Methodology, Reformulation, and Underdetermination: Essays on Realism and Interpretation in Foundational Physics

by

Kevin J. Coffey

A dissertation submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
(Philosophy)
in The University of Michigan
2010

Doctoral Committee:

Professor Gordon Belot, Co-Chair
Professor Laura Ruetsche, Co-Chair
Professor Anthony Bloch
Professor Peter Railton
Professor Lawrence Sklar

Some lessons we learn the hard way
Some lessons don't come easy
That's the price we have to pay

Some lessons we learn the hard way
They don't come right off and right easy
And that's why they say some lessons learned
We learn the hard way

Melody Gardot

for Elsa

ACKNOWLEDGEMENTS

It is tempting, with a completed dissertation already in hand, to describe its evolution with an air of inevitability, as though the many twists and turns along the way were but minor departures from an essentially linear path. Yet a dissertation that gestates for as long as this one offers ample time for honest reflect; too honest, perhaps. And the truth is that there are many ways this dissertation could have failed to be, and it surely would have had it not been for the friendship, generosity, and brilliance of many people. So it is with an extraordinary sense of humility and gratitude that I here acknowledge the enormous debts I owe to those who time and again, in ways large and small, helped prop me up and push me forward. Without them this dissertation would not exist.

More than anyone else, Gordon Belot and Laura Ruetsche kept this dissertation project alive. When I was tied in knots and on the verge of throwing in the towel, it was Gordon and Laura who set me straight, and convinced me that what I had to say was interesting and that my project was worth doing. Perhaps they would have tempered their encouragement had they known what was in store: overdue work and short timetables; last-minute meeting requests; an inveterate disregard for even the most basic deadlines dictated by common courtesy; and a general aversion to hockey metaphors. All in all, they put up with a lot. But it was my extreme good fortune that they arrived at the University of Michigan when they did, and I am grateful they were so generous and accommodating. I would especially like to thank Gordon for patiently nurturing my nascent ideas; for his willingness to meet with me often aboot

Other debts and expressions of gratitude are more personal and non-academic, but no less important to one's success in graduate school. Here the devil really is in the details, as what seem like small gestures at the time loom large in hindsight.

My erstwhile housemate, Matthew Silverstein, has been a tremendous friend, and made many of the harder years more bearable. He frequently offered to help me talk through difficulties, and, in conjunction with Bertie Wertman, was extremely giving with the cookies. I will miss the many PR dinners we shared in New York. In October 2009, undoubtedly one of the darkest months of my life, Matty and Marie Jayasekera surprised me with a Zingerman's sour cream coffee cake, just to make life a touch more bearable. It was but one of many touching gestures from them both.

I am similarly grateful that Dave Dick, while driving me to the airport in May 2009, finally convinced me that I could, and would, finish my dissertation; a short conversation, perhaps, but a meaningful one. I have thought of it often down the home stretch. Ivan Mayerhofer, an erstwhile housemate as well, has also offered much personal support and philosophical conversation throughout the years, not to mention entertainment. Ivan drives me crazy, but I am privileged to have him as a friend. He is perhaps the most giving person I have ever met.

Life at Michigan would have been quite different, and far less pleasant, were it not for the unique role played by Molly Mahony. Molly started working in the Tanner Philosophy Library the same year I started at Michigan. I logged countless hours working there, and Molly witnessed first-hand my life's many vicissitudes. She has provided encouragement, sympathy, and even furniture – including the Crayola light, which she didn't want back – and I am grateful for her friendship. It's hard to imagine life in the Michigan Philosophy Department without Tanner Library, and I wouldn't want to imagine Tanner Library without Molly.

Louis Loeb has also been a source of much needed advice over the years, at once sympathetic and candid, even though he wasn't on my committee; was no longer

placement director or chair; was on leave; and basically had no obligations to me whatsoever. He and Peter were there when I was faced with some of the most difficult decisions of my graduate career, and I was touched by the way their thoughtful and understanding emails acknowleged the fact that difficult decisions are difficult precisely because they pull us in many directions at once.

It has been a source of comfort throughout the years knowing that Linda Shultes and Sue London, in the Philosophy Department main office, were watching my back. Linda and Sue provided reassurance and stability when life seemed most unhinged, and on many occasions emailed to say that a spot had been held for me even though the administrative deadline had come and gone. Thank you both for being so understanding and thoughtful, for putting up with my incessant disorganization, and for patiently keeping track of the 14 different addresses I've had while in graduate school.

Indeed, having been a visitor at two other universities, I have a heightened appreciation of the value of competent and friendly support staff. The Philosophy Department is fortunate to have such helpful people keeping its gears oiled. I am fortunate, too, as the library fees for my misplaced books on fractional calculus would have been astronomical had Jude Beck and Maureen Lopez, both in the main office, not found them in 2303 Mason Hall and graciously returned them for me.

Although we overlapped for only the first half of my graduate education, David Velleman has been an intellectual and personal role model, and has provided sage advice on many occasions – sometimes over lunch in New York, and sometimes over the phone from Allan Gibbard's living room. I wish I had sought his counsel more often.

Other members of the Michigan community I would be remiss not to thank include: Sam Liao, who deserves much of your admiration and esteem for putting up with me as a roommate as I skidded through the job market gauntlet; Vanessa Carbonell, who sent encouraging words the morning of my brown bag, and who went out on

a job market limb for me; Tony Block, from the Mathematics Department, who agreed to be my cognate member at the eleventh hour and yet still managed to offer helpful references and insightful comments; and, finally, the 4155 Crystal Creek Poker Group – Ivan Mayerhofer, George Lin, Josh Brown, Steve Cambell, and Gabe Zamosc-Regueros. I will miss spending the better part of Tuesday evenings taking your money, only to loose it going blind all-in on the last hand.

I have also received heartfelt support and encouragement throughout the years from my parents, Steve and Nancy; my sister, Kimberly; my brother, Shaun; Pietronella van den Oever; Nino Pereira; Marva Barnett; Jocelyn Wenk; Christian Wenk; and Alec Henry.

But by far the most important personal debt I must acknowledge – a debt I cannot possibly repay – is that I owe to Elsa Pereira, my wife. I am embarrassed to admit how much of the burden of my graduate education Elsa has been asked to shoulder, and how much she willingly has. This dissertation, and the emotion and angst associated with it, has dominated my life, and our relationship, for many years, and has persisted through four states, countless semesters of long distance, and unfairly complicated tax returns. Yet Elsa has been the single most important source of joy and optimism in my life, and it's difficult to imagine how I could have done this without her. Of my many years as a graduate student, the happiest ones were those spent with her. She believed in me when I no longer did, and I'm happy to say she was right. This dissertation is for her, with love and gratitude.

TABLE OF CONTENTS

LIST OF APPENDICES

Appendix

ABSTRACT

Methodology, Reformulation, and Underdetermination: Essays on Realism and
Interpretation in Foundational Physics.

by

Kevin J. Coffey

Co-Chairs: Gordon Belot and Laura Ruetsche

Many scientific theories purport to describe empirically inaccessible aspects of the
world. The agenda of foundational physics is more ambitious still: to characterize
the world at the level of 'primitive ontology'. In this pursuit it often posits new
and peculiar physical features, a proclivity aided by the abstract, mathematical way
in which foundational theories are framed. But how do we decipher their physical
content, and in particular the accounts of primitive ontology they offer, and why
think that content is true?

This dissertation explores the relationship between these questions – between the
problem of ontological interpretation in foundational physics and the nature of scientific realism. I do not argue for a unified account. Rather, I examine three contexts
in which their interactions are particularly interesting and philosophically significant.

First, I consider the role scientific methodology plays in a prominent naturalistic
defense of scientific realism: the no-miracles argument. The most sophisticated form
of this argument rests on the broad claim that successful scientific methodology is
irredeemably theory dependent. I examine the nature of this theory dependence

within the context of Maxwell's development of electromagnetism, and argue that the presence of competing ontological interpretations undermines the realist's attempt to draw sweeping epistemic conclusions from methodological success.

I next consider the concept of theoretical reformulation and how it applies to the claim that Lagrangian dynamics is a reformulation of (part of) Newtonian dynamics. How should this claim be understood in light of the fact that the world is non-classical? What can this tell us about the concept of theory reformulation itself, which plays a central role in arguments against scientific realism? I provide an analysis of theory reformulation in terms of counterfactual interpretative judgments, and then cast doubt on its justification in the classical case by developing a non-Newtonian interpretation of Lagrangian dynamics.

Finally, I consider whether competing ontological interpretations raise underdetermination problems for realism about foundational physics. After first re-formulating the underdetermination argument to avoid recent objections that it fails to pose any distinctive threat, I suggest a formulation of realism that vitiates the underdetermination threats posed by competing ontological interpretations.

CHAPTER I

Introduction

Scientific theories purport to describe empirically inaccessible aspects of the physical world around us. They identify the entities and properties that exist; account for how they interact and develop over time; and explain how some features combine to form others. The agenda of foundational physics is more ambitious still: to describe the world at the level of 'primitive ontology'. Foundational theories are intended to be theories of the basic constituents out of which all other physical things are composed. Such theories often posit new and peculiar features of the world bearing no immediate connection to our everyday experiences, a proclivity aided by the abstract, mathematical way in which those theories are framed. How, then, do we manage to decipher their physical content? And why think that content is true?

This dissertation explores the relationship between these two questions – between the role of theory interpretation in foundational physics and the nature of scientific realism. Both are multifarious topics, and I do not argue for a unified account of their relationship (if such an account is even possible). Instead, I examine three contexts in which their interactions are particularly interesting and philosophically significant: our understanding of scientific *methodology*; the concept of theoretical *equivalence*; and our assessment of theory *underdetermination*. This introduction provides a brief overview of the topics of theory interpretation and scientific realism,

as I will understand them here, and situates the essays of this dissertation within that context.

1.1   Theory Interpretation in General

The interpretative questions traditionally associated with foundational physics often arise on account of conceptual problems internal to specific theories. Quantum mechanics, for example, is often claimed to require an interpretation in light of problems associated with its notion of measurement.[1] General relativity is similarly thought to demand an interpretation, but on account of what seem to be empirically equivalent, but physically inequivalent, space-time diffeomorphisms permitted by the theory.[2] The conceptual issues underpinning these types of interpretative questions hinge on the fact that they call into doubt our understanding of a theory's physical content – say, by challenging its coherence or metaphysical acceptability, or by pointing out in a salient way that part of what we thought was its physical content is in fact vacuous.[3] Like the conceptual problems motivating them, the interpretations purporting to be their solutions are theory-specific. An interpretation of quantum mechanics looks altogether unlike an interpretation of general relativity.

The notion of theory interpretation relevant to this dissertation is more general. It arises on account of the central role mathematics, and mathematical representation, plays in articulating the content of our best physical theories. It is a distinctive feature of such theories that their physical content is articulated via sophisticated mathematical formalisms. However, knowing a formalism is generally insufficient for knowing what a theory says about the world. One must also know how that formalism maps onto the physical – that is, what the objects and structures in the mathematical

---

[1]The literature on these specific interpretative problems is vast. For an accessible introduction to the measurement problem, see Albert (1994).

[2]See Earman and Norton (1987) and Belot (1996).

[3]I do not mean to suggest all theory-specific interpretative problems in foundational physics fit this characterization. For a recent survey, see Sklar (2000).

framework correspond to in the world. This mapping between formalism and world characterizes the notion of interpretation at issue in this dissertation.[4] To interpret a theory's formalism is, in part, to specify what that mapping is.[5]

There is at least one sense in which this more general account of an interpretation subsumes the notion of an interpretation typically associated with theories like quantum mechanics or general relativity. For interpretations of quantum mechanics and general relativity alike, despite their differences, aim to characterize the physical content of their respective theories by explaining how their formalisms map onto the world. What makes them central examples of theory interpretation is that they're intended to resolve or dissolve salient conceptual difficulties within their respective theories. But we shouldn't loose sight of the broader point that all theories in physics require interpretations (in my sense), even if our attempts to provide them don't seem as urgent because they aren't motivated by thorny conceptual difficulties.

1.2   Foundational Physics

My central concern here is with the interpretation of foundational theories. A foundational theory is, roughly, one that aims to describe the primitive features of the physical world. If we want to know what the world is ultimately like, foundational physics provides science's best answer. General relativity and the standard model of particle physics most likely count as foundational theories, but many discarded

---

[4]Part of this mapping presumably specifies how some aspects of a formalism map onto empirically accessible – i.e., 'observable' – physical features, and this generally plays an important role in the development of the theory. (How else to apply and test it as it's constructed?) Because this portion of the mapping isn't relevant to my discussion, I'll use the term 'interpretation' to pick out that portion mapping to empirically inaccessible – 'theoretical' – physical features.

[5]This notion of interpretation bears many similarities to the one championed by the positivists. They are not identical, however. The interpretation of a theoretical formalism ought to tell us how the mathematical structures encoding the theory's laws and equations are to be understood physically – as constraints? as expressing causal relationships? – and this has no analogue in the linguistic notion. Precisely what is required for this notion of an interpretation to be 'complete' or 'exhaustive' is a complicated question; fortunately the intuitive idea will be sufficient for my purposes.

theories were once taken to be foundational, classical particle mechanics and classical electromagnetism among them. These are the two theories from which many of the examples in this dissertation are drawn. In using them to extract philosophical morals, I mean to treat them as foundational. Unless otherwise noted, my talk of *theories* is intended as talk of *foundational* theories.

However the notion of a foundational theory gets specified precisely, it embodies an implicit distinction between those features of the world that are primitive and those that are not. This ontological distinction is worth developing further. Someone who believed both genetic theory and physical chemistry would be happy to accept that genes and atoms each exist, but she would presumably balk at the suggestion that genes exists 'alongside' or 'in the same way as' atoms. After all, genes are composed of atoms – they depend for their existence on the existence of the atoms constituting them – whereas the converse does not hold. Genes have a different ontological status than atoms. It is, in part, the asymmetrical nature of this existential dependence that is picked out when one says than an entity is more fundamental than another.[6]

Atoms are more fundamental than genes, although according to our best science they are not fundamental. They, too, are built out of other physical things. We may never be certain when we've hit ontological bedrock, but the theories of fundamental physics are our best hope.

However exactly the ontological distinction is drawn, three salient features can be identified. First, not all elements of a theoretical formalism typically count as ontologically primitive. Consider the case of classical gravitation theory. As it is (and was) widely understood, its primitive entities include point particles and gravitational forces. The vector $\mathbf{F}_{\mathrm{G}}$, representing the gravitational force between two particles, represents a primitive feature of the world. The same cannot be said of the vector $\mathbf{F}_{\mathrm{net}}$, which represents the net force acting on a given particle. $\mathbf{F}_{\mathrm{net}}$ is a distinct math-

---

[6]Here and throughout I treat 'foundational' and 'fundamental' as fungible. For a skeptical attitude towards this notion, see Schaffer (2003).

ematical object from the individual force vectors, yet it is ontologically primitive.[7] (Here I assume $\mathbf{F}_{net} \neq \mathbf{F}_{G}$.) The vector $\mathbf{F}_{net}$ clearly represents the combined result of various gravitational forces, each taken to be ontologically primitive, but $\mathbf{F}_{net}$ doesn't itself represent some additional ontological posit over and above those gravitational forces. It is a derivative element, like genes. Mere membership in the formalism of an (erstwhile) foundational theory, then, is no indication that something represents an ontological primitive. Of course, this is not to deny that the net force on a particle is perfectly *real*. Indeed, derivative features of a theory's ontology – e.g., tables and chairs – often seem the most real in our everyday lives.

Second, the distinction between primitive and derivative entities extends to properties and higher-order relations as well. Even if, like mass, position-at-a-time is a primitive property of point particles in classical gravitation theory, velocity and acceleration are generally not taken that way.[8] A particle's velocity is ontologically dependent upon the more primitive property of position-at-a-time, and similarly for higher-order physical properties like those represented by $\mathbf{a}$, $\frac{d^2\mathbf{x}}{dydt}$, etc.[9]

Similar considerations apply to a host of mathematical objects often introduced into the classical dynamical formalism. Indeed, most objects definable within a theory's formalism – including things like momentum and other 'dynamical variables' – are typically not ontologically primitive. Many textbooks encourage the thought that we can introduce whatever dynamical variables we like, however gerry-mandered, though some, like momentum and kinetic energy, turn out to be more useful than others.[10] Such permissiveness would be quite odd if the introduction of new math-

---

[7]This is, in any case, one common interpretation of the primitive ontology of Newtonian gravitation theory. I do not mean to suggest that it is the only one or even the best.

[8]But see Sklar (1977) for the suggestion that particle acceleration might best be interpreted as a primitive property. See Skow (2007) for a recent reply.

[9]The notion of a primitive property would thus seem to be quite different from that of an *intrinsic* property, as position-at-a-time is generally taken to be a primitive property, but not an intrinsic property, of particles.

[10]See, e.g., José and Saletan (1998). The existence of energy as a primitive in classical electromagnetism is defended in Lange (2002), and Truesdell (1968) suggests reasons to think a primitive notion of momentum may be required in Newtonian mechanics.

ematical objects were taken to enlarge a theory's ontology. These new dynamical variables, like new modes of kinematical description, do not change the underlying description of the physical world offered by a theory. In fact, as long as they are well-defined and taken to represent derivative features of the world, we are free to introduce whatever additional mathematical objects we like into a formalism.[11]

Third, the distinction between primitive and derivative ontology is not reflected in the mathematical structure of a theory. A formalism provides no indication as to which of its elements and structures represent ontologically primitive features. Sorting that out is a central part of what it is to engage in theory interpretation.

1.3   The Problem of Ontological Interpretation

Let us call an *ontological interpretation* a specification of those features of a formalism representing ontologically primitive and derivative aspects of the physical world – a sorting into lists of primitive and derivative components. This notion is included in, but is more restrictive than, the general notion of interpretation with which we began. Many interpretations of quantum mechanics take the wave-function to be ontologically primitive, for example, but they also go beyond my notion of ontological interpretation in saying exactly *what sort of primitive thing it is* that the wave-function represents.

That the distinction between primitive and derivative ontology is not reflected in the mathematical structure of a theory raises a question: given that a (foundational) theory purports to account for phenomena in terms of ontologically basic constituents of the world, what are those constituents? Which features of a formalism purport to represent ontological primitives? *Prima facie*, this question doesn't seem to have a

---

[11]When part of a theory's formalism is taken to *represent* an ontologically primitive (or derivative) feature of the physical world, I will often write that the mathematical object or structure in question is itself primitive (or derivative). Strictly speaking, the primitive–derivative distinction applies only to physical features – mathematical objects are mathematical objects are mathematical objects.

univocal answer. Different ontological interpretations seem possible for a theoretical formalism. Yet in the absence of some way of addressing this question, we have no way to determine what foundational physics is telling us about the basic structure of the world.[12]

Our inability (in some cases) to justify a particular ontological interpretation as correct raises what I will call the *problem of ontological interpretation*. It's a problem that arises for foundational theories, when it does, simply in virtue of the role mathematics plays in representing physical content. Foundational theories face this problem to varying degrees. Most interpretations of quantum mechanics, for example, take the wave-function to be ontologically primitive – that's not what's at issue in the measurement problem.[13] What's at issue there is exactly what sort of primitive thing the wave-function represents. The problem of ontological interpretation is thus a type of interpretative problem distinct from the various theory-specific conceptual problems associated with, say, quantum mechanics or general relativity. Nonetheless, we'll see that it shows up in one degree or another for most, and perhaps all, foundational theories. It is the relationship between the problem of ontological interpretation and issues in scientific realism that provides the leitmotif of this dissertation.

1.4 Approaches to Scientific Realism

Scientific realism has been understood as meaning different things to different people. Although it's widely agreed that realists maintain an epistemically robust pro-attitude towards scientific activity or successful products of scientific activity (e.g., theories), much room for variation remains.[14] The notion of scientific realism

---

[12]An ontological interpretation is a necessary condition on this, not a sufficient one. As the measurement problem in quantum mechanics makes abundantly clear, an ontological interpretation is not always enough for us to understand the theory's physical content.

[13]Unless, perhaps, one endorses a hidden-variables theory. Such an approach *could* be associated with an ontological interpretation taking the wave-function as ontologically derivative.

[14]Unless otherwise noted, 'realism' and its cognates will pertain to *scientific* realism.

that will be at issue in this dissertation is, in extremely rough terms, the following:

> Our best and most successful foundational physical theories are approximately correct descriptions of the physical world.[15]

I will not be concerned with realism in general, then, but realism about foundational physics.[16]

Multiple aspects of this characterization warrant clarification: What is meant by approximate truth, and how is it different from simple falsehood? By what measure does one judge the success (or degree of success) of a theory? These questions will go unanswered, as the issues they raise are too broad for an adequate treatment here. It is my hope that the intuitive notions behind these phrases will suffice for the arguments set out in this dissertation.

One issue, however, demands attention. The realism at issue here is a realism that is, in some sense, about *existing* science. The imagined realist endorses *something* about the current state of science. It thus makes sense to ask, for a theory T reasonably similar to the theories of existing science, what her realism applied to T amounts to – that is, what sort of world the realist about T would be committed to, on her account.

Other attitudes are certainly possible. One could, for example, view realism as a claim about science 'in the limit' of empirical inquiry. No particular attitude at all

---

[15]I won't be concerned here with semantic or metaphysical realism, just epistemic realism. I'll assume, that is, that there's a mind-independent world that isn't the product of elaborately engineered 'social construction', and also that discourse about empirically inaccessible or 'theoretical' things is meaningful in just the same way that discourse about the immediate macroscopic world around us is meaningful. See Psillos (1999).

