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On the Role of Mathematics in Scientific Representation

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Abstract

In this dissertation, I consider from a philosophical perspective three related questions concerning the contribution of mathematics to scientific representation. In answering these questions, I propose and defend Carnapian frameworks for examination into the nature and role of mathematics in science.

The first research question concerns the varied ways in which mathematics contributes to scientific representation. In response, I consider in Chapter 2 two recent philosophical proposals claiming to account for the explanatory role of mathematics in science, by Philip Kitcher, and Otavio Bueno and Mark Colyvan. My novel and detailed critique of these accounts shows that they are too limited to encompass the diverse roles of mathematics in science in historical and contemporary scenarios. The conclusion is that any such philosophical account should aim to faithfully capture the structure of our theories and their use in applied contexts.

This insight prompts the second question guiding this dissertation that I consider in Chapter 3, regarding a viable philosophical account of the role of mathematics in scientific theories. I respond by proposing a modified form of the reconstructive frameworks for philosophical analysis developed by Rudolf Carnap for theoretical entities. I propose three amendments to Carnap's account: i) a semantic view for the representation of theories, ii) a careful consideration of instances of the use of theory in representing target systems, and iii) consideration of the practical complexity of relating theory to experimental data.

The final research question for this dissertation asks what, if anything, we can legitimately conclude about the nature of theoretical entities invoked by a theory in light of its success in representing phenomena. In the backdrop of the Carnapian frameworks proposed in Chapter 3, I argue that contemporary ontological debates in the philosophy of science are largely premised on an acceptance of Willard Quine's epistemological outlook on the world and a dismissal of Carnap's approach, which can be used to offer a satisfactory deflationary resolution. This is in the service of my contention that a Carnapian attitude to central issues in the philosophy of science is decidedly preferable to the route championed by Quine.

Keywords

Mathematical representation, Mathematical explanation, Abstract explanation, Rudolf Carnap, Logical Empiricism, Philip Kitcher, Indispensability argument, Quine, James Clerk Maxwell, Electromagnetism, Mark Colyvan.

There have been and still are geometricians and philosophers, and even some of the most distinguished, who doubt whether the whole universe, or to speak more widely the whole of existence, was only created in Euclid's geometry; they even dare to dream that two parallel lines, which according to Euclid can never meet on earth, may meet somewhere in infinity. I have come to the conclusion that, since I can't understand even that, I can't expect to understand about God. I acknowledge humbly that I have no faculty for settling such questions, I have a Euclidean earthly mind, and how could I solve problems that are not of this world? And I advise you never to think about it either, my dear Alyosha, especially about God, whether He exists or not. All such questions are utterly inappropriate for a mind created with an idea of only three dimensions.

Fyodor Dostoevsky, *The Brothers Karamazov*

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1 Introduction

Mathematics has long been a handmaiden of science. The two have become ever more intimate with time such that as a matter of course, theories in many areas of science are articulated and models of these theories are developed in the language of mathematics. However, a lack of clarity persists in much of contemporary philosophical thinking regarding several matters related to this activity. These include the ontological status of the mathematics used in our scientific theories, the ways in which mathematics is useful in formulating these theories, and whether mathematics can play explanatory, predictive, or confirmational roles in science and, if so, what these precisely involve. This curiosity regarding the relation between mathematics and its use in scientific representation has manifested itself in the form of several related questions and claims in the philosophical literature. In spite of its importance, there thus far seems to be no philosophical consensus on the role of mathematics in science. Nonetheless, there does appear to be a growing realisation that mathematics can contribute to scientific representation in a variety of ways.[1]

In light of the clear epistemological benefits of the application of mathematics to science, many philosophers have adopted strong metaphysical theses regarding the existence of mathematical entities and operations used in scientific representation. Roughly, the idea is that since our most successful scientific theories seek to describe and predict phenomena and are thought to be accurate descriptions thereof, we ought to be committed to entities posited by these theories. As reference to mathematical entities is

[1] See, for instance, Wilson (2006) and Pincock (2012).

an indispensable part of our most successful scientific theories, we ought thus to believe in their *existence* as well. Debate over this thesis—known as the Indispensability Argument—has persisted for over five decades,[2] with proponents of either side weighing in with observations and arguments that have caused the discussion to evolve significantly since its commencement.

In this dissertation, I consider from a philosophical perspective three general questions concerning the contribution of mathematics to scientific representation. These questions raise issues regarding the role of mathematics in such representation, the possibility of a philosophical account of scientific theories that can help clarify and explicate this role, and those related to the ontological status of mathematics arising out of its successful application to physical systems. The instances of representation that I treat in the course of this dissertation will primarily be drawn from literature in the philosophy of science, and will vary in their complexity and detail. Furthermore, while the claims made here should be considered to hold for all scientific disciplines that employ mathematics to represent, most of the examples and cases that I use are from theoretical and applied physics. These include scenarios from fluid dynamics, graph theory, and electromagnetism. In order to be able to accommodate such a scope of instances and consistently consider them in light of diverse philosophical approaches, I offer a minimal, liberal description of a (mathematical) scientific representation as that which has as its content the existence of certain kinds of relations between a mathematical structure and the arrangement of certain properties and quantities in the relevant scientific domain[3]. A

[2] See Colyvan (2001) for a comprehensive history of the issue.
[3] This description is from (Pincock 2012, 27).

mathematical structure in turn is a collection of mathematical objects with some formal relations obtaining among them. I approach the specific questions that follow, regarding the contribution of mathematics to scientific representation, as ones that straddle the boundary between the *general* philosophy of science and the philosophy of *applied mathematics*. Moreover, as will become evident, I use traditional philosophical methods of analysis and argumentation in conjunction with an emphasis on important details of the reasoning involved in particular scientific contexts by way of methodology. The eventual result is the proposal and defence of a *Carnapian* framework to examine and explain the role of theoretical entities, including mathematics, in our scientific theories. Furthermore, I argue that the attitude towards the status of theoretical entities that follows from the kind of framework that I propose helps dissolve a number of spurious ontological debates in philosophy, such as the Indispensability Argument issue mentioned above, by showing that they are misguided because they are based on questions that are not well formed. Such an attitude can also provide direction for productive research in the discipline in general, and in the philosophy of science in particular.

I now turn to the three questions that guide my project in this dissertation, and provide an overview of the role of each subsequent chapter in advancing the above thesis as a response to them.

1.1 Guiding questions

A. How does mathematics assist in scientific representation?

B. *Is there a promising philosophical account available to represent the theoretical/mathematical entities employed in our scientific theories in order to help clarify and explain their role?*

C. *What can we conclude about the nature of theoretical/mathematical entities employed in a theory from its success in representing phenomena? More generally, what philosophical benefit, if any, is to be expected from ontological inquiries of the above sort, and how ought it to shape our preferences concerning research questions in the discipline?*

The above questions can be reasonably thought to engage a number of important questions in contemporary literature in the philosophy of science. Each of these is taken up in order in the three subsequent chapters, whereas Chapter 5 summarises my responses to them in light of my findings. My treatment of these questions will show that they continue to be the subject of intensive inquiries and lively debates in the philosophy of science. Thematically, *A* is an investigation into the *role* of mathematics in scientific representation, whereas *C* scrutinises the issue of its *ontological status* in light of its employment in our successful scientific theories. In this vein, *B* can be considered to be an intermediate inquiry that bridges *A* and *C*, and is grounded in the idea that a philosophical account of formal theories that can appropriately represent the role of theoretical/mathematical entities can help us frame, assess, and address questions regarding their status. That is, if we have available to us an account that can clarify and explain the role of mathematics as it is used in science, we will be better able to determine whether questions pertaining to the ontological status of theoretical entities are warranted and, if so, how they ought to be pursued.

All three questions form the following narrative for this dissertation: My response to *A* in Chapter 2 shows that contemporary *philosophical* accounts of the contribution of mathematics to scientific representation that claim to circumscribe all or the bulk of such instances within their ambit are unsatisfactory because they are too limited in the face of the sheer diversity of such contributions. The limitation in the frameworks proposed in these accounts also stems from a lack of attention to historical and contemporary instances of the application of mathematics to science. This prompts *B*, in response to which I propose and defend my Carnapian frameworks as appropriate to represent for the philosophical analysis of the mathematics used in science in a manner harmonious with practical considerations. Finally, in posing *C*, I do not intend to suggest that questions regarding the ontological status of theoretical or mathematical entities are inevitable or even appropriate in light of the successful contribution of mathematics to scientific representation.[4] However, given that the ontological status of theoretical entities continues to be hotly debated in the philosophies of science and mathematics, where the Indispensability Argument issue is exemplary, it is well worth inquiring into the assumptions that ground such disagreements. Furthermore, since I propose Carnapian frameworks of analysis for the explication of mathematical/theoretical terms in representation, it is only natural to extend this inquiry to investigate the merits of the attitude that follows from adherence to such frameworks towards ontological issues in the

[4] Indeed, as will become clear in §4.3, I *do* think that such questions are misguided in their standard philosophical formulations.

philosophy of science. Lastly, should I be correct in my contention that such an approach can help us reframe and dismiss a number of such debates and guide us in pursuing productive questions, the philosophy of science would benefit greatly from an according shift in focus. Thus, it is only appropriate that I try to convince the philosophical community that ontological issues are undeserving of the considerable research interest that they continue to garner.

A minor note on usage. In the statement of *C* above, I hint at the interchangeable use of the terms "theoretical" and "mathematical." As will become evident in Chapters 3 and 4, I do so because the debate in the philosophical literature regarding the ontological status of mathematics has been largely conducted in such terms. However, the reader should note that insofar as I intend to focus here on theories in physics, which are formally articulated using mathematics, an investigation into the role of theoretical entities should be considered a natural part of an inquiry into the mathematics used in our theories.

1.2 The role of mathematics in representation: Explanation

In order to address *A* above, I consider in Chapter 2 two accounts of the role of mathematics in science, one proposed by Kitcher (1981, 1989) and the other by Bueno and Colyvan (2011). To the best of my knowledge, these are the only two *all-encompassing* proposals in the recent philosophical literature for the application of mathematics to scientific representation, in that their respective claims are intended to pertain to all applied mathematics.[5] Furthermore, my choice of these accounts is motivated by the fact that, while varying in their respective aims and details, both purport to highlight the *explanatory* contributions of mathematics to scientific representation. The arena of explanation appears to be naturally suited to an investigation of the contribution of mathematics to science. Hempel proposed his famous Deductive–Nomological model of scientific explanation (Hempel and Oppenheim 1948) because of a Humean suspicion of the notion of causation (Cartwright 2004). Models of scientific explanation that have been proposed in the philosophical literature tend to gloss over the role of mathematics in furnishing such explanation.[6] Since mathematics is a science of abstraction, an account of the explanatory contributions of mathematics to scientific representation offers considerable promise: divorced from considerations that pertain exclusively to scientific explanation, an investigation into the explanatory role of mathematics in science should help illuminate the ways in which the former contributes to the latter.

Kitcher sees the explanatory activity of mathematics in science as consisting in its ability to unify by using the most economical argument patterns to generate the largest number of conclusions regarding the world, in the same manner as an optimisation problem. He writes in the wake of debilitating attacks on the covering law model of explanation as well as the enterprise of Logical Empiricism in the philosophy of science. Kitcher is sympathetic to the spirit of the empiricist enterprise, and looks to incorporate many of its features into his model. Bueno and Colyvan have put forth a bolder proposal

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[5] Strictly speaking, this is true of Kitcher only if I add the phrase "all applied mathematics within the restrictions imposed by the conceptual focus of the proposal," since his account only considers its explanatory role in these applications. However, it is intended to apply to all instances where the mathematics can be said to be explanatory.

[6] Hempel and Oppenheim (1948) and Hempel (1965a, b) are two such instances. Others, such as Salmon (1984) and Dowe (2000), ignore the role of mathematics in explanation altogether.

that aims to capture all contributions of mathematics to science by focusing on mappings between empirical and mathematical structures. It is not unreasonable to see their effort as part of a structuralist revival in the philosophy of science in Britain.[7]

In response to the question *How does mathematics assist in scientific representation?*, my examination of the above explanatory accounts yields at least two such ways: i) by connecting different phenomena using mathematical analogies, and ii) by isolating recurring features of phenomena through acausal representations. Moreover, in spite of the varying contextual motivations of the projects of Kitcher and Bueno and Colyvan, the significant differences in their respective approaches and details thereof, and the diversity in the arguments I use to critique them, I find a common way in which both accounts seem to falter: due to "structural deficiencies," whereby the frameworks for representation proposed in both these accounts are too narrow to accommodate a number of explanatory contributions of mathematics to science in historical as well as contemporary scenarios. This oversight can be seen as a symptom of an old disease in philosophy, of the making of sweeping claims about science while disregarding the details of its practice. The upshot of my detailed treatment of each proposal is that a crucial desideratum of any general framework aiming to capture the contribution of mathematics to science is that it be able to faithfully capture the structure of our theories. In particular, such an account should outline clear mechanisms for the assignment of interpretations to the theoretical entities used in a representation and clarify the inter-relationships among them. This involves taking heed of the ways in which a theory is

[7] See, for instance, da Costa and French (2003), French and Ladyman (1999, 2003a, 2003b), Ladyman (1998), and Ladyman and Ross (2007).

related to experimental contexts for its verification as well as the manner in which it is used to solve problems in the world.

It is this insight that prompts question *B* in §1.1 regarding a general account for the representation of theoretical entities in science, which is addressed in Chapter 3.

1.3 A Carnapian framework for theoretical entities

The general philosophical accounts of representation referred to in the inquiry in *B* in §1.1 can loosely be considered to be ones that can adequately reflect the structure of our scientific theories. Such accounts would clarify the logical and mathematical rules assumed by our theories. They would also be capable of articulating fundamental scientific laws, specifying the manner in which crucial theoretical notions therein are defined and empirically interpreted, and describing the assumptions needed in order to devise a representation. In Chapter 3, by way of a response to the question *Is there a promising philosophical account available to represent the theoretical/mathematical entities employed in our scientific theories in order to help clarify and explain their role?*, my proposal and defence of a Carnapian framework for the reconstruction of scientific theories is based on the recognition that any account purporting to treat theoretical entities as they are employed in science should be conceived of and structured in a manner that respects scientific reasoning as well as its dialectical relationship with the vast array of relevant experimental procedures and concerns, whereby the activity in one domain informs and is informed by that in the other. I choose a conception based heavily on Rudolf Carnap's work because frameworks for reconstruction of the kind that he proposes appear, at least *prima facie*,[8] to be well suited to an examination of theories in physics.

In addition to my defence of the treatment of theoretical entities in his linguistic frameworks against criticisms in the literature, I propose three amendments to Carnap's account: i) a semantic view for the representation of theories, whereby a theory is taken to be a family of models rather than a set of sentences, as in the syntactic view, ii) a careful, detailed "bottom-up" consideration of instances of the use of theory in representing target systems, in contrast to the traditional philosophical approach based on a priori concerns or toy examples, and iii) a consideration of the practical complexity of relating theory to experimental data. As with the rationale for the transition from question *A* to *B* as well as my adoption of a framework based on his proposal, my departures from Carnap's account, which render my proposal properly *Carnapian*, are similarly driven by the desire to be appropriately sensitive to scientific reasoning as applied to the formulation and evaluation of theories. Moreover, my defence of this conception against influential criticisms in §3.4 and §3.5 locates flaws in certain assumptions grounding them that evince a similar failure to engage scientific reasoning that came to the fore in my critiques of the accounts of mathematical explanation in Chapter 2. My aim is to show that my Carnapian frameworks can usefully represent theoretical entities in science in a manner harmonious with scientific practice.

[8] See Stein (1992) and (1994) for arguments to this end.

1.4 The ontological status of mathematics

A common tendency in the general philosophical literature is to hastily read off ontology from successful representational systems in science. Among the many unfortunate consequences of this predilection is the pursuit of misguided debates regarding the nature of theoretical entities, a major reason for which is a widespread misunderstanding of the nature and role of frameworks in scientific theorising and representation. In consonance with my proposal of a Carnapian conception of theoretical entities in Chapter 3, Chapter 4 highlights this error through a pragmatic comparison of the influential epistemic views of Carnap and Willard Quine. The difference between the two thinkers' views naturally manifests itself most starkly in the arena of the ontological status of the theoretical, and hence mathematical, entities posited by our scientific theories. Quine sees philosophy as continuous with science,[9] where the latter is an inquiry into reality, and thus sees no need to manufacture a distinction in our body of knowledge between theoretical and factual content. He thus thinks that all of our knowledge, including the theoretical components of our scientific theories, is subject to empirical verification. Carnap rejects questions concerning the status of theoretical terms as conceptually misguided, where his outlook is firmly embedded in and influenced by his linguistic frameworks.

In seeking to frame and address the final substantive question guiding this dissertation—*What can we conclude about the nature of theoretical entities employed in a theory from its success in representing phenomena, and how ought we to shape our*

[9] According to Quine (1981b), this is one of the "five milestones" achieved by Empiricism in the last two centuries.

preferences concerning research questions in the discipline in light of anticipated philosophical benefit?—I argue that contemporary ontological debates in the philosophy of science are largely premised on an acceptance of Quine's epistemological outlook on the world and the standards of justification pertaining to our knowledge of it, which implies a dismissal of Carnap's view. I then show how adherence to the Quinean perspective has sparked spurious debates in philosophy, most recently instantiated by the aforementioned Indispensability Argument issue, that continue to rage without a consensus on the conceptualisation of the issues at hand, the methodology or set of methodologies appropriate to apply to the problem once it is precisely formulated, the standards of evidence that are considered admissible, and so on. Hence, the pursuit of research questions in the philosophy of science that lead to unfruitful discussions of this nature is inadvisable. On the contrary, by engaging a recent debate that reflects on ontological questions in the backdrop of the theoretical and experimental research that led to the confirmation of the atomic hypothesis in the early 20th century, I show how Carnap's approach to questions regarding the ontological status of theoretical entities in science can be used to offer a satisfactory deflationary resolution. This is in the service of my contention that such an attitude to central issues in the philosophy of science is decidedly preferable to the route championed by Quine.

1.5 Limitations

The role of mathematics in scientific representation encompasses an enormous amount of knowledge from a number of disciplines. Hence, any attempt to treat the subject in its entirety by taking into account all relevant dimensions and perspectives is unlikely to succeed. Furthermore, even within the more restrictive confines of my philosophical approach to the issue in this dissertation, the extent and depth of my treatment of each of the research questions stated in §1.1 has been conditioned by my overarching advocacy of a Carnapian approach in considering questions concerning the role and nature of mathematics as used in our scientific theories. For instance, with regard to my response to *A*, the modes of mathematical explanation that come to the fore from my consideration of proposals by Kitcher and Bueno and Colyvan are not by any means exhaustive of the explanatory contributions of mathematics, nor are they intended to be. At the very least, the philosophical literature on the issue has yielded a number of explanatory contributions that I do not consider.[10] This limitation arises out of my interest only in general, all-encompassing philosophical accounts of the contribution of mathematics to representation. Similarly, with regard to *B*, I only consider and defend a Carnapian conception of theoretical entities in representation, to the exclusion of other accounts, e.g., Kuhn's (1962) paradigms for scientific theories.[11] However, a careful consideration of even a few representational frameworks of this sort would easily make for a dissertation of its own. And so I restrict myself to the proposal and defence of my Carnapian conception. Finally, the Quinean and Carnapian perspectives do not at all sum up philosophical opinions on the ontology of theoretical entities in science.[12] However,

[10] See, for instance, Pincock (2012, §3.2) and Batterman (2002).

[11] I do not mean to suggest that the approach or aims of Kuhn's enterprise are similar to those of Carnap's. However, among other things, Kuhn was concerned with the articulation of important scientific theories and their application for the solution of important problems—one sense in which he used the word "paradigm." In this sense, he can be said to be involved in a similar project to that pursued by Carnap. See Pincock (2012, 122) for a relevant comparison between the two.

[12] For example, see Schaffer (2009) for a summary of a neo-Aristotelian programme of metaphysics as a response to the Quinean and the Carnapian approaches.

insofar as they form the bulk of the general perspectives in the philosophy of science, I have chosen to busy myself with them to the exclusion of other outlooks.

As mentioned in §1.1, the questions posed and addressed in this dissertation engage important philosophical queries regarding the role and ontological status of mathematical entities used in science. In offering responses to the questions stated at the outset, my hope in this work is to recommend Carnapian frameworks for the philosophical examination of the contributions of theoretical entities in general, and mathematics in particular, to our scientific thought. Misconceptions about mathematical representation and the roles played by mathematical objects therein have led to philosophical misunderstandings regarding theories and theoretical entities. By offering a more promising account of mathematical structures in science, my Carnapian notion of framework also promises a more convincing and realistic way of understanding them. The specific ambition is to help repopularise the use of these frameworks in our philosophical reflections on science, for both science and philosophy stand to profit from it.

2 The explanatory role of mathematics in science

Als ik zou willen dat je het begreep, had ik het wel beter uitgelegd.

Johan Cruyff

In light of the project for this dissertation outlined in the Introduction, my aim in this chapter is to investigate two proposals for the explanatory role of mathematics in representation that claim to account for all or the bulk of its contributions to science. The lure of an overarching framework of this kind is clear, for if we have at our disposal an account that can encapsulate the vast variety of the applications of mathematics to phenomena that appear to be explanatory, we can use it to make considerable headway in responding to question *A* posed in §1.1—*How does mathematics assist in scientific representation?* At a grander scale, such a framework would probably contribute invaluably to philosophical accounts of confirmation and questions concerning structural invariance across theories. This hope is founded on the quite reasonable assumption that a successful general account boasting such breadth would tie together or relate the varied applications of mathematics in a manner more amenable to a structured inquiry into these questions than otherwise.

Philip Kitcher proposes an ambitious, holistic account of scientific and mathematical explanation that sees the unification of seemingly disparate phenomena, structures, and theories as the task of both. The accommodation in his proposal for a role for mathematics forms a major reason for my consideration of Kitcher's account of explanation in §2.1: it draws heavily on important features of the covering law model of explanation of the logical empiricists *but*, unlike this and subsequent accounts of scientific explanation in the literature, such as that proposed by Salmon (1984), offers an explicit explanatory role for mathematics as an indispensable part of scientific theories.[1] Bueno and Colyvan are concerned with a model that can represent all instances of the application of mathematics to the physical sciences. Consonant to the scale of the task, they claim that their mapping-based account yields copious rewards, of which a clarification of the explanatory role of mathematics in representation is but one. My examination of these accounts will reveal that neither succeeds in delivering on the promise of a comprehensive model of how mathematics is explanatory in science. Against Kitcher's proposal, I present and examine in §2.2 and §2.3, respectively, Margaret Morrison's claim that far from being relentless companions, explanation and unification tend to part ways in many instances where the mathematics involved is clearly functioning in an explanatory capacity. Bueno and Colyvan's conception of the application of mathematics is found to be problematic in §2.5 because it neglects basic aspects of representation in applied mathematics, and hence is neither representative nor practicable. My analysis of these proposals will also seek to uncover a general way in which these accounts of mathematical explanation and application are found wanting. As we shall see in §2.6, the insight that this yields leads us to question *B*, posed in §1.1, as well as the manner in which it is addressed in Chapter 3.

[1] It is also interesting to note in passing that Kitcher explicitly exonerates his account of explanation from a consideration of "idealisations" in science. This likely means that his model cannot accommodate mathematical explanations of the sort discussed by Batterman (1997, 2002).

