a piece of art she is making. The final description must be one which her fellow scientists would count as scientific.

It is also worthwhile noting that the aims of the scientific, representational action also supply a standard of normative evaluation for the action. Insofar as a scientist achieves her aims, her representational action can be considered successful. An action might be successful with regard to some aims and unsuccessful with regard to others. Given the wide range of aims allowed by the Means-End Account, there is room to understand a similarly wide range of normative measures of evaluation, including accuracy, predictive success, explanatory power, among others. The Means-End Account remains uncommitted to which normative standards and measures might apply in any given representational action, except that they must be understood in the context of the scientist's aims. So, certain representational actions might take accuracy or correspondence as a normative measure of success, likely (though not exclusively) when the goals include knowing, understanding, or predicting. Such is the case, for example, with the representational actions involving scale-models, e.g. the Mississippi River Basin Model. Accuracy (in at least some respects)

[18] I make this point to avoid offering normative restrictions on scientific practice, especially as it changes and evolves going forward. I take it that the scientific community at any given stage will be the best judge (even if fallible) of what counts as a scientific aim.

in the construction and use of the model allows for more useful predictions and a better understanding of the dynamics of floods in the Mississippi River Basin. There are other cases where accuracy or correspondence will neither be a success or a failure of a representational action. Such is the case for models of systems which do not exist—for example in attempting to understand the evolution of sex by studying non-existent biological species with three sexes (Weisberg 2007b, 223). Here, there is no value to correspondence (since there could be no correspondence), though there is a value to explanatory power.

The second feature of scientific, representational actions is (II) that interaction with a vehicle distinct from its target (e.g. a mathematical model, diagram, figure, etc.) stands as an earlier description which is ordered toward the final description as means to end. In the Lotka-Volterra model, there were two earlier descriptions of interaction with the vehicle: writing the equation in different ways and analyzing the dynamical features of the equation. The relevant sense of 'interaction' in the second feature is meant to be left open, and includes things like analyzing an equation, writing an equation down, tracing fingers along a chart, examining and considering different parts of a diagram, altering variables in a simulation, and so on. As was the case with the relevant sense of scientific aims, it is important only that the form of interaction be recognizably useful in a scientific context by the broader scientific community. Minimally, the scientific community's recognition will involve a reasonable expectation that the form of interaction will accomplish some scientific aim. Thus, for example, the scientist's throwing of a crumpled piece of paper with the Lotka-Volterra equations printed on it is *not* a valid form of *scientific* interaction with the model since it would not help to accomplish any scientific aims.

It is important to note that the vehicle with which the scientist interacts must be distinct from its target. The purpose of this requirement is to incorporate the idea that representation is a non-reflexive relationship, meaning that any given vehicle does not represent itself (at least not for representation as it occurs within scientific practice). What counts as distinct for the purposes of the Means-End Account includes not only separate objects (e.g. a scale model to a river valley, a mathematical equation to a theoretical system), but also different perspectives on the same object. For example, interactions with a few model organisms (e.g. these three mice) can be used to draw conclusions about the broader species or sub-species to which they belong (e.g. *Mus musculus* or C57BL/6). Similarly, interaction with this sample of lead (if it is a representative sample) can be used to understand general features about lead as a whole. The distinction in both examples is sufficient to meet the requirement as explained by the Means-End Account.

The third and final feature of scientific, representational actions is (III) the means-end structure is *licensed* by scientific practice, in the sense I have previously described (Boesch 2017). As I described above, a representation is licensed through a complex "set of activities of scientific practice by which scientists establish the representational relationship between a vehicle and its target" (Boesch 2017, §4), including "the context in which [the vehicle] was created, the application of theoretical and empirical constraints, the awareness of and management of idealizations, and the history of its reception and use" (Boesch 2017, §5). Here, I am extending the notion of licensing to include the scientific, representational *actions* which undergird scientific representation. For actions, what matters is that the means-end structure itself be licensed; i.e. that the means is an accepted form of accomplishing the end. So, in the example above, analyzing the dynamical features of the Lotka-Volterra model is a licensed means of better understanding the relationship between predators and prey, including the specific fox-rabbit population at hand. However, suppose that the scientist was analyzing the dynamical features of the model but was aiming to understand the radioactive decay of some isotope of carbon. In such a case, while both her aim and means are scientific, the relationship between the means and the end is not licensed by the scientific community since the Lotka-Volterra model is not used to represent radioactive decay.[19]

Bruno Latour (1999) has effectively described many of the elements of what I call licensing in the construction and development of a diagram. Latour describes in meticulous detail the many minute steps by which scientists move and abstract away from the target system of forest-savannah interaction in the construction of a diagram which represents that system. The task involves, for example, dividing the land into sections, sampling soil, describing colors, taking plant samples, and drawing many iterations of the diagram before it is completed. The use of the diagram as a means to gaining understanding or creating explanation about the forest-savannah system is, on my account, *licensed* by these many activities on the part of scientists which, as Latour so carefully describes, are themselves subject to the aims, norms, and theory of scientific practice. The same is true for the relationship between other representational vehicles as means and certain aims and goals: the mouse, as a model organism, is licensed to allow for certain tentative conclusions to be made about the efficacy and safety of human consumption of some pharmaceuticals; the Mississippi River model was licensed to make predictions about floods and the mitigation

[19] In some cases, representational vehicles will be used in novel ways (e.g. in the transfer of a model between disciplines). In such a case, the licensing of the representational action is given through the scientist's application of theoretical and empirical constraints to her action (for more on this point, see Boesch 2017).

provided by the implementation of proposed flood-control systems in the river basin; Schelling's model is licensed as a means of providing a potential explanation of the types of societal habits which can lead to segregation; and so on and so forth.

6.2 The Demarcation of Scientific, Representational Actions

As identified above, there are two main ways in which scientific, representational actions need to be demarcated from other sorts of actions: (1) in virtue of what are they *scientific*, representational actions? and (2) in virtue of what are they scientific, *representational* actions? The Means-End Account addresses each of these demarcation questions.

Let us consider first what makes these actions *scientific*. The answer here is to be found in all three parts of the Means-End account. Both the action's aim offered in its final description and its means offered in an earlier description must be accepted as scientific by the broader scientific community. Furthermore, the connection between the means and the end is itself licensed by scientific practice. While representational actions in other disciplines (e.g. art) might utilize *similar* means and similar ends, they nonetheless are distinguished in virtue of being part of a different practice, constituted by a different set of aims, means, and set of licensing practices (Boesch 2017). On the Means-End Account, an action is scientific when it is informed and constrained by the practice of science, much in the same way that an action is a viable chess move when it is informed and constrained by the rules and practice of the game of chess. Some move, e.g. 'Castling', is a *chess* move in virtue of its relationship to the external rules of the game of chess and an understanding of its strategies and the practices that have arisen alongside the game. For the same reason, a representational action is scientific in virtue of its relationship to the external practice of science.