[16]It is this author's opinion that the conditions under which the scientific realism debate is discussed are often much too general, and that the literature has stagnated, in part, on account of a failure to appreciate how the realism debate takes different forms in the context of different fields of science.

about current theories would follow from such a realism. Indeed, one might think this form of realism is distinctly fitting in light of this dissertation's central theme. For if the realist only endorses theories 'in the limit of inquiry', then there may well be no relationship at all between theory interpretation and scientific realism. Why think that, in the limit, all the interpretative questions won't get ironed out?

In defense of my choice of realism I offer not an argument, but two related observations. First, many contemporary scientific realists are of the first sort.[17] Thus it makes sense to explore how issues of theory interpretation bear on a widely held form of realism. Second, a central appeal of scientific realism is that it seems to offer a way of accounting for the success of existing science, and for justifying the idea that electrons, atoms, and molecules (say) really exist. The limit approach offers no such comfort, as it makes no claim regarding the relationship between current science and the truth. It thus makes sense to see how well an appealing form of realism fares before retreating.

1.5 Methodology, Reformulation, and Underdetermination

This dissertation is organized around three topics, each of which explore aspects of the relationship between ontological interpretation and scientific realism.

I first consider the role scientific methodology plays in a prominent naturalistic defense of scientific realism: the no-miracles argument. The most sophisticated form of this argument, due to Richard Boyd and Stathis Psillos, rests on the broad

---

[17]This is perhaps owning to the popularity of naturalism amongst current scientific realists. In refusing to pass judgment on current science, the limit-based approach sits less comfortably with a naturalistic point of view.

claim that successful scientific methodology is irredeemably theory dependent. I examine the nature of this theory dependence within the context of the history of foundational physics – in particular, within the context of Maxwell's development of electromagnetism – and argue that the presence of competing ontological interpretations undermines the realist's attempt to draw sweeping epistemic conclusions from methodological success.

I next consider the concept of theoretical reformulation and how it applies to the claim – widespread in the physics literature on classical dynamics – that Lagrangian dynamics is a reformulation of (part of) Newtonian dynamics. How exactly is this claim to be understood, particularly in light of the fact that the world is non-classical? And what can this tell us about the concept of theory reformulation itself, which plays a central role in assessing various arguments against scientific realism? I provide an analysis of theory reformulation in terms of counterfactual interpretative judgments, and then cast doubt on its justification in the classical case by developing a non-Newtonian interpretation of Lagrangian dynamics.

Finally, I consider the extent to which the presence of competing ontological interpretations raise underdetermination difficulties for realism about foundational physics. After first re-formulating the underdetermination argument so as to avoid recent objections to the effect that underdetermination fails to pose any distinctive threat to realism, I suggest a formulation of realism – somewhere between structural realism and 'standard' realism – that vitiates the underdetermination threats potentially posed by competing ontological interpretations.

1.6 A Terminological Disclaimer

As a final introductory comment, I should note a prevalent ambiguity in the literature concerning the term 'theory'. There is the sense of *theory* in which one might say, "Alice and Bob believe the same theory, but interpret it in different ways". There is also the sense of *theory* in which one might say, "Alice and Bob disagree about what the world is like, and thus believe different theories". The former use corresponds more closely to related terms like 'formalism' and 'mathematical framework'. I will generally rely on context to disambiguate.

CHAPTER II

Realism, Maxwell, and Scientific Methodology

Hilary Putnam first gave voice to the modern day no-miracles argument for scientific realism:

> The positive argument for realism is that it is the only philosophy that doesn't make the success of science a miracle. That terms in mature scientific theories typically refer...that the theories accepted in a mature science are typically approximately true, that the same term can refer to the same thing even when it occurs in different theories – these statements are viewed by the scientific realist not as necessary truths but as part of the only scientific explanation of the success of science, and hence as part of any adequate scientific description of science and its relations to its objects.[1]

The underlying intuition is appealing and continues to motivate many contemporary discussions of scientific realism.[2] Given the rather dramatic and unexpected empirical success of our best physical theories, the thought is, it would be astonishing if they didn't get some things right (or some things roughly right) about what the unobservable world is like.

The no-miracles argument for realism has been given its most sophisticated and systematic defense by Richard Boyd, for whom the abductive considerations on which it's based form one piece of a larger naturalistic framework for understanding the

---

[1]Putnam (1975, p.73)

[2]Recent realists motivated by considerations of this sort include Leplin (1997), Psillos (1999), and Devitt (2005).

epistemology and semantics of science.[3] If realism is the most appropriate attitude towards our best science, he argues, this is true in virtue of contingent facts about actual scientific practice. Despite the justified interest this position has received, little has been done to assess its plausibility in the context of particular sciences. *Prima facie*, though, it is not obvious that our aversion to miracles ought to present a uniformly compelling case for realism across all scientific disciplines. Given the trenchant and persistent interpretative problems in foundational physics, for example, even someone broadly sympathetic to abductive considerations might justifiably wonder whether the no-miracles argument provides as compelling a case for realism about foundational physics as it does for, say, biology.

This chapter considers the plausibility of Boyd's abductive defense of scientific realism in the context of foundational physics. After a preliminary discussion of his argument and the role of scientific methodology in it, I examine how it applies to a central (but philosophically neglected[4]) episode of 19th-century theory construction in foundational physics: Maxwell's use of the Lagrangian mathematical framework in the development of classical electromagnetic theory.[5] This episode, I argue, constitutes a counter-example to Boyd's fundamental characterization of scientific methodology, particularly in light of Maxwell's interpretative attitude towards the Lagrangian framework itself, and (what is more) this counter-example is distinguished in that it's couched entirely within his naturalistic framework.[6] Because the characteristic in-

---

[3]Boyd (1973), Boyd (1979), Boyd (1981), Boyd (1983), Boyd (1984), Boyd (1985), Boyd (1989), Boyd (1990), and Boyd (1992).

[4]A notable exception is Morrison (2000), who appeals to Maxwell's circuitous route to electromagnetism in her insightful book on explanation and unification. Maxwell, of course, is a common historical figure in discussions within the philosophy of science. Such discussions are rarely focused on the Lagrangian underpinnings of his account, though.

[5]Appendix A provides a brief overview of the Lagrangian formalism. I have tried whenever possible to keep the exposition and philosophical discussion non-technical, although the details of the Lagrangian framework are unavoidable in sections V and VI.

[6]One trade-off here is that I remain silent on a number of aspects of Boyd's argument that warrant further scrutiny. However, many controversial features of Boyd's naturalism are well documented in the literature – see especially Fine (1986a) and Fine (1986b) – whereas to my knowledge few objections have been raised from *within* his naturalistic framework. Laudan (1981) is perhaps an

terpretative features of this case generalize, at least in part, to other methodological aspects of foundational physics, as I argue in the final section, there are good grounds for rejecting his no-miracles defense as an argument for blanket realism about foundational physics.

2.1   Miracles and Methodology

Boyd's formulation of the no-miracles argument has its origin in the seemingly innocuous observation that scientific methodology – that is, the tools and techniques used in constructing and evaluating theories – is as a matter of empirical fact remarkably *instrumentally reliable*. Not only are our best theories successful in accounting for physical phenomena, as Putnam emphasized, but the *methods* used in constructing those theories reliably lead to empirically successful theories. They are, as Boyd puts it, "reliable guide[s] to the acceptance of theories which are themselves instrumentally reliable".[7] This is a claim most anti-realists, or at least most empiricists, would presumably endorse.[8] How is this rather suprising fact about scientific methodology to be explained?

What effects the transition from instrumental to theoretical commitment is the observation, attributed to Kuhn (1962), that scientific methodology is profoundly theory dependent. The experimental and inferential practices used in developing new theories are "grounded in" prior theoretical commitments.[9] In light of this, Boyd

---

exception.

[7]Boyd (1985, p.4). Boyd's shift from the empirical success of *theory* to scientific *methodology* is motivated by the opinion that Putnam's original argument, while a compelling reason to accept realism, fails to diagnose the errors in standard empiricist arguments (the underdetermination argument, in particular). For reasons that would take us too far afield, Boyd takes his version of the abductive argument to provide a more satisfactory reply to the empiricist. See, e.g., Boyd (1983, pp.54–56, 66–67). The relationship between the underdetermination argument against scientific realism and the problem of theory interpretation in foundational physics is discussed further in chapter 4.

[8]Boyd's naturalistic defense is also intended as a reply to *constructivist* anti-realists, such as Kuhn, although that feature of Boyd's argument is beyond the scope of this paper. See Boyd (1992).

[9]Boyd (1990, p.217). See also Boyd (1981, p.618).

argues, the only adequate explanation for the instrumental reliability of methodology is the approximate truth of the relevant background beliefs on which those methodological judgments are based:

> [T]he reliability (instrumental or theoretical) of scientific methods at a given time will typically be explicable only on the assumption that the existing theoretical beliefs which form the background for its operation are (in relevant respects) approximately true. The basic idea...is that theoretical considerations are so heavily and so crucially involved in the operation of actual scientific method that the only way to explain even the *instrumental* reliability of that method is to portray it as reliable with respect to theoretical knowledge as well.[10]

Denying realism in the face of the instrumental reliability of theory dependent methodology, then, is tantamount to conceding that there is no good explanation for that reliability; or, alternatively, that such reliability is a bonafide miracle – a consequence most epistemic agents would presumably prefer to avoid.

There is an air of circularity to this argument. Are not the epistemic justifications of explanatory considerations part of what's at issue here? Many empirically-minded anti-realists, for example, have thought that abductive reasoning properly belongs to the realm of pragmatics and useful heuristics. To justify realism on the grounds that it's the most explanatory thesis is for them no epistemic justification at all.[11] We should recognize, though, that the no-miracles argument is couched within a broader framework of philosophical naturalism, according to which the epistemology and semantics of science are to be treated as empirical sciences themselves. The idea

> is to use the ordinary methods of science to investigate the question of why the methods of science are instrumentally reliable. The philosophical methods here are not conceived of as *prior to* scientific methods in any sense...The principles of inference by which the realist defends realism will be no more stringent than the principles of inference whose reliability the realist is trying to explain.[12]

---

[10]Boyd (1981, pp.617–618)

[11]These difficulties are discussed, among other places, in Fine (1984b), Fine (1986a), and van Fraassen (1980, pp.19–25, 31–40). See van Fraassen (1980, Ch.5) for an empiricist defense of the merely pragmatic value of explanation, and van Fraassen (1989, Chs.6–7) for the stronger claim that abductive considerations are incoherent when taken as the basis for rational norms of inference.

[12]Boyd (1985, p.33)

While admitting that, in isolation, the abductive considerations necessary for the no-miracles argument render it circular, Boyd's contention is that what ought to be evaluated is the entire naturalistic "package".[13] Realism forms only one part of that approach, and he marshals additional reasons (reasons independent of realism) to accept that package as a whole, e.g., arguments against knowledge internalism[14] and against the stability of the empiricist's alleged middle ground between realism and radical Cartesian skepticism.[15] What Boyd concedes is only that his realist defense is circular when taken out of its proper philosophical context – a context that the empiricist is of course free to reject, but not without substantive argument. Properly understood, what the no-miracles argument claims to show is that realism is the only naturalistically-acceptable view of science.

2.2 Projectability and Theoretical Plausibility

Boyd's argument is developed through an analysis of central scientific methodologies. My focus here is *projectability*, the methodology most closely connected with theory construction. It is also the methodology most systematically developed throughout his work and the one he takes to be most relevant for addressing empiricist arguments for anti-realism.[16]

Following Goodman (1983)'s use of the term, projectability is the method by which scientists decide which conceivable theories are genuine candidates for (incre-

---

[13]See Boyd (1983, pp.70*ff*), Boyd (1984, pp.61*ff*), and Boyd (1990, pp.248–253). Devitt (2005, p.774) disputes whether the realism debate is really over abductive reasoning, and so denies that the no-miracles argument is circular.

[14]On the naturalistic approach the epistemic justification of our abductive practices, particularly concerning the instrumental reliability of scientific methodology, turns out to be an entirely contingent fact about the world. Realism is thus not an inevitable byproduct of our methodological practices; scientific methodologies are not bound to be instrumentally reliable. Rather, if Boyd is right, we as epistemic agents are extremely lucky to live in the world that we do.

[15]A similar criticism is leveled in Railton (1989).

[16]In particular, the underdetermination argument. See, e.g., Boyd (1973) and Boyd (1990, pp.224, 227–228).

mental) confirmation. It is a well-known fact about empirical inquiry that we are only ever in possession of a finite amount of observational data, and thus that there will always be an indefinite number of competing theories fitting that data. Yet from this vast theoretical cornucopia only a handful are ever taken seriously by the scientific community. Only a handful, that is, are thought to be projectable. The rest simply aren't taken to be evidentially supported (or incrementally confirmed), despite their obvious compatibility with the available evidence.

The preceding description only serves to identify the methodology at issue, not provide an account of how it functions. However projectability judgments are made, though, they contribute in a fairly straight-forward way to the empirical success of the theories we accept. They are responsible for reducing an infinite class of epistemic possibilities down to a manageable few, and thus clearly play an important role in determining which theories are ultimately accepted. Insofar as the theories we accept are instrumentally reliable, the methodology of projectability is, too.

Boyd's analysis is that judgments of projectability are, in effect, judgments of *theoretical plausibility*:

> [w]e, in fact, take seriously only those theories which relatively closely resemble our existing theories in respect of their ontological commitments and the laws they contain. We prefer theories which quantify over familiar 'theoretical entities' – or at least entities very much like familiar ones (or, in some cases, appropriate constituents of familiar entities); we prefer theories which predicate of theoretical entities familiar properties – or at least properties like familiar ones; we prefer new theories whose laws are – if not consistent with those we have previously adopted – at least compatible with the maintenance of most of our previously accepted laws as approximations. Generally, we reject outright any proposed theory which contradicts the laws we consider best confirmed unless a real crisis is at hand – and even then we will strongly prefer new theories which preserve the old laws as approximations.[17]

So analyzed, Boyd's argument is that we can't explain how projectability could be as instrumentally reliable as it is unless the relevant theoretical commitments implicit

---

[17]Boyd (1981, pp.618–619)

in that methodology – implicit in those judgments of theoretical plausibility – were approximately true.

To be sure, this account of projectability is a caricature of actual scientific practice. Judgments of theoretical plausibility are unlikely to be reflected often in the explicit reasoning of individual scientists or scientific communities. But Boyd is not suggesting that the first stage of theory construction is the tedious enumeration of hypotheses consistent with the available data, to which the method of projectability is then applied. His analysis purports to give an explicit characterization of a largely implicit dimension of scientific practice. Projectability, Boyd writes, "constrains us, *prima facie*, to accept only theories whose laws and ontologies closely resemble the laws and ontologies of theories already accepted."[18] Regardless of whether such a methodology is consciously adopted, then, the adequacy of the analysis can be judged by looking at its proposed effects; at the types of theories constructed, for example. If Boyd is right the frameworks used in the construction of new theories ought to reflect, however implicitly, existing theoretical commitments about what the physical world is like.

2.3 Essential Theory Dependence

Theory dependence plays an important role in Boyd's methodology-based version of the no-miracles argument. The realist's abductive appeal gains whatever explanatory purchase it has precisely in virtue of the role background theoretical commitments have in guiding methodological judgments.[19] If scientific methodology turned out to be theory-neutral the realist would be powerless to account for its instrumental reliability, for the truth of background theoretical claims would simply have no ex-

---

[18]Boyd (1981, p.621). The *prima facie* qualification is very important, as Boyd certainly wants to allow for the possibility of radical conceptual change (Boyd, 1981, pp.655–658). His point is that whenever possible we stick to the theoretical commitments we've already got when constructing new theories.

[19]Boyd (1990, pp.222–223)

planatory relevance. It would also be a rather surprising result, yet it's precisely this claim I wish to defend in the remainder of this chapter, at least with respect to some central and historically important cases of projectability judgments in foundational physics. To make good on this claim I must first say a bit more about the notion of theory dependence itself.

Boyd takes the realist to be offering a "perfectly ordinary causal" explanation.[20] Our background theoretical commitments, when approximately true in the relevant respects, cause us to make methodological judgments that then lead to empirical success. Clearly, this explanation makes sense only if the notion of theory dependence is itself understood causally, for that dependence is what establishes the relevant explanatory connection between our background beliefs and our methodological judgments. So to say that a class of methodological judgments is theory dependent is, on first pass, to say that the way those judgments are made is *caused* by certain background beliefs held by the relevant agents.

A mere causal connection between an agent's background beliefs and her subsequent methodological judgments, though, is too weak a notion of theory dependence to ground Boyd's explanatory thesis. The causal chain leading up to a given agent's methodological judgment undoubtedly contains many beliefs whose truth is patently irrelevant to the instrumental reliability or success of the resulting judgment – for example, the belief (perhaps) that certain sorts of methodological judgments are looked upon more favorably by those who control funding, or that certain judgments may result in simpler mathematical calculations. Although not stated in these terms, Boyd's argument requires the stronger claim that methodologies be *essentially* theory dependent: background beliefs must not only figure in the actual causal chains leading up to particular methodological judgments, but relevant changes in those beliefs must be correlated, in some appropriate way, with changes in the resulting judgments. It

---

[20]Boyd (1990, p.239)

must be the case that our overall methodological judgments would have been different had the relevant background beliefs been different – that our methodological judgments would have been unmotivated or inexplicable for an agent with very different theoretical commitments. Here what counts as motivated or explicable, and how the counterfactual gets evaluated more broadly, is to be determined in accordance with the naturalistic standards set out by Boyd. The hypothetical agents in question, for example, presumably share the same empirical and intellectual goals, and have a shared set of beliefs about what the observable world is like.

Note that this counterfactual concerning essential theory dependence is about a generic agent and not some specific historical figure. Whether a particular agent would have made a given methodological judgment had her background beliefs been different may tell us very little about the methodology in question and much more about the psychology and historical context of that agent. In the absence of particular background beliefs, for example, an agent might very well have chosen to pursue an entirely different career. (Here one thinks of Schrödinger's quip to Bohr that "[i]f all this damned quantum jumping were really to stay, I should be sorry I ever got involved with quantum theory."[21]) In the context of Boyd's argument and the notion of essential theory dependence, our interests concern the relationship between a methodology *in general* and relevant background beliefs. This relationship is best articulated via a counterfactual about 'similar' agents, however difficult it is to specify exactly what that similarity amounts to.[22]

Only if such counterfactuals hold – only if scientific methodology is *essentially* theory dependent – is Boyd in a position to argue that the approximate truth of

---

[21]This quote is taken from Heisenberg (1955).

[22]Concerning the general evaluation of counterfactuals within a naturalistic framework, Boyd writes: "It is philosophically challenging to give a general account of the nature of... comparisons with counterfactual possibilities, but such comparisons are so routine a feature of ordinary causal reasoning in science (including reasoning about the reliability of particular methods) that there is no reason to suppose that they raise difficulties in the present context" (Boyd, 1990, p.239).

background beliefs best explains the instrumental reliability of methodology. For only then is there a plausible case to be made that general applications of methodology (as opposed to isolated judgments by specific historical agents) are so closely connected with the content of background beliefs that their overall reliability depends upon the approximate truth of those background theoretical commitments. On a weaker notion of theory dependence the causal connection between background beliefs and methodological judgments is simply too loose to ground Boyd's explanatory claim. Background beliefs may cause certain methodological judgments in particular agents, but that alone gives us no reason to attribute the success of those judgments to the *content* of those beliefs.[23]

Essential theory dependence applies to projectability in a straight-forward way. Projectability judgments concern considerations associated with theory construction and development, such as the choice to begin with a particular theoretical framework or to represent observable phenomena in a particular way. The choices are essentially theory dependent if and only if background beliefs causally influence judgments of projectability in such a way that similar agents with different background beliefs would generally have made different judgments about how to go about constructing and developing theories. Thus the question of whether a specific projectability judgment depends essentially on a given theoretical belief is a question of whether it would have been explicable for a similar agent to have made the same judgment concerning theory development in the absence of that commitment.

Boyd's analysis of projectability provides a preliminary reason to think that methodology is in fact essentially theory dependent. If our (implicit) judgments concerning the appropriate techniques for constructing and developing new theories *just are* reflections of our prior theoretical commitments about the types of hypotheses

---

[23]In at least some places Boyd suggests that essential theory dependence is the sort of causal relation he has in mind, although a precise characterization remains elusive. See, e.g., Boyd (1990, pp.222–223, 239).

and ontologies we take to be plausible, it seems rather likely that variations in those plausibility judgments will cause agents to adopt different methods of theory construction and as a result different theories. Given that the projectability judgments actually made in the history of physics have proven themselves to be astonishingly reliable guides to the construction of empirically successful theories, Boyd appears to be on firm ground in claiming that the only naturalistically-acceptable explanation of this is his broadly realist thesis.