2.1 Kitcher and explanation as unification

Kitcher (1976, 1981, 1989) proposed his theory of scientific explanation in the backdrop of a series of criticisms that undermined the covering law model of explanation offered by Hempel and Oppenheim (1948, Hempel 1965). This model is based on the insight that explanation is derivation: specifically, it is the deductive or inductive derivation of a sentence describing a phenomenon to be explained—the explanandum—by using a set of sentences, called the explanans, that contain at least one general law. This is harmonious with the claim by Carnap (1966, 7) that successful explanation inevitably requires an appeal to a general law. Not only is Kitcher sympathetic to these attempts to furnish a theory of scientific explanation, he adopts the view that an explanation assumes the form of a logical derivation. He then proposes a holistic model that encompasses scientific and mathematical explanation under the common principle of unification, i.e., the comprehension of a maximum number of facts and regularities through a minimum number of theoretical concepts and assumptions (1981, 508). In fact, Kitcher explicitly cites Feigl and Hempel as inspiration for the idea that explanation is nothing other than unification, and calls this the “unofficial view” of explanation harboured by the logical empiricists.

Kitcher thinks that any purported account of scientific explanation should advance our understanding of phenomena and allow us to arbitrate in historical as well as contemporary disputes in science. The notion of unification can be easily linked to that of the enhancement of our understanding, as it presumably helps us discern how a diversity of phenomena may be the manifestations of the same underlying mechanism. Kitcher thinks that the most general problem of scientific explanation is to determine conditions that must be met to answer an explanation-seeking question (1981, 510). Hence, he sees explanation as an activity that involves answering questions. An explanation is an ordered pair consisting of a proposition and an "act type." This conceptualisation of explanation also clarifies its relevance to arguments: the ordered pair <p, explaining q> is an explanation when a sentence expressing p bears an appropriate relation to a particular argument provided by our scientific theories.

The idea is to observe common "argument patterns" (1981, 516) appearing in a wide variety of scientific representations of diverse scientific systems. Kitcher thinks that our understanding of phenomena advances by repeatedly using the same patterns of argument in different representations, which shows us how to reduce the number of facts or assumptions that we have to accept as brute (1989, 432). Explanations are not evaluated separately, in isolation from one another, but by observing how they form part of a "systematic picture of nature." They are hence ranked according to their role in a broad systematic group of derivations supplied by a particular theory.

It is important to note that Kitcher makes an explicit connection between explanation in mathematics and the ways in which it functions in science (1989, 423). He claims that mathematical knowledge is similar to all other parts of scientific knowledge, and there is thus no reason for a methodological division between mathematics and the natural sciences, in particular with regard to the ways in which each is explanatory. This implies not only that this approach provides an account of explanation in mathematics as well as science (Kitcher 1989, 437), but also that the explanatory role of the mathematics used in scientific theories is evaluated using the same criterion of unification that is used to assess the explanatory strength of these theories. Hence, on Kitcher's account, the only difference between the explanatory contribution of mathematics to pure mathematics and that to representation in the sciences is the target domain in question: in mathematics, this domain is mathematical, such that no connection need be made or sought between the mathematical representation and a physical interpretation for it; in science, on the contrary, the domain being modelled is physical, and hence the mathematics needs to be suitably linked to it.

Let *K* be a set of (sentences expressing) beliefs that is consistent and deductively closed. Kitcher encourages us to think of *K* as a set of statements endorsed by some ideal scientific community at some point in time. A set of arguments that derives some sentences in *K* from others in it is a "systematisation" of *K* (Mancosu 2011). The explanatory store over K—*E*(*K*)—is the optimal systematisation of our set of beliefs *K*, i.e., it is the set of sentences representing our knowledge that uses the fewest assumptions, or sets of arguments, to derive the largest set of conclusions. *E(K)* consists of arguments acceptable as the basis for acts of explanation by those whose beliefs are constituted by *K*. For each *K*, *E(K)* is the set of arguments that best unifies *K*.

According to Kitcher, the manner in which the optimal explanatory set of arguments constituting our scientific knowledge, and hence the one that best unifies it, is determined is fairly complicated. At the same time, much of this formalism is intended to render the account tighter than the covering law model so that it does not fall prey to the same issues.[2] In general, a theory unifies our beliefs when it provides one (or a few) patterns of argument that can be used to derive a large number of sentences that we accept (1981, 514). The notion of argument pattern is crucial to explanatory unification, but requires a

[2] See, for instance, Scriven (1959).

few preliminary definitions. A *schematic sentence* is an expression obtained by replacing some, but not necessarily all, non-logical expressions in a sentence with dummy letters. There is a set of directions called *filling instructions* for replacing the dummy letters in a schematic sentence such that for each dummy letter, there is a direction that tells us how it should be replaced. A schematic argument then is a sequence of schematic sentences. The *classification* for a schematic argument consists of a set of sentences describing the inferential characteristics of a schematic argument, i.e., it specifies the sentences that should be considered premises, those that are to be inferred from others through derivation, the rules of inference to be used, and so on. A *general argument pattern* is a triple consisting of i) a schematic argument, ii) a set of sets of filling instructions, containing a set of instructions for each term of the schematic argument, and iii) a classification for the schematic argument.

Kitcher (1981, 517) uses an example of the popular formulation of Newton's second law of motion to explicate the idea of an argument pattern and its role in unification. In the *Principia*, Newton had shown how to obtain the motion of bodies from knowledge of the forces acting on them. The unifying power of Newton's work consisted in its demonstration that one pattern of argument could be used repeatedly to derive a wide range of accepted sentences. Consider a fusilier who wants to know why a gun attains maximum range when mounted at an angle of 45^{o} to the horizontal on a flat plane. Kitcher thinks that the following general pattern of argument by Newton to treat one-body systems can be used to answer this and a large variety of related questions through the following derivation:

(1) The force on *a* is *b*.

(2) The acceleration of *a* is *y*.

(3) Force = mass × acceleration.

(4) (Mass of *a*) × (*y*) = *b*.

(5) δ = ϑ.

For the above, the filling instructions stipulate that the instances of "*a"* should be replaced by an expression referring to the body under consideration, those of "*b*" by an algebraic expression referring to a function of the variable coordinates and time, "*y*" should be replaced with an expression representing the acceleration of the body as a function of its coordinates and their time derivatives, "ϑ" by an expression referring to the variable coordinates of the body, and "δ" should be replaced by an explicit function of time.. Hence, the sentences that instantiate (5) reveal the dependence of the variable coordinates on time, and so provide specifications of the positions of the body in question throughout its motion. The classification of the argument, defined above, tells us that (1)-(3) are premises, (4) is obtained from these by substitution, and (5) follows from (4) using algebraic manipulation and techniques of the calculus. It is thus the ability of the above argument pattern to allow us to represent a wide variety of physical systems and generate conclusions regarding them that renders it explanatory, insofar as it unifies. Analogously, the mathematical structure used above is explanatory because it can be repeatedly applied in varying situations to obtain a variety of conclusions (1989, 423).

Although he concedes that arguments instantiating the Newtonian pattern above do not have identical logical structure, Kitcher thinks that the classification does impose conditions that ensure "similarity" of logical structure and non-logical vocabulary among such arguments. This is important because Kitcher claims that scientists are interested in "stringent" patterns of arguments, which are patterns fairly similar in terms of their logical structure. Stringency is determined by conditions i) on the substitution of expressions for dummy letters, jointly imposed by the non-logical expressions in the pattern and the filling instructions, and those ii) on the logical structure imposed by the classification. A set of arguments is then said to be *acceptable relative to K* if and only if every argument in the set consists of a sequence of steps that accord with elementary valid rules of inferences, and the premises of each argument in the set belong to *K*.

In consonance with the epistemic flavour of his account, Kitcher takes seriously the process by which we arrive at an explanatory store *E(K)* of knowledge that constitutes the best systematisation of our beliefs. If Σ is a set of arguments, a *generating set* for Σ is a set of argument patterns Π such that each argument in Σ is an instantiation of some pattern in Π. In determining the explanatory store *E(K)*, we first narrow our choices to sets of arguments that are acceptable relative to *K*, and then consider, for each set of arguments, the various generating sets of patterns that are *complete* with respect to *K*. A generating set Π for argument set Σ is said to be complete with respect to *K* if and only if every argument that is acceptable relative to *K and* instantiates a pattern in Π belongs to Σ.

Of the generating sets available to us in our body of knowledge, we choose the one with the greatest unifying power and call it the *basis* B of the set of arguments in question:

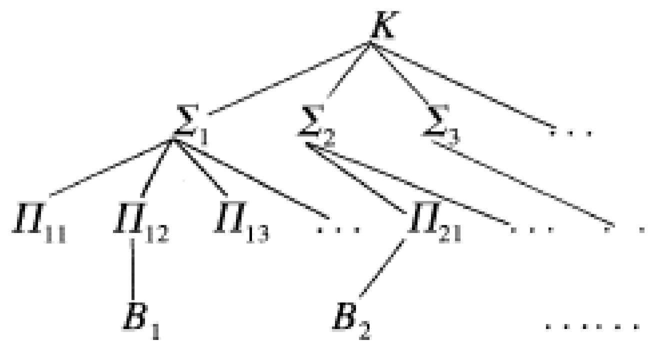

Figure 2.1. The derivation of basis sets on Kitcher's account of explanation (from Kitcher 1981, 520).

The unifying power of a basis set B with respect to K varies directly with the size of the set of conclusions derived from it, the stringency of the patterns belonging to it, and the similarity among these patterns. On the above picture, explanation in the form of unification yields understanding, since by using a few patterns of argument to derive many beliefs, we minimise the number of types of premises we must admit as underived (Kitcher 1981, 520). Kitcher thinks it important to look closely at scientific practice to determine the arguments favoured by scientists and attempt to understand the patterns common to them.

2.1.1 *Explanation as unification at work: Natural selection*

The recurring historical instance used in Kitcher's work to exemplify his account of explanation is Darwin's theory of natural selection in the 19^{th} century regarding the differential survival and reproduction of biological life. Darwin believed that natural selection should be accepted as the process by which species evolve because he thought that the "doctrine must sink or swim as it groups and explains phenomena" (Kitcher

1981, 514). However, he was unable to provide a complete derivation of any biological phenomenon. How, then, can natural selection be said to have any explanatory power?

Kitcher thinks that Darwin's evolutionary theory promised to unify a host of biological phenomena. The eventual unification proposed by his theory would consist in derivations of the descriptions of phenomena that would instantiate a common pattern. Kitcher supports this claim by observing that instead of providing detailed explanations for the presence of a particular trait in a particular species, Darwin provides a general pattern of argument that he claims can be instantiated *in principle* by a complete and rigorous derivation of the description of the characteristics that any species comes to exhibit over time. Such a derivation would employ the principle of natural selection *as well as* premises describing the relevant environment, the ancestral forms of the species under consideration, and the then-unknown laws of variation and inheritance. In other words, Darwin offers "explanation sketches" (Kitcher 1981, 515): by showing how a characteristic would be advantageous to a species, he indicates an explanation of the emergence of that characteristic in the species, hinting at an argument instantiating the general pattern. By using instantiations of this pattern, we can account for analogous variation in kindred species, the greater variability of specific characteristics, facts about their geographical distribution, and so on. Hence, Darwin though that his theory of natural selection should be accepted because it unifies and, thus, explains.

2.2 The divisiveness of explanation

Margaret Morrison (2000) has provided a fairly comprehensive critique of the identification of unification and explanation in the literature in philosophy of science, and explicitly considers Kitcher's account in the context of natural selection. Her general thesis is that there are many cases in the history of science where the demands for explanation and unification have militated in opposing directions, such that one has been rendered possible only at the expense of the other. Hence, explanation cannot be cast *solely* as unification, as Kitcher claims. Furthermore, a consideration of Morrison's argument against Kitcher reveals an explanatory role for mathematics that cannot be accommodated by the latter's proposal.

Morrison concedes that the most successful unification in biology, the synthesis between evolutionary theory and Mendelian genetics, was accomplished by using particular mathematical structures that enabled geneticists to combine natural selection and Mendelianism under a common framework (2000, 192). However, in spite of this theoretical unity, there was no unanimously accepted explanatory model for the ways in which selection operated within this new synthesis. In fact, the evolutionary synthesis only successfully afforded explanation by introducing *disunity* at the level of models used in the application of population genetics to biological phenomena. That is, a diversity of models was needed to account for the processes and effects encountered in this domain, rather than a uniform pattern of argument.

Kitcher claims that the argument pattern that he attributes to Darwin is implicit in his explanations of the prevalence of traits discussed in *On the Origin of Species* as well as other works. However, Morrison argues, although natural selection may serve to unify a wide range of phenomena on account of its applicability, it is not clear that it can also function as a source of explanation. Explanations based on derivation typically require a theoretical background to which one can appeal in order to understand how the phenomena in question came about. They are invariably situated in larger theoretical contexts that can in turn explain why the relevant derivations work. In order for natural selection to be explanatory in a diversity of areas, several additional assumptions needed to be added to the theory, many of which were not grounded in evidence at the time. For instance, selection is not the only mechanism operating in Darwin's evolutionary theory: there is the effect of use and disuse, spontaneous and directed variation (where the tendency to vary is transmitted rather than the actual variation), and so on. None of these involved selection, and could thus lead to maladaptive differentiation in local populations (Morrison 2000, 201). Hence, natural selection could only function in an explanatory manner in conjunction with specific assumptions, some of which lacked independent justification.

Darwin wanted to show how natural selection and its evolutionary effects could be used to solve a host of problems in geology, palaeontology, geographical distribution, morphology, embryology, etc. If we consider his discussion of geographical distribution and his explanation for the inhabitants of the archipelago, we find, in addition to the selectionist claim, the introduction of crucial assumptions regarding the methods of transportation across long distances. Darwin justified these methods by claiming that they should be *expected to occur* rather than simply considered possible (Morrison 2000, 206). For instance, seeds could be transported for miles over oceans if embedded in driftwood, and birds blown by gales across the water would serve as quite effective transporters of seeds. Thus, each domain in which selection operates makes use of a specific model that incorporates assumptions of varying kinds, like the one above, in order to explain. On Kitcher's criterion of the unity of a theoretical structure—where an argument pattern uses the fewest premises or assumptions to generate the largest set of conclusions—these additional assumptions—the very ones that render natural selection-based models truly explanatory—detract from this unity. Hence, in such cases, natural selection could be explanatory only if these assumptions were satisfied: while it was *necessary* for the sort of explanation provided by Darwin, *it was insufficient*. Thus, natural selection could only function in an explanatory manner in conjunction with certain assumptions, contrary to Kitcher's claim.

2.2.1 *How mathematical analogies may explain*

It is a good thing to have two ways of looking at a subject, and to admit that there are two ways of looking at it.

(Maxwell, "On Faraday's Lines of Force")

Pursuing the history of evolutionary theory, Morrison (2000, 218) shows that the synthesis between Mendelian genetics and Darwin's evolutionary theory was only made possible through the isolation of mathematical structures common to them in the work of Fisher (1918, 1922) and others. Fisher was interested in the problem of how a large number of factors might separately affect genetic variability, and whether this could be statistically represented. He wanted to show, contra Pearson (1903), that an exact specification of each factor relevant to a given population was unnecessary for a representative statistical analysis: when these factors are sufficiently numerous, the most general assumptions with respect to separate peculiarities leads to the same statistical result. Fisher saw this as analogous to the methodology employed in the theory of ideal gases, where only general statistical laws regarding interactions among particles were needed to describe and predict the behaviour of gases. By the early 20th century, the success of the kinetic theory of gases had shown that knowledge of the particular members of a population was not required to formulate representative, accurate, and general laws governing the behaviour of a population. Fisher thus treated biological populations as ideal gases. The benefit of this technique was that it provided the simplest account of a many-body system because it neglected as negligibly small the interactions among its members. This allowed Fisher to determine the role of natural selection in Mendelian populations by successfully isolating selection pressures from other relevant factors, such as migration, genetic recombination, and gene interaction, in order to render natural selection and Mendelian genetics compatible, and to show how the former operated in the latter. Tellingly, in his 1922 paper, Fisher used the law for the distribution of velocities of the particles of an ideal gas as the model for calculating the frequency ratios for different Mendelian populations.

Morrison sees two ways in which the work of Fisher undermines Kitcher's unificationist model of explanation. First, the mathematical analogy with ideal gases was the means by which Fisher was able to determine the effects of selection in isolation from other influences. This clarified the role of selection in genetic evolution in a manner considerably more informative and fruitful than the mere structural unification in the relevant argument pattern cited by Kitcher. Fisher was able to show how his assumption of a stochastic distribution yielded the conclusion that the action of natural selection on single genes, rather than mutation, random extinction, etc., was the primary determinant of evolution (Morrison 2000, 219). It is also important to note that in spite of the use of his analogy, Fisher did not propose or assume *any identity* between the actual mechanisms involved in the kinetic theory of gases and biological populations, which would be the sort of epistemic unification demanded by Kitcher's model. Second, it is not the case that Fisher used the analogy with ideal gases to justify his assumptions about biological populations; nor did he use it to explain empirical results that had been already available in hand. Instead, the mathematical analogy served as an instrument to *investigate* the role of selection in human populations by replacing actual populations with idealised ones. It should be clear that Kitcher's rather narrow conception of explanation based on patterns of argument is not susceptible to accommodating such an elaborate mathematical analogy. The methodological decision to isolate natural selection as an independent factor in variation was ultimately justified on empirical grounds. Thus, while natural selection can have explanatory power, this cannot be understood solely in terms of its unifying power. It follows that there appears to be a difference between the unifying role of natural selection and its function as an explanatory hypothesis. More generally, while a theory such as natural selection can be explanatory, its ability to explain cannot be understood only in terms of its unifying power. Unification and explanation come apart.

2.3 Reflections on Kitcher

One of the virtues of Kitcher's conception of science, and hence his account of explanation, is his explicitly *epistemic* approach to scientific knowledge. His proposed explanatory store of arguments represents the structured body of all our knowledge, and he encourages us to think of this as the product of the consensus of leading scientists in all areas. Similarly, he articulates criteria for the stringency of the argument patterns—in addition to formal demands for consistency, soundness, etc., that form part of the covering law model—as a determinant of their eligibility for our explanatory store in order that only patterns that are appropriately unifying are admitted to it. At the same time, the argument by Morrison above highlights the inadequacy of his unification-based account by showing that the relationship between unification and explanation does not endure across the body of our scientific knowledge, and that the two are often torn asunder.

More importantly, her account of Fisher's work on unifying genetics and natural selection, by using a mathematical analogy between biological populations and particles of an ideal gas, provides another avenue where the mathematics employed in scientific representation helps clarify and explain phenomena. The use of analogies between the mathematical representation of one physical system and another in fact occupies a distinguished place in the history and practice of science. Among the earliest and most influential uses of this can be found in the work of James Clerk Maxwell on electromagnetism. In his 1861 paper "On Faraday's lines of force," Maxwell explicitly considers a "physical analogy" between fluid flow and electromagnetic phenomena.[3] His purpose was to articulate Michael Faraday's lines of force conception, which described the direction and intensity of all forces in a homogeneous field about a charged particle. Maxwell was here concerned with providing a mathematical formulation of these lines of forces. The analogy that he drew to this end was between the intensity and direction of a line of force at a point, and the flow of an incompressible fluid through a fine tube of variable section. This yielded a vector representation of the lines of force in terms of the

[3] Maxwell (1965, 157) credits Lord Kelvin for first using this method to draw an analogy between heat and electrostatics, and light and the vibrations of an elastic medium.

velocity field of a fluid. Maxwell then applied this analogy to the phenomena of static electricity, galvanic current, permanent magnetism, magnetic induction, and electromagnetic induction: that is, he derived representations of almost all electromagnetic phenomena using the analogy with ideal fluid flow. This motivated his subsequent and more important physical analogies in his "On the physical lines of force" in 1862, where he found the correct field equations for electromagnetism and calculated the velocity of the transmission of electromagnetic interactions to be approximately equal to the speed of light.

It is important here to recognise the similarity between Maxwell's use of the mathematical analogy between electromagnetic phenomena and fluid flow and Fisher's groundbreaking work in relating natural selection with Mendelian genetics. Much as Fisher did not identify biological populations with the particles of an ideal gas, Maxwell does not assume an identity of any kind between fluid flow and electromagnetic phenomena. In fact, he repeatedly insists on the deceptiveness of this appearance and, at least in the first part of the 1861 paper, makes no claim about the physical nature of the phenomena being modelled. His explicit aim there is to provide a mathematical description of the lines of force conception, and he eschews the position of a "physical theory" to ground this mathematical description. One of the many important consequences of this work was the recognition that under certain restrictive conditions, the equations of ideal fluid flow are identical in form to those governing electromagnetic phenomena. In fact, this insight is still used to solve a number of problems in electrostatics to this day. I will have more to say about this in my treatment in §2.5 of the account of the application of mathematics proposed by Bueno and Colyvan. But for now, it is important to see how this method of mathematical analogy is unavailable to Kitcher on his model of explanation, and serves to rob his unificationist account of the comprehensiveness required of an all-encompassing theory. Since Kitcher sees scientific and mathematical contributions to explanation solely through the lens of unification, the argument patterns that he proposes to capture this contribution consist of an optimised formal rendition of our ever-growing body of scientific knowledge, where the optimisation consists in the generation of the largest set of conclusions using the smallest set of premises. However, as is clear from the above, insofar as epistemic and structural unification is not the only explanatory contribution of mathematics or science, Kitcher's model cannot capture these contributions on account of a structural myopia grounded in the major assumptions of his view of explanation. The major virtue that he espouses for his account of explanation—unification—becomes its major handicap.

With regard to the example of natural selection, Kitcher can reasonably claim that selection as proposed by Darwin is in fact explanatory. If accepted in conjunction with the other requisite assumptions highlighted by Morrison above, regarding the geographical distribution of species, their morphology, and so on, natural selection does provide an arguably satisfactory explanation, at least for its time, of the evolution and propagation of species. It shows how, given that natural selection is true[4] and the other assumptions are admitted, species evolve over time due to a wide variety of genetic, environmental, and other factors. Be that as it may, the point is that a subsequent, *significantly richer* explanation was afforded in Fisher's work by employing the

[4] I purposely use this word, in spite of my reservations against this as the sole aim of any physical theory, because this is required of the premises of arguments in Kitcher's model.

mathematical analogy described above. Using his model, Fisher was able to determine the independent effect of each of a multitude of environmental and genetic variables on the evolution of biological populations. Similarly, using his fluid flow analogy with electric and magnetic phenomena, Maxwell was able to provide a mathematical formulation for Faraday's theory that led to his equations of electromagnetism and yielded a useful methodology to conceptualise problems in a variety of disciplines for the future.

Another reason for why the avenue of mathematical analogy, employed to great success by these scientists, is simply not admissible to Kitcher's account is that his proposal emphasises patterns of argument, and is hence founded on the notion of derivation. The ultimate arbiter of the success of any explanation of such a theory—one that casts all explanations as arguments—is the soundness of the argument encapsulating a given explanation. In addition to satisfying a valid form, an argument of this sort is thus required to have *true* premises. However, as mentioned above, there is nothing ostensibly true about the mathematical analogies used by Fisher and Maxwell above, which were marshalled nonetheless to great explanatory and scientific success. We know that for Kitcher, the explanatory store of argument patterns that we can bring to bear upon a phenomenon in order to explain it comprises the beliefs of scientists and other epistemological experts in society; hence, all these patterns encapsulate truths about the world. That is to say, there is no room in these patterns, due to their stringency, for unsound arguments containing assumptions that are known to be false, such as mathematical analogies, that we know form a critical part of the so-called toolkit of an applied mathematician in solving problems in engineering. Kitcher's account betrays an emphasis on truth in the course of representation that thus impoverishes his account by denying it resources available to someone who privileges *the ability to represent over truth* in scientific investigation. And this is reflected in the limitedness of his view of explanation.

