A Study of the Discursive Aspect of Scientific Theorizing and Modeling

by

Micheal McEwan

A thesis
presented to the University of Waterloo
in fulfillment of the
thesis requirement for the degree of
Doctor of Philosophy
in
Philosophy

Waterloo, Ontario, Canada, 2014

I hereby declare that I am the sole author of this thesis. This is a true copy of the thesis, including any required final revisions, as accepted by my examiners.

I understand that my thesis may be made electronically available to the public.

Abstract

My dissertation contributes to the study of scientific theories and models by using a speech-act-theoretic framework to investigate the discursive aspect of theorizing and modeling practices. In the philosophical study of science some of the central questions concern the nature of theories and models and how they are used by scientists. There is debate about whether theories are best understood as representations or as tools useful for non-representational purposes, and among those that think they are representations there is disagreement about what kinds of representations they are. I argue here that a systematic investigation of the discursive aspect of theorizing and modeling practices can be useful to this debate: it offers a rich source of evidence which different accounts of theories and models can be evaluated against.

Theorizing and modeling often involves the presentation of a series of sentences and equations. In these cases the uttered expressions are constitutive parts of a discourse within which theories and models are presented and used. I consider the influential work of Cartwright, French, Giere, van Fraassen and others, to show that different accounts of what theories and models are have implications for what function expressions can be expected to play in discourse. By observing how sentences and equations are actually used by scientists I argue we can determine which accounts best capture scientific practice. Here speech act theory provides a framework for systematically identifying features of discourse that indicate how expressions are used by scientists on a case-by-case basis.

I also defend the view that theories and models are often linguistic representations in which sentences and equations are used by scientists to say things directly about the systems they study. This runs contrary both to instrumentalist views, which take theories to be non-representational, and to the semantic view, which takes theories and models to be non-linguistic. I defend my view by showing the discursive data drawn from a canonical example, the London model of superconductivity, unambiguously favours my account. On the basis of this evidence and additional considerations I conclude that many other theories and models are likely linguistic representations too.

Acknowledgements

I want to first thank my supervisor, Dr. Marino. Her encouragement and feedback have been invaluable. As a direct result of her influence I think and write better.

I must also thank my advisory committee members, Dr. Fraser and Dr. Douglas. They both put in the extra time and effort to read early drafts of my work. My dissertation is much stronger as a result. I am also grateful for the stimulating feedback I received from Dr. Schweizer and Dr. Frost-Arnold.

I am blessed by family and friends who have been relentless in their support. Mom, Dad, Steve, and Jessica, you are the best.

Table of Contents

vii

List of Figures

Chapter 1

Introduction

1.1 The Discursive Aspect of Theorizing and Modeling

This work is a contribution to the study of scientific theories and models. Theories have long been an object of inquiry among philosophers of science. Newtonian mechanics, Darwin's theory of evolution and Einstein's general theory of relativity all number among the great achievements of science. Philosophers have worked to better understand what theories are and how they function within scientific efforts to learn about and understand the world around us.

Increasingly philosophers of science also emphasize the importance of models in understanding scientific practice. The Copernican model of the solar system and the Bohr's model of the atom are two famous models which have proven to be important to science. The rising interest in models among philosophers may also be partly attributed to the influence of the *semantic view* defended by Giere (1988), Suppe (1989), Suppes (1967), van Fraassen (2008) and others. The semantic view, arguably now the most dominant view of theories, roughly identifies theories with collections of models, where models are typically construed on this view as abstract mathematical objects or structures. Besides the semantic view, Cartwright (1989, 1999), Shomar and Suárez (1995), Hughes (1997), Morrison (1999) and many others offer influential accounts that attribute distinctive roles to theories and models in scientific practices. Cartwright, for example, has argued that models are the primary vehicles of scientific knowledge and theories are merely tools for constructing models.[1]

Though the present work is a contribution to the study of scientific theories and models, I cannot hope to investigate in a comprehensive way the myriad things often called theories or models in the sciences. I shall instead undertake a more focused investigation of what I call the *discursive aspect* of theorizing and modeling practices. My interest in this aspect of theorizing and modeling practices arises from a general

---

[1]This is Cartwright's view as expressed in *Nature's Capacities* (1989) and her portion of *The Toolbox of Science* (1995). See chapter 5 for a discussion of how her views have changed more recently in her *Dappled World* (1999).

observation about scientific practice: in many cases scientific theorizing and modeling is facilitated by, and inextricably implicated in, what appears to be language-like discursive activities. To get an idea of what I mean by language-like discursive activities all one has to do is look in scientific textbooks, in journals, at classroom blackboards, or listen to discussions in laboratories or department colloquia. What we typically find are sentences and equations that scientists use to help present and make use of their theories and models. Moreover in many of these cases it appears that the utterance of these sentences and equations is an essential part of theorizing and modeling practices. In such cases it is in virtue of these sentences and equations that scientists present, express, formulate and make use of theories and models.

To take one example, Bohr famously presented his model of the atom to the general scientific community in his three-part paper (1913a, 1913b, and 1913c) "On the Construction of Atoms and Molecules." In the same work he also showed how his model could be used to account for, among other things, the observed Rydberg-Balmer series of emissions for the Hydrogen atom. When we look upon the pages of this paper all that we find are English sentences, mathematical equations, and many mixed sentences that contain both English and mathematical expressions. For Bohr to have presented (a version of) his model in the pages of this paper, he must have done so via the sentences and equations found therein. The sentences and equations in the paper are constitutive parts of the discourse through which Bohr presented and used his model of the atom.

In calling the sentences and equations in Bohr's paper parts of a discourse, I mean that the uttered expressions form a series of communicative acts performed within a common context (the context in this case is, roughly, the written paper itself). In Bohr's paper, like many examples of theorizing and modeling in the sciences, the discourse is comprised of language-like expressions. The English sentences are straightforwardly linguistic—they are grammatical expressions of the English language. Whether or not the label 'linguistic' applies equally well to the mathematical equations and mixed (English and mathematical) expressions found in the paper is, perhaps, more contentious. Nevertheless mathematical and mixed expressions, like those in Bohr's paper, are language-like expressions that bear important similarities to more paradigmatic linguistic expressions. They are, for example, comprised of a series of symbols, their formation is governed by conventionally specified syntactic rules, and these rules support a distinction between well- and ill-formed expressions. Moreover they are symbols that regularly have a communicative significance that speakers can use to help convey their communicative intentions. This allows equations and mixed expressions to count as communicative 'moves' within a discourse.

In connection to the observation that scientific theorizing and modeling often has an essential discursive aspect I shall develop and defend two main ideas in this work. The first idea is that the language-like discursive aspect of theorizing and modeling practices can provide important evidence and insight regarding what sorts of things theories and models are. Since the discursive aspect of theorizing and modeling practices is often essential to the presentation and use of theories, the thought is that these practices themselves might be helpful in trying to understand the nature of the theories and models they are used to present. As we shall see, this suggestion gains further credibility once we acknowledge that different conceptions of theories and models imply that expressions function in a different capacity in discursive practices. Insofar as the discursive practices themselves can provide evidence regarding the use of par-

ticular expressions, then the discourse can provide evidence regarding which conception of theories and models is best supported by the *discursive data*. The discursive data includes the features of expressions, their relations to one another within the discourse, and features of the context within which these expressions are uttered.

The other main idea that drives my investigation of scientific discursive practices builds upon the first. Roughly expressed, the idea is that sometimes the theories and models presented via a series of language-like expressions are intimately connected to the expressions themselves—sometimes theories and models might *almost* be identified with these expressions. This suggestion can be cashed out somewhat more precisely with the following complementary suggestions. First, the expressions associated with theories and models are sometimes used by scientists to *say things* about the systems they investigate. Second, scientific theories and models are sometimes descriptions or description-like representations of real systems. These suggestions naturally fit together. When sentences and equations are used to form description-like representations, these expressions are used to say things about the representation's target. In this case the contents of theories and the models are, roughly, the things said and the tokened expressions are the vehicles of this content. These suggestions form the basis for a view which I call the linguistic approach to scientific theories and models, or *linguistic approach* for short. According to the linguistic approach some theories and models are description-like representations and the expressions (sentences and equations) which make up these description-like representations are used by scientists to say things about the systems they investigate. This view captures the idea that theories and models are intimately connected to the expressions used to present them.

As I shall explain in the next chapter the suggestion that theories or models are description-like is not new, but it is a contentious, underdeveloped and under-represented view in the current philosophy of science literature. The linguistic approach developed in the subsequent chapters is my attempt at addressing this shortcoming with a view fleshed-out in what I take to be the most defensible way. As we shall see, there are different reasons why views like the linguistic approach are, at least currently, in the minority. Some reasons have historical roots. The *syntactic view of theories* associated with logical empiricism (which will be discussed in §3.3.1) roughly identifies theories with expressions of a formal language. Some worry that approaches like the linguistic approach will be untenable for the same reasons that the syntactic view is now commonly thought to be untenable. Other reasons are more general. For example, some worry that if a theory is too closely associated with certain expressions, then it becomes impossible to present the same theory using different expressions (see §3.3.2). Consequently we arrive at the counter-intuitive conclusion that, for example, Einsteins theory of general relativity expressed in English cannot be identified with the theory's German counterpart. If the linguistic approach is to prove tenable, it must be able to address in one way or another these and other concerns.

Animated by the above outlined main ideas, the discursive aspect of theorizing and modeling practices are investigated in this dissertation with the *general goal* of better understanding how these practices can be analyzed to help philosophers of science develop better descriptive accounts of theories, models and the practices which produce them. The discursive aspect of theorizing and modeling practices are also investigated with the *narrow goal* of evaluating the plausibility the linguistic approach.

The subsequent chapters contain a sustained defence of the linguistic approach. That is, they all contain coherent and largely continuous lines of argumentation aimed at showing the plausibility of the linguistic approach (once clarified and suitably qualified). It is important, however, to realize that this dialectical structure is intended not only to forward the investigation's narrow goal, but its general goal as well. As we shall see, the linguistic approach offers an important but under-represented view concerning the function of expressions in the discursive aspect of theorizing and modeling practices. Part of the impetus for developing the linguistic approach, then, is to fill out the menu of possible views to be investigated as part of a general effort to understand the philosophical import of scientific discursive practices. In developing and evaluating the linguistic approach the differences between it and its most well-established competitors must be explicated. The distinctions that emerge from this discussion will be used to develop methods for evaluating different philosophical views against the evidence gathered from the discursive practices associated with theorizing and modeling.

Let me now provide a somewhat more detailed overview of how I set about investigating the discursive aspect of theorizing and modeling. Then in the last section of this introduction I shall say more about what conclusions I believe we can draw from it.

1.2   The Investigation's Structure

The dissertation can be roughly divided into three parts. The first part includes chapters 2–5. In it the linguistic approach is first articulated in detail, defended, and contrasted with two of the most well established views in the contemporary literature, the semantic view and Cartwright's nonrepresentationalism. In the second part, chapters 6 and 7, methods for systematically analyzing the discursive aspect of theorizing and modeling practices are developed and applied. The last part, chapter 8, takes up an alternative version of the semantic view which poses a potential threat to motivations underlying the linguistic approach and other descriptive accounts of scientific theories and models.

1.2.1   Part 1: Adding the Linguistic Approach to the Mix

Chapters 2–5 introduce the linguistic approach and defend it by highlighting some of its relative strengths in relation to the other more well established views. In chapter 2 I utilize an example of modeling to provide a much more substantive introduction to the linguistic approach. Chapter 3 presents a number of general arguments and considerations in favour of the linguistic approach, and it works to forestall some criticisms that are likely to be raised against it. Then in chapters 4 and 5 I take up the semantic view and Cartwright's nonrepresentationalism respectively. These are two of the most influential contemporary philosophical views concerning theories and models. Each offers an importantly different perspective on the nature of theories. In chapters 4 and 5 the differences between all three views are further explicated, and the main considerations offered in defence of these competing views are outlined and evaluated. I

also draw upon the linguistic approach to develop some novel analyses and criticisms of Cartwright's nonrepresentationalism and the semantic view.

Again the linguistic approach is my attempt at fleshing-out two parallel suggestions: (1) the expressions associated with theories and models uttered by scientists in their discursive practices are, at least sometimes, used to say things about the systems they are investigating; and (2) theories and models are sometimes descriptions or description-like representations. The linguistic approach captures these two ideas in a unified way with the following analysis. When the sentences and equations associated with a model or theory are used to say things about some system under investigation, the sentences and equations function in a *direct linguistic representative capacity*. In virtue of functioning in this way the uttered expressions present (or express) a *linguistic representation* which directly targets the system under investigation. In these cases the linguistic approach identifies theories and models with the linguistic representations presented.

This analysis will be unpacked in the next chapter. To get the flavour of it, however, let me say a bit about the notion of a linguistic representation and the linguistic approach's distinctive act-theoretic explication of it. The notion of a linguistic representation is introduced to make more precise the idea of a description-like representation. The class of linguistic representations, as I characterize it, includes descriptions. It also includes assertions, claims, hypotheses, conjectures, guesses, and the like. Now there may be different ways one could try to analyse descriptions, assertions, claims, etc., so as to reveal in what sense they are intuitively categorized as 'linguistic' representations. For reasons detailed in the next chapter, I characterize linguistic representations in an act-theoretic way. Describing, asserting, hypothesizing, etc., are first understood as acts performed by uttering expressions in a discursive context. Expressions are not in and of themselves descriptions, assertions, guesses, or some other kind of linguistic representation. Rather, when it comes to linguistic representations what matters is what a speaker has *done* by uttering an expression. What the speaker has done is a function of how the expression is used by the speaker in the context of utterance. When it is used in a linguistic representative capacity the speaker has performed an *act of linguistic representation* and by performing this act they have presented a linguistic representation. So the claim that, say, a theory is a linguistic representation rests ultimately on the claim that the sentences and equations associated with the theory have been used in the context by the speaker (a scientist) in a linguistic representative capacity. The main challenge in characterizing linguistic representation act-theoretically, then, is determining what it is to use an expression in a linguistic representative capacity. This of course is something I will say a great deal more about (in both chapters 2 and 6). But looking ahead, I shall argue, roughly, that an expression is used in a linguistic representative capacity when it is used both to convey some truth-apt propositional content and when these contents are endorsed (to a greater or lesser degree) by the speaker. This presents the linguistic approach with an avenue to make contact with and be evaluated against scientific practice. Roughly, evidence that scientists do both of these things by uttering sentences and equations in discourse will count as evidence that linguistic representations are presented in that context. Conversely evidence that scientists do not do either of these things by their utterances will count as evidence that their expressions are not used in a representative capacity.

Now the act-theoretic analysis is only part of what makes the linguistic approach distinctive. The

following empirical question is left untouched by the analysis alone: when (if ever) are the sentences and equations associated with theories and models used by scientists to say things directly about the systems they are investigating? If, for example, these expressions are never used in this way, then by the lights of the linguistic approach's own analysis theories and models are never linguistic representations. So in connection to this question the linguistic approach offers the *direct representation hypothesis*. It is the hypothesis that in cases where theorizing and modeling has an essential discursive aspect the sentences and equations associated with theories and models are *often* (or regularly) used by scientists to say things *directly* about the system, or systems, under investigation in the theorizing or modeling context. If this hypothesis is correct, then in those cases where theorizing and modeling practices have an essential discursive aspect the theories and models presented in those contexts will be linguistic representations that directly target the systems under investigation.

This hypothesis too will be unpacked and explained in chapter 2. Worth mentioning now is the significance of the 'direct' part of the direct representation hypothesis. Looking ahead, one thing that will become important when distinguishing the linguistic approach from the semantic view is tracking of the representational targets of language-like expressions. That is, we need to be clear what the expressions in discourse are being used to say things about. As will be illustrated by examples in the coming chapters, it is usually clear in the context what the systems under investigation are. These are the putative objects of investigation that theories and models are deployed to help investigate and this usually means theories and models are deployed as part of a scientist's efforts to explain, predict, or otherwise account for the behaviour of them. Now the 'direct' part of the direct representation hypothesis highlights the fact that the expressions uttered in the context are used by the speaker to say things directly about the systems under investigation in the context. In cases which, for convenience, I call *applied cases*, the systems under investigation are *real systems*, i.e., systems which exist in our universe, have spatiotemporal properties and causally interact with other systems in our universe.[2] In applied cases, then, the direct representation hypothesis implies that the sentences and equations associated with theories and models are often used by scientists to say things directly about the real systems investigated in these contexts. Accordingly theories and models in these cases are linguistic representations of real systems.

The direct representation hypothesis is a substantive empirical hypothesis about how sentences and expressions are used in scientific discursive practices. Since this hypothesis is a central aspect of the linguistic approach, the linguistic approach's tenability largely rests on the hypothesis's plausibility. In chapter 3 I show the plausibility of the direct representation hypothesis by considering a few encouraging examples, and by attempting to forestall some potential objections. I also give some arguments in favour of the direct representation hypothesis that draw upon what I call the *continuity thesis*. This is the thesis that discursive practices across scientific and non-scientific domains are likely to be continuous. I use this thesis to argue for the *prima facie* plausibility of the direct representation hypothesis. The evaluation of the direct representation hypothesis is then taken up again in the second part of the dissertation (see below)

---

[2]Applied cases can be contrasted with, for a lack of a better phrase, *unapplied cases*. The clearest examples of unapplied theorizing and modeling include those where scientists explicitly characterize their theories and models as representations of non-physical, counterfactual or abstract objects *only*. Achinstein (1968, 218–220) offers a few nice examples.

when it is evaluated in a more systematic way against the discursive data.

In chapter 4 I take up the semantic view, and in chapter 5 I take up Cartwright's nonrepresentationalist view of theories. Each of these influential views challenges the direct representation hypothesis in different ways. In these chapters I work to clarify the nature of the disagreement between the three views. As we shall see Cartwright's view challenges the 'representation' part of the direct representation hypothesis. She claims that the law-like statements associated with fundamental theories "should not be viewed as claims about the nature and structure of reality which ought to have a proper propositional expression that is a candidate for truth or falsehood", since "fundamental theory represents nothing and there is nothing for it to represent" (1995, 138–139). On her view, contra the linguistic approach, the expressions associated with fundamental theories are systematically employed by scientists in some *non-representational capacity* (my terminology) in discourse.

The semantic view challenges the 'direct' part of the direct representation thesis. By associating theories and models with classes of abstract structures, the semantic view systematically interposes structures between expressions and the representational targets of theories and models. According to this view it is the mathematical structures that are used by scientists to directly represent the systems under investigation in theorizing and modeling contexts, and the sentences and equations associated with theories are a means only of describing or defining the structures. The difference between the linguistic approach and the semantic view is particularly clear in applied cases where theories and models are used in the context to help explain or predict the behaviour of real systems. In these cases the semantic view offers a multi-stage account of the representation: expressions are used by scientist to say things about mathematical structures and these structures are then used to represent the real systems under investigation. On the semantic view, then, the expressions are used only in an *indirect representational capacity* (again, my terminology). As da Costa and French put the idea, the expressions can be said to "point" to the world "by means of" a mathematical structure (da Costa and French 2003, 17).

Included in my discussion of the semantic view and Cartwright's nonrepresentationalism is a critical evaluation of the main arguments offered in defence of these views by their respective proponents. I shall conclude that their main arguments are not as strong as they might first appear. I also develop a novel line of objection to both these views which draws upon the continuity thesis. The general thrust of this line of reasoning is that both the semantic view and Cartwright's nonrepresentationalism imply a substantial discontinuity in discursive practices across, roughly, scientific and non-scientific domains. By positing this discontinuity a theoretical burden is imposed upon each view which decreases their *prima facie* plausibility.

The first part of the dissertation, then, offers my initial defence of the linguistic approach: it explains what the view is, offers some considerations that speak in favour of it, forestalls some objections against it, and highlights important weaknesses of competing views. However, the first part of the dissertation also sets the stage for the second part. It is the comparison of the linguistic approach, the semantic view and the Cartwright's nonrerpesentationalism which gives rise to the three-way distinction between non-representational, indirect representational and direct representational capacity for expressions. This sets

up the problematic which animates the second part of my dissertation. How can we determine in which of these three capacities expressions are being used in the discursive aspect of theorizing and modeling practices?

1.2.2   Part 2: Philosophy of Science Meets Speech Act Theory

The linguistic approach, the semantic view, and Cartwright's nonrepresentationalism offer three different accounts of what theories and models are, and three corresponding accounts of the function that sentences and equations play in the discursive aspect of theorizing and modeling practices. Again, the linguistic approach suggests that expressions are regularly used by scientists in a direct linguistic representative capacity. The semantic view denies this and instead implies that expressions are systematically used in an indirect representative capacity. Nonrerpesentationalists also deny the direct representation hypothesis by claiming that the expressions associated with fundamental theories are systematically used in a non-representative capacity. This raises a question: how might we determine in which ways expressions are actually used by scientists in a particular context? Here one of this dissertations main animating ideas comes to the fore. Perhaps relevant evidence can be gathered from the discursive contexts themselves in which scientists employ the expressions. But what features of discourse might be relevant as evidence? It is this latter question that the second part of the dissertation tackles directly by drawing upon speech act theory.

Speech act theory has been a going concern in both the philosophy of language and linguistics since Austin first published *How to Do Things With Words* in 1955. As the title of his book suggests, it is a theory of how people do things with words. Words, of course, are used to convey content and represent aspects of the world, but they are also used to perform other communicative and social tasks. Words can be used to greet people, give commands, make promises, christen ships, and more. Speech act theory attempts to integrate all these different functions of expressions into a general account of *speech acts*. Different acts are characterized and classified not only by the syntactic and semantic features of utterances, but also by the relevant features of an utterance's context and its role in discourse. The classification of different speech acts reveals a normative structure underlying discursive practices that enables those wielding expressions to use them to do many different things.

Since the linguistic approach, the semantic view, and Cartwright's nonrepresentationalism all imply that scientists are doing different things by uttering expressions, speech act theory provides a great theoretical framework within which to develop methods to help determine how expressions are used by scientists in their discursive practices. Speech act theorists have developed rich taxonomies of speech acts and offer insight into the normative structures that underlie different kinds of acts within these taxonomies. Drawing upon their taxonomies and analysis in chapters 6 and 7 I work to articulate an evidential basis for discriminating between direct representational, indirect representational and non-representational uses of language. Roughly, to determine if an uttered sentence or equation is being used in one of these three ways, speech act theory is used to identify relevant syntactic features and semantic features of an utterance, relevant relations between utterances in discourse, and relevant features of the utterance's context

which help differentiate these uses of expressions. These differentiae can then be used to help determine how utterances are being used case-by-case.

The task of outlining an evidential basis for discriminating between direct representational, indirect representational and non-representational uses of language is spread across two chapters: in chapter 6 I examine the distinction between representative and non-representative acts, and then in chapter 7 I examine the distinction between direct and indirect linguistic representative acts. The notion of a linguistic representative act (which is used to explicate the notion of a linguistic representation) is characterized in chapter 2 by drawing upon an existent speech-act-theoretic category, which Searle (1976) for example calls *representatives*. However in chapter 6 we look a bit deeper into speech act theory to see how this speech-act-theoretic category tracks salient features of utterances and the normative structure of discourse. By identifying the characteristic features of representatives (i.e., acts of linguistic representation) I show how these features can be used to help distinguish these acts from *non-representatives*. From my discussion of the contrast between representative and non-representative acts a set of questions are developed. These questions form a kind of diagnostic test that can be applied to help determine whether utterances are being used to perform representative or non-representative acts.

The distinction between direct and indirect representation is then taken up in chapter 7. The challenge in distinguishing between acts of direct and indirect representation is identifying features of an utterance's use that help determine the representational target of an act of linguistic representation. The key to doing this is recognizing that the expressed reasoning associated with direct and indirect representation is different in each case, and this has a clear impact on the justificatory and inferential roles that utterances play in discourse. So an evidential basis for distinguishing direct and indirect linguistic representative acts is developed by identifying relevant features of discourse that track different inferential and justificatory roles that expressions play. These features are used to develop another set of diagnostic questions. These questions can be used to test whether or not the discursive data indicates an utterance is used by a scientist in a direct or indirect representative capacity.

The two sets of diagnostic questions can be then used in conjunction. With respect to the first set of questions the linguistic approach and the semantic view stand together in opposition to non-representationalist views. Both the linguistic approach and the semantic view attribute a representational function to linguistic expressions, whereas non-representationalists attribute a non-representational function. So the first test can be used to help distinguish cases where the discursive data better supports a non-representationalist view of theories from those cases where the discursive data better supports the linguistic approach and the semantic view. It is the second set of questions which help differentiate between the linguistic approach and the semantic view. This test helps distinguish those cases where the discursive data indicates expressions are used in a direct representative capacity from cases where it indicates expressions are used in a indirect representative capacity.

In chapters 6 and 7 these diagnostic tests are not just developed, but they are also applied to a case study: the London brother's (1935) work on superconductivity. This illustrative example is chosen because it is extensively discussed both by Cartwright *et al.* (1995, 2008), and proponents of the semantic

view (French and Ladyman 1997), with each camp claiming that the London brothers model of superconductivity is best captured by their respective views. By using the act-theoretic methods developed in these chapters I put their claims to the test. What we shall find, however, is that the discursive data best supports neither non-representationalism nor the semantic view. Instead the discursive evidence best supports the linguistic approach.

1.2.3   Part 3: The Linguistic Approach and General Accounts of Representation

Chapter 8 takes up an alternative interpretation of the semantic view offered by French and his collaborators (French and Ladyman (1999), Bueno *et al.* (2002), da Costa and French (2003), French (2010), and Bueno and French (2011)). On this reinterpretation the semantic view is largely stripped of its ontological commitments. The semantic view is no longer construed as a view about what scientific theories and models are, but rather it is viewed as a way of representing features of scientific practice salient to philosophical investigation. I investigate this proposal and its relation to the linguistic approach. Doing so provides an opportunity to both re-examine the descriptive aims of the linguistic approach and to make connections between linguistic representation and more general theories of representation.

The potential threat posed by French *et al.* to the linguistic approach does not arise as a result of differing empirical claims about scientific practices (as is the case when the linguistic approach confronts more orthodox versions of the semantic view). Rather, as we shall see, their version of the semantic view threatens the motivations underlying the linguistic approach (and any other account with descriptive aims). If, as French *et al.* suggest, the semantic view's model-theoretic machinery could be used to provide a complete understanding of all of the philosophically relevant aspects of scientific practice irrespective of what theories and models actually are, then there would be little point in pursuing a descriptive account like the linguistic approach. Philosophers of science would not need to worry what kinds of representations theories and models are because the semantic view would offer an adequate representation of them regardless. However, we shall find that French *et al.*'s confidence in the representational adequacy of model-theory rests largely on their confidence in a contentious view of representation, namely the view that all scientific representations represent their targets in virtue of structural similarities. I shall argue that their confidence is misplaced. Scientists employ many different modes of representation and it is likely that many of these different modes cannot always be adequately captured by the semantic view's model-theoretic machinery. This is especially true of linguistic (and other highly conventional) modes of representation which (as even French at times seems to acknowledge) prove especially problematic for similarity based accounts of representation. In light of this I conclude that the most methodologically sound approach to the study of representation in the sciences is one built upon a descriptive account of these practices.

1.3   Conclusions

At the heart of the dissertation are a number of examples which I argue provide evidence that supports the linguistic approach's direct representation hypothesis. The most significant of these examples is the London brother's work on superconductivity. By considering in detail the London brother's (1935) work I argue there is clear evidence the equations—both specific to their own account (including the *London equation*), and those associated with the background theory (in this case Maxwell's equations)—are used by the London brothers in their paper to directly represent superconductors. On these grounds I conclude that the London brother's model presented therein is a linguistic representation.

This alone is a modest, but I think illuminating, contribution to the study of scientific theories and models. The London brother's model is a canonical example in which the linguistic approach, the semantic view and Cartwright's nonrepresentationalism all propose competing (and mutually inconsistent) accounts. The fact that this example is best captured by the linguistic approach is a result that has immediate negative consequences for nonrepresentationalism and the semantic view. As a canonical example given detailed consideration by both opposing camps, the fact that the discursive data fails to support the claims made by both nonrepresentationalists and the semantic view cast serious doubt upon the empirical generalizations regarding discursive practices that each of these views entails. In order to maintain its emphasis on mathematical structures, the semantic view must systematically deny that the sentences and equations employed by scientists are used to directly represent systems. Moreover, in order to maintain a special non-representational status for theoretical laws Cartwright's nonrepresentationalism must systematically deny that law statements associated with abstract theory are used in any sort of representative capacity. The London model proves to be an example which is problematic for both generalizations. The challenge posed by this case, of course, is greatly amplified by the fact that this is an example favoured by both camps. Neither the nonrepresentationalist nor the proponent of the semantic view can easily dismiss this example as an anomalous or special case since it is one of their preferred examples to begin with. If expressions associated with both the background theory and the model's specific content are used in a direct representative capacity in the case of the London's work on superconductivity, it is plausible that expressions are similarly used by scientist in other cases too. Thus we should be sceptical about the descriptive adequacy of both the semantic view and the Cartwright's nonrepresentationalism with respect to their ability to account for all, or nearly all, cases of theorizing and modeling in the sciences.

The results of our investigation of the London example, and others cases as well, warrant at minimum the conclusion that philosophers of science should take seriously a more pluralistic view, which includes description-like linguistic representations among the variety of different kinds of scientific theories and models produced by scientists. However I argue a somewhat stronger conclusion is warranted when we augment the discursive evidence adduced from examples with the *prima facie* considerations (presented in chapters 2–5) that speak in favour of the linguistic approach. Roughly, the *prima facie* considerations suggest it would be very surprising if we were to find that scientists do not routinely use expressions in a direct representative capacity. Direct representation is, after all, the norm in non-scientific contexts. Only in light of strong countervailing considerations should we abandon the working hypothesis that

scientists do routinely use expressions in a direct representative capacity, and the requisite countervailing considerations have not materialized. Hence I argue that there are sufficient grounds for thinking the linguistic approach's direct representation hypothesis is likely true. That is, in cases where theorizing and modeling practices have an essential discursive aspect it is likely that the sentences and equations associated with theories and models are regularly used by scientists to say things directly about the system, or systems, under investigation in the context. Thus, in cases where theorizing and modeling has an essential discursive aspect, it is likely that the corresponding theories and models are often linguistic representations.

Let me mention here a few things about these conclusions. First, a comment about their scope: the conclusions that I draw focus on cases with an essential discursive aspect. This is important to stress given the heterogeneity of things that the words 'theory' and 'model' are applied to in the sciences. There is great diversity in the ways that both philosophers and scientists used these terms—especially the term model (e.g., see Frigg and Hartman 2012)—and it would be implausible to suppose that all these uses can be understood by appeal to linguistic representations. For example, some things commonly called models are straightforwardly physical objects with mass and extension. A scale model of an airplane, the original wire model of DNA created by Watson and Crick, and the animal models common in the health sciences are all examples. For reasons that will be elaborated on in the next chapter, it would be implausible to suggest that these kinds of physical models are linguistic representations. So my conclusions are intended only to concern that subclass of theorizing and modeling practices which have an essential discursive aspect. By this I mean, roughly, cases where (1) a scientific theory or model has been presented in a context, and (2) the theory or model is presented as result of, or in course of, presenting a series of equations or sentences in that context.

Despite this scope restriction, we shall find that included within the scope of my investigation are most of the canonical examples considered by nonrepresentationalists and proponents of the semantic view. Again, the linguistic approach holds that theories and models with an essential discursive aspect are regularly direct linguistic representations. Consequently, as illustrated by the examples I consider in this dissertation, the linguistic approach, nonrepresentationalism and the semantic view all offer different accounts of common, or shared, examples of scientific theorizing and modeling. Thus all three are in direct competition with one another.

Second, the main conclusions I draw may seem somewhat cautious. I conclude that the direct representation hypothesis is *likely true* and that the descriptive adequacy of both the Cartwright's nonrepresentationalism and the semantic view is *in doubt*. This caution, however, is a product of the dissertation's evidence-oriented methodological perspective. My investigation is undertaken with a commitment to follow wherever the evidence from scientific practice might lead. This is reflected in my effort to develop act-theoretic methods that can be used to test different accounts against the evidence gathered from the discursive activities of scientists. What makes the discursive practices so interesting is that they really could provide evidence for any one of the views considered. By these lights we can't be sure what we will find until a more comprehensive investigation of the discursive practices associated with theorizing and modeling practices is carried out. What I offer in this work is a necessarily limited inceptive foray into

the investigation of these discursive practices. Despite its limitations this investigation shows the value of systematically considering the discursive data, and it offers novel arguments and evidence in favour of the linguistic approach's direct representation hypothesis.

The dissertation's methodology also respects the diversity which can be observed in scientific theorizing and modeling practices—a diversity that may require understanding theories and models to be a very heterogeneous collection of things. As we shall see, the act-theoretic framework that I draw upon builds in the possibility of a high level of context sensitivity whereby the same expression can be used in radically different ways from context to context, and even from utterance to utterance. Thus it is possible that a sentence or equation (*vis* expression type) can sometimes be used in a direct representative capacity, sometimes be used in an indirect representative capacity and at still other times be used in a non-representational capacity. Whether scientists predominantly use particular expressions in one way or another is something that only careful observation of scientific practice can determine. Nevertheless I argue, at least *prima facie*, we should expect to find that sciences regularly use expressions in a direct representative capacity and I show that the discursive evidence gathered from a few examples corroborates this. Consequently we have good, but ultimately empirically defeasible, reasons to accept that the direct representation hypothesis is likely true and, thus, many theories and models are linguistic representations.

Chapter 2

Introducing the Linguistic Approach

In this chapter I provide a more detailed introduction to the linguistic approach. This will be accomplished first by explaining the linguistic approach's main tenet, the direct representation hypothesis. To aid in this I shall draw upon a simple example, the modeling of a pendulum bob's motion. Then I shall compare the linguistic approach to a complementary view offered by Achinstein's (1968) more than forty years ago. This comparison reveals the linguistic approach and Achinstein's view have different focuses, which require each to draw upon different philosophical resources.

2.1   Simple Pendulum Model

Models of pendulums serve as a touchstone for many authors who discuss modeling in science (e.g., Cartwright (1989), Giere (1988) and Thomson-Jones (2010)). They are also examples found in virtually every introductory mechanics textbook and no doubt familiar to most who have taken an introductory physics laboratory class. In one of my own undergraduate laboratory sections we investigated harmonic motion by studying a special copper pendulum system. In this experiment a copper pendulum swung over a metal plate. Both the pendulum and the plate were hooked up to a power source that supplied an electrical impulse at a set frequency. As the pendulum swung over the plate an electrical arch would jump between the pendulum bob and the plate with each impulse. A thin piece of paper was placed on top of the metal plate so that each electrical arch would burn a small hole in the paper. This left a series of burn holes recording the bob's location over the plate as it moved through its arch. The class was asked to make a prediction about the period of the pendulum's motion by modeling it as a simple pendulum. This prediction was then tested against the period of the pendulum recorded by the series of burn holes on the paper.

   As instructed we modeled the pendulum system in the usual way (for similar examples see Benson (1995, 310–311), Bueche (1982, 339–340), and Giancoli (1985, 301–302)) by writing down a series of

equations, beginning with Newton's law,

$$\vec{F} = m\vec{a}. \tag{2.1}$$

This was simplified by considering only the component of the force acting along the tangent of the arch x through which the bob moved:

$$F_x = m\frac{d^2x}{dt^2}. \tag{2.2}$$

Treating the bob as a point mass hanging from a massless ridged rod of length L that is free to swing through angle θ without any loss of energy (due to friction or air resistance), we wrote down the following force function for F_x:

$$F_x = -mg\sin\theta. \tag{2.3}$$

The small angle approximation was then applied to the above equation to simplify it. If the pendulum swings through a small angle ($\theta \ll 1$), then $\sin\theta \approx \theta$. Applying this approximation to 2.3 yielded

$$F_x = -mg\theta. \tag{2.4}$$

The length of the arch x is equal to $L\theta$. So $d^2x/dt^2 = Ld^2\theta/dt^2$ and equations 2.2 and 2.4 can be rewritten in the following way:

$$\frac{d^2\theta}{dt^2} + \frac{g}{L}\theta = 0. \tag{2.5}$$

This second-order differential equation describes harmonic motion. The general form of the differential equation's solution is:

$$\theta(t) = A\cos(\sqrt{g/L}t - \theta_o), \tag{2.6}$$

where A is the amplitude, and θ_0 is the initial angle of displacement. Equation 2.6 describes a periodic relation between θ and t with an angular frequency $\omega = \sqrt{g/L}$. Since the period T is $2\pi/\omega$, equation 2.6 was used to arrive at the following:

$$T = 2\pi\sqrt{L/g}. \tag{2.7}$$

To finally make a prediction about the period of the copper pendulum, all that was left to do was measure the length of the rod supporting the pendulum bob (L), look up the value of the gravitational constant (g), and plug these numbers into the above equation.

This is a simple example of what I call an applied case of modeling. In applied cases theories and models are used to investigate real systems, i.e., systems which, roughly, have spatiotemporal properties and causally interact with other systems in our universe. The system under investigation in this example is the real copper pendulum system that sat on the laboratory bench in front of me and it was the period of its pendulum that the model was used to predict.

It is also a clear example of a modeling that has an essential discursive aspect. Roughly put, part of what my (and my classmates) modeling consisted of was the writing down of a series of equations. It was by the utterance of these equations that I could be said to have presented a simple pendulum model and

used it to make a prediction about the copper pendulum's period of motion. While modeling I wrote down a series of equations, 2.1–2.7. Writing these equations was an essential part of modeling in this case and was principally what I did by engaging in this modeling activity. We could not, in some sense, remove the equations presented from the context and nevertheless think that a model had been presented and used.[1]

The linguistic approach hypothesizes that cases like this one regularly involve something akin to describing. More precisely, the linguistic approach hypothesizes that in cases, like the above, where theorizing and modeling has an essential discursive aspect, the sentences and equations expressed by scientists are regularly used in a direct linguistic representative capacity. In virtue of functioning in this capacity the models and theories produced by these practices are direct linguistic representations. This hypothesis, the direct representation hypothesis, is at the core of the linguistic approach.

I shall now explain the direct representation hypothesis and its underlying analysis in a series of steps, all the while drawing upon the simple pendulum model above to anchor the discussion in a well known example. First l will say more about representation (a subject which we will return to in chapter 8). Drawing upon the work of van Fraassen, Suárez and other philosophers of science, I will explain in what sense descriptions, and other description-like entities, are representations. Second, I shall provide a characterization of linguistic representation. This characterization draws upon speech act theory to distinguish between those cases in which expressions are used in representative and non-representative capacities. Third, I shall clarify the distinction between direct and indirect linguistic representation. This will make clear what, in my terminology, it means to claim that sentences and equations expressed by scientists are regularly used in a direct linguistic representative capacity.

2.1.1 Representation

Recall that the linguistic approach is meant to capture the general idea that theories and models are often description-like representations. But the notion of a description-like representation stands in need of some clarification. I propose to do this by introducing the notion of a linguistic representation and explicating this notion using speech-act-theoretic concepts. To both get a handle on linguistic representation and see the value of using speech act theory for its explication it will be helpful to consider descriptions, which I take to be paradigmatic linguistic representations.

Suppose the witness of a convenience store robbery provides the police with the following description of the robber:

> The robber is more than six feet tall. He is heavyset, likely weighing over 200lbs. He has short light brown hair and "mom" tattooed on his right forearm. He also wore black pants,

---

[1]In contrast suppose some aerospace engineer removes some scale model airplane from a cabinet and proceeds to describe in detail features of the model and the calculations that went into its design. In this case it is plausible to suppose that the engineer has presented a model simply by removing the scale replica from the cabinet even if this act had not been accompanied by a length description. The verbal tokening of expressions in this case may not be an essential part of modeling.

white running shoes, and a camouflage-patterned Duck Dynasty T-shirt. The robber seemed very nervous because his hand holding the knife shook and his voice quivered when he talked.

In describing the robber in the above way the witness has used English expressions to say various things about the robber. By saying these things about the robber I claim the witness has represented the robber as having the features attributed to him in the description. But what kind of representation might this be? Descriptions like this are examples of what Goodman (1976), Hughes (1997), and van Fraassen (2008) call *representations-as*. Representation-as involves representing some designated thing as being some way or other. In the witness's case their description represents the robber *as* being more than six feet tall, being heavy set, having short brown hair, etc. Representation-as is contrasted with mere *representation-of*. Suppose I hold up a pen and arbitrarily declare it to be a representation of, say, my home town. In some sense the pen may be taken as a representation of my home town insofar as it is said to designate, stand for, or refer to it. However this does not ensure that the pen counts as a representation-as, since it does not necessarily represent the town as being any way in particular. The pen *qua* representation is one that conveys virtually no information about its target.

As van Fraassen (2008, 16) observes we can characterize representations-as as fitting the schema X represents Y as F. Many different kinds of representations fit this schema. Pictures, maps, scale models, and graphs can all represent their subjects as being some way or other. A photograph may represent two people as fishing in a river. A scale model airplane represents an aircraft as having a certain shape, and a subway map may represent Dufferin station as being between Lansdowne and Ossington stations along the Bloor-Danforth line. This characterization of representation-as dovetails with other characteristics commonly associated with representation. Representations are often thought to have an 'aboutness,' i.e., for X to be a representation it must be about something. Representations are also typically characterized as being contentful or information bearing. Descriptions have both an aboutness and convey content. In the above example, the description is about the robber, and it conveys information about the robber's height, weight, dress and physical state.

Another common view is that representations are characteristically a means or a basis for reasoning, especially reasoning concerning the representation's target. This functional aspect of representation is emphasized in Suárez's (2004) inferential conception of scientific representation. According to Suárez "A represents B only if (i) the representational force of A points towards B, and (ii) A allows competent and informed agents to draw specific inferences regarding B" (2004, 773). For Suárez representational force is a relational and contextual property of a representation that bestows the "capacity of a source [i.e., potential representation] to lead a competent and informed user to a consideration of the target" (2004, 768). Roughly then, the first condition requires representations to be the sorts of things that have distinguishable targets. The second condition guarantees that a representation has the capacity to allow, what Suárez calls, surrogate reasoning. When a representation is functioning as a kind of surrogate, even if one is unable to observe and manipulate the target itself, one can nevertheless use the representation as a basis for reasoning and drawing inferences about the target. Moreover Suárez highlights that the second condition "distinguishes cognitive and scientific representation on one hand from mere denotation

on the other" (2004, 774). This distinction he draws nicely parallels that between representation-of and representation-as. A cognitive representation, in Suárez's sense, is not only one that signifies, denotes, or otherwise points to its target, but it allows its user to infer that the target is some way or other. In this way I take representation-as and what Suárez calls cognitive representation to be complementary notions.

As Suárez intends, pictures, maps, scale models, graphs and a great many other kinds of representations meet his two conditions. For example, in the hands of a competent map user a Toronto subway map is a representation that will lead a competent and informed user to a consideration of the Toronto subway system and its topological features. Moreover, because the map has this representational force, it can be used to draw many inferences about the Toronto subway system. For example one may infer that having passed both the Lansdowne and Dufferin platforms the next stop will be at Ossington station. Descriptions also meet both conditions. A description always has a subject which the description points to, and a description attributes things to the subject and these attributes provide a basis from which inferences about the subject can be drawn. It will be clear to an informed and competent user that the intended target of the description is the person the witness observed committing the robbery and based on their description we are able to infer a great number of things about the robber.

Moving forward it will be helpful to regiment our terminology somewhat. Henceforth I will use 'representation' in a fairly general way to refer to those things that are both representations-as and what Suárez calls cognitive representations. So in my prefered sense representations can be accurately characterized as fitting van Fraassen's schema, X represents Y as F, and this normally means a representation X is a vehicle that conveys content or information about its target Y. Representations also meet the two conditions specified by Suárez. A representation X points to its target Y and must also facilitate surrogate reasoning about Y by allowing competent and informed agents to draw specific inferences regarding Y. Note that representation in this sense is not a success term. In classifying something as a representation we are not thereby committed to it being a particularly good representation. On my usage X can represent Y as F even if, as it turns out, Y is not F. This ensures that misrepresentation can indeed be a species of representation and it leaves open the possibility that representations can fail to be accurate, true, complete or otherwise ideal.

2.1.2   Linguistic Representation

The goal now is to characterize linguistic representation. As we have seen descriptions are representations in the above specified sense. In fact I take linguistic representations to be a kind or species of representation in this sense. Descriptions are paradigmatic linguistic representations, but what else should we count as clear examples of linguistic representation? In thinking about this question it is helpful to connect linguistic representation with the act of using expressions to *say things about* a target. As noted above, when describing one uses expressions to say things about the description's subject, but there are other similar acts of speech by which one says something about some subject.

Assertions are another clear example. If the witness asserts "the robber is more than six feet tall," then

they have used an English expression to say something about the robber, namely that they are more than six feet tall. This assertion is clearly a representation. This assertion (X) represents the robber (Y) as being more than six feet tall (F), and it straightforwardly conveys information concerning the robber. The assertion is a representation that has the capacity to lead a competent and informed user to a consideration of the target (the robber) and hence has a representational force which points to the target. Moreover, the assertions allows competent and informed agents to draw specific inferences regarding the target. For example we can infer from this representation that the robber would need to stoop or bend over if they were to walk through a passageway less than six feet in height. A prediction is similar to an assertion. When an uttered expression is used as a prediction the expression is used to say something about a future (or unobserved) event. Hypotheses also attribute things to a subject. Given the robber's nervous behaviour and his failure to cover up distinguishing features (like his tattoo), the witness might hypothesize that the robber did not have much experience robbing convenience stores. Here the witness represents the robber as being inexperienced. Asserting that the robber was inexperienced and hypothesizing that the robber was inexperienced are clearly different, but the difference is not that one represents and the other does not. Rather the main difference is the confidence or strength of conviction that the speaker expresses towards the representation. So a hypothesis is as much a representation as descriptions, assertions and predictions are, but it is one that the speaker more cautiously endorses.

There are many different acts in which expressions are used to say something about a target, and many more types of speech acts that are not. This fact has long been recognized by philosophers and linguists that study speech acts and is reflected in the taxonomies they offer. For example Searle (1976) identifies a class of speech acts he calls *representatives*, and Bach *et al.* (1979) and Recanati (1987) have similar categories that (adopting Austin's terminology) they call *constatives*. In each case asserting, hypothesizing, predicting, conjecturing, describing, and other acts are grouped together as representatives/constatives. In explicating the class of linguistic representations, then, I propose to draw upon speech-act-theoretic taxa and the considerations used to distinguish them. Here is the basic idea: when expressions are used to assert, describe, predict, hypothesize, etc., then they are used in a representative capacity and in virtue of being used in this capacity the uttered expressions count as linguistic representations. So to determine if a particular set of uttered expressions (or utterances) count as a linguistic representation we must determine how they are used in the context. If they are used to perform one of the relevant speech acts, then they are used in a representative capacity and they count as linguistic representations. Moreover, using speech act theory we can provide a deeper understanding of the speech acts associated with linguistic representation, and identify features of utterances indicative of linguistic representation.

Speech act theories provide a systematic account of the various things that people use language for. Expressions can be used to give commands, make promises, bet, scold, christen ships and more. Obviously not all uses of expressions involve representation. I may greet you by saying 'hello,' but by uttering this word I have not conveyed any information about some particular target, nor have I provided the means to draw specific inferences about any particular subject. By using speech act theory we can distinguish between those uses of expressions that do involve representation and those that do not. I will follow Searle's terminology and call assertions, descriptions, hypotheses, etc. representatives. Representatives then are

the class of speech acts I shall associate linguistic representation with. For our purposes it will suffice to group all other speech acts together into the broad category of *non-representatives*. If one or more expressions are used to perform non-representative speech acts, then they are not used in a representative capacity and in virtue of being used in this way they are not linguistic representations.

To characterize linguistic representation, then, we must characterize representatives and contrast them with non-representatives. In chapters 6 and 7 I will delve much more deeply into speech act theory in an effort to determine which features of discourse count as evidence that expressions are, or are not, being used in a representative capacity. Speech act theoretic taxonomies have been built upon a framework that captures important aspects of discursive practices. Acts of speech occur within discourse which has an implicit normative structure. It is against this background structure that utterances acquire their communicative significance and speakers acquire the ability to do drastically different things by uttering words—including, but not limited to, representing the world. In this section I shall simply use existing speech act theory taxa to provide a clear characterization of representatives. However in later chapters, when speech act theory taxa are connected to the underlying normative structure of discourse, we shall really see the advantages of explicating linguistic representation act-theoretically.