2.4 Maxwell's Lagrangian Framework

I wish to challenge this argument. I do not doubt that realism provides a naturalistic explanation of the instrumental reliability of essentially theory dependent projectability judgments, but rather that not all cases of instrumentally reliable projectability judgments are essentially theory dependent. There are important cases of theory construction in the history of physics – cases that are unquestionably instrumentally reliable – that don't depend on background theoretical commitments in the requisite way. As a consequence Boyd's argument fails to establish a broad-based realism about foundational physics. Any realist defense of foundational physics, even a naturalistic one, can at best proceed in a piecemeal way.[24]

My argument proceeds in two stages. In this section I provide an historical analysis of an important case of theory construction from 19th century physics: Maxwell's development of electromagnetism. This is a particularly appropriate historical episode to consider. First, it's the origin of Maxwell's equations and thus presents in some sense the birth of classical electromagnetic theory – a theory striking in its empirical success. Second, Maxwell's writing reflects a particular sensitivity to questions

---

[24]In this sense I don't intend to suggest that Boyd's argument can never ground realist beliefs about foundational physics, at least within a naturalistic framework. On the view developed here Boyd's realist thesis must be evaluated on a case-by-case basis, and thus his *global* argument for scientific realism fails.

of methodology, illuminating background theoretical commitments he thought relevant in constructing electromagnetic theory.[25] Third, the features making this case methodologically interesting, features connected with the distinction between mathematical representation and physical reality, are characteristic of a central technique of theory construction in foundational physics. In spite of Maxwell's acknowledged commitment to the Newtonian ontology of force and matter, I show that the central methodological considerations he invoked were independent of those background commitments. A comparable agent with quite different ontological beliefs could nonetheless have been motivated to make the same projectability judgments. This shows, in the first instance, that not all projectability judgments are judgments of theoretical plausibility. That we can thereby conclude that these judgments are not essentially theory dependent is then defended in the next section. I conclude by arguing in the final section that the methodological lessons gleaned from Maxwell generalize to an important class of projectability judgments in foundational physics, and thus that this methodological technique cannot be dismissed as an isolated historical anomaly. It forms a central piece of the history of successful scientific methodology.

Maxwell's theory of electromagnetism, codified in those equations we now call 'Maxwell's equations', is given its most comprehensive presentation in his 1873 *Treatise on Electricity and Magnetism*.[26] Despite the strong experimental confirmation

---

[25]Chalmers (1973) is an insightful discussion of Maxwell's beliefs about scientific methodology.

[26]Maxwell (1954a) and Maxwell (1954b). In modern notation Maxwell's equations are:

$$\nabla \cdot \mathbf{E} = \frac{1}{\epsilon_0}\rho \qquad \nabla \cdot \mathbf{B} = 0$$
$$\nabla \times \mathbf{E} = -\frac{\partial \mathbf{B}}{\partial t} \qquad \nabla \times \mathbf{B} = \mu_0 \mathbf{J} + \mu_0 \epsilon_0 \frac{\partial \mathbf{E}}{\partial t}$$

$\mathbf{B}$ is *now* taken to represent the magnetic field, $\mathbf{E}$ the electric field, ρ the charge density, and $\mathbf{J}$ the current density. ϵ_0 and μ_0 are constants – the *permitivity* and *permeability* of free space, respectively. To these equations we often add a fifth, the Lorentz force law $\mathbf{F} = q(\mathbf{E} + \mathbf{v} \times \mathbf{B})$, which gives the electromagnetic force on a body of charge q and velocity $\mathbf{v}$ in the presence of the electric and magnetic fields. However, for reasons that will be important below, this last equation was *not* part of Maxwell's account. As Chalmers (1973, p.109) notes, Maxwell was resistant to the idea of taking either fields or charges as primitive. Maxwell's equations as formulated here are not the equations Maxwell himself formulated – he had 26 in total – but they are mathematically equivalent to his. The equations discussed in this section have all been re-expressed in modern mathematical notation.

of certain phenomenological laws central to classical electromagnetic theory, at the time of publication the nature of electricity and magnetism was not well understood. Coulomb's law of electrostatic force was widely accepted as an accurate description of certain *observable* phenomena, for example, even though there was no agreement as to the ontology of charge itself.[27] It was similarly an open question whether electric current consisted in the flow of one 'fluid' or two (in the latter case each moving in opposite directions) and whether these fluids could be identified with magnetic fluids (again, either one or two).[28] The means or mechanism by which electromagnetic effects propagated through space was also a source of widespread speculation. Faraday posited *primitive* physical force lines distributed through space, whereas many Continental physicists were happy appealing to basic action-at-a-distance notions.[29]

In two papers preceding the *Treatise* Maxwell had sought to model electromagnetic effects by attributing the observable phenomena to interactions between underlying forces and matter. In his 1856 "On Faraday's Lines of Force" he sought to understand the nature of electrostatic attraction and repulsion, and the relationship between magnetism and electric current (electromagnetic induction), by conceiving of these effects as due to the motion of underlying incompressible fluids flowing through space.[30] In "On Physical Lines of Force," published roughly six years later, this mechanical depiction was replaced with a collection of etherial vortices distributed through space, the rotations of which were taken to represent magnitudes of magnetic strength.[31]

---

[27]In its simplest form Coulomb's law states

$$\mathbf{F} = \frac{1}{4\pi\epsilon_0} \frac{q_1 q_2}{r^2} \hat{\mathbf{r}},$$

where $\mathbf{F}$ is the electrostatic force between charges q_1 and q_2, and r is the distance between them. $\hat{\mathbf{r}}$ is a unit vector pointing from one charge to the other.

[28]There was certainly no theory of current as the motion of discrete units of negative charge, which wasn't developed until at least 1881.

[29]See Darrigol (2000, Chs.1–2) for a discussion of action-at-a-distance accounts, which were particularly popular in France and Germany.

[30]Maxwell (1952a)

[31]Maxwell (1952b). As Morrison (2000, p.65*ff*) points out, there are good grounds for thinking Maxwell did not take these models to be accurate representations of what was *really* going on at

Maxwell's approach to theory construction changes dramatically in the *Treatise*. He abandons any effort to model phenomena based on underlying interactions between force and matter, adopting instead the abstract Lagrangian framework in which *energy* is taken (at least by Maxwell) as the central and primitive notion.[32] The point of shifting to the Lagrangian formalism was not that it offered a more convenient mathematical apparatus for bringing the Newtonian world-view to bear on electromagnetic phenomena. The shift reflected a new methodological attitude: the theory of electromagnetism was to be constructed without speculating about the ontological basis of electromagnetic phenomena itself. This may appear to conflict with Maxwell's stated belief in an underlying Newtonian ontology, but therein lies the novelty of the new framework.[33] It allows him to represent electromagnetic phenomena *without* first speculating about its underlying ontology, a problem that had plagued theories of electromagnetism (including his own) up to that point. It thus provided a methodological framework for theory construction consistent with his existing Newtonian beliefs about the ontological underpinnings of electromagnetic phenomena, but which didn't make use of them in the process of theory construction itself.

That Maxwell actually used the Lagrangian formalism to this methodological end is an interpretative claim requiring defense. Early in volume one of the *Treatise* he raises the question of what electricity *is*, commenting:

---

the fundamental level, although it's clear he was committed to an underlying Newtonian picture. See also Chalmers (1973, pp.111–113). Note that Maxwell's skepticism didn't reflect a general anti-realism about science. He's unconcerned, for example, about inductive reasoning or the status of phenomenological laws.

[32]The mathematical structure of the Lagrangian formalism is outlined in appendix A. Maxwell's representation of electromagnetic phenomena within the Lagrangian framework actually first occurs in a third paper, "A Dynamical Theory of the Electromagnetic Field" (Maxwell, 1952c), although the complete theory isn't developed until the *Treatise*. The concept of energy did not play a role in Lagrange's original 1788 formulation of analytical mechanics (Lagrange, 1997). Thomson and Tait are responsible for giving the Lagrangian approach its energy-based formulation – a formulation that influenced Maxwell heavily – and Moyer (1977, pp.257–264) argues that it was this development that made possible its application to electromagnetic phenomena.

[33]Even after the publication of the *Treatise* he remained committed to the goal of reducing electromagnetic phenomena to interactions between force and matter. See Chalmers (1973, pp.154–160) and also Moyer (1977, p.266).

> The electrification of a body is therefore a physical quantity capable of measurement, and two or more electrifications can be combined experimentally with a result of the same kind as when two quantities are added algebraically. We therefore are entitled to use language fitted to deal with electrification as a quantity as well as a quality, and to speak of any electrified body as 'charged with a certain quantity of positive or negative electricity'.
>
> While admitting electricity, as we have now done, to the rank of a physical quantity, we must not too hastily assume that it is, or is not, a substance, or that it is, or is not, a form of energy, or that it belongs to any known category of physical quantities. All that we have hitherto proved is that it cannot be created or annihilated, so that if the total quantity of electricity within a closed surface is increased or diminished, the increase or diminution must have passed in or out through the closed surface.[34]

This is a striking retreat from the definition of charge offered in his 1862 vortex model, and it reflects his reservations about the role of speculative hypotheses in the process of theory construction. The only definitive physical property Maxwell does attribute to electric current in the *Treatise* is kinetic energy, but this claim is grounded experimentally in the fact that currents (in material circuits) can be made to perform work. An important consequence Maxwell draws from this is that currents must involve motion of some kind, for all kinetic energy is, at some basic level, energy due to motion.[35] However, he does not speculate as to the underlying physical nature of that motion; exactly what it is motion *of* is not said. Yet this is no methodological barrier. The Lagrangian formalism allows him to investigate the dynamics of electromagnetic phenomena without ever having to say.

This ability to prescind from underlying ontological details is a general characteristic of the Lagrangian framework, and Maxwell is quite explicit that his goal is to exploit it in the service of a dynamical theory of electromagnetism:

> What I propose now to do is to examine the consequences of the assumption that the phenomena of the electric current are those of a moving system, the motion being communicated from one part of the system to another by forces, the nature and laws of which we do not yet even attempt to define, because we can eliminate these forces from the equations of motion by the method given by Lagrange for any connected system.

---

[34]Maxwell (1954a, p.38)
[35]Maxwell (1954b, p.197)

> In the next five chapters of this treatise I propose to deduce the main structure of the theory of electricity from a dynamical hypothesis of this kind... [36]

So even though Maxwell is committed to an ontology of forces, as the above passage illustrates, it also makes clear both that his intention is not to speculate about what those force are like and that he takes the Lagrangian formalism to be uniquely suited to that task.

But if Maxwell's expressed aim is to avoid speculating about the nature of electricity, how do we explain the central role *forces* play in the resulting theory? Indeed, the *non-homogeneous* Euler-Lagrange equations Maxwell uses in constructing his account contain explicit force terms:

$$\frac{d}{dt}\frac{\partial T}{\partial \dot{q}^{\alpha}} - \frac{\partial T}{\partial q^{\alpha}} = F_{\alpha} \qquad \alpha = 1, \ldots, n.$$

Here T is the system's kinetic energy, F_{α} a generalized force, and each α is a degree of freedom of the system.[37] How is his use of these equations to be reconciled with his desire to avoid ontological speculation? The presence of these forces threatens to undermine my claim that Maxwell's Lagrangian-based methodology doesn't depend essentially upon a commitment to Newtonian forces. How could that claim be credible if the theory itself appeals to forces?

The answer to these questions lies in the unique role of *generalized coordinates* in the Lagrangian framework, and their impact on how other parts of the formalism are understood. Maxwell starts developing his account by considering how generalized coordinates (and their associated generalized velocities) might be used to represent the energy of a system composed of physical circuits with constant current. Letting the spatial configuration of the physical circuits be completely specified by the

---

[36]Maxwell (1954b, p.198)

[37]Maxwell (1954b, pp.199–210). The distinction between homogeneous and non-homogeneous Euler-Lagrange equations is discussed in appendix A.

set $x^1, \ldots, x^n$ of generalized coordinates, he then takes the generalized coordinates $y^1, \ldots, y^n$ to represent whatever physical parameters are needed to specify completely the states of electricity in the system of circuits. In doing so he does not thereby commit himself to any ontological thesis about electricity, for the y^α are *generalized* coordinates and he has made no assumptions about their individual physical meanings. All that's assumed is that the configuration of electricity in the system can be specified using a set of parameters, which is surely warranted if electricity is taken to be something physical. Whatever electricity is, then, the configuration of the entire system (i.e., material circuits and electric currents combined) can be given by specifying the set of $2n$ generalized coordinates $x^1, \ldots, x^n, y^1, \ldots, y^n$.[38]

The total kinetic energy of this system is given by

$$T = T_m + T_e + T_{me}$$

where T_m is that component due to the material circuits alone, T_e the electric currents, and T_{me} represents any kinetic energy that might exist in virtue of interactions between matter and electricity. Through a series of ingenious experimental considerations, Maxwell argues both that $T_{me} = 0$ and that the kinetic energy possessed by the currents alone must take the following functional form:

$$T_e = \sum_{\alpha=1}^{n} \frac{1}{2} L^\alpha (\dot{y}^\alpha)^2 + \sum_{\substack{\alpha, \beta = 1 \\ \alpha \neq \beta}}^{n} M^{\alpha\beta} \dot{y}^\alpha \dot{y}^\beta$$

where L^α and $M^{\alpha\beta}$ are functions of the x^α alone.[39] The first summation term represents the kinetic energy of each circuit in isolation, the second of possible inter-circuit

---

[38]In the notation of appendix A, $x^1, \ldots, x^n, y^1, \ldots, y^n$ correspond to generalized coordinates $q^1, \ldots, q^{2n}$. Here I have adopted Maxwell's notation so as to emphasize the distinction between the x^α and the y^α.

[39]Maxwell (1954b, pp.211–222)

28

energy dependencies. An important step in Maxwell's reasoning here is the recognition that, although we may have no physical understanding of the y^α, we do know that all kinetic energy exists in virtue of motion of some form or other. So given the experimental fact that currents possess kinetic energy, that energy must be a function of the motion of those physical parameters specifying the state of electricity – that is, it must be a function of the $\dot{y}^\alpha$, whatever the y^α actually are.

Having previously used the Euler-Lagrange equations to define the *generalized momentum* (p^α) with respect to an arbitrary generalized coordinate as:[40]

$$p^\alpha = \frac{\partial T}{\partial \dot{q}^\alpha}$$

Maxwell introduces the term *electrokinetic momentum* for any generalized momentum associated with one of the y^α:[41]

$$p^\alpha = \frac{\partial T}{\partial \dot{y}^\alpha} = \frac{\partial T_e}{\partial \dot{y}^\alpha} = L^\alpha \dot{y}^\alpha + \sum_{\substack{\beta=1 \\ \alpha \neq \beta}}^{n} M^{\alpha\beta} \dot{y}^\beta.$$

Consider now a system of two circuits, one of which (the 'primary circuit') is fixed and maintains a constant current. In light of the preceding expression, the electrokinetic momentum of the secondary circuit *due to the primary circuit* is given by $M^{21}\dot{y}^1$, which Maxwell relabels $p = Mi_1$.[42] Because Maxwell has already established on experimental grounds that M is at most a function of x^1 and x^2, and it's assumed that the position and current of the primary circuit (x^1 and y^1, respectively) are fixed, it follows that p depends only on the position and form (or shape) of the secondary circuit. In other words, in this system p depends only on x^2. Each part of the secondary circuit – as distinct from the current in that circuit – does not interact

---

[40]Maxwell (1954b, p.207)
[41]Maxwell (1954b, p.229)
[42]Maxwell (1954b, p.229)

with any other material part, and thus Maxwell reasons that each portion of the circuit makes its own contribution to the total value of p – i.e., that p's dependence on x^2 can be separated into its dependence on the location and orientation of each segment of material circuit collectively specified by x^2.[43] This physical fact allows Maxwell to introduce the vector potential $\mathbf{A}$ implicitly defined by the equation

$$p = \oint \mathbf{A} \cdot d\mathbf{s}$$

where $d\mathbf{s}$ is an infinitesimal line element along the secondary circuit and the integral is taken around the entire circuit. Intuitively, $\mathbf{A}$ encodes the contribution a segment of circuit makes to p's overall value, given the location, length, and orientation of that segment. Taking the line integral around the circuit then gives the total value of p. An application of Stokes' Theorem then allows Maxwell to write

$$p = \oint \mathbf{A} \cdot d\mathbf{s} = \int (\nabla \times \mathbf{A}) \cdot d\sigma = \int \mathbf{B} \cdot d\sigma$$

where $\mathbf{B}$ is here *defined* as $\nabla \times \mathbf{A}$.[44] The two integrals on the right hand side are surface integrals taken over any surface bounded by the circuit's curve, and $d\sigma$ is an infinitesimal unit of area on that surface. The introduction of $\mathbf{B}$ is then what leads Maxwell to those equations that, in modern notation, we now call 'Maxwell's equations'.[45]

---

[43]Maxwell (1954b, p.230). For example, suppose that a particular value of x^2 specifies the secondary circuit to be in the shape of a two meter loop oriented at a given angle and distance from the primary circuit. This circuit will have a particular value of p at this location and orientation. Maxwell's point is that we can divide the secondary circuit up into smaller lengths of wire, each with their own orientations in space, and can think of p's total value as being the sum of the individual contributions made by those wire segments. This only works if the individual contribution to p of each part is entirely independent of the other parts.

[44]Maxwell (1954b, pp.230–234). Stokes' Theorem establishes the identity $\oint \mathbf{A} \cdot d\mathbf{s} = \int (\nabla \times \mathbf{A}) \cdot d\sigma$.

[45]Note that the quantity we now think of as representing the electric field doesn't occur *explicitly* in Maxwell's own formulation of his equations, but is implicit in a generalized force expression he uses. Chalmers (1973, pp.141–154) argues that Maxwell brings more than just experimental facts to bear on the Lagrangian formalism in deriving his equations; in particular that the introduction

It is astonishing that *this* is how Maxwell arrives at **B** from within the Lagrangian formalism, as **B** is traditionally interpreted in classical electromagnetism as representing the (ontologically primitive) magnetic field. Maxwell makes no such claim, however. Immediately after introducing **B**, for example, he writes:

> In identifying this vector, which has appeared as the result of a mathematical investigation, with the magnetic induction, the properties of which we learned from experiments on magnets, we do not depart from this method, for we introduce no new fact into the theory, we only give a name to a mathematical quantity, and the propriety of so doing is to be judged by the agreement of the relations of the mathematical quantity with those of the physical quantity indicated by the name.[46]

B is thus identified with an experimentally determined relationship between currents and magnets. At no point does he suggest that **B** ought to be understood as anything like the modern notion of the magnetic field.[47]

Much of this sounds strange to contemporary ears, but is entirely in keeping with Maxwell's understanding of the Lagrangian framework and the role of generalized coordinates. What the preceding historical discussion illustrates is the way in which **B** ultimately derives its physical significance from the generalized coordinates x^α and y^α themselves, not in virtue of any interpretative hypothesis about the mechanism by which electromagnetic effects are transmitted. Given Maxwell's emphasis on our ignorance of the nature of the physical quantities represented by the generalized coordinates, particularly the y^α, it's no wonder that a mathematical object based on them would not be treated as representing a primitive ontological posit.[48]

---

of the displacement current, essential to Maxwell's electromagnetic theory of light, is not justified by the experimental facts but rather by electrical speculation. What's important for my purposes is that the electrical justification Chalmers has in mind is not based on any underlying Newtonian considerations.

[46]Maxwell (1954b, p.234)

[47]See Chalmers (1973, p.109) for an elaboration of this point. Maxwell does talk of 'fields' (and 'charges'), but always as elliptical for some as-yet unknown mechanical state of an underlying ether.

[48]It's worth emphasizing that Maxwell introduces **B** via the vector potential **A**, which is itself taken to be a mathematical artifact of the theory (on both our modern understanding and his). This is the exact opposite of modern treatments, which introduce **B** as a primitive field and then define **A** as that vector field for which $\mathbf{B} = \nabla \times \mathbf{A}$ (subject to the standard gauge transformation $\mathbf{A} \to \mathbf{A}' = \mathbf{A} + \nabla\lambda$, for any scalar function λ).

The way Maxwell introduced and interpreted **B** guides our understanding of forces in his theory. Recall that is was the presence of forces that threatened to undermine my characterization of Maxwell's methodology as being (essentially) independent of background ontological commitments. However, the forces introduced are *generalized forces* occurring in the non-homogeneous Euler-Lagrange equations, where a generalized force is an abstract term in the Lagrangian formalism proportional to the time rate of change of an associated generalized velocity. In the context of the two-circuit system above, for example, Maxwell defines the electromotive force (E) on the secondary circuit to be the generalized force associated with y^2. By constructing the Euler-Lagrange equation for the y^2 coordinate he arrives at the expression[49]

$$E = -\frac{d}{dt}\frac{\partial T_e}{\partial \dot{y}^2} = -\frac{dp}{dt}$$

and then uses p's line integral expression ($p = \oint \mathbf{A} \cdot d\mathbf{s}$) to determine that

$$E = \oint (\mathbf{v} \times \mathbf{B} - \frac{d\mathbf{A}}{dt} - \nabla\psi) \cdot d\mathbf{s}$$

where **v** is the velocity of the circuit itself and ψ is an arbitrary scalar function introduced for generality.[50] This equation *defines* the electromotive force E, and thus E acquires meaning only through **B** and **A**. As we have seen above, though, **B** and **A** are themselves not taken to represent genuine physical posits. Instead, Maxwell takes them to be useful ways of codifying experimentally determined relationships between circuits and magnets, and this suggests that forces are to be understood in a similar, derivative way. The forces occurring in Maxwell's account, then, in virtue of being generalized forces, are not to be understood as imbued with ontological significance.

---

[49]Maxwell (1954b, pp.224–25)

[50]Maxwell (1954b, pp.239–243). Because E is determined by taking the integral around a closed loop, the ψ-contribution always vanishes. Maxwell often treats the integrand $\mathbf{v} \times \mathbf{B} - \frac{d\mathbf{A}}{dt} - \nabla\psi$ as the electromotive force.