Of course, there are a wide range of scientific actions beyond only representation. The Means-End Account must help also to demarcate scientific, *representational* actions from other sorts of scientific actions. The central point of demarcation is that actions described by the Means-End Account have *denotational form.* Denotational form is drawn from the notion of denotation, the standing-in of a representational vehicle for its target. Nelson Goodman, whose work on representation is often cited in discussions of scientific representation, argues that "denotation is the core of representation" (Goodman 1976, 5). Several philosophers of science have similarly argued that denotation is an important part of an account of scientific representation (Hughes 1997; Elgin 2010; Frigg and Nguyen 2016b). The Means-End Account does not make reference to a relationship of denotation that holds between the vehicle and the target, but rather makes reference to the denotational form that holds of the scientist's action. Denotational form holds when an agent uses some vehicle as a stand-in for a target. Formally, this is present when there is one action with at least two descriptions (ordered as means to ends) in which the earlier description involves interaction with a vehicle and a later description involves the (potential) achievement of some end about the target. The Means-End Account describes actions which have denotational form, since features (I) and (II) require two different descriptions, one of an end to (potentially) achieve some aim about the target and the other of a means of interacting with the vehicle (which is distinct from the target found in the end).

There are two important caveats to the notion of denotational form. The first is that denotation is a non-reflexive relationship. Thus, denotational form must incorporate the non-reflexivity, meaning there must be a distinction between the vehicle in the first description and the target in the second. However, as is the case with the Means-End Account, even a difference in perspective is sufficient to account for the necessary level of distinction (e.g. it is sufficiently distinct if interaction with these three mice allows for conclusions to be drawn about *Mus musculus* as a whole). The second important caveat about denotational form is that it relies upon a deflated notion of denotation, what Suárez describes as "denotative function" (Suárez 2015a, 44). A vehicle has denotative function provided it is the sort of thing which would denote its target, supposing that its target exists. The modification to the notion of denotation is useful for an account of scientific representation since it helps to explain how it is that vehicles with fictional targets are still representations. For the purposes of the Means-End Account, it helps to explain how it is that an action can have denotational form, even if the aims involve (potential) achievements about a fictional target system.

The denotational form of a scientific action demarcates it from other forms of scientific action. Non-representational experimentation does involve scientific interaction with something and a scientific aim, but it does not have denotational form because it lacks distinction between the object of the interaction and the object of the aim. So, for example, an experiment designed to identify an unknown chemical lacks denotational form because the interaction occurs directly with the unknown chemical. Conclusions about the object of interaction (the unknown chemical) are not generalized to be about a broader species, but are instead about the unknown chemical itself.

Of course, there are plenty of cases of experimentation (and other forms of scientific action) which do have denotational form. But this is because representation is deeply intertwined with many different forms of activities in science. As just one example, representation is a common element to many types of experimental design. Thus, experimental actions which have denotational form have it because the experimental method involves representation. So, for example, a public health experiment which uses a sample of some population to draw conclusions about the value of some health protocol utilizes representation as an essential feature of its design. The experimental design is such that the sample population is a representation of the larger population. Indeed, much of the work in experimental design in such cases is involved with obtaining a representative sample and collecting data which allows the scientists to show others that the sample is a meaningful representation of the larger population—both constituting the licensing in such a case. The denotational form of the action in this case points to the representational nature of the action, even if it is also a form of experimentation. Indeed, as I will discuss below, the Means-End Account is useful in helping to identify the ways in which representational actions can be brought into and united with other forms of actions in science, even while providing the tools by which we can identify what makes a representational action in science distinct from a non-representational action.

6.3 Payoffs of the Means-End Account

Before examining some of the payoffs of the Means-End Account, it is important to pause and summarize what the account does and does not do. The Means-End Account does *not* offer an account of the nature of scientific representation. As such, it is not able to explain new cases of representation which other accounts could not explain. It begins with the assumption that some pragmatic account or group of accounts has already provided a good analysis of the nature and use of representation in scientific practice.

Following on this assumption, it offers an analysis of a component which has been heretofore unexamined: the nature of the actions that undergird and ground representation for these accounts. The Means-End Account does not, therefore, explain how some model represents its target, though it does explain what it means to say that it is used representationally by some agent working in the context of scientific practice. The Means-End Account offers an account of the actions "carried out by those communities of inquirers involved in the practice of scientific modelling" (Suárez 2010, 99).

The first payoff of the Means-End Account follows upon the very fact that it describes not the nature of scientific representation, but rather the nature of scientific, representational actions. Because it takes a different target for its analysis, the Means-End Account provides novel insights for our understanding of the important practice of representation in science. Whereas pragmatic accounts of scientific representation have terminated their discussion of actions by describing using, taking, making, inference-making, and so on, the Means-End Account gets to a deeper level by offering an analysis of the agential concepts from an action-theoretic perspective. It describes the conditions which hold of the internal structure of representational actions in science. Such an account is useful for a few reasons. One is that it offers a stronger theoretical ground for pragmatic accounts of scientific representation, by filling in some of the undescribed details about the nature of the agency which grounds these accounts—for example, by explicating the relationship between normative standards and the context of use. It can also be of use in analyzing instances of representational use associated with particular vehicles and particular aims. For example, when analyzing the representational use of some diagram, the Means-End Account can be of use in analyzing the representational action, specifically with regard to how the action in this case differs from the scientific and representational actions in other cases. Most importantly, though, is that the Means-End Account offers a significant step forward in an attempt to gain a more complete grasp of the practice of representation within science. A more complete account of the practice of representation in science will explain not only the nature of scientific representation, but also the strategies and features scientists use (e.g. similarity and other structure-preserving mapping relationships), the classes of representational vehicles used and particular case studies, and, now, with the Means-End Account, the nature of scientific, representational actions.

As I mentioned above, the Means-End Account can be of use in understanding how other forms of scientific activities (like experimentation) utilize representation, since it points to the way in which representation is deeply intertwined with other scientific activities. According to Anscombe, there are two ways we might think of a single action with multiple descriptions. One of these recognizes that each description is a full characterization of the action and can be used to describe the action as a whole. The alternative way of understanding an action with multiple descriptions is to recognize that there is a particular eminence to the final term in the series of descriptions. As Anscombe says, "the last term we give in such a series…so to speak swallows up all the preceding intentions" (Anscombe 2000, 46). So, if we are going to offer one and only one description of an action, the most proper description is the final description of the internal means-end structure. According to feature (I) of the Means-End Account, all scientific, representational actions take as their final description some scientific aim (e.g. explaining, understanding, knowing, predicting, etc.). As such, successful instances of scientific representation are not merely cases of representation, full stop. Indeed, there is no such thing as successful scientific representation which is not also properly describable as some other scientific activity, as an instance of understanding, knowing, explaining, predicting, exploring, discovering, and so on.

The reason for the intricate connection between representation and other scientific activities is connected to the way in which the Means-End Account understands representational actions of science in terms of their *form*. On the Means-End Account, representational actions occur when an action best characterized by its final description (e.g. explanation, prediction, etc.) meets certain formal requirements regarding the ordering of descriptions and the relationship between them. As such, representation takes on an 'adverbial' function within scientific practice: scientists understand *representationally*, explain *representationally*, discover *representationally*, experiment *representationally*, and so on. On the Means-End Account, then, representation turns out to be one of the ways in which the many types of scientific activities occur.