When classifying speech acts as one type or another, many different features of an utterance can be drawn upon. Searle (1976), for example, develops his taxonomy around twelve features. For our purposes the resources of a full taxonomy of speech acts are unnecessary. We are, after all, attempting to capture only the very broad distinction between representatives and its contrast class, non-representatives. For this purpose it will suffice to focus on four features: *propositional content*, *direction of fit*, *illocutionary purpose*, and the *strength of commitment*. The latter three correspond directly with three dimensions of Searle's taxonomy (1976). The first, proposition content, is a feature emphasized in Recanati's (1987) taxonomy (though, arguably, it is a criterion implicit in Searle's taxonomy). The first three will enable us to contrast representatives with non-representatives, and the fourth can be used to capture important differences among speech acts within the class of representatives.

Propositional content. To perform some speech acts the speaker must convey a proposition (or a set propositions), while the performance of other speech acts does not require the conveyance of a proposition. To put it in another way, some speech acts must convey some truth-apt content and others need not. Recanati puts the same point yet another way. As he says, some acts have a "referential" dimension. Speech acts with a referential dimension have "a representational content by virtue of which [these speech acts] relate to a state of affairs" (1987, 156). Representatives, along with many other acts, like giving a command, making a promise or making a threat, are all acts that have a referential dimension and convey propositional content. In contrast, as mentioned above, greetings are an example of an act that does not have a referential dimension.

Direction of fit. Acts that have a referential dimension can be divided into two classes, those with a *word-to-world* direction of fit and those with a *world-to-word* direction of fit (*c.f.*, Searle 1969). The difference between the two is clearly illustrated when we contrast assertions and commands. The assertion "The lights are off in the lounge" and the command "Turn the lights in the lounge off" have the same

propositional content in that each specifies the same state of affairs, i.e., the lights being off in the lounge. However when asserting that the lights are off, the specified state of affairs is purported to be the actual state of affairs. In this case the contents of the utterance are intended to be taken as a representation of the lounge and its lights. Assertions, along with all other representatives, have a word-to-world direction of fit in that the uttered words are intended to fit the world. A command, by contrast, specifies a state of affairs that the speaker would like to bring about by their utterance. In this case the world is to be made to fit the uttered words. Requests, vows, promises, and other non-representatives have a world-to-word direction of fit.

Illocutionary purpose. Speech acts can be said to have a characteristic purpose. Now there may be as many different characteristic purposes for speech acts as there are distinct act types. However, some general things can be said about the purpose of an act in relation to its direction of fit. For all acts (like commands) with a world-to-word direction of fit at least part of the characteristic purpose of these acts is to get the world to conform with a specified state of affairs. For all acts which have a word-to-world direction of fit (like assertions) part of the purpose of these acts is for the uttered proposition to accurately represent the world. Now, a proposition (accurately or perfectly) represents its target just in case it is true—'true' is the word we use to distinguish propositions that succeed as representations of their subjects. In a general sense, the truth is a characteristic aim of all representatives. This dovetails with the idea that representatives are a means of conveying information and, as such, all representative acts are successful at conveying information only to the extent that the contents of the utterance are true. Now, since truth is a characteristic aim of representatives, it is appropriate to evaluate these acts with respect to their achievement of this aim. As Searle puts it, "All of the members of the representative class are assessable on the dimension of assessment which includes true and false" (1976, 10). An assertion, hypothesis, or even a guess are all, in a sense, deficient if they are false. The same cannot be said about a command. If I command you to turn the lights off in the lounge this command is not deficient if at the time of the utterance the lights are on in the lounge. The fact that the lights are on is typically part of the motivation for issuing the command in the first place. So it is not characteristically part of the aim of a command that the propositional content of the utterance accurately represents the world.

Strength of Commitment. The above features help distinguish representatives from non-representatives. It is this last feature adopted from Searle's taxonomy which is especially important when making distinctions within the category of representatives. When we compare assertions, hypotheses and guesses we see that all aim at truth, but each is a means of expressing a different doxastic attitude, or strength of commitment, with respect to the content conveyed. By asserting the speaker is taken as fully endorsing the proposition expressed. Hypotheses, on the other hand, express a weaker commitment. Roughly, by hypothesizing that P the speaker is taken as putting forward P as likely to be true. This weaker commitment is also reflected in weaker discursive commitments associated with this act. I will have much more to say about discursive commitments in §6.1.3, but here is the general idea. When a hypothesis is challenged, the speaker is obliged to give some reasons for P, but only strong enough reasons to justify the belief that P is likely. One may not know that P is true, but hypothesize that it is true without thereby abusing the norms governing hypothesizing. The weaker commitment also modifies the role that P can play as a

premise in further reasoning. If P is only hypothesized to be true and P entails some other claim Q, then P only warrants a correspondingly weak commitment to Q. The idea, then, is that there is a gradation of commitments that one can make regarding the propositional content expressed by a representative act, and these gradations reflect distinct speech acts within the category of representatives. Assertion is at the stronger end of the commitment dimension and guessing is at the weakest extreme of the commitment dimension. To guess is to put forward some claim as true but to do so with almost no commitment. One can felicitously guess that P is true without being able to offer much in the way of reasons for P when challenged.[2]

To summarize, representatives have the following features:

1. Representatives are truth-apt and express propositional contents.

2. Representatives have a word-to-world direction of fit.

3. Representatives are acts which characteristically aim at truth, and thus are assessable on the dimension of assessment which includes true and false.

4. Representatives involve epistemic commitments of varying strengths:

    (a) Representatives are a means of expressing doxastic attitudes of differing strength towards the content expressed, and

    (b) Representatives are acts by which the speaker makes discursive commitments of differing strengths.

The first of these features is a precondition of the latter three. The utterance must be contentful before it can have a direction of fit. Moreover it must be truth-apt either for it to be assessable with respect to its truth or falsity, or for it to be the object of doxastic attitudes. Features 2–4 work in conjunction to pick out speech acts that not only convey some propositional contents—many non-representatives do this as well—but acts for which these contentful representations are used as representations. When a representative speech act is performed the speaker presents contents as world-directed and makes an epistemic commitment (of some strength) to the veracity of the content. This ensures that the contents expressed are intended to convey information *about* some target, that the utterance represents the target as being some way or other, and thus that the contents form the basis for competent and informed users to draw inferences about the target.

We can now use this characterization of representatives to provide a characterization of (simple) linguistic representation. A tokened expression (i.e., an utterance) counts as linguistic representation only

---

[2]Guesses might be better thought of as a degenerate kind of representative. Guesses clearly have a word-to-world direction of fit. Also truth is a characteristic aim of guessing; when your guess is false your guess is a bad one. In this way guesses are unlike pure hypotheticals which express propositions without making any commitment. However, guessing is similar to a hypothetical in that one can felicitously guess or hypothesize without being in a position to offer a reasoned defence of the utterance.

when it is used in a representative capacity, and it is used in a representative capacity whenever it is used to perform a representative speech act. So an utterance counts as linguistic representation when the speaker uses the utterance in such a way that it is truth-apt, has a word-to-world direction of fit, is assessable on the dimension of assessment which includes true and false, and the speaker takes on an epistemic commitment (of some strength) to the expressed contents. We can express this more concisely by employing the right notion of *endorsement*:

> An utterance counts as a linguistic representation when the speaker both uses the utterance to expresses some truth-apt propositional content, and endorses (to some degree) the expressed content.

To endorse an utterance's contents in the relevant sense here, the speaker must endorse the content as being assessable with respect to truth and falsity, and this endorsement must involve both expressing some doxastic attitude towards the expressed content and taking on discursive commitments concerning it. If an expression is used to perform some speech act, but it is not used to express some truth-apt propositional content or it is not endorsed by the speaker, then the expression is used to perform a non-representative speech act. An utterance that is used to perform some non-representative speech act is not used in a representative capacity and thus cannot be counted as a linguistic representation.

Now the characterization I have just provided concerns only simple linguistic representations that are presented by a single utterance. However linguistic representations can be more complex and may be presented via a series of utterances. The above witness's description of the robber is one such example. We can easily extend our characterization to capture descriptions by recognizing that descriptions are comprised of a series of assertions. The content of a description is simply the content expressed by the conjunction of the assertions which comprise it. Moreover this content is endorsed by the speaker in virtue of the speaker endorsing the content of each of the assertions that comprise it. However descriptions are not the only kind of complex linguistic representation possible. Any series of representative acts can form a complex linguistic representation. The content of the whole representation is again just the content expressed by the conjunction of the utterances which comprise it. The series of representative acts, however, needn't all be of the same type. Some utterances in the series could be assertions, while others could be hypotheses, conjectures, predictions, etc. These *mixed-type* linguistic representations can be used to express complex epistemic commitments (this is something I will consider in greater detail in §3.2). While the speaker may make a strong commitment to some of the content by outright asserting it, they may make weaker commitments to other portions of the representation's content by hypothesizing, conjecturing, etc.

Consider again the pendulum model introduced at the being of this chapter. We can now say more precisely what it would mean for this model to be a linguistic representation. Again this model has an essential discursive aspect. In the course of modeling the pendulum system a series of equations, 2.1–2.7, were uttered and uttering these expressions was an essential aspect of modeling in this case. Now to claim that a linguistic representation was presented is to claim that one or more of these uttered equations was

23

used in a representative capacity. This, again, requires that one or more of the uttered equations were used to express some truth-apt propositional content and this content was endorsed by the speaker. Now it's likely that "the model" is meant to refer to a complex linguistic representation expressed by a number of different utterances. Here the mostly likely reference is the complex linguistic representation presented via equations 2.1–2.3 collectively.[3] To claim that these uttered equations form a linguistic representation is to claim that a representative speech act was performed by uttering each equation and these individual linguistic representations together form a complex linguistic representation. The representational content of the model, then, would be the conjunction of the propositional contents expressed by each of these equations.

2.1.3   Direct Linguistic Representation

Not only does the linguistic approach hypothesize that the sentences and equations employed in theorizing and modeling practices are regularly used to perform acts of linguistic representation, but more specifically that they are regularly used to perform acts of direct linguistic representation.

The distinction between direct and indirect linguistic representation was briefly introduced in the previous chapter. As you will recall it is a distinction that concerns the targets of linguistic representations. In drawing this distinction we note that there is typically a primary system, or class of systems, that are under investigation in a given representational context. In theorizing and modeling contexts these are the putative objects of investigation that theories and models are deployed to help investigate. This usually means theories and models are deployed as part of a scientist's efforts to explain, predict, or otherwise account for the behaviour of them. In applied cases the systems under investigation are real systems, which have spatiotemporal properties and causally interact with other systems in our universe. Recall that our pendulum modeling example is an example of applied modeling. The primary system under investigation in this case was a real copper pendulum that swung above a metal plate; the very same pendulum system that at the time sat on the laboratory bench in front of me, and the same pendulum whose period the model was used to predict.

Now in my terminology for an act of linguistic representation to count as an act of direct linguistic representation the uttered expression must be used to directly target the primary system(s) under investigation in the context. That is, if a linguistic representation is a direct linguistic representation, then it is a representation directly of the primary system(s) investigated in the context. So to claim that the uttered equations employed in the pendulum model example are direct linguistic representations is to claim that the equations are used in this context to directly represent the real copper pendulum system that was under investigation. For example if my original utterance of equation 2.7 was in fact an act of direct linguistic representation, then the target of the representation was the copper pendulum system itself. By my utterance of equation 2.7 I represented the pendulum system on the laboratory bench in front of me as having

---

[3]It is natural to exclude the expressions derived using the small angle approximation from the model because equations 2.3 and 2.4 are inconsistent. Equations 2.1, 2.2, 2.4–2.7 be construed as expressing a distinct representation—one which approximates the first.

a period of motion equal to the square root of the length of the pendulum rod divided by the gravitational constant.

In contrast, indirect representation is a kind of multi-stage, or mediated, mode of representation. Indirect linguistic representation always involves mediating objects interposed between a linguistic representation and the systems under investigation in the context. When linguistic representation is indirect the expressions employed in the context are not directly representations of the primary systems under investigation. Rather they are used to represent some mediating objects which in turn may be used to represent the primary system under investigation. As we shall further discuss in chapter 4, according to the semantic view structures (*qua* abstract mathematical entities) are interposed between linguistic representations and the real systems investigated by scientists. On this account the representational targets of the expressions employed by scientists in their theorizing and modeling practices are structures and its the structures, not the expressions, which are used to directly represent real systems. In this case sentences and equations are indirect linguistic representations, which only represent the real systems under investigation in applied modeling and theorizing practices indirectly via the mediating structures. If we apply the semantic view's analysis to the pendulum model example, then equations 2.1–2.3 are indirect linguistic representative acts. They are not used to represent the real copper system directly. Instead they represent a structure that in turn is used to represent the copper pendulum system.

2.1.4 The Direct Representation Hypothesis

We have now outlined what, according to the linguistic approach, it is for sentences and equations to be direct linguistic representations. However the linguistic approach offers more than just this analysis. The linguistic approach also makes an empirical hypothesis about scientific theorizing and modeling practices. It hypothesizes that in cases where theorizing and modeling has an essential discursive aspect the sentences and equations associated with theories and models are often (or regularly) used by scientists in a direct linguistic representational capacity. If this hypothesis is correct, then in those cases where theorizing and modeling practices have an essential discursive aspect the theories and models presented will often be linguistic representations that directly target the systems under investigation. Given our characterization of linguistic representation, this hypothesis is a proposal about how sentences and equations are used by scientists in the discursive aspect of their theorizing and modeling practices. This makes it a descriptive, or empirical, hypothesis about scientific practices.

The direct representation hypothesis shies away from the strong view that all sentences and equations employed by scientists in modeling and theorizing practices are used in a representative capacity. Instead the direct representation hypothesis takes it to be a regular or routine occurrence. Nevertheless, as noted in the previous chapter, I take the linguistic approach to be in direct competition with the semantic view and Cartwright's nonrepresentationalism. Each of these views offers competing accounts of canonical examples of theorizing and modeling. If it were discovered that expressions are rarely used in a direct linguistic representational capacity by scientists, then the direct representation hypothesis would be dis-confirmed.

But more than this, if it were discovered that few canonical examples of theorizing and modeling were best captured by the linguistic approach, then this result would also seriously undermine the view. By illustrating above how the modeling of a pendulum might be understood in terms of direct linguistic representation I have illustrated how at least one canonical example can be accounted for by the linguistic approach. In chapters 6 and 7 I shall consider a more sophisticated example, i.e., the London brother's model of superconductivity. This is an example that has been extensively discussed both by nonrepresentationalists and proponents of the semantic view, with each party offering conflicting accounts of it. The linguistic approach offers yet another way of accounting of this example that is inconsistent with these others. I shall argue the linguistic approach's account of this canonical example is the best of the lot.

2.2   Achinstein and the Linguistic Approach

As I mentioned in the introductory chapter, views like the linguistic approach, which propose that theories and models are linguistic representations, are not well represented in the current philosophy of science literature. To my knowledge one must look back to the work of Achinstein (1968) to find a well-developed treatment of theories and models that is in-line with the linguistic approach. This might tempt one to suppose that views like the linguistic approach have been subject to devastating criticism and abandoned. However this is not the case. As Frigg and Hartmann observe, such views have "not been subject to explicit criticism" (2012) (with the exception of Frigg and Hartmann's own brief criticisms, which I shall consider in §3.3).

There are likely a number of reasons why linguistic views haven't been subject to more development and scrutiny. Contessa (2010) notes that questions concerning the ontology and epistemology of theories and models have received surprisingly little attention. Part of the reason, then, may simply be that philosophers haven't said much about linguistic accounts because they haven't said a great deal about any views concerning the ontology of theories and models. One obvious exception here is the semantic view. A great deal has been written about it and, though this is rarely a point emphasized by its proponents, most advocates of the semantic view claim that it does tell us something about what theories and model are (da Costa, French and Ladyman are notable exceptions in this regard, see chapter 8). Perhaps another part of the explanation, then, is that people are generally content with the account provided by the now dominant semantic view. One more likely ingredient is a post-positivist backlash against anything perceived to be overly focused on language. Van Fraassen expresses this kind of sentiment when he says that "The main lesson of twentieth-century philosophy of science may well be this: no concept which is essentially language-dependent has any philosophical importance at all" (van Fraassen 1980, 56). Views like the linguistic approach may strike people as too language-oriented for serious consideration. However, I shall argue in §3.3.1 that the linguistic approach's emphasis of language is quite different from that of the logical empiricist's *syntactic view* and that this emphasis is not a weakness.

Despite undertaking a different project and employing different methods, Achinstein arrives at an analysis of theories and models which is quite complimentary to that of the linguistic approach. Achinstein

undertakes a general survey and analysis of the concepts important to understanding science. In addition to the concepts of a theory and a model, he also develops a conceptual analysis of analogy, observational terms, theoretical terms and the semantic (or model-theoretic) notion of a model. Rather than striving to provide necessary and sufficient conditions for each concept, he employs an interesting alternative approach which emphasizes relevance. Concepts are analysed with respect to a term's uses. Roughly its uses are analysed to yield a set relevant properties or conditions. On Achinstein's view a condition is relevant to concept X if knowing that an item meets the condition is reason to classify the item as an X (positive relevance) or it is reason not to classify the item as an X (negative relevance) (1968, 48-50). Relevant conditions may not be necessary or sufficient. This kind of conceptual analysis results in dictionary-style definitions for concepts. Roughly each entry for a term's dictionary-style definition specifies a relevant condition or describes relevant properties of entities to which a concept applies.

When Achinstein applies his method of analysis to scientific theories he identifies six conditions for having a theory (1968, 122-128). These conditions are then used to characterize what a theory is. He provides the following summary of his findings:

> T is a theory, relative to the context, if and only if T is a set of propositions that (depending on the context) is (was, might have been, and so forth) not known to be true or to be false but believed to be somewhat plausible, potentially explanatory, relatively fundamental, and somewhat integrated.[4] (1968, 129)

The commonalities between this characterization and the linguistic approach's sketch above are striking. For one, Achinstein identifies theories with sets of propositions. This implies that theories are language-like in the sense that they are truth-apt and they can properly be the objects of doxastic attitudes. Second, his characterization imposes an epistemic criterion on theories. On his view, roughly, there must be some past, present, or future person who does not know the theory's propositions to be true or know them to be false, but believes they are likely to be true. These two features parallel the features of linguistic representative acts I introduced above. A linguistic representative act requires both that a speaker expresses some truth-apt propositional content and by their utterance the speaker thereby endorses the expressed content. These parallels guarantee that whenever a theory in Achinstein's sense is presented by a scientist, then a series of linguistic acts will have been performed and these acts will produce a (usually complex) linguistic representation. In presenting a theory the speaker must present a series of propositions and they must endorse these propositions as plausible.

Achinstein's characterization of theories associates scientific theories with only a small subset of the broader class of linguistic representations. It is, for example, possible to present a linguistic representation that is known to be true, or one that is known to be false. I do harbour some reservations about the specifics of Achinstein's epistemic criterion on theories. It seems plausible to me that there are some linguistic representations that are both correctly classified as theories and are known to be true or known to be false.

---

[4]The context dependence that Achinstein mentions here arises as a result of the requirement that someone have the appropriate epistemic relation to a putative theory, namely that someone neither knows it neither to be true nor do they know it to be false.

Nevertheless such a disagreement with Achinstein is a minor one. It concerns the finer details of which linguistic representations count as theories. What is important is that Achinstein's analysis clearly places theories within the class of linguistic representations.

Achinstein's analysis of models in science is likewise complementary to the linguistic approach. Both Achinstein's view and the linguistic approach recognize that "the items called models in the sciences are a varied lot" (1968, 209). In light of this variation he draws a distinction between three broad types of models, which he calls *representational models*, *theoretical models* and *imaginary models*. Just as the linguistic approach does not insist that all things called 'models' are linguistic representations, Achinstein similarly does not misguidedly attempt to identify all models with sets of propositions. For example, representational models on his view are three dimensional physical objects, like a scale replica of an airplane, which scientists and engineers construct to represent some target system (Achinstein 1968, 209–211). However Achinstein, like me, maintains that many models in the sciences are description-like. He classifies the billiard ball model of gases, the Bohr model of the Atom, and the free-electron model of a metal as theoretical models. Theoretical models are identified with sets of assumptions and can thus be used to serve the same functions in scientific practice as theories: "they can be used for explanation, systematization, interpretation, prediction, calculations, derivation of laws, and the like" (1968, 218). Achinstein likewise identifies imaginary models with sets of propositions. The difference is that these propositions are not used to represent some real system, but instead some imaginary system.[5] One example he uses to illustrate the idea is the Poincaré model of a Lobachevskian non-Euclidean world. In this model Poincaré assumes, among other things, that the world is spherical, the temperature is greatest at the centre, and it decreases towards the circumference. Achinstein observes that Poincaré did not take himself to be representing the real world with his model, but rather "was describing what he took to be a purely imaginary world" (1968, 220). So imaginary models are linguistic representations, but they are often explicitly representations of imaginary, non-physical, or counterfactual systems. So Achinstein's distinction between theoreitical and imaginary models follows roughly the distinction I draw between applied and unapplied cases of modeling.[6] The point I want to emphasize here, however, is that Achinstein, like the linguistic approach, maintains that many central kinds of models in the sciences are linguistic representations.

The biggest differences between Achinstein's project and my own arise from different focuses. Achinstein (1968) uses his account to challenge key aspects of opposing views then dominant at the time. He denies, for example, that the logical empiricists can use formal techniques to reconstruct scientific theories in a way that guarantee all terms have empirical meaning. He also criticizes aspects of views (including those of Hanson and Ryle) that emphasize the theory-dependence of scientific terms. When engaging these opponents he is able to begin his analysis of theories and models by merely presupposing that, roughly,

---

[5]Though Achinstein (1968, 220) does observe some equivocation, with scientists sometimes taking 'model' to refer to the imaginary (or non-physical, counterfactual, etc.) description rather than the description itself.

[6]One difference between myself and Achistein here is that I allow for the possibility that the applied-unapplied distinction may apply to theories as well as models, whereas Achinstein's distinction concerns models only. My above noted reservations about the specifics of Achinstein's epistemic criterion on theories is relevant here.

both theories and models are kinds of linguistic representations that can be identified with sets of propositions. For example, in characterizing theoretical models he asserts without supporting argumentation that "when scientists speak of a model of X they are not referring to some object or system Y distinct from X, but to a set of assumptions about X" (1968, 212). From this starting place Achinstein works to identify the relevant propositions, their features, and their uses in order to develop analyses of concepts. In my case, given the alternatives to the linguistic approach that are now popular, a similar presumption cannot serve as a reasonable starting place. Both nonrepresentationalists and proponents of the semantic view deny (for different reasons) that theories or models can be identified with linguistic representations. Part of my project involves looking to theorizing and modeling practices to find evidence that can be used to support what Achinstein merely assumes. It is only by looking deeper into scientific theorizing and modeling practices that the linguistic approach can confront nonrepresentationalism and the semantic view.

Since the linguistic approach is concerned with a different level of analysis than Achinstein's view is, it must avail itself to different resources. Consequently, the linguistic approach, unlike Achinstein's analysis, is explicitly act-theoretic. As I mentioned above and will spell out in detail in chapters 6 and 7, speech-act-theoretic concepts are used as the basis of explication. The class of linguistic representations is characterized in terms of linguistic representative acts, and this class of acts is distinguished from other non-representative linguistic acts by drawing upon speech-act theoretic categories. Achinstein relies on the unanalysed[7] notion of a proposition to characterize theories and models. As I shall explain in §3.3.4 the linguistic approach can be formulated by appeal to propositions, but it need not be. Nor is it committed to any particular account of propositions. Moreover, speech-act-theoretic categories can be used not only to explicate the class of linguistic representations, but they can be used to explicate the class of non-representational speech acts too. This allows a speech-act-theoretic framework to serve as a common basis for clarifying and comparing the linguistic approach and its alternatives. Finally, it also keeps the analysis of theorizing and modeling practices tied directly to specific theorizing and modeling contexts in which specific utterances are embedded in a discourse. As we shall see in subsequent chapters, it is by observing the significance of utterances within a discursive context that we are able to best get a handle on what function expressions are performing in the discursive aspect of theorizing and modeling practices.

---

[7]Achinstein (1968) does not, for example, offer any insight into his views on the nature of propositions.

Chapter 3

Building a Case for the Linguistic Approach

In this I chapter start building a case for the linguistic approach by offering some general arguments and considerations in its favour. This will set the stage for the subsequent chapters which get into more of the nitty-gritty details concerning the linguistic approach's competitors, speech act theory, and the London model. The chapter is organized into three sections. In the first section I consider some of the implications of the continuity thesis. This is the thesis that, roughly, language-like expressions play similar roles in inferential and explanatory practices across scientific and non-scientific domains. The second section highlights virtues of the rich analysis of theorizing and modeling offered by the linguistic approach. Finally in the third section I identify and respond to a number of potential objections that are likely to be raised against the linguistic approach. These responses are meant to help elucidate the linguistic approach and forestall potentially distracting worries.

3.1 The Continuity Thesis

I want to now highlight some very general reasons for thinking the linguistic approach's direct representation hypothesis is at least *prima facie* plausible. These reasons draw upon, as I shall call it, the continuity thesis. This is the thesis, roughly, that scientific and non-scientific representative practices that have an essential discursive aspect are largely continuous, and accordingly language-like expressions can be expected to play similar communicative and epistemic functions in both domains. The continuity thesis itself is plausible and it supports the direct representation hypothesis, thus conferring comparable plausibility upon it. I shall argue this support from the continuity thesis provides the linguistic approach many virtues that its competitors do not share. In what follows I shall flesh out the connection between the linguistic approach and the continuity thesis and highlight the virtues it bestows on the linguistic approach. I will

start by drawing upon an example in order to outline some of the basic observations that speak in favour of continuity. I use the word 'outline' here advisedly: it isn't until chapters 6 and 7, where I delve more deeply into speech act theory and the normative structure of discourse, that this outline can be filled in with detail. These details aside, I hope to make clear what the continuity thesis is, how the linguistic approach supports it, and why continuity makes the linguistic approach so attractive.

3.1.1   Evidence of Continuity

Explaining and making predictions are a part of everyday life, whether explaining why one's bike chain is derailing or predicting whether or not your favourite show will be a rerun tonight. To tackle these daily explanatory and predictive tasks, especially when we engage in these tasks with others, we often recruit expressions to make assertions, claims, hypotheses and guesses about the systems under investigation. For example I might assert to a friend, "It looks like the derailleur on my bike is bent," and from this draw the conclusion "This is why my chain is derailing." When challenged explain "a bent derailleur will push the chain too far down the cog-stack causing the chain to miss the intended gear." Or flipping through the channels I may mutter to myself "I think today's episode of The Daily Show will be a rerun; it was a rerun yesterday and usually this means that there will be a full week of reruns." Now there are many different ways that expressions are used by speakers as they go about their lives. People use expressions to warn, exclaim, promise, irritate, greet and more. One important and common function of expressions in everyday contexts is direct linguistic representation. This function is especially important and common when expressions are used in connection to our everyday predictive and explanatory endeavours. That is, we routinely use language to say things about the world around us and use what is said to help explain and predict its happenings. I can represent my bicycle as having a bent derailleur by asserting so and then use this representation to explain my bicycle's tendency to malfunction. Likewise, I may explicitly make a prediction about the Daily Show on the basis of other claims made (by myself or others) about the world, e.g., that the show's episode was a rerun yesterday and that if one episode in a given week is a rerun, then reruns will usually be aired for the entire week.

The close connection between direct linguistic representation, and explanatory and predictive practices, at least in non-scientific cases, is forged by the close connection between linguistic representation and reasoning. Expressions are the principle means by which we express reasons and explicitly reason with others. Claims, assertions, hypotheses, conjectures, guesses, and other representational speech acts are the basic building blocks of explicit reasoning practices. When reasoning is expressed these linguistic representative acts are the basis for inference and are the sorts of things which are offered as justification. Moreover to engage in the practice of explaining is to engage in a particular kind of reasoning. Likewise, predicting involves reasoning in a particular way about a system. Using expressions to explain or predict the behaviour of some system routinely involves expressing one's reasoning concerning that system and this usually involves using expressions in a representative capacity. To put it more plainly, when expressing the reasoning behind our explanations and predictions about everyday worldly systems we usually do so by saying things about it.

Now the continuity thesis is the thesis that the role played by language-like expressions in explanatory and predictive practices is similar across scientific and non-scientific domains. If correct, the continuity thesis supports the linguistic approach's direct representation hypothesis. Since direct linguistic representation is a central function of language-like expressions when it is employed in non-scientific explanatory and predictive activities, then, barring some drastic discontinuity between scientific and non-scientific domains, we should expect to find expressions playing a similar role in scientific explanatory and predictive practices.

In the subsequent sections I shall outline a number of reasons for thinking the continuity thesis, and by extension direct representation hypothesis, is certainly *prima facie* plausible. But before presenting these arguments I want to motivate this discussion by first illustrating some initial evidence of continuity. The consideration of this evidence will explain and illustrate both what I have in mind with the continuity thesis, and how it connects to the direct representation hypothesis. It shall allow us to make a first pass at confronting what will, no doubt, be the primary source of concern over the continuity thesis—a concern about the role of mathematics in scientific theorizing and modeling practices. I expect most will be sympathetic to the idea that English expressions, or German expressions, or expression in other natural languages are likely to function in similar ways across scientific and non-scientific domains. However, mathematical language-like expressions are, arguably, much more prevalent in the communicative practices of contemporary sciences and this could provide some reason for thinking that there is a substantial discontinuity in the discursive practices across domains. However we can start to loosen the grip of this sort of concern by looking more carefully at some of the details of modeling and theorizing practices.

What would count as evidence of continuity? Well, roughly, evidence that expressions in a discourse were functioning like assertions, descriptions, hypotheses, claims etc. Here one source of evidence is our pre-theoretic judgements about the communicative intentions of speakers. Does the person presenting and using the theory or model seem to be "saying things" about some physical system? If so, then these judgements suggest that their utterances are playing a representational function. The connection between linguistic representative acts and reasoning can be exploited too. Is the speaker making inferences about some physical system? Do they seem to support these conclusions by identifying relevant features of the system under investigation? Answering yes to these kinds of questions again provides us reason to suppose that a scientist is explicitly reasoning in the usual way by directly representing the target system and using this representation to draw inferences about that system. Beyond these pre-theoretic judgements, specific details about the linguistic expressions and their manner of presentation can provide evidence. Assertions, claims, hypotheses and the like are linguistic representative acts. So evidence that the speaker is asserting, claiming, hypothesizing, etc., about a physical system is evidence of linguistic representation. Since there are conventional ways of performing these acts we can look for the relevant conventional clues for evidence that linguistic representative acts are being performed.

When we start looking at examples of applied modeling and theorizing, evidence of continuity is easy to find. To illustrate consider the following excerpt from an introductory population biology textbook (Neal 2004) in which a (density-independent) population growth model is presented. The passage quoted is lengthy, but I have decided to reproduce it in full to capture the entire development of the model, the

interplay between English sentences and mathematical equations and to avoid fragmenting the continuous line of reasoning expressed throughout the passage.

If we simplify things by considering a closed population where there is no immigration or emigration we can see that the change in population size over a time interval ($\Delta N/\Delta t$) is equal to the number of births (B) less the number of deaths (D) during that same time interval, as shown in the following expression:

$$\frac{\Delta N}{\Delta t} = B - D. \tag{3.1}$$

The change in population size as well as the number of births (B) and deaths (D) are related to the size of the population, N, and the rates per capita ... are determined by dividing through by the population size, N, at the start of Δt to obtain the following:

$$\frac{(\Delta N/\Delta t)}{N} = \frac{B}{N} - \frac{D}{N}. \tag{3.2}$$

However, the birth rate (B/N) minus the death rate (D/N) is equal to the per capita, or per individual, rate of increase, R_m, and so 3.2 can be rewritten ... [and] rearranged to form our first equation:

$$\frac{\Delta N}{\Delta t} = R_m N. \tag{3.3}$$

This equation shows that the change in population size is directly proportional to population size, provided the growth rate per capita, Rm, remains constant.

Let us now develop an equation to predict the future size of the population. Population size after one time step will equal the original population size plus the change in number, which is expressed mathematically by the following expression:

$$N_1 = N_0 + \frac{\Delta N}{\Delta t}. \tag{3.4}$$

Substituting 3.3 for $\Delta N/\Delta t$ and setting $N = N_0$, 3.4 is modified ... [and] reduces to:

$$N_1 = N_0(1 + R_m). \tag{3.5}$$

The multiplication rate, λ, from one time period to the next is N_1/N_0 and therefore

$$N_1 = N_0\lambda. \tag{3.6}$$

A comparison of 3.5 and 3.6 reveals that

$$\lambda = 1 + R_m. \tag{3.7}$$

From 3.6 we see that the population size after two time steps is

$$N_2 = N_1\lambda \tag{3.8}$$

and substituting 3.6 for N_1 in 3.8 yields:

$$N_2 = N_0\lambda\lambda = N_0\lambda^2. \tag{3.9}$$

We can do this for successive time steps to show that the general case is provided by the following equation:

$$N_t = N_0 \lambda^t. \tag{3.10}$$

The size of the population at fixed intervals of time can now be predicted provided we know the starting number, N_0, and there is a constant multiplication rate, λ, during each time interval, Δt.[1] (Neal 2004, 54–56)

Following the above presentation of the simple population growth model the author then applies the model to explain the exponential growth observed in the pheasant population of Protection Island between 1937 and 1943 (Neal 2004, 62–63).

In the course of developing and applying this model the author presents a series of language-like expressions: some are English expressions, others are mathematical expressions, and in many cases both English and mathematical symbols are combined to form mixed expressions. Let us consider the English expressions first. How are they to be understood? How are they used by the author? It is natural to construe most of these as expressions that the author uses to say things about a closed population and its growth. In other words, they are used to represent closed populations and their features. The author claims that, for example, changes in size are "equal to the number of births ... less the number of deaths ... during that same time interval," and that their growth rate "is directly proportional to population size, provided the growth rate per capita ... remains constant" (Neal 2004, 54–55). This construal of these English expressions is supported by the observation that most of the sentences are in the declarative mood. Declarative sentences are conventionally used in English to perform representative speech acts, like claiming, and hence they are conventionally used to express truth-apt propositional contents.

What about the equations and other mathematical expressions? What evidence do we have for thinking that these linguistic expressions are functioning in this context in a representative capacity? One reason is that it appears as though each equation can be equivalently expressed in English without using mathematical symbols or syntax, and the equivalent English expressions are declarative sentences which are explicitly presented and endorsed by the author. Take for example equation 3.1, "$\Delta N / \Delta t = B - D$." The author systematically identifies what ΔN, Δt, B, D are meant to stand for in this context. This allows us to re-express the equation in English as follows: "The change in population size over a time interval is equal to the number of births less the number of deaths during that same time interval." This English expression of the equation 3.1 is declarative and, therefore, is conventionally used to perform a representative function. Moreover, the author explicitly presents and appears to endorse this English expression when introducing the equation. The straightforward construal, then, is that the equation and English expression are taken by the author to have the same communicative significance as representations of the population.

Evidence of linguistic representation is also apparent when we consider the reasoning expressed in the passage. Both the English sentences and mathematical equations appear to be offered as a basis

---

[1]Note that the equation numbers are my own. Also two trivial steps in the original presentation (where "... [and]" appears) have been omitted to make the excerpt a little shorter.

for drawing conclusions. This inferential role is characteristic of expressions that are functioning in a representative capacity. The acceptance of equation 3.1, for example, offers reason for accepting equation 3.2 because the latter is a consequence of the former. This inferential/justificatory relation that obtains between equations 3.1 and 3.2 is explained by the author who points out that the per capita rate of change is equal to the per capita birth rate minus the per capita death rate. Likewise, as made explicit by use of the indicator word 'therefore,' the author takes equation 3.6 to be acceptable on the grounds that the "multiplication rate, λ, from one time period to the next is N_1/N_0" (Neal 2004, 54–56). In fact the bulk of the quoted passage can be construed as expressing a continuous line of reasoning that starts with a set of assumptions (i.e., the assumption that the population is closed and the multiplication rate is constant) and ends with equation 3.10, which describes the growth of the population as a function of its initial size, multiplication rate and time allowed to grow. Insofar as the author's reasoning is good, one has grounds for accepting this description provided they already accept the underlying assumptions.

This construal is corroborated later in the text when the author applies the model to explain the exponential growth of the Protection Island pheasants. He discusses a number of considerations relevant to the acceptability of the model's underlying assumptions. He notes that the "population was a closed one because the island was too far from the mainland for pheasants to fly in or out" (2004, 62). Also there was "abundant food on the island, and there were no bird predators" (2004, 62), which are both factors that would have allowed the multiplication rate of the pheasant population to remain nearly constant despite its increase in size. I think the author's reasoning here is clear. One should expect exponential growth in a closed population with a constant multiplication rate, and there is reason to believe that the pheasant population was closed and its multiplication rate was nearly constant. It's these facts in conjunction with the reasoning expressed in the quoted passage that are offered in explanation of the observed growth in the pheasant population. With respect to the role of mathematics in this example, if we were to insist expressions containing mathematical symbols are somehow systematically used in a different way from the English sentences, then we would have to abandon the straightforward construal. Now this is a theme I shall return to in various places in the coming chapters (especially in §4.3), but what I think this example nicely illustrates is the relative ease with which scientists fluidly move between natural language and mathematical expressions when expressing their reasoning. As we shall see, it becomes hard to understand this interplay between mathematical and English expressions when one insists that each systematically play distinct roles in discourse. To maintain such a distinction one is pushed to say some implausible things about the discursive aspect of theorizing and modeling practices.

In summary, it does appear as though the expressions in this modeling example are playing a representative and inferential role equivalent to the role played by language in everyday explanatory and predictive practices. The working hypothesis, then, is that the presentation and use of this simple population growth model is rather routine or ordinary. If so, then there is good reason to suppose that expressions are used in a similar way in many explanatory and predictive practices, whether they occur in scientific or non-scientific contexts.

The linguistic approach respects the observed similarities between scientific and non-scientific uses of expressions. It takes seriously the idea that appearances are not misleading and expressions do regularly

function in explanatory and predictive practices in the same way across scientific and non-scientific contexts. Now to be clear, the continuity thesis does not imply that all expressions employed in the context of people's explanatory and predictive practices are invariably used in a direct representative capacity. Instead since direct representation is commonplace in the discursive practices associated with everyday explanatory and predictive practices, then continuity suggests that it is also commonplace in scientific contexts. This, of course, supports the linguistic approach's direct representations hypothesis. According to this thesis (where theorizing and modeling has an essential discursive aspect) the sentences and equations associated with theories and models are regularly used by scientists to say things directly about the system, or systems, under investigation in the theorizing or modeling context.

As a working hypothesis the direct representation hypothesis is something that must ultimately be evaluated on the basis of more thorough and comprehensive evidence than I have provided thus far. If an investigation of scientific modeling and theorizing practices reveals that direct representations was the exception and not the norm, then the direct representation hypothesis and the continuity thesis which supports it would have to be abandoned. Before a more thorough examination of the linguistic evidence can happen we must be more precise about what counts as evidence that language is, or is not, functioning in a direct representative capacity. I take this challenge up in chapters 6 and 7. There I elaborate on what direct representation is, how it contrasts with the role of language posited by the linguistic approach's competitors, and what counts as evidence for and against direct representation. However, as illustrated with the above example, it is pretty easy to see why the direct representation hypothesis and continuity is plausible.

3.1.2 The Virtues of Continuity

By positing continuity between scientific and non-scientific discursive practices the linguistic approach has many advantages over its competitors who posit discontinuities. In this section I shall argue that these advantages provide *prima facie* reasons to prefer the linguistic approach. As *prima facie* support, these considerations may be viewed as offering supplementary support to the evidential considerations highlighted above and those evidential considerations that will be developed in greater detail in subsequent chapters. However, I think these reasons may just as naturally be construed as motivating considerations. If prior to an in depth evaluation of the evidence there are reasons to favour an account that posits continuity over those that posit discontinuity, then it would be wise to abandon such an account only if compelling evidence to the contrary is found. In other words, these considerations suggest the linguistic approach should be the default approach to understanding scientific theorizing and modeling.

Explanatory Virtues of Continuity

The most general reason for favouring a view that maintains continuity is that it avoids unnecessarily multiplying explanandum. We know that people use expressions in non-scientific contexts to directly

represent the world around them, to express reasoning and to communicate their representations and reasoning to others. These uses of expressions in non-scientific contexts is something to be understood and explained; it is a datum to be accounted for in one's epistemology and philosophy of language. Now if expressions are generally used in similar ways across scientific and non-scientific contexts, then whatever account is given for the non-scientific cases can be extended to scientific cases as well. In other words continuity brings with it the virtue of parsimony: a unified account of expression use in explanatory and predictive practices can be given for scientific and non-scientific domains alike. Only one theory, or theoretical framework, needs to be developed to account for the discursive data gathered from scientific and non-scientific explanatory and predictive practices. On the other hand by denying the continuity thesis the linguistic approach's opponents incur an extra explanatory burden. In addition to the non-scientific cases they must also explain what scientists are doing by presenting sentences and equations, how these doings contribute to the explanatory and predictive goals of scientists, and how scientists learn to use expressions in this special way.

To illustrate this point consider the question of learning. Scientists use sentences and equations as part of their explanatory and predictive practices. The question, then, is how do scientists learn to use expressions in this way? If the continuity thesis is correct, the process of learning how to use expressions for explanatory and predictive purposes in science ceases to be a special problem of the philosophy of science. If continuous, then scientists are not learning some new abilities but extending abilities acquired to cope with everyday sorts of explanatory and predictive tasks. When one tries to explain why their chain keeps derailing, or make predictions about traffic volumes, they make observations, develop hypotheses and theories, draw inferences and use these inferences to explain and predict. Moreover, all competent language users have learned how to communicate this sort of reasoning via language. Doing similar things in scientific contexts, then, is a matter of extending and honing these same skills.

Conversely, by rejecting the continuity thesis one forgoes this parsimonious way of accounting for learning. By rejecting continuity one maintains that (at least some) expressions function in a different way in scientific practices. For scientists to employ language in a different way they must somehow acquire the ability to do so. And so the philosopher who denies the continuity thesis must offer a separate account of how scientists acquire these new abilities. Of course it is possible that scientists do use sentences and equations in a special way and they acquire the ability to do so in the course of learning their trade. But this is something that must be borne out by the evidence. Before this evidence is considered, however, we see that those who posit discontinuity are at a disadvantage because their accounts have an added explanatory burden. All else being equal one should prefer an account that posits continuity over one that does not because it avoids taking on these extra burdens.

A related methodological point speaks in favour of the linguistic approach. Whenever we have some means of accounting for phenomena in one domain, then it is good practice to see if the same account can be extended to explain similar phenomena in other domains before generating alternative explanations. When it was discovered, for example, that the elliptical orbits of planets in our solar system could be accounted for by Newton's theory of gravity, it made sense to try and explain the motions of other astronomical bodies, like comets, in the same way. Edmond Halley famously did just this. He correctly

identified a series of historical comet sightings as sightings of the same comet (which now bears his name) travelling in orbit around the sun. He also went on to correctly predict the year of the comet's return. The pay-off of finding a unified account of the motions of astronomical bodies would have been missed if Halley had not tried to extend Newton's account of planetary motions to comets. Likewise, philosophers of language and epistemologists have developed, at least partial, accounts of language's functions in everyday discourse and, more specifically, in the reasoning expressed by people in non-scientific contexts. Good practice suggests that we should attempt to see if these same accounts can be extended to understand the role of equations and other utterances in the explanatory and predictive practices of scientists. This is precisely what I attempt to do in chapters 6 and 7 by drawing on speech act theory. The linguistic approach's competitors, I shall argue in the next two chapters, have rejected the linguistic approach prematurely. Though they have not provided compelling reasons for thinking it will fail, they nonetheless seek alternative ways of understanding the role played by language in scientific contexts. The upshot, again, is that the linguistic approach which posits continuity is *prima facie* preferable.

Linguistic Demarcation Problem

Another set of considerations that favour continuity and the linguistic approach arise in connection to a version of the demarcation problem. The traditional demarcation problem concerns the distinction between science and pseudoscience, which is a distinction that is thought to have epistemic import. Roughly, science is thought to be an especially reliable means of learning about the world and classifying a theory, investigative method, or research program as 'scientific' is to attribute to it some special epistemic status. By contrast pseudoscience presents itself as science but does not deserve the corresponding status.

The demarcation problem relevant to the conflict between the linguistic approach and its competitors is a bit different. By rejecting the direct representation hypothesis both the semantic view and nonrepresentationalism imply a kind of systematic discontinuity across, roughly, scientific and non-scientific contexts. Specifically, these views imply that in scientific modeling and theorizing practices expressions are not performing the direct representational function that they characteristically play in everyday explanatory and predictive practices. The challenge then is demarcating between those cases in which expressions function in one way, and those in which they functions in another. That is, demarcating between those (non-scientific) cases when expressions are used in their usual direct representational function and those (scientific) cases where, according to the linguistic approach's competitors, expressions are systematically used in some other way. I call this the *linguistic demarcation problem*.[2]

Any views that posit a systematic linguistic discontinuity must provide some account of it. There are a couple of reasons why. First, the kinds of discontinuities posited by both the semantic view and nonrepresentationalists are *systematic* ones. These views are not attempting to capture a few isolated and special

---

[2]Though a linguistic demarcation could be offered as a basis of a science-pseudoscience distinction, the two demarcation problems are distinct. A solution to the linguistic demarcation problem does not necessarily imply a solution to the traditional demarcation problem, or *vice versa*.

cases. Rather each suggests expressions function in a distinctive way systematically across scientific theorizing and/or modeling practices. Second, the proposed discontinuities systematically exclude expressions from playing one of their most common and characteristic functions, i.e., direct representation. Of course expressions do function in a large variety of different ways in different contexts. However, this regular variation in expression usage could not explain systematic discontinuities. A regular variation in language use could not explain the supposed uniform use of expressions in scientific discursive practices. And it certainly could not explain why expressions are systematically not used in a direct representational capacity.

To account for a systematic discontinuity, then, a view must be able to provide a reasonably clear *principle of demarcation*. This will provide a means of distinguishing cases in which expressions can be expected to function in a way distinct from those in which it can be expected to function in the more common direct representational capacity. Since both the semantic view and nonrepresentationalism are purported to be views about *scientific* models and *scientific* theories, their proponents imply that the demarcation falls roughly along scientific vs. non-scientific lines. In this case the linguistic demarcation challenge is one of characterizing the relevant distinction between putative examples of scientific theories/models and non-scientific theories/models. Another thing we should expect of a solution to the linguistic demarcation problem is some means of justifying the demarcation principle specified. The distinction should not be an unprincipled one established by fiat. Some reason for drawing the line in one place and not another must be specified and, ideally, the account will explain why there is a demarcation to be drawn at all. Insofar as the purported demarcation falls roughly along the distinction between science and non-science, then we are owed some account of why expressions should be expected to play substantially different roles in explanatory and predictive practices in various cases.

As we shall see in the subsequent chapters, a failure to meet the linguistic demarcation leaves the intended scope of the semantic view and nonrepresentationalism unspecified. One cannot distinguish which predictive and explanatory practices should be construed in their preferred way and which should not. Worse, if no basis for a demarcation can be provided, then their projects in general are threatened. If communicative practices are continuous across scientific and non-scientific domains, then there is no reason to presume that a distinct kind of account of scientific theories and models is even necessary.

I shall return to the linguistic demarcation problem in the subsequent chapters where the semantic view and nonrepresentationalism will be considered in greater detail. There I shall demonstrate that demarcation principles, which at first seem promising, either prove to be untenable or otherwise saddle views that adopt them with new problems. For now I will be content to point out that the very need to address the linguistic demarcation problem is a theoretical burden that the linguistic approach does not have to bear. By positing continuity between linguistic practices in both scientific and non-scientific domains no demarcation needs to be sought because *ex hypothesi* no useful linguistic distinction exists. Again, all else being equal one should prefer the linguistic approach because it avoids theoretical burdens faced by views that reject the continuity thesis.