Given our modern understanding of Maxwell's equations, it's easy to overlook the fact that for Maxwell these forces are *generalized forces*. Whereas ordinary Newtonian forces are always associated with changes in *spatial* velocities, and hence can be thought of as entities directly affecting physical motion, generalized forces derive their physical significance from the generalized coordinates with which they're associated. And in the present case, Maxwell has been at pains to emphasize that the generalized coordinates at issue are *placeholders* about whose underlying physical significance he is deliberately non-committal. Indeed, Maxwell is quite clear that generalized forces are not to be taken as representing actual physical forces themselves:

> We shall denote the force which must be applied to any [generalized] variable q_r by F_r. The system of forces (F) is mechanically equivalent (in virtue of the connexions of the system) to the system of forces, *whatever it may be, which really produces the motion.*[51]

Simpson (1970) puts the point more emphatically, writing in reference to the *Treatise* that

> [i]t is important to recognize just how general the generalized coordinates may be. They need not be linear displacements at all, but any quantities which suffice to determine the state of the system. Similarly, the velocities, moments and forces related to them in the equations of motion are not literally what their names indicate, but new, generalized quantities only metaphorically related to their Newtonian originals. In this sense, Lagrangian mechanics introduces a pervasive new figure of speech into physics.[52]

Like $\mathbf{B}$ and $\mathbf{A}$ themselves, then, generalized forces are not to be understood as actual posited forces, but rather abstract ways of expressing relationships between experimentally determined phenomena.

Thus the apparent invocation of Newtonian forces in Maxwell's construction of electromagnetism is merely illusory, and as a result the Lagrangian-based methodology Maxwell employs does not depend essentially upon a background theoretical commitment to such forces. Because generalized forces are not to be understood as

---

[51]Maxwell (1954b, p.201; my emphasis)
[52]Simpson (1970, p.253)

representing physical posits, but instead as defined quantities within the Lagrangian framework, an agent who eschews Newtonian forces is free to invoke generalized forces in manipulating the Lagrangian formalism without thereby committing herself to actual physical forces. But the preceding account also demonstrates that the *only* forces Maxwell invoked in the development of electromagnetism were generalized forces. Hence an agent could endorse the needed projectability judgments without ever having to commit herself to an ontology of Newtonian forces, despite the fact that we know Maxwell himself privately embraced such an ontology.

2.5 Rejoinders

Two worries might lead one to doubt my conclusion as to the essential theory independence of Maxwell's Lagrangian methodology. The first concerns whether a commitment to Newtonian forces might play a more subtle (but no less dispensable) role in Maxwell's process of theory construction than I have recognized. Even accepting that Maxwell's methodological judgments don't require any *specific* assumptions about underlying Newtonian forces, it might still be the case that those judgments require a *general* commitment to the existence of Newtonian forces; a commitment, that is, to the claim *that there are Newtonian forces.* Such a situation would arise, for example, if the very use of the Lagrangian formalism to represent the physical world implicitly presupposed a commitment to Newtonian forces, even if that commitment didn't require specific assumptions about what those forces were like. As previously noted, Maxwell seemed to harbor just this sort of commitment to forces. Indeed, given that the Lagrangian and Newtonian formalisms are widely thought to be mathematical reformulations of a single physical theory, one might plausibly hold that the methodological choice to start theory development within the Lagrangian framework only makes (or made) sense in virtue of an implicit Newtonian ontology.

Only in virtue of Newtonian forces, that is, might the Lagrangian formalism even represent a coherent physical framework for theory construction.

The second worry is that my argument only succeeds in showing that the essential theoretical dependencies lie elsewhere. Suppose we grant that Maxwell's application of projectability wasn't essentially dependent on Newtonian forces. It's still true that Maxwell took *energy* as a primitive physical notion at the heart of the Lagrangian framework. Might his central methodological judgments have been unmotivated in the absence of a background realist commitment to energy? In that case I would not have succeeded in showing that Maxwell's methodology wasn't essentially theory dependent, only that it wasn't essentially theory dependent *on a commitment to Newtonian forces.* Since the concept of energy itself plays a central role in modern foundational physics, this result presumably wouldn't saddle Boyd's realist with any unwanted theoretical commitments.[53]

To assuage the first worry, one must show that the Lagrangian formalism Maxwell used admits of a non-Newtonian physical interpretation for which Maxwell's methodological judgments are still plausible – an interpretation, that is, not grounded in the notion of *force*.[54] What's at issue here is a conceptual connection: could a realist take Maxwell's Lagrangian framework as a basis for theory construction without thereby committing herself to an underlying Newtonian ontology? The interpretation at issue,

---

[53]One might think this worry can also be developed in another direction. In discussing Newtonian ontology I have spoken mainly of *forces*, but perhaps Maxwell's methodology depends essentially on that other element of Newtonian ontology: *mass*. Even conceding this point, though, my argument would still amount to a significant setback for Boyd. According to his realist thesis, the approximate truth of background beliefs explains methodological success precisely because the entities and properties figuring in those beliefs form part of the causal structure of the world – a causal structure successful methodologies latch onto and exploit. But it's difficult to see how that causal explanation would work in the absence of forces, as they are what mediate the causal interactions between masses. In addition, there may be no reason to concede an essential mass dependence. As Simpson (1970, pp.253–254) points out, "virtually nowhere in [the Lagrangian dynamics] chapter does the Newtonian term 'mass' even occur... [T]he strictly Newtonian concepts have been totally displaced, and the new quantities are treated by Maxwell from the beginning as dynamical terms meaningful in themselves."

[54]And perhaps not *mass* either. (See n.53.)

if there is one, need not be the 'best' or most plausible. It need only be consistent and physically coherent. Such an interpretation would guarantee that the Newtonian and Lagrangian formalisms really do come apart conceptually, that using the latter doesn't invoke a tacit acceptance of the ontology of the former. An interpretation of this sort would also address the second worry, provided that the physical picture it specified didn't require a primitive appeal to the concept of energy.[55] That would show that Maxwell's methodological judgments remained plausible with respect to a range of background theoretical commitments, and thus that those judgments weren't essentially theory dependent upon particular ontological views. Let us call an interpretation satisfying both these constraints *radically non-Newtonian.*

Is a radically non-Newtonian interpretation of the Lagrangian formalism, as Maxwell applied that formalism, even possible?[56] Here we must be careful to identify the conditions any proposed interpretation must satisfy. A radically non-Newtonian *instrumentalist* construal is ready at hand, for example, yet such an interpretation is irrelevant within the context of Boyd's naturalistic framework. That one can repeat the methodological motions (as it were) after the fact, without any commitment to the physical significance of the representational discourse, tells us nothing about which background theoretical commitments are dispensable in the actual practice of theory construction. What is needed is a naturalistic interpretation: an interpretation that seeks to explain observable phenomena by positing the reality of theoretical entities and structures causally giving rise to those phenomena. No radically non-Newtonian

---

[55]Or, perhaps in light of n.53, of mass as well.

[56]The requirement that the interpretation be of the formalism *as Maxwell understood it* amounts to the condition that the formalism have the same scope of application as Maxwell took it to have. In particular, Maxwell took the Lagrangian formalism to apply to conservative and non-conservative systems alike. See, e.g., Maxwell (1954b, pp.140–141) and Maxwell (1925, pp.67–68). This is one reason he used the non-homogeneous Euler-Lagrange equations. (The relationship between non-conservative systems and the non-homogeneous Euler-Lagrange equations is discussed in appendix A.) An adequate radical non-Newtonian interpretation must thus be one that understands the Lagrangian formalism as applying to both conservative and non-conservative systems. Although the mathematical details are beyond the scope of this paper, this constraint is a significant obstacle to the construction of such an interpretation.

interpretation satisfying this condition exists in the literature. However, an interpretation of this sort *is* possible, although its one whose mathematical details take us beyond the scope of this chapter. I develop this interpretation in greater detail in chapter 3.

I conclude that the Lagrangian-based methodology Maxwell used in developing electromagnetism wasn't essentially theory-dependent. Given the astounding empirical success of classical electromagnetic theory, Maxwell's methodology of projectability thus represents a counter-example to the core characterization of scientific methodology needed for Boyd's naturalistic defense of scientific realism.

2.6   Generalization and Conclusion

I have argued that Maxwell's development of electromagnetism presents a counter-example to the characterization of methodology central to Boyd's argument. His naturalistic no-miracles defense of scientific realism requires our methodological judgments to be not just theory-dependent, but *essentially* theory-dependent. While I do not doubt that some scientific methodologies fit this description, I take myself to have shown that Maxwell's Lagrangian-based use of projectability does not.

The implications of this conclusion for a naturalistically-motivated realism about foundational physics depends, in part, on the extent to which the distinguishing features of Maxwell's methodology generalize to other cases of theory construction in the history of foundational physics. Might the historical case I've considered be a methodological anomaly? Boyd's argument concerns scientific methodology *in general*. Surely if what's at issue is the best overall explanation of instrumental reliability, even within the restricted context of foundational physics, Boyd can permit that isolated applications of successful methodology might not be essentially theory dependent. Are there reasons to think that the lessons of the historical case examined

here generalize – that there are other methodological applications central to the history of physics that aren't essentially theory dependent either? This is a very broad historical question, but in this final section I'd like to sketch a reason to think there are.

The distinguishing feature of Maxwell's Lagrangian-based projectability judgments is the use of abstract mathematics as a means for representing connections between experimental phenomena without regard to the ontology grounding those connections. It was this methodological choice that motivated the adoption of the Lagrangian framework. We see this quite clearly in the way he represented the state of electricity in a system of circuits using generalized coordinates whose underlying physical significance remained unknown. That he could construct an entire theory of electromagnetism using this technique is surely a rather extraordinary feature of the Lagrangian framework. But his broader process of theory construction, in which a mathematical framework is adopted with no regard for the underlying ontology, was part of a methodological tradition that emerged at the turn of the 19th century as a result of the *mathematization* of physics – a tradition that made important contributions to the empirical success of physics, but which has been largely ignored by philosophers of science.

The methodological tradition in question draws its inspiration from the 18th century program of *rational mechanics*. Whereas mathematical analysis in Newton's *Principia* is always given a geometrical significance, the bifurcation of analysis and geometry at the beginning of the 18th century freed mechanicians of the need to provide physical accounts of mathematical terms in ways that could be readily visualized. As a result, mathematical quantities in mechanical theories lost much of their direct physical significance.[57] The mathematical concepts introduced in the service of extending and supplementing Newtonian mechanics were often constructed with no

[57]See, e.g., Harman (1982, Ch.2).

38

regard for their underlying physical significance – with no regard, that is, for whether they represented real physical things. This became a defining feature of the rational mechanical program of (among others) Euler, d'Alembert, and the Bernoullis.[58]

The influence of rational mechanics on other parts of physics during the 18th century is evident in the increased role of mathematics as a tool for representing the physical world. At the time, mathematics in science was primarily restricted to mechanics. Phenomena associated with, for example, optical and thermal effects were still treated in fundamentally qualitative ways. This begins to change towards the end of the 18th century, and associated with that change is a new methodological attitude of "mathematical instrumentalism", according to which the primary aim of theory construction is the quantitative representation of experimental relationships.[59]

This is the methodological tradition in which we ought to situate Maxwell's work.[60] Developing out of rational mechanics, this broad approach to theory construction sought to extend the representational power of mathematics to other, less overtly mechanical phenomena. Maxwell's is not the only theory within this tradition to have been empirically successful. In developing his 1822 theory of heat, for example, Fourier explicitly stressed the distinction between mathematical representations and physical reality. He manages to derive the empirically correct equation of heat diffusion without making any assumptions about the underlying physical nature of heat itself. Similarly, George Green and James MacCullagh each stressed the same distinction when they developed their (separate) theories of elastic solids without concern

---

[58]See, e.g., Bos (1980) and Harman (1982, Ch.2). As Truesdell (1968) notes, 19th century classical physics owes much to these mathematically grounded rational mechanical theories; perhaps much more than it owes to Newton.

[59]See, e.g., Heilbron (1980).

[60]That this was a distinct methodological tradition is evident in the competition that existed between it and the so-called Laplacian program in physics dominant at the end of the 18th century and first decade of the 19th century. The Laplacian program is perhaps the quintessential theory *dependent* methodological program. It assumed that all phenomena could ultimately be explained in terms of central forces between matter and used that ontological assumption to guide theory construction. As Fox (1974) notes, the fall of the Laplacian program is due in part to the development of successful rival theories squarely within the tradition that emerged out of rational mechanics.

for the underlying mechanical properties of those solids. These theories, as much as any other theories in the first half of the 19th century, helped to shape the future course of physics. Yet they are clearly within a methodological tradition of theory construction for which essential theory independence is the goal. Whether this goal is always achieved requires further historical investigation, but we have a clear reason to think that Maxwell's essentially theory independent methodological approach is far from being an isolated historical anomaly.

CHAPTER III

Interpretation and Reformulation: A Case Study from Classical Dynamics

The classical theories of Newtonian and Lagrangian dynamics seem, on the surface, to offer rather different accounts of the physical world.[1] The Newtonian picture is typically taken to consist of material bodies possessing mass, interacting via the mediation of forces. A physically possible point-particle trajectory (i.e., equation of motion) is represented mathematically by any continuous and twice differentiable function $\mathbf{r}(t) = (x(t), y(t), z(t))$ in $\mathbb{R}^3$ for which there's a force function $\mathbf{F}(t) = (F^1(t), F^2(t), F^3(t))$ satisfying Newton's second law:

$$\mathbf{F}(t) = m\ddot{\mathbf{a}}(t),$$

where m is the particle's mass. A complex system on the Newtonian approach is, at least in principle, treated as an n-particle system.

The Lagrangian picture is rather different. A system is characterized using generalized coordinates in an abstract n-dimensional configuration space M, where n is the system's degrees of freedom. Dynamical relationships internal to the system are

---

[1]My discussion is restricted to finite-dimensional systems, in keeping with the various passages quoted below. Additional details about the Newtonian and Lagrangian formalisms can be found in Appendix A.

treated kinematically. A scalar-valued Lagrangian function L is assigned to a system, and a physically possible time-evolution of that system is any continuous and twice differentiable function $q(t) = (q^1(t), \ldots, q^n(t))$ on M that's a solution of the Euler-Lagrange equations:

$$\frac{d}{dt}\frac{\partial L}{\partial \dot{q}^\alpha} - \frac{\partial L}{\partial q^\alpha} = 0, \qquad \alpha = 1, \ldots, n.$$

Unlike the Newtonian approach, forces, at least on the surface, seem to play no role in the Lagrangian picture.

It's commonly thought among physicists, however, that Lagrangian dynamics is a *reformulation* of sorts of Newtonian dynamics – a convenient mathematical repackaging of (part of[2]) core Newtonian principles.[3] Hand and Finch (1998) make this explicit in their introduction to analytical dynamics when they write:

> Lagrange reformulated Newton's laws in a way that eliminates the need to calculate forces on isolated parts of a mechanical system. (1)

> Lagrange did not introduce new physical principles to mechanics. The physical concepts are due to Newton and Galileo. But he succeeded in giving a more powerful and sophisticated way to formulate the mathematical equations of classical mechanics, an approach that has spread its influence over physics far beyond the purely mechanical problems... As always in classical mechanics, the heart of the dynamics lies in the expression of Newton's Second Law: $\mathbf{F} = \dot{\mathbf{p}}$. (5)

> The physical content of Newton and Galileo's mechanics remains intact after Lagrange. His physics is completely equivalent to Newton's but mathematically much more powerful. The physics of mechanics did not change until Einstein's theory of special relativity was formulated in 1905. (23)[4]

Marion and Thornton (1995) seem to express a similar view about the physical content

---

[2]This qualification will be developed below.

[3]A similar view has been expressed in the recent philosophical literature by Wilson (2007), who argues that analytical dynamics provides no grounds for doubting the central ontological role of Newtonian forces in classical dynamics. Another philosophically-oriented discussion of the relationship between different formulations of classical dynamics – from an altogether different perspective – is contained in Wilson (2009).

[4]But note that many of the central physical principles of 'Newtonian dynamics' were developed after Newton (and Galileo). See, e.g., Truesdell (1968) for a detailed discussion of this point.

of the Lagrangian framework:

> [T]he Lagrangian and Newtonian formulations of mechanics are equivalent: the viewpoint is different, but the content is the same. (258)

> [W]e must reiterate that Lagrangian dynamics does not constitute a *new* theory in any sense of the word. (262)[5]

Lest one think this a particularly anachronistic and modern understanding, Mach (1919) held perhaps the strongest view of all, writing with characteristic authority:

> The principles of Newton suffice by themselves, without the introduction of any new laws, to explore thoroughly every mechanical phenomena practically occurring, whether it belongs to statics or dynamics. If difficulties arise in any such consideration, they are invariably of a mathematical, or formal, character, and in no respect concerned with questions of principle. (256)

> [Regarding analytical dynamics,] [n]o fundamental light can be expected from this branch of mathematics. On the contrary, the discovery of matters of principle must be substantially completed before we can think of framing analytical mechanics; the sole aim of which is a perfect practical *mastery* of problems. Whosoever mistakes this situation, will never comprehend Lagrange's great performance, which here too is essentially one an *economical* character. (480)

What precisely does this general reformulation claim mean, particularly in light of the fact that classical dynamics is false, and why should we think the reformulation claim is true? These are, concisely put, the two questions around which this essay is focused. It is a case study in how the concept of theory reformulation works in foundational physics – a concept that's important to our (or at least the realist's) assessment of the underdetermination threat. Although the passages above are written in ways suggesting their meanings are clear, the implicit notion of reformulation at work turns out to be surprisingly subtle. In the course of unpacking the 'reformulation claim' (as I will call it), we'll see the central role judgments about ontological interpretation play in our general understanding of when one theory is a reformulation of another.

I start by considering one natural way of understanding the reformulation claim that I think falls short of capturing its meaning, and then consider an argument to

---

[5]A similar statement also appears in José and Saletan (1998, p.65), and this attitude is arguably implicit in Goldstein *et al.* (2001) as well.

the effect that any other construal of the claim is semantically incoherent. I then offer a competing analysis of the notion of theory reformulation in general, at the center of which is the idea of ontological interpretation. The central argument for it is simply that it provides a way of capturing the physical meaning of the above passages without lapsing into nonsense. This proposal is then applied to the particular case of dynamics with which we started, where I argue that the interpretative judgments required to justify the reformulation claim are suspect.

3.1   The View from Mathematical Modeling

One construal of the reformulation claim, widespread in applied mathematics and mathematical physics, understands it as a statement about solutions of differential equations. The operative notion here is one of *mathematical equivalence*.[6] The two mathematical frameworks are equivalent if for every solution of one differential equation there's a corresponding solution of the other, where this correspondence is established by the (invertible) coordinate transformation matrix:

$$q^\alpha = q^\alpha(x^1, y^1, z^1, \ldots, x^N, y^N, z^N, t) = 0, \qquad \alpha = 1, \ldots, n.$$

Here (x^i, y^i, z^i) are the Cartesian coordinates of the ith particle in an N-particle system. On this sense of equivalence, it can be shown that Newtonian and Lagrangian dynamics are *not* equivalent. There are solutions of Newton's equations for which there are no corresponding solutions of the Euler-Lagrange equations, for any Lagrangian L. But the converse does not hold: all solutions of the Euler-Lagrange equations have corresponding solutions of the Newtonian equations.[7] The solutions of Newtonian dynamics thus 'subsume' the solutions of Lagrangian dynamics, and

---

[6]See, e.g., Arnold (1989, pp.59, 65). This approach is by no means ideosyncratic. See Gallavotti (1983) and Abraham and Marsden (1994) for similar expressions of this approach.

[7]Proofs and discussions of both claims are contained in Santilli (1978).

the reformulation claim can be understood as a statement of this relationship. Lagrangian dynamics is a reformulation of Newtonian dynamics in the sense that it's a way of re-characterizing part of the mathematics of the Newtonian formalism.

Despite its mathematical cogency, this construal of the reformulation claim would seem to miss part of its meaning. The passages above clearly make a substantive claim about the relationship between the *physical* content of Newtonian dynamics and the Lagrangian formalism – namely, that the physical picture encoded in the Lagrangian mathematical framework is, in some sense, the Newtonian physical picture. To understand the reformulation claim simply in terms of solutions to differential equations would seem to miss the *physics* of it.[8] Capturing the physics of it, though, turns out not to be a straight-forward matter.

3.2 Semantic Qualms

Whatever way we understand the physical meaning of the reformulation claim, it's clear there's an asymmetry built into the alleged relationship. Lagrangian dynamics re-expresses the principles of Newtonian dynamics, not the other way round.[9] However, it might be objected that this sort of 'physical comparison' is inherently confused in light of the fact that classical dynamics (in any of its forms) is a false theory.

To ask about the physical content of Newtonian dynamics itself would seem to be to pose the following question: What would the fundamental physical world be like, were Newtonian dynamics true?[10] Clearly, this question is not asking about what the *actual* world is like, the one in which we live, as the primitive ontology of our

---

[8]None of this, of course, impugns the modeling approach to Lagrangian dynamics as a tool of applied mathematics.

[9]This may seem obvious in light of the narrower representational scope of the Lagrangian formalism, a point I'll revisit (and question) below.

[10]There are, of course, many theories of Newtonian dynamics: the theory of gravitation, of the pendulum, of friction, etc. The reader is encouraged to pick her favorite.

world isn't Newtonian. Rather, in accordance with the usual semantics for assessing counterfactuals, what's at issue are various *possible worlds* – various ways the world might have been – and their 'distance' from the actual world.[11] These worlds will differ from each other and our own in a multitude of ways, and many of them are not relevant for assessing the physical content of Newtonian dynamics. How do we single out the ones that are?

We identify possible worlds and distinguish between them by specifying what they are like; we cannot just point to them the way we can the actual world. In asking about what would have happened had I brought my umbrella today, I'm asking about the set of possible worlds in which I *did* bring my umbrella. I'm specifying that set of possible worlds as relevant. More specifically, I'm asking about which world in that set is 'closest' to the actual world.