There are a couple of useful consequences to this final insight about scientific, representational actions, each of which could be more fruitfully explored in future work. For one, it suggests that there may be interesting forms of strategies associated with representational forms of certain types of activities. For example, explaining representationally might employ certain techniques that discovering representationally does not employ (and vice versa). It is possible, therefore, that studies of representation could be classified not in terms of classes of vehicles (e.g. models, simulations, diagrams, etc.), but rather in terms of classes of activities (e.g. explaining, understanding, predicting, etc.). Saying anything in detail about these differences will require a closer analysis of the practices associated with these forms of activity, perhaps including an analysis of case studies. The Means-End Account offers the tools to be able to perform such an analysis which will offer insights about a wide range of scientific practices.

6. Conclusion

In this paper, I have taken seriously the suggestion offered by a wide number of philosophers of science in recent years that representation in science must be understood in terms of a scientist's agency, actions, and activities. I argued that in order to have a more complete picture of the practice of representation in science, it is important to offer an account of scientific, representational actions. Following some guidelines already present in the literature on scientific representation, I argued that Anscombe's account of intentional action has the right sorts of features to be able to fit with the other pragmatic accounts of scientific representation which have already been offered. Following her insight that intentional refers to the *form* of description of events we give to an action, I argued that scientific, representational actions have three features which help to demarcate them from other sorts of actions. The Means-End Account of Scientific, Representational Actions contributes to a more thorough picture of the practice of scientific representation, by offering a novel account of the scientific, representational actions. It also points to the potential for future work on the way in which representation is intertwined with other forms of actions in science.

CHAPTER 5

PRAGMATIC ACCOUNTS OF SCIENTIFIC REPRESENTATION[20]

1. Introduction

Many recent philosophical accounts of scientific representation are pragmatic in nature.[21] Pragmatic accounts of scientific representation are marked off from other accounts because they give an irreducible place within the representational relationship to the role of the scientist and her intentional activity. Put otherwise, all pragmatic accounts are minimally triadic (Knuuttila 2005) insofar as they reference not only the representational 'vehicle' (models, equations, diagrams, etc.) and its representational 'target' (real-world systems, data, theoretical objects, etc.), but also include the intentional actions or 'intended use' activities (Suárez 2004) of a scientist or group of scientists.

Pragmatic accounts have much to suggest for themselves qua philosophical explanations of scientific representation. For one, they are not subject to the same sorts of criticisms that have been levied by Mauricio Suárez (2003) and Roman Frigg (2006) against alternative dyadic or two-place accounts (e.g. similarity and isomorphism).[22] Furthermore, both Suárez (2003) and Frigg (2006) argue that pragmatic accounts are better

[20] Boesch, B. Pragmatic accounts of scientific representation: A tapestry of explanation. Under review at *Journal for General Philosophy of Science*.
[21] For an overview of the literature on scientific representation, see (Boesch 2015; Suárez 2015b; Frigg and Nguyen 2016a).
[22] Defenses of isomorphism or partial isomorphism are given by van Fraassen (1980), French and Ladyman (1999), French (2003), Bartels (2006), Bueno and French (2011), among others; defenses of similarity are given by Giere (1988) and Weisberg (2013). For a response to the objections of Suárez (2003) and Frigg (2006), see Chakravartty (2010), Bueno and French (2011) and Toon (2012).

able to deal with intentional misrepresentations, allowing them to better explain the use of a wide range of representational devices within the varied domains of scientific practice. For this reason, it is not surprising to see so many pragmatic accounts developed and defended in recent years, including RIG Hughes' (1997) DDI account, Mauricio Suárez's (2004) inferential account, among many others (Teller 2001; Bailer-Jones 2003; Giere 2004, 2010, Contessa 2007, 2011, Knuuttila 2005, 2011; Van Fraassen 2008; Mäki 2009; Frisch 2014).

What is more surprising is the variation that exists between pragmatic accounts of scientific representation. While there appears to be agreement that representation is grounded in human action, there is no similar agreement about how this is to be cashed out, in terms of the analysis of scientific representation. Within pragmatic accounts of scientific representation, we find references to denotation, demonstration, and interpretation (Hughes 1997), representational force and surrogative reasoning (Suárez 2004), a scientist's intending to use a vehicle to represent the world for certain purposes (Giere 2004), and a scientist's using, making, and taking (Van Fraassen 2008), just to name a few. What are we to make of these variations? Do differences in terms represent a difference in analysis of representation? Call this puzzle the question of external unity of pragmatic accounts of scientific representation.

One answer to the question of external unity is to suggest that there is a substantial difference between these accounts. That, e.g., reference to denotation, demonstration, and interpretation is substantially different from reference to a scientist's intentions to use a vehicle to represent the world for certain purposes. If this is the case, then there would be a conflict between the many pragmatic accounts: to accept one would be to reject the others.

For anyone seeking to analyze an instance of scientific representation, much would hang upon precisely which account taken. Of course, such a response would help to explain the proliferation of pragmatic accounts of scientific representation, since, on this interpretation, each offers its own distinct view of the representational relationship. All the same, it is not clear what the relevant difference is between an account which references a scientist's intending to use a vehicle to represent the world for certain purposes and one which references denotation, demonstration, and interpretation.

A different answer to the question of external unity, and the one which I will defend in this paper, argues that pragmatic accounts are complementary with one another. I will argue that the many pragmatic accounts offer a tapestry of explanation through which we can understand the complex practice of representation in science. Each account takes up a different focus and places stronger emphasis on different elements within the activity of representing. In different ways and to different extents, accounts highlight the role of ends and aims in representing, or the interaction with the vehicles and their features, or the translation that occurs between vehicle and target. Representation in science, I will suggest, is a complex and varied practice and cannot be quickly and succinctly understood. Thus, the myriad of pragmatic accounts constitutes a tapestry of explanation by which we can better understand the nature of representational practice in science through drawing our attention to the many important elements of representational activities.

To argue that the many pragmatic accounts offer a tapestry of explanation of scientific representation, we must turn to understand the nature of the common element which undergirds pragmatic accounts: the irreducible reference to the agency of the representing scientist. Using an account of the nature of intentional actions offered by

G.E.M. Anscombe (2000), I show that any given intentional action can be described in a number of ways. The difference in description does not indicate a difference in action, but rather a difference in how the action is understood and characterized. A similar point holds true for pragmatic accounts of scientific representation. Each offers a different set of descriptions of the scientific representation which holds in virtue of the representing actions of the scientist. The differences do not indicate a fundamental point of disagreement between proponents of pragmatic accounts, but rather demonstrate a difference in form of description of the representational relationship which is grounded in their actions. Put differently, in any given instance of scientific representation, there is a single representational relationship, which holds in virtue of a single representing action. The many pragmatic accounts of scientific representation merely offer different characterizations of the representing action and the representational relationship that holds because of it.