Approaching the issue from another direction, the linguistic demarcation problem throws down the

gauntlet to views that posit systematic discontinuities. By highlighting the theoretical costs involved, it challenges proponents of these views to justify positing discontinuities. It may be the case that specific communities, or sub-disciplines, or certain theorizing and modeling practices within the sciences do involve the systematic use of expressions in unique ways. This type of phenomena, if it were discovered, would indeed be interesting and worthy of careful investigation. However it is unreasonable to simply assume that discontinuities like these do exist. Nor should we give a free pass to views that posit discontinuities without providing some idea where and why the supposed fault lines in discursive practices exist. So the continuity thesis offers some reason to expect the linguistic approach's direct representation hypothesis to be true. The other side of the coin, the linguistic demarcation problem, offers some reason to think views that posit systematic discontinuities are unlikely. More than this, the linguistic demarcation problem pushes proponents of accounts like the semantic view to examine problematic and undefended assumptions about the functions of expressions within the discursive aspect of theorizing and modeling practices.

3.2 Epistemic Richness

In this section I want to highlight an additional virtue of the linguistic approach. Beyond the theoretical and explanatory virtues mentioned in the previous section, I shall now show that the linguistic approach adds a sophistication to the analysis of theories that enables it to capture important aspects of scientific representative practices. Specifically, on the linguistic approach theories and models are *epistemically rich* objects. This enables the approach to capture both the diverse epistemic attitudes that scientists adopt towards different aspects of their representations and the diverse inferential and justificatory relations that they posit to exist between different aspects of representations.

According to the linguistic approach: (1) expressions can take on different significances when different speech acts are performed, and (2) models and theories can be complex linguistic representations that are comprised of simpler linguistic representations. Both of these features allow the linguistic approach to capture the complex epistemic structure built into representations by scientists.

As you will recall, according to the linguistic approach modeling and theorizing involves the utterance of a series of linguistic expressions. The utterance of each expression in the series is a linguistic representative act. Acts of asserting, hypothesizing, and conjecturing are all examples of linguistic representative acts. Each act produces a simple linguistic representation which, in its own right, is contentful, truth-apt and the proper object of epistemic attitudes.

A central feature of speech act theory is that the same expressions that express the same semantic contents can be employed in different ways through different acts (for further discussion see §6.1). For example I can assert outright that greenhouse gases are causing the average temperature of the Earth to increase, or I can forward the same proposition, i.e., with the same semantic content, merely as a hypothesis. There are two important ways in which assertions, hypotheses and other representative speech

acts have different significances. First, each act is characteristically used to express a unique epistemic attitude. To sincerely assert proposition P is (at least to a first approximation) to express an attitude of belief towards that proposition.[3] However, by hypothesizing P one does not characteristically express full-fledged belief in P. The expressed attitude is more attenuated. One expresses an attitude that, roughly, P is likely true (more on this in §6.1.3 and §6.2). Second, the distinct communicative significances of acts of assertion and acts of hypothesizing are also reflected in the roles they play in inferential and justificatory practices. To take a simple example, if Q is entailed by P and P is asserted, then on the authority of P's assertion one is also thereby warranted in asserting Q. In other words, the assertion of P provides grounds for the assertion of Q. On the other hand, if P is forwarded as a hypothesis only, then the utterance of P does not warrant the assertion of Q in this case. At most it can warrant offering Q as a hypothesis. So we see that assertions and hypotheses are acts that confer warrant on claims in different ways.

By analysing uttered expressions as linguistic representative acts (as assertions, hypotheses, etc.) one can capture these linguistic practices as content conveying activities, and also capture the different communicative significances that speakers attribute to these contents. This makes linguistic representations epistemically rich in the following sense. Linguistic representative acts encode information about the epistemic attitudes expressed by the scientists presenting the representation and distinct roles that scientists take these representations to be able to play in inferential and justificatory practices.

The compositional nature of complex linguistic representations also allows the linguistic approach to capture the epistemic richness of theories and models. Typically theories and models are expressed by a series of utterances with the performance of each utterance counting as a linguistic representative act. So complex linguistic representations like theories and models are comprised of simple linguistic representations. Each simple act is part of a complex linguistic act. Because complex representations have these parts, they can have an epistemically rich structure. Different parts of the overall representation can take on a different communicative significance: some utterances may be asserted while others are put forward as, say, mere conjectures. These mixed type complex representations enable the speaker to express complex epistemic attitudes towards the whole representation.[4] The speaker can, for example, express a conviction that some parts of the representation are true while expressing more attenuated or modified convictions regarding the truth of other parts.

Moreover, in presenting a complex linguistic representation as a series of utterances the speaker can make explicit both the inferential and justificatory relations that they believe obtain between the represen-

---

[3]As I discuss in Chapter 6 it is contentious which epistemic attitude theorists take to be characteristic of sincere assertions. Moreover, consideration of insincere assertions adds an extra dimension of complexity to the analysis of the discursive significance of the these, and other, speech acts. For current purposes it is not important which epistemic attitude(s) are expressed by acts of assertion, but the fact that there are clear differences between the attitudes characteristically expressed by acts of assertion, conjecture, hypothesis etc.

[4]Achinstein (1968, 122–123) makes a similar move recognizing that theories, as complex objects, are often not best understood as true or false *simpliciter*. On his view a theory is associated with a set of propositions and the conjunction of any subset of these propositions may be true (or false) without the conjunction of the entire set of propositions all being true (or false). My proposal here suggests even more complexity because the speaker can express a whole range of convictions to each uttered proposition.

tation's parts. In presenting a series of utterances the speaker will often indicate that some of the utterances in a series are intended to be taken as providing warrant for later utterances in the series. The contents expressed by one or more utterances may entail the contents of another, but deductive (or non-ampliative) relations are not the only kind that can be expressed through linguistic representative acts. The inferential connections may also be ampliative (e.g., inductive or abductive). The result is a richly structured representation with parts of the representation having complex inferential and justificatory relations to other parts of the representation.

Thus the linguistic approach avoids treating theories and models as having undifferentiated contents that scientists endorse (or reject) univocally. Scientists express complex epistemic attitudes by expressing distinct attitudes towards different portions of a theory or model's contents. Nor on the linguistic approach is there any grounds for thinking that theories and models function in a simple, or singular way, within broader inferential and justificatory practices. Because linguistic representations have a rich epistemic structure, different parts of a representation can play distinct roles. So parts of a theory or model can confer warrant on other claims within some chain of reasoning in different ways or to different degrees. So it needn't be the representation as a whole that straightforwardly warrants some prediction or explains some phenomenon, but its parts that do. Moreover, a scientist's commitment to a prediction may be attenuated in proportion both to the attenuated commitments that are appealed to as warrant for the prediction and the defeasibility of the (typically ampliative) inferences that are relied on in making the prediction. In short, on the linguistic approach theories and models are epistemically rich objects and this richness can be exploited to account for the complex roles that these representations play when they are pressed into the service of various epistemic activities.

3.3 Objections and Replies

In this section a series of objections to the linguistic approach will be raised and replied to. The aim is both to further elucidate the linguistic approach and to forestall potentially distracting worries that the reader may have about the linguistic approach. This, I hope, will further the overall goal of the chapter: to establish the linguistic approach as a bona fide contender, which has many advantages over its competitors and the resources to respond to criticism that are likely to be raised against it.

Most of the objections listed below are anticipated objections. This is necessary because views like the linguistic approach, as Frigg and Hartmann note, have "not been subject to explicit criticism" (2012).[5] The exceptions are a few objections raised in Frigg and Hartmann's own brief discussion. I shall start with their objections and then consider a few additional objections that I think are most likely to be raised.

---

[5]Frigg and Hartmann (2012) consider a view of models according to which "what scientists display in scientific papers and textbooks when they present a model are more or less stylized descriptions of the relevant target systems." Though the linguistic approach does not identify models exclusively with descriptions, a *descriptivist* view, as they call it, is clearly similar to my own.

3.3.1 Doesn't the linguistic approach face many of the same criticisms that the now defunct syntactic view faces?

Roughly, according to the syntactic view (alternatively referred to as the received view) theories are identified with expressions of a formal language. So in a sense the syntactic view, like the linguistic approach, identifies theories with something broadly language-like. This raises the general concern that, perhaps, "some of the criticisms that have been marshalled against the syntactic view of theories equally threaten a linguistic understanding of models" (Frigg and Hartmann 2012). In connection to this general worry Frigg and Hartman identify a specific criticism of the syntactic view, which concerns the properties and identity conditions of models. I shall directly respond to their more specific criticism below. However I would like to first address the more general worry. As I mentioned earlier, I suspect that part of the reason views like the linguistic approach have not been subject to more discussion and criticism is because people have wrongly assumed that all language-oriented views inevitably face the same problems and fate of the now defunct syntactic view. It will be worthwhile, then, to briefly compare the syntactic view and the linguistic approach and highlight the key differences that enable the linguistic approach to avoid the central objections raised against the syntactic view.

The syntactic view is associated with logical empiricism, and some version of it has been advocated by Carnap, Hempel, Reichenbach, Schlick and others. Since it is associated with a philosophical movement (with many different contributors whose views evolved over time) there are attendant difficulties talking unequivocally about what the syntactic view entails. However there are common aspects shared by most versions of the syntactic view and for current purposes it will suffice to focus our discussion on four of these aspects. On most versions of the syntactic view: (1) scientific theories are reconstructed, or explicated, axiomatically in a formal (usually first order) language; (2) the non-logical constants of this language are bifurcated into observational and theoretical vocabularies, and observational and theoretical sub-languages are then defined in terms of each vocabulary respectively; (3) the observational language is interpreted, roughly, on the domain of observable phenomena; (4) correspondence rules partially interpret claims that employ theoretical vocabulary (see, for example, Mormann 2007 and Suppe 1977).[6] The linguistic approach, by contrast, does not make commitments anything like (1)-(4). For one, its aims are descriptive. It does not attempt to provide any reconstruction or explication of scientific theories (or models) in any particular language. Consequently it does not make any commitments regarding a particular language of reconstruction, its interpretation or its structure.

The syntactic view has been challenged (rightly or wrongly) on many fronts.[7] The general philosophical underpinnings of it have been attacked by Quine (1951) and Putnam (1962) who challenge the tenability of the observational-theoretical distinction and the various views of meaning which the distinction has been used to support. Other criticisms point to problems of a more technical nature. For example

---

[6]For a more detailed formulation of the syntactic view see Suppe (1977, 50–51), who outlines a "final version" of the view after the development and changes made to it by Carnap and Hempel.

[7]In collecting and organizing objections to the syntactic approach I am indebted to the helpful overviews prepared by Suppe (2000, S102–S104; 1977, 62–118), and da Costa and French (2000).

van Fraassen (2000, 190) draws upon limitative meta-theorems that show non-trivial theories axiomatized in first-order languages of logic will have unintended models (c.f., §4.2.2). He argues that the existence of unintended models is an artifact of the logical reconstruction of scientific theories and for this reason the syntactic view distorts theories. The adequacy of the syntactic view as an explication of actual scientific practice has been challenged. Frederick Suppe (1977, 62–66), for example, challenges the syntactic view's emphasis on axiomatization by arguing that many scientific theories are simply too underdeveloped to be fruitfully axiomatized. Patrick Suppes (1969c; 1967) argues that coordinating definitions inadequately capture the complex ways that theories relate to observations.

However, none of these objections to the syntactic view apply to the linguistic approach. There a number of reasons why:

i. The linguistic approach does not endorse or rely upon either the observational-theoretical distinction or any particular theory of meaning. Consequently, criticism of either the observational-theoretical distinction or the different views on meaning that this distinction has been used to support have no force against the linguistic approach. Each plays a central role in the unique brand of empiricism espoused by the proponents of the syntactic view. The linguistic approach is not directly motivated by, or beholden to, any particular epistemological or metaphysical project like this.[8] It aims instead to offer a descriptive account of actual scientific theorizing and modeling practices that have an essential discursive aspect.

ii. The linguistic approach to theories and models does not posit, or require anything like correspondence rules. As such the linguistic approach is immune to the criticisms raised in connection to them. Since the linguistic approach does not subscribe to any particular theory of meaning it does not rely on coordinating definitions to account for the meaning of theoretical terms.

iii. The linguistic approach does not identify theories, or models, with partially interpreted axiomatic systems formulated in any particular formal language. Doubts about the axiomatizability of theories do not raise any concerns for the linguistic approach. On the linguistic approach whenever theories are linguistic representations they are identified with the expressions that the scientists themselves utter. As a result the linguistic approach is not syntactic in any interesting sense. Contemporary investigations of communicative practices draw upon more than the syntactic features of the languages they study. Consequently the linguistic approach can also draw upon the semantic and pragmatic features of expression to account for scientific representative practices.

iv. The linguistic approach is not one that seeks to reconstruct or explicate scientific practice. Regardless what virtues or defects different reconstructions of scientific practice might have, the linguistic approach seeks to characterize theories, models and the scientific practices that produce them.

---

[8]For more on this see my response to the objection raised in §3.3.3, which addresses the relation between the linguistic approach and realism and anti-realism.

We see that there are many important differences between the linguistic approach and the syntactic view. There is no general reason to assume that each view will be subject to similar criticisms.

3.3.2 Aren't the wrong properties or identity conditions attributed to models and theories if they are characterized as linguistic representations?

Frigg and Hartmann single out the following objection as one which applies equally well to the syntactic and linguistic approaches alike:

> [I]t is a commonplace that we can describe the same thing in different ways. But if we identify a model with its description, then each new description yields a new model, which seems to be counterintuitive. One can translate a description into other languages (formal or natural), but one would not say that one hereby obtains a different model. Second, models have different properties than descriptions. On the one hand, we say that the model of the solar system consists of spheres orbiting around a big mass or that the population in the model is isolated from its environment, but it does not seem to make sense to say this about a description. On the other hand, descriptions have properties that models do not have. A description can be written in English, consist of 517 words, be printed in red ink, and so on. None of this makes any sense when said about a model. The descriptivist faces the challenge to either make a case that these arguments are mistaken or to show how to get around these difficulties. (2012)

Frigg and Hartmann raise this objection against, as they call it, the descriptivist account of models which, similar to the linguistic approach, takes models to be descriptions of systems. However we must consider this with respect to both models and theories since the linguistic approach characterizes both models and theories alike as linguistic representations.

In responding to this objection the potential ambiguity of 'model' must first be addressed. Frigg and Hartmann claim that models have different properties than descriptions. This claim is clearly true of certain kinds of things often called 'models.' A scale model airplane, for example, has mass, aerodynamical properties, and even a smell. A description has none of these things. For precisely these kinds of reasons the linguistic approach does not suppose that these representations can be plausibly construed as being language-like. The linguistic approach offers an account of scientific theorizing and modeling practices that have an essential discursive aspect.

However, the claim that models have different properties than descriptions is not obviously true when we consider models presented via language-like discursive practices. Those models presented via sentences and equations do not have a mass, aerodynamical properties or a smell. Of course these models could be used to represent systems with mass, or other physical properties, but these are not properties of the representations themselves. To enquire into the features of these models is to enquire into the nature of representations and not of their targets. When we compare models presented via sentences and equations to theories or descriptions that are also presented via sentences and equations, it seems much more

45

plausible to suppose that each, qua representations, have similar properties. There is no obvious reason why, for example, a model cannot be expressed using English, and thus be "written in English, consist of 517 words, be printed in red ink, and so on" (Frigg and Hartmann 2012). To simply presume that all models, including those presented via sentences and equations, have different properties than descriptions is to simply beg the question against the linguistic approach.

By attending to the potential ambiguities of 'model,' however, we cannot ameliorate all of Frigg and Hartmann's objection. They argue "it is commonplace that we can describe the same thing in different ways ... [b]ut if we identify a model with its description, then each new description yields a new model, which seems to be counterintuitive" (2012). Similar objections have been raised by critics (e.g., Suppe 2000) of the syntactic view in connection with theories. Roughly, if the same theory can be expressed in different ways, then to identify the theory with one of its expressions is to get the identity conditions of the theory wrong. The theory is the thing which can be variously expressed or formulated.

Two responses to this part of the objection are amenable to the linguistic approach, though for reasons I shall outline, I think the second response is superior. The first response involves accepting the argument's central premise that a single, i.e., numerically identical, theory or model can be expressed in different ways. If this premise is accepted then a theory or model cannot be identified with any one of its expressions otherwise multiple expressibility would be impossible. In response the linguistic approach can change what 'theory' and 'model' refer to in the analysis of scientific representative practices, but still leave the overall account with its discursive emphasis unchanged. On this proposal theorizing and modeling are still linguistic representative practices that involve the presentation of linguistic representations, but theories and models are not identified with the linguistic representations presented. Rather they are identified with the contents that are expressed by a series of linguistic representative acts. Insofar as two representations have the same contents, then we can say that the two distinct representations express the same theory or express the same model.

This response is acceptable so long as appealing to the notion of content is unproblematic. Though most (myself included) will find this unobjectionable, some may not. By identifying theories and models with the contents of representations, the linguistic approach would thereby take on certain theoretical commitments associated with the notion of content and its attribution. A consequence of these commitments would be that theories of language that cannot accommodate an appropriate conception of content would no longer be amenable to the linguistic approach. Of course it is preferable to avoid taking on these kinds of theoretical burdens. When possible the linguistic approach should aim to remain amenable to whatever general account of meaning(s) that proves to be the best.

Another response is available to the linguistic approach. The objection turns on the claim that a single theory or model can be multiply expressed, but one need not accept this premise. Frigg and Hartmann suggest that a denial of this premise leads to counterintuitive consequences, but upon scrutiny we find that rejecting multiple expressibility isn't nearly as counterintuitive as it might first appear. Instead I want to argue that part of the seeming appeal of multiple expressibility can result from the conflation of a representation and its target and that most of the intuitions underlying multiple expressibility can be

captured without identifying theories or models with extra-linguistic entities.

The linguistic approach seeks an understanding of modeling and theorizing as representative practices. It is as representations that models and theories have the potential to be vehicles of scientific knowledge. When addressing questions concerning representation it is important to keep distinct a representation from its target. Representations are not (at least in non-degenerate cases) identical with their targets. Each has unique properties and it is the relations between the representation and target that are relevant to the evaluation of the representation's accuracy, fidelity, or truth.

Revisiting Frigg and Hartmann's objection with the distinction between representations and their targets in mind, it becomes apparent that the way they have presented their objection is misleading. They first claim that the "same thing can be described in different ways" (2012). This, of course, is true and amounts to the simple observation that distinct representations can have the same target. But Frigg and Hartmann then say "if we identify a model with *its* description, then each new description yields a new model, which seems to be counterintuitive" (2012; my emphasis). The "its" is the contentious part. Frigg and Hartmann imply that the description is a representation of the model, and conversely that the model is the representational target of the description. This slippage between the consideration of models qua representations and models qua representational targets is partly responsible for the consequent of the above conditional seeming problematic. The antecedent of their conditional claim simply presupposes that descriptions and models are distinct and stand in a representational relationship to one another. In such cases the consequent is problematic because the creation of distinct representations does not entail the creation of distinct representational targets. But precisely what is at issue is the natures of models qua representations. If models are kinds of descriptions, then there is absolutely nothing counterintuitive in saying that each new description/model yields a new description/model.

Moreover I think that much of the underlying intuition driving the objection can be captured without identifying theories or models with extralinguistic intermediary objects that are distinct both from linguistic representations and their ultimate representational targets. There is certainly some appeal to the idea of multiple expressibility. Presumably scientists themselves sometimes talk of theories and models being multiply expressed. How can the linguistic approach make sense of this? The key is distinguishing identity from equivalence. Non-identical representations can be equivalent in many different ways. Equivalences, then, can be invoked to capture intuitions about multiple expressibility.

Questions of equivalence are not unique to scientific representation. They routinely arise in regards to paradigmatic examples of linguistic representation. It is commonplace to ask whether two sentences of English are synonymous, or whether a sentence of English and one of French can have the same significance. Six editions of *On the Origin of Species* were published in Darwin's lifetime and it has been translated into many different languages by different translators.[9] Each edition and translation, though using different vocabularies, phrases and even different alphabets, are nonetheless treated for most purposes as though they are "the same book." We say of a Russian speaker who is familiar with Darwin's work only

---

[9]Note that the title of the 6th edition dropped 'On' and was subsequently titled *The Origin of Species*.

through Sergei Rachinsky's translation that they have read Darwin despite the fact that they have never looked at a single English phrase that Darwin himself composed.

There are standard ways in which the equivalence of non-identical linguistic entities is accounted for. One way of understanding equivalence has already been introduced. Two assertions, descriptions or two books are in a clear sense equivalent if they express the same contents. Notice that conceding that two assertions have the same content does entitle us to claim that they are identical assertions. Two assertions with the same content may be uttered at different times, uttered by different speakers, or be expressed by using different words or symbols.

Appealing to content, however, is not the only way in which equivalence is often understood. One can also capture a relevant notion of equivalence by appeal to substitutability or translation. Roughly if two expressions can be substituted for one another in some discursive context without substantially changing the communicative or epistemic role played by the expression, then there is a clear sense that each can be thought of as equivalent. Note that substitutability can be used to capture a kind of equivalence which is independent of any particular view concerning semantic content as long as the communicative or epistemic role played by expressions can be characterized without appealing explicitly, or implicitly, to the semantic contents of expressions. When we are dealing with expressions from different languages then the relevant kind of equivalence can be captured by appeal to translation. If, relative to some standard, one English description is an acceptable translation of a description expressed in French, then there is another clear sense in which these descriptions, at least for most practical purposes, can be treated as though they are interchangeable. Similar to substitutability, this kind of equivalence is also one that can potentially be cashed out without relying on a specific notion of semantic content so long as a standard of translation can be specified without relying on some prior understanding of content.

The same approaches can be used to capture the equivalence of theories and models. If, say, two distinct theories have the same content, then there is a clear sense in which these representations can be understood as equivalent. Likewise, if content equivalence is not on the table, then substitutability or translation can be appealed to. For example the theories and models that appear in French and Russian editions of Darwin's work can for many purposes be treated as though they are interchangeable insofar as both are good translations of his works.

Identifying theories and models as equivalent in one of the above senses does not undermine the important ways in which theories and models formulated in different ways are distinct. It is commonplace for scientists to investigate the relationships between different theories and models to see if and to what extent they are equivalent. A famous example is von Neumann's proof of the equivalence of wave mechanics and matrix mechanics. In 1925 Heisenberg, Born and Jordan's matrix mechanics, and Schrödinger's wave mechanics both appeared on the scene. As Schrödinger (1978, 45) and von Neumann (1955, 5–16) observed, each quantum mechanical theory was developed from different starting points, incorporated different concepts and employed different methods. According to Schrödinger "the whole mathematical apparatus seem fundamentally different" (1978, 45) in each theory. Despite these differences, von Neumann even-

tually proved that these theories are equivalent in an important respect.[10] He identified functions from each theory that "enter most essentially into the problems of quantum mechanics" and took them to be the "real analytical substrata of the matrix and wave theories" (1955, 28). What von Neumann showed is that relative to a suitable set-theoretic interpretation each theory's models are isomorphic and hence the "two theories must always yield the same numerical results" (von Neumann 1955, 33). However by showing these theories are equivalent in this way, von Neumann did not change the fact that these theories were developed from different starting points, incorporated different concepts and employed different methods. It is precisely because of these differences that there was reason to investigate the relationship between them in the first place. Prior to von Neumann's proof there was no reason to be confident that each theory would make identical predictions. So by identifying each theory with distinct linguistic representations we are able to capture both the differences between them, which played such an important role in the historical development of quantum mechanics, and the relevant equivalence between them.

3.3.3 Does the linguistic approach favour scientific realism?

It may be worried that the linguistic approach presupposes, or at least favours, scientific realism. On the linguistic approach theories and models are often description-like linguistic representations. The worry, then, is that by construing them as description-like the linguistic approach closes off avenues towards anti-realist construal of science. This worry is generally unfounded. The linguistic approach, as a descriptive account of scientific theorizing and modeling practices, can be accepted by most realists and anti-realists alike.

The first thing to note is that by characterizing theories or models as linguistic representations, one does not thereby make any commitment regarding the truth, accuracy, or general success of the representation. Representation here is not being used as a success term. The assertion "Obama was a one term President" is surely a linguistic representation, but one that happens to be false and thereby misrepresentative. In this way, the linguistic approach is clearly amenable to anti-realist positions of an error-theoretic variety. According to an error-theoretic construal theories and models are truth-apt, however we are not warranted in believing in their truth. In this sense van Fraassen's constructive empiricism (1980; 1989; 2000) is an error theory. He claims we are warranted in believing theories are empirically adequate, but he denies that we are warranted in believing that they are true. Nevertheless he admits that truth is "a category that applies to scientific theories" and the "content of a theory ... is either true or false" (2000, 177). No conflict arises between constructive empiricism and the linguistic approach.

Other avenues are also available for the anti-realists whom adopt the linguistic approach. The linguistic approach is descriptive in its aims and not prescriptive. Consequently one may hold both that many

---

[10]Von Neumann (1955) credits Schrödinger with the original equivalence proof, however Muller (1996) rejects this claim calling it the "equivalence myth of quantum mechanics." He argues that at the time of Schrödinger's supposed proof the theories were neither mathematically nor empirically equivalent. The real proof came much later only after the "mathematical structures of matrix mechanics and wave mechanics were stretched, parts chopped off and novel structures added." (Muller 1996, 35)

theories and models are linguistic representations—this is just the kind of things they are—but nevertheless believe that they *ought* not be construed as truth-apt representations of the world. In this case one may adopt the linguistic approach as the best characterization of actual scientific practice, but believe an instrumentalist or non-representationalist attitude towards scientific theories or models is the most reasonable attitude to adopt towards them.

3.3.4 Can't the linguistic approach and the semantic view be reconciled by appeal to propositions?

Some have suggested that theories can be construed as sets of propositions (see for example Chakravartty 2007, 188). It may be tempting to think that this promises a way of reconciling the semantic view and the linguistic approach. Propositions are often construed both as abstract entities and truth bearers. Since the linguistic approach takes theories and models to be truth-apt, insofar as propositions are truth-bearing abstract objects then models and theories maybe identified with sets of abstract objects (qua propositions). This sounds similar to the semantic view which associates theories with sets of structures, where structures are construed as abstract, typically mathematical, entities. On both proposals theories are identified with sets of abstract objects. Despite these similarities propositions do not offer any path towards reconciliation.

The first thing to highlight is that the linguistic approach is not committed to the identification of a theory or model with a set of abstract objects, because it is not committed to a particular account of meaning, truth, or reference. It is committed only to the identification of a theory or model with a linguistic representation. Linguistic representations are presented when uttered sentences and equations are used to perform representative speech acts. As you will recall, an utterance can be used to perform a representative speech act only if the utterance (or its contents) is truth-apt, a potential object of doxastic attitudes, and sort of thing that can be offered as justification for inferences. We could additionally ask what makes an utterance meaningful, what its truth conditions are, what its referents are, or what its truth or falsity consists in. However these are further questions which the linguistic approach does not addressed. Answers to these questions are supplied by theories of meaning, truth and reference. Any such theory will be consistent with the linguistic approach so long as it allows for the possibility of linguistic representation—and this is a very minimal constraint. For example, one could adopt both a Fregean view of meaning and the linguistic approach. As a Fregean one would insist that an utterance is meaningful and truth-apt in virtue of expressing some proposition. The fact that an utterance expresses a proposition, then, would be part of the explanation why that utterance is capable of being used to perform a linguistic representative act. Alternatively, a proponent of the linguistic approach might instead adopt a Davidsonian view about meaning. On a Davidsonian view no appeal to propositions is made. Instead an utterance is meaningful and truth-apt when (using a language's truth definition) we can determine its truth conditions. In this case, it's the fact that an utterance has determinate truth conditions which explains why the utterance is capable of being used to perform a linguistic representative act. Both Fregean and Davidsonian views (and most others too), despite offering very different accounts of meaning, are compatible with the linguistic approach.

Now we could, as is common (see McGrath 2012), suppose that propositions are truth bearers and that they are expressed by representative speech acts. Propositions would then be truth-apt things expressed via acts of assertion, hypothesizing, claiming, conjecturing etc. In this case, it is consistent with the linguistic approach that theories and models be associated with sets of propositions. However, depending on what further account of proposition is supplied, this may or may not entail a commitment to abstract objects. It remains to be determined what kinds of things truth-bearers are and this is something that the linguistic approach simply does not address. Its neutrality on such issues means the proponent of the linguistic approach is free to endorse or reject proposals which construe truth bearers as mind-independent abstract objects. Should such a proposal prove to be the best account for propositions then the proponent of the linguistic account is free to adopt it. Since these are matters of general concern for theories of truth and meaning, the identification of theories and models with sets of mind-independent abstract objects will be warranted only if one is warranted in identifying all kinds of linguistic representations, occurring in scientific and non-scientific contexts alike, with sets of abstract objects. General considerations concerning language, truth and logic—not considerations specific to scientific representation—should determine whether propositions are best construed as abstract objects.

Even if the linguistic approach were to identify theories and models with sets of abstract objects, this would not suffice to reconcile it with the semantic view. One of the supposed virtues of the semantic view is that by associating a theory with a class of structures one gains a measure of language independence (see, for example, Giere (1988, 80) and van Fraassen (2000)). A consequence of this independence is that according to the semantic view notions like truth and reference are of little importance when accounting for scientific theorizing. A linguistic approach that identifies a theory with a set of propositions, no matter how the propositions are construed, is not language independent in this sense. Propositions, as the bearers of truth, play a part in an account of language's representative function and must feature centrally in accounts of truth and reference. Also (as will be further discussed in §4.1) structures and propositions have completely different functions. Where propositions are the bearers of truth, in model theory structures are truth makers. Sets of sentences are true in a structure relative to an interpretation. Notice that if one identifies a scientific theory with a set of structures, this means that the theory is not truth-apt. In this respect the linguistic approach is directly opposed to the semantic view and no reconciliation is possible.

Chapter 4

The Semantic View: Mediation and Demarcation

The semantic view can roughly be characterized as the view that scientific theories are collections of structures (extralinguistic, usually mathematical, abstract objects). As discussed in previous chapters, one of the key differences between the semantic view and the linguistic approach is that expressions employed by scientists as part of the discursive aspect of their theorizing practices are generally attributed different functions on each view. The semantic view holds that these expressions systematically play an indirect representational role while the linguistic approach hypothesizes that they regularly play a direct representational role. Expressions play only an indirect representational role because the semantic view identifies theories with structures. Expressions, then, only function as a means of specifying or describing the relevant structures and the structures (not the expressions) are used by scientists to represent physical systems. Consequently structures always mediate any representational relationship that obtains between language and physical systems.

It is this structure mediated account of representation characteristic of the semantic view that shall be the focus of this chapter. In the following section I shall explain this view's origins and motivations. This requires explaining—through the work of Suppes, van Fraassen and Giere—the semantic view's connections to, and ultimate divergence from, mathematical model theory. With its model-theoretic underpinnings exposed it is easy to recognize the semantic view's structure mediated account of representation as an innovation upon model theory. But in subsequent sections I shall argue that, although initially plausible, this innovation proves to be problematic in many respects.

4.1 Beyond Model Theory

The connection between the semantic view and model theory is arguably the clearest in the early work of Patrick Suppes. As one of the semantic view's originators he first proposed the semantic view as a way to enrich the "far too simple" (1967, 57) syntactic view of the positivists. One of the principal ways he attempted to sophisticate the syntactic view was by supplementing an axiomatic characterization of scientific theories with, as he called it, an *extrinsic* characterization. Extrinsic characterizations, unlike syntactic ones, focus on the 'models' of theories. Here 'models' are understood in the model-theoretic (or Tarskian) sense as structures that, relative to an interpretation, satisfy theories. In this sense structures are *possible realizations* of theories. By directly defining and investigating a theory's structures Suppes hoped to avoid some of the technical and practical problems that face a positivist's syntactically focused account of scientific theories.

Suppes' inspiration comes from the frequent practice in mathematics of using set-theoretic notions as a basis for providing a common semantics for theories in different branches of mathematics. Roughly, mathematical theories can be interpreted as theories about structures and structures can be construed as set-theoretical entities. Upon such interpretations "a possible realization of a theory is a set-theoretical entity of the appropriate logical type" (1967, 12) and "to axiomatize a theory is to define a predicate in terms of the notions of set theory" (1957, 253). To use one of his examples (1957, 250), the predicate 'is a quasi-ordering' can be defined in the following way:

> $\mathbf{U}$ is a quasi-ordering if and only if there is a set $\mathbf{A}$ and a binary relation $\mathbf{R}$ such that $\mathbf{U} = \langle \mathbf{A}, \mathbf{R} \rangle$ and
> Q1. $\mathbf{R}$ is reflexive in $\mathbf{A}$,
> Q2. $\mathbf{R}$ is transitive in $\mathbf{A}$.

The above is an axiomatization, in Suppes' sense, of the theory of quasi-ordering. This theory (and the predicate) is satisfied by the class of structures $\langle \mathbf{A}, \mathbf{R} \rangle$ that have the specified properties. Suppes' calls axiomatizations like the above *intrinsic characterizations* because each axiomatization delineates a class of set-theoretical entities by identifying intrinsic properties common to every structure in the class. In the above example the theory of quasi-ordering identifies two intrinsic properties (reflexivity and transitivity) of a binary relation defined on the structure's domain.

In contrast to the intrinsic/axiomatic presentation of the theory of quasi-ordering Suppes exploits the resources of model theory to argue that an extrinsic characterization can also be given. An extrinsic characterization is achieved by characterizing a theory's structures without identifying intrinsic properties common to all structures in the class. One way to do this is by directly defining each of the theory's possible realizations (1967, 60). When given a set-theoretic construal a structure is simply "a certain kind of ordered tuple consisting of a set of objects and relations and operations on those objects" (1969a, 13). And so a structure can be defined directly by specifying its objects, relations, and operations.

$U = \langle A, R \rangle$ where:
$\quad A = \{1, 2\}$
$\quad R = \{\langle 1, 1 \rangle, \langle 2, 2 \rangle, \langle 1, 2 \rangle\}.$

$U' = \langle A', R' \rangle$ where:
$\quad A' = \{4, 5, 6\}$
$\quad R' = \{\langle 4, 4 \rangle, \langle 5, 5 \rangle, \langle 6, 6 \rangle, \langle 4, 5 \rangle, \langle 5, 6 \rangle, \langle 4, 6 \rangle\}.$

Both U and U' are directly defined structures which also happen to satisfy the theory of quasi-orderings. A whole class of structures, $\{U''', U'''', \ldots\}$ can be defined in the same way. If every structure which realizes the theory of quasi-orderings is directly defined in this way, then a completely extrinsic characterization of the theory would be achieved. Of course direct definition is impossible when the theory has an infinite number of possible realizations. Another way to provide an extrinsic characterization of a theory is to "designate a particular model of the theory ...and then characterize the entire class of models of the theory in relation to this distinguished model" (1967, 60). For example there are an infinite number of structures isomorphic to U. This class of structures can be extrinsically characterized by defining U and defining an isomorphism (which is structure preserving bijective mapping between the domains of two structures). Note that because an isomorphism is a kind of relation that obtains between structures, 'being isomorphic to U' is an extrinsic property of a structure.

In most cases axiomatic and extrinsic characterizations of mathematical theories are complimentary. Both characterizations can be exploited through proof-theoretic or model-theoretic methods to investigate a theory and its structures. Complementarity is guaranteed when a theory is axiomatized using a sound and complete formal system like first-order predicate logic. As John Worrall puts it, "so far as logic is concerned syntax and semantics go hand-in-hand—to every consistent set of first-order sentences there corresponds a non-empty set of models, and to every normal ('elementary') set of models there corresponds a consistent set of first-order sentences" (1984, 71). And so a syntactic approach to the study of mathematical theories can be supplemented by an extrinsic one and the corresponding model-theoretic methods.

Suppes (1969a; 1969c; and 1967), responding to the syntactic focus of the logical empiricists, argues that scientific theories, just like mathematical theories, can be extrinsically characterized and such characterizations are as useful in the scientific cases as they are in the mathematical ones. Suppes' underlying reasoning is simple enough. Many scientific theories are expressed mathematically. These scientific theories, because of their mathematical expression, can be provided with the exact same semantics as their mathematical counterparts. So scientific theories can be interpreted as theories about mathematical structures and these structures can be construed as set-theoretic entities. Suppes offers the following example:

> We may axiomatize classical particle mechanics in terms of the five primitive notions of a
> set P of particles, an interval T of real numbers corresponding to elapsed times, a positions

54

function s defined on the Cartesian product of the set of particles and the time interval, a mass function m defined on the set of particles, and a force function f defined on the Cartesian product of the set of particles, the time interval and the set of positive integers. A possible realization of the axioms of classical particle mechanics, that is, of the theory of classical particle mechanics, is then an ordered quintuple $\mathbf{P} = \langle P, T, s, m, f \rangle$. (1969a, 13)

On Suppes' treatment the theory of classical particle mechanics can be extrinsically characterized by directly defining the relevant class of structures, $\langle P, T, s, m, f \rangle$.

However there is a problem. Mathematical theories and scientific theories are different in at least one important respect. On the face of it theories in mathematics are theories about mathematical objects (however mathematical objects are ultimately to be understood). Mathematical objects, like sets, are the putative objects which mathematicians seek to learn something about through their theorizing practices. In other words (given a set theoretic interpretation) sets are the intended domain of mathematical theories and sets are, therefore, the truth-makers for mathematical theories. The scientific case is different. Physical systems, and not mathematical structures, are often the systems that scientists are trying to learn something about. If one interprets the equations used to present a scientific theory as equations about set-theoretic objects, then something needs to be added to forge some connection between the theory and the physical world. When the equations are interpreted as being satisfied by set-theoretic objects, the model-theoretic analysis captures only the relationship between the equations and the class of set-theoretic objects which function as truth-makers for them.

Now it is a little unclear exactly how Suppes proposes to deal with this issue. I have elsewhere (McEwan 2006) considered various ways of interpreting Suppes on this question. It would be an unnecessary distraction to delve into such exegetical questions here. However I think he is most plausibly interpreted as holding a view very much in line with other advocates of the semantic view like van Fraassen and Giere. These authors all propose adding one (or more) layers to the account of representation. They maintain that the equations presented in the course of scientific theorizing are to be interpreted as defining or characterizing mathematical structures and these structures may additionally be taken to represent the physical systems which scientists use theories to investigate. It's the structure-system relation that connects scientific theories to real systems. And so a theory's language-like expressions characterize a class of structures and these structures may then be taken to represent physical systems—thus structures mediate between any linguistic presentations of a theory and the world.

Not only do advocates of the semantic view introduce mediating structures into their analysis, but they also emphasize these structures as being the most epistemically important features of their analysis. Van Fraassen believes that theories are first characterized (extrinsically) by directly defining a class of structures. It is possible to further characterize the theory linguistically, but "the language used to express the theory is neither basic nor unique; the same class of structures could well be described in radically different ways, each with its own limitations" (1980, 44). Since we start with a set of structures, any axiomatization of the theory in some formal language will be an "image" of the original "produced through the lens (which is more or less limiting or distorting) of the specific chosen language" (1985, 302). Consequently

"If the theory as such, is to be identified with anything at all—if theories are to be reified—then a theory should be identified with its class of models" (1989, 222). Structures, and not linguistic expressions, must be the primary vehicles of scientific knowledge. It is only via a theory's structures that undistorted access to the theory is possible. Likewise the accuracy, or empirical adequacy, of a theory must also be reflected in the relationship between its structures and its targeted physical systems. On the resulting picture "the link between language and reality is mediated by models" (2000, 192). The language-reality relation is

> . . . a very incomplete link without depriving the language of a complete semantic structure. The idea is that the interpretation of language is not simply an association of a real denotata with grammatical expressions. Instead the interpretation proceeds in two steps. First, certain expressions are assigned values in the family of models and their logical relations derive from relations among these values. Next, reference or denotation is gained indirectly because those model elements may correspond to elements of reality. (van Fraassen 2000, 192)

By severing a direct link between language and reality the semantic view is an account of scientific theories that "makes language largely irrelevant" (2000, 178).

Giere likewise emphasizes the epistemic importance of structures. The theory is most closely identified with a class of structures and the "the particular linguistic resources used to characterize those [structures] are of at most secondary interest" (1988, 79). The relationship between a theory's language-like expression and its structures is a definitional relationship. It is thus appropriate to speak of equations as true or false with respect to the structures they define, but Giere hastens to add that "truth here has no epistemological significance" (1988, 79). Equations deployed in the presentation of a scientific theory are true only trivially. Equations "truly describe a model because the model is defined as something that exactly satisfies the equations" (1988, 79). We see here again an expression of the idea that scientific theories are importantly independent from language.

By emphasizing structures in the way van Fraassen and Giere do, however, another problem arises. Theories are normally thought to be the sorts of things that can be true, false, believed or doubted. Structures on the other hand may be similar or dissimilar to some physical system, but structures are not the kinds of things that are truth-apt, nor are they the objects of doxastic attitudes. It is simply a category mistake to claim that an abstract mathematical object, like a number or a set, is true. This problem is a result of the semantic view's subtle but significant departure from the standard model-theoretic account. On a model-theoretic treatment structures are the truth-makers for the sentences used to express the theory. By identifying a scientific theory with structures, the semantic view is thereby identifying the theory with the objects that normally function as a theory's truth-makers.

Recognizing the problem Giere offers the following solution—a solution which van Fraassen (2000, 109) now also endorses. Giere introduces *theoretical hypotheses* to the account of scientific theories:

> Unlike a model, a theoretical hypothesis is . . . a *linguistic* entity, namely, a statement asserting some sort of relationship between a model and a designated real system (or class of real

systems). A theoretical hypothesis, then, is true or false according to whether the asserted relationship holds or not. The relationship between model and real system, however, cannot be one of truth or falsity since neither is a linguistic entity. (1988, 80)

He maintains his emphasis on structures ('models' in his terminology) as the true embodiment of a theory, but adds theoretical hypotheses as an additional element which makes explicit the representational significance of the structures for a specific class of physical systems. Consequently a scientific theory is a kind of conglomerate consisting, first, of a population of structures and, second, various hypotheses linking these structures with physical systems (1988, 85).

It should now be clear that with Giere's refinement the semantic view cannot be construed as a simple application of model theory to scientific theories. Though it is inspired by model theory, the semantic view is divergent in many respects. The most significant divergence is the mediating role attributed to structures: structures function in a dual role as truth-makers for expressions and as representations of physical systems. It is this mediating role that supports the claimed language independence of the semantic view and necessitates adding theoretical hypotheses to the account of scientific theories.

4.2 The Dilemma of Mediation

The semantic view's structure-mediated account of representation does not appear to be the simplest possible account of scientific theorizing practices. When sentences and equations are used to present theories the semantic view affirms that these expressions are used in a representative capacity, but nevertheless denies that they are used to directly represent the physical systems that sciences investigate. This raises the question: what is gained by interposing extralinguistic structures between expressions and physical systems? We know that scientists often present theories using sentences and equations, and we know that they use theories to make predictions and explain the behaviour of physical systems. How could adding a mediating layer of representations and appealing to abstract objects for this purpose do anything more than complicate matters? After all structures cannot be inspected, poked, or measured.

By introducing mediating structures proponents of the semantic view claim that it acquires a kind of language independence. It is this independence that purportedly allows the semantic view to escape problems which (allegedly) plague the syntactic view (see for example van Fraassen (2000, 178–179) and Giere (1988, 79–85)). I take it the basic idea here is that some kind of language independence is necessary to avoid the obvious mistakes of the past, and the semantic view achieves the necessary independence by making use of the model-theoretic tools so well understood and ready at hand. Now I will have more to say about language dependence below in connection to some of the specifics of the proposals tabled by Giere and van Fraassen. But I hope now, given the virtues of the linguistic approach outlined in chapter 2, it seems dubious to simply presume that language independence is a virtue of an account of scientific theories. Moreover as I argued above (see §3.3.1) the linguistic approach, though in no way language independent, does not fall victim to the same problems of the syntactic view. This suggests that the

syntactic view's difficulties may not arise from its 'language dependence,' but rather from other doctrines unique to the particular brand of empiricism that the syntactic view was designed to service.

On the other hand there are reasons for worrying that the introduction of mediating structures only leads to complications and problems. For one, by introducing structures into their account of scientific theorizing it seems the semantic view exposes itself to familiar puzzles concerning abstract objects. How then can scientists know if, for example, their linguistic characterizations of structures are true? Or how can they determine what kind of relations structures have to physical systems? Another worry about the mediating thesis is that by adding an intermediary level of representation the semantic view simply has twice as much work to do. They have two representational relationships to account for: one between language-like expressions and structures, and the other between structures and the physical systems. Even if we were to accept that the semantic view is language independent in some sense, it certainly cannot wash its hands of language all together. As van Fraassen himself admits "to present a theory, we must present it in and by language . . . for any effective communication proceeds by language" (2000, 178). So the semantic view must still be able to provide some account of the role of language in theorizing and modeling practices. However, as Halvorson (2012; 2013) has recently argued, it may not be so clear that model theory really can be relied upon to provide an account of theories that is interestingly language independent.

Given all this, it is important that a more thorough consideration of the semantic view's structure-mediated account be undertaken. Again given the structure mediated representational account, according to the semantic view there are (at least) two important relationships that structures enter into when theorizing has an essential discursive component: (1) structures are related in some way to the sentences and equations that sciences use to present theories, and (2) structures are related in some way to the ultimate representational targets of theories, which often are physical systems whose behaviour sciences attempt to explain and predict. Given my interest in this work on the discursive aspect of theorizing practices, it is the first of these, the *expression-structure relations* as I shall call them, that shall be the focus in the remainder of the section. As I will now discuss below, there are a couple of different ways in which the expression-structure relationship is explicated by proponents of the semantic view. Roughly, upon one explication expressions are used to (stipulatively) *define* structures and upon the other explication expressions are used to describe or characterize them. Corresponding to each approach the language-independence of theories is understood differently. I shall argue the choice between approaches is a choice between two different horns of a dilemma with problems facing the semantic view whichever horn is chosen.

4.2.1 First Horn: Triviality

Giere (1988, 78–82) is a good example of someone who adopts a definitional approach when explicating the relationship between uttered expressions and structures (i.e., 'models' on Giere's terminology). As he says the "relationship between some (suitably interpreted) equations and their corresponding model may be described as one of characterization, or even definition." Consequently the "equations truly describe

the model because the model is defined as something that exactly satisfies the equations" (1988, 79).[1] Provided the uttered set of equations is consistent, by uttered sentences and equations associated with the theory (with the exception of the theoretical hypotheses) are necessarily true of the corresponding structures. This definitional relationship is a representational one (at least according to my regimented sense of 'representation'). Since the equations "truly describe" (1988, 79) a structure, the equations are used to represent the structure as having certain features. In other words, the structures have the features they are described as having. Moreover, by truly describing structures the uttered expressions facilitate surrogate reasoning about the structure. A competent user can use the uttered equations to draw specific inferences about the structures.

This approach has a couple of consequences. First a happy consequence: it defuses many of the potentially vexing epistemological problems associated with abstract objects. It is no mystery scientists are able to come to learn things about these abstract objects because by definition scientists know that the structures in question have exactly the properties they are claimed to have. The accuracy of the scientists' linguistic representation of the structures is guaranteed. This, again, is why Giere takes equations to be true, but denies that their truth has any epistemological significance (1988, 79).

There is a cost to taking a definitional approach when understanding the expression-structure relation. In this case the proponent of the semantic view seems to guarantee that structures are dispensable. If structures have only those properties they are defined as having, then there is in principle no problem eliminating structures from the analysis. Any inferences that can be drawn about the target system from the structures can also be drawn from the expressions that define them. Since the relation between a theory's language-like expressions and the satisfying structures is trivialized, nothing is knowable about the structures beyond that which can be inferred from the information encoded in their definitions. In other words, the theory—whether identified with its linguistic expressions or with the class of structures it defines—has no content beyond that expressed via the defining sentences and equations. The worry, then, is that it now seems (at best) pointless to insist on introducing and emphasizing mediating structures when one can work directly with the linguistic representations required to define the structures.

Two sorts of responses to this worry may seem appealing to Giere and others. Giere could argue that even if in principle structures could be eliminated from the analysis of scientific theories, (1) it would be impractical, or difficult to do so, or (2) it is a matter of fact that scientists do interpose structures and therefore it would be a misrepresentation of scientific practice to eliminate structures from the analysis.[2] Both these responses rest upon empirical claims about scientific practice. Response (2) is plainly a claim

---

[1]Given Giere's (1988) examples and other unqualified comments concerning the application of the semantic view to scientific theorizing and modeling practices, I take it that Giere believes most equations uttered by scientists in theorizing and modeling contexts (especially applied cases) are "suitably interpreted." After all his analysis can only be applied in cases where equations are interpreted. Purely syntactic expressions that are uninterpreted (or only partially interpreted) do not define or characterize any models, and thus they cannot be used to specify the models that, according to Giere, a theory is to be identified with.