This raises a problem for making sense of the question, "What would the fundamental physical world be like, were Newtonian dynamics true?" The relevant possible worlds are clearly those in which Newtonian dynamics is true, but how do we specify which those worlds are without already knowing what the ontology of Newtonian dynamics is? We can stipulate, of course, yet in doing so trivialize the counterfactual. We seem to have no ability to 'latching onto' the worlds relevant to our discourse in a way that's independent of what we're trying to find out. It's hard, then, to see how the ontological question at issue is really coherent.[12] Yet if we can't make sense of what it means to inquire into what fundamental ontology would be like were

---

[11]For canonical statements of this semantic account of counterfactuals, see Lewis (1973) and Lewis (1986).

[12]Note that the problem is *not* that there are possible worlds possessing radically different ontologies that might all be labeled 'Newtonian worlds' – e.g., the world in which particles have mass and motion arises on account of real physical forces, and the world in which the notion of force is defined and only mass is real. For the meaning of the claim that Newtonian dynamics is true is not held fixed across such worlds (because the interpretation of Newtonian dynamics changes). That would be like objecting that counterfactuals involving what would have happened had I gone down to the bank today are indeterminate because worlds in which I came back with money are just as close as worlds in which I came back with fish.

Newtonian dynamics true (short of simply stipulating), then we can't say that the ontology of Newtonian dynamics is one particular way and not another (again, short of stipulating). But that implies that the physical content of the reformulation claim is incoherent, relying as it does on an assertion about the ontologies of both Newtonian and Lagrangian dynamics. Perhaps the mathematical modeling construal is the way to go, after all.

3.3 An Interpretative Reassessment

But it certainly seems like questions about the fundamental ontology and physical content of Newtonian (and Lagrangian) dynamics make sense, so we ought to hesitate before conceding that no sense can be made of them simply because we no longer think they're approximately true descriptions of the world. Fortunately, there is a way of understanding the reformulation claim that preserves the idea (clearly expressed in the passages above) that it is, in part, a claim about physics, and yet which doesn't fall into the carefully laid semantic trap just outlined.

The construal I propose rests on an appeal to the notion of an ontological interpretation, and the judgements agents make about the acceptability of such an interpretation. Recall that an ontological interpretation is a specification of how the elements of a theory's mathematical formalism get mapped onto ontologically primitive and derivative features of the physical world. If a theory is false, then of course any such mapping associated with it will be mistaken – perhaps because the physical features it purports to map to don't actually exist, or, conversely, because relevant features that do exist aren't represented in the formalism. But we shouldn't take that to mean all proposed interpretations of a false theory are on equal footing. Some may be incoherent or inconsistent, for example, whereas others may be consistent but fail to satisfy other desiderata one has of a good interpretation.

How one decides whether a proposed interpretation is acceptable (or even coherent) is a rather intricate affair, but on the surface it has nothing to do with whether a theory is true or false. One isn't asking what the ontology is of a world in which that theory is true. Rather, one is asking about how well a proposed interpretation fares with respect to whatever standards we actually use for evaluating interpretations in general. That a candidate interpretation fares the best with respect to these criteria – whatever they may be – tells us nothing about the ontologies of possible worlds. It does, however, tell us something relevant and interesting: namely, what we *would* judge the physical content of a theory to be *were* it an empirically adequate theory. It tells us, that is, how we would interpret a theory were we to do so without regard to the fact that the theory was false. Entirely absent from this is any commitment to what the primitive ontology of such a world is.

Given that this proposal appeals to a counterfactual, we must still identify a relevant set of possible worlds. Here there is no risk of circularity or incoherence, though, because the feature used to pick out the set of worlds (namely, that a particular theory is empirically adequate) doesn't require that one first know the feature being evaluated at those worlds (namely, our interpretative judgments).

This, then, is how I think we should understand inquiries into the physical content of false theories: they are not questions about the ontologies of possible worlds, but questions about what our interpretative judgments would be as to what those ontologies are, were those false theories empirically adequate. In the limiting case where a theory is believed to be true, this diagnosis returns the correct result. In that situation the most we can *know* is that the theory is empirically adequate. So in claiming that the physical content of the theory is such-and-such, what we're doing is announcing our preference for one particular interpretation over the others in light of its empirical adequacy. The only difference is that in this case the world in which

the theory is empirically adequate happens to be the actual one.

This analysis allows us to make sense of the reformulation claim in such a way as to avoid the above semantic difficulty, while at the same time preserving the idea that the claim carries physical significance. However, a preliminary complication arises. If we want to apply the analysis to claims involving the interpretation of multiple theories, as occurs in the reformulation claim, which possible worlds are we to consider in assessing our interpretative tendencies? This question seems particularly pressing because, as noted in section two, there are solutions to the Newtonian equations that aren't solutions to the Euler-Lagrange equations. So the sets of possible worlds in which each dynamical theory is empirically adequate won't be co-extensive. But we need not assume there's a systematic answer to this question; it may depend on the context of each assertion and what our particular interests are. Different sorts of theoretical comparisons can be made by fixing the relevant class of possible worlds differently. The context in which the reformulation claim is asserted would seem to suggest that the relevant set of worlds includes those containing some systems that are Newtonian and not Lagrangian. But this choice will turn out not to matter, as I'll argue below that the Lagrangian framework can be adapted to account for such systems.

On the account I'm suggesting, then, the claim that Lagrangian dynamics is a reformulation of Newtonian dynamics is a normative assertion about how these theories ought to be interpreted were the world one in which some systems were well represented by the Lagrangian framework and all systems were well represented by the Newtonian framework. The reformulation claim asserts that the best interpretation of Lagrangian dynamics in such worlds is the interpretation taking the physical content represented in the Lagrangian formalism to be derived from the physical content represented on one's preferred interpretation of Newtonian dynamics. This construal

isn't as physically robust as our original, problematic reading, but neither does it reduce the reformulation claim to an assertion about mathematics alone.

I have suggested that we understand the reformulation claim as an assertion to the effect that the best ontological interpretation of Lagrangian dynamics is the one on which its physical content is determined by one's preferred interpretation of Newtonian dynamics. Are there not other possible interpretations of Lagrangian dynamics? Why is this the best one? Whether the reformulation claim is justified on my analysis will depend on the sorts of considerations actually used for assessing interpretations, which have thus far not been given in any detail. It's to these considerations that I now turn.

3.4 Gauge Invariance

The Lagrangian treatment of a physical system avoids the invocation of forces by instead associating with that system a scalar Lagrangian function L, the variation of which determines, in conjunction with the Euler-Lagrange equations, the motion of the system. So a natural initial choice for constructing an alternative interpretation of Lagrangian dynamics, one on which it's not just a reformulation of Newtonian dynamics, is to interpret L as an ontologically primitive scalar field in the configuration space dynamically interacting with the system point of the world.

A preliminary problem with this suggestion concerns the fact that Lagrangians are not gauge invariant quantities, a status often thought to be necessary for any mathematical object representing a primitive feature of the physical world. Given a curve $\gamma : t \rightarrow M$ representing the n solutions of the Euler-Lagrange equations for a given Lagrangian L and set of initial conditions, L exhibits a *gauge symmetry* in the sense that there's a systematic way of transforming L into a different Lagrangian L' for which the solutions of the Euler-Lagrange equations are *also* given by γ. Indeed, the

differential equations determined by the Euler-Lagrange equations – the differential equations whose solutions determine γ, that is – are the *same* for any L and L' such that $L' = L + \frac{d\Phi}{dt}$, where Φ is an arbitrary smooth function of the q^α and t. As Smith (2008) observes:

> The different gauge equivalent Lagrangians give rise to the same differential equations of motion... And, since the Euler-Lagrange equations for two gauge equivalent Lagrangians are the same, the dynamics is obviously the same as well. So, if we can find one Lagrangian, we can find an infinite number of gauge-equivalent Lagrangians such that their variation gives rise to the same integral curves.[13]

From a single Lagrangian L, then, we can construct an infinite number of different Lagrangians, all generating the same differential equations of motion and hence the same dynamics on M. As a result, the numerical value of L at a given point $p \in M$ doesn't actually seem to matter dynamically, since through a suitable choice of Φ a gauge equivalent Lagrangian L' can be constructed whose numerical value at p is anything we'd like. What this suggests is that the Lagrangian associated with a particular physical system shouldn't actually be interpreted as measuring the magnitudes or intensities of some primitive physical *thing* existing alongside the system, as those magnitudes can vary without changing the dynamics.[14] We are thus led back to the received view on which Lagrangian dynamics is a reformulation of (part of) Newtonian dynamics.

3.5   Polygenic Systems

A second argument for the interpretative position embodied in the reformulation claim – or what amounts to the same thing: an argument against any interpretation of the Lagrangian formalism not tied to Newtonian dynamics – concerns the repre-

---

[13]Smith (2008, p.331)

[14]This is not to say, of course, that one can simultaneously vary, in arbitrary ways, the Lagrangian's magnitudes over a *set* of points. The relationships holding between the Lagrangian's values at different points are still instrumental in determining the overall motion.

sentational scope of the Lagrangian framework. This is the concern alluded to in the previous section, which threatens to trivialize my reconstruction of the reformulation claim. Even supposing sense can be made of an ontologically primitive (gauge symmetric) Lagrangian field, the theory associated with such an interpretation would be so limited in scope that any non-Newtonian interpretation of it would be physically uninteresting. Hence the *best* interpretation of Lagrangian dynamics is to take it as an elaborate mathematical reformulation of an underlying Newtonian world.

The technical details of this line of thought turn out to be important. In general, in order to associate a Lagrangian with a system, the external forces acting on it must be derivable from a *work function*.[15] Letting the net force on the ith particle be $\mathbf{F}_i = X_i\hat{\mathbf{x}} + Y_i\hat{\mathbf{y}} + Z_i\hat{\mathbf{z}}$, the work done by all applied forces in an infinitesimal displacement of the system is expressed by

$$\bar{dw} = \sum_{i=1}^{N}(X_i dx^i + Y_i dy^i + Z_i dz^i)$$

where $\bar{dw}$ is a differential form but need not be the true differential of any function (as indicated by the 'bar'). The coordinate transformations of section two then allow us to express $\bar{dw}$ in terms of the generalized coordinates as

$$\bar{dw} = \sum_{\alpha=1}^{n} F_\alpha dq^\alpha$$

where the F_α are now functions of the q^α. The F_α together constitute the vector components of the *generalized force* on the system, which can be thought of as acting on the system point in M.[16]

Assume for the moment that the ordinary forces (as represented in $\mathbb{E}^3$) acting on

---

[15]Here, of course, I'm putting the point as a Newtonian would. Readers unfamiliar with these details of Lagrangian dynamics are encouraged to consult Appendix A.

[16]Strictly speaking, the generalized force vector at a point $p \in M$ exists in the tangent space $\mathbb{T}_pM$, but this detail need not concern us yet.

our system are *monogenic* – that is, assume that they are such that $\bar{d}w$ is the true differential of a function U such that $\bar{d}w = dU$. The scalar U may be expressed as a function of the generalized coordinates, their time derivatives, and perhaps time as well:

$$U = U(x^1, \ldots, x^{3N}, \dot{x}^1, \ldots, \dot{x}^{3N}, t) = U(q^1, \ldots, q^n, \dot{q}^1, \ldots, \dot{q}^n, t).$$

U takes the form $U(q^1, \ldots, q^n)$ in the case of *conservative* forces, and setting $V = -U$ we get the familiar result $F_\alpha = -\frac{\partial V}{\partial q^\alpha}$. Here V can be interpreted as a system's potential energy. However, what's important for constructing a system's Lagrangian is not that V can be understood as the potential energy, but rather that the function U exists at all.[17] Even for non-conservative systems a Lagrangian will exist as long as $\bar{d}w$ is the true differential of some function U, often called the *work function.*

As we've seen, though, U only exists for monogenic forces, and it turns out that a great many physical systems are *polygenic*, such as systems involving friction and other dissipative effects. The Newtonian framework has no difficulties treating polygenic phenomena, and so it looks as though many systems treated by Newtonian dynamics will simply not have associated Lagrangians, and thus won't be governed by the Euler-Lagrange equations. This is one sense in which there are solutions $\mathbf{r}_i(t)$ to the differential equations $\mathbf{F}_i(t) = m\ddot{\mathbf{r}}_i(t)$ for which there is no Lagrangian L such that the corresponding $q(t)$ are solutions to the different equations $\frac{d}{dt}\frac{\partial L}{\partial \dot{q}^\alpha} - \frac{\partial L}{\partial q^\alpha} = 0, \ \alpha = 1, \ldots, n$. Lanczos (1986) emphasizes this difference in representational scope, writing:

> ...Newton's approach does not restrict the nature of a force, while the variational approach assumes that the acting forces are derivable from a scalar quantity, the 'work function'. Forces of a frictional nature, which have no work function, are outside the realm of variational principles, while the Newtonian scheme has

---

[17]In this sense many discussions of the Euler-Lagrange equations, including that in the Appendix, are a bit misleading. There I've followed modern treatments in defining the *Lagrangian* as the difference between kinetic and potential energies, although, strictly speaking, it ought to be defined as the difference between kinetic energy and the negative work function.

no difficulty in including them.[18]

This is not to say that the Lagrangian framework cannot treat polygenic phenomena. In some cases, special techniques have been developed to accommodate polygenic forces, and in general one can always resort to the *non-homogeneous* Euler-Lagrange equations:

$$\frac{d}{dt}\frac{\partial T}{\partial \dot{q}^\alpha} - \frac{\partial T}{\partial q^\alpha} = F_\alpha, \qquad \alpha = 1, \ldots, n$$

which place no restrictions on the generalized forces (and thus place no restrictions on the underlying Newtonian forces in $\mathbb{E}^3$).[19] But of course this approach to Lagrangian dynamics is clearly tied to Newtonian dynamics, for the generalized forces are alternative ways of representing collections of (ordinary, Newtonian) forces on a system.[20] What makes the reformulation claim interesting in the context of standard presentations of Lagrangian dynamics is that forces, at least on the surface, seem to play no part. To the extent that we're forced to appeal to the non-homogeneous Euler-Lagrange equations in order to construct an interpretation of Lagrangian dynamics that might compete with the one advanced in the reformulation claim, that reformulation claim seems fairly secure.

---

[18]Lanczos (1986, p.xxv)

[19]Friction, for example, can be modeled using the *Rayleigh function*

$$\mathcal{F} = \sum_{i=1}^{N} \frac{1}{2} b_i v_i^2 (q^1, \ldots, q^n, \dot{q}^1, \ldots, \dot{q}^n),$$

where v_i is the 3-space velocity of particle i, expressed in terms of the generalized coordinates and velocities, and b_i is a constant of friction. The correct dynamics is then captured via *modifed* Euler-Lagrange equations:

$$\frac{d}{dt}\frac{\partial L}{\partial \dot{q}^\alpha} - \frac{\partial L}{\partial q^\alpha} = -\frac{\partial \mathcal{F}}{\partial \dot{q}^\alpha}.$$

See Vujanovic and Jones (1989) for general ways of treating such systems using variational techniques.

[20]See Casey (1994) for further discussion of this relationship.

3.6 Tangent Bundle Dynamics

A central feature of the Lagrangian approach is that it represents an N-particle system (with n degrees of freedom) as a single point in an n-dimensional configuration manifold M and not in $\mathbb{R}^3$, the latter representing ordinary 3-dimensional physical space in which we typically think classical particle motion actually occurs. Spatial trajectories in $\mathbb{E}^3$ can be recovered from solutions of the Euler-Lagrange equations, but the equations themselves constrain objects defined only on the configuration manifold. To the best of our knowledge there is no way of 'projecting' those equations down into $\mathbb{E}^3$ in any way that might preserve their unique (and seemingly force-free) focus on energy. A non-Newtonian interpretation of Lagrangian dynamics (i.e., an interpretation that doesn't take the Lagrangian formalism to encode a Newtonian ontology) is thus committed to M representing an ontologically primitive feature of the world. Such an interpretation, while perhaps consistent, might seem so outlandish that it couldn't compete with the Newtonian interpretation of Lagrangian dynamics – that is, the interpretation on which Lagrangian dynamics is just taken to be a particularly elegant mathematical framework for representing the Newtonian picture. This would clearly provide additional support for the reformulation claim, which on my analysis is centrally concerned with the relative merits of different possible interpretations of Lagrangian dynamics.

Making matters worse for anyone wishing to question the reformulation claim, a system's Lagrangian L isn't actually defined on the configuration manifold. Rather, as a function of both the q^α *and* the $\dot{q}^\alpha$ (and perhaps also t), it's defined on the $2n$-dimensional *tangent bundle* of M, $\mathbb{T}M$, which is constructed by joining M to each n-dimensional tangent space $\mathbb{T}_p M$ for all $p \in M$. Since the most natural strategies for constructing a non-Newtonian interpretation of Lagrangian dynamics involve taking the Lagrangian as ontologically primitive, one would thus seem committed to

the primitive reality of the $2n$-dimensional tangent bundle as well. Whereas M is constructed out of the generalized *coordinates* alone, $\mathbb{T}M$ requires n additional dimensions given by the generalized *velocities*. So even if we manage to make plausible an interpretation on which the physical world is, ultimately, n-dimensional, it's not at all clear that we can make sense of what it would mean for the physical world to be $n + n$-dimensional, where half of those $2n$ dimensions are fundamentally *different* from the other half.

3.7 Quantum-Mechanical Motivations

This distinction between whether an interpretation of Lagrangian dynamics takes $\mathbb{T}M$ or just M to be physically primitive is important. For the criteria relevant for assessing interpretative judgments in counterfactual scenarios are the criteria we *actually* use, however ill-defined they may be, not the criteria we *would* use were we to exist in those worlds. So interpretative considerations relevant to our actual best theories can help guide our interpretative judgments in counterfactual scenarios, and recently it's been argued on the basis of quantum mechanical considerations that positing a physically primitive higher-dimensional configuration space might not be so outlandish after all.

David Albert has argued that any realistic interpretation of quantum mechanics will be one that takes the wavefunction – that mathematical object representing a quantum system's state and whose dynamical evolution is governed by the Schrödinger equation – as representing a primitive feature of the physical world. Since the wavefunction is only defined on a higher-dimensional configuration space, we have no choice but to accept that higher-dimensional space as itself being physically primitive. As Albert (1996) puts it:

> ...[I]t has been essential (that is) to the project of quantum-mechanical *realism*...to learn to think of wave functions as physical objects *in and of them-*

selves...

> And of course the space those sorts of objects *live* in, and (therefore) the space *we* live in, the space in which any realistic understanding of quantum mechanics is necessarily going to depict the history of the world as *playing itself out*...is *configuration*-space. And whatever impression we have to the contrary (whatever impression we have, say, of living in a three-dimensional space, or in a four-dimensional space-time) is somehow flatly illusory.[21]

Of course, a central challenge on Albert's view is to explain how we manage to 'extract' or 'reconstruct' the ordinary three-dimensional world out of this higher-dimensional primitive reality – how we manage to create this "flatly illusory" world out of configuration space – and Albert is careful to note that this will depend (in part) on the quantum mechanical interpretation one gives. But the underlying point of Albert's argument, the point I wish to highlight here, is simply that (for Albert) a quantum mechanical realist is committed to the existence of a higher-dimensional configuration space (*not* tangent space) that is ontologically primitive in a way that ordinary three-dimensional space is not.

Albert's argument is controversial, and my aim here is not to defend this interpretative claim.[22] Rather, the point is only that an interpretative commitment to the reality of M (as opposed to $\mathbb{T}M$) might not be as big a black mark for a proposed interpretation of Lagrangian dynamics as initially thought.

3.8   The Fractional Calculus

But are there really any grounds for re-thinking the reformulation claim? Is there any interpretation of Lagrangian dynamics that can meet the obstacles canvassed in the preceding sections? I think there is. Developing this interpretation will require

---

[21] Albert (1996, p.277)

[22] For recent discussion see Monton (2002), Lewis (2004), and Monton (2006). I do not mean to endorse Albert's view, only to suggest that, were L defined only over M, the need to accept the reality of a higher-dimensional physical space in order to account for L might be less of a unique metaphysical cost to this interpretative approach than otherwise assumed.

the resources of the factional calculus, however, and so in this section I indulge in a slight digression to fill in the necessary background. My discussion here is quite brief, as only a very preliminary understanding of the fractional calculus is needed to follow the argument in the next section.[23]

Historically, the idea of fractional calculus was motivated by the following sort of question: can we make sense of the expression $\frac{d^n f(x)}{dx^n}$ for fractional n?[24] Original attempts to do so focused on constructing definitions suited to particular types of functions. For example, given that the nth-order (integer) derivatives for functions of the form $y = x^m$ are given by

$$\frac{d^n y}{dx^n} = \frac{m!}{(m-n)!} x^{m-n}, \qquad m \geq n,$$

one could generalize this to arbitrary valued of n as

$$\frac{d^n y}{dx^n} = \frac{\Gamma(m+1)}{\Gamma(m-n+1)} x^{m-n}.$$

Here Γ is the "gamma function" such that for n a non-negative integer, $\Gamma(n+1) = n!$. This definition was then used to define fractional derivatives for any function expressible as a power series.

For a number of reasons it turned out to be a good deal more complicated than this – e.g., a similar sort of generalization for exponential functions produced an inequivalent definition – and the modern treatment proceeds by defining a fractional (or "arbitrary order") *derivative* in terms of a fractional *integral*. Let ν be a rational complex number such that Re $\nu > 0$, and let $f(t)$ be a function piecewise continuous

---

[23]A more involved exposition of the fractional calculus is contained in Appendix B.