1. Two Pragmatic Accounts of Scientific Representation

While the analysis that follows is applicable to all accounts of scientific representation which are grounded in action, it will be helpful to defend it in terms of particular and concrete examples, so as to show specifically how it applies to the accounts in question. I will begin by describing two prominent pragmatic accounts: Hughes' (1997) DDI Account and Suárez's (2004) Inferential Account.

2.1. Hughes' DDI Account

Hughes' (1997) DDI account describes scientific representation with reference to three features: denotation, demonstration, and interpretation. That is to say that a scientist represents by performing an activity in which she denotes, demonstrates, and interprets.

The first of these features, denotation, is the way in which a scientist uses a representational vehicle as a stand-in for the target system (Hughes 1997, S329-S331). According to Hughes, denotation is a broad term meant to include things like the denotation of concrete particulars (e.g. a model of the solar system will denote particular planets), the denotation of specific types (e.g. Bohr's theory models not just *one* hydrogen atom, but *all* hydrogen atoms), and the denotation of a model of some global theory, e.g. this particular model is "represented as a *quantum system*" (Hughes 1997, S331). Common among each type of denotation is the notion of 'standing-in': in each case, the representational vehicle is serving as a stand-in for something else.

Denotation or standing-in is insufficient by itself for an account of representation in science, since we must also pay attention to the particular ways in which these representations are used within the practice to bring about certain results and show how these results hold of our target system. The second part of Hughes' DDI account is demonstration, the feature which explains how certain results are brought about (1997, S331-S332). In order to allow for demonstrations, it must be the case that representational vehicles "contain resources which enable us to demonstrate the results we are interested in" (Hughes 1997, S332). So, for example, a physical model of a protein will allow us (through manipulation of the parts of the model) to see what sorts of configurations allow for connections or separations between the various parts of the model. Similarly, the mathematical parts of the Lotka-Volterra model will allow the scientist to identify the simple harmonic motion it identifies. Notice that in both cases, the demonstrated results are discussed entirely in terms of the model. For Hughes, demonstration occurs when a scientist shows that there are certain features which hold of the representational vehicle which allow her to demonstrate results (e.g., make predictions, identify trends, analyze connections, and so on) which themselves hold of the representational vehicle itself.

Of course, representation in science must not stop with inferences about the mathematics (or whatever feature of the representational vehicle). Scientific representation must show that the demonstrated results do not hold *only* of the vehicle, but that they connect and inform scientists about the real world or theoretical targets. As such, Hughes offers a final element in his DDI model: interpretation (1997, S332-S335). Interpretation takes the scientists from the vehicle back and returns her to consideration of the target. In interpretation, the demonstrated results (predictions, trends, connections, etc.) are taken to hold of the target as well. So, for example, the scientist interprets her results as informing her not only of the plastic model of the protein, but about the actual stability of the configuration of the protein in the real world. Similarly, the Lotka-Volterra model is understood to demonstrate simple harmonic motion not only for the dynamical equation, but also for the simple harmonic changes in population sizes for a group of predators and prey in virtue of their predator-prey interactions.

Before moving on to describe Suárez's inferential account, it is important to pause for just a moment and point out that the three features of the DDI account are understood by Hughes as "three activities" (1997, S329). That is to say that Hughes is not ultimately describing his account in terms of the features of representational vehicles, but in terms of the activities done by scientists. Of course, neither I nor Hughes is suggesting that representational vehicles are without important features which offer affordances for the activities of denotation, demonstration, and interpretation. All the same, Hughes is clear that, for the DDI account, scientific representation is grounded in the activities scientists conduct with the vehicles rather than the features of the vehicles. Scientific representation is a practice in which a scientist takes some vehicle to denote some target, demonstrates certain results which hold of the vehicle, and finally interprets those results to bear upon her understanding (or explanation or knowledge) of the target system.

2.2. Suárez's Inferential Account

The second account which I wish to examine in greater detail is Suárez's (2004) inferential account.[23] Suárez understands scientific representation in virtue of two features. The first of these features is what he calls representational force, defined as "the capacity of a source to lead a competent and informed user to a consideration of the target" (Suárez 2004, 768).[24] Representational force carries with it not only the denotative function, insofar as a scientist uses the vehicle as a stand-in for its target, but also an important place for the rules and norms of the practice. The need for representational force does not imply that a vehicle must serve as a stand-in for anyone who uses the vehicle, but rather for the "competent and informed user" (Suárez 2004, 768). All the same, one of the important elements of representation in science is the vehicle's pointing-towards its target, which is described by Suárez as representational force. So, for example, the representational force of the physical model of the protein points towards the real-world protein (or the theoretical structure of that protein). Similarly, the Lotka-Volterra model, when used by a competent and informed user, point to the relationship between populations of predators and prey.

As Suárez argues, representational force alone will not suffice for representation *in science*. Scientific representation is objective insofar as it is constrained by the aims of understanding, explaining, or knowing the target system and by the scientific practice in

[23] Suárez offers further details about his inferential account elsewhere (Suárez 2010, 2015a).
[24] N.B. Suárez uses the term 'source' where I use the term 'vehicle'.

which the representation is embedded.[25] As such, representational vehicles must also have the second feature, what Suárez calls "the capacity to allow surrogate reasoning" (2004, 773). That is to say that when informed and competent scientists use the representational vehicle, they will not only be led to consider the target system, but to draw specific sorts of inferences with regard to those targets. Suárez is not particular about whether the inferences are instances of understanding, or explaining, or knowing, since he argues that the relevant sorts of inferences can be the result of "any type of reasoning…as long as [the source] is the vehicle of reasoning that leads an agent to draw inferences regard [the target]" (2004, 773). Scientists do not represent because they merely need to be reminded of the targets or because they simply need to be led to consider the target system. Scientists represent because they want to make inferences about the target—to know more about the system, to better explain some causal relationship, to better understand some relationship, etc. The capacity to allow for surrogate reasoning captures this important feature of scientific representation. So, for example, the physical model of the protein will not only direct a scientist to consider the real-world protein, but will also be such that she can draw inferences about the stability of some configuration of the real-world protein. Similarly, the Lotka-Volterra model does not exist only to help scientists draw predator-prey relationships to mind, but also to help them draw specific sorts of inferences and insights about that relationship.

As was the case with Hughes' DDI account, it is important to pause for just a moment to explain that Suárez also takes his account to describe conditions of the representational activities performed by scientists in accord with a practice. Indeed, Suárez

[25] For more on the relationship between a representation and the broader community, see (Boesch 2017).

has explicitly stated that representational force and the capacity to allow for surrogate reasoning are properties of activities: "[W]hen appropriately placed in their context, [the two parts] are best understood to appropriately refer to properties of particular activities within a normative practice" (Suárez 2015a, 42). According to Suárez's inferential account, scientific representation occurs when a scientist performs activities which have certain features: moving from consideration of the vehicle to consideration of the target (representational force) and using the vehicle to draw certain inferences about the target (the capacity to allow for surrogate reasoning).