[2]Claims along the lines of (1) and (2) are hinted at by Giere (1988, 62–91) but are not made explicit or thoroughly defended. For example he has a lengthy discussion of the different kinds of models (like the linear harmonic oscillator, the damped harmonic oscillator, and the simple pendulum) that appear in introductory physics textbooks. Following this discussion he proposes that "we regard" (1988, 78) these models as abstract entities (i.e., as structures in my terminology). The implication is that, somehow,

about what scientists do when theorizing. Response (1), at least in its strongest form, also rests upon a claim about scientific practice so long as we can expect that scientific practices will typically reflect those practices which are the most practical or expedient. If interposing mediating models was actually an easier means of representing physical systems, then we should expect practically minded practitioners to favour a structure-mediated mode of representation. And so for responses along the lines of (1) and (2) to be compelling, evidence in support of them must be gathered from scientific practice. My full rebuttal to (1) and (2), then, will have to wait until chapter 7. In that chapter I identify what counts as evidence for and against the semantic view's structure mediated approach, and argue that on one of semanticist's own favoured examples there is little evidence in favour of the semantic view.

Giere also suggests some interesting theoretical reasons for including mediating structures as a central feature of his semantic view. He forwards his version of the semantic view as part of a larger project which aims to provide a naturalistic account of scientific theorizing by drawing upon cognitive science. He calls it a cognitive theory of science. Unfortunately the details of the supposed connection between the semantic view and cognitive science are not fleshed out by Giere in great detail. However, from the few comments interspersed in Giere (1988) and elaborated upon in Giere (1994) his reasoning seems to be something along the following lines. Scientific theories are representations constructed by scientists and owing to their human origin they are likely to be similar in kind to the mental representations studied by the cognitive sciences. Schemata, cognitive maps, mental models and frames are all different kinds of mental representations which are increasingly featured in theories of cognition (1988, 6). These kinds of mental representations are "not generally described as being true or false, but as fitting the world" and "accumulating evidence from the cognitive sciences . . . suggests that human cognition and perception operate on the basis for some sort of similarity" (1988, 6, 81). Therefore Giere suggests that scientific theories are best not construed as true or false, but instead represent like mental representations via some sort of similarity relation. Since linguistic entities do not represent in virtue of similarity, to maintain a close connection to cognitive science the emphasis must then be placed on mediating structures which are properly construed as similar or dissimilar to physical systems.

I am sympathetic to Giere's desire to have an account of scientific theories cohere with our best theories from the cognitive sciences. However even accepting coherence with cognitive science as a desideratum for an account of scientific theories, I think such coherence puts very few constraints on what an account of scientific theorizing must look like. The first thing to note is that the cognitive sciences can hardly be said to speak univocally about the nature of mental representation and cognition. Cognitive science includes many different disciplines and within each the debate continues regarding the structure and functions of mental representation.[3] So it seems premature to conclude that no mental representation is language-like or that all mental representation represents via similarity.

More importantly, the studies of mental representation and scientific representation investigate phe-

there is something about these models which gives us reason to regard them as abstract structures, but as far as I can tell he does not explain why. For my part, as illustrated by my treatment of the simple pendulum in chapter 2, I take these same examples to support the linguistic approach and I do not think they give us reason to posit mediating structures.

[3]For an overview of some of these debates see Thagard (2005).

nomena exhibited at different levels. The kinds of representations Giere discusses—schemata, frames, mental maps etc.—are introduced as parts of cognitive theories that attempt to account for mental phenomena (i.e., mental representation and its role in the cognition). Mental phenomena are exhibited by individuals and, thus, are phenomena at an *intra*personal level. By contrast scientific theorizing is a social representative activity. It is implicated in communicative and epistemic practices which inhere in groups of scientists. Moreover theorizing makes use of language-like expressions and this communication relies on social norms and conventions.[4] These norms and conventions are features of linguistic communities not of individual language users. So an account of scientific theorizing must capture phenomena that inhere at a social, or *inter*personal, level. By drawing upon theories of mental representation Giere is thus attempting to assimilate phenomena exhibited at an interpersonal level with mental representation and cognition which are phenomena exhibited at an interpersonal level. However Giere does not provide reasons for thinking this move is warranted.

Of course there are likely important relations between mental representation and language-like communication, but the exact nature of these relations is an open question. We should not be too quick to assume that public representations, like scientific theories presented via sentences and equations, are identical in structure and function to mental representations. Since scientific theorizing is both a practice embedded in communicative practices governed by social conventions and it takes place against a background of social epistemic norms, it may only be possible to understand scientific theorizing as a social activity. That is, it may not be possible to understand what the contents of scientific theories are or how they come to have their representational significance without understanding their connection to broader social practices. If this is correct and the social aspects of scientific theorizing are essential aspects, then there is no reason to suppose that theories of mental representation can adequately capture scientific representation too. Moreover by assimilating scientific and mental representation Giere may be prematurely closing off a potentially fruitful investigation of the connections between the study of scientific theories, philosophy of language, linguistics and social epistemology.

By contrast the linguistic approach does not presuppose any particular relation between the nature of mental representation and linguistic representation. Moreover the linguistic approach is well equipped to capture the social aspects of scientific theorizing because it identifies scientific theories as linguistic representations. Linguistic representations are public and, in virtue of employing language-like expressions, are immediately embedded in the communicative and epistemic practices integral to all complex communication. These are themes which I shall return to in chapters 6 and 7 where I consider in more detail what is and is not evidence that expressions are being used to directly represent physical systems.

To summarize, then, Giere adopts a definitional approach where structures are straightforwardly defined by equations, meaning the structures have only those features they are defined to have. The problem with such a definitional approach is that the mediating role of structures is trivialized leaving the structures to play an ineffectual, and thus eliminable, role in his analysis of scientific theories. I've argued

---

[4]I noted in chapter 1 the formation of equations (whether properly 'linguistic' or not) is governed by conventionally specified syntactic rules, and these rules support a distinction between well- and ill-formed expressions. Moreover mathematical symbols regularly have a communicative significance that speakers can use to help convey their communicative intentions.

that appealing to cognitive science and theories of mental representation do not provide good reason to retain eliminable mediating structures in the analysis of scientific theories. Arguably the best reasons for retaining structures in the analysis of theories and models would be empirical: one might justify including mediating structures if there was clear evidence that scientists systematically represent indirectly via structures. Giere has not provided such evidence and in chapter 7 I shall muster countervailing evidence that indicates, instead, that scientists do not systematically interpose mediating structures.

4.2.2 Second Horn: Knowledge of Abstract Objects

In contrast to Giere's definitional approach, the proponent of the semantic view can take structures to have an independence that makes it possible for structures to have features which our language-like characterizations of them may not capture. I think van Fraassen is most plausibly interpreted as choosing this route.

In some places van Fraassen follows Giere and talks about mathematical expressions as defining structures (e.g., 1989, 222). However in other places he makes it relatively clear that he has a 'looser' representational relationship in mind. He notes, for example, that many laws which feature prominently in theories (like Schrödinger's equation, or the Hardy-Weinberg law of population genetics) are just "partial descriptions of special subclasses of models" (2000, 180). More generally, the "scientific literature on a theory makes it relatively easy to identify and isolate classes of structures to be included in the class of theoretical models" (1989, 224). So rather than precisely define the class of structures associated with a theory, the various descriptions of structures provide a good handle on which structures are and are not of interest. Van Fraassen also claims that a linguistic formulation of a collection of structures is merely an "image …produced through the lens (which may be more or less limiting or distorting) of the specific chosen language" (1985, 302). The structures are not strictly defined by a set of uttered expressions, otherwise these expressions could never be distorting. This means structures are, in some sense, independent of the sentences and equations that scientists use to represent them and it is because of this independence that structures cannot be eliminated from an adequate account of scientific theories. Unlike the definitional approach, there is no guarantee that inferences about the target system drawn on the basis of features of the theory's structures can equally be drawn from any description of these structures. The structures may have unrepresented or misrepresented properties that are pertinent when drawing inferences about some real system.

On this non-definitional version of the semantic view the function of sentences and equations is, roughly, to characterize the structures well enough to distinguish or isolate most of the structures that are of interest. This expression-structure relationship is not the tight definitional representational relationship that Giere proposes. Expressions are used to partially describe, or creating an image of, the structures that ultimately constitute scientific theories and models. This makes the expressions function in representational capacity. Expressions are used to represent a theory's structures as having certain properties and by doing so they allow the competent user to draw inferences about the distinguished class of struc-

tures. Nevertheless it's possible for the linguistic representation of the structures to be incomplete and unrepresentative in certain ways.

Why might it be appealing to attribute to abstract structures this kind of independence? We shall consider the details of van Fraassen's model-theoretic argument for this position below. But first, I think we can see some of the appeal of this kind of move by considering the analogy with physical representations. By physical representations I mean representations that are straightforwardly real physical objects with spatiotemporal properties. These include things like scale replicas. Like any physical system, a physical representation's properties are not determined by our representations of them. Moreover we can investigate the physical representation itself and come to learn new things about it. For example, a scale replica airplane can be put into a wind tunnel to learn about its aerodynamical properties. In this sense, we can be said to have *representation independent access* to the physical representation: it is possible to learn things about it which may not be inferred from any particular representation of it. *Language-independent access*, then, is a related but more restricted notion. We have language-independent access to a structure when it is possible to learn things about the structure which may not be inferred from any particular linguistic representation of it. For example the scale replica airplane can be represented in different ways—it can be photographed, described or represented mathematically—but none of these representations are likely to capture all of the properties of the replica. Moreover some of the unrepresented properties are likely to be relevant when using the replica to predict or explain phenomena associated with the target system. For this reason when, say, trying to draw inferences about the aerodynamic properties of a full sized airplane, a linguistic representation of the replica (though useful for some purposes) is no substitute for the replica itself. The physical replica can be used to draw inferences about the full scale airplane that will not be available to someone working only with a description of the replica. In this case the physical representation is used to directly represent the target system (the full sized airplane) and the description of the representation, at best, only represents the target system via the intervening physical object. If structures can play a representative role analogous to that played by physical representations, then structures can play a non-trivial mediating role.

Positing abstract structures with a similar independence does avoid the triviality associated with the first horn of the dilemma. However familiar issues concerning abstract objects reappear. The semantic view is now committed to the existence of independent abstract structures which scientists are somehow able to access and exploit as representations. This means scientists must be able to learn things about structures and their relations to their representational targets, otherwise scientists would be unable to use structures for predictive and explanatory purposes. But here the semantic view is in a tough spot. On the one hand structures must have an independence from our representations of them (*à la* physical representations) so that structures can function in a robust ineliminable representative capacity. On the other hand structures must be accessible so that scientists can exploit structures for predictive and explanatory purposes. So what the semantic view needs in this case is an account of how scientists have representation independent access to structures. Such an account must contend with the abstractness of structures. Unlike physical representations, we cannot see structures, poke them, break them, or put them in wind tunnels. To my knowledge neither van Fraassen, nor any other proponents of the semantic view, directly address the

problems associated with abstract objects. In the absence of such an account the semantic view's policy of positing mediating abstract structures leaves scientific theorizing inexplicable. The semantic view insists on identifying theories with independent structures, but it offers no account of how scientist can actually use these abstract objects for such representative purposes.

van Fraassen's Model-theoretic Argument

As I mentioned van Fraassen doesn't directly address these problems associated with abstract objects. Nevertheless I think he does give some indication as to why he takes the semantic view's appeal to independent abstract structures to be unproblematic when he discusses the supposed language independence of the semantic view. Remember van Fraassen claims that language is a lens that provides a distorted view of a scientific theory (1985, 302). In his (2000) paper he outlines an interesting model-theoretic argument in support of this claim. Now I think grounds for believing language is distorting could potentially also be grounds believing that representation independent access to structures is possible. Why? Well in order to know that a linguistic representation is distorting one must have some way of comparing a linguistic representation and the structure(s) it targets, and this implies that one has some access to the structures (i.e., we have some means of learning what they are like) that is not derived from the linguistic representation being evaluated. So it may be hoped that van Fraassen's model-theoretic argument could be marshalled to show that scientists do (at least sometimes) have representation independent access to structures. If his argument fits the bill, then the proponent of the semantic view may have grounds for positing mediating structures even if they don't have a well-developed explanation of how scientists come by this representation independent access.

Here is van Fraassen's argument:

> . . . when a theory is presented by defining the class of its models, that class of structures cannot generally be identified with an elementary class of models in any first order language. The reason is found in the limitative meta-theorems, which brought to light the dark side of completeness. To take only the most elementary example, if a scientist describes a class of models, the mathematical object he is most likely to include is the real number continuum. There is no elementary class of models of a denumerable first-order language each of which includes the real numbers. As soon as we go from mathematics to metamathematics, we reach a level of formalization where many mathematical distinctions cannot be captured—except by fiat, as when we speak of 'standard' or 'intended' models.[5] (2000, 190)

Let us unpack this argument. Suppose some theory T is "presented by defining the class of its" structures. Call this set of structures, **M**. We can axiomatize T. Take the set of sentences $T_{\mathscr{L}}$ in some formal first-order language $\mathscr{L}$ to be an axiomatization of T in $\mathscr{L}$. The axiomatized version of the theory will admit

---

[5]What van Fraassen refers to as 'models' here are 'structures' in my terminology.

some set of structures **N**, which in the usual model-theoretic fashion satisfy these sentences. If we have done a good job of axiomatizing T then at least $\mathbf{M} \subset \mathbf{N}$. The structures in **M** will also be models of $T_{\mathscr{L}}$. However **N** will also include unintended structures. **N** will have members not included in **M**. In van Fraassen's example he stipulates that all of the members of **M** have the real numbers (or some structure isomorphic to the real numbers) embedded in them. Consequently **M** contains only structures with an uncountably large cardinality. Now the (downward) Löwenheim-Skolem theorem guarantees that any countable first-order theory which is satisfied by a structure with an uncountable cardinality will also be satisfied by a structure with a smaller countable cardinality. So **N** includes both countable and uncountable structures. Therefore the axiomatization is satisfied by unintended models with a smaller cardinality. In general, provided the theory makes reference to structures complex enough to include the real numbers, **M** will not be an elementary class of models. There will be no countable set of first-order sentences that uniquely admit the members of **M** and only the members of **M** as satisfying structures. We can stipulate that within **N** there are 'intended models' (the models in **M**) and 'unintended models' (every member of **N** not also in **M**), but in doing so "we certainly have not come up, in our axiomatic theory, with a new and precise ... description of **M**." (1985, 302) The axiomatization $T_{\mathscr{L}}$ is an 'image' of the original content **M** which is, more or less, distorted by our choice of $\mathscr{L}$, and the sentences that we choose to constitute $T_{\mathscr{L}}$. Van Fraassen's main thrust is that, "if after formalization, we discard the original [i.e., the structures **M**] and concentrate solely on the isolated abstract pattern [the sentences $T_{\mathscr{L}}$]..., we shall have lost the theory" (1985, 302). Since the original structures, the truth bearers of the theory's content, cannot be discarded without loss, the emphasis must be placed on the structures alone.

How could van Fraassen's argument be connected to the issue of representation independence access to structures? Well whenever we are in a position to recognize that an axiomatization of a theory is satisfied by unintended structures we must have some information which allows us to distinguish between intended and unintended structures. Obviously the axiomatization itself cannot be the source of this information since the axiomatization provides no means for distinguishing between them—this, after all, is why axiomatizations that admit unintended structures are thought to be deficient in this regard. Consequently we must have some information about the theory's structures sufficient to distinguish between intended and those unintended structures that is accessible prior to the theory's axiomatization. For example, suppose we were to try to specify a countable set of axioms in a first-order language that is satisfied only by the real numbers and structures isomorphic to the real numbers. The Löwenheim-Skolem theorem tells us we will fail in our efforts because our theory will be satisfied by countable structures not isomorphic to the real numbers. The key to making this negative evaluation of our axiomatizations of arithmetic is some prior knowledge of the real numbers. We know enough about the real numbers to recognize when other structures are importantly dissimilar to them, and it is exactly this kind of knowledge that is required to distinguish between those structures which are intended and those which are not. It seems this knowledge, however we came by it, cannot be encoded in any first-order linguistic representation of the real numbers. Hence we have some representation independent knowledge of the real numbers.

However the above argument does not quite go through. There is an unwarranted step at the end. Van Fraassen's example successfully shows that, at least sometimes, we must have information about struc-

tures that is not encoded in any linguistic representation expressed in a first-order formal language. But representation independence does not necessarily follow. The reason is simple enough: the information needed to distinguish between intended and unintended structures may be encoded in another representation. Suppose, for example, that a mathematical theory (i.e., a theory about mathematical structures) is first presented in natural language. In this case all van Fraassen's model-theoretic argument shows is the dangers of substituting the original linguistic representation expressed in natural language for some reconstruction expressed in a formal first-order language. Where the original linguistic representation expressed in natural language may have the resources to capture the difference between countable and uncountable structures, the first-order reconstruction of the theory will not. The natural language theory is not "language independent" in any interesting way because its contents are still exhausted by the contents of the expressions used to present the theory in the first place. It is only "language independent" in an extremely narrow sense: it is not expressible without distortion by a countable set of sentences in a first-order formal language.

The message I think we should take away from van Fraassen's model-theoretic argument is that reconstructing theories will often lead to distortions. To avoid this we need to pay special attention to theories *as originally presented*. If a theory is to be identified with anything it should be the representation that is immediately presented by scientists. Which kinds of representations are immediately presented is an empirical question about scientific practice. By identifying theories with abstract independent structures van Fraassen's semantic view seems to require that scientists, somehow, present the raw unrepresented structures themselves. While this may be possible for other kinds of representations—I can for example present a scale model airplane to you by pulling it out of the drawer and bringing it before your eyes— the abstract nature of structures makes it rather implausible that structures can be presented in this way. Moreover, van Fraassen and others do little to explain how it could be possible. By contrast the linguistic approach offers a very plausible account of how theories are presented. Theories are presented in the same way that descriptions are, by uttering a series of sentences and equations. The only sure way to avoid distorting theories and theorizing practice, then, is to identify the theory with the linguistic representations actually presented by scientists through these utterances.

4.3   The Semantic View and the Linguistic Demarcation Problem

In the above section I highlighted problems inherent in the semantic view's multi-stage, or mediated, view of scientific theorizing and modeling. In this section I want to consider some of the problematic implications of the semantic view's selective application of their mediated view. Roughly, by taking this account to apply to scientific theorizing practices I shall argue that the semantic view introduces a problematic discontinuity between scientific and non-scientific theorizing, or an intra-scientific discontinuity between mathematized and non-mathematized theorizing.

The problem I have in mind can be understood in connection to the linguistic demarcation problem introduced in §3.1.2. There I argued that the linguistic demarcation problem is one faced by the semantic

view and nonrepresentationalism, but not by a linguistic approach. Both reject the continuity thesis and thereby suppose expressions systematically function in a different capacity in, roughly, scientific contexts than in non-scientific contexts. Let me now take up the linguistic demarcation problem and its implications for the semantic view in more detail. Though I will not attempt to argue that the semantic view cannot possibly offer a solution to this problem, by considering the most initially plausible demarcation principles amenable to the semantic view I will highlight issues that are likely to plague the semantic view no matter how its proponents attempt to deal with the problem.

Recall that the linguistic demarcation problem can be contrasted with the traditional demarcation problem of science. Where the latter concerns the epistemological distinction between science and pseudoscience, the linguistic demarcation problem concerns the role played by expressions in the discursive aspect of theorizing and modeling practices. By rejecting the direct representation thesis the semantic view implies that in scientific practices language-like expressions are not performing the direct representational function that they characteristically play in everyday explanatory and predictive practices. In this way the semantic view implies that there is a substantive distinction between the way expressions are used in everyday contexts and the way they are used in those cases that are purportedly captured by the semantic view. Insofar as the semantic view is a view about scientific theories, then the selective application of the semantic view to account for scientific theorizing implies a substantive distinction between the way expressions function in the discursive aspect of scientific theorizing practices and the discursive aspect of non-scientific linguistic representative practices. To provide a solution to the linguistic demarcation the proponent of the semantic view must provide some account of this purported discontinuity. An adequate account must provide a reasonably clear principle of demarcation, which offers a means of distinguishing between cases in which expressions can be expected to function in the way specified by the semantic view (indirect representation) and cases in which they can be expected to function in the usual direct representational capacity. An account of the discontinuity should also provide some means of justifying the demarcation principle specified. The demarcation principle should not be an unprincipled one established by fiat.

The semantic view attributes an indirect representational function to the expressions used to present theories. Again on the semantic view the sentences and equations presented by scientists in the course of theorizing and modeling represent abstract structures and these abstract structures in turn represent the ultimate representational targets of these practices. In applied cases this means that the sentences and equations presented by scientists in the course of theorizing represent abstract structures and these abstract structures in turn represent real systems. In this case the linguistic demarcation problem challenges the semantic view to delineate between those cases in which linguistic expressions are functioning in a direct and indirect representational capacity. Without an account of this distinction the intended scope of their view is indeterminate. The proponent of the semantic view cannot distinguish which predictive and explanatory practices should be construed in their preferred way (*viz* indirect representation) and which practices should be construed in other ways. Of course if no evidence of a robust distinction can be found, then the semantic view's special treatment of scientific theorizing practices may be unwarranted. If discursive practices are typically no different in kind across scientific and non-scientific domains, then

there is no reason to suppose that scientific theorizing warrants a special account, which includes positing mediating structures.

Again, since the semantic view is described as an account of scientific theories (e.g., see Giere (1988, 82–87), and van Fraassen (2000)) and its proponents never explicitly delineate a more restricted domain of application, it would seem that proponents of the semantic view take their account to apply to all examples of theorizing in science. This implies a demarcation between direct and indirect representation that falls somewhere near the border between scientific and non-scientific theorizing practices. But I don't think the proponent of the semantic view should be happy relying on some inchoate distinction between science and non-science. It is notoriously unclear and it does not provide an obvious explanation why discursive practices would be different in each.

Given the semantic view's model-theoretic inspiration, I think the most initially plausible demarcation principle amenable to the semantic view is one that appeals the the distinction between mathematical and non-mathematical expressions. In the sciences an increasing number of theories are expressed mathematically. When we have the semantic view's model-theoretic roots in mind, it does seem fairly plausible to suppose that these cases of applied mathematics can be construed in the same way as their counterparts in pure mathematics: mathematically expressed scientific theories can be construed like pure mathematical theories as being satisfied by mathematical structures. If the distinction between mathematical and non-mathematical expressions is used as a demarcation principle, then the semantic view's structure-mediated account would naturally apply to all and only those scientific theories which are expressed mathematically. Non-mathematical theories (i.e., theories expressed using non-mathematical sentences) could then be accounted for in a different way. Moreover if we were to judge only by the examples chosen to illustrate the semantic view, one would think that this demarcation is already, at least tacitly, appealed to by many advocates of the semantic view. For example, in their explicit discussions of the semantic view both van Fraassen (1980, 41–68; 1989, 217–232; 2000) and Giere (1988, 62–91; 1994) exclusively use examples from physics. Moreover Suppes' most well worked out examples are from physics (1969a, 13), quantitative studies of learning theory (1969c), and from measurement theory (1969b). These are all theories that are expressed mathematically.

How does the distinction between mathematical and nonmathematical expressions fare as a solution to the linguistic demarcation problem? One worry, of course, is that the distinction between the mathematical and the non-mathematical is too vague or imprecise to do the work required of it. Mathematical expressions and concepts permeate natural language. Talk of numbers, sets, lines, planes, etc., is common place in otherwise non-mathematical settings. It is not obvious, for example, what makes number concepts distinctly mathematical. Moreover, as discussed above in connection to the population growth model in §3.1.1, it seems possible to express most mathematical equations in grammatical English. For example "$\mathbf{F} = m\mathbf{A}$" can often be expressed by "The force is equal to the mass times the acceleration of an object." If indeed the same proposition is expressed by each expression, it is hard to see what other than the symbols and surface grammar marks one expression as mathematical and the other as non-mathematical.

Even if we suppose the distinction between mathematical and non-mathematical expressions is neither

problematically vague or imprecise, evoking it as a demarcation principle introduces the problem of *intra-scientific discontinuity*. In contrast to heavily mathematized disciplines like physics, other disciplines (e.g., some branches of biology) make less use of mathematics. In these cases it is common to present theories without presenting equations or employing any other expressions that are explicitly mathematical. If the distinction between mathematical and non-mathematical expressions marks the border between direct and indirect uses of language, then even within scientific practices expressions are being used in drastically different ways. On this solution to the linguistic demarcation problem the semantic view entails a discontinuity in theorizing practices across different scientific disciplines and contexts.

There are, broadly, two ways that the proponent of the semantic view might respond to the threat of intra-scientific discontinuity. They may accept it, or they may try to extend their analysis of scientific theorizing beyond the mathematical cases. Each response is problematic in its own way.

Suppose that the proponent of the semantic view affirms that there is a fundamental difference between the way scientists represent their subjects in mathematical and non-mathematical branches of the science. On such a view when classifying scientific theories (and models), all mathematical theories fall in one category. These theories are to be understood in the way prescribed by the semantic view, where structures are posited as mediating the representational relationship between equations and a theory's representational target. Other scientific theories fall in another category, presumably along with most non-scientific representative practices. These representative practices should not be given a model-theoretic gloss because they are presented non-mathematically.

This proposal has a few unhappy consequences. First, it greatly reduces the scope of the semantic view. The semantic view of scientific theories is not an account of scientific theories as such, but an account of theories in branches of science that make heavy use of mathematics. This also means that other philosophical positions that rely upon the semantic view are likewise reduced in scope. Many structural realists, e.g., Ladyman (1998), and French (2003), make heavy use of the semantic view as a means of explicating and defending their views. Similarly van Fraassen's (2008) latest formulation of constructive empiricism, so called *structural empiricism*, is similarly tied to the semantic view. By limiting the scope of the semantic view, then, their respective realist and empiricist views may likewise need to be limited to those cases in which mathematics is used in the sciences.

More troubling are mixed presentations of theories, where scientists switch back and forth between mathematical and non-mathematical expressions when presenting a theory. In chapter 2 I introduced an example of a population growth model. One of the observations I made about the model's presentation is that the author moves line-by-line back and forth between mathematical expressions and English expressions. In fact the author embeds mathematical expressions within English sentences. I take this to be the norm and not the exception. Most presentations of theories and models, whether printed in journals and textbooks, or presented to an audience with the aid of a white board, involve this intermingling between mathematical and non-mathematical presentations. If mathematical and non-mathematical expressions are understood as representing in different ways (indirectly and direct), it is not clear what the proponent of the semantic view can say about these mixed presentations. If a scientists presents an English sentence

69

with a mathematical expression embedded in it, is the scientist directly representing their target system, indirectly representing it (or both, or neither)? How are we to understand the reasoning expressed by scientists when they move from one kind of representative practice to another? Is it a kind of analogical reasoning characteristic of indirect representation or more direct reasoning about the target system characteristic of direct representation? These are questions that must be answered before this response to the linguistic demarcation problem can be accepted. Of course they only arise if a discontinuity is accepted along mathematical and non-mathematical lines.

The threat of intra-scientific discontinuity can be responded to in another way. The semantic view's analysis could be extended to all scientific theorizing and modeling practices, whether they are explicitly mathematical or not. Many proponents of the semantic view show an inclination to respond in this way. Suppes (1969a) for example argues that Tarski's concept of a model (a structure on my terminology) is applicable across mathematics and the sciences making a model-theoretic analysis of all scientific theories possible. More recently French and Ladyman have argued that regardless whether particular theories are presented mathematically (specifically in set-theoretic terms) they can nonetheless be adequately "captured" or "represented" (1999, 107) for philosophical purposes as a collection of structures.

But extending the semantic view in this way undermines the initial appeal of relying on the distinction between mathematical and non-mathematical expressions as a demarcation principle. If the semantic view applies to non-mathematical scientific theories, then it is clearly not the fact that a theory is presented mathematically that justifies treating expressions as representing structures directly and physical systems only indirectly. And so, the semantic view still owes us some non-question-begging account of the distinction between scientific and non-scientific linguistic practices which would demarcate the scope of their view. Also by moving away from the distinction between mathematical and non-mathematical expressions, the semantic view's original guiding analogy becomes strained. On a model-theoretic analysis the linguistic expressions used to present a scientific theory are treated like axioms of a mathematical theory which pick out a class of structures. In mathematical cases we often have a relatively clear handle on which mathematical structures are reasonably interpreted as satisfying a theory. However it is much less clear which mathematical structures could be interpreted as satisfying a theory expressed in English or some other natural language. If the semantic view cannot provide a clear account of which structures are to be identified with a non-mathematical theory, then it cannot hope to offer a satisfactory account of scientific theorizing in general.

Let me finish my discussion of the semantic view and the linguistic demarcation by saying a bit about an even more ambitious proposal. I have argued thus far that by choosing the distinction between mathematical and non-mathematical expressions as a demarcation principle, the semantic view accepts a discontinuity across scientific theorizing practices and the unwelcome consequences that follow. To avoid this discontinuity the semantic view may try to extend their analysis beyond those theories expressed mathematically but this undermines the analogy between mathematical and scientific theories that makes the semantic view appealing in the first place. There is another way that one might seek to address the linguistic demarcation problem. The proponent of the semantic view may embrace a truly imperialistic impulse and suppose that the semantic view can be used to account for all linguistic representative

practices, scientific or otherwise. I think da Costa and French (2000), for example, take a step in this direction by proposing a model-theoretic framework as a basis for an analysis of truth.[6] I shall return to critically evaluate da Costa and French's view of representation in chapter 8. In the meantime I would like to highlight some challenges facing this sort of proposal.

On a positive note, it must be acknowledged that if the semantic view, with its mediating structures, were offered as the basis for understanding all linguistic representation, then the linguistic demarcation problem would be neutralized. Continuity would be preserved across scientific and non-scientific domains. On such a proposal expressions never play a direct representational role, but always an indirect one. If I were, say, describing the appearance of my father to a friend, the description I produced would not be a direct representation of my father, but instead it would describe a structure and the structure would in turn represent my father. The structure would be the epistemically important representation and its accuracy would be determined by the (structural) similarities between the structure and my father. But extending the semantic view's intended domain in this way the semantic view becomes a general theory of linguistic representative practices and must then compete directly with other general theories found in the philosophy of language and epistemology. As a general theory of linguistic representation it must be evaluated against the same body of linguistic evidence and theoretical considerations that all its other competing general theories are evaluated against. Defending the semantic view becomes a massive undertaking and its plausibility as an account of scientific representative practices is hostage to its general acceptability as a framework for understanding all linguistic representative practices. In short, the proponent of the semantic view would need to do a lot of work before this response to the linguistic demarcation problem could be promising.

4.4   Model Theory and a Linguistic Approach

I would like to end this chapter with a brief discussion about formal semantics, model theory, and the linguistic approach. As I have discussed the semantic view has its roots in mathematical model theory. By rejecting the semantic view it may be thought that the linguistic approach in some way shuns model theory or formal semantics. This, as I shall now explain, is not the case.

What a formal semantics provides is a representation of certain semantic features of a language. It is a representation that, among other things, allows one to determine when (relative to some interpretation) expressions of the language are true or false. Thus, a formal semantics represents the truth-conditions of expressions of the language. The linguistic approach can happily accept that such representations may be useful for many purposes. Standard mathematical model theory provides one framework, among others, within which a formal semantics can be developed. There are two parts of such a semantics: a partially interpreted formal language $\mathscr{L}_f$ and the mathematical structures that interpret sentences of $\mathscr{L}_f$. There

---

[6]French (2003) has also argued that all representation is ultimately to be analysed in terms of structural similarity, again opening the door for a model-theoretic treatment of all representative practices.

are roughly two routes by which a formal semantics may be given for a language $\mathscr{L}$, where $\mathscr{L}$ is a natural and/or fully interpreted language. Route 1 is to devise a scheme for translating sentences of the language being modelled into sentences of the formal language. Then a theory T expressed in $\mathscr{L}$ can be translated into a set of sentences T_f in $\mathscr{L}_f$. Theory T_f will in turn be satisfied by a set of structures $Mod(T_f)$. The theory T_f is one representation of the T and the set of structures $Mod(T_f)$ are another representation. Each in their own way represents semantic features of the theory. Roughly speaking, T_f syntactically represents some of the semantic features while the structures $Mod(T_f)$ specifies the remaining information required to determine the truth or falsity of sentences of the language. It is in this sense that a theory's structures can be thought, roughly, to represent the theory's truth conditions. The other route, route 2, is to directly associate sentences in $\mathscr{L}$ with structures, thereby bypassing the translation of sentences of $\mathscr{L}$ into sentences of $\mathscr{L}_f$. Nevertheless the significance of these structures is still the same. They are still, in a sense, just representations of the conditions under which sentence are true.

Again the linguistic approach has no problem accepting that model-theoretic representations may be useful for many purposes. But the semantic view attributes significance to structures beyond that prescribed by model theory. This is where the semantic view and the linguistic approach part ways. When associating a theory—expressed in a natural or interpreted language—with a class of structures (route 2) it is important to recognize that the specified structures are only representations of truth conditions. These representations should not be confused with the systems that make the original theory true (or false). Mathematical structures are not typically among the systems in the intended domain of scientific theories. Theories about, say, protein synthesis describe proteins and the mechanisms of their synthesis. These theories do not describe set-theoretic objects and therefore there are no set-theoretic objects which make theories of protein synthesis true.

Moreover, when using the model-theoretic framework to give a formal semantics for a scientific theory, the mathematical structures associated with the theory are representations of the theory's truth conditions. As such the felicity of these structures as representations is determined by their accuracy at representing the relevant semantic features of the theory. Since the scientific theory is itself a representation (a linguistic representation according to the linguistic approach) the structures specified are just representations of representations (or more accurately, representations of certain features of representations). They, like any other representations, may be accurate or inaccurate. Here questions about non-standard semantics arise. The choice of, say, sets as the structures one uses to represent truth conditions is not a completely neutral one. Non-standard semantics avail themselves to other kinds of mathematical objects to better capture certain semantic features.

Now the same structures employed in one's formal semantics could be put to work to represent physical systems, but this is a separate representational relation with a distinct representational target. Consequently their accuracy with respect to each target must be evaluated separately. A structure may accurately represent one case in which a theory is true, but grossly misrepresent the physical systems in the scientific theory's intended domain. It is an empirical question if and when scientists use mathematical structures to directly represent physical systems. According to the semantic view scientific theorizing nearly always

involves the use of structures to directly represent physical targets and almost never involves the use of language to directly represent physical targets. The linguistic approach makes no such claim. Instead, the linguistic approach hypothesizes that when theories and models are presented using sentences and equations these expressions regularly function in the usual way, i.e., as direct representations of physical systems. Notice that this disagreement between the semantic view and the linguistic approach does not turn on the adequacy of model theory as a framework for representing the semantic features of language. It is for this reason that the linguistic approach may avail itself to model theory, or any other frameworks for developing a formal semantics.

Chapter 5

Cartwright's Nonrepresentationalism

Nonrepresentationalism and the linguistic approach offer competing accounts of the role that sentences and equations play in scientific theorizing and modeling practices. Nonrepresentationalism (my terminology) is the view that, roughly, many expressions employed in the discursive aspect of theorizing and modeling practices systematically function in some non-representational capacity. This stands in contrast with the linguistic approach which hypothesizes that these same expressions are regularly used by scientists to directly represent the systems under investigation.

In this chapter I take up Cartwright's work on theories and models. She offers (at least in her earlier work; see below) one of the most well-known, influential and well-developed nonrepresentationalist positions. I shall offer some arguments against the non-representational aspect of her view. In particular I shall contend that Cartwright's nonrepresentationalism (like the semantic view) does not provide an adequate solution to the linguistic demarcation problem and for this reason as it stands her view does not pose a serious threat to the linguistic approach. However the message I shall urge that we take away from the investigation of Cartwright's work is not that her general account of theorizing and modeling is necessarily incorrect, or otherwise problematic from the perspective of the linguistic approach. Instead we shall find that many aspects (arguably most aspects) of her view are consistent with the linguistic approach. As we shall see, central to Cartwright's account is a substantive theory-model distinction. Upon this distinction theories are abstract and models are concrete. The abstractness of theories leads Cartwright, at times, to claim (I think mistakenly) that theories are non-representational. But as I shall show, given Cartwright's own understanding of abstraction and the distinction between theories and models, the abstractness of theories does not imply that they are non-representational. Consequently her non-representational construal of theories proves to be an unnecessary and unwarranted aspect of Cartwright's view that can be isolated and discarded. Once discarded the remainder of her view proves to be amenable to the linguistic approach. In fact what remains of her view nicely illustrates how a proponent of the linguistic approach might maintain a substantive theory-model distinction in their account of scientific representative practices and still capture the intuitive 'abstractness' of theories.

Cartwright's views on theories and models have shifted somewhat over the years. The position expressed in her book *The Dappled World* (1999) is in certain respects importantly different from the position she expresses in her earlier works. For this reason below I will be considering separately, as I shall call them, Cartwright's *Dappled World* and pre-*Dappled World* views. I associate her pre-*Dappled World* view of theories and models primarily with her work roughly between 1989 and 1995. This includes her book *Nature's Capacities* (1989) and her portion of the "The Toolbox of Science" (1995), both of which clearly espouse a non-representational construal of abstract theories.[1] It isn't until the *Dappled World* that Cartwright backs away from a nonrepresentationalist view. However, the continuities between her pre and post *Dappled World* works shall prove just as important as the discontinuities to our investigation. We shall see that her general view on theorizing and modeling actually remains largely unchanged throughout her work and this points to a core of her view on theorizing and modeling that is both independent of her nonrepresentationalism and consistent with the linguistic approach.

5.1 Cartwright's Pre-Dappled World View

In Cartwright's portion of the "The Tool Box of Science" (1995) she writes:[2]

> it does not make any sense to talk of "reading the theory" literally, nor to talk of what the theory "implies". ... Theories and auxiliaries do not imply data—or ... "phenomena"—even in principle. Representations of phenomena must be constructed and theory is one of many tools we use for the construction. ... I want to urge that fundamental theory represents nothing and there is nothing for it to represent. There are only real things and the real ways they behave. And these are represented by models, models constructed with the aid of all the knowledge and technique and tricks and devices we have. Theory plays its own small important role here. But it is a tool like any other; and you can not build a house with a hammer alone. (1995, 139–140)

This is a clear expression of a non-representational construal of theories, while allowing models to function in a representational capacity. As you will recall in §2.1.1 I stipulated that representations, on my usage, are representations-as (as per Goodman, van Fraassen and others) and they are, what Suáez calls, cognitive representations. This is a fairly permissive notion of representation which allows many different things functioning in many different ways to qualify as representations. Nevertheless, Cartwright is clearly denying that theories are representations even in this permissive sense. She claims theories do

---

[1]Though the nonrepresentationalism is less explicit and well developed, I believe her views in *How the Laws of Physics Lie* (1983) are also in line with these other pre-*Dappled World* works and could, at least for our purposes, be considered part of her pre-*Dappled World* corpus.

[2]Though all three authors are clear that all the views expressed in the paper are intended to be complementary, the first part of the paper—which makes Cartwright's nonrepresentationalism explicit—is acknowledged by the paper's authors solely as Cartwright's contribution (1995, 137).

not represent anything, nor is there anything for them to represent. If this is correct, then van Fraassen's scheme, X represents Y as Z, does not apply to theories, since there is nothing, no Y's, for theories (X) to represent. Moreover Cartwright claims theories are not the sorts of things that can be read literally, nor the sorts of things that have implications. If so, then they are not cognitive representations that allow surrogate reasoning in Suáez's sense. Theories do not allow competent and informed agents to draw specific inferences regarding any targets because they are not the sorts of things that have or express (cognitive) contents that can imply anything.

Here we see Cartwright positing a substantive theory-model distinction which marks the boundary between the products of scientific practice that are genuine representations and those products of scientific practice that are mere tools for constructing representations. But, of course, both theories and models are presented via sentences and equations. What is it, then, that distinguishes theories from models? What is it that leads Cartwright here to suppose theories are so different from models that they cannot even in principle function in representative capacity? To answer these questions we must look to Cartwright's *Nature's Capacities* (1989), where she presents a developed account of the theory-model distinction. As we shall see in detail below, she argues that idealization and abstraction are distinct processes that work in conjunction in scientific practice. Idealizations yield *concrete laws* and abstractions yield *abstract laws*. Concrete laws are constitutive of models and abstract laws are constitutive of abstract theories. What crucially distinguishes theories, then, is that theories are abstract in a way models are not. The guiding intuition in Cartwright's pre-*Dappled World* view, then, is roughly that theories are non-representational because they are abstract. It is their abstractness that renders them incapable of performing a representative function in scientific practice.

To evaluate Cartwright's nonrepresentationalism we will have to better understand what on her account makes theories abstract and why the abstractness of theories makes them non-representational. We shall do this first by contrasting the processes of idealization and abstraction that Cartwright claims are operative in scientific modeling and theorizing practices. This will provide a general picture of how theories and models relate on her view. Then we shall have a more in-depth look at her pre-*Dappled World* account of abstraction to see if it is able to ground her non-representational construal of theory.

5.1.1 Contrasting Idealization and Abstraction

Idealization, according to Cartwright, is a process of modification: "in idealization we start with a concrete object and we mentally rearrange some of its inconvenient features—some of its specific properties—before we try to write down a [concrete] law for it" (1989, 187). She takes the frictionless plane to be a paradigm example. These are models that represent objects moving down a plane without being retarded by frictional forces. What Cartwright takes to be crucial, however, is that the relevant forces (those acting in the opposite direction of the object's motion) are not simply omitted. Instead they are replaced with factors "which are easier to think about" or "easier to calculate" (1989, 187). For example, if a ball is modeled rolling down the plane, to "calculate its motion, you must know the forces in each

of three orthogonal directions", otherwise "the problem is undefined" (1989, 187). So, all three forces are specified in the ideal model, but the forces are idealized in such a way as to eliminate the effects of friction.

The products of idealizations are what Cartwright calls concrete laws. Concrete laws constitute or express models.[3] Models, i.e., sets of concrete laws, are idealized descriptions of systems. That is, they are perfectly accurate descriptions of only ideal, or counterfactual, systems. Nevertheless Cartwright maintains that idealized descriptions are "realistic" in the sense that all the factors deemed relevant to characteristics under study are included in the description (1989, 191–192). For this reason Cartwright (1989, 188) suggests it is coherent to talk about models as better or worse approximations of real systems.

In contrast to idealization, abstraction is "not a matter of *changing* any particular features or properties, but rather of *subtracting*" particular features (1989, 187; original emphasis). Abstraction, claims Cartwright, is a matter of subtracting the "concrete circumstances" (1989, 187) in which causes are embedded. The process of abstraction produces abstract laws. Cartwright considers the laws associated with high-level theory to be abstractions in this way. Newton's second law, $\vec{F} = m\vec{a}$, is an example. As an abstract law, in Cartwright's sense, it does not encode information about any of the physical circumstances of systems which are supposed to be subsumed under this law (so-called Newtonian systems). It tells us nothing, for example, about the specific structure or arrangement of the forces acting on any particular masses in any particular real systems that Newton's law is purported to apply to. In the absence of this information she believes that abstract laws are non-representational. They "describe no real concrete situations nor any counterfactual ones either" (1989, 192). Consequently, Cartwright claims abstract laws cannot be literally true or false, nor can they coherently be construed as better or worse approximations of real systems (1989, 188).[4]

Cartwright argues idealization and abstraction work in tandem in scientific practice. Figure 5.1 illustrates the general picture of modeling and theorizing she outlines. Scientists construct models to do the representative work. Models are used to explain and predict the behaviour of real systems. Now models can be modified in various ways to make them more or less idealized. In §2.1 I introduced a simple pendulum model. In the model the pendulum bob is represented as a point mass which moves unencumbered by friction or air resistance. As a representation of the real copper pendulum system investigated, the model was an idealization in Cartwright's sense. The real bob's mass is not concentrated at an infinitesimal point, nor is the bob able to move without being influenced by friction or air resistance. According

---

[3]Cartwright sometimes uses 'model' to refer to an ideal system and at other times she uses it to refer to a representation of an ideal system. Following my preferred usage, however, I will use 'model' to refer to representations only and not the targets of representations.

[4]Cartwright does say that "abstractions can be taken as claims about capacities" (1989, 185). At first blush this appears inconsistent with the non-representational view she clearly expresses elsewhere in the same work. If claims about capacities can be true or false, then abstractions are representations of capacities. To resolve the apparent inconsistency I interpret Cartwright as claiming that abstract laws are not literally claims about capacities, but they can be "taken" (reinterpreted) as claims about capacities. In other words, abstract laws are not representations if literally construed, but they could be representations if they are construed in non-literal ways. Since it is the literal construal of expressions that concerns the linguistic approach, the details of the potential non-literal interpretations are not pursued in this work.
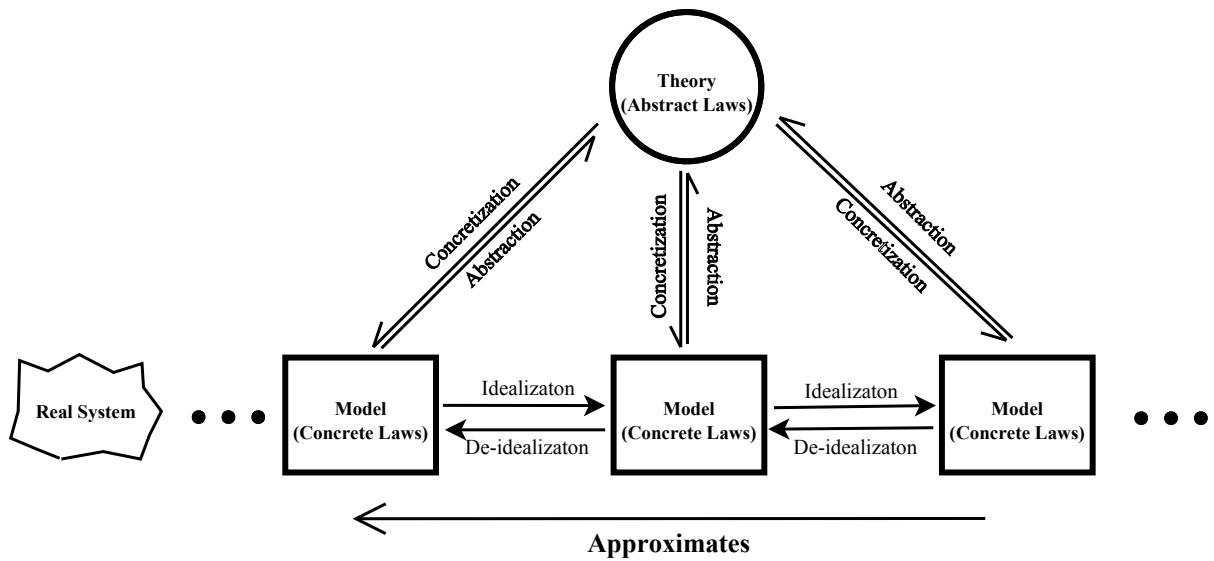
Figure 5.1: An illustration of the relations between models and theories on Cartwright's view.

to Cartwright the simple pendulum model can be corrected or improved upon to some degree. Modeling the system as, say, a *damped harmonic oscillator* is a means of representing some of the effects of forces like friction and air resistance on the bob's motion. For Cartwright by correcting the model in this way it is *de-idealized* in certain respects, resulting in a representation which better approximates the real copper pendulum system represented. It may be possible for other corrections to be made to produce even more de-idealized models. For example the bob may be represented as an extended body or the rod holding the bob may be treated as a non-rigid (elastic) object. Cartwright admits successive de-idealizations produce models which could, at least in certain respects, better approximate the real system represented.