2.4 The inferential conception of applied mathematics

Otavio Bueno and Mark Colyvan (2011) have recently proposed an account of the application of mathematics to phenomena. The motivation underlying their "inferential conception of applied mathematics" is that embedding features of the empirical world into a mathematical structure allows us to draw inferences that would be otherwise difficult to obtain. Bueno and Colyvan frame their inferential account as an extension of and improvement on the "mapping account" of the application of mathematics proposed by Christopher Pincock (2004, 2007) and others.[5]

Pincock considers cases involving a mapping of some sort between a physical and a mathematical domain such that it yields an "abstract explanation," one that mainly relies on the structural features of the physical system in question (2007, 257). He considers as instance of this the problem of the famous bridges of the city of Königsberg. Some preliminary graph theory is first in order. A *graph* is an ordered pair consisting of vertices and edges. The *path* of a graph is a series of edges where one of the vertices in the n^{th} edge overlaps with one of these in the $n + 1^{th}$ edge. Connected graphs have a path between any two vertices. The number of edges on a vertex is called its *degree*. A

[5] They attribute variants of this approach to Baker (2003), Balaguer (1998), and Leng (2002).

connected graph G is said to be *Eulerian* just in case it is connected and contains a path, from an initial vertex *v*, that features each edge exactly once and ends at *v*.

Figure 1 shows a map of the seven bridges of Königsberg. The question is whether it is possible, starting at any given bridge, to traverse all bridges exactly once and return to the origin—if an Eulerian path is possible.

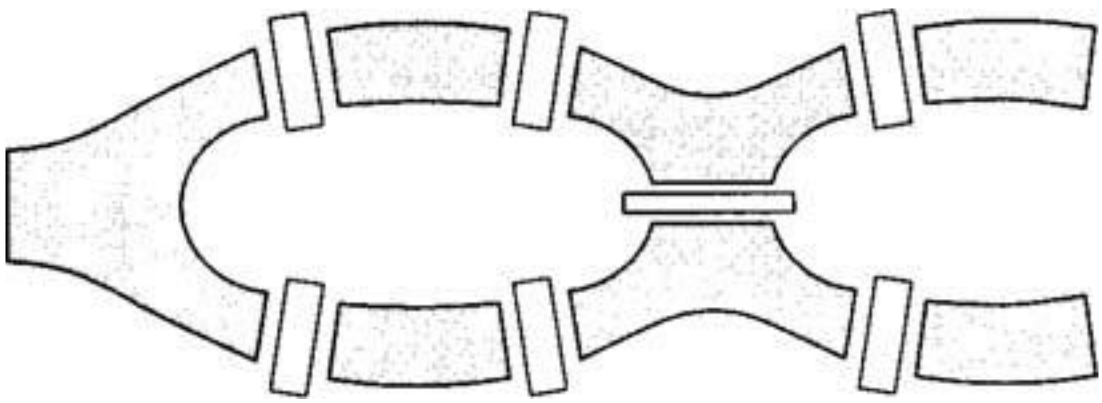

Figure 2.2. The bridges of Königsberg.

We can represent the bridges as a graph by abstracting from the details that are irrelevant to the problem at hand—the material constitution of the bridges, their elevation, the distance between any pairs of them, and so on—and by considering the bridges as edges, and the banks and islands as vertices. This yields the simple graph shown in Fig. 2. Now, we know that according to a theorem of graph theory, the existence of Eulerian circuits requires that all vertices of a graph have an even degree. As shown in Fig. 2, on the contrary, each of the vertices of the graph representing the bridges of Königsberg has an odd degree. This allows us to conclude that it is impossible for anyone to cross all bridges exactly once and return to the origin (Pincock 2007, 258). If someone were to ask how we can confirm, in particular without attempting traversals, that a Eulerian path is not possible, we would point to the isomorphism between the relevant features of the bridges and the graph to claim that the graph is sufficiently representative of the bridges to the extent required for our purposes. Having secured the consent of our interlocutor[6] to the isomorphism, we can then invoke the above theorem of graph theory and claim that this holds for the bridges as well, insofar as the mapping is representative. To clarify our reasoning, we can show other, Eulerian, graphs representing possible bridges where such a traversal is possible. This allows us to conclude that, as in our representative graph, the number of paths or bridges connecting any two vertices or parts of the city determines that a path of the kind we sought is not possible.

[6] I should point out here that Pincock and other proponents of the mapping account do not frame their proposals in such a context of accountability. I purposely use language here that evokes a view of explanation that requires that it somehow be confirmed as competent by an evaluator, preferably a lay one. My initial impression is that this is not mutually exclusive with the possibility of the development of formal measures to this end. The major inspiration for this idea is Gregory Vlastos's (1993) very creative work on the Socratic elenchus, and a central feature of the method of investigation advertised by Socrates in what are considered "early" Platonic dialogues: *Charmides, Crito, Euthydemus, Euthyphro, Gorgias, Hippias Major, Hippias Minor, Ion, Laches, Lysis,* and *Protagoras* (see Vlastos (1991)).

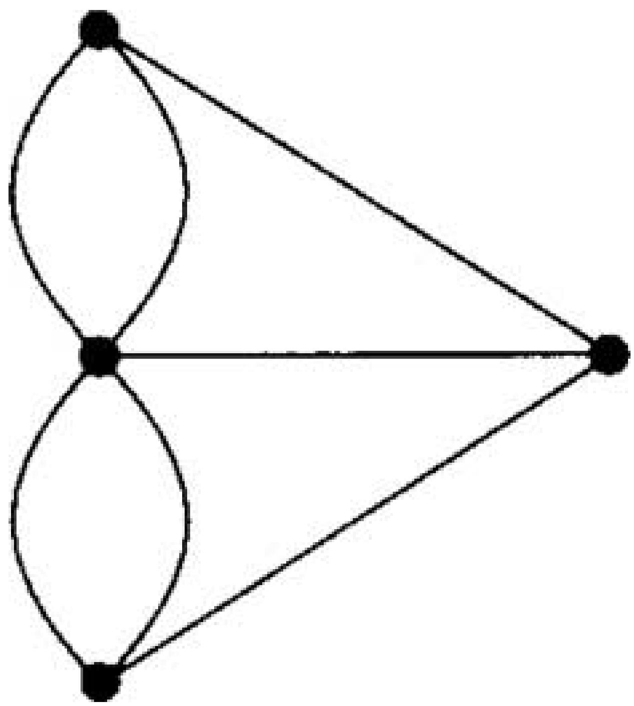

Figure 2.3. The bridges of Königsberg rendered as a simple graph.

This, then, is an instance of a static (time-invariant) system where an abstract explanation is afforded by the mathematics through a mapping from the phenomena at hand. It is important to note that this mapping captures only the features of the bridge system relevant to the problem being considered—whether an Eulerian traversal is possible—and ignores a host of other details—the constitution of the bridges, their microphysical properties, and so on—that do not contribute to the investigation. Hence, mathematics can be used to supply explanations of phenomena that are dependent solely on the abstract structural features of the system in question. Such representations have also been referred to as “acausal” (Pincock 2012, §3.3).[7]

[7] The reader should note here that in calling such representations “acausal,” I do not intend to suggest that I subscribe to any “causal” representations in the pre-theoretic, metaphysically repugnant sense of the

It is in the context of this mapping account of the application of mathematics that Bueno and Colyvan propose their inferential conception. They agree with Pincock's contention that a variety of mappings from the phenomena to representative mathematical structures are crucial to applied mathematics. However, they think the mapping account is incomplete because it does not say much about the kinds of mappings that can be effected between a physical system and a mathematical structure. Specifically, it cannot accommodate the fact that mathematical theories often have more structure than the target empirical setup in any given situation and, when suitably interpreted, some of this mathematical structure has empirical implications (Bueno and Colyvan 2011, 356).

To address this shortcoming in the mapping account, Bueno and Colyvan propose an account of applied mathematics that accommodates the central features of the application process, including the mapping of mathematical structures to a physical one (2011, 346). The crucial feature of this account is that it captures the important fact that the fundamental role of the application of mathematics to physical systems is inferential: by embedding certain features of the empirical world into a mathematical structure, it is possible to obtain inferences that would otherwise be extraordinarily difficult, if not impossible (Bueno and Colyvan 2011, 352). They claim that all roles of mathematics in science involve the ability to establish inferential relations between phenomena and mathematical structures, or among mathematical structures themselves. They are careful to point out that their account is not a purely structural account, since it makes room for

word. The term "acasual representation" seems to have stuck. So I use it only because this is what it has come to be known in the literature (See, for instance, Räz (2014)).

pragmatic and context-dependent features in applying mathematics to phenomena (Bueno and Colyvan 2011, 8).

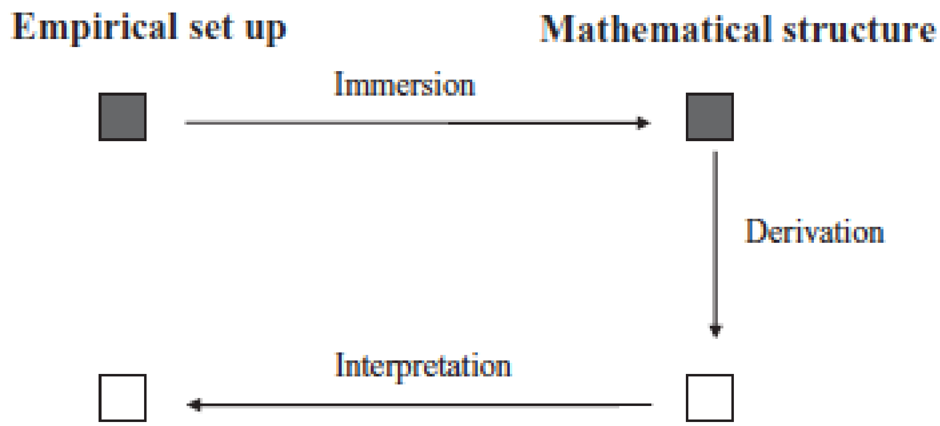

Figure 2.4. The inferential conception of applied mathematics (from Bueno and Colyvan (2011, 353)).

The inferential conception consists of the following three steps (Bueno and Colyvan 2011, 9):

1. *Immersion*. This involves a mapping from the physical setup to a convenient mathematical structure.[8] Bueno and Colyvan recognise that not all elements of a physical situation may be relevant to the purposes of the application in question, and hence stress that the mapping might omit certain structural features of the physical setup. Furthermore, the empirical setup is assumed to have structure, i.e., it either naturally has structure suited to the establishment of a mapping with a mathematical structure, or an appropriate one can be imposed on it. The latter may be a non-trivial exercise. The point of this step is to relate the relevant

[8] Bueno and Colyvan (2011, 347) offer a fairly inclusive definition of "structure" as a set of objects (nodes or propositions) with a set of relations on them.

aspects of the empirical situation with the appropriate mathematical context/structure.

2. *Derivation*. The mathematical structure obtained as a result of the mapping is then employed to derive consequences.
3. *Interpretation*. The mathematical consequences obtained in the derivation stage are then interpreted in terms of the initial empirical setup. A mapping from the mathematical structure (obtained in *Derivation*) to the initial empirical setup is needed to establish an interpretation. This need not simply be the inverse of the mapping used in *Immersion*.

Bueno and Colyvan emphasise that the above distinction between an empirical setup and a mathematical structure does not imply that the former is free of mathematics or other formalism. On the contrary, they claim that a representation of the empirical setup in practice will very often invoke a great deal of mathematics. The mappings are obtained by using partial homomorphisms between the relevant theoretical and mathematical structures, with partial isomorphisms between the theoretical[9] structures and those closer to the phenomena, down to structures directly representing appearances (Bueno and French 2012, 2). Partial structures are used in the setup in order to cater to the need for a formal structure that deals adequately with the openness and incompleteness of information involved in scientific practice.

Bueno and Colyvan cite several reasons for viewing their proposal as an improvement over the mapping account of Pincock. They think that the detail they provide enables

[9] "Theoretical" here is identical to the "physical" or "empirical" setup in the *Immersion* step. Presumably, the physical setup will, from the beginning, be an abstraction from the real world, will contain only relevant details, and may also incorporate theoretical entities, e.g., electrons.

their inferential conception of the application of mathematics to unify disparate theories, make novel prediction, and facilitate mathematical explanation.[10] With regard to the latter, they claim that the establishment of inferential relations between mathematical structures and the empirical setup is crucial to mathematical explanations of phenomena (2011, 366). It seems that the *Immersion* and *Interpretation* steps are important for mathematical explanation, since these steps involve what Bueno and Colyvan call "inferential relations." It is unclear how they intend for the inferential conception to be explanatory. It seems that on their account, a mathematically viable interpretation of the physical domain—in *Immersion*—and a physically coherent understanding of the mathematical results—presumably in *Interpretation*, once the derivations have been made—can contribute to an account of explanation of the phenomena that invokes the relevant mathematical structures. Thus, the view seems to be that the mappings will feature as necessary constituents in the formulation of an explanation of the phenomena.

In sum, Bueno and Colyvan think that their account enables the conceptualisation of two central issues in the application of mathematics to science: i) selecting appropriate mathematical structures to represent the empirical setup, and ii) assessing the representational and, perhaps, explanatory adequacy of these structures (2011, 356).

[10] I will not describe how they think their account accomplishes each of these because my criticism undermines the very applicability of their proposed structure to problems in applied scientific reasoning. If successful, such a strategy undermines any claim that they make regarding its fruitfulness for science.

2.5 The applicability of the inferential conception

I think it uncontroversial that there is some truth to the sort of mapping account described in the foregoing. If I ask you to divide 97 tomatoes among 10 people such that each person gets a whole number of tomatoes and the same number of tomatoes, you will not need to experiment with attempts to effect such a distribution and fail in order to conclude that this is impossible. In this and countless such instances, an isomorphism is assumed (typically not consciously, perhaps) between a certain set of numbers (the natural numbers from 1 to 97 in this case) and the target system in the physical world. Be that as it may, the account of the application of mathematics proposed by Bueno and Colyvan is far more ambitious than proposing to account for mental arithmetic. My treatment of it in the following will seek to emphasise the fact that this account of applied mathematics fails because it overlooks the basic details in scientific practice that enable frameworks of representation to model scenarios in the first place. Hence, the inferential conception as proposed by Bueno and Colyvan is in fact inapplicable to most such contexts.

Recently, Erik Curiel (2012) has convincingly argued that an adequate semantics for a physical theory must be based on notions of meaning that are determined prior to concerns regarding the accuracy with which the theory represents the dynamical behaviour of the physical systems that it treats. Following scientific practice, Curiel proposes distinguishing between the kinematical and dynamical components of a theory in its representation. Roughly, *kinematical* components are features of a system that are constant, or are assumed to be so for the sake of argument or analysis, as the system evolves over time, on pain of the system transforming into another in the representation.

On the other hand, *dynamical* components are quantities of the system that can vary with time and place. Kinematics comprises all that one needs to know in order to fix the kind of system in question (Curiel 2012, 9) and imposes constraints on the possible range of values of the relevant quantities in order to be able to provide a complete description of the system with regard to the representation at hand. For instance, there are several different physical systems that contain shear and stress, e.g., Nävier–Stokes fluids, electromagnetic fields, elastic solids, etc. To merely claim that a system has a shear-stress tensor is far from sufficient to characterise it in a manner that will allow a meaningful representation of it. One also needs to know whether the tensor is symmetric or divergence free, or if it stands in some relation to another quantity of the system, such as heat or flux (Curiel 2012, 10). These constitute part of the system's kinematical constraints.

Recall from §2.4 that the inferential conception of Bueno and Colyvan consists of three stages: a mapping is established between the target physical setup and a suitable mathematical structure in *Immersion*, the mathematics thus obtained is manipulated in the *Derivation* stage to obtain results, and these are then interpreted in terms of the original physical setup in the *Interpretation* phase. Hence, it is evident that Bueno and Colyvan propose reinterpreting the mathematical structure, which is derived by purely formal manipulation of the structure abstracted from the initial empirical setup, in terms of the physical setup once the necessary derivations have been made from it. Presumably, the underlying idea is that a reinterpretation of the formalism in question is provided once it has been used to derive consequences purporting to have physical significance. Such a view of representation is unfeasible as it is unrepresentative of scientific practice. This is because an interpretation of the formalism obtained by derivation requires, among other things, the *prior stipulation* of the types of systems and the sorts of conditions for which the system of equations (the formalism) at hand would be adequate. Without this having been done at the outset, before using the formalism to derive any consequences, it is impossible to know the sorts of derivations that are and are not reasonable to make in the context of the interpretation, since a given system of equations can be manipulated in any number of ways depending on the objective at hand. In such a case, it would not be possible to distinguish, without prior interpretation of the formalism, the parts of it with actual physical significance from those that have none in the context in question.

However, Bueno and Colyvan do recognize that the initial empirical or physical setup, from which the mathematical structure is obtained in *Immersion*, may itself be fairly formal and will very often involve “a great deal of mathematics” (2011, 354) that will presumably have been interpreted at that stage. Given that the consequences obtained in *Derivation* are subsequently reinterpreted in terms of this highly mathematised initial empirical setup, Bueno and Colyvan can claim that the requisite part of the formalism is in fact interpreted prior to derivation. And after all, the principle of charity requires that their proposal be viewed in its strongest viable manifestation.

Nonetheless, such a response is unsatisfactory. It is important to point out that Bueno and Colyvan market their inferential conception as an improvement over the mapping account proposed by Pincock et al. Their major criticism of this family of accounts, *and a feature that they claim is corrected in their proposal*, is that it is vague with regard to the details of the establishment of relations between phenomena and formal structures. Insofar as this is the case, there is by implication an onus on them to show how their account is detailed in *precisely* the ways in which they fault the proponents of the mapping account for being vague. Furthermore, the criticism made above regarding the inevitable role of interpretation in our derivations runs deeper. As Rizza (2013) has recently pointed out, while the corresponding mapping between an empirical structure and a mathematical structure can play an important role in representation, the applicative relevance of a mere mapping is secondary to and, more importantly, *contingent upon* the isolation of relevant formal properties of the empirical structure and their use as constraints on the mathematical structure corresponding to it. This is to say two things. First, the isolation and selection of salient formal properties characteristic of an empirical structure are tasks that are non-trivial. Insofar as Bueno and Colyvan do not say much more about the initial empirical setup in their account than that it might be highly mathematised, to conclude thus that this empirical setup accounts for the considerations offered above would be akin to largesse rather than charity. Second, such "kinematical" constraints cannot be defined in terms of mappings because the scientist is required to acquit himself/herself of this task before the "immersion" of the physical setup into a mathematical model. Since Bueno and Colyvan do not even gesture in general at interpretive work required to obtain a coherent empirical setup that is conducive to subsequent moves in their account, it is unreasonable to attribute to them a sophisticated view of this stage of representation generation. Thus, the inferential account must contain a specific articulation of detailed mapping-independent steps in order to determine the constraints on the system at hand, which in turn would enable it to represent physical systems in the first place. Otherwise, it is fair to claim that adherence to the inferential conception leads us to a situation, corresponding to the *Interpretation* step, where we have made a number of derivations that may or may not represent the target physical system because the quantities that need to be defined and the constraints that need to be imposed on their range of values have not been specified.

A related objection regarding the lack of interpretive detail in the inferential conception can be described as follows: Consider the equations of electrostatics with a dielectric:

$$E = -\nabla \cdot \tag{2.1}$$

$$\nabla \,.\, (\kappa \,.\, \nabla \phi) = -\rho_{\text{free}} / e_0 \tag{2.2}$$

where ϕ represents scalar potential. Equations of the same form have been found to represent and solve a litany of simple problems in various branches of physics, such as irrotational fluid flow, steady heat flow, certain problems in mechanical engineering, the diffusion of neurons, and certain areas of optics. In every problem scenario in each of these areas, the scientist is faced with an empirical situation that needs to be modelled. The outcome is a set of equations representing the physical situation at hand. This presumably occurs in *Immersion* on Bueno and Colyvan's model. Bracketing the above objection regarding the extent of interpretation required at this stage in the face of Bueno and Colyvan's disregard of it, the scientist is then required to derive this form from the set of equations generated in *Immersion*. The important mathematical similarity among the solutions of these systems is the satisfaction of Laplace's equation $\nabla^2 \,.\, \phi = 0$. This would occur in the *Derivation* phase of the inferential conception. The result would be a set of equations in the same form as Eqs. (2.1) and (2.2) that solves the system in question. However, in the course of manipulating the equations of the system generated in *Immersion* to obtain the harmonic functions that would solve it, the scientist frequently needs to refer to the interpreted physical scenario *during Derivation.* This is to ensure that he/she makes mathematical inferences that continue to be representative of the situation at hand—say, a case of irrotational fluid flow—while looking to reduce the equations to harmonic functions in order to render them solvable. However, Bueno and Colyvan's model does not accommodate this alternation between *Derivation* and the empirical situation (or *Immersion*, depending upon how structured the consequence of this phase is). Insofar as this alternation is necessary to solve all but the simplest problems in applied mathematics, the inferential conception appears to be unsuited to accurately account for the application of mathematics because it misses a structural detail of the process that is crucial to application. This disregard of scientific practice manifests itself as a general failure, by many in the philosophical literature, to appreciate the interplay between theory and experimentation in Chapter 3, when I present and defend a Carnapian conception of mathematical entities in representational systems in science.

2.6 Conclusions

The preceding sections reveal an error common to Kitcher's unificationist model of explanation as well as Bueno and Colyvan's inferential conception: a failure to account for a number of contributions of mathematics to representation due to structural deficiencies in their proposals. By "structural deficiencies," I simply mean that the frameworks for representation proposed in both these accounts are too restrictive to accommodate a number of explanatory contributions as seen in historical as well as contemporary instances of the application of mathematics. In the case of Kitcher, his proposed argument patterns to formally represent our scientific knowledge are designed to admit only instances where scientific inquiry, and the relevant mathematics, appears to unify disparate phenomena. As a consequence, we saw in §2.2 and §2.3 how this view cannot accommodate Fisher's seminal mathematical work that synthesised genetics and natural selection by using the familiar technique of making mathematical analogies between diverse physical systems, or Maxwell's use of the analogy with ideal fluid flow to model electromagnetic phenomena. Structural deficiency is even more pronounced in Bueno and Colyvan's proposal, which is simply incorrect in its assumptions about how physical systems are modelled. As I have argued in §2.5, this is because it completely ignores the interpretive work needed to establish an empirical setup that is capable of even representing, let alone accurately modelling, the target system.

The common error of structural shortcomings in the face of practical instances of mathematical representation suggests that any philosophical account purporting to explicate the various contributions of mathematics to science ought to at least faithfully reflect the structure of our scientific theories. Specifically, it should explicate the relationships among the theoretical/mathematical apparatus that it employs and outline clear procedures for their interpretation. Furthermore, it should take cognisance of the ways in which theory is related to experimental procedures for its verification, as well as the manner in which it is used to solve real-world problems. Such a framework would be more likely to accurately capture the details of scientific reasoning of the sort that are overlooked by the accounts examined in this chapter. This observation prompts question *B* posed in §1.1— *Is there a promising philosophical account available to represent the theoretical/mathematical entities employed in our scientific theories in order to help clarify and explain their role?* The linguistic frameworks for the reconstruction of scientific theories proposed by Carnap appear to satisfy this requirement. This is because as Stein (1992, 1994) has pointed out, these frameworks are modelled after the highly mathematised structure of canonical theories in physics, with a clear distinction between theoretical and observational vocabularies, the relationship between which is mediated by rules of correspondence. Hence, in Chapter 3, I propose and defend frameworks for the representation of theoretical entities in science that are based on Carnap's proposal, but deviate from it in detail.