2. A Turn to the Philosophy of Action

In order to understand how these and other pragmatic accounts are related to one another, we will need to understand the nature of the common feature which unites them**:** the irreducible role of the actions of a scientist. Though other accounts could be used, there are advantages to using the account of intentional action offered by G.E.M. Anscombe. Most importantly, her account allows us to understand the way in which an action is embedded in a practice and denies that we can understand intentional action in terms of mental states. These features are central elements of the actions that undergird pragmatic accounts of scientific representation, according to Suárez (2010) and my prior work (Boesch 2017).

Anscombe argues that intentional actions are marked off from non-intentional actions because they are those "actions to which a certain sense of the question 'Why?' is given application" (Anscombe 2000, 9). The relevant sense of the 'Why?' question is "that in which the answer, if positive, gives a reason for acting" (Anscombe 2000, 9). So, for example, we can ask why an agent is doing some description of her action. If she answers the question positively, she allows application of the question, showing the description to be an *intentional* description of the action. To better understand this, consider Anscombe's famous water pump example:

> A man is pumping water into a cistern which supplies the drinking water of a house. Someone has found a way of systematically contaminating the source with a deadly cumulative poison whose effects are unnoticeable until they can no longer be cured. The house is regularly inhabited by a small group of party chiefs, with their immediate families, who are in control of a great state; they are engaged in exterminating the Jews and perhaps plan a world war.—The man who contaminated the source has calculated that if these people are destroyed some good men will get into power who will govern well, or even institute the Kingdom of Heaven on earth and secure a good life for all the people; and he has revealed the calculation, together with the fact about the poison to the man who is pumping. (Anscombe 2000, 37)

There are, of course, a wide range of descriptions we can give to his action. For example, we might say that he is helping to institute a better government, or that he is replenishing the water supply, activating certain neural pathways, moving a pump up and down, making a rhythm to Queen's "We Will Rock You", earning his wage, and so on. Through the use of the 'Why?' question, we can narrow down the list to consider "all and only his intentional actions" (Anscombe 2000, 38).

To understand which set of descriptions characterizes the agent's *intentional* actions, we must first eliminate three general types of descriptions. Each of these types of descriptions forms a way in which the agent might reject the 'Why?' question. First, there are certain descriptions which would not be countenanced by the agent because she lacks knowledge or awareness (Anscombe 2000, 11–13). So, for example, if we ask the agent why he is making the rhythm to Queen's "We Will Rock You," he can deny application of the 'Why?' question by explaining that he does not know the song. Since the 'Why?' question does not apply, this is not an *intentional* description of his intentional action. A second set of descriptions which are denied application of the ‘Why?’ question are those which describe an activity which can never be intentional (Anscombe 2000, 11–13). For example, the agent might reject the description that he is activating certain neural pathways, since this is not the level at which we can control things. The third set of descriptions which are excluded from the set of intentional action descriptions are those which are ‘further downstream’ from the action at hand. These descriptions occur after what Anscombe calls a “break”: the descriptions for which it “is not such that we can now say” that the agent is doing the action, e.g. “to save the Jews, to put in the good men, to get the Kingdom of Heaven on earth” (Anscombe 2000, 40). This is not a good description of the agent’s intentional action, because it is not what the agent is doing. He, of course, aims to do these things, and his action may be a vital step in the plan. But in completing the action at hand, he will not have accomplished the aim. For our purposes, we need not linger any longer on the descriptions which are excluded, except to note that while any given action has a wide range of potential valid descriptions, the range is not boundless. It matters what the agent knows, what she is capable of controlling, and what she is accomplishing in the action at hand.

Moving on from the rejected descriptions, we can recognize that an intentional action will have some descriptions which the agent will accept as meaningful responses to the ‘Why?’ question. For the water pump example, Anscombe suggests that we assume that there are four: A) He is “moving his arm up and down”; B) He is “operating the pump”; C) He is “replenishing the water supply”; D) He is “poisoning the inhabitants” (Anscombe 2000, 41). All the same, Anscombe argues that even though there are four separate action descriptions, we should not suppose that there are four actions, but rather a *single* action which can alternatively be described in four different ways: “moving his arm up and down with his fingers round the pump handle *is*, in these circumstances, operating the pump; and, in these circumstances, it *is* replenishing the house water supply; and, in these circumstances, it *is* poisoning the household” (Anscombe 2000, 46).

For the purposes of answering the question of the external unity of pragmatic accounts of scientific representation, it is important to briefly pause to emphasize that descriptions A-D are not the *only* potential descriptions which could have been offered for the agent’s action. Had we so wished, we could have selected four different descriptions for the pump example above, e.g., A*) He is pushing and pulling a pump; B*) He is drawing water from the ground; C*) He is performing his job; D*) He is committing an act of murder. The exact details of the A*-D* descriptions are not particularly important. What is important is to note that, A-D are not inherently a better set of descriptions than A*-D*. Depending on what you attend to in the action, you can offer a different account of the action descriptions present in an intentional action. Put differently, descriptions A-D and descriptions A*-D* are equally acceptable means of describing the agent’s actions.

3. Multiple Describability and External Unity

At this stage, we are in a position to be able to put forward an answer to the question of external unity of pragmatic accounts of scientific representation. Consider an example of the representational use of the Lotka-Volterra model to describe the relationship between a population of foxes and rabbits. According to Hughes, the model is a representation because its use involves the three activities of denotation, demonstration, and interpretation. So, we can imagine a series of hypothetical ‘Why?’ questions asked to the scientist which follows Hughes pattern:

Why are you writing down the model?

I'm using it as a stand-in for the fox and rabbit populations.

Why are you using it as a stand-in?

I'm able to manipulate the model to identify mathematical and analytical features of the model.

Why are you showing mathematical features of the model?

I am interpreting these mathematical features of the model to hold also of the fox-rabbit population.

The exact nature of the answers might vary, but if they are following the three elements Hughes had in mind, we might expect them to be something like the above.

The use of Suárez's Inferential Account will yield a different set of descriptions and answers to the series of 'Why?' questions. We could imagine interrupting our scientist yet again (hoping she is not too annoyed by our question asking), this time asking questions and receiving answers with the inferential account in mind:

Why are you writing down the model?

I'm using it to consider the fox-rabbit system.

Why are you considering the fox-rabbit system?

I am drawing inferences about their relative population sizes over time.

As before, the exact descriptions offered with the inferential account in mind may differ.

The question of the external unity of pragmatic accounts of scientific representation has to do with the relationship that holds between these two ways of describing one and the instance of representing action and representational relationship which holds because of it. My suggestion is that the differences between the descriptions offered in each account is the same as the difference between descriptions A-D and A*-D* as described above. Recall that A-D offered four descriptions of the action of the man at the pump whereas A*-D* offered for descriptions of the very same action. These differing sets of descriptions, A-D and A*-D*, should not be considered as inconsistent and competing accounts of the action (indeed, we could imagine the man at the pump to agree to both). Instead, they are accounts of the action which emphasize different points and offer a different perspective on the very same action.

The same is true for the difference of descriptions offered by the DDI account and the inferential account. They do not offer accounts of the action which are inconsistent with one another; to accept the DDI's means of understanding the representational use is not thereby to deny the inferential conception's accounting of the representational use (and vice versa). Instead, both accounts offer unique perspectives and emphases about the nature of the action.