For Cartwright, abstraction and its dual, *concretization*, are different processes (for this reason in figure 5.1 I have illustrated them as being orthogonal to idealization and de-idealization). To establish an abstract law one looks for commonalities in various idealized models. These commonalities are aspects of the models which remain unaltered throughout various idealizing steps. Having identified common elements, one then strips away all of the other information about a particular system's structure and material construction that is encoded in the model. This process leaves only a rarefied abstract law (or set of laws) (1989, 191). For example a common element in the simple pendulum and the damped harmonic oscillator models is Newton's law $\vec{F} = m\vec{a}$. This law statement remains unaltered by the idealizations incorporated in these models. Newton's law becomes an abstract law, then, when it is isolated and removed from these models. Many different models and corresponding concrete laws may be subsumed under a single abstract law. According to Cartwright this is the source of the abstract law's utility. Once established an

abstract law becomes a tool which can be used to help with the construction of new models. This involves a process of concretization where the details of a concrete system (an ideal system which is a counter-factual possibility) are added back. Cartwright claims the process of concretization is typically not one determined by the consideration of the theory alone. One does not, for example, deduce the concrete laws of a model from the abstract laws alone (1989, 206–212). It is only after the process of concretization is completed that the expressions associated with specific models become candidates for literal truth.

We can now see that there are really two distinct aspects of Cartwright's pre-*Dappled World* view. The first I shall call Cartwright's *general account of theorizing and modeling*, or GATM. The aim of GATM appears to be straightforwardly descriptive. It includes the following theses: (i) there is a substantive theory-model distinction, whereby theories and models are distinct products of scientific practices with distinct properties and functions; (ii) abstraction and idealization (and their counterparts concretization and de-idealization) are distinct processes that work in conjunction in scientific practice; (iii) theories (*viz* abstract laws) are the products of abstraction; (iv) models (*viz* concrete laws) are the products of concretization, idealization and de-idealization; and (v) models are (relatively) concrete and theories are (relatively) abstract. Figure 5.1, then, illustrates the various relations constitutive of Cartwright's GATM. The second aspect is Cartwright's nonrepresentationalism. It involves selectively construing models as representational and theories as non-representational. Obviously if her general account of theorizing and modeling did not maintain a substantive theory-model distinction, then she could not delineate between the representative and non-representative products of scientific practice by appealing to the theory-model dis-tinction. However, this does not mean that GATM entails nonrepresentationalism. It does not immediately follow from theses (i)–(v) that theories fail to be the sorts of things that can function as representations in scientific practices. The connection between GATM and Cartwright's nonrepresentationalism must be forged via an account of abstraction. It is this account which is burdened with showing that theories are non-representational in virtue of being abstract. Should her account of abstraction fail in this regard, then it becomes possible to accept GATM without accepting nonrepresentationalism.

5.1.2 Abstraction and the Linguistic Demarcation Problem

The conflict between Cartwright's pre-*Dappled World* view and the linguistic approach can be nicely captured in terms of the linguistic demarcation problem introduced in chapter 3. As you will recall the linguistic approach adopts the continuity thesis. According to this thesis expressions play a similar com-municative and epistemic function across scientific and non-scientific domains. Since direct linguistic representation—which is characteristic of assertions, descriptions, hypotheses, etc.—is a regular function of expressions used to predict, explain and reason about the world in non-scientific contexts, the linguistic approach hypothesizes that expressions used in modeling and theorizing practices regularly perform the same function. By supposing that abstract laws are systematically non-representational, Cartwright rejects the continuity thesis with respect to abstract laws. On her view the expressions associated with models (*viz.* concrete laws) can function in the usual representative capacity, but those associated with theories (*viz.* abstract laws) are non-representational.

By rejecting continuity Cartwright faces the linguistic demarcation problem and must provide a demarcation principle that explains where the discontinuity is, and ideally why there is a discontinuity at all. In this case, Cartwright must provide a reasonably clear way of distinguishing abstract laws from concrete laws. If no demarcation principle is specified, there is no way to determine the intended domain of her account. We would have no way of knowing in which cases her nonrepresentationalist analysis is supposed to apply and in which cases it is not, and thus we would have no way of evaluating whether or not her account is correct. Moreover, in the absence of a suitable demarcation principle no explanation of the purported discontinuity can be produced. If a theory claims there is a discontinuity, but cannot tell us where the discontinuity is or why we should expect there to be a discontinuity at all, then it is hard to see what ground we could have for accepting the theory. So if Cartwright cannot provide an account of the abstract-concrete distinction that can explain why abstract and concrete laws perform different functions in discursive practices, it is hard to see why we should follow her in believing that, contra the linguistic approach, there is such a discontinuity.

Cartwright proposes to use the theory-model distinction as a means of distinguishing between expressions that are used in a representational and non-representational way and she appeals to an account of abstraction to distinguish theories from models. Thus it's her pre-*Dappled World* account of abstraction that must do the work of demarcating between expressions that function in representational and non-representational capacities within scientific practice. To evaluate the plausibility of her proposal as a potential solution to the linguistic demarcation problem, then, we must investigate her account of abstraction in greater detail. As we shall see, Cartwright's general idea is that abstraction is a process that involves the "subtraction" of information from a representation. On her view when, roughly, when enough of the right kinds of information gets stripped away, what was once a representation ceases to be able to perform a representational function—it becomes some kind of degenerate, or non, representation. What is crucial in understanding her account, then, is determining what information a putative representation must have before it is capable of functioning in a representative capacity. In other words, we need to see what information Cartwright takes to be essential to representation: information that is sufficient to transform a representation into a non-representation when it is removed. It is her account of abstraction that must provide these details.

In Cartwright's *Nature's Capacities* (1989) she provides more details about abstraction. Her account has two central elements. The first element, claims Cartwright, is Aristotelian in origin:

> For Aristotle we begin with a concrete particular complete with all its properties. We then strip away—in our imagination—all that is irrelevant to the concerns of the moment to focus on some singular property or set of properties, 'as if they were separate'. (1989, 197)

To illustrate the idea she imagines starting with a particular substance, like a triangular marking on a slate, with a full host of accidental and essential properties. The process of abstraction, then, is a psychological one in which we "subtract the stone and chalk, the colour, and other properties incidental to being a triangle." The result is "the triangle which is to be treated as if it were a substance" (1989, 213).

Cartwright suggests we can get a firmer grip on this Aristotelian notion of abstractness by comparing the number of properties that are attributed to an object. Roughly, the more properties the more concrete the representation is, and the less properties the more abstract it is. Cartwright acknowledges that problems can arise when trying to enumerate properties and that these problems make it unlikely that a *total ordering* along the abstract-concrete spectrum can be specified in this way (1989, 214). However she insists that a natural way of *partially ordering* objects can be achieved along these lines. On her proposal "A is a more abstract object than B if the essential properties, those in the description of A, are a proper subset of the essential properties of B" (1989, 214). On this proposal right-triangles are more concrete than triangles because the right angle of the right-triangle is subtracted from the description of a triangle. Conversely, all of the properties of triangles can also be ascribed to right-triangles.

This method of comparing properties is not the only criteria that Cartwright wishes to take into account. The second element of her account concerns explanation. According to Cartwright some properties are more important than others to the abstract-concrete distinction. Which properties are more important is determined for Cartwright by the nature of explanation. Roughly, the more explanatory a description is the more concrete it is and the less explanatory it is the more abstract it is. She uses representations of lasers and reservoirs to illustrate the idea. A "laser is characterized by its structure and its internal causal processes, as well as its output, unlike a reservoir, which is characterized only by its output" (1989, 218). A thermodynamic reservoir is just a system which acts in a certain way: it is as a heat sink absorbing or radiating heat without itself ever appreciably changing temperature. There are many different systems with different material constructions that can function as thermodynamic reservoirs, but in describing them all as "reservoirs" the details of the structure and composition of these systems is abstracted away. By contrast a laser is characterized with respect to distinct components which interact. A laser is the sort of thing that has an active material which emits the laser light, a pump which pumps energy into the active material and a set of semi-transparent mirrors that direct most of the laser light back through the active material to amplify the laser beam (1989, 217). For Cartwright the characterization of a system as a reservoir is more abstract than the characterization of a system as a laser. To characterize a system as a laser is to characterize it as having a laser's components and the interactions between these components provide some explanation of the laser's behaviour. By contrast Cartwright denies that the characterization of a system as a reservoir can be similarly used to explain its behaviour. To know only that a system is a reservoir is to know nothing of the explanatorily relevant information required to explain the behaviour of reservoirs.

According to Cartwright, then, the process of abstraction involves removing explanatory properties, and the process of concretization involves adding back explanatory properties to a representation of a system. She suggests that the explanatory properties are those pertaining to a system's structure and causal mechanisms:

> to add to a scientific model a description of an objects' structure and causal mechanisms is not just to add some information or other, but to add crucial explanatory information. In fact it is to add just the information that usually constitutes explanation in modern theoretical sciences.

81

(1989, 218)

She seems to be appealing to some variety of a causal mechanistic account of explanation. To explain the behaviour of a system is to provide some causal or mechanistic understanding of its behaviour or function.

Pulling together both the Aristotelian and explanatory elements, Cartwright (see also Cartwright and Mendell 1984) offers the following criteria for making comparative judgements about abstractness:

(a) an object with explanatory factors specified is more concrete than one without explanatory features;[5]

(b) if the kinds of explanatory factors specified for one object are a subset of the kinds specified for a second, the second is more concrete than the first. (1989, 220)

By these criteria we can see why Cartwright classifies lasers as more concrete than reservoirs. Descriptions of lasers includes information about both the form and function of these objects and descriptions of reservoirs include information only about their function (1989, 222). It is the information about a laser's form that, for Cartwright, adds to the laser's description the explanatory factors missing from the description of the reservoir. So it is this information, according to criteria (a), that makes the laser less abstract than the reservoir.

Now let us consider this account of abstractness in connection to the linguistic demarcation problem. Can abstractness as Cartwright has characterized it, be used to mark the difference between expressions that are used in a representational and non-representational capacity? Could we, for example, simply take abstractness as sufficient for counting an expression as non-representational? On the face of it the answer is "no." As a tool for demarcating representational and non-representational expressions the above account of abstraction faces a very serious shortcoming: it characterizes a difference in degree but what Cartwright needs to specify is a difference in kind. Her criteria for abstractness grades representations as more or less abstract. Criterion (b) partially orders representations with respect to shared property subsets. This allows for a representation to be both more abstract than some representations and less abstract than others.

However what Cartwright needs is a distinction that marks a difference in kind that does not admit degrees. Why? She holds that one class, concrete laws, have the capacity to represent, but another class,

---

[5]As mentioned earlier, Cartwright's discussion of abstractness slips between the classification of representations and the classification of 'objects' as abstract. I think calling objects abstract in this context is quite misleading and we are best sticking with putative representations being abstract or concrete. In the above criteria she talks of "an object with explanatory factors specified" (1989, 220), but it is a putative representation of an object that will or will not include explanatory factors. Moreover so long as objects are the targets of representations, then it is possible for the same object to be represented by two different representations. If one of the two representations specifies more explanatory factors than the other, then by Cartwright's account one representation would be more abstract than the other. This is unproblematic so long as we do not try to use this as a basis for classifying the represented object as abstract or concrete, since the same object would be differently classified according to each of the representations. For these reasons I shall only consider Cartwright's account as an account of the abstractness of putative representations and not objects.

abstract laws, do not. However the capacity itself is not something that comes in degrees. Representations can be more or less accurate, they can be more or less comprehensive and provide more or less information about their targets. Representations can be more or less useful, and they can be more or less explanatory. However the capacity to represent does not come in degrees. An uttered expression either is or is not the sort of thing that has the capacity to be accurate, comprehensive, useful, or explanatory to any degree. Cartwright says, for example, that "it does not make any sense to talk of 'reading the theory' literally, nor to talk of what the theory 'implies'" (1995, 139). When it comes to laws statements, then, it either makes sense to talk about them having implications or it does not, it is hard to see how this could admit degrees. She also claims that abstract laws "describe no real concrete situations nor any counterfactual ones either" (1989, 192). Concrete laws, on the other hand, can at least describe counterfactual situations and for this reason are capable of approximate truth. Laws either do or do not describe possible situations (actual or counterfactual situations), and was either are or are not capable of being approximately true. These are dichotomies that mark differences of kind rather than differences of degree.

If Cartwright is to use her account of abstractness to demarcate between representational and non-representational expressions, it needs to be supplemented with additional criteria that would establish a threshold between representative and non-representative abstractions. She does not explicitly specify such criteria. But are there criteria implicit in her view that might do the job? I think criterion (a) is suggestive in this respect. Upon this criterion a characterization of a system with explanatory factors specified is more concrete than one without explanatory features. What is unclear from (a) is whether two characterizations of systems without any specified explanatory factors can also be graded as more or less abstract than one another. One could interpret criterion (a) as implicitly presupposing that some explanatory threshold must be met before putative representations can even be graded with respect to their concreteness.

Such an interpretation is supported by other aspects of Cartwright's view. As noted she appeals to some variety of a causal mechanistic account of explanation according to which an object's structure and material construction are features crucial to the explanation of the system's behaviour. It is plausible, then, that a putative representation that specifies no information about an object's structure or material construction could, for Cartwright, be completely unexplanatory and therefore fall below the minimal explanatory threshold.

What might count as a completely unexplanatory characterization? To answer this question I think we should take our cue from Cartwright's discussion of functional characterizations, e.g., like the characterization of a 'reservoir' as any system that functions in a certain way. A functional characterization includes virtually no information about the physical construction of the system. Objects characterized purely functionally, as Cartwright says, "may be made of any material that allows them to do what they are supposed to do" (1989, 215). So a functional characterization alone tells us nothing of the crucial explanatory information required to understand how the system does what it is characteristically supposed to do. Knowing only that a system is a thermodynamic reservoir tells us nothing about the causal or mechanical features of the system that allow it to function as a reservoir. In the same way Newton's law second law, $\vec{F} = m\vec{a}$, does not encode any information about the physical circumstances that would explain why a Newtonian system behaves like a classical mechanical system. It is this complete abstraction of the "concrete cir-

cumstances" and the material constructions within which causes are "embedded" that Cartwright calls *material abstraction* (1989, 188). On this interpretation of Cartwright, she takes the products of material abstraction to be so impoverished that they fall below the minimum explanatory threshold required for them to count as being representational or truth-apt. Consequently, though not all abstractions result in abstract laws, material abstractions do because the resulting expressions fail to encode any of the requisite information for them to provide even the most minimal explanation of the characteristic behaviour or function of the objects which are subsumed under it.

We can summarize what I take to be Cartwright's implicit proposal in the following way. For an expression to be able to function in a representative capacity (i.e., as having truth-apt propositional contents) it must encode enough information about its target's structure and material composition to meet an explanatory threshold whereby the included information provides the basis for some minimal explanation of the characteristic behaviour or function of the targeted system. Material abstraction is a process which produces abstract laws that fail to be minimally explanatory. For this reason Cartwright takes abstract laws and the theories constituted by them to be non-representational. She believes concrete laws and the models associated with them surpass the minimum explanatory threshold. Though on her view concrete laws and models are still abstract to a high degree, by surpassing the threshold they can be straightforwardly truth-apt and, though likely literally false, they have the potential to be approximately true.

Of course there a lot of details regarding this proposal that still need to be filled in. It is still unclear exactly what information about a system might be required for it to minimally explain the system's characteristic behaviour or function. This, presumably, must be specified by an account of explanation. In addition to the above quoted comments Cartwright makes about the explanatory relevance of a system's material construction and structure (1989, 218), Cartwright (1989, 219–224) also suggests that Moravcsik's (1975) view of explanation may be helpful.[6] On his view Aristotle's four causes are construed as different explanatory factors. Drawing upon Moravcsik's view she suggests that an "account which specifies all four factors will give all the relevant information for identifying the material substance and for understanding what it truly is" and the "four causes of Aristotle constitute a natural division of the sorts of [properties] that figure in explanation, and they thus provide a way of measuring the amount of explanatory information given" (1989, 219). The idea here is that information can be specified about a system's source (efficient cause), constituents (matter), structure (form), and function (final cause). All of this information would be explanatorily relevant and the more of it that is specified the more explanatory the representation. Unfortunately Cartwright does not develop this account in detail and it is left unclear how this view might be used to establish an explanatory threshold that could demarcate between representational and non-representational uses of sentences and equations.

Nevertheless I am confident we can make a negative evaluation of Cartwright's implicit proposal

---

[6]Cartwright does not mention here anything about the simulacrum account of explanation that she presents in her earlier work (1983, 151–162). This conspicuous omission suggests that Cartwright does not take it to be relevant to her account of abstractness. This is likely because, as far as I can tell, the simulacrum account of explanation is exclusively a negative view about the adequacy of the deductive-nomological model of explanation. It does not offer any substantial account of what it is that distinguishes genuine, or successful, explanations from non-explanations.

even without all of the details filled in. Her implicit proposal will not serve as an adequate means of meeting the linguistic demarcation problem. Let us suppose for the sake of argument that an account of explanation can be specified that has the following features. First, it is a generally plausible account of explanation that at least correctly distinguishes between (most) of the paradigm examples of explanation and (most) paradigm examples of non-explanation. Second, it bears the basic demands placed on it by Cartwright's account of abstractness: the account of explanation provides a basis for specifying a suitably clear explanatory threshold. Upon this supposition there is an account of explanation that we could use to determine whether or not a putative representation surpasses the explanatory threshold. By Cartwright's proposal, if a putative representation succeeds in minimally explaining a system's characteristic function or behaviour it can function as a genuine representation. I claim that even if we could demarcate between minimally explanatory and unexplanatory expressions in this way, it is implausible to suppose that the unexplanatory will even roughly correlate with the non-representational. In other words, it is implausible to suppose that only explanatory expressions can be representations-as, or be cognitive representations that facilitate surrogate reasoning. Cartwright is demanding that explanation track representation in a way that is simply not supported by our pre-theoretic judgements about explanation, nor is it supported by any of the major accounts of explanation found in the philosophy of science literature.

Starting with everyday examples we see that explanation and representation are not well correlated. On the face of it "My computer is on," "The car has a flat," and "I am tired" when uttered in most contexts are assertions that represent things as being some way or other. However they do not explain the characteristic functions or behaviour of the objects they represent. In most circumstances my computer will either be on or it will not, and my claim that it is on will be, at least in these circumstances, straightforwardly true or false. By describing my computer in this way I am representing my computer as being in a particular state. Moreover, knowing that it is in this state is knowledge that can be used to draw inferences. For example, by appealing to some basic knowledge of electronics from "My computer is on" it can be inferred that the computer is receiving power. However, describing it as on doesn't explain much. There are relatively few why-questions that would satisfactorily be answered with this description alone. It certainly does not explain the characteristic function or behaviour of computers. The description does not explain why the computer is on, why it is the sort of thing that can be on or off, nor does it explain how the computer computes. So we have a case in which a description is representational but not explanatory in the way that Cartwright suggests in necessary for representation.

Moreover, the literature on scientific explanation draws heavily upon the distinction between mere description and explanation. Consider the well-known barometer example.

(1)   The reading on the barometer always drops when a storm is approaching.

(2)   The reading on the barometer is dropping.

(3)   A storm is approaching.

85

(1) is a perfectly well-formed sentence which, when uttered in most contexts, is a linguistic representation. However it does not explain barometers, the weather, or the correlation between them. An utterance of (1) can also be appealed to help ground other claims. (1) and (2) are grounds for asserting (3). The lesson that is commonly drawn from such examples is that expressions that ground belief do not always explain (see, for example, van Fraassen (1985, 103–106)). I suggested in chapter 2—and this is something I will elaborate on in greater detail in the next chapter—that the capacity of an utterance to serve as grounds for other utterances is a hallmark of linguistic representation. If this is correct, the straightforward conclusion to be drawn from this example is that (1) is used to represent the correlation between the barometer reading and the weather, but it is not used to explain this correlation. The divergence of representation and explanation is something that most accounts of explanation preserve. As Woodward observes, "most models of explanation assume that it is possible for a set of claims to be true, accurate, supported by evidence, and so on and yet be unexplanatory." However different models of explanation "provide different accounts of what the contrast between the explanatory and merely descriptive consists in" (Woodward 2011). So by appealing to explanation as a basis for demarcating between representational and non-representational uses of language, Cartwright is demanding that explanation track representation in a way that the most popular (and presumably the best) models of explanation do not. This implies that explanation simply cannot do the work of demarcating between representative and non-representative expressions in the way Cartwright's view demands.

Let me add a related, but broader, methodological reason for being sceptical of Cartwright's implicit proposal. As I will discuss at greater length in the next chapter, utterances which express truth-apt propositional contents play a wide variety of functions in communicative and epistemic practices. Among other things, these utterances are used to:

> (i) convey information, (ii) express beliefs, (iii) convince others to believe or act in certain ways, (iv) serve as a basis for inference, and (v) justify other utterances.

Now it is plausible to suppose that an utterance which is incapable of performing one or more of these functions is simply not an utterance that should be considered truth-apt (or propositional, or representational). If so, these functions could be construed as essential for, or constitutive of, an utterance being truth-apt (or of having propositional contents, or being representative). Which of these functions is plausibly constitutive of truth-aptitude? This is a broad theoretical question with deep implications for a general theory of language. If we are looking for a way of demarcating between truth-apt and non-truth-apt expressions, then we should seek a demarcation principle by drawing upon similarly broad theoretical considerations. Cartwright's proposal implies that explanation should be added to the above list and distinguished as an essential function for truth-apt utterances. Moreover if, as the barometer example suggests, expressions can be used to warrant inferences but fail to be explanatory, then the inferential and justificatory functions should be deemed inessential to truth-aptitude. This is a fairly radical proposal; a proposal I doubt many philosophers of language would find *prima facie* plausible. Before Cartwright's proposal should be taken seriously, then, the view that explanation ought to play such a central role in our

understanding of language needs to be defended in the right sort of way. It needs to be defended as a proposal which has broad implications for the philosophy of language. By narrowly appealing to intuitions about abstractness without at least connecting these considerations with a broader theoretical framework, Cartwright just doesn't provide reasons that are targeted at, or have clear implications for, an appropriately broad theoretical level.

By contrast, in the next chapter I look to speech act theory to help determine when an utterance should or should not be considered representational. Speech act theory provides a general account of linguistic acts which seeks to capture diverse uses of expressions. In seeking a way of demarcating representative from non-representative uses of expressions speech act theory is a more methodologically sound framework to draw upon. Speech act theory concerns itself with suitably broad theoretical considerations. By drawing upon speech act theory we shall find that the distinction between representative and non-representational utterances does not fall along the lines suggested by Cartwright.

To summarize, I have outlined Cartwright's pre-*Dappled World* account of the abstract-concrete distinction which she appeals to demarcate between representative expressions, *viz* concrete laws and models, and non-representative expressions, *viz.* abstract laws and theories. Her account of abstractness faces a serious problem in this regard. Abstractness on Cartwright's picture is a difference of degree but she needs it to mark a difference in kind. Otherwise the abstract-concrete distinction cannot underwrite the distinction between expressions used in a representational and non-presentational capacity. This problem may be overcome if Cartwright can identify a suitable threshold that would impose a sharper distinction. I suggest that a threshold is implicit in her view and the threshold she implicitly appeals to is an explanatory one. But I argue it is implausible that explanation can be used to meet Cartwright's aims. There is just no reason to think that the ability of an expression to explain will closely track the expression's truth-aptitude, or other characteristics that are indicative of linguistic representation. If, as I suggest, explanation is an unreliable means of demarcating between representative and non-representative uses of expressions, then many abstract theories are representational even though they fail to meet some explanatory threshold. I have shown, then, that Cartwright's pre-*Dappled World* nonrepresentationalism fails to provide a demarcation principle that can support the kind of discontinuity she claims exists within the discursive aspect of scientific theorizing and modeling practices.

Of course a non-representational view could be formulated in other ways. If one thinks that theories, for example, are non-representational, then some alternative theory-model distinction could be proposed. Similarly, if the guiding intuition is that abstract expressions are non-representational, then an alternative abstract-concrete distinction could be drawn. How might the linguistic approach address these alternatives? I take the argumentative strategy illustrated in this treatment of Cartwright's to be generally applicable. Every adequate non-representational proposal must characterize some distinction between representational and non-representational uses of sentences and equations, and it is this characterization that must be scrutinized. When confronted with a non-representational proposal, then, the first step is to identify the demarcation principle it appeals to. If no clear demarcation principle is articulated that could serve to mark a difference in kind, then the proposal cannot hope to meet the linguistic demarcation problem. Should a relatively clear demarcation principle be found, then we must determine whether it can

reliably mark the boundary between representation and non-representation. If it does not, then the nonrepresentationalist fails to provide a credible alternative to the continuity thesis and the linguistic approach it supports. The working hypothesis of the linguistic approach is that no satisfactory linguistic demarcation principle is likely to be found.

5.2 Cartwright's Dappled World View

As I explained at the beginning of this chapter Cartwright's views have undergone some changes over the years. For this reason I have drawn a distinction between her pre-*Dappled World* and *Dappled World* views. The previous section was concerned only with the former, and this section shall consider the latter.

Cartwright (1999, 23–24) herself describes the changes in her pre and post *Dappled World* as a transition from anti-realism to anti-fundamentalism. In Cartwright's pre-*Dappled World* writings her primary target of criticism are realists who construe abstract laws and abstract theories as literal truths about the behaviour of systems. By contrast in the *Dappled World* she attacks *fundamentalism* (1999, 23–28). The fundamentalist, as she explains, has a "tendency to think that all facts must belong to one grand scheme, and more that this is a scheme in which the facts [that are legitimately regimented into theoretical schemes] have a special and privileged status" (1999, 25). She rejects fundamentalism in favour of a pluralistic view upon which a patchwork of laws and models offer scientists a means of predicting and explaining phenomena in limited domains only. According to Cartwright we should not presume that the patchwork of laws produced by the sciences form, or fit into, some grand scheme. Nor, argues Cartwright, should we expect the patchwork of laws to capture all phenomena. There are many facts about the world that simply fall outside the narrow domains of the theories which constitute modern science.

Despite the shift from anti-realism to anti-fundamentalism many aspects of her view remain largely unchanged. In fact her general account of theorizing and modeling (GATM), outlined in §5.1.1 (see also figure 5.1), is retained. The processes of abstraction and idealization work in tandem. Idealizing and de-idealizing moves are employed by scientists to produce models which are better or worse approximations to the real systems they represent (1999, 83–84). Theories, and the laws that constitute them, are the products of abstraction. Moreover, abstract theories are applied to real systems only via concrete models (1999, 3, 35–74). So we see that a substantive theory-model distinction is retained on her latter view and, again, it is the abstract-concrete distinction that Cartwright uses to distinguish between theories and models.

Nevertheless two changes between Cartwright's pre and post *Dappled World* views are of interest for our purposes. First, in the *Dappled World* Cartwright offers a different account of abstraction, and subsequently she offers a somewhat different distinction between theories and models. Second, Cartwright's move towards anti-fundamentalism corresponds with her abandoning a non-representational construal of abstract theories and laws. Cartwright claims her *Dappled World* view is consistent with a kind of *local realism*, upon which theories yield knowledge about the natures of some systems, but do so only in very

limited domains (1999, 23). For her theories apply only where models apply: the "abstract theoretical concepts of high physics describe the world only via the models that interpret these concepts more concretely" (1999, 4). By taking models to play this interpretive role Cartwright thinks that abstract laws may be "'literally' true," but their truth does not necessitate positing new properties or objects beyond those described in corresponding models (1989, 36). Moreover, since laws are applicable only in those cases where models are applicable, "[t]o grant that a law is true ... is far from admitting that it is universal—that it holds everywhere and governs in all domains" (1999, 24). The positing of limited domains for theoretical laws is characteristic of her anti-fundamentalism.

As a consequence of these changes Cartwright's *Dappled World* view is consistent with the linguistic approach. She accepts that abstract law statements can be used to describe the world (and may even be literally true in some cases). Notice that this consistency has been achieved even though Cartwright's general account of theorizing and modeling has remained largely unchanged. To better understand how her general account can be reconciled with the linguistic approach we need to consider her updated *Dappled World* account of abstraction in more detail.

5.2.1 The Abstract-Concrete Distinction Again

Cartwright's *Dappled World* view drops its predecessor's Aristotelian trappings and appeals instead to Gotthold Lessing's work on fables (1999, 37–43). The general idea is that the relation between models and abstract laws is analogous to the relation between fables and their morals. Aesop's fable about the lion and the fox is a story about a fox who encounters a lion numerous times. The moral of the story is that familiarity breeds contempt. Not only is the fable a story which illustrates the moral, on Lessing's view it is a kind of interpretation of the moral in more specific, or concrete, circumstances. The moral is a general claim or command. It could be applied to many different situations (fictional or otherwise). Nevertheless the moral is unlikely to be universally applicable. There are some cases, for example, where familiarity does not breed contempt. Analogously Cartwright suggests that abstract laws apply to many cases, but they do not apply universally. They are applicable in only those cases where concrete models apply. This, of course, is in line with Cartwright's anti-fundamentalism. Abstract laws are not construed as universal claims. They do not apply without exception to every physical system.

Drawing on the analogy with fables and morals, Cartwright specifies the following criteria for distinguishing the abstract from the more concrete:

> First, a concept that is abstract relative to another more concrete set of descriptions never *applies* unless one of the more concrete descriptions also applies. These are descriptions that can be used to 'fit out' the abstract description on any given occasion.[7] Second, satisfying

---

[7]In this quotation Cartwright shifts between talk of concepts as abstract and descriptions as abstract. Given her surrounding discussion it is clear her primary focus is on descriptions and I will interpret her first conditions as a condition that antecedently applies to descriptions. That is, a description that is abstract relative to another more concrete set of descriptions never applies

the associated concrete description that applies on a particular occasion is what satisfying the abstract description *consists in* on that occasion. (1999, 39; my emphasis)

On this view the applicability of abstract descriptions is contingent upon the applicability of other, more concrete, descriptions. For this reason I shall call this the *contingent applicability account* of abstraction.

Cartwright uses descriptions involving 'work' to illustrate her view (1999, 40–41). Suppose it is true that Cartwright worked this morning. This is an abstract description of Cartwright's morning activities. On her view in order for the description "Cartwright worked this morning" to apply, there must be another description of her activities that 'fits out' the abstract description. Perhaps she did the dishes, wrote a grant proposal or had a meeting with the Dean. Any or all of these activities would make the abstract description applicable. If Cartwright did the dishes, then her having worked this morning consists in her having done the dishes. For Cartwright by "working" and "doing the dishes" she has not done two separate things, only one thing. Working and doing the dishes are different descriptions of the same activity. Working is the more abstract description and doing the dishes is the more concrete.

Cartwright takes descriptions involving forces to be similarly abstract. As she says, "being located at distance r from another charge q_2 is what it consists in for a particle of charge q_1 to be subject to the Coulomb force $q_1 q_2 / 4\pi\epsilon_0 r^2$ in the usual cases when that force function applies" (1999, 39). On this view laws which make general claims about forces and their relations to other quantities, like mass and acceleration, are abstract relative to the models which fit out these general claims. It is the applicability of one of the models which makes the law applicable in a specific case.

Now there are two different ways to interpret Cartwright's proposal, each corresponding to a different way of interpreting her use of "applies". If we read "applies" as pertaining to truth, then on her view an abstract description is true only if a member of some other set of descriptions is also true. On this reading the relation between abstract and concrete descriptions, and the 'fitting out' and 'consists in' relations, are relations of *semantic dependence*. The truth of an abstract description is contingent upon the truth of some other concrete description. The truth of "Cartwright worked this morning" is contingent upon the truth of claims like "Cartwright did the dishes this morning," "Cartwright met with the Dean this morning," etc. On another interpretation "applies" pertains to something more pragmatic like *assertability*. In this case an abstract description is only assertable in a context if some other description is also assertable. I cannot, for example, assert that Cartwright worked this morning unless in the very same context it is permissible for me to assert that Cartwright did the dishes, or wrote a grant proposal, etc. In this case the relation between abstract and concrete descriptions is one of *pragmatic dependence*.

We can now see why Cartwright's *Dappled World* view of theorizing and modeling is consistent with the linguistic approach. Regardless whether applicability is interpreted either as pertaining to truth or assertability, both abstract theories and their concrete models can be linguistic representations. Consider the semantic dependence interpretation first. On this account both abstract theories and concrete models

---

unless one of the more concrete descriptions also applies. We can then understand abstract concepts as those concepts that comprise abstract descriptions.

must be the sorts of things that can be "applied," and given a truth-theoretic interpretation this means they must be the sorts of thing that can be true or false. But of course linguistic representations can be true or false. As outlined in §2.1.2, linguistic representations are comprised of truth-apt utterances. Thus linguistic representations can stand in the relevant semantic dependence relations that Cartwright uses to characterize abstract and concrete representations. An abstract linguistic representation can be 'fit out' by a set of more concrete linguistic representations such that the abstract linguistic representation is only true when one or more of the concrete linguistic representations is true.

Similarly when the contingent applicability account is interpreted pragmatically, linguistic representations can stand in the relevant pragmatic dependence relation. On this interpretation a theory is abstract in relation to a set of concrete models if the content of the abstract theory is assertable only in those contexts were the content of one or more of the concrete models is assertable. But of course assertion is a linguistic representative act, so all expressions that are assertable are expressions that can be used to perform linguistic representative acts. Thus abstract theories and concrete models can be linguistic representations because the requisite pragmatic dependence can obtain between a set of linguistic representations.

What the contingent applicability account offers, then, is an account of the relations between abstract and concrete representations that is consistent with the linguistic approach. Linguistic representations are exactly the kinds of things that count as abstract or concrete on this distinction. Moreover, using this version of the abstract-concrete distinction to ground the theory-model distinction results in a theory-model distinction which is also consistent with the linguistic approach. Both models and theories can be linguistic representations in virtue of being the sorts of things that can be relatively abstract, or relatively concrete. So, what Cartwright's *Dappled World* view illustrates is the compatibility between at least one conception of the abstract-concrete distinction, the theory-model distinction, and the classification of theories and models as linguistic representations.

5.3   Some Further Conclusions

In Cartwright's pre and post *Dappled World* views, we see two different proposed ways of distinguishing theories from models by appealing to two different conceptions of the abstract-concrete distinction. I have argued that in neither case does the abstract-concrete distinction provide a means of demarcating between representational and non-representational uses of expressions. On this basis I think we can draw some additional tentative conclusions.

A central motivation in Cartwright's earlier non-representational position seems to draw upon the intuition that abstract theories should not be construed as representational precisely because they are abstract. However, as we have now seen, this intuition is not supported by Cartwright's own work on abstraction. This, I take it, is good reason to be sceptical of any intuitive connection between abstraction, theories and non-representation. If one chooses to try and explicate the distinction between theories and models in terms of abstraction, one should not simply presume that those things that get classified as 'theories' will

be non-representational. Likewise, if one seeks some basis for distinguishing the non-representational products of scientific practice from the representational products, one should not presume that either the theory-model distinction or the abstract-concrete distinction will provide such a basis. A necessary connection between theory, abstraction and non-representation has not been established. Instead Cartwright's work, despite her attempt to forge such a connection, suggests it is generally possible for theories to be both highly abstract and representational. Again, in her pre-*Dappled World* account abstract laws are distinguished from concrete laws with respect to their explanatory power. However a law's failure to explain is no guarantee that it will also fail to represent; many claims represent but do not explain. Moreover on her contingent applicability account the representational status of laws is actually a precondition for a law to be abstract. A description can be abstract only if it is applicable in some circumstances, and 'applicability' is most straightforwardly construed as implicating representation (via truth or assertability). Of course other accounts of abstraction and the theory-model distinction may be proposed. By considering only Cartwright's analyses I do not purport to have shown conclusively that there is no connection between abstraction, theories and non-representation. However these considerations do show that the burden of proof is squarely on the shoulders of those who take there to be a reliable connection between abstraction and non-representation in theorizing practices.

Moreover, as noted earlier, Cartwright's general account of theorizing and modeling (GATM) in her pre-*Dappled World* view (see figure 5.1) remains unaltered in her later *Dappled World* view. Given this continuity and the consistency of her later view with the linguistic approach, we see that most of the things she claims about models and theories in all her works is amenable to the linguistic approach. To be clear I am not endorsing GATM. I shall not venture an opinion here either on the adequacy of her account or its relative strengths and weaknesses in comparison to competing accounts. What I do want to stress is that should GATM, or a similar account, prove to be the best account of theorizing and modeling practices, then the linguistic approach could avail itself of it. As we have seen Cartwright's view maintains a substantive theory-model distinction, which incorporates roles for idealizing and abstracting processes in scientific practices. None of these views concerning scientific practice are problematic from the perspective of the linguistic approach. None of them necessitate construing theories or models as non-linguistic or non-representational. So the linguistic approach is in no way hostile to a picture of scientific practice that maintains central roles for idealization and abstraction, nor is it hostile to a picture of scientific practice that maintains a substantive theory-model distinction. This speaks to the versatility of the linguistic approach. There are many conceptions of idealization, abstraction, and many different ways of distinguishing between theories and models that could be adopted by the proponent of the linguistic approach.

Chapter 6

Discursive Evidence of Linguistic Representation

Our investigation of the discursive aspect of scientific theorizing and modeling has thus far considered three general views: the semantic view (of Suppe, van Fraasen and Giere), Cartwright's pre-*Dappled World* nonrepresentationalism and my own linguistic approach. According to each of these views expressions perform different functions in the discursive aspect of scientific theorizing and modeling practices. On Cartwrights view expressions associated with abstract theory are systematically taken to perform a non-representational function and on the semantic view expressions are systematically taken to perform an indirect linguistic representational function. By contrast the linguistic approach hypothesizes that expressions regularly play a direct linguistic representational function. In the previous chapters I provide some arguments in favour of the linguistic approach and some arguments against the other two competing views.

In this chapter and the next I take up a more specific question about scientific discursive practices. What would count as evidence that a particular utterance is used in one of the three considered functions? What would count as evidence that an utterance is used to directly represent some target, indirectly represent the target or is used in some other non-representational capacity? The goal here is to see how evidence drawn from actual examples of scientific discursive practices can be brought to bear. By helping to determine in which capacity specific utterances are functioning in theorizing and modeling practices we can determine which view, the linguistic approach, the semantic view, or nonrepresentationalism, is best supported by evidence drawn from scientific practices. The linguistic approach will be bolstered by evidence that sentences and equations are regularly used to perform a direct representational function, the semantic view will be supported if it is discovered that sentences and equations are systematically used to perform an indirect representational function, and nonrepresentationalism will be supported by evidence of sentences and equations performing some non-representational function.

The current chapter will focus on distinguishing acts of linguistic representation from the contrast

class of non-representative acts. The subsequent chapter will take up representative acts to establish an evidential basis for distinguishing between direct and indirect representative acts. As you will recall, in §2.1.2 I provided a characterization of linguistic representation that drew upon speech act theory. I associated linguistic representation with a class of speech acts Searle (1976) calls representatives (Bach, Harnish, and Rechanti call them constatives). This class of speech acts includes describing, asserting, theorizing, conjecturing, predicting and more. Linguistic representations are presented when one or more utterances are used to perform a representative speech act. Conversely expressions perform some other non-representative function when they are used to perform other speech acts, which I collectively call non-representatives.

I will now delve a little deeper into speech act theory to see what discursive data can be used to help determine when an utterance is used to perform a representative or when it is used to perform a non-representative speech act. Speech-act-theoretic taxonomies are designed to capture observable features of discursive practices. They not only capture syntactic and semantic features of utterances, but they also capture the social communicative significance of utterances. Acts of speech occur within discourse which has an implicit normative structure. It is against this background structure that utterances acquire their communicative significance and speakers acquire the ability to do drastically different things by uttering words. Consequently speech act theory provides a rich framework for developing an evidential basis for distinguishing representative acts from non-representative ones. To see this I will begin by focusing on assertions. By considering what might count as evidence that an utterance is or is not used to assert we will see both the utility and nuances of speech act theory. Our investigation and conclusions regarding acts of assertion will also serve as a blueprint when we extend our discussion beyond assertions to the entire class of representatives.

What will come out of this discussion is a series of diagnostic questions that can be used to help determine if a particular utterance is or is not used to perform a linguistic representative act. In the last section of this chapter these diagnostic questions will be put to use on a real example, the London brother's model of superconductivity. I have chosen this example as my test case because it is a canonical one that has been extensively discussed by both nonrerpesentationalists and proponents of the semantic view. I shall argue in this chapter that, contra nonrepresentationalists, the discursive data is indicative of linguistic representation. Then in the next chapter where I take up the distinction between direct and indirect linguistic representation I shall argue, contra the semantic view, the London brother's model is a direct linguistic representation of superconductors.

6.1 Assertions

In the philosophical literature accounts of assertion abound. For current purposes we do not need to avail ourselves to a fully developed account in all its complexity and subtlety. As I shall explain, one of the central concepts of speech theory is *illocutionary force*, or *force* for short. Roughly, distinct act types have distinct forces. To determine whether an act of assertion is performed by a particular utterance we need to

determine whether the utterance has the force of assertion. For our purposes in accounting for assertoric force it will suffice to consider just a few features of utterances: their *propositional content*, *grammatical mood*, the speaker's *communicative intentions*, and an utterance's *social normative significance*. In what follows I shall outline the most important ways that these features relate to an utterance's force and explain how consideration of these features can be used to identify evidence that will help us classify specific utterances as assertions.

6.1.1 Content, Mood and Illocutionary Force

Probably the least controversial thing that one can say about assertion is that

    (i)   To assert P is (among other things) to present P as true.

Wright (1992, 34), for example, takes the above to be a mere platitude about assertion. Many other, but related, claims are made (see, for example, Brandom (1983), Dummett (1973), Searle (1976), and Williamson (2000)):

    (ii)   To assert P is to present P as information.

    (iii)   To assert P is to present P as knowledge.

    (iv)   To assert P is to present P as something to be believed.

    (v)   To assert P is to present P as something which it is appropriate to draw inferences from.

    (vi)   To assert P is to present P as representing an actual state of affairs.

All of these statements reflect the general idea that assertions are communicative acts by which people, loosely put, share information.

To try and determine when an utterance counts as an assertion we need to be able to determine, roughly, when an uttered sentence is presented as though it were true. What would count as evidence of this and where is it to be found? The obvious place to start looking for evidence is the uttered expression itself. In this regard two features of expressions seem promising as potential ways of distinguishing assertions. First, assertion usually involves the utterance of declarative sentences which are conventionally expressed grammatically in the *indicative mood*. Second, many sentences include phrases like 'I assert' or 'I claim.' In Austin's terminology (1962), these are phrases that employ *illocutionary verbs*. Illocutionary verbs are terms that denote speech acts. 'Assert' and 'claim,' for example, are two illocutionary verbs associated with acts of assertion.

Unfortunately neither the grammatical mood nor the presence (or absence) of illocutionary verb phrases can be reliably used to distinguish assertions. Some uttered sentences in the indicative mood are not assertions (e.g., one can issue a command by saying "You will remove your hat at the dinner table"). Moreover, in the right context uttered sentences that employ illocutionary verbs associated with assertoric acts are not assertions. An actor on stage during a performance can say "I claim that there is a bomb in the next room!" without presenting the sentence as true.[1]

What is lacking in the actor's utterance? The actor's utterance—which in other circumstances would count as an assertion—fails in this context to have the illocutionary force of an assertion. Lacking the force of assertion the utterance does not have the same communicative significance that genuine assertions have. This example illustrates a general feature of speech acts: the force of an utterance is underdetermined by its propositional content. Two utterances with identical contents can nevertheless have different forces and, thereby, have different significances. It is not that the actor's sentence means anything different than it would if it had been used to assert that there is a bomb in the next room, but in the context it does not have the same significance that it would if asserted.

The divergence between an utterance's content and its force allows a speaker to do dramatically different things with the same sentence. In one context saying "You will be nicer to me next time we meet" may be a prediction, in another context it may count as a command, and in a third context it could be a threat. This is just an extension of the *Frege-Geach point*: "a proposition may occur in discourse now asserted, now unasserted and yet be recognizably the same proposition" (Geach 1965, 449). I shall follow Geach here in taking 'proposition' to mean simply "a form of words in which something is propounded, put forward for consideration" (1965, 449). When a proposition is asserted it is presented as true. However, a proposition can be presented without the speaker actually making any commitment regarding its truth. If, for example, someone were to ask you what the Prime Minister said in his press conference, you could respond by saying "Canada is a Northern European welfare state in the worst sense of the term."[2] In this case you have presented the proposition as something said by the Prime Minister, but you have not endorsed the content of the utterance as true. In fact, in the very same breath you may dismiss the Prime Minister's statement as false without undermining or frustrating your initial utterance. In this case the proposition is expressed but not asserted.

Distinguishing assertions from other speech acts is a matter of distinguishing those utterances with assertoric force from those without. But, what is it that bestows a particular force upon an utterance? We have thus far provided only a partial negative answer to this question: it is not always, if ever, an utterance's propositional content or grammatical mood that determines its force. So one must look to extra-semantic aspects of communication to account for the force of an utterance.

---

[1]This is an adaptation of Dummett's (1973, 310) example.

[2]From a speech by Mr. Harper to the Council for National Policy, June 1997, as reported by the CBC News: http://www.cbc.ca/canadavotes2006/leadersparties/harper_speech.html.

6.1.2 Intentions and Conventions

In accounting for the illocutionary force of utterances both the speaker's intentions (e.g., see Bach and Harnish (1979), Recanati (1987), Searle (1969), Strawson (1964)) and social conventions are important (this is emphasized by Dummett (1973), and Searle (1969)). It is a controversial matter whether conventions or intentions are in some sense more important, or explanatorily prior.[3] For our purposes however this controversy can be set aside. It will suffice to acknowledge that, at least typically, both social conventions and communicative intentions contribute, often in complex ways, to determine the force of a particular utterance.[4] To distinguish assertions from other acts we must identify both the relevant conventional cues and the information that is relevant when determining a speaker's communicative intentions.

A convention can be construed of as a kind of rule. Schematically, a convention is a rule according to which the utterance of a sentence, *X*, under certain conditions, C, *counts as* an act of type T (Searle 1969, 33–42). Of course the propositional contents of a sentence are themselves, at least to a large extent, conventionally determined. But, as we have already seen, these conventions underdetermine an utterance's force because two utterances that express the same contents do not always have the same force. So to the extent that assertions are conventionally determined there must, in addition to semantic conventions, be relevant extra-semantic conventions too. There is at least one obvious extra-semantic convention that governs assertions and it is related to a feature of utterances we have already discussed: grammatical mood. Acts of assertion conventionally involve the utterance of declarative sentences which are in the indicative mood. When a declarative sentence is uttered this counts as evidence, but defeasible evidence, that the utterance has the force of assertion. It is defeasible evidence because, as mentioned, mood is not always a reliable indicator of assertoric force. This is, to a large degree, because evidence regarding the speaker's communicative intentions can serve to override, or frustrate, this conventional cue.

Searle, in an oft quoted passage, nicely sketches the connection between communication and intentions:

> Human communication has some extraordinary properties . . . One of the most extraordinary is this: if I am trying to tell someone something, then (assuming certain conditions are satisfied) as soon as he recognizes that I am trying to tell him something and exactly what it is I am trying to tell him, I have succeeded in telling it to him. Furthermore, unless he recognizes

---

[3]Force conventionalists, like Searle (1969) and Dummett (1973), have suggested that conventions are, in a sense, more fundamental. They argue that it is conventions that give rise to distinct illocutionary act types and this creates the very possibility to perform acts of those types. Opponents to force conventionalism, like Strawson (1964), suggest instead that certain illocutionary acts can be performed in the absence of any conventions. All that is required is that the speaker make their audience come to understand their communicative intentions.

[4]This claim should be acceptable even for conventionalists like Searle and Dummett. Searle (1969, 38) explicitly admits that some illocutionary acts can be performed without using any conventional devices. What seems to be important for both authors is not that each individual act is performed by invoking a convention, but rather that the conventions form the background against which one can perform these acts. Dummett (1973, 362–363) seems to suggest that the very possibility of intending to perform a particular illocutionary act may antecedently require there to be conventions which define the act.

that I am trying to tell him something and what I am trying to tell him, I do not fully succeed in telling him. (1969, 47)

The success of a speaker's attempt to tell the audience something is dependent upon the speaker being able to convey to their audience that they intend to tell them something. Connecting this to assertions, successful acts of assertion involve both having and conveying the intention to assert something.