3 Carnapian frameworks for mathematical entities

My examination of the accounts of mathematical explanation in Chapter 2 was intended to highlight some of the contributions of mathematics to scientific representation. In addition to discharging this burden, my inquiry revealed that Kitcher's unificationist account and the inferential conception of Bueno and Colyvan fail to accommodate many explanatory contributions of mathematics to science because of varying structural limitations and shortcomings. The upshot was that a scheme that can competently reflect the role of mathematics in representation ought to be amenable to the mathematical structure of theories in physics. This prompts the second of the research questions for this dissertation stated in §1.1— *Is there a promising philosophical account available to represent the theoretical/mathematical entities employed in our scientific theories in order to help clarify and explain their role?* In response to this challenge, I argue in this chapter that a modified form of the linguistic frameworks for the philosophical analysis of theoretical entities proposed by Carnap is competent to represent mathematical entities in just this role. The modifications I propose to such frameworks—what renders my proposal *Carnapian*—are three:

i) The position of a semantic view for the representation of theories, whereby a theory is taken to be a family of models rather than a set of sentences, as in the syntactic view of Carnap (§3.2).

ii) The proposal and pursuit of a methodology based on a careful, detailed "bottom-up" consideration of instances of the use of theory in representing target systems, in contrast to the traditional philosophical approach based on a priori concerns or toy examples (§3.2 and §3.6).

iii) A consideration of the practical complexity of relating theory to experimental data (§3.7).

I outline in §3.1 the major commitments shared by my proposed Carnapian view with Carnap's own account of linguistic frameworks. In §3.2, I attend to some preliminary considerations and explain the first two of my amendments to Carnap's framework for theoretical entities. Section 3.3 features a detailed consideration of criticisms of Carnap's account of theoretical entities in his reconstructive frameworks. The general strategy pursued in all these is to show that Carnap's approach leads to consequences for scientific theories that are counter-intuitive or outright false, is unrepresentative of scientific practice, and hence must be rejected. While I consider and address objections from a number of thinkers, my discussion centres on criticisms made by Pincock, since these satisfactorily encapsulate the concerns expressed by the others and explicitly engage the question of whether Carnapian frameworks can capture scientific practice. In order to better understand the context of the concerns and the nature of a possible response in the spirit of Carnap, I will briefly summarise in §3.4 his *mature view* of the theoretical language in his framework. It should be noted here that my amendments to Carnap above are consistent with this view. Given the strategy underlying the criticisms of Carnap, it would be argumentatively effective and efficient to consider an instance of the application of a Carnapian framework to a physical theory. I will thus provide in §3.5 an example from the history of science—Maxwell's equations of electromagnetism—and consider it in the backdrop of Carnapian frameworks in §3.6 to show that these concerns are unjustified. My responses to these concerns reveal insights into how Carnapian frameworks are representative of scientific practice. They also unearth the third way, stated above, in which my approach to frameworks for scientific reconstruction diverges from that of Carnap. I briefly sketch this in §3.7.

3.1 Major points of agreement with Carnap

Carnap has provided the most comprehensive and systematic programme in philosophy for the rational reconstruction of science. The aim of his enterprise is to articulate scientific theories in a manner that reveals hidden assumptions by clarifying the multiplicity of relations involved in their construction and regimentation. To my mind, this accounts offers considerable promise for the use of philosophy as both a servant and critic of scientific theorising and practice. Hence, I will dedicate the remainder of this chapter to clarifying and defending an account of the role of theoretical entities in science in the *spirit* of Carnap's enterprise in general, and largely in accord with his mature view (1934, 1956, 1966) of this issue in particular. I write "theoretical entities" instead of "mathematics" because the debate in the philosophical literature has been carried out in these terms. However, it should be clear that insofar this dissertation has emphasised theories in physics, and since theoretical entities in our physical theories are rendered in the language of mathematics, an investigation of the role of such entities should be considered part of an inquiry into the mathematics employed in the relevant theories.

It is pertinent at this point to briefly highlight the major tenets of Carnap's programme that are retained in my Carnapian approach, over and above my subscription to his mature view of theoretical entities in linguistic frameworks detailed in §3.4. This is particularly relevant because each of the following subscriptions plays an important role in the arguments that I mount in this and the next chapter, either as assumptions (as with the analytic–synthetic distinction in §4.4) or as commitments that are defended against criticisms in the literature (as with the criterion of cognitive significance in §3.3). First, my Carnapian conception endorses its eponym's distinction between analytic and synthetic propositions in the context of formal frameworks for science. There are many formulations of this distinction in the philosophical literature. With regard to frameworks for scientific theories, the distinction translates into one between propositions that can be verified or falsified without reference to empirical data and those that cannot. This is in contrast to Quine's (1951) famous rejection of this division, and his view that no distinction in kind can be made between the a priori and the a posteriori in our formalised epistemology. As we shall see in §4.4, it is adherence to the analytic–synthetic distinction that permits Carnap to pose and answer questions concerning the formal/mathematical components of reconstructed theories as internal to his frameworks, and to reject ontological questions concerning theoretical entities as questions without content that are external to them. Second, the Carnapian view remains faithful to Carnap's criterion of cognitive significance as formulated in his later works, and described in §3.4. Roughly, this holds that a proposition is meaningful just in case it is in principle verifiable or falsifiable. In light of the analytic–synthetic distinction endorsed above, the criterion of cognitive significance provides two kinds of mechanisms for the verification of propositions within a framework: formal proof in the case of purely logical and mathematical propositions, and empirical verification in the case of synthetic propositions. I defend this criterion against influential criticisms in the literature in §3.5

and §3.6 by using Maxwell's equations of electromagnetism as an instance. Furthermore, in §4.4, I use the distinction between these two methods of verification to address concerns shared by Demopoulos and Maddy regarding the viability of Carnap's attitude to ontological questions regarding theoretical entities in linguistic frameworks. The third such notable commitment of the Carnapian conception of theoretical entities is harmonious with a revision by Carnap to his early work. Long before he espoused his mature view of theoretical terms in linguistic frameworks, Carnap had abandoned his ambition for a *single unified language* for all of science, and had begun exploring alternative frameworks for the representation of the structure of *particular theories*.[1] In the backdrop of his view of philosophy as the logic of science, this stance allows me to propose and defend in the remainder of this chapter Carnapian frameworks for the analysis of theoretical entities in a manner useful to science and philosophy. The usefulness of this perspective is exemplified by my use of Carnapian frameworks to represent Maxwell's work.

The final significant debt that my Carnapian view owes to Carnap is also the most tentative. It is the claim that the aim of such frameworks is the reconstruction of scientific theories. For Carnap, the primary task of philosophy is to make explicit the various assumptions made by scientists in formulating theories in order to clarify the methodological and epistemological commitments entailed by them. Hence, as frameworks of reconstruction, Carnapian systems are not intended for use by practicing scientists in the so-called context of discovery, but to be employed by philosophers

[1] This assumes the form of his "Principle of Tolerance" in *The Logical Syntax of Language*, whereby one is free to use the framework of one's choice for the representation of theories so long as the rules invoked and the methods used are clearly articulated (Carnap 1937, 52).

following the establishment and acceptance of theories in order to analyse them. Scientists do not know of or care about Carnapian frameworks, and certainly do not use them. Nonetheless, I have said that my subscription to this component of Carnap's programme is tentative. By this, I mean that in the context of my argument in this chapter, and my project in this dissertation, I continue to subscribe to the thesis that Carnapian frameworks are fundamentally reconstructive in their intended application and should not be burdened with the onus of being attractive or useful to scientists engaged in cutting-edge research.[2] At the same time, beyond the scope and aims of this dissertation, I do not see a principled reason for restricting Carnapian frameworks solely to reconstructive enterprises. It appears that scientists in many fields already use some implicit form of proto-Carnapian frameworks in their research. But over and above this cursory observation, I see three ways in which some scientists would benefit from the use of a more explicit framework along the lines developed by Carnap in their work.[3]

i. In the case of mathematical physics, scientists working on algebraic quantum field theory, for instance, or those engaged in more or less strictly mathematical problems, such as characterising formal properties of initial-value problems for a set of equations, such as the Nävier–Stokes equation or the Einstein field equations, already have available to them an explicit and purely formal system much like the Carnapian frameworks being recommended here. Since, the physical interpretation in the case of some fields

[2] In fact, I use this stance in my preliminary response to Maddy's criticism of Carnap's view of the ontological status of theoretical entities in §4.4.

[3] I am grateful to Erik Curiel for making this suggestion.

is unclear, the formal framework is all that they have to ensure that they remain within the bounds of the theory.

ii. In the presence of a well worked out and acceptable interpretation of a theory, such as General Relativity, physicists can use Carnapian frameworks to guide their search for novel and interesting results as well as to confirm the potential meaningfulness of results that have already been established.

iii. In case a theoretical or experimental scientist wants to construct rigorous theoretical models of experiments or families of types of possible experiments, he/she will need more or less explicit Carnapian frameworks that contain clear physical interpretations of the theoretical terms invoked by the relevant theory.

Such a possible extension of Carnapian frameworks for use by practicing scientists is a subject that I intend to pursue in subsequent research. As stated above, however, for the purpose of this dissertation, I will consider reconstruction as the proper setting for the deployment of Carnapian frameworks.

3.2 Preliminary considerations, and two amendments to Carnap

While I am sympathetic to Carnap's reconstructive enterprise and agree with much of what he says in relation to it, there is an important way in which my approach here is a departure from his. Carnap advocated what has come to be known as a "syntactical view" of the representation of theories in his reconstruction, whereby these are to be expressed entirely in the language of logic. As Suppes (2002) points out, this narrow restriction on the form of expression of the theory makes its expression unduly laborious. Consider

Euclidean geometry. If we want to define a line, say, as a set of points, this requires that the concepts of set theory be readily expressible in the language of our framework, which is a decidedly painstaking task. The concern is amplified when we consider theories involving more complicated mathematical structures, such as general relativity or statistical mechanics, that would require the rendition in a logical language of quite complicated mathematics, such as results from spectral theory, symplectic theory, and so on. It is perhaps a testament to the complexity and the superfluity of the task of the *linguistic* reconstruction of theories that no substantive instance of the representation of a theory as a logical calculus seems to have been provided in the literature.

Contrasted to the syntactic approach summarised above is the so-called "semantic view" of theories,[4] which encourages the conception of a theory as a family of models rather than a set of sentences. A model is an abstract, non-linguistic entity (Suppes 2002, 3) that occurs naturally in the context of a scientific theory. For instance, the measurement of the predicted consequences of a theory, given an experimental procedure and accordingly conducive datasets, involves a representation theorem that establishes an isomorphism between numerical models of the theory and the experimentally obtained datasets.[5] This allows us to use familiar computational methods, which we know to be nicely applicable to the numerical model, to the experimentally determined sets of observations. Needless to say, such a task would be incredibly tedious in linguistic

[4] See, for instance, van Fraassen (1980), Suppe (1989), and Suppes (2002) for different versions of this view.

[5] This is a bit quick and dirty. The datasets themselves are not the raw results of measurement. The measurements are typically subjected at least to data reduction and curve fitting.

representation. Hence, I will adopt this semantic view in the backdrop of Carnap's account of theoretical entities in his reconstructive frameworks.

Bas van Fraassen (1980, Ch. 3) embraces the semantic view in the course of a defence of his "Constructive Empiricism." In addition to the variation in the nature and breadth of our respective concerns, my view here is different from his for two main reasons. First, in spite of his commitment to empiricism, van Fraassen's reservations against Carnap's project are quite thoroughgoing;[6] on the contrary, I think it pertinent to label my approach *Carnapian* due to my subscription to the spirit of his project as well as much of its mature detail. Furthermore, in spite of his empiricist leanings, van Fraassen is fundamentally uninterested in a reconstructive project of the sort proposed by Carnap and favoured by me.

It is also worthwhile to draw attention to the methodological approach that I adopt here, primarily to contrast it with strategies pursued in classic critiques of logical empiricism in general and Carnap in particular. These have tended to focus solely on general philosophical considerations (Quine 1951), objections relating to formalism,[7] or arguments from common-sense analogies and toy examples (Suppe 1977), almost to the exclusion of an investigation of instances of the articulation and application of theories in science. This can be considered a "top-down" approach of sorts to the analysis and critique of philosophical theses regarding science. While there is nothing objectionable about this approach *per se*, its overemphasis in the course of the evaluation of proposals regarding approaches to scientific theories leads to analyses that are limited in their

[6] For instance, he rejects the logical empiricist criterion of cognitive significance (1980, §2.7).
[7] Admittedly, this is shaped in large part by Carnap's own focus in his programme.

vision and often erroneous as a consequence because they abstract from the details of these approaches. We will witness instances of these in my discussion of the criticisms of a Carnapian approach to theoretical entities in §3.4.

In contrast, the methodology that I suggest through my critique of the accounts of mathematical explanation in Chapter 2 consists of a careful, “bottom-up” consideration of instances of the use of theory in representing target systems in the service of solutions to problems. The idea was to compare the general accounts of the explanatory role of mathematics in representation with practice in order to highlight their limitations. This approach has so far been fruitful in revealing how a consideration of the details of scientific theorising and practice may be critical to the success of a putative all-encompassing account of this kind. To this end, I analyse Maxwell’s system of equations of electromagnetism using the Carnapian framework that I propose. This helps illuminate the ways in which top-down criticisms of such frameworks are misguided because they ignore the detail afforded by a bottom-up approach, one that seeks to compare general theory with practice in order to make the former better informed and more reflective of the latter. It also helps exemplify the second, methodological, way in which my Carnapian proposal differs from Carnap’s original conception of frameworks.

3.3 Criticisms of theoretical entities in Carnap's frameworks[8]

In his recent book, Pincock (2012) investigates a variety of ways in which mathematics contributes to science. One of these is related to its role in the formulation of "constitutive representations." While he does not provide a strict definition of the term, the general idea is that these constitute the general assumptions underlying other, derived representations that are specific to the physical system at hand.[9] He provides the following definition of what he calls a "derivative representation": "A representation r_1 is derivative when its success depends on the success of another, *constitutive* representation r_2" (2012, 121). Pincock concedes that the notion of a derivative (constitutive) representation posed by this definition is relative, in that a representation r_1 can be constitutive with respect to another representation r_2, but can also be derivative with regard to yet another representation, r_3.

Pincock employs the following general strategy to argue against Carnap's account of theoretical entities: to show that the view regarding the *meaning* of scientific terms and propositions that appears to follow from his proposal is flawed. According to Pincock's reading, rules of a framework for Carnap are ones for the proper use of signs that form the language of the framework in question, and exhaust its constitutive representations (2012, 124). The success of derivative representations is related to beliefs derived from the rules of the framework and/or other beliefs adopted on the basis of experience. The

[8] The reader unfamiliar with Carnap might struggle with this section because my description of criticisms of his view inevitably involves, albeit minimally, the use of some vocabulary particular to his frameworks. This difficulty can be obviated by first reading §3.4, where I introduce the required apparatus to address these concerns.

[9] Strictly speaking, there is no requirement for a constitutive representation to represent a physical system. However, that is their purpose in the context of Pincock's study and coheres well with his overarching aim: to provide an account of the contribution of mathematics to the success of science.

discussion of a "new domain" requires the introduction of new signs, according to Carnap. The use of these signs must be governed by rules along with their specification in order for them to be meaningful. These rules would include at least some specification of how existence questions within the framework can be resolved for the entities referred to by corresponding signs. Pincock sees Carnap as proposing the following test to evaluate the success of constitutive representations: Does adopting a framework contribute effectively to our stated goals (2012, 125)? If it does, the constitutive representation in question is accepted as justified. For instance, the fluid flow framework is founded on a set of rules that define the mathematical vocabulary employed. This allows the definition of further (physical) terms by reference to the logical and mathematical terms, which in turn make possible truth-functional claims involving mathematical and physical components related to the framework at hand. For instance, the Nävier–Stokes equations can then be formulated and determined to be true or false based on empirical testing.

From this reconstruction of his view, Pincock observes that Carnap's argument is grounded in two assumptions: "(1) the meaning of a new term is given by rules for its proper use, and (2) the rules will relate, at least in part, to how sentences using that term can be supported." Pincock claims that both these assumptions are incorrect. Against (1), he invokes semantic externalism about the meaning of scientific terms, whereby the meaning of a term—including novel terms—invoked in science (and presumably referring to the system to be represented) is based on some kind of causal or other interaction between the agent and something in the world. Thus, Pincock argues, the meaning of a new term is *not* exhausted by the set of rules of its use outlined by the speaker, nor by the possible inferential rules that could be derived from such rules.

Consider the term "fluid." On Carnap's understanding, if two linguistic frameworks contain different rules for the use of the term "fluid," the meaning of the term changes across the two frameworks. There seems to be something wrong about this, for we take the term "fluid" to refer to the same thing regardless of the successively sophisticated theories of fluids in the development of physics over the course of history. Furthermore, a framework can only be critically examined with regard to the objectives of the representation through pragmatic evaluation. There is no constant, framework-independent subject matter on Carnap's view that can be assumed in order to evaluate scientific progress (2012, 126). In a similar vein as Pincock, Glymour (1980, 61) attributes to Carnap the view that the rules of correspondence in his frameworks are stipulated, their truth is guaranteed by virtue of their meaning alone—they are analytic—and *they are never tested*. Against this, he claims that not only are rules of correspondence subject to empirical verification, they are also susceptible to rejection or falsification.

In his opposition to (2) above, Pincock claims that the rules for a framework need not be involved in specifying how derivative claims can be supported, but might simply reflect the features of the objects being referred to by the terms in question. That is to say that the mere specification of rules does not guarantee reference to any object, but that a new term must correspond to some feature of the world in order to refer. Pincock claims that Carnap would reject this link between rules and reference as traditional metaphysics, but that the latter's own picture "makes meaning too easy to achieve and clashes with the way that mathematicians and scientists go about deciding when their words have successfully picked out something in the world" (2012, 126).

Another popular line of objection to Carnap's mature view involves formal manipulation of his criterion of the cognitive significance of theoretical entities to show that it leads to unacceptable consequences. Versions of this objection have been posed by Rozeboom (1960) and Kaplan (1975), and have been endorsed by Glymour (1980) and Creath (1976).[10] The criticism has been framed and argued in the context of Carnap's logical definition of cognitive significance (1956) and involves a fair bit of formalism.[11] I omit a formal statement of the criterion in question as well as the details of the formal manipulations involved in generating the objectionable consequences because this does not mitigate the force of the complaint; nor does my Carnapian response to it turn on the logical formalism used. Hence, a description of the objection in formal terms might, if anything, distract the reader from the issue that it seeks to bring to the fore.

The idea is that in the context of Carnap's frameworks, extending a theory by adding theoretical postulates or rules of coordination may cause some theoretical entities to lose significance (Rozeboom 1960, 37). Similarly, Kaplan (1975) claims that such an addition may cause hitherto insignificant theoretical entities to become meaningful. The former consequence is counter-intuitive because assuming that the additional rules or postulates are consistent with the existing content of the theory, this ought not to affect the significance of a theoretical term that draws its meaning from the original rules and postulates that remain part of the theory. The latter consequence is analogously untenable

[10] Creath proposes a modification of the criterion of cognitive significance in defence of Carnap.

[11] The interested reader can find it in (Carnap 1956, 49). Roughly, a term of the theoretical vocabulary is said to be cognitively or empirically significant when a certain assumption involving the physical magnitude that it designates makes a difference in the prediction of an observable event.

because "definitional extensions"[12] of the sort above are typically considered to add no empirical content to the original theory (Kaplan 1975, 90).[13] It is germane to note that neither Kaplan nor Rozeboom provides a positive, alternative account of significance in the context of his criticism. The argumentative strategy employed by both seeks to reduce to absurdity Carnap's formal criterion of significance for theoretical entities by showing that it yields the above consequences, which are *intuitively unacceptable*. It is further notable that neither Kaplan nor Rozeboom provides a formulation of even this intuitive notion in the course of his criticism. In all fairness, this is not needed for the success of the negative cases posed by them, so long as their readers acquiesce to the argument whereby the implications of Carnap's criterion of significance *ought not* to follow from any cogent criterion of this kind, no matter what it might precisely be.

From the above, we can identify five related reservations against Carnap's account of theoretical entities in his frameworks. First, the meaning of a novel term introduced in a constitutive representation cannot be exhausted by the mere specification of general, theoretical rules because it leads to incommensurability in reference to, ostensibly, the same entity across different theoretical frameworks. Second, there is no framework-independent method in Carnap's conception that can be used to assess scientific progress.[14] Third, a framework need not specify how propositions using a novel term can

[12] If one were being a stickler for accuracy, one would point out that Carnap in fact does not think that theoretical entities in his framework can be defined. He thinks that they admit an indefinite number of "descriptions," which may be thought of as rules of coordination, each of which provides a different method of measurement (1966, 234-236).

[13] Kaplan (1970, xlvi-xlvii) reports that in a meeting, Carnap agreed with his criticism and concluded that Hempel, Quine, and others were correct in claiming that theories must be accepted or rejected wholesale.

[14] This objection is implicitly addressed in Chapter 4.

be verified, but may simply reflect the features of the object designated by a term. Fourth, the addition of theoretical postulates or rules of correspondence to a framework can cause theoretical entities to gain or lose meaning, which is intuitively unacceptable given our knowledge of scientific theories. Lastly, Carnap's view of frameworks is simplistic, and thus does not accurately reflect scientific practice.

The last of these criticisms can be considered to be the crux of the reservations against Carnap's proposal, since each of the other four, when articulated at length, ultimately relies on the dissonance between Carnap's account of representational systems and the manner in which scientists actually reason about such systems.

3.4 Carnap on theoretical terms

Carnap's mature reflections on the status and function of theoretical terms in his linguistic frameworks can be found in "The methodological character of theoretical concepts" (1956, 38-75). For these frameworks, the language of science is divided into a theoretical and an observational part. The theoretical language L_T consists of logical and descriptive constants, the latter of which form the theoretical vocabulary V_T of the language (1956, 42). A theory according to Carnap consists of a finite number of postulates T in L_T, where these postulates correspond to axioms or constitutive representations on Pincock's conception. The theoretical language and the observational language L_O are connected through rules of correspondence, or C-rules (1956, 39), which only provide a *partial, indirect interpretation* of the theoretical terms of V_T. This means that only some terms of the theoretical vocabulary are directly connected to observational terms through the correspondence rules, and the remaining theoretical terms are connected to the theoretical terms mentioned first through the postulates of L_T, and are hence indirectly connected to the observational language.

It is important to note that according to Carnap, L_T, consisting of postulates T and the rules of deduction of the chosen logical system, is an *uninterpreted calculus* prior to the specification of the C-rules in the language (1956, 46). All interpretation that can be accorded to L_T is by virtue of its relation with the observational language L_O through the rules of correspondence. These rules permit the derivation of certain sentences of L_O from those of L_T, or vice versa.[15] The C-rules indirectly derive conclusions in L_O, such as the prediction of observable events. Thus, without the rules of correspondence, terms of V_T would have no observational significance (1956, 47).