Consequently, we can think of the differences between these accounts as making salient certain elements of the practice of representation in science. Through the use of two examples of representational uses of vehicles, I will discuss the differences in emphases in greater detail below. But, as a first pass, we can say that Hughes' DDI model of scientific representation, while it is certainly interested in the whole practice of representation, is focused most heavily upon the scientist's actions *qua* manipulation of the vehicle. He specifically describes the way in which results are demonstrated first and foremost to hold of the vehicle, and then interpreted to hold of the target. Suárez's inferential account seems more interested in describing the greater purposes of the representation in making inferences about the target system. His account focuses heavily on the inferences drawn about the target system, drawing our attention to the role the inferences may play in achieving the aims of the scientist. Whatever the case, it is important to note that the difference in emphasis does not imply that the accounts are incompatible with each other. The very same instance of representing can be described by each account. And, as I argued above, we need not think that one set of descriptions is privileged over the others.

We should also note that the compatibility between the accounts does not imply that they collapse into one another. The difference in salience in each account provides us a different means of understanding the complex practice of representation in science—some which may be more useful when explaining representation within particular domains (e.g. in economics versus in chemistry), as it occurs with particular types of vehicles (models versus equations versus diagrams, etc.), or for the sake of particular aims and goals (e.g. explanation versus prediction). Indeed, it is for this very reason that my analysis provides a positive argument in favor of the plurality of pragmatic accounts of scientific representation. The practice of representation in science is "a complex phenomenon" and "a laborious art" (Knuuttila 2014, 304). Any single account, while it may offer an account of the nature of the representational relationship present in virtue of the representing actions, will not be able to sufficiently draw our attention to each of the elements of representation and the various strategies which may be employed. It is for this reason that the numerous accounts offer us a tapestry of explanation, through which we can better understand representational practice (both in general and in particular case studies). There is thus good reason to discussion and development of both of these accounts and of others (new and old), since, jointly, they can better describe the complex practice of scientific representation—across strategies, devices, and disciplines.

4. Two Examples

To better understand the difference in emphases between the DDI Account and the Inferential Account, it will be of use to consider a couple of examples in greater detail. I will consider a scale-model and a mathematical equation with a fictional target. By examining the analysis provided by each account in terms of the intentional action descriptions, we will be able to notice the way in which the DDI Account focuses attention on the use of the vehicle and the inferential account focuses our attention on the achievement of epistemic aims in representation.

4.1 Mississippi River Basin Model

The Mississippi River Basin Model (MRBM) was a scale model of the Mississippi river basin designed by the United States Army Corps of Engineers. Starting from its semi-completion in 1949, the MRBM was used by the Army Corps of Engineers to study the effects of floods and proposed new flood management systems in the Mississippi River Basin for nearly twenty years, most usefully predicting the effects of a flood on the Missouri River (a major tributary of the Mississippi River) in 1952 (Foster 1971). The model was used representationally in this case among many others. Engineers at the MRBM used data collected by engineers in the field to simulate certain conditions of water in-flow for the model which allowed the engineers to identify which levees were most likely to break or be overtopped, and of these, which could be helped by adding sandbags, and which were a lost cause (Foster 1971, 27). The model's use was successful in predicting the various flood stages that were seen at observation points along the Missouri River, as well as in strengthening levees or evacuating populations at risk of flooding. In this one case, the MRBM was credited with avoiding nearly 65 million dollars of damage (Foster 1971, 27).

Depending on which account of the nature of scientific representation you take, you might offer different explanations of the representational nature of the MRBM in its use in predicting levee damage in the 1952 Missouri River floods. Each of the accounts is complementary: to accept the DDI Account of the representational use of the MRBM is not to reject the Inferential Account. Each account can draw our attention more closely to certain elements of the representational relationship.

Hughes' DDI account offers an analysis in terms of denotation, demonstration, and interpretation. In the first place, the DDI Account describes the denotational relationship that exists in using the MRBM as a representation of the Mississippi River Basin. The denotation is seen best in the way that the MRBM was designed so as to be useful as a stand-in for flooding and the effects of flood control systems in the Mississippi River Basin. As a map, it is only marginally useful, since its scale (1:100 vertical and 1:2000 horizontal) made for a fairly distorted map (Mississippi Basin Model Board 1945). Thus, while the MRBM allows individuals familiar with the river system to know what part of that system some channel of the model represents, it is not useful as a means of measuring, say, the distance between the Ohio river and the Missouri rivers. However, the scale (and many other features of the model) were, in fact, designed to denote the *flooding* effects and stages of the Mississippi river basin. Thus, scientists, in their testing associated with the Missouri River flooding of 1952 began by taking the relevant portion of the model to stand-in for (a small portion of) the Mississippi River Basin, with regard to its flooding and the effects of the flooding.

But this was not all they were doing, since it was important for them to be able to make meaningful and useful predictions. Thus, they first needed to demonstrate results that hold of the model. By using a complex set of instruments for both controlling water inflow and measuring water height and outflow rates as a function of time, the engineers were able to demonstrate that certain results held of the MRBM itself. That is, they showed that given inflow rates of some value, this part of the MRBM became inundated, or this point of outflow measurement measured at some new value. But, of course, since they take the model as a stand-in of the Mississippi river basin, they do not stop there. The demonstrations are taken to hold not only of the MRBM, but also to bear upon the flooding in the Missouri River. To do so, engineers would have to understand the results (measured in units of minutes and centimeters) as holding of the river basin (interpreted in units of days and meters) (Foster 1971, 20–21). They had to notice what results held only of the model. Engineers would communicate the meaningful results they gathered to engineers at the field offices, who could apply them in the field to put them to use.

Using Hughes' DDI account helps draw our attention to the way in which the model was made as a stand-in as well as to the particular things which were performed on the model itself. It draws our attention to the manipulability and the interactions with the model, showing how an important element of representational practice is the manipulation of a vehicle and the collection of results understood entirely in terms of the model itself. Measurements at this point are made in terms of the model, with references to the model's grid system and model-type levels of water flow and channel height. But the representational action is not complete at this stage. The scientist who stops here, with demonstrations only in terms of the model has not represented. Thus, Hughes' account also draws our attention to the way that the model results are interpreted and understood in terms of the target system. In this case, that draws our attention to the use of scale and the conversion that scientists make from model-type levels of water flow and channel height to the predicted river-type levels of water flow and channel height.

The use of Suárez's account of scientific representation to analyze the same case is not in competition with the account offered by Hughes, though it does offer different emphases. First, Suárez's account describes the representational force of a representational vehicle. The representational force exists when a competent scientist is led to consider a target when using a source. The consideration here is similar to what was said about the demonstration within the DDI Account, though Suárez is more explicit about the role of the scientific community in the use of the MRBM as a stand-in. In particular, it matters that the engineers using the model understand the way in which the model is useful and the ways in which it is not useful. It matters not only that the model was designed to describe flooding and flood-control systems of the Mississippi River Basin, but also that the engineers using it are aware of this fact and are acting as members of the community.[26] Second, according to Suárez, it is important that surrogate inferences be drawn about the target through the use of the model. In the case of the MRBM, we can see how it is that the use of the model allows the engineers to draw inferences about the Mississippi River Basin, and more specifically about flooding on the Missouri River. The predictions about flood stages and knowledge of vulnerable levees came to the engineers not through a direct examination of the river or theorization about the same, but through the use of the MRBM.