We now need to clarify what having the intention to assert something amounts to. One important thing people do by asserting is that they give expression to their own epistemic commitments. This, as I call it, is assertion's *expressive function*. They express both some propositional content and their own doxastic attitudes (belief, knowledge, etc.) towards this content.[5] A person's epistemic commitments include those propositions they endorsed as true. For example, I believe that the Eiffel tower is in Paris and I can express this commitment by asserting that the Eiffel tower is in Paris. But what might be the point of expressing one's epistemic commitments? We get traction on this question when we appreciate the significance of assertions as part of information sharing communicative activities. If I know something and I wish to share this knowledge with others, then I can do so by asserting what I know. As an information sharing activity the aim of assertion is not only for the audience to grasp the contents of what I purport to know, but also that the audience will come to endorse the contents themselves and thereby incorporate what was asserted into their own stock of beliefs. This information-sharing perspective on assertion suggests two characteristic communicative intentions. First, by asserting P the speaker intends to convey to the audience their endorsement of P. Second, by asserting P the speaker intends that the audience will also endorse P.

No doubt most *sincere assertions* are made with both of these intentions. However, there are some cases of assertion in which these intentions may be absent. As has been emphasized by Dummett (1973), one important class of assertions are lies. It is by asserting that one lies, but when lying one does not intend by their assertion to express to their own doxastic attitudes—quite the opposite. Suppose I choose to lie to my nephew Charlie and tell him Santa Claus is real. Though I believe Santa Claus to be a fictional character, I assert he is real and do so with the intention of giving the impression that I believe he is real.

The consideration of insincere assertions suggests an amendment to the above proposed intentions is necessary. It is not essential to acts of assertion that the speaker believes the content of their utterance, or that the speaker intends for the audience to believe the content of the utterance. Successful acts of assertion require only that the speaker intends to be taken as though they endorse the content of their utterance.[6] Let

---

[5]It is a matter of debate which attitude is, or ought to be taken as, characteristic of sincere assertions. Searle (1969), for example, suggests assertions express belief while others, like Williamson (2000) suggests knowledge is more appropriate. However, since I take linguistic representation to be associated with a whole class of different acts and these are used to express various doxastic attitudes, for current purposes there is no need to weigh in on the issue.

[6]I take this formulation to be essentially the same as Dummett's. He says "A man makes an assertion if he says something in such a manner as deliberately to convey the impression of saying it with the overriding intention of saying something true" (1973, 300). In Dummett's formulation, the primary intention is to convey an impression, the impression is that the speaker is committed to the truth of their utterance.

us call this the *characteristic intention of assertion*. For example, to assert that Santa Claus exists, whether the assertion is a lie or a sincere expression of one's beliefs, one must intend to be taken as though they endorse the claim that Santa Claus exists. For the audience's part, an attempted assertion is successful only if the audience comes to recognize that the speaker's utterance is made with the characteristic intention of assertion. Once this intention is recognized the speaker has thereby succeeded in being taken as though they endorse the content of their utterance. It is then up to the audience to decide if the assertion is sincere and if they are warranted in endorsing the assertion themselves.

Any information gathered from an utterance's context that indicates the speaker has uttered something with the characteristic intention of assertion is evidence that the speaker has attempted to assert something. Conversely, any contextual information that indicates the characteristic intention of assertion is lacking will count as evidence that the utterance is not an assertion. If the characteristic intention is lacking or is failed to be recognized by the audience, then the utterance fails to have the force of assertion.

For example suppose that immediately upon walking into a room you overhear a friend say "Canada is a Northern European welfare state in the worst sense of the term." You also notice that your friend's utterance is made with a look of disapproval on her face and she also holds up the index and ring fingers of each hand to signal the utterance is a quotation. These are both contextual clues that your friend has not uttered this sentence with the characteristic intention of assertion. Other pieces of evidence may be relevant too. Perhaps you find out that her utterance immediately followed the question "What did Harper say in his address to The Council for National Policy?" and you know the speaker to have advocated in the past for a government that provides a robust social safety net and, otherwise, to disagree with the Prime Minister's policies. From these clues you may reasonably infer that your friend did not intend to be taken as though they believe that Canada is a Northern European welfare state or that this has any negative connotations.

Even from this simple example it is clear that a great variety of contextual information has the potential to be relevant when making inferences about a person's communicative intentions. Consequently, by considering the utterance's context we make available a rich source of evidence that can be used on a case by case basis to determine whether or not an act is an assertion. In the subsequent section, by considering not only an assertion's expressive function but also its social epistemic function, we will be able to identify some of the most salient features of an utterance's context that can be used to determine the force of an utterance.

6.1.3 The Social Epistemic Function of Assertion

Assertions can be observed to play a central role in social, or discursive, reasoning practices. In connection to these practices we can distinguish assertions as performing a characteristic social epistemic function. This aspect of assertion is emphasized by Brandom (1983, 1994) who takes the social epistemic function to be constitutive of the the act itself. For him assertions are essentially moves in reasoning practices. Others, like Searle (1969), see the social epistemic function as a reflection or product of a speaker's communicative

intentions. Regardless, assertions do play a characteristic role in reasoning and this can be exploited to further enrich our evidential basis for distinguishing assertions from other acts.[7] By characterizing assertion's role in explicit reasoning we can identify what counts as evidence that an utterance is, or is not, functioning like an assertion. In turn, this will count as evidence that the utterance in question is, or is not, an assertion.

The social epistemic function of assertion is revealed when utterances are considered in their discursive contexts. As parts of discourse utterances enter into inferential and justificatory relations with one another. Asserting is not just a means of expressing one's own epistemic commitments, but it is an act whereby one makes a kind of *discursive commitment*. This commitment carries with it obligations and licences privileges. Roughly, when one asserts a proposition one takes on an obligation to defend or retract their assertion when questioned or when presented with countervailing evidence. By taking on such a justificatory obligation the asserter, and their audience, gain certain inferential privileges. The speaker having asserted P is thereafter entitled to draw inferences upon P, present these inferences as true, and have the legitimacy of these inferred consequences rest (at least in part) upon the authority of the original assertion P. Similarly, the audience is invited to endorse P as true, and use P to draw further inferences of their own. The audience is entitled to accept P, and its consequences, on the speaker's authority. These obligations and privileges characterize the way in which assertions are 'moves' in the game of the giving and asking for reasons.

The fact that a speaker has made a commitment to stand by their utterance, and thereby accept the corresponding obligations and privileges, is evidenced in the inferential and justificatory practices an utterance is embedded in. If the speaker responds to challenges by defending or retracting the content of their utterance, then we have evidence that the utterance was an assertion because the speaker recognizes and acts in accordance with the obligations incurred by the act of assertion. Since the audience deems it appropriate to challenge the utterance, this is evidence that the audience also takes the speaker to have asserted something. Moreover, if the speaker, or audience, acts as though they are entitled to draw inferences from the speaker's utterance, then this is evidence that the speaker, or audience, takes the utterance as an assertion. We can summarize this in the following way. There are inferential and justificatory norms corresponding to the obligations and privileges that accompany assertions. The speaker and audience should observe these norms when an utterance has the force of assertion. We can call these the *norms of assertion*. Evidence that the norms of assertion are observed by the speaker and interlocutors in the discursive context of an utterance is evidence that the utterance is an assertion. The upshot of all this is that while any contextual information may be relevant when classifying an utterance as an assertion or as a non-assertion, the justificatory and inferential practices as evidenced in an utterance's discursive context

---

[7]Those who do not follow Brandom in taking the social epistemic function of assertion to be constitutive of the act, should still recognize its evidential relevance. Pagin, for example, argues that the social effects of assertions are no part of the act itself. Nevertheless he does recognize that typically "having made an assertion the speaker is responsible for backing up her claim if challenged" (2004, 835). He observes, in effect, that there is a correlation between acts of assertion and the speaker assuming some responsibility for backing up their claim. This correlation is all that is required to secure the evidential relevance of discursive commitments.

is an especially salient source of information.

When considering contextual evidence related to the norms of assertion it is very important to keep distinct what Austin (1962) calls *misfires* and *abuses*. Misfires happen when the speaker tries to perform a speech act but is unsuccessful. Such utterances fail to count as a given type of speech act even if the speaker intended the utterance to be an act of that type. Abuses, on the other hand, are speech acts that are infelicitous. They count as speech acts of the intended type, but they are imperfect and fall short of the ideal.

To illustrate the difference consider the act of bidding. Under the right circumstances uttering "I bid fifty dollars" at an auction counts as a bid. However, not every utterance of this sentence will be successful as a bid. I may, for example, utter this sentence after the auctioneer has closed the bidding. Though I intend my utterance to be a bid, my utterance has misfired because I offered my bid too late. Once the bidding is closed no utterance can count as a bid. On the other hand I could utter "I bid fifty dollars," but fail to have the money to pay for the item if my bid wins. Provided my utterance is intended as a bid and this intention is recognized by the auctioneer before the bidding is closed, then it counts as a bid. It is an abuse, however, because I cannot make good on the obligations that come along with a successful bid. In registering a bid I have taken on an obligation to pay the offered price for the auctioned item. A failure to meet this obligation does not negate the original act itself. I have already succeeded in bidding and have taken a commitment in the process. However I am blameworthy if I do not meet my obligations. My blameworthiness is evidence that my act was an abuse and not a misfire.

The act of assertion can also be abused. Three interrelated kinds of abuses can be identified. First, the potential for abuse arises from the characteristic use of assertion as a means of sharing information. Roughly, one cannot share information by asserting falsehoods. When assertion is employed as a means of sharing information truth becomes an aim of assertion. However, as we have already seen (recall the above discussion of insincere assertions), one can succeed in asserting something despite falling short of this aim. A speaker can successfully assert P even if it turns out that P is false. This abuse can happen either knowingly or not. One might be warranted in believing a false proposition and they may unknowingly assert a falsehood. The more egregious abuses are outright lies. I can succeed at asserting P while knowing P to be false because I can still intend to be taken as though I endorse P as true. To do so is to abuse the act of assertion by using it as a means of spreading disinformation. Two additional kinds of abuses can be identified, each arising from the inferential and justificatory norms of assertion. One may assert P, believe P, but when challenged be unable to offer satisfactory justification for P. In this case the speaker is unable to discharge the obligation to defend their assertion. This is an abuse in the same way that bidding with insufficient funds to pay for the auctioned item is an abuse. Finally, the inferential privileges accompanying assertion can also be abused. If, for example, I purport to be entitled to believe another proposition on the authority of an earlier assertion, but the earlier assertion does not ground the latter, then I have abused my inferential privileges. In all three kinds of abuses, the very opportunity to abuse acts of assertion arises only because the original act was successfully performed.

Attempted assertions can misfire too. For a person to successfully assert a proposition they must

convey their intention to be taken as though they endorse some proposition. This can only happen if an endorsable proposition is presented. One way an assertion can misfire is if the speaker fails to present a truth-apt proposition. This can happen when a person misspeaks and utters an ill formed or otherwise incomplete sentence. I may unwittingly utter the phrase "The cat mat," when I had intended to say "The cat is on the mat." Since "The cat mat" is not a truth-apt expression, and in this context the audience has insufficient information to know what truth-apt expression I may have been trying to convey, the utterance cannot count as an assertion regardless of my communicative intentions. Attempted assertions can also misfire if the there is no *uptake*. This happens when the audience fails to recognize the communicative intentions of the speaker. If the audience does not hear the speaker, or cannot understand their words, then the speaker has not succeeded in asserting anything.[8]

I have taken the time to outline the distinction between misfires and abuses so as to avoid tempting but misguided inferences. One may be tempted to categorize all false utterances as non-assertions (and likewise tempting to categorize all false theories and models as non-representations). This is a mistake. Only evidence of a misfire is direct evidence that an utterance fails to be an assertion. An utterance may still be an assertion if the content of an utterance is false, the speaker does a poor job justifying their utterance, or the speaker draws dubious inferences from something they have said. What is important is the recognition that the speaker is obliged to justify their utterance when challenged, the recognition that one is entitled to draw inferences from their utterance and the recognition that what has been presented is appropriately evaluated as true or false. Evidence that the speaker or audience recognizes that the norms of assertion are in force in the context of a particular utterance is evidence that the corresponding utterance is an assertion; evidence that the speaker or audience does not recognize the norms of assertion to be in effect is evidence that the utterance does not have the force of assertion. When people behave as though they are attempting to observe the norms of assertion this is evidence that they recognize the norms of assertion to be in effect.

6.2 An Evidential Basis for Distinguishing Representatives

Numerous sources of evidence have now been identified as relevant when distinguishing assertions from other acts of speech. If the uttered sentence is in the indicative mood or it contains the relevant illocutionary verb phrases, like 'I assert,' 'I claim' etc., then there is evidence that the utterance is an assertion. An utterance's context can also be drawn upon when distinguishing assertions. Any contextually available information indicating that the speaker utters a sentence with the characteristic intention of assertion is evidence that the utterance is an assertion. Also, when an utterance is embedded in a discursive context where the norms of assertion are observed or the interlocutors act as though they are attempting to observe

---

[8] Misfires due to a failure of uptake prove to be less important for the purposes of this dissertation. We are ultimately looking for evidence that bears on whether theories and models are linguistic representations. When the only problem with an attempted act of assertion is a failure of uptake, we can still say that the person presented a linguistic representation. It is just a representation which fails to be successful as a means of communicating something to others.

these norms, then we have grounds for thinking the utterance is functioning in social epistemic practices as an assertion. The challenge now is to extend this analysis of assertion to the broader class of representatives. This requires attending to the relevant similarities and dissimilarities between assertions and other types of representatives.

In §2.1.2 I characterized representatives with respect to four different features of utterances: propositional content, direction of fit, illocutionary purpose, and strength of commitment. Representatives have the following features:

1. Representatives are truth-apt and express propositional contents.

2. Representatives have a word-to-world direction of fit.

3. Representatives are acts which characteristically aim at truth, and thus are assessable on the dimension of assessment which includes true and false.

4. Representatives involve epistemic commitments of varying strengths:

   (a) Representatives are a means of expressing doxastic attitudes of differing strength towards the content expressed, and

   (b) Representatives are acts by which the speaker makes discursive commitments of differing strengths.

As noted, the first three features are especially important when contrasting representatives with non-representatives. However it is the fourth feature, strength of commitment, that is especially important when capturing salient differences among speech acts within the class of representatives.

So a crucial difference between assertions, hypotheses, conjectures, and other representatives is that each involves a different level of epistemic commitment. As explained in the above section, assertion involves a strong commitment. To assert is to present oneself as fully endorsing the content of their utterance. However, one can make a weaker commitment to the same propositional contents. One can present themselves as making a partial or attenuated endorsement of the content of their utterance. By making a weaker commitment one is nevertheless still using an expression to represent. So different linguistic representative acts reflect a spectrum of commitments that can be made when expressing truth-apt representations via sentences and equations. As I shall now explain, to a first approximation different representative speech acts can be construed as assertions that have been attenuated or modified to reflect weaker epistemic commitments. The evidential basis we developed above for distinguishing assertions will then serve as a blueprint for the entire class of representatives. This blueprint need only be adapted to reflect differing levels of commitment associated with different representatives.

As explained above in connection to assertion, evidence relevant when determining the illocutionary force of an utterance can come both from the features of the uttered expression itself and from the context of utterance. With respect to the former, two relevant features can be highlighted (though other features

may be relevant too). All representatives involve the expression of truth-apt propositional content. Since the indicative mood is the conventional mode by which truth-apt claims are presented, the mood of the uttered sentence is evidence that an utterance is or is not a representative. Moreover, as with 'assert,' there are illocutionary verbs, like 'hypothesize,' 'conjecture,' 'guess,' etc., associated with different representatives. These words can be used by speakers to help make the force of the utterance explicit in the utterance's content. However, we must recognize that the grammatical mood of an utterance and its propositional content underdetermines the utterance's force.

Evidence that an utterance is a representative must also be drawn from its context. Again, distinct speech acts have distinct illocutionary forces. The force of an utterance is determined, in part, by the social conventions governing that act and by the communicative intentions of the speaker. Representatives are performed with communicative intentions similar to the characteristic communicative intentions of assertion. When representatives are sincere they express the epistemic commitments of the speaker. A sincere assertion of P expresses the speaker's commitment to P's truth. Likewise, when sincerely hypothesizing that P a speaker expresses, roughly, the speaker's commitment to P's likelihood. The analysis of insincere assertions is extended to the entire class of representatives in the expected way. When a speaker makes an insincere assertion the speaker nevertheless intends to be taken as though they endorse their utterance as true. An insincere hypothesis P is similar. In this case the speaker intends to be taken as though they endorse P as likely true. Evidence gathered from the context that the speaker's utterance was made with this intention is evidence that the utterance has the force of a hypothesis. Evidence that the speaker has no intention to be taken as making some modified or partial endorsement of the content of the utterance is evidence that the utterance is not a hypothesis or any other representative.

Finally the context also yields information pertaining to an utterance's social epistemic function. All representatives are moves in the game of the giving and taking of reasons. They are a means of making discursive commitments and, accordingly, by performing a representative act the speaker takes on justificatory obligations and receives inferential privileges. Just like assertion, each distinct type of representative act has its own characteristic norms, e.g., the characteristic norms of hypothesis, the characteristic norms of conjecture, etc. By considering the justificatory and inferential practices that accompany an utterance we can see whether an utterance is being used in ways that conform with the norms governing specific representative acts.

To illustrate how the social epistemic function of different representatives compare consider hypothesizing and asserting. Asserting involves putting forward a claim as true, defending, revising or retracting the claim when challenged, and acts of assertion can be appealed to as warrant for further inferences. Hypothesizing has a similar normative structure as assertion, but the norms reflect the weaker commitment hypotheses carry. To hypothesize that P is, roughly, to put forward P as likely true. To perform the act of hypothesis one must convey the intention to be taken as putting forward a claim as likely true. The speaker having forwarded the hypothesis P is thereafter entitled to draw inferences that follow from P's likelihood, present these inferences as true, and have the legitimacy of these inferred consequences rest upon the legitimacy of the original act of hypothesis. Similarly the other participants in the conversation are invited to accept P as likely and use P's likelihood to draw further inferences of their own. For exam-

ple, if Q is entailed by P and P is likely true, then this warrants the inference that Q is also likely true. Offering a hypothesis carries with it obligations. When asked, the person who forwards a hypothesis is obliged to offer reasons which justify the hypothesis's likelihood. Moreover when challenged the speaker has an obligation to defend, revise or retract their hypothesis. Of course the relevant standards of evidence are different when hypothesizing and when asserting. One may have sufficient evidence to warrant hypothesizing P, while having insufficient evidence to warrant the full endorsement of P. Correspondingly, what may be sufficient grounds to oblige one to retract an assertion, may be insufficient grounds to oblige one to retract a hypothesis because the strength of evidence required to warrant the belief that a claim is likely to be true is less than the strength of the evidence required to warrant the belief that a claim is true.

As with acts of assertion, acts of hypothesis can be abused and attempted hypothesizing can misfire. The clearest cases of misfires involve a failure to convey a complete truth-apt expression. Even if the speaker utters "I hypothesize that cat mat" with the requisite communicative intention the utterance fails to count as a hypothesis. Abuses result from a failure on the speaker's part to act in accordance with the norms which govern the act. By hypothesizing one makes a commitment to produce supporting reasons when challenged. If one fails to produce these reasons when prompted, then they have abused the act of hypothesizing. They have nevertheless still hypothesized because they have taken on the corresponding commitments and can fairly be admonished for failing to meet them.

Evidence that the norms of hypothesis are (or are not) observed can be gathered from the justificatory and inferential practices surrounding an utterance. In this regard, acts of hypothesis share many of the same features of assertion. For instance, as with assertion, those hypothesizing can be expected to demonstrate their recognition of an obligation to retract, refine, or defend their hypothesis in the light of countervailing evidence. Suppose, for example, a friend utters "There are no blue lobsters" and you suspect this to be offered as a hypothesis. If it is a hypothesis, then showing your friend a photograph of a blue lobster ought to prompt a response. They may challenge the offered evidence, "That is not a true colour photograph"; they may revise or refine their hypothesis, "There may be blue lobster-like creatures, but if it's blue it is a different species"; or they may simply retract their hypotheses by saying "I guess I was mistaken."

The difference between a hypothesis and an assertion is reflected in the differing levels of commitment interlocutors display. For example, the challenge "How do you know there are no blue lobsters?" is one that the speaker is obliged to address if they are asserting a claim, but it can be dismissed as inappropriate in the case of hypothesis, e.g., "I never said I did know, it is only a hypothesis." Also, the speaker who offers a hypothesis is likely to act as though they have discharged their justificatory obligation with weaker supporting reasons than the speaker who asserts the same proposition. It may be difficult at times to determine whether an utterance is an assertion, a hypothesis, or a representative involving an even weaker commitment. However, it is much easier to classify the utterance with respect to the broad class of representatives, or the even broader class of non-representatives.

In cases where language is used in non-representative ways we should not expect the speaker to act as though they are committed to the content of their utterance in any way. If "There are no blue lobsters" were

105

a line in a song being sung by the speaker, then we can imagine their reaction to the audience's challenge "how do you know?" would be much different. The speaker is likely to act as a person responding to a question that is inappropriate in the context: they may express confusion and ignore the challenge, they may seek clarification of the question by uttering "what to you mean 'how do I know'?," or they may proceed to explain what they were doing, "Oh, I was just singing."

Pulling together all of the kinds of information identified as relevant when distinguishing representatives from non-representatives, the following is a set of diagnostic questions that can be used to help determine if a particular utterance is, or is not, a linguistic representative act.

(i) Is the utterance truth-apt?
(ii) Is the utterance in the indicative mood?
(iii) Are there illocutionary verbs in the uttered sentence itself, or in the surrounding discourse, that indicate the intended force of the utterance?
(iv) Is there evidence in the utterance's context that suggests the speaker intends to be taken as though they endorse, in at least some partial or attenuated way, the content of the utterance?
(v) Do the speaker and audience act as though they are attempting to observe the discursive norms associated with representative acts?

(v.i) Does the speaker act as though they are obliged to defend the content of their utterance?
(v.ii) Do the speaker or audience act as though they are entitled to draw inferences from the utterance's content?

Answering 'yes' to any of these questions is positive evidence that the utterance is a linguistic representative act and conversely 'no's are positive evidence that the utterance is used to perform a non-representative act.

6.3   London Account of Superconductivity

We have thus far outlined in some detail an evidential basis for distinguishing linguistic representative acts from non-representational linguistic acts. It is now time to put the linguistic approach and its competitors to the test. Though a comprehensive investigation of scientific modeling and theorizing is not possible in this work, I want to use a canonical example, the London account of superconductivity, to illustrate how the linguistic data can be analysed to evaluate the linguistic approach. I have chosen the London account of superconductivity because it has received a great deal of attention from both nonrepresentationalists and proponents of the semantic view. Both camps have laid claim to the London brothers' work on superconductivity arguing it is an example of scientific modeling best captured by their respective accounts. By

considering this example in detail, I hope to compare the linguistic approach to its competitors on their terms. If it can be shown that the linguistic approach is superior in even this case, then it will go a long way towards establishing the linguistic approach as a plausible alternative to its more well established competitors.

6.3.1 London Model and Nonrepresentationalism

In their well known 1935 paper the London brothers, Fritz and Heinz, proposed a new account of superconductivity.[9] Superconductors, as their name implies, are special materials that act as (near) perfect resistanceless conductors of electricity at low temperatures. Since superconductors are resistanceless conductors, even in the absence of some supporting electromagnetic field to drive a current, stable currents can persist in superconductors indefinitely. These *persistent currents* are one feature that any adequate model of superconductivity must account for. Before the London brothers, models capturing persistent current had been developed by treating superconductors like ferromagnets (French and Ladyman 1997). However, these models also imply that superconducting materials have a persistent magnetic field "frozen in" when the superconductor was last cooled below its transition temperature. As the London brothers point out, "the existence of "frozen in" magnetic fields in superconductors was believed to be proven theoretically and experimentally" (1935, 73) until the experimental work of Meissner and Ochsenfeld. Their work revealed that after the transition to the material's superconducting phase the magnetic field lines are pushed from the interior of the material. As a result, no magnetic field remains in the bulk of the superconductor. This phenomenon, now called the *Meissner effect*, could not be captured by previous models. To address this shortcoming the London brothers offered a new set of equations which could be used to capture both the Meissner effect and persistent currents in superconductors.

Cartwright, Shomar and Suárez argue that the London account supports a non-representational construal of theories. According to this construal the equations that comprise theories "should not be viewed as claims about the nature and structure of reality which ought to have a proper propositional expression that is a candidate for truth or falsehood," nor does it makes sense "to talk of "reading the theory" literally, nor to talk of what the theory "implies" (1995, 138, 139).[10] Instead theories are tools which aid in the construction of representative models. They take the London model as supporting this brand of instrumentalism because it is, they claim, a clear example of phenomenological model building (1995). They contrast phenomenological model building from *theory-driven views* of modeling. On a theory-driven view models are either derived from the background theory (supplemented with appropriate boundary conditions or physically motivated auxiliary assumptions) or the product of some de-idealizing process

---

[9]Though 'superconductor' is now the accepted terminology, the London brothers refer to them as 'supraconductors.'

[10]In Carwright, Shomar and Suárez (1995), the authors indicate that the section outlining a nonrepresentational approach to theories is written by Cartwright, and the other sections, including the discussion of the London model of superconductivity, were written by Shomar and Suárez. It is a little unclear whether Shomar and Suárez completely endorse Cartwright's nonrepresentationalism, as the authors say only that both parts of the paper are "complementary." However in the absence of a clear disavowal of Cartwrights view I will, for simplicity's sake, assume all the authors do endorse everything written in the paper.

(involving the addition of correction terms) performed on a model derived from the theory. Phenomenological models, on the other hand, are not directly derived from the theory, but instead are constructed using heuristics, practical knowledge, and *ad hoc* tricks. The result are models built employing "methods and aims" (1995, 148) distinct from those involved in scientific theorizing. They are representations whose construction is driven by the phenomenon observed.

Why think that examples of phenomenological model building, like the London account, are evidence of nonrepresentationalism? An answer to this question, while sketched in Cartwright *et al.* (1995), is fleshed out in more detail in Cartwright and Suárez's (2008) followup paper. There they explain that the London model is incompatible with Ohm's law, but "there is nothing in the theory and the descriptions of different materials that legitimates using Ohm's law for some materials while holding it in abeyance for those that turn out to be superconducting" (2008, 65). The implication here is that consideration of the background theory, in this case electromagnetic theory, would suggest that Ohm's law should apply to all conductors alike.[11] They draw two consequences from this *ad hoc* restriction upon application of electromagnetic theory:

> First, the ability of vast evidence for electromagnetic theory to carry over to confirm the occurrence of the phenomena was undermined ... Second, the capacity of the success of the London model to confirm the theory used in its construction is eroded. (2008, 65–66)

They claim the independence of phenomenological models from theory severs the direct evidential link between the two. However, they argue the London model proved to be an important advancement to our understanding of superconductors. This success of the model "argues for the instrumental reliability of the theory rather than its truth" (2008, 66). Its success cannot confirm the truth of electromagnetic theory because of the evidential disconnect between theory and model. But the theory is one tool (among many) that aided in the model's construction and so the theory demonstrates its instrumental reliability as a tool for successful model construction.

We must evaluate Cartwright *et al.*'s analysis of the London model against the discursive evidence. Before doing so, however, let me make a few comments about the arguments offered by Cartwright and Suárez's connecting phenomenological model building and nonrepresentationalism. If we insist, as Cartwright and Suárez do in their earlier work, that theories have no "proper propositional expression that is a candidate for truth or falsehood" (1995, 138), then it is unclear in what sense electromagnetic theory could be supported by "vast evidence." On such an account theories cease being the kinds of things that one can straightforwardly have evidence for or against—one cannot, for example, have evidence in

---

[11]There is reason to worry that Cartwright and Suárez are placing too much significance on the incompatibility of the London model with Ohm's law. Ohm's law is one that many physicists do not take to be part of classical electromagnetism. As Griffiths says: "I don't suppose there is any formula in physics more widely known than Ohm's law, and yet it's not really a true law, in the sense of Gauss's law or Ampère's was; rather it is a "rule of thumb" that applies pretty well to many substances. You are not going to win a Nobel prize for finding an exception" (1998, 289).

favour of a hammer, or any other non-representational tool. I think this highlights how radical a non-representational account of theories is. Second, even if we agree that the London model is a phenomenological model with an independence from electromagnetic theory and that this severs the evidential link between them, this alone is insufficient grounds for thinking the theories are non-representational. The independence between theory and model may show that the success of the model offers little grounds for believing the theory is true, but it does not show that the theory is not truth-apt. Representations can themselves be useful tools for constructing other representations. Consequently, the instrumental reliability of a theory with respect to model construction does not preclude it from being a representation. I don't think Cartwright and Suárez's arguments here are very compelling. One can easily accept their characterization of the London model as the product of phenomenological model building practices and still reject their nonrepresentationalism.

On the other hand, if theories are non-representational as Cartwright *et al.* propose, then the theory-driven view must be incorrect. There is no way to think of models as derived from theory because theories, unlike models, do not have propositional contents. On this story models and theories are different kinds of objects which play distinct roles in scientific practices. So evidence of an independence of models from theory, including evidence of phenomenological modeling building, is at least consistent with their nonrepresentationalism. It is important, then, to look at the work of the London brothers to see if there is additional reason to suppose that they use theory exclusively in a non-representational way. For this we can draw upon the evidence available from discourses in which the theories and models are presented and used. For the discursive evidence to support Cartwright *et al.* analysis we should expect to find those expressions associated with the background theory functioning in discourse as non-representatives. We can do this by considering the diagnostic questions outlined in §6.2.

6.3.2   An Examination of the Discursive Evidence

I will follow Cartwright *et al.* and focus my discussion on the London brother's 1935 paper where they first proposed their account of superconductivity. This way I will be examining the exact same passages and expressions that Cartwright *et al.* appeal to when both arguing against the theory-drive view, and arguing for a nonrepresentational construal of theories. In the London brothers' paper—like most papers published in physics—the London brothers present a series of equations. Some of these equations are used to express (or characterize) their own account of superconductivity, other equations express competing accounts, and still others are associated with the background theory of electromagnetism. If there is evidence that some of the equations presented in the paper are non-representatives, then we would have a clear historical example of modeling that counts against the linguistic approach. Moreover, if we see a difference between the way equations associated with theory are used in relation to the other equations presented in the discourse, then we could have grounds for thinking that theory and model play distinct representative roles in this case. This would corroborate Cartwright *et al.*'s selective non-representational construal of theories.

However, the evidence does not support either of these conclusions. Instead the evidence speaks clearly in favour of the linguistic approach. The evidence indicates that: (1) the equations central to their own account, including the *London equation*, are used by the brothers as linguistic representations of superconductors; (2) the equations associated with the background theory (in this case Maxwell's equations) uttered in the context, like the other equations presented in the paper, play an inferential role indicative of their use as linguistic representations; and (3) the London account is best construed as a complex linguistic representation which is partly constituted by some of the content of the background theory.

In their 1935 paper the London brothers motivate and develop their model in a number of steps. First they consider an alternative approach centered upon the *acceleration equation*,

$$\Lambda \dot{\mathbf{J}} = \mathbf{E}. \tag{6.1}$$

This equation relates the electric field $\mathbf{E}$ to the time derivative of the current density $\dot{\mathbf{J}}$ (Λ is a constant determined by the mass and charge of the number of free electrons in a unit volume). It can be derived from electromagnetic theory if superconductors are treated as a limiting case (i.e., zero resistance) of ordinary conductivity (London 1937). As the London brothers observe, the acceleration equation can be used to account for persistent currents within superconductors even when $\mathbf{E} = 0$. However, 6.1 also implies that magnetic fields persist "frozen in" when the superconductor transitions into its superconducting phase. This runs afoul the Meissner effect. On these grounds they reject the acceleration equation and propose instead the following equation:

$$curl\Lambda \mathbf{J} = -\frac{1}{c}\mathbf{H}. \tag{6.2}$$

This equation is now typically called the *London equation* in the literature and the London brothers suggest it is "the fundamental equation which replaces Ohm's law in supraconductors" (1935, 73) The London brothers argue that this equation (which relates the current density, $\mathbf{J}$, to $\mathbf{H}$, which is often called the "magnetic field"[12]) can account for both persistent currents and the Meissner effect.

A good place to start our investigation is by considering whether either the London equation or the acceleration equation are used as representatives. It turns out that there is excellent evidence to suggest that both utterances are representatives. The best reason for thinking so is that the Londons explicitly refer to them as descriptions. At the beginning of their paper they note that "field strength $\mathbf{E}$ and the current density $\mathbf{J}$ has sometimes been described by means of [6.1]" and then they reject the acceleration equation on the grounds that it "gives too general a description" (1935, 71,73). Their own account offers "a new description of the electromagnetic field in a supraconductor" (1935, 71). The illocutionary verb 'describe' is used in the context to indicate equations 6.1 and 6.2 are intended as descriptions.[13] Moreover, they talk about these equations (and others) as "propositions" which "say" or "express" things about superconductors, and equations which have "content" which can be compared (1935, 73–74). All of these

---

[12]While $\mathbf{H}$ is often called the 'magnetic field' it is related to the true magnetic field, $\mathbf{B}$, but not identical to it. See, for example, Griffiths (1998, 269–273).

[13]In Fritz London's 1937 we find him similarly talking about equations as descriptions.

locutions in the surrounding discourse indicate that these equations are treated as contentful expressions. So the answer to the diagnostic question (iv), "Are there illocutionary verbs in the uttered sentence itself, or in the surrounding discourse, that indicate the intended force of the utterance?", is an unambiguous "yes."

With respect to the London equation and the acceleration equation the Londons also appear to be observing the discursive norms associated with acts of description. First, they act as though they recognize an obligation to defend, revise or retract the content of each description in light of countervailing evidence— an affirmative answer to diagnostic question (v.i). It is information about the nature of superconductors, specifically the Meissner effect, that the London brothers site as grounds for rejecting the acceleration equation. They also work to show that their own model can account for the same empirical evidence. This effort discharges their obligation to defend their favoured description against the same challenge that led them to reject the acceleration equation. Also having uttered 6.2 the London brothers act as though they are entitled to draw inferences from this equation about superconductors. This is a positive answer to question (v.ii). They draw inferences from the London equation, in conjunction with other utterances, to show their model can account for both persistent currents and the Meissner effect. In subsequent sections of the paper they also draw upon the London equation to consider its implications for modeling superconducting spheres and superconducting wires. In the latter case they suggest that these implications show quantitative conformity with experimental evidence gathered by de Haas and Voogd (1935, 85–86).

By taking the London brothers at their word and construing their inferential practices in a straightforward way, we can conclude that they present and evaluate two competing descriptions of superconductors and then choose one (their own account) as the better description of the two. This is a clear example of linguistic representation in physics. This conclusion, however, does not completely address the non-representationalism proposed by Cartwright, Shomar, and Suárez. They deny only that theories, and not models, are representative. They may counter that the descriptions presented by the London brothers are models and not theories. Even if the models are linguistic representations this does not mean the background theory used to help construct the models is also a representation. So we need to consider whether the data supports this selective nonrepresentationalism.

The London brothers explicitly present and use some of Maxwell's equations. They make use of the Maxwell-Faraday equation and the Maxwell-Ampère equation when drawing inferences from the acceleration equation and in the course of deriving the London equation. Both are uncontentiously core equations of electromagnetic theory. Moreover, the Londons appear to use Maxwell's equations in the same way they use all of the other uttered equations in the paper. This continuity of use suggests that all equations presented in their paper are intended to have the same descriptive force. Moreover, their discursive activity itself, i.e., the act of uttering a series of equations, is intelligible as reasoning only when we treat these utterances in a consistent way. Let me elaborate on these points by considering parts of the London brother's paper in more detail.

Consider first the London brother's treatment of the acceleration equation (1935, 72–72). They start

with 6.1, take the curl of both sides, and then use the Maxwell-Faraday equation,

$$curl\mathbf{E} = -\frac{1}{c}\dot{\mathbf{H}},\tag{6.3}$$

to obtain

$$curl\Lambda\dot{\mathbf{J}} = -\frac{1}{c}\dot{\mathbf{H}}.\tag{6.4}$$

They then appeal the Maxwell-Ampère equation,[14]

$$\frac{1}{c}\mathbf{J} = curl\mathbf{H},\tag{6.5}$$

to derive the following equation from 6.4:

$$curlcurl\Lambda\dot{\mathbf{H}} = \dot{\mathbf{H}}.\tag{6.6}$$

From here, recognizing that $div\mathbf{H} = 0$ and integrating with respect to time they obtain the following inhomogeneous equation for $\mathbf{H}$:

$$\Lambda c^2\nabla^2(\mathbf{H} - \mathbf{H_0}) = \mathbf{H} - \mathbf{H_0}.\tag{6.7}$$

They argue the solutions of this inhomogeneous equation allow for "frozen in" magnetic fields which need to be ruled out if the Meissner effect is to be accounted for by the model. It is 6.7 that the London brothers identify as ultimately warranting the rejection of the acceleration equation.

How should we interpret this series of utterances? The simplest, most consistent, and most explanatory construal of the linguistic evidence is one upon which the London brothers endorse both the Maxwell-Faraday equation and the Maxwell-Ampère equation in their paper. Moreover, given the inferential/justicatory relations between these equations and the other propositions expressed in the paper, there is no neat and tidy way to isolate the content of the model and that of the theory in a way that would facilitate commitment to one without the other. The equations 6.1 and 6.3–6.7 collectively express a complex linguistic representation (a description) and this representation, as a whole, is ultimately rejected.

In their derivation of 6.7, they appear to present and draw inferences from Maxwell's equations (6.3 and 6.5) in exactly the same way that they present and use the acceleration equation. There is nothing in the context that would suggest that either of Maxwell's equations presented are intended to have any force other than the descriptive force explicitly attributed to the acceleration equation. Also, if we take the London brothers to be outlining their reasons for rejecting the acceleration equation by presenting this series of utterances, this reasoning is intelligible only if we take the London brothers as endorsing the contents of 6.3 and 6.5. By endorsing the contents of these utterances they are treating the equations as the sorts of things that can legitimately be appealed as justifying inferential moves. The London brothers are entitled to draw conclusions about the acceleration equation from the consequences of the inhomogeneous

---

[14]The Londons note that they are neglecting the displacement current (1935, 72).

equation, 6.7, because the inhomogeneous equation follows from the acceleration equation and other propositions they have already endorsed, i.e., Maxwell's equations. Maxwell's equations can only play a role in this line of reasoning if they are truth-apt expressions which are endorsed by the London brothers.

Similar things can be said about the London brother's positive account. The Maxwell-Ampère equation is also explicitly appealed to when deriving the London equation. Again, the inhomogeneous equation 6.7 is identified by the London brothers as furnishing solutions that conflict with empirical evidence. But they observe that in special cases, when $\mathbf{H_0} = 0$, the problematic solutions are eliminated. As an alternative to 6.7, they propose the homogeneous equation,

$$\Lambda c^2 \nabla^2 \mathbf{H} = \mathbf{H}. \tag{6.8}$$

They claim this to be a "fundamental law" (1935, 73), and then immediately derive the London equation from this homogeneous equation. Again, 6.2, 6.5 and 6.8 collectively comprise a complex linguistic representation, which forms the core of the London model. Simply put, the Maxwell-Ampère equation expresses part of the content of the representation and it is this content that makes 6.8 (and its consequences) inferentially relevant to the London equation. In other words, if the Londons do not endorse the content expressed by the Maxwell-Ampère equation, then the consistency of 6.8 with experimental evidence does not offer any grounds for endorsing the London equation as an adequate "description of the electromagnetic field in a supraconductor"(1935, 71). For this reason, I don't see any way to selectively construe the model as representative while maintaining a Cartwright-style nonrepresentationalism about all things theoretical.

Suppose we were to follow Cartwright *et al.* and affirm that Maxwell's equations, 6.3 and 6.5, do not have a "proper propositional expression that is a candidate for truth or falsehood" (Cartwright et al. 1995, 138). How then could we understand what the London brothers are doing by presenting the equations 6.1 and 6.3–6.7? What might the communicative significance be of the equations 'derived' from Maxwell's equations?[15] On the one hand, we could construe the derived equations, 6.4, 6.6, and 6.7, as contentful truth-apt linguistic representations despite being derived from non-propositional expressions. This has two unhappy consequences. First, it means that the subsequent expressions are endorsed without any explicit justification and we are left with an implausibly uncharitable interpretation of the London brother's reasoning. Second, we have a seemingly unprincipled method of analyzing expressions. We construe some expressions in the paper (6.1, 6.4, 6.6, and 6.7) as representations and others (6.3 and 6.5) as non-representations despite a lack of textual evidence to support this inconsistent treatment. Alternatively we could construe all equations derived from a non-propositional expression as equally non-propositional. In this case, equations 6.3–6.7, and every English sentence they are embedded in, fail to be candidates for truth or falsehood. This means very little of what the London brothers wrote about the acceleration equation can be construed as saying anything—they are doings of some other kind. Consequently, very little of what was written could be construed as an attempt on the part of the London brothers as presenting

---

[15]Here 'derive' is in scare quotes because this question only makes sense for Cartwright *et al.* if the word is understood in some way that does not connote inference.

reasons. The series of equations presented by the London brothers could not be understood as inferential and justificatory moves because they are non-propositional and, therefore, are not the sorts of things which can be offered as reasons for endorsing or rejecting anything. This leaves unexplained both what is being done by the London brothers when presenting this series of (non-propositional) expressions and how these doings have anything to do with their goal of producing "a new description of the electromagnetic field of a supraconductor" (1935, 71).

I want to finish my discussion of Cartwright, Shomar and Suárez's treatment of the London account by addressing some of their other claims. Recall they argue that the London model is an example of phenomenological model building. The construction of phenomenological models, unlike models constructed in the way described by the theory-driven view, is guided by aims and methods independent of those which drive theory. Cartwright *et al.* argue this independence of phenomenological models from theory is grounds for accepting their instrumentalist view regarding theories. I have now shown the last step in this line of reasoning is unwarranted: their analysis is not born out by the evidence gathered. However, their characterization of the London model as a phenomenological model can be accommodated by the linguistic approach. By construing models and theory as linguistic representations one does not presuppose any particular relationship between theory and models. The advocate of the linguistic approach can accept that models, in many or most cases, are constructed in ways that make them independent of theory. This independence can be captured by reference to the contents of these representations. Because of the inclusion of domain specific information, *ad hoc* construction techniques, and pragmatic constraints on model building, models will typically have propositional contexts that are different from the relevant background theory. It is the divergent contents of theories and models that make the two representations independent.

In the case of the London model, though some of electromagnetic theory (i.e., the contents expressed by the Maxwell-Ampère equation) is part of the content of the model, not all of electromagnetic theory is explicitly endorsed by the London brothers in the course of presenting and defending their model of superconductivity. Insofar as one takes Ohm's law to be a part of electromagnetic theory and takes the London model to be in conflict with Ohm's law, then the contents of the two representations are incompatible and, in at least this sense, the model and theory are independent.[16] Though the London brother's model shares some content with electromagnetic theory, the contents are divergent and the model and theory will each logically entail different consequences. This opens up possibility that the model and the theory are confirmed or disconfirmed by different evidence. Moreover, this divergence in content makes it impossible for one to derive (by some non-ampliative inference) the model from the theory. In this way the London model, despite being a linguistic representation, may still be a counter-example to the theory-driven view.

In general we can expect the contents of models and theories to diverge for relatively mundane reasons. As is often pointed out, very little follows from the fundamental laws of scientific theories all by themselves. It is not surprising, then, that additional information must be incorporated in more domain-specific representations if they are to provide details about a system's behaviour. If we call the latter

---

[16]There is reason to doubt that Ohm's law should be construed as part of electromagnetic theory. See note 11 above.

representations 'models,' models have different contents than the background theory and, in a sense, are independent for this reason. Also, due to any number of extra-theoretic considerations (often pragmatic considerations), scientists may incorporate assumptions or features in their models that result in inconsistencies with the background theory. These inconsistencies would constitute an even starker example of theory-model independence, but nevertheless a notion of theory-model independence that is comfortably captured within the framework of the linguistic approach.

Chapter 7

Discursive Evidence of Direct Linguistic Representation

In the last chapter we considered the distinction between representative and non-representative uses of language-like expressions. That investigation yielded a set of diagnostic questions that could be used to help determine when particular utterances in a discourse are being used in a representative or non-representative way. In this section I take up the distinction between direct and indirect linguistic representation with the aim of both clarifying the distinction and identifying what could count as evidence that expressions are used in either capacity.

As explained in §2.1.3, when expressions are used in a direct linguistic representational capacity they are used to directly represent the system(s) under investigation in the context. On the other hand expressions are used in an indirect linguistic representational capacity when they function in a multi-stage, or mediated, representational relation. Here the expressions are used to represent some mediating object, or class of objects, (which, according to most versions of the semantic view, are mathematical structures) and these mediating objects in turn are used to represent the system under investigation. In this chapter we shall consider what discursive data might be brought to bear when trying to determine whether individual utterances are functioning in one of these two ways.

My investigation of direct and indirect linguistic representation will proceed by first considering *explicit acts of indirect linguistic representation*. Explicit acts of indirect representation are the clearest examples of indirect representation because the indirect representational role of an utterance, or series of utterances, is made explicit by the speaker. I will then move to consider *implicit acts of indirect linguistic representation*. In these cases the indirect representational relation between an expression, mediating objects, and the target system is not made explicit by the speaker. To determine if utterances are used implicitly as indirect representations, I shall argue we can look to their roles within discourse, especially their justificatory and inferential roles. This will furnish an evidential basis for distinguishing direct and

indirect representative acts even when the representative role of utterances is left implicit. As I did in the previous chapter, these evidential considerations will be codified into a series of diagnostic questions. These questions will then be put to work testing the London brother's account of superconductivity to see if evidence for direct or indirect linguistic representational acts can be found. We shall find the evidence best supports construing the London brother's model as a linguistic representation used to directly represent superconductors. Thus the discursive evidence in this case better supports the linguistic approach over the semantic view that takes expressions to function systematically in an indirect representational capacity.

7.1 Explicit Acts of Direct and Indirect Linguistic Representation

When distinguishing between acts of direct and indirect linguistic representation the clearest evidence comes when the speaker makes their intentions clear by explicitly saying what the intended representational targets of their utterances are. They can do this by simply stating that some representational relation obtains between an expression and a system. This will often be done by using verbs like 'represents,' 'describes,' 'defines,' and verb phrases like 'is a description of,' 'is a hypothesis about,' 'is a prediction of,' 'makes an assertion about,' etc. To assert that "X describes Y" is to assert that X is a linguistic representation of Y whenever X refers to some expression (or set of expressions) and Y refers to the intended representational target of the expression. For example one might say "The passage at the top of page 54 of the book describes the mood of the delegation on the first day of the G8 Summit." This assertion makes explicit what the speaker takes to be the intended representational target of some set of expressions. In this case a passage of a book is asserted to represent something about the delegation, namely its mood. The mood of the delegation is claimed to be the intended target of the expressions which comprise the passage.

To make the direct representational function of an utterance explicit a speaker must assert that the representational target of an expression is the system under investigation within the context. In applied cases the system under investigation within the context of theorizing and modeling practices are real systems. So in applied cases to make the direct representational function of an utterance explicit it must be claimed that a real system is the representational target of an expression (or set of expressions). So for example by asserting "Equation 5 describes the population growth of flies in test group A" the speaker makes it explicit that the equation in question is intended to represent the population of some particular group of flies. The equation would in this case be functioning in a direct representational capacity and the utterance of the sentence above makes this explicit.

A speaker can also make the indirect representational function of expressions explicit asserting both that the direct target of an utterance is a mediating object and that this object in turn represents some particular physical system. I shall co-opt Giere's terminology (introduced in §4.1) and call any utterance that makes the indirect representational role of expressions explicit a *theoretical hypothesis*. When theoretical hypotheses are included among the utterances found in a modeling context, then in these cases the indirect

representational significance of one or more expressions is made explicit. To get clearer on what kinds of utterances can count as theoretical hypotheses let us first consider Giere's discussion of them.

Recall that on Giere's version of the semantic view (discussed in §4.1 and §4.2.1) a theory has two elements. The first element is a class of 'models,' where these 'models' are extra-linguistic abstract objects (often mathematical structures). This class of abstract objects associated with a theory are defined by a set of sentences or equations. So these sentences and equations are used to represent the mediating objects (Giere 1988, 79). But on Giere's view the expressions that define the mediating objects do not explicitly say anything about the relationship between the mediating objects and the system under investigation. This is where the second element of Giere's account comes into play. For Giere to fully and explicitly specify a theory not only must one define a collection of mediating objects, but one must also specify one or more theoretical hypotheses that makes explicit the relation between the mediating objects and the real systems they represent. And so a theoretical hypothesis is "a statement asserting some sort of relationship between a model [qua abstract object] and a designated real system (or class of real systems)" (1988, 80). On this picture there are two different kinds of expressions that serve different purposes in the specification of theories. There are the expressions that define the mediating objects and there are the theoretical hypotheses that assert some relationship between the mediating objects and real systems.