An instance of the use of C-rules in the framework is to connect a location in physical space with corresponding space-time coordinates *x*, *y*, *z*, *t*. The C-rule R, say, relates to an observable space-time region, say u, through a class of coordinate quadruples of intervals about (*x*, *y*, *z*, *t*). Theoretical quantities such as mass, length, volume, velocity, etc., are assigned interpretations after a similar fashion.[16]

A more involved example of a C-rule is the definition of "kinetic energy" for Newtonian particles: "measure the inertial mass of the particle; measure the velocity of the particle; its kinetic energy is one-half times the mass times the square of the velocity; it follows that the concept of kinetic energy requires the fixation of a frame of reference

[15] In this formulation, Carnap hints at the dialectic between theory and experimentation, the neglect of which by large parts of the contemporary philosophical community has led to inaccurate criticisms of Carnap. I discuss this in §3.6.

[16] Even though general, this is admittedly too simplistic an account of how theoretical entities are related to phenomena and hence rendered significant. See my remarks in §3.7 for a brief discussion of how the Carnapian programme can be extended to provide detail here.

for the representation of its value, insofar as velocity itself is not defined outside the context of a fixed frame of reference."[17] Note that in this instance, kinetic energy is a theoretical term defined using other theoretical terms—inertial mass and velocity—that are in turn defined through C-rules.[18]

In light of the above, Carnap thinks that a criterion of significance or meaningfulness for L_T should constitute exact conditions that terms and sentences of the theoretical language must fulfil in order to play a positive role in the explanation and prediction of observable events and, thus, to be accepted as empirically meaningful (1956, 38).[19] He articulates the following criterion: a term of V_T is said to be *cognitively* or *empirically significant* if, when a certain assumption involving a physical magnitude m is specified by theoretical term M, a certain assumption involving m makes a difference in the prediction of an observable event. Specifically, there is a sentence S_M of T, regarding the term M, such that it can be used to infer S_O in L_O (1956, 49).

As one might imagine, the notion of "real" in L_T, pertaining to theoretical entities, differs from the manner in which it is used in L_O.[20] To say, for instance, that a magnetic field is real is to agree to understand the acceptance of the reality of the electromagnetic field in the classical sense as the acceptance of L_T and a term E in it, as well as a set of postulates T, which includes the laws of classical electromagnetism (Maxwell's

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[17] I am indebted to Erik Curiel for providing this example in personal correspondence.

[18] Strictly speaking, only inertial mass is defined directly through a C-rule that connects it with physical observation. Velocity is defined by a C-rule in terms of displacement and time, and displacement in turn is expressed by yet another C-rule that links it to empirical observation, on this account.

[19] Note that this consideration is contra Pincock's contention that the Carnapian theoretical language—or, equivalently, constitutive representations—is adjudicated solely on the basis of pragmatic aspects.

[20] In L_O, the statement that an event is "real" means that the sentence of L_O describing it is true (e.g., "This valley has a lake.").

equations), as postulates for E (1956, 45). Then, for an observer to "accept" the postulates of T means not simply to admit T as an uninterpreted calculus, but to use T along with a specified set of C-rules to guide his/her expectations by deriving predictions regarding future observable events from observed events, based on the postulates T and the C-rules. Carnap encourages us to think of the postulates T as representing the fundamental laws of physics, but not other statements, however well established they may be (1956, 48). Furthermore, both T and the C-rules are completely general, e.g., they do not contain any references to particular positions in space-time, etc.

Interestingly, Carnap offers a response to Pincock's concern that his framework renders the meaning of novel theoretical terms too easy to obtain. He claims that a new theoretical term is introduced to V_T only when a "radical revolution" is effected in the system of science, and not otherwise (1956, 50-1). This is because the postulates T, and the class of terms of L_T admitted as significant, contain only fundamental scientific laws, which are not altered whenever new facts are discovered. Furthermore, as Carnap emphasises, even though all of T is presupposed in the criterion of significance, the issue of meaningfulness is separately considered for each theoretical term, and not merely for V_T as a whole. As we shall now see, when we consider Maxwell's formulation of his equations of electromagnetism, it was precisely a radical revolution in our conception of the nature of electromagnetism that was brought about by the introduction of a novel term in the theoretical language.

3.5 Maxwell and the displacement current

> From a long view of the history of mankind—seen from, say, ten thousand years from now—there can be little doubt that the most significant event of the 19th century will be judged as Maxwell's

> discovery of the laws of electrodynamics. The American Civil War will pale into provincial insignificance in comparison with this important scientific event of the same decade.
>
> (Feynman 1964, vol. 2, 1-6)

Maxwell's discovery of the laws of electromagnetism is one of the most significant events in the history of scientific thought. He first derived them in his "On physical lines of force" (1862) as 20 differential equations of 20 variables. He was also the first to show that these laws are expressible as first-order partial differential equations (Fitzpatrick 2008, 116).

Michael Faraday had previously revolutionised physics in 1830 by showing through extensive experimentation that electricity and magnetism are interrelated.[21] Maxwell was the first to clarify and articulate the nature of this relationship between the two phenomena, in the form of equations that are as remarkable for their elegance as they are for their immense range of applicability. In modern notation, these four equations are as follows:

$$\nabla \cdot E = \rho/\varepsilon_0 \qquad (3.1)$$

$$\nabla \cdot B = 0 \qquad (3.2)$$

$$\nabla \times E = -\partial B/\partial t \qquad (3.3)$$

$$\nabla \times B = \mu_0 j + \varepsilon_0\mu_0 \, \partial E/\partial t \qquad (3.4)$$

where E represents the electric field, B is the magnetic field, ρ is the charge density, j is the current density, ε_0 is the permittivity of free space, and μ_0 is its permeability.

[21] Maxwell, who was heavily influenced by Faraday's experimental work, subscribed to his "lines of force" model (Faraday 1852) to explain electric and magnetic forces, contra the action-at-a-distance theory of forces held by the majority of physicists at the time, such as Weber (Weber and Kohlrausch, 1856).

As we can see, Eqs. (3.1) and (3.3) are correspondent, as are Eqs. (3.2) and (3.4). Equation (3.1) states that the divergence of the electric field E is charge density/ε_0, which is true of static as well as dynamic fields. Equation (3.2) says that since there are no magnetic charges, the flux of the magnetic field B through any closed surface is always zero (Feynman 1964, vol. 2, 18-1). Equation (3.3) describes the induction of electric fields by changing magnetic fields, and Eq. (3.4) describes the generation of magnetic fields by electric current *as well as* the induction of magnetic fields by changing electric fields over time (Fitzpatrick 2008, 122).

Prior to Maxwell's work, the magnetic field of steady currents was expressed as

$$\nabla \times B = j / \varepsilon_0 c^2 \qquad (3.5)$$

which is Ampere's original circuital law. A divergence of the above equation reduces the left-hand side to zero because the divergence of a curl is always zero. Hence, the divergence of j ought also to be zero. But if so, the net flux of current out of any closed surface is zero as well (Feynman 1964, vol. 2, 18-1). This cannot be true in general because we know that charges can move from one place to another. Hence the introduction by Maxwell of the extra term to yield Eq. (3.4).

Feynman provides a simple example to explain where Ampere's original law encounters difficulties (1964, vol. 2, 18-2). Imagine a large symmetrical, spherical block of Jello that is a conductor with a hole in the centre, into which some charge has been injected through a hypodermic needle, and is slowly leaking. We assume that the current is moving radially outward, with the same magnitude in all directions. The question, then, is whether the current generates a magnetic field. It does not. This is because since the sphere is symmetric, it can only generate a symmetric magnetic field. However, the only fields possible in this case are one that points everywhere outwards and one directed everywhere inwards, both of which correspond to non-existent monopoles by Eq. (3.2) above. Hence, Ampere's law must be wrong because we know that a magnetic field always exists around a charge.

The most commonly used instance to clarify this problem and underscore Maxwell's contribution involves a parallel plate capacitor (Fitzpatrick 2008, 118). I will use it to clarify why Maxwell needed the additional term that distinguishes Eq. (3.4) from Eq. (3.5) above.

Consider a long, straight wire interrupted by a parallel plate capacitor, as shown in Fig. 3.1. The letter "C" in the figure represents a loop circling the wire. In time-dependent situations, transient current flows through the wire as the capacitor charges up or down, generating a transient magnetic field. Hence, the line integral of the magnetic field B around C is non-zero. According to Ampere's circuital law, the flux of current density through any surface attached to C should be non-zero as well.

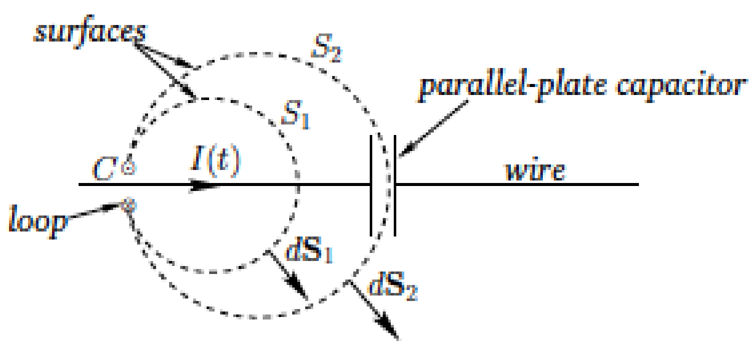

Figure 3.1. The application of Ampere's circuital law to a charging/discharging capacitor (from Fitzpatrick (2008, 118)).

Now consider two such surfaces, S_1 and S_2. S_1 intersects the wire; hence, the flux of j through the surface is non-zero because it intersects a current-carrying wire. S_2 passes between the two capacitors, as shown in the above figure, and does not intersect the wire. Hence, the flux of current density j through S_2 is zero. However, since both surfaces are attached to the same loop C, Ampere's law

$$\oint_C \mathbf{B} \cdot \mathbf{dl} = \mu_0 \int_S \mathbf{j} \cdot \mathbf{dS}.$$

requires that the two fluxes be identical. Ampere's law is thus incorrect in this context.

Note, however, that while S_2 does not intersect the electric current (loop C), it *does* pass through a region of strong changing electric field as it threads between the plates of the capacitor. Hence, Maxwell altered Ampere's law to

$$\oint_{\partial S} \mathbf{B} \cdot \mathrm{d}\mathbf{l} = \mu_0 \oint_{\partial S} \left(\mathbf{J} + \varepsilon_0 \frac{\partial \mathbf{B}}{\partial t} \right) \cdot \mathrm{d}\mathbf{A}$$

or

$$\nabla \times B = \mu_0 j + m_0\mu_0 \partial E / \partial t$$

by adding the new second term—$m_0\mu_0 \partial E / \partial t$—describing the induction of magnetic fields by changing electric fields. This was called "displacement current density" by Maxwell.[22]

3.6 Reconsidering criticisms of Carnapian frameworks

Equations (3.1)-(3.4) are clearly fundamental laws, and hence would correspond to T-postulates for Carnap and, equivalently, constitutive representations for Pincock. In fact,

[22] As is well known, the term "displacement current" is a misnomer because it is not current at all, but the induction of magnetic fields by time-dependent electric fields. Maxwell subscribed to the existence of the aether, which was thought to permeate all space. He called the phenomenon "displacement current" under the assumption that it was caused by displacement in the aether. Maxwell considered electric and magnetic fields to be manifestations of stress in the aether.

as Feynman states, in the context of 19th century physics, Maxwell's equations in conjunction with the others shown in Figure 3.2 below, constituted *all* known fundamental classical physics (excluding thermodynamics) until 1905 (1964, vol. 2, 18-3). The C-rules, though not specified, are presumably constituted by general guidelines for the association of theoretical terms, such as charge, flux, electric and magnetic intensities, etc., with physical magnitudes and spatial coordinates in order to provide an interpretation of these in the observational language. The ability to measure the magnitudes of these theoretical terms of the system also determines the criterion of significance specific to each.

The new term in Eq. (3.4), $\epsilon_0\mu_0\, \partial E / \partial t$, signifies the displacement current. Hence, we see that the new term representing the induction of magnetic fields due to changing electric fields is defined in terms of other theoretical terms of the system— $\epsilon_{,0,}$ $\mu_{0,}$ and $\partial E/\partial t$—the values of which can be determined through the relevant C-rules.

Maxwell's equations

$$\text{I.}\quad \nabla \cdot \boldsymbol{E} = \frac{\rho}{\epsilon_0} \qquad \text{(Flux of } \boldsymbol{E} \text{ through a closed surface)} = \text{(Charge inside)}/\epsilon_0$$

$$\text{II.}\quad \nabla \times \boldsymbol{E} = -\frac{\partial \boldsymbol{B}}{\partial t} \qquad \text{(Line integral of } \boldsymbol{E} \text{ around a loop)} = -\frac{d}{dt}\text{(Flux of } \boldsymbol{B} \text{ through the loop)}$$

$$\text{III.}\quad \nabla \cdot \boldsymbol{B} = 0 \qquad \text{(Flux of } \boldsymbol{B} \text{ through a closed surface)} = 0$$

$$\text{IV.}\quad c^2 \nabla \times \boldsymbol{B} = \frac{\boldsymbol{j}}{\epsilon_0} + \frac{\partial \boldsymbol{E}}{\partial t} \qquad c^2\text{(Integral of } \boldsymbol{B} \text{ around a loop)} = \text{(Current through the loop)}/\epsilon_0 + \frac{\partial}{\partial t}\text{(Flux of } \boldsymbol{E} \text{ through the loop)}$$

$$\left[\begin{array}{l}\textbf{Conservation of charge} \\ \qquad \nabla \cdot \boldsymbol{j} = -\dfrac{\partial \rho}{\partial t} \qquad \text{(Flux of current through a closed surface)} = -\dfrac{\partial}{\partial t}\text{(Charge inside)}\end{array}\right]$$

Force law

$$\boldsymbol{F} = q(\boldsymbol{E} + \boldsymbol{v} \times \boldsymbol{B})$$

Law of motion

$$\frac{d}{dt}(\boldsymbol{p}) = \boldsymbol{F}, \quad \text{where} \quad \boldsymbol{p} = \frac{m\boldsymbol{v}}{\sqrt{1 - v^2/c^2}} \qquad \text{(Newton's law, with Einstein's modification)}$$

Gravitation

$$\boldsymbol{F} = -G\frac{m_1 m_2}{r^2}\boldsymbol{e}_r$$

Figure 3.2. All of classical physics (from Feynman (1964, vol. 2, 18-1)).

Thus, the displacement current is defined, and hence assigned a definite meaning, in terms of these theoretical terms, and the definition immediately provides a method to measure it by virtue of the correspondence rules used to determine the values of t_0, μ_0, and $\partial E/\partial t$.

As we saw in §3.3, Pincock attributes to Carnap the view that the meaning of a theoretical term is *exhausted* by the rules for its use, and objects to it on the ground that a theoretical term in physics needs to somehow be linked to phenomena in any framework that purports to successfully describe theories in physics, which predict affairs in the world with remarkable success. However, this attribution is incorrect. Carnap in fact claims that a "theoretical term can never be explicitly defined on the basis of observable terms" (1966, 234). And he justifies this claim by appealing to the history of science. The rules of correspondence in Carnapian frameworks should roughly be understood as procedures for the measurement of the magnitudes of the theoretical quantities with which they are associated. They are intended to supply the elusive connection between theory and observation by *partially* interpreting the theoretical terms in a manner that corresponds to a certain observation. In order to understand why the interpretation should be partial, Carnap invites us to consider the theoretical term "temperature" in the kinetic theory of molecules (1966, 265-266). There are rules of correspondence that link this term with the construction and use of a thermometer. The thermometer, when suspended in a liquid, records a measurement that the correspondence rules associate with "temperature" in a way that provides an interpretation of the term. This interpretation is partial because it does not apply to all sentences of the theory featuring the theoretical term "temperature:" an ordinary thermometer can only measure temperature in a limited interval (e.g., the mercury-in-glass thermometer covers a range from -37 °C (-34.6 ºF) to 356 °C (672.8 ºF)). For temperatures below which any test liquid would freeze and those above which any test liquid would solidify, special measurement techniques are used; these in turn require different C-rules from the ones that govern the measurement of "temperature" with a mercury-in-glass thermometer. Even if an alcohol thermometer is used to measure temperature in a range that overlaps with that measureable by a mercury-in-glass thermometer, different correspondence rules would be required for the former than those used for the latter, at least because a different fluid with different properties is involved. Now each of these C-rules provides a different interpretation of "temperature," but none of these by itself can be said to exhaust the meaning of the term.

Carnap explicitly invokes the practice of science in defence of his resistance to (exhaustive) definitions of theoretical terms in physics. He hints at an instance—the extension of Maxwell's equations of electromagnetism, in fact—in a different context (1966, 242), but I think it applies nicely here. There was a parameter "c"[23] in Maxwell's equations that described the velocity of waves in an electromagnetic field in case of a disturbance. Coupled with the theoretical observation that the electromagnetic field in free space following the elimination of either the electric or the magnetic field from his equations was describable by the wave equation of classical optics, Maxwell conjectured that light is a special case of electromagnetic oscillation. This was borne out following the brilliant experiments by Hertz in 1888 (Goldstein 2010, 575). Here is an example where a theoretical term, c, that was assigned one interpretation was accorded another in a move that led to a massive advancement in physics. And this is not an isolated instance, as Carnap points out (1966, 237). The history of 19th century physics is peppered with instances where additions to the interpretations of theoretical terms have yielded revolutionary insights.

I make much ado of this because it is not about nothing. It reveals another benefit of Carnapian frameworks as it concerns their harmony with scientific theorising and practice. With regard to the reconstruction of theories, the Carnapian refusal to explicitly define theoretical terms nicely reflects the tenor of the historical development of scientific theories, as we have seen above.[24]

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[23] In Eq. (3.4), $c = 1 / \epsilon_0\mu_0$.

[24] Furthermore, if I may be allowed to speak loosely, with regard to prediction, it offers a clear, albeit very general, possibility of the contribution that a Carnapian reconstruction might make to theories in science that are current. Given that our (theoretical) knowledge in physics seems to be tending towards a

The above tangent notwithstanding, charity demands that I consider Pincock's criticism above by applying it to the partial interpretation of theoretical entities. That is, if, as I gathered from my consideration of his remarks in §3.3, the crux of Pincock's criticisms of Carnapian frameworks is that they do not accurately track scientific progress, his objections ought then to be considered in light of a modified criticism. This would be as follows: Carnap claims that all the interpretation that can be accorded to a theoretical term is due to rules for its use (its rules of correspondence). Since Carnapian frameworks are presumably intended to reconstruct scientific theories, which describe and predict events in the world, some link is required between the interpretation of theoretical terms and the phenomena that they are supposed to represent. But theoretical terms are analytically defined, which forestalls the possibility of empirical content. Hence, Carnapian frameworks fail to fulfil their purpose.

It is true that in a Carnapian framework, the partial interpretation of a new theoretical term is determined by the postulates in conjunction with the C-rules. However, as the historical example in §3.5 has shown, this is nothing other than the assignment of interpretation to theoretical terms through the delineation of rules for the measurement of quantities associated with the corresponding observable terms. This is accepted, common practice in science. Pincock's argument appears to be premised on the conception of an armchair theoretician who has little regard for whether representational systems, or the terms employed in the postulates constituting these, actually represent phenomena. The

convergence, exemplified in the spectre of the Grand Unified Theory model, a Carnapian reconstruction can help identify possible connections between theoretical terms representing seemingly disparate phenomena in different contexts. This would enable it to assist scientists by anticipating these relationships.

criticism that Carnap's view "makes meaning too easy to attain" is a consequence of this assumption, and one of the commonest misunderstandings of the nature of Carnap's frameworks. One can easily devise a completely arbitrary framework, containing the minimally required postulates, C-rules, etc., such that all theoretical terms can be assigned meaning. However, the criticism is misplaced because while such a toy framework would never be judged to be fruitful or desirable by a reasonable scientist, Carnap is not at all concerned with frameworks of this nature, based on little more than a priori whimsy. The generation and appropriate articulation of a theoretical Carnapian framework that can be useful to scientific modelling and inquiry, such as that provided by Maxwell, is an extremely complex exercise. Furthermore, even though Maxwell was not an experimentalist, he had access to mountains of experimental data, based on work by Faraday, Coulomb, Ampere, and others, that profoundly shaped and informed his research. In fact, Maxwell's discovery of the inadequacy of Ampere's circuital law, through the experiment involving a parallel plate capacitor, is based on intimate knowledge of the experimental procedures in electricity and magnetism at the time. Large parts of his corpus, in particular *A Treatise on Electricity and Magnetism* (1873), are devoted to establishing the adequacy of his proposed equations for various experimental situations.[25] Hence, it is incorrect to assume that Carnapian frameworks are to be used by theoreticians without regard for experimental research in their respective areas.

Maxwell's addition to Ampere's Law in the above also serves to address the concerns expressed by Rozeboom and Kaplan, and endorsed by Glymour. The objection was that

[25] Item: Part III of Ch. VII of the 1873 treatise is entitled "Magnetic measurements," Ch. XV is called "Electromagnetic instruments," XVII is titled "Electricity measure of the coefficient of induction," and so on.

the addition of theoretical postulates or rules of coordination, provided these are consistent with pre-existing content of a given theory, should not cause theoretical entities that were already part of the framework to gain or lose significance because this militates against our intuitions and is contrary to scientific practice. As we saw, Maxwell added the term for the displacement current—$\epsilon_0\mu_0 \partial E / \partial t$—to Ampere's circuital law—$\nabla \times B = j / \varepsilon_0 c^2$. Note that Maxwell's addition is consistent with Ampere's law, and does not render it false: as we now know, the circuital law is known to hold in magnetostatic situations—systems where the electric current is steady—and Maxwell's addition is required in all other cases. Hence, this is a case of addition to the set of postulates of a theory. Moreover, it certainly alters the significance of the theoretical term associated with the flux in the magnetic field by rendering it dependent on displacement current in addition to current density. This falsifies the assumption, *based on mere intuition*, underlying Rozeboom and Kaplan's criticisms, whereby theoretical terms must retain their significance in case the theory is extended. Relatedly, insofar as the addition of the rules of coordination constitutes an extension to the theory in a Carnapian framework as well as in scientific practice, and since these rules would typically be exemplified in a Carnapian framework as a wide variety of models of measurement procedures, representation theorems, data collection procedures, data normalisation procedures, etc., there is *no reason* based on scientific theorising and practice to think that the significance of a theoretical entity is somehow independent of extensions to the theory.

Furthermore, given the criterion of significance for theoretical entities provided by Carnap, it is clear that the framework contains an *internal* yardstick by which to gauge the success of the employment of a theoretical term, or a postulate, which can then be revised if the criterion is not satisfied, i.e., if a change in the value of the relevant theoretical term does not yield a difference in the observation or prediction of the relevant events. Analogously, a theoretical framework as a whole is assessed according to its success in representing the phenomena in question.

3.7 Limitations, and another amendment to Carnap

It is apt to conclude this chapter by highlighting the limitations of my argument in defence of Carnapian frameworks. I have shown that the criticisms against the fitness of these frameworks to represent scientific theories described in §3.3 dissipate when we analyse instances from science. At the same time, it should be clear that my analysis of Maxwell's equations of electromagnetism in the context of Carnapian frameworks goes only into sufficient detail to address such a priori concerns. The detailed reconstruction of Maxwell's theory along the lines of a Carnapian framework, even assuming a semantic or model-theoretic view, is an extremely complex and challenging task, and one that is better left for a more ambitious enterprise in more capable hands.[26]

Furthermore, the instance of the sort that I have analysed in §3.5, *strictly speaking*, proves nothing conclusive regarding the soundness of Carnapian frameworks for representing scientific theories in general; nor does it decidedly refute all the objections to Carnap presented in §3.3. At the same time, it should cast significant doubt on the correctness of these and other a priori criticisms of Carnapian frameworks.