[24]The name "fractional calculus" is a bit misleading, since the definitions to be discussed below allow for n to be any rational complex number. The name survives for historical reasons, since the discipline originally grew out of an investigation into the fractional n cases. Two excellent sources on which the discussion here is based are Oldham and Spanier (2002) and Miller and Ross (1993).

on the interval $J' = (0, \infty)$ and integrable on any finite subinterval of $J = [0, \infty)$. Then for any t and a such that $t > a \geq 0$, we define

$$_aD_t^{-\nu}f(t) = \frac{1}{\Gamma(\nu)}\int_a^t (t - \xi)^{\nu-1}f(\xi)d\xi$$

to be the *fractional integral of $f(t)$ between a and t of order ν*.

Now let δ be a number such that Re $\delta > 0$, and let n be the smallest integer greater than Re δ. Define $\nu = n - \delta$ such that $0 < $ Re $\nu \leq 1$. The expression

$$\frac{d^\delta f(t)}{d(t-a)^\delta} = \frac{d^n}{dt^n}[_aD_t^{-\nu}f(t)]$$

is the *fractional derivative of $f(t)$ with respect to t of order δ*, where $\frac{d^n}{dt^n}$ is the ordinary (i.e., integer) n^{th}-order derivative with respect to t. So to form the derivative of $f(t)$ to the arbitrary order δ, one *first* integrates to the order ν and *then* takes the nth ordinary derivative. This means that the fractional derivative of a function depends on the values of that function through a given interval, instead of just the behavior of the function in an infinitesimal region around a point; this is one feature that makes fractional derivatives quite different from ordinary derivatives. (The $a = 0$ case, called the Riemann-Liouville fractional derivative, is standard.)

Despite these rather peculiar features, the definitions of the fractional integral and derivative are the foundations of a well-developed calculus. One can prove versions of the Leibniz formula and the law of exponents; introduce the Laplace transform of a fractional integral; and solve fractional differential equations. Recent work has also shown that the necessary concepts can be introduced to incorporate fractional calculus into abstract geometry.[25]

---

[25] Albu and Opriş (2009), for example, shows that we can construct a notion of a *fractional tangent bundle*. Similarly, Cottrill-Shepherd and Naber (2001) provides a preliminary characterization of *fractional differential forms*, including the properties of being closed and exact.

3.9 A Force-free Lagrangian Dynamics?

What I now want to suggest is that there's a way of re-working the Lagrangian formalism that lends itself to a rather natural non-Newtonian interpretation, and that this interpretation succeeds in avoiding the three objections outlined above. To develop this account it will be helpful first to consider an alternative way of re-working the formalism that falls short of the mark, but which highlights several relevant interpretative relationships between mathematical structure and underlying ontology that will be needed.

In accordance with the mathematical structure of the Lagrangian formalism laid out above, we suppose that an N-particle system with n degrees of freedom is associated with a real-valued Lagrangian function $L : \mathbb{T}M \rightarrow \mathbb{R}$ defined over a $2n$-dimensional tangent bundle $\mathbb{T}M$. As noted in the first objection ("Gauge Invariance") L is not unique to the system – an infinite number of other Lagrangians would describe the same dynamics – but let's set that concern aside for now; its resolution will be built into the suggested reformulation of the Lagrangian formalism. If any interpretation is even to get off the ground, we must be able to make use of Albert's argument lending plausibility to the reality of the configuration space. This means that we must find a way of re-construing the Lagrangian formalism such that the ontologically-committing objects only exist on M.

The Lagrangian is a function of both the q^α and the $\dot{q}^\alpha$, and so one strategy might be to somehow encode L's $\dot{q}^\alpha$-dependencies into a property of M itself. Then one could think of the dynamics as arising from the interaction of the system with a *modified* (i.e., $\dot{q}^\alpha$-independent) Lagrangian and some feature of the configuration manifold. L's $\dot{q}^\alpha$-dependencies generally arise on account of the kinetic energy term, and it turns out that there is an established method for encoding the kinetic energy of the system into the *metric structure* of M itself. Instead of using the metric tensor $a_{\alpha\beta}$

induced by ordinary physical space, we fix M's line element by building the system's kinetic energy into a new metric $h^*_{\alpha\beta}$ implicitly defined by the equation

$$ds^2 = 2Tdt^2 = h^*_{\alpha\beta}dq^\alpha dq^\beta.$$

Here T is expressed as a function of at least the generalized velocities $\dot{q}^\alpha$, as determined by the original metric $a_{\alpha\beta}$, and the equation provides a numerical way of implicitly defining $h^*_{\alpha\beta}$ at each point $p \in M$ in terms of $a_{\alpha\beta}$. This line element ds^2 is called the *kinematical line element*.[26]

If we restrict ourselves to conservative systems alone – systems for which $U = U(q^1, \ldots, q^n)$ and for which there's a potential energy $V = -U$ – then the kinematical line element allows us to think of the dynamics as occurring just on M. For now any $\dot{q}^\alpha$-dependencies in the Lagrangian have been built into $h^*_{\alpha\beta}$. When adapted to $h^*_{\alpha\beta}$, the Euler-Lagrange equations tell us that the system particle moves on M in such a way that the magnitude of its acceleration vector in any direction, at any point, is equal to the negative derivative of the system's potential function, in that direction, at that point. That is, letting the dynamical evolution of the system be given by $\gamma : t \to M$ such that $\gamma(t) = (\gamma^1(t), \ldots, \gamma^n(t))$, the modified Euler-Lagrange equations state:

$$\frac{d^2\gamma^\alpha(t)}{dt^2} = -\frac{\partial V}{\partial q^\alpha}, \qquad \alpha = 1, \ldots, n.$$

This form of the Euler-Lagrange equations[27] have a natural force-free interpretation on M: Instead of taking the Lagrangian as physically primitive, the potential V

---

[26]See, e.g., Lanczos (1986, pp.17–24) and Synge (1927).

[27]See, e.g., Synge (1927, p.47). Note that this result also holds for monogenic systems in general, where the potential energy is replaced with the (negative) work function, and for polygenic systems involving generalized forces. Synge (1927), for example, derives the equations using generalized forces. In that case the Euler-Lagrange equations simply reduce to the claim that the magnitude of the system particle's acceleration in a given direction is equal to the magnitude of the generalized force in that direction. These formulations would be of little help in constructing a force-free interpretation on M, though.

could be taken as a physically real field dynamically interacting with our system in a configuration space whose metric is given by $h^*_{\alpha\beta}$. For a conservative system $h^*_{\alpha\beta}$ would be fixed, but I note for future reference that it could just as easily be taken as a second physical field over M were the formulation to permit systems of varying energy.

The kinematical line element formulation suggests a way of formulating Lagrangian dynamics on the configuration manifold alone, but in the process it runs up against the other two objections in a rather flagrant way. By construction it applies only to conservative systems, and hence has an even more restricted range of applicability than the standard formulation of Lagrangian dynamics. Moreover, although the Lagrangian function no longer plays an explicit role in the modified formulation, potential energy exhibits a comparable gauge symmetry, and thus faces a similar argument against its primitive reality. What we would like – what would make a force-free Lagrangian dynamics a genuine interpretative possibility – is a formulation of the Euler-Lagrange equations that restricted attention to M in a similar way as the kinematical line element formulation, but which does so both without restricting the sorts of systems to which it applies and without requiring an ontological commitment to gauge invariant quantities like L and V.

There *is* such a formulation, although it's not traditionally recognized as such. In addition to the kinematical line element, a *monogenic* system can be represented in a configuration space equipped with a third type of metric structure, as given by the *action line element*

$$ds^2 = 2(E - U)Tdt^2 = (E - U)a_{\alpha\beta}dq^\alpha dq^\beta = h_{\alpha\beta}dq^\alpha dq^\beta.^{28}$$

---

[28]Synge (1927, p.56). Synge constructs the action line element using the potential V, not U, but nothing requires this. Since $h_{\alpha\beta}$ is here being given an implicit numerical specification, any well-defined, smooth function U will do. Of course, if a system has no potential energy function then its total energy E isn't constant, and thus $h_{\alpha\beta}$ won't be either. This, I assume, is why Synge uses V. Here we're allowing the metric to vary, though, so that constraint need not constrain us. Note

When M is equipped with the metric $h_{\alpha\beta}$, the Euler-Lagrange equations turn out to have a surprisingly simple form: the integral curves specifying the dynamical evolutions of the system (for different initial conditions) *just are* the geodesics defined by $h_{\alpha\beta}$.[29] So if we take the metric $h_{\alpha\beta}$ to represent a real tensor field over the configuration space, then we have a force-free ontological interpretation available according which dynamical evolution arises from interactions between a system and the metric structure of configuration space, much as the field equations of general relativity are often thought to specify a relationship between the distribution of matter in space (or space-time) and the metric structure of that space.

Like the kinematical line element formulation, the modified Euler-Lagrange equations of the action line element only make reference to objects defined on M, not on $\mathbb{T}M$. But, in addition, building *all* of the dynamics into the metric structure provides a solution to the gauge invariance problem. Although the work function used in defining the action line element will still exhibit gauge symmetries for conservative systems, the function now only serves as a mechanisms for helping to fix relative distances between points in configuration space, not as a primitive field itself. Gauge invariant work functions will determine the same relative distances and hence a *single* overall metric structure, so this interpretation doesn't commit itself to an entity whose mathematical representation exhibits a gauge invariance.

Of course, we've assumed that our system is monogenic, and so yet again we run up against the problem of polygenic phenomena. There doesn't seem to be any way of taking a formulation of the Euler-Lagrange equations on M and extending it to systems having no work function (let alone no potential energy function). But here we should note an important difference between the kinematical and action line elements.

that when understood this way E (*defined* as $T+U$) can't always be interpreted as representing the system's total energy, but again this poses no barrier to defining $h_{\alpha\beta}$.

[29]Here we assume unconstrained motion. When a system is constrained to move in various ways, Synge (1927, pp.59–60) shows that the system particle obeys a principle of least curvature (for either holonomic or non-holonomic constraints).

In building the work function into the metric structure, the action line element is – at least in principle and unlike the kinematical line element – capable of representing on the configuration space the dynamics of systems whose work functions aren't straight-forward functions of the generalized coordinates (e.g., monogenic systems whose work functions depend on the $\dot{q}^\alpha$). So if there were a way of constructing work functions for arbitrary polygenic forces, then the action line element formulation of the Lagrangian formalism *would* support a force-free interpretation – there *would* be a coherent force-free version of Lagrangian dynamics.

And it turns out to be a rather remarkable fact that there *is* a way of doing this, of constructing a work function for any arbitrary collection of (possibly polygenic) forces. Rabei *et al.* (2004) have shown that the Laplace transform of fractional integrals can be used to construct potentials (i.e., work functions[30]) for arbitrary forces. Letting q^β be a set of arbitrary variables (e.g., $q^1 = \dot{x}$ and $q^{1/2} = \frac{d^{1/2}x}{d(t-a)^{1/2}}$), and $F(q^\beta)$ be any arbitrary function of those variables, $F(q^\beta)$ can always be characterized by a potential function of the form:

$$ U = (-1)^{-(\alpha+1)} \int \left[\mathcal{L}^{-1}\{\frac{1}{s^\alpha}\mathcal{L}\{F(q^\beta)\}\} \right] dq^\alpha $$

where

$$ F(q^\beta) = (-1)^{\alpha+1} \frac{d^\alpha}{d(t-a)^\alpha} \frac{\partial U}{\partial q^\alpha}, \qquad \alpha, \beta \geq 0. $$

Here $\mathcal{L}$ is the Laplace transform of a fractional integral and s is the variable associated with the Laplace transform.[31] So, mathematics aside, it looks as though there is a way of representing polygenic phenomena on the action line element formulation, and thus a coherent force-free account of Lagrangian dynamics.

---

[30]Rabei *et al.* (2004)'s use of the term 'potential' corresponds to my 'work function'. They do not take the potentials they construct to be energies.

[31]See, e.g., Oldham and Spanier (2002, p.133–136) for discussion of the Laplace transform in the fractional context.

In order for my argument to succeed, though, one must appeal to fractional derivatives, and such mathematical oddities might, with some justification, be regarded with suspicion. We must let a system's work function contain fractional derivatives, and hence we must also permit the Lagrangians associated with arbitrary physical systems to contain fractional derivatives. Is there any reason to think the standard Euler-Lagrange equations would still hold if such obscure mathematical dependencies were permitted? There is. Recent work in fractional calculus has shown that the Euler-Lagrange equations can be adapted to account for Lagrangians that depend on fractional derivatives. As Riewe (1996) formulates them, for example, the generalized Euler-Lagrange equations are expressed as follows:

$$\sum_{n=0}^{N}(-1)^{s(n)}\frac{d^{s(n)}}{d(t-a)^{s(n)}}\frac{\partial L}{\partial q_{s(n)}^{\alpha}}=0,$$

where N is the number of derivatives (of any order) in the system's Lagrangian, $s(n)$ is the order of the nth derivative (when all derivatives are indexed with an integer), and $q_{s(n)}^{\alpha}$ is the $s(n)^{th}$-order derivative with respect to the coordinate q^{α}.[32] So we *do* preserve the dynamics represented in the Euler-Lagrange equations even when we allow for a wider class of functional dependencies, and thus the proposed force-free interpretation really is an interpretation of Lagrangian dynamics.

On the non-Newtonian interpretation suggested here, then, possible worlds are represented by points in a higher-dimensional configuration space. Different broad *types* or *classes* of worlds correspond to different metric structures over the configuration space. The law stating the temporal evolution of individual systems is then

---

[32]See, e.g., Riewe (1996), Riewe (1997), and Albu and Opriş (2009). Although the notation involved in stating the Euler-Lagrange equations for fractional Lagrangians is considerably more cumbersome, in essence there is no qualitative different from the usual Euler-Lagrange equations. But now, instead of the equations just ranging over $\alpha = 1, \ldots, n$, one for each generalized coordinate q^{α}/generalized velocity $\dot{q}^{\alpha}$ pair, we must include additional equations (of the same form) for when a fractional derivative is taken.

given by the principle that the 'configuration points' follow geodesics through this space.

3.10 Conclusion

Physicists often claim that Lagrangian dynamics is a reformulation of Newtonian dynamics, at least in the finite-dimensional case. This essay has addressed two questions concerning this reformulation claim: what does this claim mean, and is it justified? I've argued that we can make sense of the claim, despite the fact that it concerns the ontology of a false theory, as making a counterfactual assertion about our interpretative judgments. After considering various interpretative criteria that might be invoked to defend the reformulation claim, I sought to cast doubt on its justification by suggesting a non-Newtonian interpretation that takes the world's primitive ontology to be explicitly represented in Lagrangian dynamics. In doing so, I've tried to illustrate the role theory *interpretation* plays in our – or at least the realist's – notion of theory *reformulation*.

CHAPTER IV

Underdetermination and the Content of Scientific Realism

The underdetermination argument was once thought to have sounded the death knell of scientific realism. If the evidence used in the construction and support of our best scientific theories always provides an equal measure of support for other, competing theories – in short, if theories are always *underdetermined* by the evidence – then belief in their approximate truth is unjustified. More precisely:

> PREMISE 1: Empirical equivalence implies epistemic equivalence.
>
> PREMISE 2: All theories have empirically equivalent rivals.
>
> CONCLUSION: Belief in any individual theory is epistemically unjustified.

The first premise, which we'll call the *equivalence premise*, remains a stalwart of empiricist epistemology, and the second, the *ubiquity of empirical equivalence (UEE) premise*, was thought by many to be obviously true in virtue of first-order model-theoretic considerations.[1]

Recently, the underdetermination argument has fallen on hard times. Realists

---

[1] See, e.g.,Quine (1975) and Earman (1993).

have dismissed it as preying upon logical contrivances altogether removed from scientific practice, or as treating the mere possibility of widespread underdetermination as though it was actually the case. Anti-realists have followed suit. Stanford (2006) argues that even under charitable circumstances the argument fails to raise any *distinctive* challenge for scientific realism, being instead a re-packaging of traditional philosophical concerns with which everyone, realist and anti-realist alike, must cope. There has thus been a shift in the scientific realism debate towards other issues.[2] Underdetermination just isn't thought to be the threat to realism it once was.

Is this shift warranted? That is, in part, the question this essay aims to address – or at least to lay the groundwork for addressing. Two things must first be specified: an account of the argument that makes clear what sorts of considerations are relevant for adjudicating it, and a clear articulation of the realist position taken to be its target. The former has received much (albeit misguided) attention, and the latter has generally been thought to be unproblematic. For reasons developed below, both of these issues warrant re-thinking. The central contribution of this essay is to re-frame and clarify the threat of underdetermination by providing novel accounts of how these two precursors ought to be understood, in the context of foundational physics.

In the first half of this essay I assess Stanford's anti-realist rejection of the underdetermination argument, and offer a reformulation that makes the issues at stake, and the parameters relevant for deciding them, perspicuous. In the second I consider how realism about foundational physics ought to be understood in the context of underdetermination. Here issues of ontological interpretation become relevant, as I use seemingly unproblematic cases of 'interpretative underdetermination' as a methodological tool for (partially) delimiting the content of scientific realism.

---

[2]For example, Worrall (1989) sites the pessimistic meta-induction as being the single greatest threat to scientific realism, and is largely dismissive of underdetermination concerns.

68

4.1 Stanford's Collapse Argument

Kyle Stanford has argued that underdetermination doesn't pose a distinctive problem for scientific realism, and should be abandoned in favor of alternative anti-realist strategies.[3] His argument is a two-parter. Part one argues that the difficulties ostensibly raised by underdetermination actually collapse, on further analysis, into well-known philosophical concerns that don't bear any special relationship to scientific realism. There is thus no problem of underdetermination the scientific realist incurs in virtue of being a scientific realist. Part two argues that anti-realists should instead embrace the "problem of unconceived alternatives", which he thinks is a more satisfactory way of capturing much of the spirit of the underdetermination argument. My discussion here is generally confined to the first part of Stanford's argument.

The basis for Stanford's argument is the observation that defenses of the underdetermination argument unwittingly end up collapsing into arguments having nothing to do with scientific realism. The scenarios he describes arise on account of attempts to justify the UEE premise – the premise that all theories have empirically equivalent rivals. Kukla, for example, has sought to establish this premise by appealing to a general sort of 'algorithmic strategy'.[4] For every theory proposed, he purports to provide a method for generating an empirically equivalent rival. But the sheer generality of his claim seems to undermine whatever relevance it might have had to the scientific realism debate. For the only sorts of algorithms taking *any* possible theory as input and generating an empirically equivalent rival seem to invariably trade on contrivances irrelevant to science. How else to ensure that the algorithm works for *any* theory? Consider, for instance, the following algorithm, characteristic of Kukla's approach: For any theory T, the algorithm generates an empirically equivalent rival theory T^* holding that "the world behaves according to T when observed, but some

---

[3]Stanford (2006)
[4]See, e.g., Kukla (1996) and Kukla (1998).

specific incompatible alternative otherwise".[5] Stanford objects as follows:

> *[W]hether or not* such farfetched scenarios are real theories they amount to no more than a salient reminder in a scientific context of the general possibility of the sort of radical skepticism captured by a famous thought experiment development by Descartes: that there might be an all-powerful 'Evil Demon' who devotes his energies to deceiving us about what the world is really like. (Stanford, 2006, p.12)

Stanford is surely onto something here. If the *only* way of establishing the UEE premise is by invoking scenarios that also seem to license radical skepticism, then underdetermination wouldn't pose any problem for the scientific realist in virtue of her epistemic attitude towards science itself.

Similarly, Stanford argues that another attempt to justify the UEE premise, more limited in scope, falls prey to another classical epistemological problem – the problem of irrelevant conjunction:

> Consider the now-famous example of TN(0): Newtonian mechanics and gravitation theory, including Newton's claim that the universe is at rest in absolute space. This theory supports any number of empirical equivalents of the form TN(v), where v ascribes some constant absolute velocity to the universe... The sensible realist will surely insist that we are not here faced with a range of competing theories making identical predictions about the observable phenomena, but instead just a single theory being conjoined to various factual claims about the world... This realism should no more extend to the conjunction of Newtonian theory with claims about the absolute velocity of the universe than with claims about the existence of God.(Stanford, 2006, pp.13–14)

Again, since this sort of problem is a problem for most everyone, not just the scientific realist, Stanford concludes that, surface impressions to the contrary, this defense of the underdetermination argument doesn't actually raise any special problem for scientific realism.

In general, then, Stanford's argument against underdetermination is that all such efforts to justify the UEE premise end up collapsing the underdetermination argument into some other problem. The underdetermination argument, that is, just takes problems afflicting everyone and re-packages them so as to suggest that they raise

---

[5]Stanford (2006, p.12)

a special challenge for scientific realism. If the underdetermination argument raises problems that are really problems for everyone (or almost everyone), and not just the scientific realist, it hardly seems legitimate to use those problems to undermine scientific realism. This would be like objecting to scientific realism on the grounds that it didn't include an argument against brain-in-vat skepticism. That form of skepticism is an unresolved issue, and there's *some* sense in which it counts as an objection to scientific realism, but it hardly seems relevant to the issues thought to be at stake in the *scientific* realism debate.