Now Giere believes similarity is the appropriate relationship with which to characterize the connection between mediating objects and systems. Accordingly he takes theoretical hypotheses to have the following general form: "Such-and-such identifiable real system is similar to a designated model in indicated respects and degrees" (1988, 81).[1] Giere offers the following example:

> 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, 81)

In this formulation we need to remember that 'model' on Giere's usage never refers to any language-like objects. Instead models are extra-linguistic abstract objects. So in this example (1) the mediating object is distinguished from the system which is under investigation; and (2) it is made explicit that the mediating object is taken to represent the system. I take any utterance that does both (1) and (2) to count as a theoretical hypotheses. So in the above statement "a two-particle Newtonian model with an inverse square

---

[1]Other proponents of the semantic view, including French and Da Costa (2003), Ladyman (1999) and van Fraassen (2008), prefer a structuralist explication of the representational significance of the mediating objects (structures). So rather than the theoretical hypothesis asserting that some similarity obtains between the mediating model and the target system, on their accounts it would assert that some structural relation (e.g., an isomorphism, partial isomorphism, embedding relation, etc) obtains. However, this story may be complicated by the fact that these authors suggest there is typically a whole hierarchy of structures that stand between a (highly abstract) theoretical model and some physical system. I say 'may' because it is a little unclear if the hierarchy is intended to be a representational one (in which case representation has many stages with many mediating layers of objects), or one concerned only with the evidential relations required to connect representations of phenomena to high level theory. Regardless, in principle a theoretical hypothesis could be constructed on their accounts, though they may be very complex and hard to formulate explicitly. If complex theoretical hypotheses are unlikely to be explicitly formulated, then the indirect representational function of expressions will typically be left implicit and, hence, these cases will be covered by my discussion of implicit indirect representation below.

central force" is distinguished from its representational target, "the earth-moon system". The above statement also makes clear how the mediating object is intended to represent the target. The positions and the velocities of the particles in the abstract model are taken to be "very close" to the positions and velocities of the earth and moon, and this is the basis for taking the abstract model as a representation of the earth and moon system. So if we were to discover the above theoretical hypothesis explicitly stated along with the other sentences and equations associated with some model, the presence of this theoretical hypothesis would indicate all those expressions in the discourse which characterize the "two-particle Newtonian model with an inverse square central force" function in an indirect representative capacity. These expressions define the abstract object which the author refers to as "a two-particle Newtonian model" and it is this object which represents the earth-moon system.

So the presence of theoretical hypotheses within the discursive aspect of theorizing and modeling practices is strong evidence that at least some of the other utterances comprising these discourses are functioning in an indirect representational capacity. This raises the question: do scientists systematically, or even regularly, include theoretical hypotheses as part of the discursive aspect of their theorizing and modeling practices? This is an empirical question about scientific practices that can be answered only by detailed observations of these practices. However, I suspect that theoretical hypotheses are rarely explicitly stated. Moreover, this seems to be something that Giere himself would likely admit. In Giere's (1988, 78–86) longest and most in-depth discussion of theoretical hypotheses the only example he offers is the above quoted example, but this is clearly just an illustrative example created by Giere. He does not attribute this or any other theoretical hypotheses to any particular scientists in any particular theorizing contexts. Moreover, immediately after offering the above example, Giere demurs and puts the following statement forward as a "less stilted formulation . . . closer to how scientists actually talk":

> The earth and moon form, to a high degree of approximation, a two-particle Newtonian gravitational system. (1988, 81)

This reformulation is one that Giere acknowledges "blur[s] the distinction between the theoretical model [vis mediating object] and the real system" (1988, 81).

Whether or not reformulated expressions like the above are commonly uttered by scientists is an open question. But even if such 'less stilted' expressions are common, these expressions blur the line between mediating objects and real systems to the point that they no longer count as a clear examples of theoretical hypotheses. The problematic word in this formulation is 'form.' If I were to say "the sub-atomic particles form a hydrogen atom" I am not clearly asserting a relationship between distinct objects (as per requirement (1) listed above as a distinguishing feature of theoretical hypotheses). Rather I am predicating something to the sentence's subject. I am predicating atom-hood to the sub-atomic particles, by claiming that there are sub-atomic particles arranged in such a way that they constitute a hydrogen atom. Or to put it another way, the expression is used to represent the sub-atomic particles *as* forming an atom. In the same way to assert "The earth and moon form a two-particle Newtonian gravitational system" is naturally read as representing the earth-moon system *as* a Newtonian gravitational system. Consequently,

statements like Giere's re-formulated theoretical hypothesis no longer count as clear evidence of indirect representation.

In the end I doubt Giere (and other proponents of the semantic view) intends to be committed to the empirical claim that theoretical hypotheses are systematically and explicitly presented by scientists as a part of their theorizing and modeling practices. Consequently, if indirect representation is the rule and not the exception, then an indirect representational role must be something that expressions implicitly play. So I think Giere and other proponents of the semantic view are most plausibly read as taking expressions as implicitly playing an indirect representational function in scientific theorizing and modeling practices.

7.2   Implicit Acts of Indirect Linguistic Representation

In the case of implicit indirect linguistic representation expressions tokened in discursive contexts play an indirect representational role even though no theoretical hypothesis is asserted. We are interested in trying to determine when expressions are and are not used in an indirect representational capacity. When the representational function of expressions are not made explicit the challenge is determining what might count as relevant discursive evidence to help us justify interpreting acts as directly or indirectly representing systems. In parallel to our discussion of the representational and non-representational uses of expressions in the previous chapter here again we can avail ourselves to any information drawn from the context of the utterance that can give us clues regarding the speaker's communicative intentions. Any information that indicates the speaker intends their utterance to directly represent some real system would be evidence that utterance is used by the speaker to perform an act of direct linguistic representation. Conversely any information drawn from the context that indicates the speaker intends to use an utterance to represent some mediating object would count as evidence that the utterance is used to perform an act of indirect linguistic representation. Here again it must be noted that almost any piece of information gathered from the context of utterance could potentially count as evidence in this respect. However, as I shall now explain, we can highlight some particularly salient features of the context by paying special attention to the different justificatory and inferential roles that direct and indirect representational acts play in discourse.

It is important to remember that all acts of direct and indirect linguistic representation are representative acts. One may either directly or indirectly represent by way of an assertion, directly or indirectly represent by way of a hypothesis, directly or indirectly represent by way of a conjecture, etc. So the differences between direct and indirect representative acts are not to be found at the level of general act types. To be able to distinguish between direct and indirect representative acts we need to identify salient differences between two acts of the same type. Luckily there are salient differences that arise because direct and indirect representative acts have distinct representational targets, and typically targets of different kinds. This is especially clear when we are dealing with applied cases where theories and models are used to explain or predict the behaviour of real systems. In these cases direct representations immediately target real systems and indirect representations immediately target mediating abstract objects (i.e., structures on

most versions of the semantic view). On the justificatory side of the ledger, this means that direct and indirect representations are responsible to, or beholden to, distinct kinds of objects. On the inferential side of the ledger, this means that representational acts licence inferences about different kinds of objects.

Consider assertions. When asserting a speaker takes on a commitment to defend the content of their utterance. One can always challenge an assertion on the grounds that things are not as they are asserted to be. If things are not as they are asserted to be, the assertion is deficient as a representation of its target. This connects with truth and falsity in an immediate way. Accurate linguistic representations are true; 'true' is the label we use to distinguish successful linguistic representations. Conversely, linguistic representations that are inaccurate are false. By asserting one takes on an obligation to defend their assertion as true and this amounts to an obligation to defend the assertion as an accurate representation of its target. In applied cases by performing acts of direct representation the speaker takes on the obligation to defend the linguistic representation as accurately representing some physical system. When dealing with indirect representation the obligation is to defend the linguistic representation as accurately representing some abstract object.

One place we might expect to notice the differences between direct and indirect representation, then, is in which challenges the speaker recognizes as legitimate or illegitimate. It is legitimate to challenge an assertion on the grounds that it poorly represents its target. It is illegitimate to challenge an assertion on the grounds that it poorly represents something other than its intended target. In the latter case, the challenge is one that the speaker is under no obligation to attempt to meet, though there may be conversational norms that require the speaker to respond in some other way. Suppose, for example, I assert "The Thames bisects the main University campus in London." The Thames in Southern Ontario does bisect the university campus in London Ontario (i.e., the campus of the University of Western Ontario), but the Thames in the United Kingdom does not bisect the main campus of the University of London. If the target of my assertion is the University of Western Ontario (and I make this clear in the context of my utterance), then to object that the Thames is nowhere near the main campus of the University of London is to mount an illegitimate challenge. The appropriate response to this challenge (if any is required at all) is to explain it away as a misunderstanding, e.g., "No you misunderstand, I am talking about the Thames in Southern Ontario."

To put the point in a more general way, only in the light of relevant information is the speaker obliged to defend, revise, or retract an assertion. Information about the Thames in Southern Ontario and the location of the campus of the University of Western Ontario is relevant to the evaluation of the acceptability of the above assertion. On the other hand, information about the University of London is irrelevant and, therefore, does not form the basis of a legitimate challenge. By identifying what information the speaker and their interlocutors treat as relevant to the acceptability of a representative act, we can determine what the intended target of a linguistic representation is. If a speaker intends by uttering some expression to have performed a representative act which targets an abstract object, then we can expect the speaker to treat only those challenges which they perceive as drawing upon relevant information as legitimate challenges to their utterance. Consequently, if a speaker's utterance is challenged on the grounds that it fails to accommodate some information about some physical system and the speaker in turn dismisses the challenge

as illegitimate, then we have good evidence that the speaker's utterance is intended as a representation of something other than the physical system in question. On the other hand, if a speaker treats challenges that draw upon information about some physical system as legitimate, then this is evidence that the utterance is intended as a representation of the physical system in question.

These differences between direct and indirect representative acts are brought even more sharply into relief when we consider the peculiarities associated with the representation of abstract objects. Recall that on Giere's account the mediating abstract objects are defined by a set of expressions. To challenge any of these expressions on the grounds that they fail to accurately represent the real system is, strictly speaking, to mount an illegitimate challenge because the equations are responsible to the mediating object—they are not responsible for any physical system directly. In fact, there is very little grounds one could possibly have for challenging the veracity of these expressions because their representational targets, abstract objects, are defined as those objects which satisfy the equations. So the expressions are empirically indefeasible: their truth is not contingent upon the empirically accessible world being any way in particular. The core insight here is nicely expressed by Einstein in his discussion of geometry: "as far as the propositions of mathematics refer to reality, they are not certain; and as far as they are certain, they do not refer to reality" (1954, 233). If the uttered equations are treated as though they are empirically indefeasible, then we have good reason to think that the equations are not being used to directly represent any physical target. Conversely, if the equations are treated as though their truth is contingent upon empirical evidence, then we have evidence that the equations are being used to represent some physical system directly.

Evidence that an utterance is treated as though it is empirically indefeasible can, again, be gathered from the speaker's responses to potential challenges. Since there are few reasons one could offer as grounds for rejecting an equation on Giere's account, we should expect most purported challenges to be treated as illegitimate. Insofar as the speaker has any conversational obligation to address an illegitimate challenge, then their only obligation is to explain away the challenge. On the other hand, if the speaker responds to empirical evidence as though they recognize this evidence as a basis for a legitimate criticism of some utterance, then we have evidence that the speaker is using their utterance as a direct representation. In this case, in attempting to meet their justificatory obligations by defending their utterance the speaker can be expected to respond to empirical evidence by attempting to show the evidence is consistent with the content of their utterance, or by countering and challenging the acceptability of the purported evidence. Failing to do either successfully, the speaker can be expected to revise their utterance so that it becomes consistent with the empirical evidence, or retract the utterance altogether by choosing to remove their endorsement of its content.

Mirroring these justificatory differences between direct and indirect representation, there are differences we can expect to see in inferential practices too. If an assertion is made about some abstract object, then this assertion cannot by itself warrant inferences about any physical systems. Other assertions, like theoretical hypotheses, are needed to make the warrant for such indirect inferences explicit. This reflects the fact that on the semantic view when theories are employed in reasoning about real systems this reasoning is always analogical. On the semantic view scientists use sentences and equations to directly represent mediating objects and these objects are in turn used to analogically reason about real systems.

By contrast the linguistic approach does not suppose scientists only use theories and models in analogical reasoning about real systems. Rather, by directly representing real systems, scientists can reason directly about these systems. When equations are used to infer things about real systems directly, these equations can be directly appealed to as warrant for these inferences about the real system. So assuming speakers tend to make the basis for their inferences explicit, when a speaker drawing upon some utterances makes an inference about some physical system without explicitly appealing to a theoretical hypothesis (or some similar statement concerning the relationship between an abstract object and the physical system), we have evidence that the utterance which formed the basis of the inference targets the real system directly.

Of course evidence drawn from the inferential practices displayed in discourse is defeasible. Indirect representation can play a role in a kind of analogical reasoning, and the basis for analogical reasoning is not always made explicit in discourse. Sometimes it may be best to interpret some speaker as implicitly appealing to some theoretical hypothesis as the basis for an indirect inference about some physical system. However, one should only interpret inferential practices in this way if they have other evidence of indirect representation. This, I take it, is just a version or extension of the *principle of charity*. All else being equal, one should interpret speakers as being explicit when drawing inferences. If, for example, the speaker responds to challenges in a way indicative of indirect representation, then this would be evidence that could warrant interpreting inferential practices within the discourse as implicitly indirect too. In lieu of such evidence, we are better off explaining the inferential behaviour of speakers in the simplest way by taking the speaker to be directly representing some physical system by their utterance.

Note that proponents of the semantic view posit an indirect representational role to most, if not all, equations uttered in the discursive aspect of theorizing and modeling practices. If, as Giere himself seems to recognize, theoretical hypotheses are rarely explicitly presented in these contexts, this means proponents of the semantic view systematically interpret scientific discursive representative practices as implicitly indirect. This implies that scientists systematically fail to make explicit the basis for their inferences whenever predicting or explaining the behaviour of real systems. To interpret representative practices in this way clearly runs afoul the principle of charity. Moreover, anyone who is inclined to think scientific reasoning tends to be especially careful, explicit, and clear will find this conclusion unpalatable.

Drawing together the above considerations, the following are a series of diagnostic questions which can be used to help determine whether an utterance directly or indirectly represents some real system.

(I) Does the speaker make the direct representational role of expressions explicit by saying that some expression, or set of expressions, is a representation of some real system or class of systems?

(II) Is there information available in the context that indicates the speaker likely intends an utterance, or set of utterances, to represent some real system or class of real systems?

(III) Does the speaker treat information about a particular real system as directly relevant to the acceptability of their utterance?

(IV) Does the speaker draw inferences about some real system from the utterance in question without also appealing to some theoretical hypothesis to warrant this inference?

If the answer is affirmative to any of the above questions, then we have evidence that the utterance in question directly represents a real system. On the other hand, affirmative answers to the following questions would mean there is evidence of indirect representation.

(V) Does the speaker make the indirect representational role of other expressions explicit by presenting a theoretical hypothesis?

(VI) Is there information available in the context of the utterance that indicates the speaker likely intends an utterance, or set of utterances, to represent some mediating object which in turn is intended to represent some real system?

(VII) Does the speaker treat information about a particular real system (or class of systems) as irrelevant to the acceptability of their utterance?

(VIII) Does the speaker treat their utterance as empirically indefeasible?

Moreover, if we accept something akin to the principle of charity, then a negative answer to questions (V)–(VIII) will also count as evidence of direct representation.

7.3   The London Account Revisited

Let us now put the above diagnostic questions to work on the London brothers' account of superconductivity. As mentioned earlier this is an example that French and Ladyman (1997) claim is well captured by the semantic view. When we first considered the London account in the previous chapter we sought to determine whether there is evidence of expressions being used in representational or non-representational ways. It was determined that the evidence clearly indicates the London brothers' utterances are used to perform representative acts. Now we must determine whether these representative acts are used to directly or indirectly represent superconductors. If this example of modeling is best captured by the semantic view, then we should expect to see evidence of expressions functioning as indirect representations. However I shall argue that the evidence favours construing their uttered equations as direct representations that describe superconductors.

The first piece of evidence we should observe is that the London brothers do make a number of claims that appear to make the direct representational function of some equations explicit. Consider the second sentence of their paper:

> Thus the relations between the field strength E and the current density J in a superconductor has been described by means of an "acceleration equation," of the form $\Lambda \dot{\mathbf{J}} = \mathbf{E}; \Lambda = m/ne^2$. (1935, 71)

By this sentence it appears they claim that the acceleration equation has in the past been used to directly represent a feature of superconductors, namely the relationship between the electric field strength and the

current density. Moreover, it is in this same representational capacity that they evaluate the acceleration equation in their paper and reject it on the grounds that it fails to explain Meissner's Effect. To address the shortcomings of the acceleration equation they offer their own description. As they put it,

> Apparently a model was wanted which would explain that in its most stable state the supra-conductor has always a persistent current. We shall give a *formulation* that is more restricted in this respect. On the other hand it includes one more important fact, namely, the experiment of Meissner and Ochsenfeld. In this way we get *a new description of the electromagnetic field in a supraconductor*, which is consistent and, as it eliminates unnecessary statements, is in closer contact with experiment. (1935, 71, my emphasis)

In this passage they claim to provide a formulation which results in a new description of the electromagnetic field in supraconductors. Which "formulation" are they referring to here? It is clear from the context that the formulation they provide is the London equation, $curl\Lambda\mathbf{J} = -\frac{1}{c}\mathbf{H}$ (eqn. 6.2 above). In parallel to noting the acceleration equation (which they say describes superconductors) "might replace Ohm's law for supraconductors" (1935, 71), they forward the London equation "as the fundamental equation which replaces Ohm's law in supraconductors" (1935, 73). Moreover they say that it is the London equation that "includes Meissner's effect" (1935, 73). So the London equation is taken by the London brothers as a formulation to replace the acceleration equation, and they explicitly claim this new formulation provides a new description of superconductors. This is evidence of direct representation because "a formulation" in this case refers to an equation, superconductors are real objects, and the phrase "description of" indicates a representational relationship is intended between the equation and these real systems.

If the acceleration and London equations were explicitly used to perform indirect representational acts we would expect the London brothers to talk about these equations in different ways. They would not claim to provide "a new description of the electromagnetic field." Rather they would say something like "we define a new model that is similar (in such-and-such respects) to the electromagnetic field in superconductors." Nor would they say "the relation between the field $\mathbf{E}$ and the current density $\mathbf{J}$ in a superconductor has been described by [the acceleration equation]." Instead they would say something like "the relations $\mathbf{E}$ and the current density $\mathbf{J}$ in a superconductor have been represented by the mathematical models satisfying the acceleration equation, where the models and superconductors are similar in such-and-such respects." But we just don't find the London brothers saying things like this. In general we observe the conspicuous absence of any utterances in the London brothers' 1935 presentation that can plausibly be understood as theoretical hypotheses. There is simply no utterances in the London brothers' paper that distinguish mediating objects from the system which is under investigation (superconductors, in this case), nor are there any expressions that make explicit that particular utterances in the context are intended to represent something other than superconductors.

Connecting these observations to the diagnostic questions outlined above, we see the answer to diagnostic question (V) is negative: the speakers do not make the indirect representational role of other expressions explicit by presenting a theoretical hypothesis. In the absence of any theoretical hypotheses

it cannot be claimed that any of the expressions are explicitly acts of indirect representation. On the other hand the answer to question (I) is affirmative: the speakers do make the direct representational role of expressions explicit by saying that some expression, or set of expressions, is a representation of some real system or class of systems. This is clear evidence that the central equations (the acceleration and London equations) are used by the London brothers to perform acts of direct representation.

Of course at this point the defendant of the semantic view may try to argue that the evidence of direct representation is not decisive. Perhaps the London brothers were not as careful or perspicuous as they could have been when explaining the significance of the acceleration and London equations. The defendant of the semantic view would be in a much stronger position to make a move like this if they could bring their own discursive evidence to bear. So, let us then consider the possibility that the equations employed by the London brothers are implicitly functioning as indirect representations. For this we need to consider the inferential and justificatory practices observed by the London brothers.

Question (III) asks whether the speaker treats information about a particular real system as directly relevant to the acceptability of their utterance. The answer here is affirmative. Consider the acceleration and London equations again. The London brothers do explicitly consider and reject the acceleration equations on empirical grounds (London and London 1935, 72–73). The acceleration equation, they argue, cannot account for the Meissner effect. By presenting and rejecting the acceleration equation the London brothers are presenting and challenging the acceleration equation based on information about the observed behaviour of superconductors. They clearly take this information to be relevant and, in the end, find it to be sufficient grounds for rejecting the acceleration equation. When they introduce their own description, via the London equation, they are careful to show that it does not fall to the same challenge. The result is a description of superconductivity that is "in closer contact with experiment" (1935, 71). So we have a positive answer to question (III) and a negative answers to questions (VII) and (VIII). All of this is evidence of direct representation and not indirect representation.

To clarify, what is important here is not just that the London brothers take experimental results about real superconductors to be important in some general way when assessing their model. Even on the indirect representational picture offered by the semantic view experimental results can be important to model and theory evaluation. What is important is that experimental evidence is taken by the London brothers to be directly relevant to the acceptability of specific equations. The experimental evidence is offered immediately, without further explanation or qualification, as a reason in and of itself for rejecting the acceleration equation. On an indirect representational account this should not be the case. Experimental evidence could be reason for thinking that one or more structures associated with the model is dissimilar to its target in certain respects. If the structures were intended to be similar in this respect, then the uncovered dissimilarity could be reason for deeming the model unacceptable or problematic. This might in turn be traced back to a particular equation. If the problematically dissimilar features of the structures are described/defined by a particular equation, then this equation could be identified as problematic given the scientist's representational aims. This, however, is a more circuitous, or indirect, route by which experimental evidence is taken to be relevant to the acceptability of specific equations. Since there is nothing in the London brother's example that indicates this kind of indirect relevance relation, there is no positive

evidence in favour of the semantic view. On the other hand, there is discursive data that indicates the London brothers do take information about superconductors to be directly relevant to the acceptability of the acceleration equation, and this is positive evidence of direct linguistic representation.

We also see that their inferential practices show evidence of direct representation. Nothing like a theoretical hypothesis is presented. Despite this the London brothers clearly draw inferences from both the London equation and the acceleration equation that are treated as though they have empirical import. As outlined earlier, from the acceleration equation they derive the homogenous equation (eqn. 6.7) whose general solution "means . . . that the original field persists forever in the supraconductor" (1935, 72). The London brothers draw conclusions about superconductors from the acceleration equation without drawing on any theoretical hypotheses. This constitutes a positive answer to question (IV). Of course this inference drawn from the acceleration equation is one that is in conflict with experimental evidence. As they make clear, this is a problem not faced by the London equation. In fact it is chosen precisely because such a conflict cannot be inferred from it.

Beyond the inferential and justificatory practices, I don't think there is anything about the discursive context (no diagrams or figures, odd grammar, etc.) that someone trying to attribute a mediated account of representation could seize upon to argue that the London brothers intended any of their uttered sentences to perform acts of indirect linguistic representation. Overall there is just no discursive evidence which could be brought forward to undermine what appears to be the explicit intention of the London brothers to use the equations to describe features of superconductors. The discursive evidence clearly favours the linguistic approach. The sentences and equations are used by the London brothers to represent supercon-ductors. As they say themselves, their model describes superconductors and for this reason it is a linguistic representation which targets real superconductors.

Chapter 8

Representing Scientific Representation: Similarity and Structure

French has collaborated with Bueno, da Costa and Ladyman (henceforth French *et al.* will refer to these authors) to offer a unique reinterpretation of the semantic view which strips it of its ontological commitments (see French and Ladyman (1999), Bueno *et al.* (2002), da Costa and French (2003), French (2010), and Bueno and French (2011)). Instead of being construed as a view about what scientific theories and models are, it is construed as a means of representing theories and models for philosophical purposes. In this chapter I consider this proposal as a way of potentially sidestepping, or deflating, debates over what scientific theories and models are. In particular, I consider whether this proposal might undermine the motivation for seeking descriptive accounts of scientific theories and models.

As we shall see, at the centre of French *et al.*'s proposal is an interesting, but contentious, view on scientific representation. Their confidence in the adequacy of the semantic view as a means of representing theories and models largely derives from their confidence that all genuine scientific theories and models represent their targets in virtue of (structural) similarities that obtain between them. It is this supposed connection between scientific representation and structural similarities that shall be accessed. I shall argue that a compelling case for this view on scientific representation has not been made and consequently it remains too implausible to justify abandoning the search for a descriptive account of scientific theories and models. The sensible way forward, I conclude, is to determine what kinds of things theories and models are, and then see which accounts of representation prove to be the best. Thus I advocate an approach to the study of scientific representation that is driven by a descriptive account of scientific practice.

8.1 Reinterpreting The Semantic View

Insofar as the semantic view identifies theories with collections of structures, or identifies theories with an amalgam of structures and theoretical hypotheses (*à la* Giere), then the semantic view makes commitments regarding what kinds of things theories and models are. French and his collaborators argue that the semantic view needn't make such commitments. According to French and Ladyman,

> . . . the issue is whether a set-theoretical description can capture the kinds of models used in scientific practices. This practice presents us—philosophers of science—with an array of "theories" and "models", some of which are expressed mathematically, some materially, and so on.. . . Thus, whether or not the models *are* first order or set-theoretical at the level of practice, the issue is how they should be *represented* so as to best capture relevant aspects of this practice. (1999, 107)

If the philosophical study of scientific theories and models is framed in this way, then

> . . . advocates of the semantic view need not be committed to the ontological claim that models *are* structures . . . Set-theoretic structures provide a useful representational (or better, perhaps, descriptive) device at the meta-level of the philosophy of science. What theories and models are, *qua* objects, is then a further matter. (Bueno and French 2011, 890)

By 'ontological claim' I take Bueno and French to mean simply a claim about what kind or class an object falls under. In avoiding ontological claims the reinterpreted semantic view is avoiding any claims about what kind or class of objects theories and models fall under. Nevertheless French *et al.* hold that the semantic view can be used to adequately represent scientific theorizing and modeling practices for philosophical purposes. This is a provocative proposal. The semantic view is taken to be an adequate means of representing scientific practice regardless of what theories and models are. So it is claimed that any diversity at the ontological level will have no impact at the meta-level where (supposedly) philosophers of science are concerned only with the best way to represent salient features of scientific practice.

Now regarding the "further matter," i.e., the question of what theories and models are, two different proposals are evident in the work of French *et al.*. The first and weaker position is one of agnosticism. On this proposal the semantic view remains completely agnostic about what theories and models are. A second more radical proposal is offered by French (2010) alone. He councils us to adopt a *quietist view*. He claims that the question of what theories and models are is "an inappropriate question for the philosopher of science to ask" (2010, 238). He suggests not only that philosophers of science can make do without an account of what scientific theories and models are, but that they should not pine after or seek to develop one. Answers to ontological questions concerning theories and models should not be pursued because they offer "nothing to explain the features of scientific practice" (2010, 245). Exactly why he thinks answering certain ontological questions cannot help us explain features of scientific practice he

does not say. Presumably he thinks this in part because he believes the semantic view has all the resources to explain the features of scientific practice of interest to philosophers of science. As I critically evaluate French *et al.*'s proposal below I will, for simplicity, focus on the weaker agnostic view regarding the ontology of theories and model.

8.2   A Threat to the Linguistic Approach

The advocate of the linguistic approach is wise to adopt a somewhat nuanced attitude towards French *et al.*'s proposal. Insofar as French *et al.* remain agnostic about what kinds of things theories and models are, then strictly speaking the reinterpreted version of the semantic view and the linguistic approach are immediately concerned with different questions. The linguistic approach attempts to determine what scientific theories and models are. This reinterpreted version of the semantic view is concerned with how theories and models can be adequately represented. So long as models and theories can be identified as linguistic representations and still be adequately represented set-theoretically these views are consistent. Moreover the advocate of the linguistic approach has no reason in general to deny that set-theoretic representations may be useful. As I highlighted in section §4.4, standard mathematical model theory provides one framework, among others, within which a formal semantics for a language can be developed. Formal semantics provide a way of representing certain semantic features of a language and the proponent of the linguistic approach can happily accept that such representations maybe useful for many purposes.

The friction between French *et al.*'s proposal and the linguistic approach, however, becomes more acute when the semantic view is assumed to be useful for more than select purposes. It threatens to erode the linguistic approach's motivations for seeking a descriptive account of scientific theories and models in the first place. If, as French (2010) suggests, the semantic view's model-theoretic machinery could be used to provide us with a complete understanding of all of the philosophically relevant aspects of scientific practice irrespective of what theories and models are, then there would be little point in pursuing a descriptive account like the linguistic approach. In this case the semantic view could effectively be used to sidestep the descriptive considerations that drive the linguistic approach altogether. Philosophers of science need not worry if theories and models are linguistic representations if determining this would have no impact on our philosophical understanding of them. The semantic view would offer an adequate representation of theories and models regardless of what they are (language-like, set-theoretic, etc.).[1]

So the disagreement between French *et al.*'s proposal and the linguistic approach is not a disagreement concerning any specific ontological claims (in the sense of 'ontological claim' specified above). The linguistic approach does claim that theories and models are often linguistic representations. However, French *et al.* neither deny or affirm this to be true. Instead the disagreement concerns the relevance of

---

[1]This conflict with the linguistic approach is even starker in the case of French's stronger quietist position. If we were convinced that a descriptive account, like the linguistic approach, concerned itself only with questions that are "inappropriate" given the aims of the philosophy of science, or if we were convinced that descriptive accounts could never explain anything about scientific practice, then the search for such a descriptive account would at best be an unhelpful distraction.

ontological claims to philosophical investigations. French *et al.* deny that philosophers need to know anything about what theories and models are in order to understand how they are used by scientists to represent. Consequently, they argue that developing a descriptive account is not a worthwhile philosophical pursuit. By contrast the linguistic approach takes the development of a descriptive account to be part of a methodologically sound approach to the study of theorizing and modeling. It does not presume that ontological claims are irrelevant to philosophical investigations of scientific practices. It recognizes that knowledge concerning what theories and models are may be (and likely is) important to understanding how theories and models are used by scientists.

What I want to examine in the remainder of the chapter, then, is whether French *et al.*'s proposal can actually succeed at obviating a descriptive account of theories and models, and thereby undercut the motivation for developing one. I think it does not, and I shall offer a burden-of-proof argument in support of this conclusion. Roughly, the burden is on French *et al.* to convince us that their account can indeed capture all of the philosophically relevant aspects of scientific theorizing and modeling practices while remaining completely agnostic regarding the ontology of the theories and model. If this burden is not met, then we should not abandon the search for a descriptive account of scientific theorizing and modeling. We should be dissuaded from developing a descriptive account only in the light of good reasons to expect that a descriptive account either cannot be found, or that such an account will be of little value. French and his collaborators suggest the latter. They suggest a descriptive account would be of little value because (their own version of) the semantic view can be used to adequately capture all of the aspects of theorizing and modeling salient to philosophical inquiry, thereby leaving nothing of philosophical interest left for a descriptive account to capture or explain. This line of reasoning should succeed at discouraging the development of a descriptive account only if we are highly confident that French *et al.*'s version of the semantic view is up to the task. This places the burden of proof on French *et al.*'s shoulders, but I don't think the arguments and considerations they offer in favour of their account can bear this burden.

Should doubts linger about the representational adequacy of the semantic view, then the development of a descriptive account remains a laudable aim. Unlike French *et al.*, in seeking to develop a descriptive account I do not assume that our best accounts of scientific theorizing and modeling practices can remain silent regarding the ontology of theories and models. I leave open the possibility that the ontology of theories and models might impact how these representations function in scientific practices. Should the ontology of theories and models prove to be salient to our philosophical understanding of theories and models, then good philosophical accounts of scientific practice must be developed in conjunction with, and perhaps even driven by, descriptive accounts which provide us with an understanding of what sorts of diverse things theories and model are.

Again French *et al.* claim that the semantic view can be used to capture all the philosophically salient features of scientific theorizing and modeling practices. Presumably there are many different aspects of these practices that could be of philosophical interest. Rather than trying to systematically identify and elevate the adequacy of the semantic view with respect to them all, I shall focus on the representational aspect of them. As we shall see capturing representation is at the very heart of French *et al.*'s project. They take the central role of theories and models in scientific practice to be representational and their overall

confidence in the adequacy of the semantic view largely stems from their confidence in its ability to capture the central representational function that theories and models play. So I will grant, at least for the sake of argument, that if the semantic view can be used to represent how theories and models are used by scientists to represent their targets, then French *et al.*'s proposal does threaten to obviate descriptive accounts of theories and models (and thereby threaten descriptive accounts like the linguistic approach). However, if it remains reasonable to suspect that ontological questions (regarding what theories and models are) are relevant to our philosophical understanding of scientific representation, then the search for a descriptive account of theories and models remains well motivated.

I shall start my investigation of French *et al.*'s proposal by first examining how their version of the semantic view is purported to capture the representational function of theories and models in scientific practice. I shall then consider why they think their model-theoretic representations are adequate. We shall find that a contentious view about the nature of scientific representation is appealed to by French and his collaborators. They suppose, roughly, that all scientific representations represent their targets in virtue of similarities. The evaluation of this contentious view will occupy the last portion of the chapter. I shall argue there is good reason to doubt this contentious view and for this reason French *et al.*'s confidence in the adequacy of their account is misplaced.

8.3 Representing Scientific Representative Practices

French *et al.* claim that the semantic view can be used to capture the representative function of theories and models. The version of the semantic view they advocate, however, has a few distinctive features that make it a little different than the versions of the semantic view we considered in chapter 4. Let me first outline their unique formulation of the semantic view and then explain how this formulation is supposed to capture scientific representation.

8.3.1 A 'Partial' Twist on the Semantic View

One distinctive feature of French *et al.*'s version of the semantic view is its special emphasis on the intrinsic-extrinsic distinction. You will recall from §4.1 this distinction was first drawn by Suppes (1967). According to Suppes the paradigmatic intrinsic characterization of a theory is its axiomatization in some formal logical language. He claims a theory can also be intrinsically characterized using set-theoretic concepts by defining a set-theoretic predicate. All that is required is that some class of structures is delineated by identifying their common intrinsic properties. According to Suppes theories can also be extrinsically characterized by specifying a theory's structures directly. One way to do this is by directly defining each of the theory's structures. In set-theoretic terms, this means specifying a set of ordered tuples consisting of a set of objects, relations and operations on the objects in the domain (Suppes 1969a, 13). Now Suppes

is a little unclear on this point, but I have argued he is best interpreted as identifying theories with collections of structures. On this interpretation Suppes holds that intrinsic and extrinsic characterizations are complementary ways of characterizing the structures that a theory is to be identified with.

Of course French *et al.* do not choose to identify theories with structures, but nevertheless they make use of both intrinsic and extrinsic characterizations of theories. According to da Costa and French (2003, 33-36) both characterizations are understood as distinct perspectives from which to view theories. They suggest both perspectives are required in order to capture different aspects of scientific practice.

The extrinsic perspective is important for the semantic view to avoid many of the problems associated with the syntactic view:

> . . . our primary concern is with the structure of the theory and with the relationships between theories themselves and between theories and "the world," understood in terms of that structure. Characterizing that structure and these relationships in purely logico-linguistic terms generates a disparity with scientific practice that verges on absurdity: Different formulations come to be regarded as different theories[2] and, with the relationship between theories and the world characterized logico-linguistically in terms of correspondence rules, a change in theory follows on a change in experimental technique.[3] What is required here is an "extrinsic" characterization in terms of which we can regard theories from "outside" a particular logico-linguistic formulation. (2003, 33)

However, da Costa and French recognize that extrinsic characterizations have their limitations too: "Set-theoretic structures cannot be truth-bearers" and so if we were simply to identify a theory with structures, then "we could no longer claim that a theory is true or false" (2003, 34). So when there is a need to consider one's epistemic attitudes towards theories, they must be viewed from the intrinsic perspective where they are truth-apt and the objects of epistemic attitudes (2003, 33). So each perspective can be appealed to as needed to understand different aspects of theories and our relations to them.

Another distinctive aspect of French *et al.*'s version of the semantic view is its use of *partial structures* and *quasi-truth*. This is a twist on standard model theory which normally considers complete structures. Tarskian notions of truth relativize the truth of sentences to an interpretation. A sentence is interpreted as true in a structure. A sentence, or set of sentences, is interpreted in a structure by mapping the non-logical elements of the language onto elements of the structure's domain. The structure's domain forms the extensions of the elements of the language. Unary relations (i.e., predicates) are assigned a set of elements in the domain, binary relations are assigned a set of ordered pairs of elements in the domain, three-place relations are assigned a set of order triples of elements in the domain, and so on. Complete,

---

[2]In §3.3.2 I argue that it is not absurd to suppose that different linguistic formulations can be regarded as non-identical theories. Any apparent absurdity is removed once we recognize that non-identical theories can nevertheless be equivalent in many important respects.

[3]As discussed in §3.3.1, this criticism has no force against the linguistic approach. The linguistic approach does not appeal to correspondence rules to capture theory-world relations.

or normal, structures are formed when the complete extension for each relation is defined over the entire domain of the structure.

By contrast partial structures specify only partial extensions for relations. Thus they leave unspecified whether certain n-tuples are or are not within the extension of an n-ary relation. Da Costa and French offer the following example:

> Consider a binary relation R, which can be introduced as follows: R is an ordered triple $\langle R_1, R_2, R_3 \rangle$, where R_1, R_2 and R_3 are mutually disjoint sets such that $R_1 \cup R_2 \cup R_3 = A^2$. In model-theoretic terms, R_1 is the set of ordered pairs which are satisfied by those sentences expressing the relationships between the entities concerned, R_2 is the set of ordered pairs not satisfied by these sentences, and R_3 is the set of ordered pairs for which it is left open whether they are satisfied. When R_3 is empty, R constitutes a normal binary relation and can be identified with R_1. (2003, 19)

When R_3 is not empty, however, R is only a partial relation. Partial structures consist of one or more partial relations.

Just as sentences are interpreted as true in a (complete) structure, on the partial structures approach sentences are quasi-true in partial structures. Roughly, a sentence is quasi-true in a structure if the structure could be completed in such a way as to make the sentence true in the completed structure (for details see da Costa and French (2003, 18–20)). This makes quasi-truth strictly weaker than truth. If a sentence is true in a structure then it is quasi-true, but the converse does not hold in general (Bueno and French 2011, 860). As we shall see immediately below, by appealing to partial structures and quasi-truth French *et al.* hope to better capture the "imperfect" agreement between theories and reality and key aspects of a pragmatic theory of truth (da Costa and French 2003, 11–16).

8.3.2   A Partial Structures Representation of Scientific Representation

French *et al.* use their unique version of the semantic view to formulate a view of scientific representation, which French (2003) calls a *model-theoretic account of representation*.[4] Let us see how this account is meant to capture the representational function of theories and models in scientific practices.

French and Ladyman's (1999) discussion of the billiard ball model of a gas and Watson and Crick's wire model of DNA illustrates the basic idea behind their account of representation. They claim that the billard ball model of a gas represents gas atoms, and the "efficacy of the representation obviously hangs on the degrees to which and respects in which the systems [i.e., the model and the target] can be said to be similar" (1999, 107). Likewise, they claim the function of Watson and Crick's wire model is to represent the structure of the DNA molecule (1999, 109). So even though the first model is usually presented

---

[4]Here "model-theoretic account" is taken by French to be synonymous with the "semantic view." So his view could just as accurately be called the semantic view of representation.

via a set of equations and the second is presented materially, they claim each has the same underlying representational mechanisms. Both represent their targets in virtue of structural similarities. Moreover, they argue these structural similarities can be formally captured and analysed using the model-theoretic machinery of the semantic view. Roughly, both the structure of the representation and the structure of the target are represented set-theoretically and the relevant similarity between the two is captured by the appropriate mapping relations that obtain between them, e.g., isomorphisms, homomorphisms, embedding, and partial isomorphisms.

Filling in the details of the model-theoretic account of representation requires attending to some of the features that make French *et al.*'s version of the semantic view unique. In light of French *et al.*'s dual intrinsic and extrinsic perspectives on theories a question immediately arises: from which perspective should the representational function of theories to be captured? French *et al.* favour the extrinsic perspective. Part of the reason for this choice seems to be that the intrinsic perspective provides a clear way to capture the "similar to" relation in terms of relations between set-theoretic structures. Two structures are similar insofar as they have a shared structure and the notion of shared structure can be formally captured by the appropriate mapping relations (isomorphisms, homomorphisms, etc.) that obtain between them (1999, 110–112). French *et al.* also argue the model-theoretic framework provides a way of understanding representation from the intrinsic perspective in terms of representation viewed first from the extrinsic perspective. That is, we can capture some sense in which expressions are true or false in terms of structural similarities. In the normal Tarskian way, the expressions that are used to intrinsically characterize a theory are interpreted as true in a class of structures. A sentence represents some target insofar as it is interpreted as true in a structure and that structure represents the system. On this analysis a sentence "can be said to 'point' to the world by means of a [structure]" (2003, 17). In the same way an expression can be said to be 'true' if one or more of its interpretations is sufficiently structurally similar to the structure exemplified by the system in question. Now it is a further matter whether a theory's linguistic formulation can be evaluated as true in a non-relative way. Since French *et al.* do not identify a theory with its intrinsic characterization they believe the connection between the intrinsic formulation and the target system need not be confronted so long as the representational function of theories are adequately captured extrinsically.

According to French *et al.* representing scientific representation from within the extrinsic perspective involves capturing the "similar to" relation in set-theoretic terms (1999, 110–112). Specifying a structure (partial or complete) requires defining a family of relations (partial or complete) on a set of objects which constitute the structure's domain. This family of relations is said to structure the "bare" objects of the domain. Two structures have similar (or shared) structure when, roughly, there is some correspondence between the families of relations of each structure. Applying this account of structural similarity to the relation between scientific theories and their targets French and Ladyman arrive at the following:

> . . . to say that the model [*qua* structure] is similar to the system is to say, at least in part, that the family of relations [which structure the domain] is similar to the relevant family of the latter. To say this, is to say, in turn, that certain [relations in the structure's family of relations]— some subfamily—stand in a one-to-one correspondence to certain of the relevant family of

> relations in the model [*qua* structure] which completely represents the system. (1999, 111)

When all of the relations structuring the domains of each structure (one representing the theory and the other representing the target) can be mapped in a one-to-one, then the structures are isomorphic. But since the representation and the system are usually not completely similar, some weaker type of correspondence is often required to capture their similarity. French and Ladyman (1999) suggest that the relevant weaker type of correspondence is a *partial isomorphism*, in which only subfamilies of partial relations of each structure are taken to stand in a one-to-one correspondence. More recently, Bueno *et al.* (2002) (see also Bueno and French (2011)) argue that the relevant type of correspondence is often a *partial homomor-phism*. Either way, the underlying idea is the same: the similarities between a representation and target are represented set-theoretically by some kind of structure-preserving mapping between partial structures, i.e., some kind of *partial morphisms* (Bueno and French 2011, 863).

Now French *et al.* admit that partial isomorphisms, or other mapping relations, cannot be taken to hold between structures and the real systems targeted by theories and models. To do so would be to make "the grossest of category mistakes" (2003, 111), since these relations are only well defined as holding between mathematical structures. The model-theoretic machinery can be used to capture similarities between two objects only after each object is extrinsically represented by set-theoretic structures. So our ability to adequately represent the similarities that French *et al.* take to underlie representation is contingent upon our ability to adequately represent both representation and target by set-theoretic structures.

But when can we be confident that both theories and real systems can be represented set-theoretically? Bueno and French claim that for a system to be represented by a structure the system must "be described in such a way that one can take the relevant structure to apply" and this requires that the system be describ-able using "at the very least some minimal mathematics and certain physical assumptions" (2011, 887). I interpret these comments as placing the following condition on the applicability of their partial structures approach: a system can be represented by a set-theoretic structure so long as one can truly describe (i.e., intrinsically characterize) the system using a language which includes some minimal mathematics and, perhaps, some additional physical (non-mathematical) concepts. This reading is in line with a similar discussion offered by da Costa and French concerning how partial structures are related to a *domain of knowledge*. They suggest that a structure $\mathcal{A}$ is "suitable or appropriate" with respect to the domain of knowledge Δ if the proposition "$\mathcal{A}$ 'captures' the relevant aspects of Δ" is true (2003, 35). So, French *et al.* believe that the model-theoretic machinery can be used to capture the representational relation-ship between scientific representations and their targets when both the representation and its target can be described using a language which contains "a goodly portion of extant set theory" (da Costa and French 2003, 34). Bueno and French claim (without explanation) that if this condition were not met for a representation's target, then it "could not even be said to be a candidate for scientific representation in the first place"(2011, 887).

Pulling all the pieces of French *et al.*'s account together, the following conditions must be met for the model-theoretic account of representation to capture how a scientific theory or model R represents some target T.

1. R and T are similar (in certain respects and degrees) and it is in virtue of these similarities that R represents T.

2. R and T can each be truly described using a language with a 'goodly portion' of set theory.

3. The descriptions of R and T are quasi-true in the partial structures m_R and m_T respectively.

4. Structures m_R and m_T are partially isomorphic, partially homomorphic, or structurally similar in some other way.

5. The structural similarities between m_R and m_T capture, or represent, the similarities between R and T that make R a representation of T.

Whenever these conditions are met, it is plausible that the model-theoretic account of representation can provide an adequate account. However when one or more of these conditions are not met the adequacy of this account becomes doubtful.

French *et al.* claim their model-theoretic account can be used to capture the philosophically salient features of scientific theories and models of various kinds (see, for example, Bueno and French (2011, 890–891)). Included in the intended domain of their account are the same theorizing and modeling practices that the linguistic approach attempts to capture (i.e., those with an essential discursive aspect). For example, as mentioned in the last chapter, French and Ladyman (1997) offer their partial-structures version of the semantic view as a means of capturing the London model of superconductivity. On their treatment the London model and the background theory (associated with Maxwell's equations) are both represented by partial structures of the form $\langle A, R_i \rangle_{i \in I}$, where A is the structure's domain and R_i is a family of partial relations defined on A. French and Ladyman (1997, 381–384) argue that the London model retains some of the structure of the background theory, this structure retention can be captured by partial isomorphisms that obtain between the partial structures that represent each, and in virtue of these partial isomorphisms the London model and the background theory are not completely independent of one another. They also make the general claim that their partial structures version of the semantic view offers "a better picture of how theories represent the world" (1997, 369). This representational function of theories (and models[5]) is, again, captured in terms of partial isomorphisms between partial structures.

Of course this analysis of the London brothers' work on superconductors is plausible only if, as they claim, the London model and background theory can be adequately captured for philosophical purposes by partial structures. Thus French and Ladyman are assuming that in this case, as in all cases of scientific theorizing and modeling, the above conditions (1-5) are met. Remember that French *et al.* remain agnostic about the ontology of theories and models. So they do not explicitly deny (or affirm) that the London model is a linguistic representation. Instead they hold that their version of the semantic view applies regardless of whether the London model is or is not a linguistic representation. What we must now

---

[5]French and Ladyman (1997, 371–372) deny that there is sharp distinction to be drawn between high-level theory and low level 'phenomenological' models, and propose to represent them all by partial structures.

evaluate, then, is how plausible it is to assume that all of the above conditions (1-5) will be met when it comes to representing different types of theorizing and modeling practices.

8.4   Challenging Representation as Structural Similarity

Arguably the most contentious of the above conditions are 1 and 5. Their plausibility relies on a contentious view about representation that has been directly challenged by critics. Conditions 1 and 5 are only likely to be accepted in cases where both some similarity relation is taken to be constitutive of representation, and the similarities can be captured in terms of mapping relations between structures.[6] So long as structural similarities are essential to scientific representation, then the "underlying representational device" (Bueno and French 2011, 891) of scientific theories and models—whatever they are ontologically—can be captured by the relevant structures and their structural relations. Should there be cases where representation-target similarities fail to be constitutive of the representational relation or should structural similarities fail to capture the relevant similarities, then there would be little reason for thinking the model-theoretic account of representation can adequately capture these cases.