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[26] Prof. DiSalle has pointed out to me that such a detailed reconstruction may not always be required, as it is possible that the relevant philosophical issues are addressed by the sort of sketch I have provided here.

In spite of the above, it is understandable to view the above reservations against Carnap's frameworks as prompted and encouraged in part by the absence of important details in his elaboration of his programme for the reconstruction of scientific theories. I refer to his unsatisfactory description of the manner in which the rules of correspondence provide (partial) interpretations of theoretical terms. Consider the example cited in §3.4 of the how the C-rules are used: "the C-rule R, say, relates to an observable space-time region, say u, through a class of coordinate quadruples of intervals about (x, y, z, t)." We know that the assignment of physical magnitudes to theoretical entities is certainly not as simple a task as suggested by this. Underlying such an assignment is a series of complex processes that mediate the connection between theoretical entities and observational reports. Carnap might claim that this simplification is justified given that he intends to provide a framework that is applicable to the practice of science in general, rather than a particular formulation that can provide the requisite detail for some branches thereof but might prove too restrictive for others. However, the issue is *precisely* that the process of coordinating theoretical constructs with experimental data is *in general* a very complicated exercise. The reconstructive project of the kind that Carnap proposes thus requires a more detailed, albeit schematic,[27] account of the various steps involved in arriving at an epistemological rapprochement between theory and observation. As Quine points out in a different context,

I think [Carnap's example of locational coordination, as above,] is a good schematization (deliberately oversimplified, to be sure) of what science really does; but it provides no indication, not even the

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[27] I am using schematic in the sense described by Stein (1994). Speaking in a similar context regarding a possible way of circumventing the intractable problem of *deducing* observations from a Carnapian framework, he proposes mathematical structures within the theory that can represent generic experimental procedures and empirical content.

sketchiest, of how a statement of the form 'Quality *q* is at *x; y; z; t*' could ever be translated into Carnap's initial language of sense data and logic.

(Quine 1951, 37-38)

Among other things, a more representative account of the connection between theory and observation would take cognisance of the fact that this correspondence is obtained through a series of procedures involving, on the one hand, the development of a tractable numerical model of the theory that is susceptible to testing and, on the other, the manipulation of the results of experimental procedures to obtain datasets in a form that fits with the models of the theory. All this does not even take into account considerations of the theory involved in the design of experiments and the interpretations of the results of these in order to render them in a form conducive to models of the theory.[28] At the same time, Carnap's frameworks are readily susceptible to the provision and addition of this detail because they are designed in light of canonical physical theories.[29] The sort of model-theoretic, semantic approach to theories that I outlined in §3.2 is well suited to this, and can help provide this structure.

Hence, this is a third way in which my proposal here deviates from Carnap's original programme—the other two being a semantic view of theories rather than Carnap's syntactic view, and a detail-oriented, bottom-up approach to instances of the use of theory. Nonetheless, this departure can help provide the sort of detail that, on the one hand, will help make such frameworks more representative of the details of scientific practice and, on the other, will forestall objections that do not engage scientific practice

[28] It is this complexity that leads Stein (1992, 1994) to think that a deductive "dictionary" of correspondence rules linking theory to observation is not forthcoming.

[29] Curiel (2005, 2012) has undertaken some promising work in this area.

by presenting the problems of the relation between theory and observation as detail-oriented puzzles that inevitably require such engagement.

I have argued in this chapter that Carnapian frameworks are adequate for the representation of theoretical entities as they are employed in our scientific theories. In the context of the philosophy of science, this helps partly address one of two related, general considerations. The first involves issues relating to the adequacy of the theoretical/mathematical apparatus used in a theory to represent phenomena, its role in the design and methodology of experiments that can confirm or infirm its hypotheses, the accuracy with which a theory so formulated can predict features of the target system, and so on. While my proposal and defence here has been limited to a Carnapian conception of *theoretical entities* in philosophical reconstructions of scientific theories, a complete linguistic framework of this sort should help provide insights into conceptualising and examining the above matters. Such a complete account is beyond the scope of this dissertation. The second consideration related to the use of theoretical entities in scientific representation pertains to the justification for the use of mathematics in scientific theories, and is couched in questions regarding the ontological status of the formalism used in representational systems. As mentioned in Chapter 1, the use of abstract and highly complex mathematics allows us to model and predict goings on in the physical world with remarkable accuracy, and this nourishes the idea that mathematics is somehow “real” in the way the things it (oftentimes) describes are real. This leads to demands by many to ground our mathematical knowledge on a firm epistemological footing such that its ready application to the representation of phenomena is vindicated. Hence, in the next chapter, I attempt to address inquiries concerning the ontological status of theoretical entities in science by answering the third of the research questions posed for this dissertation in §1.1: *What can we conclude about the nature of theoretical/mathematical entities employed in a theory from its success in representing phenomena? More generally, what philosophical benefit, if any, is to be expected from ontological inquiries of the above sort, and how ought it to shape our preferences concerning research questions in the discipline?*

4 The status of mathematical entities in science

Les métaphysiciens sont des musiciens sans dons musicaux.

Rudolf Carnap

The ontological status of abstract entities has long been a controversial subject in philosophy. In the literature on the philosophies of science and mathematics in the last few decades, the recognition of the mathematisation of science has prompted renewed demands for efforts to justify the use of mathematics in representation. We employ our scientific theories to gain knowledge of the world, the structures and features of the phenomena therein, and to predict the course of events based on the representations of the world facilitated by mathematics. Hence, the argument goes, in order to be certain that our knowledge of the world is well grounded, the mathematics employed in our scientific theories needs to be justified.

While demands for the justification of the mathematics used in science have been variously articulated by different thinkers, a shared feature of these is the emphasis on doing so by establishing some kind of a connection between knowledge that is already relatively securely grounded, such as empirical evidence based on sense experience, and abstract mathematical formalism. Hence, Benacerraf thinks that to this end, any theory that interprets mathematical truth as "theoremhood" also needs to explicate the connection between truth and theoremhood (1973, 666). This would be tantamount to having obtained "mathematical objectivity" of the sort desired (Putnam 1979a). According to Maddy, it was this desire for a firm grounding for mathematics, and hence for all our scientific knowledge, that drew Gödel to commit to realism regarding mathematics. Kfia (1993, 19) even claims that the examination of the ontological status of mathematical entities has "far-reaching implications" for the method of science in general, and for physics in particular. In a spirit similar to that of Benacerraf's inquiry, Pincock regards as a most pressing issue the justification of the "purely mathematical beliefs" involved in the theoretical frameworks of our representational systems (2012, 139). Mathematical claims have truth conditions; hence, in order to *know* these claims, we must possess evidence that these truth conditions have been satisfied. Merely deriving a claim from axioms is thus insufficient to generate knowledge because one has yet to establish a connection with truth in such cases. These appeals to ground the mathematics used in our scientific theories seem to be based on a commitment to some variety of semantic externalism, whereby one needs a connection, in this context, between a formal claim and events in the world.

In this chapter, in response to the final research question for this dissertation stated in §1.1—*What can we conclude about the nature of theoretical/mathematical entities employed in a theory from its success in representing phenomena? More generally, how should the philosophical benefit, if any, to be expected from ontological inquiries of this sort shape our preferences concerning research questions in philosophy?*—I consider two major responses to the above concern regarding the status of the theoretical components of scientific representation, offered by Quine and Carnap. I consider these two thinkers because not only have they been among the most influential figures in the philosophy of science in the last few decades, the general position that each espouses is also representative of a major side in the debate on the status of theoretical entities. In very rough terms, Quine represents the so-called "naturalist" position, which denies any distinction between the analytic and synthetic parts of our knowledge, and hence looks to science for its ontological commitments. Carnap likewise represents a “neutralist”[52] stance that seeks to offer a deflationary response to the question of the existence of theoretical entities, including the mathematics used in our theories. Furthermore, while philosophers sympathetic to the views of Carnap and Quine have critiqued and further developed their respective positions on the above issue, they have remained faithful to the fundamental claims that shape their general positions. Hence, for instance, while Maddy’s Second Philosopher (2008, 87) proposes “friendly amendments” to the Quinean programme,[53] these amendments do not result in a significant or principled modification in her stance on the status of theoretical entities. Any deviations from Quine’s views are either not pertinent to the issue at hand, or are sufficiently small in the context of the generality of the discussion to be neglected as internal disputes.[54] I mention Maddy as the most influential representative of a Quinean position on the issue, but the same general commitments regarding the status of theoretical entities are shared by Colyvan (2001), Baker (2005), Resnik (1995), and many others. Similar considerations apply to contemporary philosophers sympathetic to Carnap’s enterprise, such as Friedman (2001) and Stein (1989, 1992), although these are far fewer in number than those seduced by Quine. A notable exception is William Demopoulos (2012), whose deviation from Carnap in the context of the status of theoretical entities is discussed in detail in §4.4.

[52] This term is due to Psillos (1999, Ch. 3). I, for one, consider his stance rather militant.

[53] For instance, “[Contra Quine,] the Second Philosopher resists the characterization of her commonsense beliefs about ordinary physical objects as inferred from some sensory ‘data’; it now emerges that she also departs from Quine’s naturalistic analysis of higher scientific theorizing.”

[54] For instance, Maddy (1992, 280) disagrees with Quine about the ontological status of higher mathematics.

Section 4.1 is devoted to a description of Quine's attitude to the challenge of the ontological status of theoretical entities in general, including mathematics, in our scientific theories. As we shall see, he embraces the proclaimed need to ground our mathematical knowledge and proposes a conception whereby the entirety of our knowledge—and, *a fortiori*, all our scientific theories—is subject to empirical verification, without countenancing a distinction in kind between theoretical (including mathematical and logical) statements and empirical claims. Section 4.2 contains the details of Carnap's deflationary response. He convincingly argues that questions concerning the status of mathematical entities are misguided at best and meaningless at worst. I showed in Chapter 3 that Carnapian frameworks are conducive to the articulation of scientific theories, and can handle theoretical mathematical entities in a manner that tracks scientific practice. In §4.3, I argue that Carnap's approach to questions of the ontology of mathematics, grounded firmly in and flowing naturally from his conception of frameworks, offers far more promise for philosophical investigation than the Quinean alternative. I do this by showing how commitment to a Quinean view of theoretical entities in science has spawned the Indispensability Argument debate in the philosophy of mathematics, which appears misguided and seems to offer little by way of methodological and epistemic insights. While I highlight the assumptions driving the debate in order to relate these to Quine, I will eschew a consideration of these in any detail. This is largely because engaging such debates is tantamount to contributing to futile inquiries in philosophy.[55] Instead, I make a novel, *pragmatic* argument for why

[55] However, see, Field (1980), Maddy (1992), Leng (2005), and Bangu (2008) for objections to various premises of the argument.

Carnap's approach to the status of theoretical/mathematical entities is a more appropriate attitude for meaningful progress in the philosophy of science. Section 4.4 is devoted to a recent criticism of Carnap's position by Maddy and a critique by Demopoulos in the context of experimental proof for the discovery of the atom. A concern shared by both is that Carnap's distinction between internal and external questions in the context of theoretical entities tends to misrepresent and undermine instances of genuine scientific discovery. The outcome of my consideration is that the atomic hypothesis and similar instances pose no problem for Carnap's view, and hence that *no refinement of his position on the issue is needed.* Note that this is at variance with the *Carnapian* stance that I assumed in Chapter 3, which involved a modification to Carnap's reconstructive project.

A reminder of my usage is in order. As the reader might notice, I will interchangeably use the terms "mathematical entities" and "theoretical entities." Unless otherwise specified, they should be taken to be identical, insofar as the mathematical apparatus is a subset of the machinery required for a theory. Furthermore, theoretical entities in science in general, and in physics in particular, are described in mathematical vocabulary. The above identification will become particularly stark in §4.4, when I consider the atomic hypothesis. However, this is not a problem because in the context of his views on the ontological status of theoretical entities, neither Quine nor Carnap makes a distinction between mathematical terms and other theoretical terms. This is not to claim that there is no difference at all for Carnap between purely formal systems, such as Peano Arithmetic, and physical theories, such as Newtonian physics (Carnap 1966, 237). In the case of the former, there is no obligation on the scientist to supply a physical interpretation for the framework in question, since Peano Arithmetic by itself does not purport to describe anything in the world. On the contrary, such a physical interpretation is required in the case of the latter, insofar as a physical theories purport to describe events in the world. As we shall see in §4.4, this difference in the presumptive burden between a scientist working with a purely formal system using Carnap's frameworks and one using them to articulate a physical theory translates into two methods of addressing questions concerning the status of theoretical entities within a linguistic framework.

4.1 Quine and the tribunal of experience

Quine's response to concerns regarding the grounding of theoretical entities in science, including mathematics, is rooted in his famous rejection of the analytic–synthetic distinction (1951) that forms part of his theory of meaning. According to one formulation of this distinction, analytic propositions are true by mere virtue of the meaning or the logical form of their constituent terms, whereas synthetic propositions are not. In my description of his view on the issue, I will only engage as much of Quine's criticism of Carnap as is pertinent for my purposes here, especially since his attack on the latter has been extensively discussed in the literature.[56] In particular, I will not detail or assess Quine's arguments against the analytic–synthetic distinction, nor will I evaluate his

[56] See, for instance, George (2000) and Stein (1992) for opinions on the issue that I find compelling.

reasons for subscribing to the various philosophical positions that lead him to adopt the perspective on science that I describe below.[57]

Quine thinks that language is “a social art, which we all acquire on the evidence solely of other people’s overt behaviour under publicly recognisable circumstances” (1968, 185). His empiricism assumes a commitment to behaviourism about meaning: meaning is nothing other than is manifest in behaviour. By using an elaborate thought-experiment involving the construction by a linguist of a translation manual between English and a novel foreign language, Quine shows that it is possible to devise a number of such manuals that, while mutually inconsistent, are all harmonious with empirical evidence exemplified as behaviour. Hence, it is possible to assign varying, contradictory meanings to the same sentences in different translation manuals such that they are all consistent with experience. Insofar as experience of behaviour is the sole arbiter of meaning, there is thus no fact of the matter about meaning (Quine 1960, 74). This is known as Quine’s indeterminacy thesis. Note that if there is no fact of the matter about meaning, the notion of a class of statements that are true by virtue of their *meaning*—the definition of analyticity with which Quine takes issue in his criticism of Carnap—is rendered nonsensical. Furthermore, if the indeterminacy thesis is correct, then there is no fact of the matter about what the speaker meant when he/she says “Rabbit,” say. If there is no fact of the matter about what the speaker meant when he/she says “Rabbit,” there is no fact of the matter about whether the speaker is referring to a rabbit, a stage in the life of a rabbit, or a physical part of a rabbit (1987, 127-8). This is known as the inscrutability of

[57] For Quine’s behaviourism-based thesis regarding the “indeterminacy of translation” and the consequent “inscrutability of reference,” see Quine (1960, Ch. 2). For an elaboration of its implications for his relativistic ontology, see Quine (1969).

reference—the Quinean thesis that "referents of terms in a language and the range of quantifiers are not determined by physical or behavioural facts" (Hookway 1988, 141). Quine's solution to the issue of referential inscrutability is the relativity of ontology. This is the view that there is no absolute fact of the matter about the ontological commitments of a language or a theory (Hookway 1988, 25). This means that reference in language makes sense only relative to a linguistic framework. It would be meaningless to inquire about the meaning of terms absolutely; such an inquiry can be made only relative to a background language (Quine 1969, 200). Quine's epistemological holism concerning all knowledge, which I detail below, and his conformational holism in the context of scientific theories, which I summarise in §4.3 while discussing the Indispensability Argument, are grounded in this view of language and meaning.

Another Quinean commitment that is critical to shaping his view of the ontology of theoretical entities in scientific theories is his naturalism. He writes:

> Naturalism: abandonment of the goal of a first philosophy. It sees natural science as an inquiry into reality, fallible and corrigible but not answerable to any supra-scientific tribunal, and not in need of any justification beyond observation and the hypothetico-deductive method. (Quine 1981, 67)

There appear to be two facets to this view of naturalism. The first is the rejection of foundational epistemic enterprises of the kind undertaken by Descartes, which seek to ground all knowledge on principles that are known with absolute certainty. Such projects assume a privileged office for philosophy as seeking to justify our successes in science by providing a firm basis for its epistemology. Note that Quine takes Carnap's plan for the rational reconstruction of science as an instance of such foundationalist endeavours. The second aspect of his naturalism is a commitment to science as our best means of learning the nature of the world and, thus, determining the contents of our ontology. The reference to the hypothetico-deductive method in the above quote indicates that Quine embraces all generally recognised sciences as falling within the ambit of science proper.

Quine’s holism and naturalism in conjunction determine his view of the status of theoretical entities. According to his epistemological holism, there is no fundamental distinction between the a priori and the a posteriori, the logical and the factual, the analytic and the synthetic (Friedman 2001, 32). Our system of knowledge should be viewed as a vast network of interconnected beliefs where experience only impinges along the periphery. The centre of this network is occupied by the formal, theoretical components of our knowledge that are not modified or replaced often, such as rules of logic and the postulates of scientific theories that are current. If, as Quine claims, the analytic–synthetic distinction does not hold, there is no difference in kind between theoretical/analytic claims and observational/synthetic[58] ones. Hence, “our statements about the external world face the tribunal of experience not individually, but as a corporate body” (Quine 1951, 38). That is, both the theoretical and empirical components of scientific theories are beholden to empirical verification. He likens “total science,” which constitutes our structured knowledge of the world, to a force field the boundary conditions of which are constituted by experience. A conflict with experience at the periphery occasions adjustments in the interior of the field: truth values have to be redistributed over some of the statements. However, the total field of science is so underdetermined by its boundary conditions—experience—that there is considerable

[58] Strictly speaking, synthetic propositions are not identical to empirical propositions. The former are defined as not being true merely by virtue of the meanings of their constitutive terms, whereas the latter are simply based on experience. Hence, a synthetic proposition is not necessarily empirical. For instance, Kant regarded geometry as synthetic (and a priori) but not empirical. See (Carnap 1966, 267) for the suggestion that Carnap does not respect this distinction.

leeway in the choice of statements to reconsider in light of any single infirming experience. This is because no particular experience is linked to specific formal statements that occupy the interior of this field or "web of belief" (Quine and Ullian, 1978), except indirectly through the consideration of coherence and consistency affecting the entire field (Quine 1951, 39). Hence, it becomes folly to seek a boundary between synthetic statements, which hold contingently based on experience, and analytic statements, which hold come what may. *Any* statement can be held to be true if we make sufficiently drastic changes elsewhere in the system. Quine thinks that taken collectively, science is dependent on language and experience, but this dual dependence is not traceable in the statements of science one by one. The *unit of empirical significance* is the whole of science. Empirical evidence spreads over a conjunction of all elements of our total system of science.

Quine thinks that total science is extremely underdetermined by experience, but the edge of our web of belief must nonetheless be kept consistent with it. The remainder, with all its elaborate "myths or fictions," be it mathematics or the Homeric gods, one translation manual or another, has as its objective the simplicity of the relevant laws (1951, 42). That is, so long as our theories agree with empirical observation, the ontology underlying them is determined based on pragmatic values, since there is no fact of the matter about the "correctness" or "truth" of rival ontologies that are all consistent with experience. Our natural tendency to disrupt the total system as little as possible would lead us to focus on empirical statements for our revision, since these are closer to the periphery of our web and, hence, emendations to them are likely to be far less turbulent for the enterprise of science than some statement—at the centre of the web—more important to the theoretical integrity of our (set of) beliefs that ground the system, e.g., the law of the excluded middle. Crucially, on this view, ontological questions, including those pertaining to mathematical entities, are on par with questions of natural science. For instance, the question of whether to countenance classes as entities is simply one of whether to quantify over variables that admit classes as values. In this conception, the *only* way to make ontological commitments is by using bound variables (Quine 1953, 31-2): "to be is, purely and simply, to be the value of a variable." This heuristic[59] is used to determine the ontological claims made by a particular theory. Hence, "a theory is committed to those and only those entities to which the bound variables the theory must be capable of referring in order that the affirmations made in the theory be true" (Quine 1953, 33).

Quine grants that certain beliefs, such as those of logic and arithmetic, are relatively central in the web, whereas others, such as those of biology, are relatively peripheral. However, this only means that the former are less likely to be revised than the latter in case of recalcitrant experiences at the periphery. On such a view, Quine claims, the difference between the existence of classes, say, and that of physical objects is only one of degree, in that it turns on our pragmatic inclination to adjust one strand of the "fabric of science" rather than another in accommodating some recalcitrant experience. Hence, Quine advocates a more rabid and thoroughgoing pragmatism than that espoused by Carnap.

[59] I use this word, instead of "rule" or "principle," because in spite of his remarkable facility with language, Quine maintains a frustrating glibness with regard to this and other critical components of his philosophy.

4.2 Carnap on the justification of theoretical entities

> A physicist who is suspicious of abstract entities may perhaps try to declare a certain part of the language of physics as uninterpreted and uninterpretable, that part which refers to real numbers as space-time coordinates or as values of physical magnitudes, to functions, limits, etc. More probably he will just speak about all these things like anybody else but with an uneasy conscience, like a man who in his everyday life does with qualms many things which are not in accord with the high moral principles he professes on Sundays.
>
> (Carnap 1992, 72)

Unsurprisingly, Carnap takes up the issue of the status of abstract entities in the context of his linguistic frameworks for science. The sum of his stance is that the use of a formal language that refers to abstract (theoretical) entities does not imply the acceptance of a Platonic (realist) epistemology, and is perfectly compatible with empiricism and strictly scientific thinking (Carnap 1992, 73). Recall that for Carnap, in order to speak in his or her language about a new kind of entity, one needs to introduce a system of novel ways of speaking subject to new rules. This system is a linguistic framework. Carnap makes two crucial distinctions in the context of his frameworks. The first is between formal/analytic sentences, which correspond to logical or "L-rules" in his framework, and empirical/synthetic ones, which correspond to physical or "P-rules." The second distinction is that between internal and external questions (Friedman 2001, 32).