[26] The importance of the community's role in scientific representation is described in greater detail in (Boesch 2017).

As before, Suárez's account is not in competition with that of Hughes, though there are a different set of emphases. Apart from drawing our attention more closely to the communal element into the representational activities, Suárez's account focuses on the outcomes of the representational practice. His account draws our attention to the way in which representational practice is productive: through the creation of knowledge, predictions, and explanations. As such, Suárez's account invites us to consider the way in which aims are present in representational practice, not as an achievement after-the-fact of representation, but as part of the activity of representing itself.

4.2 Hardy-Weinberg Equations

There are similar insights with other representational vehicles, even those which have a fictional target. Consider, for example, the Hardy-Weinberg equations (Hardy 1908). The Hardy-Weinberg equations ($p+q=1$ and $p^2+2pq+q^2=1$) are used to describe the change in allelic frequency of a population over time and were initially used to show that, assuming random mating and a stable population, recessive alleles would not become extinct over time. A population is described as being in a state of Hardy-Weinberg equilibrium provided that the allelic frequency of the population is static over consecutive generations of a population. Importantly, there are a number of assumptions required to reach a state of Hardy-Weinberg equilibrium. Many of these assumptions are fairly realistic: for example, that the organisms are diploidic, that organism reproduce only sexually, that generations do not mate with one another, and that allele frequencies are equal in the sexes. But a number of the underlying assumptions are less realistic, for example, that mating is random, that there is no mutation, migration, or selection. One of the assumptions is actually *impossible*: that the population is infinitely large. As such, the use of the equation to represent a population which is at a state of Hardy-Weinberg equilibrium will involve representing a population which could not possibly exist.

Let us reverse the order for this example and consider Suárez's inferential account of scientific representation first. His account will explain how a scientist's use of the Hardy-Weinberg equation will allow her to represent a population in a state of equilibrium through reference to the two properties of activities. First, the scientist will take it that the equations represent an infinitely large population of a diploidic, sexually reproducing species (and so on). Importantly, the scientist will understand that it can represent such a fictional target because she is an informed user. She is aware of a number of features of the equation and its representational force that other, uninformed users would not know; for example, that the assumptions reduce any opportunity for selection to occur. Second, the scientist will use the equations to draw surrogate inferences about the target. In this case, the scientist can show, by calculating the allelic frequency for each generation, that the allelic frequency does not change. By violating certain assumptions, she can make additional important and insightful inferences about the population which can inform the theory of population genetics. For example, she can infer that the allelic frequency of the infinite, non-migrating, diploid, sexually-reproducing, randomly-mating, etc. species will not change over time. From this, she could—as Hardy originally aimed to show (1908, 49)—infer that, in the absence of selective pressures, the dominant allele will not take over.

To employ Hughes' account of scientific representation, we must make a minor (and friendly) amendment to his account, following a suggestion of Suárez (Suárez 2015a). Hughes' use of the concept of denotation suggests that representation can occur only when a target actually exists. Given the fact that scientists often represent fictional targets, it is important to modify his account to allow for such uses. Suárez suggests that instead of a discussion of denotation, Hughes should have made reference to the "denotative function" of a representational vehicle. A vehicle has denotative function when it is the sort of object which *would* denote its target, provided the target actually exists. I take it that this is a friendly amendment to Hughes' account, given the pragmatic nature of his explanation of scientific representation.

With this small change in mind, let us consider the representational use of the Hardy-Weinberg equations according to Hughes' account. First, there is the denotative function, in which a scientist allows the equations to stand in for the fictional population. Second, there is demonstration in which the scientist shows certain features that hold of the equation (e.g., that the frequency of p is stable over multiple iterations of the equation). Here, the manipulation of the equation is done entirely in terms of the equation, demonstrating particular results and insights that hold of the equation itself. Finally, the scientist interprets these demonstrated results to hold of the fictional population as well, showing that the allelic frequency of the population would hold steady as well.

Similar to the example of the MBRM, Suárez's account draws our attention more heavily to the role of the knowledge of the representing scientist (in terms of being informed) as well as the aims and goals of the representing scientist, by pointing us to the specific inferences she is drawing. In this case, it allowed us to see the representational uses in explaining that the dominant allele would not increase in frequency in the absence of selective pressures. Indeed, Suárez's account can be useful in drawing our attention to the way in which a vehicle is constructed for the achievement of particular aims. Representational practice is all ordered towards these aims and ends, and the inferential account is useful in making this fact salient across a wide range of examples. As was the case with the MRBM, the DDI account draws our attention more closely to the *means* of the representational use, to the way in which the equation itself is used, manipulated, and modified, before conclusions are drawn about the target system. We are made to consider the successive iterations of the equation that the scientist might write down, one after the other, to show the stability in the numbers of *p* and *q*. Suárez's inferential account makes such an element less explicit, by drawing our attention more quickly to the aims and end results. Hughes' account makes salient the nitty-gritty work associated with the use of the model itself. The role of interpretation also points to another important element of representational practice in science, that of translating between the vehicle and its target. Hughes does not describe the nature of the translation that occurs, but does draw our attention to the importance of such a translation, which is itself the focus of other pragmatic accounts of representation (as I describe in greater detail below).

4.3 Other Accounts

It is important to note that the insights mentioned here are not restricted to a comparison between Suárez's and Hughes' accounts. All pragmatic accounts are subject to the same sort of analysis as I have offered and described for these two accounts, in which the features they identify are a set of descriptions of the representing actions and the representational relationship they undergird. Similarly, each of the accounts will demonstrate additional benefits in terms of emphases when explaining particular representational uses. Where Suárez's account draws our attention to the the communal nature of the representational relationship as well as the aims and goals of the representing and Hughes' account draws our attention to the manipulation and interaction with the vehicle and points to the role for translation, other accounts will draw our attention more closely to other features.

Giere's (2004, 2010) account offers a means of better understanding the translation between vehicle and target which Hughes' account makes salient. Giere's Agent-Based Similarity Account suggests that representation in science is a four-place relationship in which some "Agents (1) intend; (2) to use model, M; (3) to represent a part of the world, W; (4) for some purpose, P" (2010, 274). He goes on to suggest that similarity is "one way, perhaps the most important way, but probably not the only way" that scientists use a model to represent the world—it is a feature exploited by scientists in translating from vehicle to the world. So, for example, a scientist using the Lotka-Volterra model will exploit the similarities that hold between the change in values of variables to the change in values of the population of foxes and rabbits. The Army Corps of Engineers used similarities in flow-rates, channel height, and outflow between the MRBM and flooding on the Mississippi River basin to be able to translate between the results demonstrated on the vehicle and those which would hold of the river basin itself.