This strategy can be extended in a way Stanford doesn't consider. Even if the realist concedes that all theories have empirically equivalent rivals, she's likely to maintain that our best scientific theories occupy privileged epistemic positions in virtue of features going beyond mere adherence to the empirical data – that is, in virtue of so-called "extra empirical" or "theoretical" virtues. Part of the realist's reply imagined here makes a substantive claim about science: the claim that our best scientific theories fare better than possible (empirically equivalent) alternatives with respect to these virtues. The other half is a rejection of the equivalence premise, which says that empirical equivalence implies epistemic equivalence. Although clearly bearing on issues in science, this premise states a very general epistemic position having implications for a good many philosophical views – e.g., about ordinary, run-of-the-mill inductive and abductive reasoning. It is not a premise, that is, uniquely targeting the scientific realist, for the role and justification of such extra empirical virtues is an epistemic problem some anti-realists (e.g., Stanford himself) must also confront. As such, it's a third way that debates over underdetermination can collapse – and have collapsed – into other philosophical problems that aren't unique to scientific realism. Indeed, this is arguably *the* issue to which discussions of underdetermination often reduce, as the status of these virtues often underpins disputes about general

71

skepticism and irrelevant conjunction.

4.2   Reformulating the Underdetermination Threat

To condemn the literature on underdetermination is not to condemn underdetermination itself. Stanford has provided a cogent analysis of how existing anti-realist efforts to justify the UEE premise reduce to more general philosophical concerns, and I have argued that a similar collapse often occurs with the equivalence premise. Neither of these claims impugns the underdetermination argument directly, though. What they show is that the literature on this topic is often confused or at cross-purposes. Rather than *constituting* the underdetermination threat, these side issues – issues to which arguments concerning underdetermination often collapse – seem to have obscured the real concern.

So then what *is* the threat posed by underdetermination – a threat that the scientific realist faces *simply in virtue of those beliefs constituting her scientific realism*? Without an answer Stanford's diagnosis starts to look more compelling. There are really three related issues here, which I'll address in turn. In what sense might the notion of underdetermination pose a distinct problem for scientific realism? What constraints or parameters are in place for adjudicating the debate? And how ought the argument to be formulated so as to accurately reflect this?

I take it to be uncontroversial that the accounts of the world offered by foundational theories – indeed, by most scientific theories – outrun the empirical evidence used in their construction. This is the sense in which physical theorizing 'goes beyond' the empirical data, and manages to underwrite new and better understandings of what the world is ultimately like. When the scientific realist endorses a particular theory T, she's endorsing one way of going beyond the data – the way that scientists have proposed for extending our beliefs about the world – over the multitude of other

possible theories. Some of these 'belief extensions' will be ridiculous and unscientific, whereas others will (or may) give scientists pause. The anti-realist, of course, seeks to undermine this endorsement, and there are various strategies he might pursue. He might, for example, seek to undermine the very methods used by science to extend our beliefs – to argue that they are bankrupt in one way or another, or fail to license the sort of epistemic attitude involved in the realist's endorsement of a theory. Alternatively, and quite distinctly, the anti-realist might very well embrace the methods of science in principle (at least for the sake of argument) but argue that there's some reason to think their actual application provides inconclusive (or even defective) results. This is the sense in which I suggest we think of underdetermination. The essential idea behind the argument is that the methods of science have not issued (and will not issue) univocal judgments concerning which theories are best suited for endorsement – the belief extensions scientists have proposed on their behalf are not unique. Because other, incompatible accounts fare equally well with respect to science, the scientific realist has no grounds for endorsing one extension over the other.

This way of understanding underdetermination differs in two important ways from how underdetermination is typically characterized. Assuming the threat it poses to realism is real, it is, in the first instance, an entirely *contingent* argument. It makes no claim about how scientific methodology must function in any possible world, but only about how it does (and will) in the actual one. Just as naturalistically-inclined realists like Boyd and Psillos maintain that we're epistemically lucky to live in the world we do, the anti-realist argues for our epistemic unluck.[6]

Second, on this construal of underdetermination the threat raised is distinctive to scientific realism; it does not turn upon some other set of beliefs held by the scientific realist. The justifications for the general methodologies and epistemic norms implicit in scientific practice are not what's at issue here. What's at issue is whether the be-

---

[6](Boyd, 1981), (Psillos, 1999)

lief extensions – the theories – licensed by those methodologies and norms are unique (with respect to the empirical evidence) *from the point of view of those methodologies and norms themselves.* The general legitimacy of extra-empirical epistemic norms or the justification of ordinary beliefs about the external world may be fair things to question in a broader context, but they are not what the underdetermination argument *against scientific realism* is about.[7] They must be screened off in this context, where the issue concerns whether there are alternative accounts of the world that are as good as our existing accounts *by the standards of science itself.* So if underdetermination is a problem for the realist, it's a problem in virtue of her endorsement of the theories produced by actual applications of scientific norms and methodologies, not by a general adherence to such norms and methodologies.

It is perhaps not a stretch to euphemistically describe this sort of underdetermination as a Trojan horse. The idea is to meet the realist on her own ground by accepting her preferred epistemic norms and standards (if only for the sake of argument), and then try to vitiate her position from within by showing that those very norms and standards imply that her pro-attitude isn't warranted. There are lots of other, complementary ways the anti-realist might wish to attack realism, but they need to be screened off if we're to investigate the threat of underdetermination.

How ought an underdetermination argument of this sort to be formulated? The traditional two-premise formulation has contributed much to the confusion surrounding this topic. In the first instance, it has suggested – via the equivalence premise – that the adequacy of the realist's epistemic norms and the methodologies of science itself are up for grabs, when in fact they're held fixed for the purposes of assessing the underdetermination threat. The traditional UEE premise has also contributed its

---

[7]This is not to suggest that the threats raised by such problems aren't problems of underdetermination *per se.* Many forms of skeptical argument invoke general patterns of underdetermination. But as previously explained – in agreement with Stanford – those instances of underdetermination don't place a burden on the scientific realist that isn't shouldered by many others as well.

share of confusion. By making a claim about *all* theories, it suggests that there are no constraints on the sorts of theories at the anti-realist's disposal for the purposes of establishing the UEE premise, and it's precisely the outlandish and unscientific ones that starts one down the road towards general skepticism

The formulation I think best captures the threat posed by underdetermination is the following:

PREMISE 1: For every theory T whose endorsement fares well (i.e., is justified) with respect to the epistemic standards and methodologies of science, there's an alternative theory T' whose endorsement fares equally well.

PREMISE 2: Theories whose endorsements are on an epistemic par ought to be treated, epistemically, the same way.

CONCLUSION: Realists are – and by their own lights should take themselves to be – unjustified in endorsing any theory faring well with respect to the norms and methodologies of science.

The locution I've used to characterize the realist's position – her *endorsement* of a theory – clearly bears much of the argumentative weight in this formulation. That's by design. So what does it mean? It could mean any of a variety of things, depending on the particular form of realism at issue. On many contemporary accounts of scientific realism, to endorse a theory T is to believe in the approximate truth of T. But this is not the only attitude one might take towards theories that would count as realist, and clearly the force of the underdetermination argument will depend on how one understands what it means to endorse a theory. This makes intuitive sense: the threat of underdetermination ought to depend on the precise claims about science that are asserted to be underdetermined.[8] Disambiguating the locution of endorsement allows

---

[8]*c.f.* van Fraassen (1980), who characterizes realism in terms of the *aim* of science. One reason I find such a description puzzling is that I wonder whether activities without rigid and well-defined rules (such as games) can have collective aims over and above the aims of the individual agents participating in those activities. This would undercut the normative element van Fraassen presumably intends realism to have.

us to separate not just different forms of realism, but varying realist attitudes towards different types of theories. Insofar as the realist's inclined to adopt different attitudes toward different branches of science – a possibility that's been underrepresented in the literature on realism – the threat of underdetermination may well vary. My interest here is with foundational physics, and with the underdetermination threat in that context. How should we understand what it means to endorse a theory of foundational physics? That is the topic of the next section.

4.3 Approaching the Content of Realism

To assess the threat of underdetermination for realism about foundational physics, as I have re-formulated the argument, we must first know what it means for the realist to *endorse* a theory of foundational physics. There is no right answer to this question, only answers more or less susceptible to underdetermination (and other sorts of objections). Different realists answer this question differently. There are, however, certain types of cases of *alleged*[9] underdetermination in foundational physics – cases that might be thought to ground an anti-realist argument to the effect that underdetermination is widespread in foundational physics – which are generally taken by realists to be *un*problematic.[10] Many of these cases appeal to the role of ontological interpretation in discerning physical content. On the surface at least, they concern genuinely distinct and ostensibly scientific accounts of the world, each seeming to fare equally well with respect to the norms and methodologies of science. In dismissing such cases and arguments for rampant underdetermination – and I emphasize that

---

[9]In calling them 'alleged cases' I do not mean to suggest that anyone has actually advocated for such cases. Indeed, that no one has provides further support for the claim that they are widely viewed as illegitimate.

[10]This is not to suggest that all such cases of alleged underdetermination based on interpretation are viewed as unproblematic. See, e.g., Belousek (2005) and Fraser (2009) for discussions a underdetermination in foundational physics on account of theory-specific interpretative problems. As will be clear below, the cases of interpretation-based underdetermination I have in mind are much more general than these theory-specific sorts.

I think such actions are, intuitively, justified – the realist thereby tells us something about the content of their realism, whether that content is explicitly acknowledged or not.[11] For only if their realist endorsement of a theory fails to differentiate between such purported rivals will this blithe attitude make sense – will the rivals not pose an underdetermination threat to their realism.

Thus the aim of this section is to articulate a realist account of what it means to *endorse* a foundational theory in a way that doesn't fall prey to intuitively misguided cases of alleged underdetermination. In this sense, it offers an answer to the question, 'How *ought* realism about foundational physics to be understood in light of the central role considerations of ontological interpretation play in determining physical content?'[12]. Not surprisingly, the account offered here will differ in some surprising ways from an account of endorsement in terms of straight-forward approximate truth of a theory.[13] Although it will remain an open question whether this subtler approach to realism about foundational physics can avoid a more robust underdetermination threat, the target – and the argument – will now be clearly drawn.

The strategy pursued in this essay may seem like a methodologically perverse way of addressing the relationship between ontological interpretation and the content of realism. Shouldn't we focus instead on those instances of interpretative underdetermination that seem to pose genuine problems, and ask how realism could be adapted?

---

[11]In some instances there appears to be a tension between how realists characterize their view and the ability to avoid falling victim to these intuitively bankrupt cases of alleged underdetermination. But that is perhaps a topic for another paper.

[12]As discussed in the introduction, the 'central role' in question is the idea that the physical content of a foundational theory is always mediated by an interpretative mapping from the theory's mathematical formalism to features of the physical world. This is true of foundational theories whether they possess unique, internal conceptual problems or not (although philosophers of physics have been particularly focused on the role of interpretation in those latter cases). So in talking about the role of theory interpretation in foundational physics, I'm *not* talking about the fact that some foundational theories possess conceptual problems that seem to demand an interpretative resolution. I'm discussing the more general (and seemingly innocuous) fact that determining the physical content of a theory always proceeds via an interpretation of the mathematical formalism.

[13]Of course, this is not to suggest that analyzing the notion of approximate truth is itself straight-forward.

No. Those cases tend to turn on highly theory-specific features not shared with other theories of foundational physics, and we shouldn't assume that the realist maintains the same attitude towards all theories independently of whatever unique conceptual problems they may have. Such conceptual problems may count as a reason for the realist to shift her default realist attitude. It is thus difficult to draw any conclusions about the general content of realism from such theory-specific cases of interpretative underdetermination. But we *may* be able to learn something about the content of realism about foundational physics in general – of what it ought to mean to endorse a theory of foundational physics in general – by considering the central role ontological interpretation plays *across the board* in determining the physical content of foundational theories. Looking at intuitively unproblematic general cases of 'interpretative underdetermination' provides one way of probing that issue.

4.4 Non-Problem Cases

It will be helpful to start by looking at two sorts of cases that don't make explicit appeals to interpretative concerns.

4.4.1 Numerical Variants

The first type of case the anti-realist might invoke to demonstrate the ubiquity of underdetermined rivals is grounded in the role numerical constants play in physical theories, and in particular in the fact that those constants are generally only known to a certain degree of precision. The best estimates of the gravitational constant G, for example, put its value at $G = 6.67428 \times 10^{-11} \frac{\text{m}^3}{\text{kg s}^2}$, with an uncertainty of one part in 10^4. Adapting an example from Wilson (1980), the anti-realist might try to exploit this limitation in precision to construct a host of rival theories that differ only in extremely small variations of G's value – say, NGT$'$ (with $G' \equiv G + 10^{-40}$) and NGT$''$

(with $G'' \equiv G + 10^{-80}$). There are an infinite number of such alleged rivals, and, in a very strict sense, they all make incompatible claims about the empirical world. In the most straight-forward sense, then, they would appear to be different – and scientific – theories, all on an epistemic par given the norms and methodologies of science. Given the prevalence of physical constants in all known (and most likely future) foundational theories, one can imagine an anti-realist claiming this demonstrates the presence of rampant underdetermination in foundational theorizing.

The realist is surely correct in dismissing this sort of argument as misguided. Theories like NGT′ and NGT″ *just are* different theories – different 'specifications' – of Newtonian Gravitation Theory, and in her endorsement of NGT she has no intention of singling out one such specification as correct. Her claim about the world is more coarse-grained, and doesn't distinguish between the infinite number of more fine-grained possibilities. Similar considerations would apply to any other physical constant-laden theory she endorses. This imagined argument for rampant underdetermination, even if successful, turns on an appeal to theories that no realist actually endorses. The realist is inoculated.

To stop here, though, as one might be tempted to do, is to miss an important insight about the *content* of realism. The realist is surely right to dismiss the (alleged) underdetermination between NGT′ and NGT″ as being problematic for her realism about NGT. However, as accounts of the world, NGT′ and NGT″ are clearly incompatible – there are empirical claims about which they disagree – and yet they both fall under the banner 'Newtonian Gravitation Theory'. While accepting that the realist is clearly right not to see these rivals as problematic in the course of asserting her realism about NGT – not to see them as rivals between which she is forced to choose in virtue of their compatibility with the empirical evidence supporting NGT – there is still the question of what her realism *about NGT* amounts to in light of the

fact that the theory she endorses admits an infinite number of incompatible versions. In professing her adherence to Newtonian Gravitation Theory, how can the content of her realism be understood such that it contains as instances – or fails to discriminate between – empirically incompatible accounts of the physical world?

Whatever it might mean for the realist to endorse NGT, one thing seems clear: insofar as G is only taken to be a (best) *estimate*, the realist about NGT is committed to there being some further theory like NGT′ or NGT″ – some further 'specification' of NGT – warranting her endorsement in a way that other specifications of NGT do not.[14] We can imagine the realist dismissing some of these rivals as empirically inadequate as better experimental data becomes available. To deny this existential claim would either undercut her claim to realism or make her susceptible to the underdetermination strategy outlined above. So although the existence of underdetermined theories like NGT′ or NGT″ doesn't pose problems for realism about NGT, it's not correct to say that our realist has *no* beliefs about these more fine-grained specifications. She believes some merit her endorsement more than others, she just doesn't know which ones. It is part of (or follows from) her realism about *NGT itself*, then, that the following existential claim holds: of the many incompatible and empirically adequate theories falling under the label 'Newtonian Gravitation Theory' – of the many specifications of NGT – some merit her endorsement more than others. Perhaps one even merits her endorsement most of all.

This obviously doesn't tell us what it means to *endorse* a particular theory, but it does emphasize a feature of realism that often goes unrecognized. Standard discussions of realism often assume that the realist, in endorsing a particular theory T, is endorsing a single description of a particular domain of phenomena. And in some sense that is right, insofar as NGT on the surface seems to offer a single description of the world. But what the case also illustrates is that, even before questions of inter-

---

[14]Clearly, the situation described above then arises for the more specific theory.

pretation arise, the realist about T is, in principle[15], already committed to a certain sort of vagueness or incompleteness in T's description of the world, at least for those theories invoking physical constants.[16] This vagueness doesn't seem to present an obstacle to the coherence of the realist's endorsement, but may help us to figure out what that endorsement means.

4.4.2 Purely Metaphysical Rivals

A second conceivable underdetermination strategy the anti-realist might employ concerns what we might label *purely metaphysical rivals*. Here the idea is to appeal to theories differing only with respect to some metaphysical issue that, at least on the surface, looks to be largely irrelevant to scientific practice and applications. Take the metaphysical debate over the nature of properties: when objects are judged to have a common property, such as being purple, do we literally take the same thing (a 'universal') to exist in both – to exist really in two places at once – or are we just expressing a certain resemblance between fundamentally distinct aspects of both? Competing answers to this question might be thought sufficient to characterize rival theories. The rivals in question are otherwise identical physical theories, differing only with respect to their views regarding the nature of physical properties.[17] Different attitudes about the metaphysics of causation could be put towards similar ends, one theory invoking a counterfactual analysis of causation and the otherwise identical rival invoking a transference analysis (or perhaps eschewing talk of causation altogether). The theories in these pairs are genuinely distinct: they really do possess quite different fundamental ontologies. Many of the statements each make about the

---

[15]Clearly, the *ontologies* of the different NGT specifications are identical.

[16]The existential claim suggested here complicates the task of characterizing realism in terms of a straight-forward pro-attitude about theories, as it embeds the pro-attitude within the scope of an existential operator. One advantage of adopting the locutions associated with *endorsement* at this stage of the argument is that it allows me to dispense with this complication.

[17]This preliminary characterization is actually a bit misleading. See note 18 below.

physical world, although expressed using the same typographical symbols (the same words and sentences, e.g.), actually *mean* different and incompatible things. To say that all electrons have negative charge is to make a different physical claim depending on one's view of the metaphysics of properties.[18]

One might suspect competing theories of this sort arise only by flagrant appeal to pseudo-scientific elements and features. There seems to be, superficially at least, little scientific value in metaphysical analyses of properties or causation, and so perhaps the realist can dismiss these alleged rivals as falling outside the re-formulated framework of the underdetermination argument. However, that a consideration doesn't seem relevant to science as currently practiced need not mean it isn't part of science, at least if we intend that notion to be something more than a descriptive distinction concerning science as practiced today. The metaphysical views discussed above make specific claims about what the physical world is like, and help to articulate what the claims of 'ordinary scientific theories' really mean. So despite their remoteness from ordinary experience, and in the absence of well-motivated *and realistically-acceptable* criteria for what constitutes science, we should be extremely cautious about dismissing purported rival theories like those described above as pseudo-scientific.

Moreover, related examples turn on disputes less obviously removed from the concerns of scientists. Whether the Einstein equations of general relativity ought to be understood as expressing a constraint condition holding between space-time geometry and the distribution of stress–energy, or whether they reflect a dynamical or causal relationship, remains an open question about which many physicists are not indifferent; yet it arguably bears little on applications. And surely the question of

---

[18] This is, incidentally, one reason why this strategy for showing the ubiquity of underdetermined rivals doesn't collapse into the problem of irrelevant conjunction. Although our preliminary characterization of the rivals is that they differ only with respect to the nature of properties – and thus the differing metaphysical views might be thought to be irrelevant conjuncts – in fact this seemingly localized difference has important consequences for the entire theoretical picture. It percolates through the entire physical account, as it were.

how to interpret $E = mc^2$ – whether it expresses a functional relationship between the *values or magnitudes* of energy and mass, considered as distinct properties, or whether it reflects the idea that energy *is the same thing as* mass – is of scientific interest, but again not relevant to how the theory of special relativity actually gets used.[19]

These interpretative questions, like the more esoteric metaphysical issues raised above, can be the source of rival theories. As in the previous examples, the physical claims on which they most obviously differ cannot be 'quarantined' or 'shielded' from the many sentences to which they both seem to assent, but rather are instrumental in determining the *meanings* of those sentences. This is not a case of irrelevant conjunction. The interpretative rivals offer entirely different physical accounts of the world. Of course, the examples in this second set are specific to individual theories, and thus don't immediately generalize to all theories. By themselves they couldn't ground an argument for rampant underdetermination. However, insofar as they both turn on competing interpretations of the equality sign ('='), one might think the overall strategy for generating rival theories could be extended to any equation-based scientific theory. This includes, to say the least, all of foundational physics.

The realist is again likely to be dismissive of such a demonstration of rampant un-derdetermination – and, also again, rightly so.[20] Like the case of numerical variants,

---

[19]It's possible that a situation might arise in which competing interpretative views of this sort *are* somehow relevant to scientific practice and applications. That would be noteworthy. If past episodes are any guide, realists, and also physicists, would then take these individual cases of un-derdetermination much more seriously. Our interest, however, is with the realist's *existing* attitude towards these alleged rival theories.

[20]One possibility is that the alleged rivals aren't on an epistemic par. After all, metaphysicians argue over these analyses, and the arguments they offer are presumably intended to carry epistemic weight (even if those arguments are primarily conceptual or *a priori* in nature). So if one has a preferred account of, say, properties or causation, one presumably thinks that these alleged rivals *aren't* epistemically equivalent – the theory in which the central equations are understood via a transference account of causation might be taken to be epistemically superior to the corresponding counterfactual-based theory. But that, I think, is an extremely small percentage of scientific realists. Most have no view whatsoever concerning the nature of properties or causation – or, what is more, think that we ought to look to science itself to help us answer these metaphysical questions. See, e.g., Maudlin (2007). Yet realists almost universally dismiss these cases as unproblematic, so a broader

the alleged rivals seem to characterize pictures of the world that are more fine-grained than anything the realist means to commit herself to in endorsing a particular theory. In her realist endorsement of a given theory T, that is, she doesn't intend her assertion about the world to distinguish between the rival descriptions generated through this process of metaphysically- or interpretatively-motivated meiotic division.[21] In endorsing T she is asserting something more general. Even if the anti-realist succeeds in showing rampant underdetermination at this more fine-grained descriptive level, then, it's not an underdetermination applying to what the realist asserts about the world.