According to Carnap, there are two kinds of questions concerning the reality of entities: i) questions regarding the existence of certain new kinds of entities *within the framework*—internal questions—and ii) those concerning the existence or reality of *the system of entities as a whole*—external questions (Carnap 1992, 73). Internal questions and possible answers to them are formulated using the new forms of expression, either through purely logical methods or empirical ones, depending on whether the question is a logical or a factual one, respectively. "Reality" with regard to internal questions is an empirical, scientific, and non-metaphysical concept. To recognise something as a real thing or event means to successfully incorporate it into the system of things at a particular space-time position such that it fits with other things recognised as real according to the rules of the framework. External questions, on the other hand, concern the reality of the world hypothesised by the framework itself.[60]

Carnap claims that all standards concerning notions such as "correctness," "validity," and "truth" are relative to the logical rules definitive of the framework. Thus, it makes no sense to ask whether one's *choice* of a framework is "valid" or "true" because the logical rules on the basis of which these notions are defined are not yet in place (Friedman 2001, 31). He claims that disputes in philosophy concerning external questions about the ontological status of theoretical entities arise because these questions are framed in an inappropriate manner. To be "real" in the scientific sense means to be an element of the system. Hence, this concept cannot be applied to the system itself, which forms the subject of external questions. However, if one chooses to accept a framework, this must not be interpreted as *belief* in the reality of the framework: there is no such belief or assumption because the relevant question is not an internal question. To accept a framework means nothing more than to accept a certain form of language, to accept rules for forming and testing propositions in order to accept or reject them (Carnap 1992, 74). At the same time, on the basis of observation, the acceptance of a certain framework leads to the acceptance of, or a belief in, the assertion of certain propositions. Decisions regarding the acceptance or rejection of a framework will be influenced by theoretical

[60] Prof DiSalle has pointed out to me that this is only the metaphysical interpretation of external questions— Carnap remarks wryly that such questions are raised "neither by the man in the street nor by scientists, but only by philosophers" (Carnap 1964, 241). External questions might instead concern the pragmatic value of using a framework, or the comparative pragmatic values of different frameworks.

knowledge, and the intended purpose of the framework will determine the factors relevant to this decision. For instance, having introduced a set of rules related to defining and performing operations on the natural numbers, the question “Is there a prime greater than 100?” is an internal question that is answered by logical analysis—a proof— and yields an analytic answer instead of one based on observation. Similarly, the answer to the question “Are there numbers?” is, rather trivially, “Yes!” if the question is construed as an internal question because the relevant rules added to the framework in order to allow the use of numbers establish their existence in the framework. Hence, when asking questions regarding the existence of theoretical entities such as numbers, philosophers, such as Quineans, presumably do not mean to ask an internal question. In fact, they would readily admit that they are asking a question that is conceptually prior to the acceptance of a new framework. These may be posed as questions regarding the ontological status of numbers, some ideal reality, and suchlike inquiries. These questions have not thus been formulated in scientific language. Hence, the above external questions, and possible answers to them, have no cognitive content. Until this is supplied, we are justified in regarding this as a pseudo-question, a non-theoretical inquiry disguised as a theoretical one. In this context, this is expressed as the practical question of whether to incorporate the relevant system of entities into our linguistic framework.

Hence, for all questions related to the status of abstract entities in the framework, responses are readily available through formal or empirical methods incorporated into the framework if the question is construed as an internal one (Carnap 1992, 75). The only feasible interpretation of these questions as external to the framework leads to their reformulation as pragmatic inquiries concerning the effectiveness of the entity in question in fulfilling the intended purpose of the framework (Carnap 1992, 77). Critics of the use of abstract entities in semantics overlook the fundamental difference between the acceptance of a system of entities and an assertion internal to the system, e.g., that there are elephants, electrons, etc. Whoever makes an internal assertion is obliged to justify it by providing the necessary evidence, empirical in the case of electrons and elephants, formal proof in the case of numbers. Hence, the demand for a theoretical justification, appropriate in the case of internal assertions, is sometimes incorrectly applied to the acceptance of a system of entities (Carnap 1992, 81). For instance, with regard to disagreements among philosophers over the status of numbers, Carnap feels compelled to regard the relevant external question—“Do numbers exist?”—as a pseudo-question until both parties to the argument offer a common interpretation of the question in scientific language as a cognitive question. This would involve an indication of possible evidence regarded by each side as having a bearing on deciding the issue.

4.3 The fruitfulness of ontological inquiry

Trends of research in several areas of the philosophy of science in the last few decades indicate that a large number of scholars in the English-speaking tradition have sided with Quine on the issue of the justification of theoretical entities, including mathematical ones. In particular, the Quinean slogan “to be is to be the value of a variable,” in conjunction with his epistemological holism and naturalism, have prompted a long dispute over the status of mathematical entities, called the Indispensability Argument debate, that persists to this day. The argument is attributed to Quine (1976, 1980a, b, 1981a) and Hilary Putnam (1979a, b).[61] It is as follows (Colyvan 2001, 11):

1. We ought to be ontologically committed to all and only those entities that are indispensable to our best scientific theories.
2. Mathematical entities are indispensable to our best scientific theories.

   Therefore:
3. We ought to be ontologically committed to mathematical entities.

The crucial first premise of the argument relies on Quine's naturalism and his confirmation holism described in §4.1. Quinean naturalism rejects metaphysics as first philosophy, and views the project of philosophy as continuous with that of science, which tells us what the world is like. It is only proper, thus, that we look to our scientific theories to determine our ontological commitments. The doctrine of conformational holism claims that theories are confirmed or disconfirmed in their entirety, and not piecemeal. Hence, if empirical evidence confirms or infirms the hypotheses of a theory, the entire theory, *including its mathematical component*, is verified or falsified, respectively. As mentioned in §4.1, I will not consider and evaluate these assumptions in part because they have been extensively treated in the literature, and I have nothing to add to this.[62] Furthermore, critiques of Quinean naturalism and confirmation holism, in light of the looseness and generality with which they are employed in the Indispensability

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[61] Liggins (2008) has claimed that the argument for the indispensability of mathematical entities actually offered by Quine is different from that ascribed to him in the literature. However, even if Liggins is correct, the differences he cites between the original Quinean argument and that associated with him have no bearing on my reasoning here.

[62] For other views, see Parsons (1983) and Laudan (1990) on conformational holism. See Gregory (2011) and Haack (1993) for contrasting views on Quine's naturalism.

Argument, have shown that these theses are suspect at best. My own views on the issue are influenced by Wilson.[63]

So, the first premise states that we should be guided by prevalent scientific theories with regard to our ontological commitments, and the confirmation of a theory directly confirms all of its theoretical components, including the mathematics used. The argument has been hotly debated by realists, anti-realists, as well as instrumentalists over the years.[64] Over time, the realist claim has evolved into an "Enhanced Indispensability Argument," which is as follows (Baker 2005, 613):

1. We ought rationally to believe in the existence of any entity that plays an indispensable explanatory role in our best scientific theories.
2. Mathematical objects play an indispensable *explanatory* role in science.
3. Hence, we ought rationally to believe in the existence of mathematical objects.

This argument seeks to be more specific about the contribution that mathematics makes to our scientific theories and representation in order to bolster the ontological claim.

It is notable that each of the above arguments contains a first premise that invokes the abovementioned assumptions due to Quine. Hence, debate on the issue has been legitimised through the participants' acquiescence to Quinean holism as well as his insistence on experience as the sole arbiter of all knowledge—his naturalism. Once it is granted that all knowledge, theoretical as well as empirical, is subject to a uniform

[63] For Wilson's critique of Quinean holism, see (2006, Chapter 5, §xii). For his objection to Quine's naturalism, see (Ladyman et al. 2013, 198-207).

[64] See, for instance, Maddy (1997) and Pincock (2004).

standard of evidence, and the distinction between the formal components of a framework and its empirical content is rejected, it becomes kosher to seek and locate justifications for the existence of theoretical entities. That is, philosophers inquiring into extra-formal justification of the formal, analytic components of a theory or a linguistic framework are siding with Quine in his rejection of the analytic–synthetic distinction, and endorsing the so-called tribunal of experience as the proper setting to ground the formal as well as empirical content of our theories. This is all well and good, but for the fact that no one, from Platonist mathematicians such as Gödel to the participants of the Indispensability Argument debate, has proposed a framework for the conception and articulation of empirical evidence for or against formal claims. A formal proof as justification of such claims is presumably unsatisfactory to the Quinean due to its remoteness from experience. Quine occasionally writes of “experiential meaning” (1963, 389) to be assigned to the formal components of frameworks in order to justify them. However, neither he nor any other thinker has detailed or even outlined a proper method to do so satisfactorily. And it is important to note that this is *precisely* Carnap’s objection. He writes:

> Unfortunately, these philosophers have so far not given a formulation of their question in terms of the common scientific language. Therefore our judgment must be that they have not succeeded in giving to the external question and to the possible answers any cognitive content. Unless and until they supply a clear cognitive interpretation, we are justified in our suspicion that their question is a pseudo-question, that is, one disguised in the form of a theoretical question while in fact it is non-theoretical …
>
> (Carnap 1992, 75)

“Common scientific language” in the above can be read as the language and epistemology of one’s choice to represent the relevant scientific knowledge. In addition to his complaint concerning the failure of philosophers to meaningfully articulate ontological questions of this kind, it is interesting to note that in the above quote, Carnap does not dismiss out of hand the possibility of the development of methods or frameworks in which such inquiries can be meaningfully and fruitfully made. Carnap's objection is methodological: philosophers involved in debates regarding the status of abstract entities, be it in the contemporary Indispensability Argument debate, the disputes concerning scientific realism from a few decades ago,[65] or even the problem of universals of old, have not even managed to arrive at an agreement regarding an appropriate manner of conceptualising these issues, let alone investigate them to the satisfaction of scientific standards that are more widely accepted. Hence, engaging in debates on these issues without a common, robust methodology to settle the problem is tantamount to putting the cart before the horse. Furthermore, the implication in the above quote is that Carnap would be more than willing to accept these questions if, in the future, they are rendered susceptible to meaningful articulation. Such a generous attitude to an ill-formed dispute is further testament to Carnapian tolerance in what he regards as a pragmatic issue in science.

One possible route to rendering meaningful the ontological questions regarding mathematical entities is to develop a language containing a uniform account of notions such as "meaning," "reference," "truth," etc., for all concepts in the language, whether formal or not. Benacerraf (1973) outlines a few conditions for a project of this kind, which incidentally presumes a variety of naturalism, but this has not been pursued any further in the context of the justification of theoretical entities in scientific theories. Until such an all-encompassing theory is developed, a Carnapian attitude to these questions is justified.

[65] See Boyd (1984) for the classic formulation of the thesis of scientific realism.

My discussion of Maxwell's equations of electromagnetism in §3.5 showed, among other things, that Carnapian frameworks are well suited to represent scientific theories in general, and theories of physics in particular, due to the cleavage therein between a highly mathematised theoretical component and empirical content, to which it is related through rules of correspondence. Specifically, my discussion showed how Carnapian frameworks can accommodate novel theoretical entities. We saw that Maxwell's modification of Ampere's circuital law by adding displacement current density to describe the induction of magnetic fields due to changing electric fields is easily accommodated by a modified Carnapian framework, which assigns to the new term a definite meaning in terms of other theoretical concepts through the C-rules, and hence provides a method to measure it as well. In contrast to this, Quine's prescriptions contain little detail beyond the repeated insistence on an extreme empiricism. This is particularly problematic in the case of a drastic innovation in science that requires the rejection of a prevalent theory in favour of another. While this seems to pose no problem for Carnapian frameworks, as I showed for Maxwell's groundbreaking work, no such methodology is forthcoming in Quine's work. The closest he approaches to considering such revolutionary changes in total science is to note that in the face of obstinate evidence, his epistemic holism allows for alterations to be made elsewhere in the web of belief in order to preserve the centre, where the formal, theoretical components of our knowledge reside, as described in §4.1. Apart from inciting debates that appear to promise scant philosophical fruit, which, if anything, is harmful to the discipline, the Quinean perspective offers no comprehensive plan for pursuing an epistemological enterprise as bold as the one he proclaims.

4.4 Carnap's view of theoretical terms and the atomic hypothesis

Thus far in this chapter, I have shown that Carnap's approach to the status of theoretical entities in scientific representation is fruitful for philosophical inquiry in a number of ways, particularly in contrast to the views espoused by Quine. Not only are frameworks of the kind proposed by Carnap amenable to representing scientific theories, and theories of physics in particular, they also help distinguish issues that can be resolved within the resources of a given theoretical framework from those that cannot. This is accomplished by means of the corresponding distinction between questions that are internal to a framework and those external to it. Among other things, this apparatus allows us to identify certain concerns that have been posed as ontological puzzles pertaining to the nature of the theoretical entities employed in science as misguided or confused. This confusion may arise through a misunderstanding of the aims of science, the content of theories and their relation to experimentation, the scope and limitations of the claims made by theories, or, in the case of representative debates in the philosophy of science, a failure to investigate and appreciate the practice and methodologies of science in order to provide much-needed context. Sections 3.6 and 3.7 developed this in some detail. Debates in the literature on scientific realism and the Indispensability Argument, summarised in §4.3, are exemplars. For these are cases where both realists and nominalists have engaged each other for decades without a clear idea of the formulation of the problem in the varying contexts in question, the methods to pursue in order to arrive at a solution to the problem once it is formulated, and, most importantly, the significance of possible solutions to the problem for science and the philosophy of science. I have also shown in the foregoing that these futile debates in the literature can be traced to a subscription to a Quinean approach to questions of ontology.

In spite of the above, the impression that the Carnapian approach to the status of theoretical entities is incorrect has proven to be considerably resilient in the literature. The theoretical and experimental research that led to the discovery of the atom in the early 20th century has recently been used as an instance. The claim is that a consideration of the research that led to the verification of the atomic hypothesis reveals that Carnap's attitude towards theoretical entities unfairly trivialises questions of their existence to ones of the choice of linguistic framework. Such a perspective unreasonably undermines the importance of such epistemological scientific achievements and hence misrepresents them. In the following, I will consider two recent treatments of the issue, by Maddy and Demopoulos. A reason for choosing this particular instance is that these two thinkers conveniently fall, roughly speaking, along the Quinean and the Carnapian sides, respectively, of the philosophical divide on the consideration of theoretical entities in science. This choice is additionally useful because I think that Maddy's understanding of Carnap's enterprise and its details evinces misconceptions that are widespread in the literature and, although I will not pursue this issue here, should be considered representative. Demopoulos does not accept Maddy's analysis of Carnap but agrees that the atomic hypothesis poses a problem for him. The general conclusion to be drawn from my consideration of Demopoulos' remarks is that Carnap's distinction between internal and external questions in its original form is sufficient to address the above concerns.

At the turn of the 20th century, there was considerable disagreement about the kinetic theory of gases and fluids. This theory describes fluids as composed of a very large number of subatomic particles in constant random motion. Contrasted to this was the thermodynamical approach espoused by Mach, Ostwald, and Duhem. Emboldened by the success in physics and chemistry of thermodynamical approaches, which abstract from and are independent of the underlying structure of matter, these thinkers were sceptical of commitment to a theory of matter based on invisible and undetectable particles.[66]

In the second of his four ground breaking papers in 1905, Albert Einstein derived an equation for the diffusion of particles through a fluid and speculated that this occurs through Brownian motion. In his analysis, he assumed Maxwell–Boltzmann statistics and its relation to the molecular–kinetic theory of heat. In a series of ingenious experiments starting in the same year, Jean Perrin was successfully able to measure the density distribution, the mean displacement, and the mean rotation of Brownian particles in a solution. Crucially, through several different methods, he was able to determine the value of Avagadro's number—the number of particles in a mole of a substance—to an accuracy of within a few percentage points of contemporary estimates. This confirmed the correctness of the kinetic theory and, hence, established the existence of atoms.

To clarify her criticism of Carnap's attitude towards theoretical entities in representational systems, Maddy (2008) asks us to consider the following scenario:

> [S]uppose we've adopted a linguistic framework for simple scientific observation and generalization—perhaps an elaboration of the thing language—and we're wondering whether or not to embrace a new range of entities, say atoms. As our current language has no terms for such things, no predicate 'is an

[66] This was but one reason for their scepticism. Their objections to the kinetic theory were far more nuanced, involving practical considerations as well as the concern to maintain consistency with well-established empirical laws at the time. See Chalmers (2009) for an excellent treatment of the history of the atom.

> atom', no evidential rules with which to settle questions of their existence or nature, Carnap holds that this is not a question that can be asked or answered internally, that we must step outside our linguistic framework and address it pragmatically, as a conventional decision about whether or not to adopt a new linguistic framework. This new framework would include new evidential rules linking various indicators to the presence of atoms, just as the thing language includes evidence rules linking various experiences to the existence of ordinary objects. … [T]he meticulous and decisive work of Jean Perrin on Brownian motion came as a welcome surprise. In circumstances like these, where the new evidential rules are such elusive and hard-won scientific achievements, the Second Philosopher is unlikely to agree with Carnap that their adoption is a purely pragmatic matter, a conventional choice of one language over another. Instead, she insists that the development of the Einstein/Perrin evidence was of a piece with her standard methods of inquiry, that it required careful examination and justification of the usual sorts. … [Even if] the empirical study of human language use might justify some notion of purely linguistic truth, [the Second Philosopher] doubts that a distinction so grounded would put the relevance of Einstein/Perrin's work to the existence of atoms on the linguistic side of the ledger.
>
> (Maddy 2008, 71-2)

There are a number of explicit and implicit issues of interest in the above, but I will confine my observations to the extent required by my purposes here. The thrust of Maddy's argument is that in the context of his framework, Carnap would regard Perrin's crucial experiments to prove the existence of the atom merely as one of many choices that need to be made in the adoption of a language for the corresponding theory. This serves to reduce the question of the existence of the atom, a significant cause for dispute at the time, as well as Perrin's experiments to settle it, to one of which framework to use based on pragmatic considerations. This militates strongly against the intuition, well grounded in science, that this issue is one of ontology, of what does and does not exist. That Carnap considers this a problem concerning the pragmatics of language choice is sufficient, Maddy thinks, for us to reject his stance on the status of theoretical entities.

Thought experiments serve as a powerful tool for conceptual analysis in science and philosophy, and a rich tradition attests to their usefulness in such inquiries. At the same time, there is a widespread tendency in philosophy to forget that the major purpose of thought experiments is to clarify and refine our pre-theoretic concepts in order to develop general methodological principles for subsequent research in the field in question.

Instead, it is common among philosophers to employ these for largely critical and invariably superficial analyses of proposals in numerous areas of philosophy without considering their details.[67] Hence, I think it pertinent to explore the thought experiment offered by Maddy above in order to evaluate the merit of her criticism.

In the context of a Carnapian framework, Maddy asks us to consider a situation where a new kind of entity is being posited, for which there is no theoretical or relevant observational apparatus in our language. This is problematic with respect to betraying a misunderstanding of Carnap's frameworks in two ways. First, it is important to remember that these frameworks are not intended for use in scientific practice: We know that most scientists do not know or care about them; nor does Carnap prescribe the use of his frameworks to scientists to formulate theories. They are instead intended for a *rational reconstruction* of these theories in a meticulous and regimented manner, so that the numerous assumptions and inferences implicit in the relevant theoretical and experimental procedures are laid bare. Hence, the hypothetical question of the application of Carnap's framework to Einstein's analysis of Brownian motion or Perrin's experiments *at the time that they were conducted* is one that is irrelevant to its purpose. Hence, the thought experiment fails to get off the ground in the first place. The second manner in which Maddy's thought experiment misconstrues Carnap's frameworks is a consequence of the first. The fact that these frameworks are neither used in scientific practice nor, *a fortiori*, in the context of the discovery of novel theoretical entities blunts Maddy's criticism by denying the burden that she seeks to impose on them. Her objection

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[67] See, for instance, Horgan and Timmons' (1992a, 1992b, 1993) use of the twin Earth thought experiment to argue against Boyd's moral realism.

draws its strength from the idea that while *using a framework* in the backdrop of research concerning a novel theoretical entity, it is counter-intuitive to the point of courting absurdity for a scientist to consider the issue of incorporating the relevant theoretical machinery, correspondence rules, experimental procedures, and so on, as a pragmatic choice of language. However, once it is clear that this is not the proper setting for the employment of such frameworks, we are no longer required, as Maddy enjoins us, to think of the question of the existence of atoms as one of choice of language. In the context of the rational reconstruction of a theory—the proper context for the application of such frameworks—the need for an appropriate linguistic framework translates into one that contains the logical, mathematical, and methodological resources required to represent the phenomena at hand. In case of the atomic hypothesis, for instance, this requires a framework that can represent the kinetic theory along with its underlying assumptions—the Maxwell–Boltzmann statistics, the equipartition of the energy of the particles in Brownian motion, and so on—formulate experimental methods that can be used to test the theoretical hypothesis, and develop appropriate correspondence rules to link them. Such a framework is needed in order to represent the theory, and Maddy should have no objection to this.

While he agrees that Maddy's criticism of Carnap's frameworks is mistaken, Demopoulos (2012, Ch. 3) is keen to the force of an argument that lurks underneath her thought but requires some development. He uses the idea of the Ramsey sentence to clarify this argument. The assumption underlying Ramsey sentences is one of a theory the non-logical vocabulary of which has been divided into theoretical and observational terms. Consider such a theory TC:

$$\text{(TC)} \qquad [T_1, T_2, \ldots; O_1, O_2, \ldots]$$[68]

where T_1, T_2, … represent theoretical terms and O_1, O_2, … represent observational terms. The Ramsey sentence of this theory is formed by existentially generalising over all theoretical terms:

$$(TC^R) \qquad \exists X_1, \exists X_2, \ldots [X_1, X_2, \ldots; O_1, O_2, \ldots]$$

The crucial feature of TC^R is that theoretical terms have been eliminated from it. Furthermore, TC and TC^R are equivalent in that anything that follows from the former also follows from the latter. Hence, the Ramsified theory TC^R has the same explanatory and predictive power as the original theory TC. Ramsey wanted to show that it is possible to formulate any theory in a language that does not require theoretical terms but conveys the same observational content. The motivation underlying such a move is that if theoretical entities can be eliminated from the expression of a theory without affecting its content, it can help avoid repugnant metaphysical speculation.

With this machinery in place, Demopoulos poses a puzzle for Carnap (2012, 66). Given any theory, an archetypal realist and an instrumentalist[69] would agree on its observational reports or consequences. Given its Ramsey-sentence reconstruction, the theory is reduced to *nothing but* its observational consequences. Hence, both the realist and the instrumentalist would agree on the *content* of the Ramsified theory. In such a

[68] I have omitted symbolism for correspondence rules because this is not important here.

[69] Following Carnap (1966, 255), I define a realist as someone who thinks of theoretical entities posited by our scientific theories as "actual" in some supra-theoretic sense. An instrumentalist, by contrast, is someone who views theories, and theoretical entities by implication, as tools to organise observed phenomena that are useful but not "true."

case, Carnap must conclude that the two disagree on an external question. However, Demopoulos writes:

> [C]arnap's deployment of his Ramsey-sentence reconstruction should strike us as unsatisfactory: it portrays the question of the reality of unobservables as metaphysical; hence, one that should be transformed into a question of preference for theoretical vocabulary. But then it is difficult to see how the question of the reality of atoms—which are just a special case of unobservables—should not also be regarded as a question of linguistic preference. This is to relinquish at the level of the realism–instrumentalism debate everything we struggled to sustain in connection with the work of Einstein and Perrin, since it leaves Carnap open to the charge that the question the atomic hypothesis raises can be settled by a choice of language.
>
> (Demopoulos 2012, 66)

Hence, given the Ramsified theory, Carnap is faced with a choice of modifying or abandoning his distinction between internal and external questions, or maintaining on pain of absurdity that questions pertaining to the existence of theoretical entities, such as the atom, amount to no more than inquiries regarding the choice of framework. This argument highlights Maddy's concern as well. Demopoulos thinks that Carnap does not have a satisfactory response to it, and hence formulates one on his behalf by extending the distinction between internal and external questions in the spirit of Carnap.[70]

I propose and defend a modification of Carnap's project for the rational reconstruction of science in Chapter 3 because I think that problems persist in this mature view. However, I do not think that the concern shared by Maddy and Demopoulos is one of these, and hence an appropriate response to it can in fact be found in Carnap's work. While explaining correspondence rules in his *Philosophical Foundations of Physics* (1966, 234), Carnap claims that a theoretical entity can never be explicitly defined in terms of observational content. He then writes:

> There is no answer to the question: "Exactly what is an electron?" Later we shall come back to this question, because it is the kind that philosophers are always asking scientists. They want the physicist to

[70] While I will not discuss it here, I should mention that I find Demopoulos' solution unsatisfactory.