Similar attention to the translation between vehicle and target is found in van Fraassen's (2008) account which acknowledges the irreducible role for the agent in the representational relationship, but suggests that "a model can (be used to) represent a given phenomenon accurately only if it has a substructure isomorphic to that phenomenon" (2008, 309). Van Fraassen's account thus draws our attention the relationship that holds between structures and substructures of a vehicle and a target, offering a different means of understanding the translation between vehicle and target.

5. Conclusion

There are a myriad of pragmatic accounts of scientific representation, each of which, I have argued, contributes to our understanding of the wide-reaching and complex practice of representation in science. By drawing our attention to different features by providing alternative emphases, the accounts offer a tapestry of explanations. Further studies of this sort may reveal that certain accounts are more useful for an understanding of the representational practices found in some particular discipline or subdiscipline, associated with the use of particular types of vehicles, or performed for the sake of particular aims and goals. Whatever the case, it is clear that the plurality of pragmatic accounts is to be valued, not because each is *partially* correct, but rather because each is a unique perspective by which we can understand the wide, varied, and complex practice of representation in science.

CHAPTER 6

CONCLUSION: SCIENCE IS REPLETE WITH HUMAN ACTIONS

1. Four Approaches to Representation

One of the ways of explaining the value of my dissertation is to show how the perspective of locating scientific representation at the level of human action is distinct from other means of exploring scientific representation. Apart from the pragmatic approach, which I have deepened and further developed in my dissertation, there are three other prominent means of explaining the nature of scientific representation—the substantivist, internalist, and sociological accounts. The first of these, substantivist accounts of representation (similarity and (partial) isomorphism), approaches representation as if it were a relationship "out there" in the world. It appeals to a more fundamental relationship which is supposed to exist between features of the representational vehicle and features of its target.

On the other hand is the view proposed by internalists (e.g., Callender and Cohen 2006) which places the representational relationship entirely inside the minds of scientists. According to the internalist picture of scientific representation, we cannot explain the representational relationship in terms of the features holding of objects of the world, but rather in terms of the representational nature of our internal mental states. Representation, wherever it is utilized, turns out to be a relationship which is explained with reference to our minds and mental capacities, rather than a relationship which is explained by features in the world (as the substantivist perspective suggests).

A third approach, advocated by some sociologists of science (e.g., Lynch and Woolgar 1990), argues that representation is a feature of scientific practice. In order to understand representation in science, they argue, we must understand a wide range of heterogenous activities which are called representation. However, these sociologists of science argue that there is no "originary reality" (Lynch and Woolgar 1990, 8) to be represented. As such, there is no real 'representation' in science, but rather a set of activities by which scientists construct and abstract away from their targets. Sociologists of science present what Giere called a "no representation account of representation" (1994, 115).

The fourth approach to the problem of representation is that offered by pragmatic accounts of scientific representation. As I have advanced and defended the pragmatic approach in this dissertation, it has both points of agreement and of significant departure from each of the three alternative approaches to the nature of representation in science.

While I deny the substantivist's claim that representation in science can be *reduced* to features of representational vehicles and their relationship to the world, I agree with the substantivist that representational use relies heavily upon the features which are built into representational vehicles. In chapter four, I argued that an investigation of representational means (like similarity and isomorphism) is one of the ways we can come to a better understanding of scientific practice. Indeed, the construction or development of similarity and isomorphism in any given representational vehicle would constitute a significant element of what I describe in chapter two as the representational licensing of the vehicle.

I agree also with the internalists (and with the pragmatists) that there is an irreducible role for the scientist in understanding the nature of scientific representation. My understanding of scientific representation defended in this dissertation requires that there be individual, intelligent agents who perform scientific, representational actions. However, the internalist goes wrong by missing the broader role of scientific practice in these actions, as I argued in chapters three and four. Representation is not a feature that holds in virtue of a scientist's mental states, but rather in virtue of her practical rationality: her actions performed in the context of the broader scientific community.

With the sociologist of science, I acknowledge the central place and value of the scientific community in understanding representational practice in science. However, despite its complexity, I maintain that there is a real relationship of representation in science. To accommodate the complexity of scientific representation, we must acknowledge the wide range of means and ends which are involved in representational actions in science (as I described in chapter four).

The pragmatic approach to scientific representation, as I have developed and defended it here, suggest that the relationship of representation is not located 'out there' in the world, nor is it located in the minds of agents, and neither is it a mere family resemblance of distinct activities. Instead, the relationship of representation in science is located at the level of human actions performed in accord with a practice. As Suárez has put it: scientific representation is located "'in the world', and more particularly in the social world – as a prominent activity or set of activities carried out by those communities of inquirers involved in the practice of scientific modelling" (Suárez 2010, 99). Over the course of the chapters of my dissertation, I have further developed this idea—describing the role of the community in scientific representation (in chapter three), offering an account of scientific, representational actions (in chapter four), and explaining the history of and continued relevance of pragmatic approaches to scientific representation (in chapters two and three).

2. Value for Future Work

Apart from deepening and developing the pragmatic approach to the nature of scientific representation, there is additional value to my dissertation project in that it will serve as a groundwork for additional fruitful projects. There are three general types of future project which I see flowing directly from the work I have begun and developed in this dissertation.

The first main area of further research will continue to explore and investigate scientific representation through the lens of action theory in the way that I have initiated in this dissertation. One project that has strong promise is a further development of the concept of representational licensing, which I first introduced in chapter three. As a reminder, in that chapter I argued that an irreducible component of the representational relationship is the way in which it is licensed as a representation by the broader scientific community. While I provided a brief analysis of the concept and demonstrated its importance in reference to the development of the Lotka-Volterra model, there is more work to be done in better elucidating the concept and showing its relevance to representation in other contexts. As such, one potential project coming out of this dissertation is the further exploration and elucidation of the notion of representational licensing, both theoretically (providing a deeper analysis of the notion of licensing) and practically (using case studies to show how licensing functions within scientific practice).

One of the features that makes my dissertation project novel is the way in which it connects two literatures which are otherwise fairly unconnected: philosophy of science and philosophy of action. The connection, as explored and utilized in this dissertation, has been unidirectional—using the insights from action theory to better elucidate and understand concepts in philosophy of science. I think there are also important and interesting studies which could be made by reversing this strategy, that is, by using work in the philosophy of science to better understand important concepts within the philosophy of action. As just one example, the literature within the philosophy of action on the topic of know-how and skills tends to focus on simple examples, e.g., riding a bike, skiing, playing piano, and playing chess. These may be helpful examples for initial forays into the concepts, but they lack much of the complexity which is often associated with many real-world skills which involve dynamic aims, complex sociological and practice-oriented interactions, and inter-skill interactions. If we are going to develop a concept of skill which is useful in more complex studies—e.g., in education or virtue ethics—we will need to rely on more complex examples, which mimic the complexity found in learning and in living a virtuous life. As such, the complexity of skills as they function within scientific practice might form a solid basis on which to form, develop, and test an account of skills

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