Downes (2009), Frigg (2006), Hughes (1997), and Suárez (1999, and 2003) challenge French *et al.*'s proposal by attacking its underlying supposition that all scientific representations represent their targets in virtue of (structural) similarities. All these authors draw upon the aesthetics literature concerning representation in the arts. Authors like Goodman (1976) and Lopes (1996) challenge the idea that pictures represent their targets in virtue of a similarity, resemblance, likeness or other similarity-like relations. French's critics use similar examples and arguments to challenge the idea that theories and models represent in virtue of structural similarities.

For example Suárez (2003) offers five different arguments against what he calls the *similarity conception of representation* and the *isomorphism conception of representation*. According to the former "A represents B if and only if A is similar to B," and according to the latter "A represents B if and only if the structure exemplified by A is isomorphic to the structure exemplified by B" (2003, 227). Suárez argues neither conception can be applied universally to all representational devices, neither conception captures the logical properties of representation, and neither conception can be used to understand misrepresentation. He also draws upon examples to argue that sometimes representations are neither similar or isomorphic to their targets, and sometimes an object can be similar or isomorphic to another object, but neither object represents the other. Thus representation-target similarities are not always necessary, or are they always sufficient for representation.

The arguments offered by Suárez and the others are used to draw at least two conclusions. First, even if similarity-like relations are constitutive for some kinds of representations, we should not expect them to be constitutive of all kinds of representation. Thus no single unified account of all kinds of

---

[6]See Suárez (2003, 229–230) for a helpful distinction between the *means* of representation and the *constituents* of representation.

representation, in the arts and sciences, is likely to be produced by appealing to similarity-like relations. Second, even in cases where similarity-like relations do seem important to representation, an account that focuses exclusively on these relations will be inadequate. Some kind of intentional component (see Suárez (2004) and van Fraassen (2008)) or a system/language relativity (see, for example, Goodman (1976) and Block (1983)) are features essential to representation that are not adequately unaccounted for by a view like French's that considers only representation-target similarities.

French (2003) and Bueno (2011) have directly responded to these aesthetics inspired criticisms. They employ two strategies. First they attempt to meet the arguments head-on and push for the strong thesis that their model-theoretic account may be seen as laying the groundwork for "a unitary account of representation in science and art" (2011, 876). Defending this position French allies himself with Budd (1993) who, contra Goodman and Lopez, defends the view that pictures in art do represent their targets in virtue of isomorphism that obtains between the structure of the surface of a picture and the structure of the relevant visual field. By finding an ally in the aesthetics literature French (2003, 1477) claims that accounts that invoke isomorphism to capture representation in art cannot be easily dismissed. Bueno and French (2011) also attempt to enumerate and address each of their critics main arguments hoping to show that none successfully undermine their view. This head-on strategy, however, is employed with fall-back strategy in mind. French and Bueno (2011, 875–879) also hedge their bet on a unitary account by leaving open a secondary fall-back position. If need be they suggest the stronger thesis about a unified account of representation can be abandoned for the weaker thesis that a model-theoretic account of representation can be used to adequately capture scientific representation, regardless of whether or not it can also capture representation in other domains like the arts. On this fall-back position scientific representations are taken to be distinctive enough that one cannot reliably re-purpose arguments concerning representation in art. Behind Bueno and French's two strategies is roughly the following conviction: it is likely that structural similarities are constitutive of all kinds of representation, but even if there are special (or degenerate) cases found in the arts that cannot be captured by the model-theoretic account, these kinds of cases will not be found in the sciences.

Now I will not have the opportunity to consider in detail all of the arguments and counter arguments offered by each party in this debate. Instead I think we can get to the heart of the matter by focusing on those cases which are likely to be among the most problematic for their view. The cases I shall focus on are exactly those cases which have been the focus of this dissertation: they are cases in which scientific theorizing and modeling practices have an essential discursive aspect. These theories and models are presented via sentences and equations. Let us see how the model-theoretic account of representation can capture representation via language-like expressions. Should the model-theoretic account fail to capture them, then it will prove inadequate as a basis for both a unitary account of representation and, more specifically, as an account of scientific representation.

8.4.1 Linguistic, Symbolic and Iconic Representation

When considering French *et al.* on the issue of linguistic representation it is important that we remember two things. First, as discussed above, French *et al.* bear the burden to prove the adequacy of their view. It is prudent to abandon the search for a descriptive account of scientific theories and models only if we have compelling reasons to think that a descriptive account will be of little philosophical value, and this requires being confident that their version of the semantic view and its underlying model-theoretic account of representation can adequately capture all the philosophically salient features of scientific theorizing and modeling practices. Second, French *et al.* need to be able to demonstrate the adequacy of their view while maintaining their agnosticisms regarding the ontology of theories and models. What makes their proposal so interesting is the suggestion that their account is adequate regardless of what kinds of things theories and models are. Maintaining such a position, however, means that they must not smuggle in ontological commitments in order to defend the adequacy of their account.

Under consideration are linguistic representations, where sentences and equations are used in a representative capacity. These are among the most challenging cases for accounts, like French *et al.*'s, which focus on similarities and similarity-like relations. This is alluded to by French who acknowledges an intuitive connection between the ontology of theories and our understanding of how theories represent:

> Of course, which of these analyses [of representation] one favors will depend on how one conceives of theories in the first place. If one thinks of them in terms of an axiomatized set of logico-linguistic statements, then one might naturally be drawn to accounts of linguistic representation in which notions of denotation, for example, feature prominently. If, on the other hand, one conceives of theories in nonlinguistic terms, as in the model-theoretic approach, then one might look to analyses of representation in the arts where notions of resemblance tend to be brought to the fore. (2003, 1472–1473)

Here French appears to admit that the differences between linguistic and non-linguistic representations are likely to lead some to think resemblance is relevant only if theories are understood to be non-linguistic. This intuition, or course, is something that Bueno and French (2011) argue is mistaken. Before considering their proposed way of handling linguistic representation, however, I want to take a moment to see which differences between linguistic and other non-linguistic representations may be salient to our discussion of representation and consider why these differences might lead some to think denotation, and not resemblance, is the constitutive relation of linguistic representation.

There are some kinds of representations that, at least pre-theoretically, are naturally thought to represent in virtue of some resemblance, likeness, or some other kind of similarity. Photographs, scale models, topographical maps and (literal or realistic) pictures are such cases. What is it about these representations that seem to make them likely to represent in virtue of similarity? Well, roughly, for each we have the impression that the representations have an explicit, or overt, structure which is non-trivially similar to its target and this similarity is central to what makes it a good representation of its target. For example both

140

photographs and pictures are representations that roughly "look like their targets" and it is this similarity in visual appearance that matters when we determine both what a photograph represents and how accurate the representation is. Both have a structure that is explicitly presented by the different coloured regions on the surface of these representations and it is this structure that matters when evaluating the fidelity of the representation. Likewise the scale model explicitly presents a geometric structure and it is in virtue of this structure that it can represent physical systems of larger proportions. Maps, like pictures and photographs, have structures that are presented visually, but the relevant structures are topological. It is the topological structure that seems to matter.

These of course are just pre-theoretic impressions. However they should not be summarily dismissed on these grounds. There is reason to think these impressions reliably provide some insight into different representational modes. Representations have contents and they convey their contents in different ways by different mechanisms. Roughly, photographic and pictorial representations employ a visual mode of representation because it conveys its contents visually. An audio recording of, say, a bird's mating call employs an audible mode of representation. It conveys its contents via audibly discriminable sounds. Recognizing the broad differences in representational modes is not only important to the analysis of representation, but to a representation's use. One can only use representations competently if they come to appreciate the means by which representations convey their contents. The Bloor-Danforth line is coloured green on standard Toronto subway maps. However were one to infer on this basis that the physical track will appear green when observed from a station platform, this would indicate a serious misunderstanding of how subway maps conveys information about the subway system. Learning how to use a representation, then, is largely a matter of learning how within a mode of representation information is conveyed. Since most of us have a basic competence when it comes to using pictures, photographs, maps and scale models, we must have come to learn enough about the mechanisms by which these different representations convey contents. Our impressions about these representations, even if we struggle to express them precisely, no doubt draw upon this know-how.

What of our pre-theoretic impressions concerning language? Here I think it is clear that linguistic representations are not naturally classified as representations that represent in virtue of similarities. The impression we have about linguistic representations is that usually there is no immediate connection between an utterance and its representational target. Direct utterance-target relations are irrelevant when determining either what contents are conveyed by a linguistic representation or whether the representation is accurate. Utterances do have an explicit structure. Sentences are constructed of words and punctuation marks, and words in turn are made of characters which have distinctive shapes. Spoken words have audibly discriminable syllables, pauses and other sounds. But it is not the similarities between these structures and their targets that make them the representations they are. We do not expect the shape of the letters, their colour, their relative spacing, or the sounds of the words to have any interesting similarity to the things they are used to represent. To look for such similarities, it seems, is to misunderstand how linguistic representation works.

So pre-theoretically there do appear to be salient differences that distinguish, on the one hand, linguistic representation and, on the other hand, photographs, maps and pictures. This distinction tracks

well with Pierce's distinction between *iconic* and *symbolic* representation. According to Pierce iconic representations are "[t]hose whose relation to their objects is a mere community in some quality" while symbolic representations are "[t]hose the ground of whose relation to their objects is an imputed character" (2003, Vol. 2, 56). For Pierce iconic representations, like photographs, share qualities with their objects and thus have a likeness with their objects. Linguistic expressions represent symbolically. Here there is no immediate community in quality, but rather the relation between symbolic representations and their objects is imputed, or ascribed, to them.

What is the source of the impression that some kinds of representations are iconic while others are symbolic? This impression is no doubt partly due to the different roles that conventions are observed to play in representations. Roughly, symbolic representation is dependent upon convention in a way that iconic representation does not appear to be. This is suggested by Pierce in distinguishing symbolic representations as those which have their representational significance imputed. The blinking light on my cell phone is a symbolic representation of some state of affairs, namely that a message is awaiting me. That the light represents this, and not something else, is purely a matter of convention. For this reason there is nothing about the properties of the light itself (its colour, the rate at which it blinks, etc.) that are intrinsically important to its representational function—though these features can be rendered important if the right kind of convention is adopted. Conventions clearly play a prominent role in linguistic modes of representation too. The fact that "snow is white" represents snow as white is contingent upon the meanings of the words 'snow' and 'white,' and these are conventionally determined by the linguistic community. If the words had different meanings, then the utterance would represent something different. Accordingly, before a person can effectively use linguistic representations they need to learn the conventionally determined meanings attached to them. On the face of it things are different when it comes to other representations like photographs. Our ability to use photographs is spontaneous or automatic, and does not require learning conventions in the same way. We can, for example, imagine handing a prelinguistic person a photograph. Despite being without language and without any prior experience with photographs, it seems plausible that the person would still be able to use the photograph to identify the person pictured in it. In this way photographic representation is clearly less sensitive to convention, and may even be thought to represent in the absence of any conventions at all.

The reason highly-conventionally determined modes of representation are problematic for similarity-based accounts is that conventions can be set up in almost any way one pleases. We can choose symbols to represent what we like and consequently, as Goodman observes, "almost anything may stand for anything else" (1976, 5). There is no need for symbolic representations to share a likeness or share common qualities with their targets because conventional relations can be established regardless. It is the ascribed connections that appear to do the representational work in the case of symbolic representations. Denotation, at least from this pre-theoretic standpoint, appears to be much more amenable to symbolic representation. Denotation-based accounts appear well suited for capturing the flexibility of symbolic representation. The denotations of elements of a symbolic representation can be set up conventionally so that almost anything may stand for anything else.

To summarize, there is a fairly clear pre-theoretic distinction that can be drawn between iconic and

symbolic representations, with linguistic representations falling clearly on the symbolic side of this distinction. Moreover, accounts of representation that focus on representation-target similarities are likely to have a hard time capturing symbolic representations. Since the significance of symbolic representations is highly determined by convention, symbolic representations needn't be similar to their targets. So French *et al.* seem to face an uphill battle in trying to capture linguistic representation with a model-theoretic account that focuses on structural similarities. In other words, there is reason to doubt the acceptability of conditions 1 and 5 (see page 136) when trying to capture any highly conventionally determined modes of representation, like representation via language-like expressions.

8.4.2 Linguistic Representation as Similarity

The way Bueno and French propose to deal with linguistic representations only becomes clear in the last section of their (2011) paper. There they consider an example discussed by Suárez (2003, 231) as part of what he calls his *argument from variety*. Suárez argues that there is great variety in the representations produced by scientists. Though similarity (or isomorphism) may be among the common means of representation, he argues that it is not the means by which all scientific theories and models represent. He offers four examples. The fourth example is the quantum diffusion equation. Suárez, echoing the basic intuition behind the symbolic-iconic distinction, observes that "A mathematical equation, written down on a piece of paper, represents a certain physical phenomenon but is not similar to it in any relevant respect" (2003, 231).

Bueno and French offer the following response:

> The fourth case of the quantum diffusion equation may appear to be the most difficult. However, Chakravarrty ... has noted that this is no counter-example since it is the semantic content—articulated perhaps through the appropriate mathematical structure, such as configuration space—that is to be regarded as similar to the target, not the superficial representation of this content via pen and paper, or chalk and board. Indeed, we would insist that insofar as the equation is satisfied in the relevant model, it is the structure encapsulated in the latter that is truly important. Equations do not represent by themselves (or perhaps we can say that their representational capacity is 'parasitic' on that of the underlying structures); rather it is the models that function as the relevant medium. Here, we bump up against broader issues to do with arguments in favour of the semantic approach to theories and models in general. Given that our account is articulated within this approach, we can turn to the accepted understanding of how it treats equations—namely, by taking them to be satisfied in the relevant models—in order to appropriately locate the underlying representational device, that is, the relevant structures. (2011, 891)

Here Bueno and French concede that inscribed expressions are not typically interestingly similar to their representational targets. To maintain their emphasis on similarity, then, something else must stand in the

relevant similarity relation to the target. Following a suggestion made by Chakravartty (2010), they look to the semantic contents to stand in the necessary relation. It is the semantic contents that must be similar in the relevant way for the equation to represent some target. This response entails a substantial view about the nature of linguistic representation. Bueno and French are committed to the view that all sentences and equations used by scientists to represent have semantic contents and these contents are regarded as (non-trivially) similar to the representation's target.

From the perspective of the linguistic approach there is no reason to dismiss Bueno and French's proposal out of hand. It may be possible to assimilate linguistic representation to the model-theoretic account in the proposed way, and this proposal is in no way inconsistent with the linguistic approach. What I think Bueno and French have not done, however, is provide sufficient grounds to convince us that their proposed way of capturing linguistic representation is the best available account, and consequently they have not offered sufficient grounds to warrant an agnosticism towards the ontology of theories and models. French *et al.* claim that no commitment regarding the ontology of theories and models is necessary because we can be assured that, whatever theories and models are, their role in scientific practices can adequately be captured by their version of the semantic view. When sentences and equations are used in a representative capacity no special treatment is required because, according to Bueno and French, linguistic representation too can be adequately captured with the semantic view's model-theoretic machinery. While I do think Bueno and French sketch a possible strategy by which linguistic representation might be accommodated within their framework, their proposed way of understanding linguistic representation remains contentious. Bueno and French make substantial commitments concerning the nature of linguistic representation and by making such commitments their account must confront and compete with other views concerning language which, in one way or another, pertain to how uttered expressions are used to represent. Thus their view must confront alternative theories of meaning, truth and reference which are inconsistent with the model-theoretic account. Let us consider a few examples.

Bueno and French are committed to the view that sentences and equations have semantic contents and these contents are understood to be of the right kind that they can stand in a (non-trivial) similarity relation to the representation's target. However many alternative views of language make no reference to entities that might count as 'semantic contents' in Bueno and French's preferred sense. Some accounts, for example, purposely avoid positing abstract, or extra-linguistic, objects that could be identified as propositions (qua extra-linguistic objects) or contents. For example Davidson's (Davidson 1967) view, roughly put, attempts to understand language by appealing to a theory of reference alone, and this theory of reference does not posit any intermediary abstract entities that could be identified with the contents of sentences. So if a Davidsonian view is correct, there is no role to be played by any extra-linguistic 'contents' in our understanding of linguistic representation. Hence there would be no basis for positing the existence of these extra-linguistic contents and certainly no basis for thinking that their similarity to anything is required for linguistic representation.

Even among those views that do identify the contents or meanings of expressions with extra-linguistic entities not all suppose that these extra-linguistic objects are of the right kind to stand in the sort of similarity relation that Bueno and French require. Consider another example. According to Lewis's

144

(1970) possible worlds semantics the meanings, or contents, of utterances are functions from worlds to referents of expressions at worlds. So in Lewis's account there is an additional extra-linguistic element not included in a Davidsonian account, but since these objects are functions it is hard to see how they could be interestingly similar to systems. Functions are the wrong sorts of entities to stand in the kind of similarity relation that Bueno and French take to be constitutive of linguistic representation. So here again we have a view of language which does not support Bueno and French's specific commitments regarding the nature of an expression's semantic contents.

What Bueno and French owe us, then, are reasons why we should accept their view over the alternatives offered by Davidson, Lewis, and others. If any of these competitors prove to be superior, then their proposed way of accommodating linguistic representations fails, and we would have no grounds for thinking that linguistic representations can be captured by their model-theoretic account.

In the above quoted passage Bueno and French allude to other considerations that may be offered in support of their view. They mention that in defending the model-theoretic account of representation "we bump up against broader issues to do with arguments in favour of the semantic approach to theories and models in general" (2011, 891). It is not entirely clear which arguments they have in mind. In chapters 1 and 4 I have considered many of the general arguments offered in favour of the semantic view and found none to be decisive. However, to hazard a guess I think it is likely that some of the considerations they allude to concern *structural realism*, which is a version of scientific realism defended by both French and Ladyman (see French (2006), Ladyman (1998, 2008, and their co-authored papers (2003, 2011)).[7] Let us then briefly consider how arguments for structural realism might be brought to bear here.

Recent interest in structuralism can be credited to Worrall (1989). He suggests that structural realism is a way of accommodating two arguments which seem to pull in opposite directions. The first is the pessimistic meta-induction, which pushes us towards anti-realist positions in the philosophy of science. The second is the no miracles argument, which pushes us in the other direction towards scientific realism. Worrall argues that both can be reconciled if we are willing to admit there is continuity across theory change, but the "continuity is one of form or structure, not of content" (1989, 117). Consequently, he argues we should be realists only about the structure of our best contemporary scientific theories and not about their contents. Worrall's insights have been interpreted and developed in different ways. Ladyman (1998) distinguishes between two formulations of structural realism. *Epistemological structural realism* can roughly be characterized as the view that all we can ever know (through science) is the world's structure. Ladyman contrasts epistemological structuralism with a more metaphysical structuralism, now commonly called *ontic* structuralism. On this view, roughly, when we describe only the structure of the world there is nothing left out of our description because entities do not have 'natures' beyond their relations within a structure. Structure is taken "to be primitive and ontologically subsistent" (1998, 420). In French and Ladyman (2003, 2011) both defend an ontic version of structural realism.

How might arguments for structural realism be used to show that the semantic view adequately rep-

---

[7]Bueno (1999) advocates a version of structural empiricism instead of structural realism. My arguments below apply *mutatis mutandis* to his position as well.

resents scientific representative practices? I take it the argument would go something like this. We have compelling reasons to be structural realists. The semantic view, especially French *et al.*'s partial structures variant of it, is indispensable to structural realism. So we have compelling reasons to accept the semantic view. By this sort of argument the grounds for accepting structural realism also become grounds for accepting the semantic view.

The connection between the semantic view and structural realism is discussed in Ladyman (1998) and French and Ladyman (2003). In both papers French and Ladyman claim that the semantic view is an ideal framework for structural realism. They note that the semantic view shares structural realism's emphasis on structures. But more importantly, they argue that it helps structural realism avoid problems (like the Newman problem, see Newman (1928) and Demopoulos and Friedman (1985)) which arise when theory structure is syntactically explicated. French and Ladyman suggest Worrall's formulation of structural realism in particular falls into this trap. As they say, "Worralls approach is thoroughly embedded in the so-called syntactic view of theories that adopts first-order quantificational logic as the appropriate form for the representation of physical theories" (2003, 33). As a consequence they claim Worrall's formulation of structural realism falls prey to the Newman problem, which purports to show that according to structural realism scientific theories can never tell us anything more than the cardinality of systems in the world. French and Ladyman also argue that their partial structures version of the semantic view can "capture precisely the element of continuity through theory change that is emphasised by the structural realist" (2003, 34). The partial structures approach is supposed to better capture the incomplete, or partial, continuity of structures through theory change.

I think the proponent of the linguistic approach should respond to these considerations in much the same way as above: it should be admitted structural realism might well be correct, but the considerations in favour of structural realism do not provide sufficient grounds to warrant abandoning the search for a descriptive account of theories and models. As I discussed in §3.3.3, the linguistic approach, as much as possible, attempts to remain neutral regarding questions about realism and anti-realism in the philosophy of science. The linguistic approach neither endorses nor rejects structural realism. Nor are there any immediate reasons to think the linguistic approach is inconsistent with structural realism. The question we are considering in this chapter is whether French *et al.*'s proposed reinterpretation of the semantic view might provide a basis to challenge the motivations for pursuing a descriptive account of theories and models. Again, if we had compelling reasons to be confident that the semantic view can adequately represent the philosophically salient features of them, then we would have no reason to try to develop a descriptive account. But the arguments for a structural realism simply do not provide compelling reasons to discourage our search for a descriptive account. For one, structural realism—like most positions in the scientific realism debate—is itself a contentious view. It has been challenged by Psillos ((1995), 2001), Chakravartty (2003)), Demopoulos (2003), Ketland (2004) and others (for a helpful critical summary see Frigg and Votis (2011)).

Moreover, arguments in favour of structural realism confer warrant on the semantic view only if the semantic view is indispensable to structural realism. That is, it must be the only view of theories and models that can furnish an adequate formulation of structural realism. Though some reasons for preferring

the partial structures version of the semantic view have been offered by French and Ladyman, they have not shown that accepting structural realism necessarily requires accepting the adequacy of the semantic view. They argue the semantic view can be used to avoid problems thought to afflict Ramsey-style syntactically explicated notions of theory structure. However there is no consensus in the literature on this point. Syntactically formulated versions of structural realism have been defended against these criticisms by Votis (2003), Worrall (2007), and Cruse (2005). So again, perhaps French and Ladyman are right that the semantic view is indispensable to structural realism, but this remains contentious. In the end I just don't see how the consideration of structural realism could provide sufficiently strong reasons to instil the kind of confidence in the semantic view's adequacy that is required to dissuade us from attempting to develop descriptive accounts of scientific theories and model.

8.5 Conclusions

In the absence of compelling reasons to be confident in the adequacy of the semantic view, the development of a descriptive account of theories and models remains central to a methodologically sound approach to the study of theorizing and modeling. There appears to be very little that can be lost by developing a descriptive account. If French *et al.*'s view were to turn out to be correct, then at worst a descriptive account would prove to be philosophically ineffectual. On the other hand, if French *et al.* are incorrect, then a descriptive account is likely to greatly impact our understanding of scientific theorizing and modeling practices. French suggests that diversity at the ontological level, i.e., diversity in what kinds of things theories and models are, has no impact at the meta-level of philosophical inquiry. If he is wrong about this, our philosophical inquiries must be sensitive to the ontology of theories and models. Our consideration of representation is a case in point. I argued that there is a pre-theoretic distinction to be drawn between iconic and symbolic representations. Contra French *et al.*, I think it is plausible that this distinction (and others) is likely to have an impact on how theories and models represent phenomena. That is, it is likely linguistic/symbolic representation must be understood differently than, say, representation via images, maps, or scale models. To prematurely assume otherwise, i.e., to assume that all modes of scientific representation can be understood in the same way, could lead to a distorted picture of scientific theorizing and modeling practices.

To minimize this threat the sensible way forward is to develop a descriptive account of theories and models that captures potentially salient differences among scientific representations and the practices they are employed in. My investigation of the discursive aspect of theorizing and modeling contributes to this in its own way. By drawing upon features of discourse I have argued we can better determine whether expressions are being used in a direct representational capacity, an indirect representational capacity, or non-representational capacity. Each supports a different understanding of the nature of theories and models. If the direct representational hypothesis is correct—and throughout this dissertation I have provided evidence and arguments in its favour—then many theories and models are linguistic representations. Armed with this information we can approach various philosophical questions from a different direction. For ex-

147

ample, if we want to know how theories and models represent, then we can start by using our descriptive account to identify what kinds of representations they are. Knowing, say, that some theories and models are linguistic representations should lead us to investigate the nature of linguistic representation. Here hopefully philosophers of science can draw upon the hard work of their colleagues who study meaning, truth and reference. If this investigation reveals that there are good grounds for thinking linguistic representation turns on similarity, then so be it. However if it reveals that a denotational account, or some other non-similarity bases account is best, then this account should be used to inform our views about these cases of theorizing and modeling. In this way, a descriptive account of science can drive philosophical inquiry.

Chapter 9

Conclusion

In this work a study of the discursive aspect of theorizing and modeling practices was undertaken in an effort to develop and investigate two main ideas: (1) the idea that the discursive aspect of theorizing and modeling practices can provide important evidence and insight into what theories and models are; and (2) that often theories and models are description-like representations in which language-like expressions are used by scientists to say things about the systems they study.

Idea (2) finds expression in the linguistic approach here developed. The linguistic approach hypothesizes that the sentences and equations constitutive of those theorizing and modeling practices with an essential discursive aspect are regularly used by scientists in a direct representational capacity. When used in this capacity an expression is used to say something directly about the system under investigation in the context. As we saw, the linguistic approach stands in contradistinction with non-representational and indirect representational views, each of which attribute different functions to the expressions found in the discursive aspect of theorizing and modeling practices. By attributing different functions each view naturally leads to different accounts of what theories and models are. When constitutive expressions are used in a direct representational capacity, the associated theories or models are straightforwardly understood as description-like linguistic representations. When these expressions function in a non-representational capacity, then theories or models are naturally understood as non-representational tools, and when expressions function in an indirect representational capacity the associated theories or models are understood as extra-linguistic abstract objects.

The promise of idea (1) really becomes evident when considering the distinction between direct, indirect and non-representational capacities that sentences and equations can play in discursive practices. If we can determine in which capacity expressions are employed in theorizing and modeling practices, then this can provide crucial evidence regarding which conception of theories and models is best supported by the discursive data. In this way (1) finds expression in the act-theoretic methods outlined in this dissertation. By using existing speech act theoretic categories and concepts I was able to specify an evidential basis for distinguishing direct, indirect and non-representational uses of expressions. That is, I was able

to identify salient features of the discursive aspect of theorizing and modeling practices that provide information regarding which capacities sentences and equations function. These considerations lead to the specification of two sets of diagnostic questions, which in conjunction can be used to help determine when utterances are used in a direct, indirect, or non-representational capacity. Now the very fact that (1) so naturally finds expression within an act-theoretic framework speaks in its favour. Speech act theory has independently proven itself to be a valuable tool for capturing and explaining observed features of speech and speech's function in various human activities. I have shown that the same act-theoretic framework can be used to delineate an evidential basis for distinguishing between different uses of expressions in scientific theorizing and modelling contexts. Consequently, I think it would be hard to dismiss discursive data as irrelevant to the investigation of theorizing and modeling practices without directly challenging the underlying act-theoretic framework itself—a framework which has already proven valuable in other domains. The strength of (1) was also impressively demonstrated in our investigation of various examples throughout the dissertation. This is especially true in the case of the London brother's model of superconductivity. This model is a canonical one that has been the subject of dispute among philosophers who approach its study with different conceptions of what theories and models are. Speech act-theoretic methods offer a way of evaluating these competing views against evidence gathered from an essential and easily accessible source, i.e., the discursive aspect of the London brother's modeling of superconductors. The evidence that we found in this case is remarkably coherent, with all the evidence pointing to one clear and consistent way of understanding modeling as direct linguistic representation. In this example, then, we have a good demonstration of the power of the discursive data to shed new light on existing debates.

Now that we are at the end of our investigation of the discursive aspect of scientific theorizing and modeling we can draw a couple conclusions in connection to (1). First, no account of theories or models that claims to be supported by, or sensitive to, scientific practice can reasonably ignore the discursive data. I have shown that it is an important source of evidence that adequate accounts of theorizing and modeling must be able to accommodate. I have also demonstrated how an act-theoretic framework can be fruitfully used to systematically adduce evidence for and against different accounts of theorizing and modeling. So the framework and methods I have developed offer a useful tool for investigating scientific theorizing and modeling.

Of course the evidence drawn from the London brother's 1935 paper on superconductivity points neither to a non-representational account, nor to an indirect representational account of modeling. Rather the evidence indicates that the key sentences and equations presented by the London brothers in their work are used in a direct representational capacity. In other words, the discursive data indicates the London brothers used these key sentences and equations to say things directly about superconductors and thus, based on this evidence, their model is best understood as a linguistic representation of superconductors. In this example we find clear evidence in favour of idea (2). I have also provided some *prima facie* arguments in favour of the direct representation hypothesis. I argued that the linguistic approach's direct representation hypothesis entails a continuity in the discursive aspect of representative practices across scientific and non-scientific domains. This continuity confers many advantages to the linguistic approach over its competitors that do not maintain such a continuity. In light of these *prima facie* considerations

I argued it would be surprising if, contra the direct representation hypothesis, we were to discover that language-like expressions were not routinely used by sciences to directly represent the subjects of their investigations.

Where, then, does our evaluation of idea (2) stand at the end of our investigation? Since (2), at least as expressed by the linguistic approach, entails a substantial empirical claim about the use of expressions in theorizing and modeling practices, it is important not to hastily generalize beyond observed cases. Nevertheless I have shown that there is excellent evidence that the London brothers present and use a linguistic representation of superconductors in their 1935 paper. On these grounds, we have excellent evidence that at least some models are linguistic representations. This alone is reason for taking seriously a more pluralistic view of theories and models which includes description-like linguistic representations among the variety of different kinds of scientific theories and models produced by scientists. However I have argued a somewhat stronger conclusion is also warranted. I think that there is sufficient ground for thinking the linguistic approach's direct representation hypothesis is likely true. That is, in cases where theorizing and modeling practices have an essential discursive aspect it is likely that the sentences and equations associated with theories and models are regularly used by scientists to say things directly about the system, or systems, under investigation in the context. Thus, in cases where theorizing and modeling has an essential discursive aspect, it is likely that the corresponding theories and models are often linguistic representations. In drawing this stronger conclusion the *prima facie* arguments in favour of the direct representation hypothesis are important. Direct representation is, after all, the norm in non-scientific contexts. Only in light of strong countervailing considerations should we abandon the hypothesis that scientists routinely use expressions in a direct representative capacity. I have argued that the requisite countervailing considerations have not been produced. Instead evidence corroborating the direct representation hypothesis was found even in a canonical example favoured by the linguistic approach's competitors.

So as an initial, and necessarily limited, study of the discursive aspect of theorizing and modeling practices, the current investigation has proven fruitful. Of course there are many opportunities for further investigation. One important next step is to examine in detail more examples of theorizing and modeling in the sciences. Only by considering more cases can the direct representation hypothesis be thoroughly evaluated against the discursive data. The consideration of more examples could also help us make better estimates of just how many theories and models are linguistic representations. I harbour the suspicion that linguistic representations are not only routinely produced by scientists, but that the majority of theorizing and modeling practices with an essential discursive aspect produce linguistic representations. This possibility is certainly consistent with our initial findings. If this suspicion proves correct, then linguistic representation would be one of the most pervasive modes of representation in the sciences and this could have implications for many other philosophical projects. In the previous chapter we got a taste of this. The investigation of scientific representation could take a very different form if we knew that linguistic representation was ubiquitous in the sciences. In this case understanding scientific representation would centrally be a matter of understanding linguistic representation, and this will impact which theories and tools will seem promising as a basis for understanding these representative practices.

There is also opportunity for the further development of the act-theoretic categories and methods out-

151

lined in this work. I was concerned, among other things, with the classification of utterances into the two extremely broad categories of representatives and non-representatives. This resulted in a very coarse-grained analysis of the features of utterances salient to these different act-theoretic categories. It would, I think, be interesting to develop a more fine grained analysis of the various kinds of representative speech acts performed and apply it to the study of theorizing and modeling practices. For one, it would be interesting to develop a better account of how different kinds of representative acts might lead to more refined sub-classifications of theories and models within the general class of linguistic representations. For example, a more refined act-theoretic analysis could shed further light on the distinction between theories and models. So, picking up on some themes found in Achinstein's work, it is possible that the same features of utterances salient to the distinction between assertion, hypothesis and, say, guesses, might also be salient to the kinds of intuitive differences that cause philosophers and scientists to call some representations 'models,' while reserving the title of 'theory' for other representations. Also worthy of a more fine grained analysis is the notion of endorsement used to specify the general class of representatives. Again, different kinds or degrees of endorsement characterize assertions, hypotheses, guesses, etc. A richer way of understanding endorsement could help an act-theoretic analysis of linguistic representation provide a better understanding of the kinds of complex epistemic attitudes that scientists can adopt towards complex (multi-utterance) linguistic representations. This in turn could help us better understand, and perhaps even evaluate, the kinds inferential practices that complex scientific theories and models are implicated in. In general, I think there is great potential for the linguistic approach's epistemically rich conception of theories and models to shed light on many aspects of scientific practices.

References

Achinstein, Peter. 1968. *Concepts of Science: A Philosophical Analysis*. The Johns Hopkins Press.

Austin, J.L. 1962. *How To Do Things With Words*. Harvard University Press, second ed.

Bach, Kent and Robert Harnish. 1979. *Linguistic Communication and Speech Acts*. Cambridge: M.I.T. Press.

Benson, Harris. 1995. *University Physics*. Wiley.

Block, Ned. 1983. Mental pictures and cognitive science. *The Philosophical Review* 92(4): 499–541Published by:.

Bohr, Niels. 1913a. On the constitution of atoms and molecules. *Philosophical Magazine and Journal of Science* 26(151): 1–25.

Bohr, Niels. 1913b. On the constitution of atoms and molecules: Part ii–systems containing only a single nucleus. *Philosophical Magazine and Journal of Science* 26(154): 476–502.

Bohr, Niels. 1913c. On the constitution of atoms and molecules: Part iii–systems containing several nuclei. *Philosophical Magazine and Journal of Science* 26(155): 857–875.

Brandom, Robert. 1983. Asserting. *Noûs* 17(4): 637–650.

Brandom, Robert. 1994. *Making it Explicit*. Cambridge Massachusetts: Harvard University Press.

Budd, Malcolm. 1993. How pictures look. In *Virtue and Taste*, eds. D. Knowles and J. Skorupski, 154–175. Oxford: Blackwell.

Bueche, Frederick. 1982. *Principles of Physics*. McGraw-Hill Book Company, fourth ed.

Bueno, Otávio. 1999. What is structural empiricism? scientific change in an empiricist setting. *Erkenntnis* 50: 59–85.

Bueno, Otávio and Steven French. 2011. How theories represent. *British Journal for the Philosophy of Science* 62: 857–894.

Bueno, Otávio, Steven French, and James Ladyman. 2002. On representing the relationship between the mathematical and the empirical. *Philosophy of Science* 69(3): 497–518.

Cartwright, Nancy. 1983. *How the Laws of Physics Lie*. Oxford: Clarendon Press.

Cartwright, Nancy. 1989. *Nature's Capacities and their Measurements*. Oxford: Clarendon Press.

Cartwright, Nancy. 1999. *The Dappled World: A Study of the Boundaries of Science*. Cambridge University Press.

Cartwright, Nancy and Henry Mendal. 1984. What makes physics' objects abstract? In *Science and Reality*, eds. Cushing, Delaney, and Gutting, 134–152. University of Notre Dame Press.

Cartwright, Nancy, Towfic Shomar, and Mauricio Suáez. 1995. The tool-box of science. In *Theories and Models in Scientific Processes*, eds. William Herfel, Wladylaw Krajewski, Iikka Niiniluoto, and Ryszard Wójcicki, vol. 44 of *Ponzan Studies in the Philosophy of the Sciences and Humanities*, 137–149. Amsterdam: Roldophi.

Chakravartty, Anjan. 2003. The structuralist conception of objects. *History and Philosophy of Modern Science* 29: 391–408.

Chakravartty, Anjan. 2007. *A Metaphysics of Scientific Realism: Knowing the Unobservable*. Cambridge University Press.

Chakravartty, Anjan. 2010. Informational versus functional theories of scientific representation. *Synthese* 172(2): 197–213.

Contessa, Gabriele. 2010. Introduction. *Synthese* 172(2): 193–195.

Cruse, Pierre. 2005. Ramsey sentences, structural realism and trivial realization. *Studies in History and Philosophy of Science* 36: 557–576.

da Costa, Newton and Steven French. 2000. Models, theories, and structures: Thirty years on. *Philosophy of Science* 67: S116–S127. Supplement: Proceedings of PSA 1998.

da Costa, Newton and Steven French. 2003. *Science and Partial Truth: A Unitary Approach to Models and Scientific Reasonsing*. Oxford: Oxford.

Davidson, Donald. 1967. Truth and meaning. *Synthese* 17: 304–323.

Demopoulos, William. 2003. On the rational reconstruction of theoretical knowledge. *British Journal for the Philosophy of Science* 54: 371–403.

Demopoulos, William and Michael Friedman. 1985. Bertrand russell's the analysis of matter: Its historical context and contemporary interest. *Philosophy of Science* 52(4): 621–639.

Downes, Stephen. 2009. Models, pictures, and unified accounts of representation: Lessons from aesthetics for philosophy of science. *Perspectives on Science* 17(4): 417–428.

Dummett, Michael. 1973. *Frege Philosophy of Language*. Cambridge: Harvard University Press, second ed.

Einstein, Albert. 1954. Geometry and experience. In *Ideas and opinions : Based on Mein Weltbild*, 232–246. Bonanza Books.

French, Steve and James Ladyman. 2011. In defence of ontic structural realism. In *Scientific Structuralism*, eds. Peter Bokulich and Alisa Bokulich, Boston Studies in the Philosophy of Science. Springer.

French, Steven. 2003. A model-theoretic account of representation (or, i don't know much about art . . . but i know it involves isomorphism). *Philosophy of Science* 70(5): 1472–1483.

French, Steven. 2006. Structure as a weapon of the realist. *Proceedings of the Aristotelian Society* 106: 1–19.

French, Steven. 2010. Keeping quiet on the ontology of models. *Synthese* 172(2): 231–249.

French, Steven and James Ladyman. 1997. Superconductivity and structures: Revisiting the london account. *Studies in the History and Philosophy of Modern Physics* 28: 363–393.

French, Steven and James Ladyman. 1999. Reinflating the semantic approach. *International Studies in the Philosophy of Science* 13(2): 103–121.

French, Steven and James Ladyman. 2003. Remodelling structural realism: Quantum physics and the metaphysics of structure. *Synthese* 136: 31–56.

Frigg, Roman. 2006. Scientific representation and the semantic view of theories. *Theoria* 55: 49–65.

Frigg, Roman and Stephan Hartmann. 2012. Models in science. In *The Stanford Encyclopedia of Philosophy*, ed. Edward N. Zalta. The Metaphysics Research Lab Center for the Study of Language and Information Stanford University. URL http://plato.stanford.edu/archives/fall2012/entries/models-science/.

Frigg, Roman and Ioannis Votis. 2011. Everything you always wanted to know about structural realism but were afraid to ask. *European Journal for Philosophy of Science* 1: 227–276.

Geach, Peter. 1965. Assertion. *The Philosophical Review* 74(4): 449–465.

Giancoli, Douglas. 1985. *Physics: Principles with Applications*. Prentice-Hall Inc., second ed.

Giere, Ronald. 1988. *Explaining Science*. Chicago: University of Chicago Press.

Giere, Ronald. 1994. The cognitive structure of scientific theories. *Philosophy of Science* 61(2): 276–296.

Goodman, Nelson. 1976. *Languages of Art*. Indianapolis Indiana: Hackett Publishing Co., second ed.

Griffiths, David. 1998. *Introduction to Electrodynamics*. Addison-Wesly Publishing Co.

Halvorson, Hans. 2012. What scientific theories could not be. *Philosophy of Science* 79: 183–206.

Halvorson, Hans. 2013. The semantic view, if plausible, is syntactic. *Philosophy of Science* 80: 475–478.

Hughes, R. 1997. Models and representation. *Philosophy of Science* 64: S325–S336. Supplement. Proceedings of the 1996 Biennial Meetings of the Philosophy of Science Association.

Ketland, Jeffrey. 2004. Empirical adequacy and ramsification. *British Journal for the Philosophy of Science* 55: 287–300.

Ladyman, James. 1998. What is structural realism? *Studies in History and Philosophy of Science* 29(3): 409–424.

Ladyman, James. 2008. Structural realism and the relationship between the special sciences and physics. *Philosophy of Science* 75: 744–755.

Lewis, David. 1970. General semantics. *Synthese* 22: 18–67.

London, Fritz. 1937. A new conception of superconductivity. *Nature* 140: 736–739 and 834–836.

London, Fritz and Heinz London. 1935. The electromagnetic equations of the supraconductor. *Proceedings of the Royal Society* A149: 71–88.

Lopes, Dominic. 1996. *Understanding Pictures*. Oxford: Claredon Press.

McEwan, Micheal. 2006. The semantic view of theories: Language and representation. Master's thesis, University of Calgary.

McGrath, Mathew. 2012. Propositions. In *The Stanford Encyclopedia of Philosophy*, ed. Edward Zalta. The Metaphysics Research Lab Center for the Study of Language and Information Stanford University, summer 2012 ed. URL http://plato.stanford.edu/archives/sum2012/entries/propositions/.

Moravcsik, J. 1975. Aitia as generative factor in aristotle's philosophy. *Dialogue* 14: 622–638.

Mormann, Thomas. 2007. The structure of scientific theories in logical empiricism. In *The Cambridge Companion to Logical Empiricism*, eds. Alan Richardson and Thomas Uebel. Cambridge University Press.

Morrison, Margaret. 1999. Models as autonomous agents. In *Models as Mediators*, eds. Mary Morgand and Margaret Morrison, 38–65. Cambridge: Cambridge University Press.

Muller, F. 1996. The equivalence myth of quantum mechanics–part i. *Studies in the History and Philosophy of Modern Physics* 28(1): 35–61.

Neal, Dick. 2004. *Introduction to Population Biology*. Cambridge University Press.

Newman, M. H. 1928. Mr. russell's "causal theory of perception". *Mind* 37(146): 137–148.

Pagin, Peter. 2004. Is assertion social? *Journal of Pragmatics* 36: 833–859.

Peirce, Charles S. 2003. The writings of charles s. peirce – a chronological edition.electronic edition. URL http://crkn.nlx.com/xtf/view?docId=peirce_w/peirce_w.02.xml;chunk.id=writings.charles.v2.d025;toc.id=writings.charles.v2.d025;jumped2page=yes;brand=default;query=56#pb-56.

Psillos, Stathis. 1995. Is structural realism the best of both worlds? *Dialectica* 49: 15–46.

Psillos, Stathis. 2001. Is structural realism possible? *Philosophy of Science* 68: S13–S24.

Putnam, Hilary. 1962. The analytic and the synthetic. In *Minnesota Studies in the Philosophy of Science*, eds. Feigl and Maxwell, vol. 1, 350–397. University of Minnesota Press.

Quine, W. V. 1951. Two dogmas of empiricism. *The Philosophical Review* 60(1): 20–43.

Recanati, François. 1987. *Meaning and Force: The Pragmatics of Performative Utterances*. Cambridge: Cambridge University Press.

Schrödinger, Erwin. 1978. On the relation between the quantum mechanics of heisenberg, born and jordan, and that of schrödinger. In *Collected Papers on Wave Mechanics*, 45–61. New York: Chelsea Publishing Company, second ed.

Searle, John. 1969. *Speech Acts: An Essay in the Philosophy of Language*. Cambridge University Press.

Searle, John. 1976. A classification of illocutionary acts. *Lanaguage and Society* 5(1): 1–23.

Strawson, Peter. 1964. Intentions and convention in speech acts. *The Philosophical Review* 73: 439–460.

Suáez, Maricio and Nancy Cartwright. 2008. Theories: Tools versus models. *Studies in the History and Philosophy of Modern Physics* 39: 62–81.

Suárez, Mauricio. 1999. The role of models in the application of scientific theories: Epistemological implications. In *Models as Mediators*, eds. Margaret Morrison and Mary Morgan, Ideas in Context, 168–195. Cambridge University Press.

Suárez, Mauricio. 2003. Scientific representation: Against similarity and isomorphism. *International Studies in the Philosophy of Science* 17: 225–224.

Suárez, Mauricio. 2004. An inferential conception of scientific representation. *Philosophy of Science* 71: 767–779.

Suppe, Frederick. 1977. The search for philosophic understanding of scientific theories. In *The Structure of Scientific Theories*, ed. Frederick Suppe, 3–241. Urbana: University of Illinois Press, second ed.

Suppe, Frederick. 1989. *The Semantic Conception of Theories and Scientific Realism*. Chicago: University of Illinois Press.

Suppe, Fredrick. 2000. Understanding scientific theories: An assessment of developments 1969–1998. *Philosophy of Science* 67: S102–S115. Proceedings of the 1998 Biennial Meetings of the Philosophy of Science Association.

Suppes, Patrick. 1957. *Introduction to Logic*. Princeton: D. van Norstrand Co. Inc.

Suppes, Patrick. 1967. What is a scientific theory? In *Philosophy of Science Today*, ed. Sidney Morgenbesser, 55–67. New York: Basic Books Inc.

Suppes, Patrick. 1969a. A comparison of the meaning and uses of models in mathematics and the empirical sciences. In *Studies in the Methodology and Foundations of Science: Selected Papers from 1951 to 1969*, 10–23. Holland: D. Reidel Publishing Company.

Suppes, Patrick. 1969b. Foundational aspects of theories of measurement. In *Studies in the Methodology and Foundations of Science: Selected Papers from 1951 to 1969*, 46–64. New York: D. Reidel Publishing Company.

Suppes, Patrick. 1969c. Models of data. In *Studies in the Methodology and Foundations of Science: Selected Papers from 1951 to 1969*, 24–34. New York: D. Reidel Publishing Company.

Thagard, Paul. 2005. *Mind: Introduction to Cognitive Science*. MIT Press, second ed.

Thomson-Jones, Martin. 2010. Missing systems and the face value practice. *Synthese* 172(2): 283–299.

van Fraassen, Bas. 1980. *The Scientific Image*. Oxford: Oxford University Press.

van Fraassen, Bas. 1985. Empiricism in the philosophy of science. In *Images of Science: Essays on Realism and Empiricism, with a Reply from Bas C. van Fraassen*, eds. Paul M. Churchland and Clifford A. Hooker, 245–308. Chicago: University of Chicago Press.

van Fraassen, Bas. 1989. *Laws and Symmetry*. Oxford: Oxford University Press.

van Fraassen, Bas. 2000. The semantic approach to scientific theories. In *The Nature of Scientific Theory*, ed. Lawrence Sklar, vol. 2 of *Philosophy of Science*, 175–194. New York: Garland Publishing Inc.

van Fraassen, Bas. 2008. *Scientific Representation: Paradoxes of Perspective*. Oxford: Oxford University Press.

von Neumann, John. 1955. *Mathematical Foundations of Quantum Mechanics*. Princeton: Princeton University Press. Trans. by Robert Beyer.

Votis, Ioannis. 2003. Is structure not enough? *Philosophy of Science* 70(5): 879–890.

Williamson, Timothy. 2000. *Knowledge and its Limits*. Oxford: Oxford University Press.

Woodward, James. 2011. Scientific explanation. In *The Stanford Encyclopedia of Philosophy*, ed. Edward N. Zalta. The Metaphysics Research Lab Center for the Study of Language and Information Stanford University. URL http://plato.stanford.edu/archives/win2011/entries/scientific-explanation/.

Worrall, John. 1984. An unreal image. *The British Journal of Philosophy of Science* 35(1): 65–80.

Worrall, John. 1989. Structural realism: The best of both worlds. *Dialectica* 43: 99–124.

Worrall, John. 2007. Miracles and models: Why reports of the death of structural realism may be exaggerated. *Royal Institute of Philosophy Supplements* 82(61): 125–154.

Wright, Crispin. 1992. *Truth and Objectivity*. Cambridge: Harvard University Press.