> tell them just what he means by "electricity," "magnetism," "gravity," "a molecule." If the physicist explains them in theoretical terms, the philosopher may be disappointed. "That is not what I meant at all," he will say. "I want you to tell me, in ordinary language, what those terms mean."

The claim here is that philosophers erroneously burden the scientist with providing definitions of highly theoretical terms such as the above in ordinary language, abstracted from the theoretical framework in which they are developed, verified, and subsequently used. Carnap thinks that the question here is improperly phrased. When a child asks what an elephant is, we can tell the child that it is a large animal with big ears, and can even show a picture. The temptation among philosophers is, by analogy, to think that theoretical terms can be similarly defined in familiar terms. We can describe an elephant as a large animal with certain characteristics. Why can we not do the same with an electron, say?

> The answer is that a physicist can describe the behaviour of an electron only by stating theoretical laws, and these laws contain only theoretical terms. They describe the field produced by an electron, the reaction of an electron to a field, and so on. If an electron is in an electrostatic field, its velocity will accelerate in a certain way. Unfortunately, the electron's acceleration is an unobservable. It is not like the acceleration of a billiard ball, which can be studied by direct observation.
>
> (Carnap 1966, *ibid.*)

Hence, what Carnap is resisting here is a definition and description of theoretical terms in a language alien to the ones in which they have been formulated, a context foreign to that in which they are designed to feature and function, and vocabulary that is simply not susceptible to yielding a precise or useful description. My suggestion is that Carnap's resistance to definitions of theoretical entities in ordinary language is of a piece with his prescription to distinguish between internal and external questions pertaining to a linguistic framework. Both are motivated in part by the concern that speaking of highly abstract concepts beyond the context of a scientific theory in a language ill-suited for this is inaccurate, unrepresentative of the nature of a "reality" beyond the medium of interpretation provided by the relevant theory, and can easily lead to the development of erroneous beliefs through misuse. This is reminiscent of Carnap's discussion of philosophical concerns regarding the reality of numbers in "Empiricism, Semantics and Ontology" (Carnap 1992, 75). When philosophers ask whether there are numbers, they are not asking whether a linguistic framework in which numbers have been accepted will, if accepted, be found to contain any. Instead, they are making a pre-theoretic inquiry that is conceptually prior to the adoption of one or another framework. It is such inquiries that Carnap resists and wants to discourage. This is borne out in his discussion of the disagreement between a realist and an instrumentalist concerning theoretical entities: "To say that a theory is a reliable instrument—that is, that the predictions of observable events that it yields will be confirmed—is essentially the same as saying that the theory is true and that the theoretical, unobservable entities it speaks about exist" (1966, 256). Of course, it is reasonable to assume, for the sake of consistency with his enterprise, that Carnap is here speaking of answers to relevant internal questions.

Nonetheless, Demopoulos would be correct in pointing out that in the same passage, Carnap refers to the disagreement between the realist and the instrumentalist as "essentially linguistic." This reinforces the opinion that in spite of my clarification above, issues such as the acceptance or rejection of the atomic hypothesis for Carnap are determined by choice of framework. Briefly, I think there is nothing repugnant about this. On Carnap's view, with regard to the Ramsified theory above, both the realist and the instrumentalist have the freedom to accept or reject a framework that countenances the assumptions required to formulate a theory that helps establish the existence of molecules, atoms, and the like. However, once they have accepted a framework, neither has the freedom to make assertions internal to it without justification. As Carnap says, "Whoever makes an internal assertion is certainly obliged to justify it by providing evidence, empirical evidence in the case of electrons, logical proof in the case of the prime numbers" (Carnap 1992, 81). Hence, on Carnap's conception of linguistic frameworks, the realist is not permitted to make extravagant claims about the existence of theoretical entities without providing requisite evidence, just as the instrumentalist cannot deny such a claim in the absence of the same.[71] The choice of language is open to each in consonance with Carnap's Principle of Tolerance regarding framework selection. I have more to say about this below.

There is another reason for doubting the grounds for the concerns raised by Maddy and Demopoulos. Both claim that Carnap's attitude towards the ontological status of theoretical entities *reduces* the issue of the reality of the atom—in a repugnant sense of the word—to one of mere choice of framework to adopt. In their view, this appears to be at variance with the fact that many scientists, such as Poincaré and Ostwald, were compelled to change their views about the ontological status of atoms following Perrin's experiments: they did not believe atoms were real before, and had to subsequently concede that they were wrong. It seems injudicious to history to present this significant epistemological discovery as constituted by nothing more than choice of language. Since Carnap's framework-dependent attitude yields this counter-intuitive result, they claim that his view of the issue is mistaken.

[71] Friedman (2001, 258-9) makes a similar observation regarding the instrumentalist in the context of a Ramsified theory.

The above objection is premised on the following assumption about Carnap's frameworks: that the *mere inclusion* of a term in the theoretical vocabulary of such a framework is *sufficient* for its interpretation and, hence, empirical verification. This assumption explains the setup that Maddy invites us to consider in her quote from a few pages ago. Assume that we have adopted a certain linguistic framework for scientific observation, and we are wondering whether to embrace a new theoretical entity called the atom. "As our current language has no terms for such things, no predicate 'is an atom,' no evidential rules with which to settle questions of their existence or nature, Carnap *holds that this is not a question that can be asked or answered internally, that we must step outside our linguistic framework and address it pragmatically, as a conventional decision about whether or not to adopt a new linguistic framework*" (my emphasis). There are two points to make here. First, the above is a misrepresentation of the historical circumstances surrounding empirical proof for the atom. As is evident from the work of Perrin (Chalmers 2009, 236-8), as well from reflective accounts of the issue offered by Stein (2014) and Poincarê himself (1946, 135), it is not as if the issue of the existence of the atom was resolved, or is resolvable, by the mere stipulation of a theoretical entity in the theoretical vocabulary. In fact, both Einstein and Perrin were working within a general framework that was acceptable to both energeticists and atomists at the time. This framework, nonetheless, allowed the resources for an *empirical argument* to be made for the existence of the atom. With regard to the discovery of the atom, the task of the framework—Carnap's framework in the context of a philosophical reconstruction of the system—in that case was to allow for the *conditions for the possibility* of an empirical case to be mounted to the effect that matter is discrete rather than continuous. In this sense, this case is analogous, although converse,[72] to Maxwell's introduction of the theoretical term for the displacement current described in Section §3.5. His consideration of the induction of magnetic fields by changing electric fields over time to amend Ampere's circuital law did not require, in the context of Carnap's or Carnapian frameworks, the adoption of a different framework requiring the stipulation of a completely novel theoretical entity alien to the apparatus of the system of electric and magnetic equations, as suggested by Maddy's claim above. Instead, based on the experimental knowledge whereby charges can move from one place to another in general and a magnetic field always exists around a charge, Maxwell was able to introduce an additional term—$m_0\mu_0\,\partial E / \partial t$—to correct Ampere's law. In sum, my first point is that the stipulation of a framework that contains a novel theoretical entity is not sufficient in Carnap's frameworks to claim that such an entity exists, even as a response to an internal question to this effect. In fact, as I have explained in §3.4 while outlining Carnap's view of the role of theoretical entities in his frameworks, a theoretical framework is an *uninterpreted calculus* prior to the introduction of correspondence rules that (mostly indirectly) connect theoretical terms with observational content. Furthermore, Carnap states that the question of the partial interpretation of theoretical terms, which involves formulating procedures for their measurement, is to be taken up separately for each theoretical term in the framework. Hence, the simple admission of a theoretical term to

[72] I write "converse" because while Maxwell provided a (general,) theoretical formulation to account for a mistake in Ampere's law that he detected by studying the results of experimental observations, Perrin devised experimental procedures to test an alternative (theoretical) hypothesis about fundamental particles.

our vocabulary that *may* designate something called the atom is not tantamount to admitting atoms to our ontology as a response to a relevant internal question.[73]

Still, one might argue, even if the historical case of the discovery of the atom does not map on to the objection presented above, and even if it is the case that the methodological apparatus used by Perrin to prove the existence of the atom was common to both energeticists and atomists at the time, one still may imagine a case where, with regard to Carnap's frameworks, the existence of a novel theoretical entity is confirmed (or disconfirmed) by the *mere selection* of a framework that can accommodate the requisite verifying (or falsifying) procedures. This brings me to the second, more general point that is pertinent to both Maddy and Demopoulos's general objection above: to wit, that there is nothing repugnant about considering the acceptance of certain entities in the stead of others in a framework as a linguistic choice. Carnap allows for two ways in which a response can be offered to internal questions concerning the existence of theoretical entities invoked by a framework:

[73] In fact, Poincare''s reaction to the debate concerning the status of the atom is exemplary in this regard as consistent with a Carnapian manner of thinking of the ontological status of theoretical entities. Until Perrin's experiments, Poincare' regarded the hypothesis that atoms exist as "indifferent" because they had no bearing on or relation to the empirical results obtained. This strikes me as very similar to Carnap's notion of an "uninterpreted" theoretical term, one that has been stipulated in the framework but is not associated with observation in any way, direct or indirect. This also explains how Poincare' was even able to make the claim that he did not believe in the existence of atoms, as this presupposed a definition of the concept in his system.

Following Perrin's experiments, the atomic hypothesis transitioned in Poincare''s thinking from being an indifferent hypothesis to an empirical one. This is borne out by the fact that the atom was in fact a concept/term in the frameworks that Poincare' worked with prior to Perrin's experiments. Hence, it is not the case that he was compelled to accept the existence of atoms following Perrin's work, where he had not done so before, but rather that he was forced to change his mind about the attribution of truth values to statements concerning an entity (atom) that was already part of the framework. In this sense, Demopoulos and Maddy can also be considered to be mistaken about the historical details of the change in Poincare''s attitude towards atoms.

i. Formal (logical or mathematical) proof in case the question can be answered by these means. This would include questions concerning the properties of the logical or mathematical apparatus assumed in the framework, such as "Is the set of natural numbers non-empty?," "Are there such things as potential functions?," "Do certain structures assumed in the framework have certain formal properties?," and so on.

ii. Empirical verification, by showing that a theoretical entity articulated in the framework has a physical interpretation in terms of observation, where the interpretation is provided by coordinating principles or rules of correspondence that connect, typically not directly, the theoretical term with the relevant, measurable observational terms. This answers questions such as "Is there such a thing as an atom?," "Are there gravitational waves?," and so on. Answers to these questions are not forthcoming using formal proof, but require verification through the results of observation in order to have a physical interpretation in the framework and, hence, the theory. That is to say, a physical interpretation is a crucial condition for the possibility of answering questions of this kind.

As is evident from the articulation and methods for the verification of scientific theories in practice, the procedures that constitute (ii) above form major portions of various areas of scientific inquiry. For instance, a part of the empirical verification required in response to the relevant class of internal questions mentioned above concerns all of experimental physics, devoted to data acquisition and data acquisition procedures. In the context of Demopoulos's argument, claiming that the Ramsified theory TC^R has the same content as the original theory TC, the tacit but crucial assumption in his presentation of his objection to Carnap is that a theory is *adequately and accurately* captured through its articulation in first-order logic. However, this claim is at best contentious.[74] Given the limitations on quantification and predication inherent in such a logical system, as well as the insistence on such articulations in a language of (typically first-order) symbolic logic, the idea seems to be that once a framework for the representation of a theory has been determined, linking a theoretical term with verificatory procedures in such a framework is easy or relatively trivial. In a similar way, Maddy's objection assumes that the sole act of the *stipulation* of a theoretical term in a framework will yield readily available, or at least simple, methods for its *partial interpretation* through correspondence rules that link it to measurable phenomena. This is the most plausible reason for them to think it appropriate to claim that the question of the existence of atoms is a *mere* linguistic choice in Carnap's frameworks. From a considered perspective of Carnap's frameworks, this is too hasty. The assignment of a partial interpretation to a theoretical term is what accords it significance in Carnap's frameworks, and it is precisely downplaying the methodological complexity of this practice that allows Maddy and Demopoulos to assume that *interpreting* a novel theoretical term is simpler in Carnap's frameworks than is reflected by such episodes in the history of science as the discovery of the atom. In fact, it took a significant technological advancement—the invention of the ultra-microscope by Siedentopf and Zsigmondy in 1903—as well as years of work on experimental design for Perrin to successfully execute his groundbreaking experiments. In a sense, Carnap would agree that the existence of the atom is established as a consequence of the stipulation of the

[74] See Psillos (1999, 60).

relevant term in light of his frameworks. At the same time, he would claim that experimental work by Perrin to answer a crucial, *internal* question about the existence of the atom in the affirmative was vital in terms of the provision of rules of correspondence that provided an interpretation of the theoretical term "atom." He would also admit that this yielded a significant epistemological insight for its time: that matter should be regarded as composed of discrete particles that obey well-known laws in certain distributions. Of course, this is an answer to a question posed within a framework to articulate a physical theory that posits atoms. So long as one does not seek to ask a question about the nature of reality independently of any scientific framework, such a response should be considered satisfactory. If, however, Maddy or Demopoulos seeks to assert that confirmed scientific hypotheses make assertions about the nature of reality beyond the considerations that pertain to a corresponding framework, a stronger argument is needed for why theoretical claims made or confirmed with the assumption of an extensive, often abstract, apparatus should be assumed to hold without it.

Hence, while the *stipulation* of a theoretical entity corresponding to the atom may be a framework-dependent choice, the assignment to it of an *interpretation* and, hence, the discovery or delineation of procedures by which it can be associated with appropriate observational terms as well as the methods to determine the magnitudes of these latter terms, while framework dependent in a sense, constitute a far-from-trivial exercise. This leaves room for the scientist and the philosopher to make insights that can be considered to be genuinely epistemically significant, with the proviso that they remain internal to the framework in question. Hence, contra Maddy and Demopoulos, there is no reason to take issue with the fact that the existence of the atom is determined by choice of framework in Carnap.

4.5 Conclusion

In this chapter, I have offered a response to the third major research question guiding my project in this dissertation—*What can we conclude about the nature of mathematical entities employed in a theory from its success in representing phenomena, and how ought the anticipated philosophical benefit of such inquiries shape our preferences concerning research questions in the discipline?* By way of response, I presented in §4.1 and §4.2 the attitudes of Quine and Carnap, respectively, to ontological questions regarding theoretical entities in the milieu of scientific theories. Using the Indispensability Argument as an instance, I then showed in §4.3 how a commitment to the Quinean view of ontology in science has led to debates in the literature where there appears to be no consensus on satisfaction conditions that would be acceptable to all parties to the debate. Hence, the philosophical and methodological profit to be drawn from debates of this kind, grounded firmly in a Quinean outlook on the world and science, is suspect at best. On the contrary, Carnap's deflationary position on ontological questions that flows from his conception of frameworks for the reconstruction of scientific theories demands precisely the sort of methodological clarity that is absent in Quine, and hence is superior for the pursuit of research questions in the philosophy of science in general.

Finally, in order to underline the contemporary relevance and effectiveness of Carnap's distinction between questions that are internal to a framework and those external to it, which helps identify and dismiss misguided metaphysical inquiries as meaningless, I defend this distinction in §4.4 against recent concerns raised by Maddy and Demopoulos in the context of experimental proof for the existence of the atom. The general concern shared by both is that Carnap's view of ontology in science tends to unfairly trivialise instances of genuine epistemological discovery in science as a simple consequence of choice of linguistic framework to reconstruct a given theory. I have pointed out in response that such an objection presumes that the assignment of an interpretation to theoretical entities in Carnap's frameworks as well as in science is a straightforward matter. Since neither scientific practice nor Carnap's description of the mechanism of the assignment of physical interpretation to theoretical entities in his frameworks suggests that this is the case, there is no reason to accept this presumption. To the contrary, Carnap's frameworks impose stringent demands on their users, regardless of their ontological predilections, to clarify their assumptions and support their claims with the necessary evidence. Hence, the distinction between internal and external questions in his linguistic frameworks does not misrepresent the significance of such epistemological achievements as the atomic hypothesis.

5 Conclusions

5.1 Responses to guiding questions

I had motivated my project in this dissertation at the outset by asking three general questions in §1.1 concerning the role and nature of mathematics in scientific representation. My aim in posing and considering these questions has been to illuminate certain explanatory ways in which mathematics can contribute to scientific representation, and highlight shortcomings in contemporary proposals that claim to be all-encompassing in this regard. Furthermore, I have sketched and defended a proposal for the treatment of theoretical entities in scientific frameworks in the spirit of Carnap, and have argued that adherence to such a conception is beneficial for research in the philosophy of science, particularly in the context of ontological debates regarding the status of theoretical entities invoked in scientific theories.

By way of responding to the first of the questions posed in §1.1—*How does mathematics assist in scientific representation?*—my examination of the accounts of mathematical explanation put forth by Kitcher and Bueno and Colyvan in Chapter 2 yielded a number of insights. We saw that there are at least two general ways in which mathematics is explanatory in scientific representation by drawing on the work of Pincock and Kitcher, and examining episodes in the history of science: i) connecting different phenomena using mathematical analogies, and ii) isolating recurring features of phenomena through acausal representations.[75] An instance of the first is the famous Königsberg bridge example, considered in §2.3 in the context of the mapping account of

[75] Pincock (2012, §3.2) also thinks that mathematics can be explanatory in science by tracking causes.

explanation. This is a steady-state representation, one where the main features of interest of the representation do not change over time. The second kind of explanatory contribution results from employing the mathematical structure used for one kind of physical system to represent another kind. The interesting aspect of this practice is that the target systems are fairly diverse and have little in common as physical systems, but are unified by their common mathematical form. As examples, we saw in §2.2.1 Fisher's mathematical analogy between biological populations and the representation of ideal gases in statistical mechanics, as well as the mathematical framework common to Laplace's equations representing the velocity of irrotational fluids and the forces acting on electrostatic charges in an electric field, among other phenomena. In addition to their use in appropriately conceptualising intractable problems and clarifying interdependencies among the variables involved, Pincock has claimed that a benefit of such analogies is that a small amount of experimental testing to confirm one of the above-mentioned representations would lend it a larger confirmational boost than if it were not mathematically related to the other representation, assuming that the latter has been successfully confirmed. Thus, little testing of the electrostatic case lends it far greater confirmation than would be the case if it were not linked to the representation of irrotational fluid flow. This is because "the independent confirmation of the way the mathematics is deployed for the fluids gives the scientist a template against which to judge the success of the electrostatic representation" (Pincock 2012, 79).

Furthermore, my critique of the unificationist account proposed by Kitcher and the inferential conception of Bueno and Colyvan helped reveal that on account of structural shortcomings, both are inadequate as their corresponding frameworks for representation are too restrictive to accommodate a number of kinds of explanatory contributions of mathematics to science and historical as well as contemporary instances of application of theories. One clear desideratum of a framework that can appropriately represent mathematical entities that emerges from my examination is that it appropriately reflect the structure of scientific theories, specifically theories of physics. It is this conclusion that prompted question *B* posed in §1.1— *Is there a promising philosophical account available to represent the theoretical/mathematical entities employed in our scientific theories in order to help clarify and explain their role?*—as well as my proposal and defence of a Carnapian framework for the representation of theoretical entities in scientific theories in Chapter 3. On the one hand, my choice of the linguistic frameworks of the sort proposed by Carnap was dictated by the need for a representation capable of adequately representing scientific theories. As we saw in §3.4 and §3.5, Carnap's detailed proposal for the treatment of theoretical terms appears to be faithful to the reasoning deployed in formulating such theories and sensitive to the various considerations at play. In fact, a careful treatment of the popular criticisms of Carnap's frameworks in the literature showed that these were based on a similar disregard for the details of scientific reasoning to the kind found in the proposals of Kitcher as well as Bueno and Colyvan. On the other hand, my departure from Carnap's exact view of theoretical entities in his linguistic frameworks—what renders my proposal *Carnapian*—was motivated by the same desire to render such frameworks even more harmonious with scientific reasoning and practice. First, the ease of use of models, in accord with a semantic view of theories in contrast to the syntactic view advocated by Carnap, as well as their widespread employment in theoretical and the applied sciences, prompted my adoption of these in

§3.2. Second, my emphasis on a bottom-up methodology to investigate the role of theoretical terms in scientific systems, as opposed to the general, top-down approach favoured by Carnap, was driven by a similar desire to capture all the important features of scientific reasoning and practice. The idea is to compare the structure of theory with instances of its application to render the former better informed and more reflective of the latter. As my treatment of Maxwell's discovery of the equations of electromagnetism in §3.6 showed, such an approach offers considerable reward, particularly by way of clarifying the interdependence of theory and experimentation in science. Lastly, my proposal in §3.7 that Carnapian frameworks take cognisance of the dirty details of the establishment of relationships between models of the theory and those of the data, as well as the manoeuvres involved in rendering each tractable to computation in the first place, is motivated by similar concerns. I should clarify that the above is intended to point out the ways in which my proposal departs from Carnap's approach while remaining firmly embedded in the general insights and framework supplied by his genius. It is certainly not intended to be anywhere near the final word on fruitful representations of physical systems in philosophy. Instead, the above considerations are meant to act as a springboard for future research into such questions, especially for philosophers sympathetic to the approach sketched above.

Furthermore, in response to the final question posed for my project in this dissertation—*What can we conclude about the nature of mathematical entities employed in a theory from its success in representing phenomena, and how ought the anticipated philosophical benefit of such inquiries shape our preferences concerning research questions in the discipline?*—I endorse in Chapter 4 Carnap's approach to ontological questions concerning theoretical entities in science, whereby such questions, as they are typically formulated, evince a confusion between inquiries that are meaningful within the context of a framework and those that are not. I then make a pragmatic argument to the effect that Carnap’s response to questions regarding the ontological status of theoretical entities in science, based on his linguistic frameworks, is preferable in framing and investigating philosophical problems to Quine’s approach to the issue. To this end, I show how commitment to a Quinean epistemology, and hence a subscription to his view of theoretical entities in science, has led to misguided discussions such as the Indispensability Argument debate that offer neither a satisfactory resolution nor any methodological boon. It is thus to the benefit of research in philosophy to seek guidance from the approach of Carnap rather than Quine.

5.2 General philosophical lesson

I also hope that the reader can see a general philosophical lesson in my work in this dissertation. The tendency to consider theory in isolation from considerations pertaining to its practical implementation is by no means unique to the philosophy of science. In fact, if anything, this has become second nature in a number of issues in meta-ethics, normative ethics, the philosophy of mind, and many other areas. In ethics, for instance, such an approach is evidenced in the absence of any methodology regarding the formulation and assessment of an ethical theory. This results, first, in the approval of crude heuristics[76] as competent substitutes for a careful methodology and, second, in the

[76] The Open Question Argument due to Moore (1903) is representative.

acceptance or dismissal of theories based on extreme and unrealistic problems that occupy the periphery of our experience and, hence, our ethical considerations.[77] This yields discourse that is as unfertile as the Realism–Anti-realism debate in the philosophy of science,[78] without a clear idea of or agreement on the formulation of the question at issue, the standards of evidence considered acceptable, and the implications of the possible outcomes in the context of practical life.

Insofar as the general considerations of my work are transferrable to other domains of the subject, this dissertation should be considered to espouse method and detail in our pursuits in philosophy.

[77] An apt example is the famous trolley problem. See, for instance, Foot (1967), Thomson (1976), Unger (1996), and Singer (2005).

[78] I suppose the best instantiation is a homonymous debate in meta-ethics. See Sayre–McCord (2015) for a summary.

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