But an issue lingers. If the realist about T isn't committed to the (approximate) truth of any one of these rivals, any of which could be fairly represented by T's formalism, then what exactly is she endorsing when she endorses T? A natural possibility is structural realism: endorsing T amounts to asserting that the approximate *structure* of the physical world is represented by T's mathematical formalism. However, this form of realism is a good deal weaker than realists like Boyd and Psillos think is warranted, and it would be surprising if these sorts of metaphysical considerations were sufficient to undermine it.[22] We should thus look for a stronger form of realism, but not one that it leaves itself open to this sort of underdetermination threat.

It's helpful to consider how a similar underdetermination strategy might be treated in the context of a non-foundational science like biology. A realist about cell biology would surely scratch her head upon being told that her preferred theory of cellular structure was underdetermined by the existence of rival accounts of sub-atomic phenomena (such as Bohmian mechanics and some interpretation affiliated with the

---

explanation is needed.

[21]This is not to say that she doesn't view the rivals as offering different physical accounts. Indeed, she may even have preferred views on the relevant metaphysical questions. But the view about the world that she incurs in virtue of her realism alone – i.e., in virtue of endorsing a theory – is something that encompasses all the alleged rivals.

[22]Although something like this may perhaps be occurring in Lewis' "Ramseyan Humility". (Lewis, forthcoming)

standard formalism of quantum theory). The anti-realist would perhaps try to convince her that, because cells are ultimately constituted from sub-atomic particles and our theories of sub-atomic particles are (arguably) underdetermined, that quantum-level underdetermination 'percolates up' to all theories about phenomena ultimately constituted from sub-atomic matter. Underdetermination is thus rampant! There is one theory of cell structure infused with Bohmian mechanics, and one theory with (say) a theory of spontaneous quantum wave-function collapse. Moreover, a similar argument concerning meaning and ontology, and hence concerning the genuine distinctness of these theories, could also be made. The theory associated with Bohmian mechanics takes the ontology of cells to be quite different than the theory associated with wave-function collapse, for the former takes cellular processes to be governed, in part, by an extremely complicated guidance equation and classically-behaved sub-atomic particles, whereas the latter involves no such equation and classically-behaved particles. The physical system of the cell is thus different according to the two theories, and so statements about cells might be thought to carry different physical content.

But clearly this is crazy. Something has gone wrong here. Would our imagined anti-realist deny that the Bohmian and collapse theorists both mean the same thing in their discourse about ordinary objects like tables and chairs? Similarly, the realist about cell biology is right to object that quantum theory is irrelevant to the epistemic status of cell biology. The theory she endorses aims to characterize (part of) the physical world at a certain level of description – a level at which certain descriptive notions (such as 'molecule') are taken to be primitive. Of course she knows they are not primitive – just as we know tables and chairs are not primitive – but for the purposes of her realism she treats them that way, because that's precisely how the theory itself treats them. To argue that underdetermination is rampant at the cellular

level because underdetermination occurs at the sub-molecular level misses the point of what the realist about cell biology is claiming. She's endorsing particular claims about cells and cellular structure and prominent molecular features of cells, but is entirely non-committal regarding lower level physical questions about the constitution of those entities and properties she takes, for the purposes of her theory, to be primitive.

The same thing holds, I think, for the metaphysical rivals with which we started this section. What the alleged rivals have in common is more than just the structure represented in their formalisms. At a certain level of description – the level set by foundational physics – they make the same physical claims about the entities and properties asserted to populate the world. At this level certain sorts of seemingly related claims about the (metaphysical) nature of the world also clearly play no role. What a theory of foundational physics offers, then, is an account of part of the physical world couched in a discourse in which certain notions – certain general ontological categories – are taken as primitive. When the realist endorses such a theory, she's endorsing the description it offers *at the level of description for which its intended*. That there are more fine-grained descriptions of the world, all of which might plausibly be represented by that single physical theory, are generally beside the point.[23] For they do not show that the norms and methodologies of science license different and competing theories.

A consequence of this is that the realist's endorsement of a theory, whatever that means exactly, is (or ought to be) relative to the descriptive level for which the theory is intended. Embedded within the realist's endorsement of a particular theory T is a tolerance for many (an infinite number, actually) incompatible accounts of the physical world, all represented by T and all characterized at a more fine-grained level of description than T itself. Unlike the previous case of numerical variants, though, the more fine-grained competitors subsumed under T may have radically different

---

[23]As discussed below, there can be exceptions to this.

ontologies. However we understand the notion of endorsement, then, we know it must leave room for some ontological vagueness. This doesn't make realism about T contentless, though, just as it didn't make realism about the theory of cell structure contentless; there are still plenty of ways the realist's preferred theory could get the world wrong.

4.4.3 Ontologically Derivative Variants

Recall from chapter 1 that a distinguishing feature of foundational physics is that it purports to provide an account of the ontologically primitive features of the physical world, and that it's in virtue of these ambitions that foundational theories are faced with the problem of theory interpretation. It is widely recognized, I think, that there are some theory-specific interpretative problems grounding isolated instances of underdetermination, but could the general problem of ontological interpretation be used to ground an argument for rampant underdetermination in foundational physics?

It will help to start with a specific example. Consider classical electromagnetism, a theory whose central equations I take to be Maxwell's equations, the Lorentz force law, and whatever features of classical dynamics are needed for all of this to make sense. Electromagnetism is no longer taken to be one of our best foundational theories, but it provides a nice illustration of the sort of anti-realist underdetermination strategy I have in mind. As it is typically presented and taught, electromagnetism is primarily about four things: electric fields; magnetic fields; charges; and the forces mediating between fields and charges. The central equations then tell us how these things interact and change.

Treated as a foundational theory, these features are (or have been) often thought to be the primitive ontological posits of classical electromagnetism. In introducing the electric field **E**, for example, Abraham (1951) writes:

> Maxwell's theory then goes on to ascribe to this vector **E** a self-existent reality

independent of the presence of a testing body. Although no observable force appears unless at least two charged bodies are present..., nevertheless we assert with Maxwell that the charged piece of metal by itself produces in the surrounding space a change of physical conditions which the field of the vector $\mathbf{E}$ is exactly fitted to describe. The Primary cause of the action on a testing body is considered to be just this vector field *at the place where the testing body is situated.* As for the piece of metal, its part of the matter is merely to maintain this field. We speak accordingly of a theory of *field action*, as contrasted with the theory of *action at a distance.* (Abraham, 1951, p.55)

More precisely, the vector fields given by $\mathbf{E}$ and $\mathbf{B}$ (the $\mathbf{E}$- and $\mathbf{B}$-fields, for short) are taken to represent the essential properties of ontologically primitive fields (magnitudes and directions, at locations); q the ontologically primitive property of charge; and $\mathbf{F_{em}}$ the essential properties (magnitude and direction) of a constitutive electromagnetic force.

This particular ontological interpretation of classical electromagnetism doesn't command universal acceptance, though. As O'Railly (1965) characterizes the $\mathbf{E}$-field,

[t]he assertion [of the field's physical existence], taken by itself apart from the quantitative force-law, is scientifically otiose. It is merely the physically irrelevant statement of a metaphysical conviction.... This is certainly not a legitimate physical theory at all; it is the confusion of metaphysical belief with metrical physics.... The 'field' may act as a metaphysical background, but it certainly does not act as a scientifically verifiable physical intermediary.... The cause [of the electric force on a body] may be all kinds of things, some of them rather queer; but we do not need to consider how it is brought about; in fact we have not got the faintest notion. The important point is that another charge if placed at that point would be acted upon by a force. It is not merely the important point; so far as physics is concerned, it is the only point. (O'Railly, 1965, pp.653–654)

Clearly, O'Railly takes the $\mathbf{E}$-field to be a derived quantity, a derivative quantity. A student using O'Railly's textbook could be forgiven for thinking there was a scientific consensus concerning the ontological interpretation of classical electromagnetism that was precisely the opposite of Abraham's.

We have here, then, differing ontological interpretations of electromagnetism. Which is correct or most appropriate? It's doubtful there was ever any real consensus in the scientific community about this question. Although it's not certain

that decisive considerations won't be found hidden within electromagnetism itself[24], that looks unlikely. Scientific methodology alone doesn't seem to have answered the question.

It's easy to see why the anti-realist might seek to capitalize on this interpretative indecision as an instance of underdetermination, and as part of a broader strategy for establishing rampant underdetermination. We have here different ontological interpretations, painting different and incompatible pictures of the physical world, and yet the norms and methodologies of science seem at a loss to specify one account – one interpretation – as preferred.

Moreover, the anti-realist's strategy would seem to generalize, buttressing the anti-realist's contention of rampant underdetermination in foundational physics. These sorts of questions of ontological interpretation should be expected to crop up for any foundational theory – which is just another way of stating the problem of theory interpretation, as outlined in Chapter One – and it's hard to see how the norms and methodologies of science would, at least in general, help decide between them. Yet the different accounts also seem to count as different foundational theories.

Realists are generally dismissive of such alleged underdetermination in this instance, too, but here the explanation is less straight-forward (and thus more illuminating). Unlike the other cases of alleged underdetermination considered above, the current strategy appeals to a distinction (the distinction between ontologically primitive and derivative features of the world) that is arguably implicit in foundational physics itself, and which distinguishes it from other branches of science. Appeals to 'levels of description' and protests to the effect that endorsing a foundational theory T doesn't involve substantive claims about what's ontologically primitive thus seem contrived and disingenuous. If the realist about T really endorses T *as a foundational*

---

[24]Lange (2002) uses energy-based considerations to argue for the "reality" (i.e., the ontological primitiveness) of the **E**-field – or rather the reality of a primitive electromagnetic field.

theory (whatever that endorsement amounts to) shouldn't that involve substantive claims about what's ontologically primitive? But then underdetermination of the above sort would seem to threaten.

The account I want to develop below of the content of realism – of what it means for the realist to endorse a particular (foundational) theory – aims to make sense of this indifference. Such a goal might be thought to be completely misguided from the start. Although to interpret the **E**-field as derivative is not to say that there's no sense in which the **E**-field *exists*, when most realists assert the **E**-field's existence they mean to assert its ontological primitiveness – so as to distinguish it, for example, from the ontologically derivative *potentials*. This commitment to an ontologically primitive **E**-field, what is more, is often claimed to arise precisely in virtue of her realism about electromagnetism, and so we might reasonably expect this particular ontological interpretation to play an important role in what she means when she *endorses* electromagnetism.[25] How can we make sense of this in a way that permits the realist about electromagnetism to have something less than firm convictions about what features of electromagnetism are ontologically primitive? Am I not being led, in a desperate attempt to evade the threat of underdetermination, into providing an account of realism about foundational physics that seeks to legitimize an indifference of sorts to questions about ontological primitiveness?

The beginnings of an answer rest on how we think about the origins of the realist's beliefs about the fundamental physical world. The theory of electromagnetism contributes in two different ways (I suggest) to the particular beliefs our realist above has about the fundamental world. There is, in the first instance, the contribution in virtue of her realism about electromagnetism – in virtue of her endorsement of that theory (whatever that amounts to). And then there is the contribution in virtue of

---

[25]More accurately, in virtue of their realism about our best theory of electromagnetic phenomena, which surely isn't classical electromagnetism. But fidelity to that fact does more harm than good in this context.

the particular interpretative beliefs she has about electromagnetism. These interpretative beliefs are ontologically inert – they involve no commitment to the world being any particular way at all at the fundamental level – in the absence of an accompanying (realist) endorsement of electromagnetism. In this sense we can understand why some realists might say things like "I believe in the electric field because I'm a realist about electromagnetism". But such statements are also a bit misleading, as the relevant interpretative beliefs do not follow, strictly speaking, from their realist endorsement of electromagnetism.

That it's a mistake to think interpretative commitments form part of the content of scientific realism itself is suggested by two natural judgments about electromagnetism. First, an agent who takes charges and electromagnetic forces to be ontologically primitive (in accordance with the equations of electromagnetism) is, by almost any intuitive standard, a realist about electromagnetism. So is an agent who takes fields as primitive. It follows that neither of these specific interpretative views are partially constitutive of realism about electromagnetism. Second, if new empirical considerations were to come to light suggesting that the $\mathbf{E}$-field was ontologically derivative, it's unlikely our realist would say this discovery had undermined her realism about electromagnetism itself. When the discovery of the Aharanov-Bohm effect was widely touted in the scientific community – correctly or not – as establishing that the vector potential was ontologically primitive, no realist took their faith in realism to have been thereby undermined. Again this suggests that particular interpretative beliefs about what's ontologically primitive don't form part of what it means for the realist to endorse a theory.

An appreciation of this distinction suggests a way of understanding why the above underdetermination strategy could fail to move the realist, and thus it suggests a way of teasing out what it means for the realist to endorse a foundational theory. For the

anti-realist's underdetermination strategy turns on showing that there are (many) instances in which interpretative beliefs about a theory T that are at odds with the realist's fare equally well respect to the norms and methodologies of science, not that there are many theories different than T warranting the realist's endorsement equally well. So while the anti-realist may have demonstrated that a certain sort of underdetermination is widespread, that demonstration may only illustrate something the realist already knows – namely, that it remains a puzzling issue how to go about interpreting foundational theories. It won't have demonstrated that other, incompatible theories are equally deserving of the realist's endorsement.

The extent to which this reply vitiates the force and interest of realism depends entirely upon what it means for the realist to endorse a theory T. If it only amounts to the claim that there is an approximately true ontological interpretation of T – if it fails, that is, to impose any constraints on what that interpretation is like – then the resulting realism about T may well be too weak to be of much use and interest to anyone. Can a middle-ground can be found between ontological abdication and ontological totalitarianism?

Here it's helpful to consider a case in which differing ontological interpretations *do* seem to have given rise, even by the realist's own lights, to genuinely underdetermined rivals — space. The anti-realist's strategy in this section would founder if he tried to show that Newtonian particle mechanics was underdetermined by suggesting a rival interpretation – a rival theory, in his mind – according to which space was taken to be ontologically derivative (as Descartes and Leibniz thought) rather than primitive (as Newton thought). Here the formal framework of classical dynamics, which is what the ontological interpretations are intended to be interpretations of, is not indifferent to whether space is ontologically primitive or derivative. Certain concepts and laws playing a central role in that framework – e.g., rectilinear motion at constant

speed (as figuring in the Law of Inertia) and rotational acceleration – would seem to make no sense, at least on the surface, if space is understood derivatively. As a result, the relationalist strategy has been to construct (or try to construct) an alternative formal framework for capturing the dynamics that doesn't implicitly require all coherent ontological interpretations to posit space as an ontological primitive. The competing framework constructed *is* taken by the realist to constitute a case of theory underdetermination.[26]

How exactly does the case of space differ from that of the **E**-field, the former being a genuine case of underdetermination but not the latter? There doesn't seem to be any reason to think space is taken more seriously by the realist than the electric field; indeed, one might think, *prima facie*, that the opposite was true. The difference clearly has to do with the formalism and theoretical principles that characterize the theory. In the case of the **E**-field, the realist doesn't consider competing ontological interpretations to generate an underdetermination problem because both interpretations are compatible with the *same* formalism and set of theoretical principles. Had the same been true of space – had different ontological interpretations all been compatible with the formalism and theoretical principles of Newtonian particle dynamics – then in all likelihood space would have been viewed the same way by the realist. What this tells us is that formalism and formal expressions of theoretical principles play an important role in the realist's judgments as to which sorts of cases of alleged underdetermination are genuine, and hence in what it means for the realist to endorse a particular foundational theory. There is an important sense in which a theory's formalism is the primary 'object' to which the realist's endorsement attaches. To *endorse* a theory T is to say something about how T's formalism relates to the world.

A natural suggestion is to say that endorsing a foundational theory T simply

---

[26]See, in particular, Pooley and Brown (2002).

amounts to saying that there's an approximately true ontological interpretation of T's formalism. We know from the discussion of numerical variants that the realist's position is, in the first instance, and existential claim. And we know from the discussion of purely metaphysical rivals that it doesn't pin down a physical ontology completely, even in the case of realism about foundational physics. Although I think something along these lines is right, this preliminary characterization is too simple. Consider a possible ontological interpretation of electromagnetism according to which fields, charges, and forces are all taken to be ontologically derivative. Without supplementing the formalism with other mathematical elements featuring in its central theoretical principles, such an interpretation would be incompatible with the realist's treatment of electromagnetism *as a foundational theory*. $\mathbf{E}$, $\mathbf{B}$, q and $\mathbf{F_{em}}$ are all mathematically interrelated quantities. To treat one as derivative – that is, defined – thus requires that others be taken as ontologically primitive, at least if one is going to maintain that electromagnetism provides an account of certain phenomena in terms of ontologically primitive features of the world. A related issue arose in the case of space above, although there the interpretative incompatibility with the formalism and theoretical principles arose in a different way. An ontological interpretation taking space as derivative isn't compatible with the formalism and theoretical principles of Newtonian particle mechanics, at least as standardly formulated.[27]

There is a general lesson here: not all superficially adequate ontological interpretations of a given foundational theory T really are *compatible* with T's formalism and theoretical principles. Just because one can write down an ontological interpretation of T doesn't mean that interpretation is compatible with T's theoretical principles

---

[27]The same lesson applies to interpretations of Newtonian particle dynamics concerned with the ontological status of *force*. It *might* be possible to interpret Newtonian dynamics in such a way that force is taking to be a derivative (defined) notion, but it doesn't seem as though one could both interpret *force* and *mass* this way *and* also see the theory was foundational. For there would then be no way of understanding the meaning of Newton's second law such that it made a substantive assertion about any ontologically primitive feature of the world.

and formalism. For an interpretation to be compatible, it must make sense of T's formalism and theoretical principles in such a way that T's account of the physical phenomena can be understood just in terms of ontological primitives. (For they are, after all, the ultimate *constituents* of the world.) Clearly, different proposed interpretations may run afoul of condition in different ways – e.g., because an interpretation renders a central notion or principle incoherent, or because an interpretation is incompatible with the assumption that T offers a self-contained description of physical phenomena in terms of ontological primitives.

My suggestion, then, is that we understand a realist's endorsement of a foundational theory as follows: to endorse a foundational theory T is assert that there is an approximately true ontological interpretation of T's formalism that is compatible with T's theoretical principles and the assumption that T is foundational. Such an account of the content of realism is, clearly, much weaker than often assumed, but it's not so weak as to be vacuous or to reduce to structural realism. It's not correct to say that the realist's endorsement of a foundational theory is empty if it doesn't contain a commitment to some specific ontological interpretation or other, for in endorsing a particular theory T as foundational, she's committing herself to the claim that the world is one of a handful of ways, as given by the set of compatible ontological interpretations. As we saw, for example, for the realist to *endorse* Newtonian particle mechanics is for her to rule out accounts of the world's primitive ontologies that don't include space as primitive. Surely that's a rather robust ontological commitment.

So this account is flexible enough to allow that the realist may be genuinely undecided as to whether to interpret a particular feature as primitive or derivative, but not so flexible that anything goes. This seems right. We see this in the space case, but also in electromagnetism. While it may not be necessary to believe in a primitive **E**-field to be a realist about electromagnetism, surely one must believe that an

appropriate (i.e., compatible) combination of fields, charges, and forces are primitive in order to be a realist about electromagnetism. Taking them all as derivative surely wouldn't count.

CHAPTER V

Envoi

The preceding essays have hopefully made clear that the relationship between ontological interpretation in foundational physics and scientific realism is complicated and diverse. More work is clearly required, on both the topics considered in this dissertation and other dimensions of the interpretation–realism relationship. By way of conclusion, I'll briefly outline some of the areas where I think additional work is needed.

My discussion of methodology in chapter 2 sought to show that questions of ontological interpretation in foundational physics limit the sorts of realist epistemic conclusions one can justifiably draw on the basis of methodological success. This argument was developed in the context of a particular historical example – Maxwell's use of the Lagrangian mathematical formalism in his development of electromagnetism – for the purposes of assessing the naturalistic no-miracles defense advocated by Richard Boyd.

Many relevant issues remain to be addressed, though. Although I have argued that some historical cases do not exhibit the theory dependence required for Boyd's explanationist argument, other historical scenarios clearly do. In the latter cases we may find Boyd's realist conclusions quite plausible. But the two situations need not be independent: a theory developed with one sort of methodology may latter be

invoked as a background commitment in the service of applying a new, more theory-dependent methodology – say, for the purpose of new theoretical construction. If I am right about classical electromagnetism, for example, this sort of situation would seem to arise for quantum electrodynamics. Given this historical interplay between different types of methodologies, each with varying degrees of background theory dependence, how should we sort out the epistemic value of methodological success in light of interpretative problems that sometimes, but not always, rear their heads?

Moreover, although I have argued that certain prominent theoretical commitments do not always play the sorts of background roles in guiding methodological judgments that Boyd asserts, I have not shown – nor do I believe – that methodology can be entirely theory neutral. Is it thus not possible that a variation on Boyd's argument could be used to ground a significantly vitiated form of scientific realism? This depends, I suppose, on the exact nature of those background commitments and how widespread they are within actual methodological practices – both questions worth examining further.

Beyond the specific context of Boyd's argument, larger questions loom. As a descriptive matter, how can a finer sensitivity to problems of interpretation lead to a more nuanced and accurate picture of those methodologies central to foundational physics? As a normative matter, how might this bear on the prospects for a naturalistic defense of scientific realism? Whether a more general characterization can be given of how problems of ontological interpretation bear on scientific methodology and scientific realism remains to be seen.

In chapter 3 I looked at the concept of theory reformulation. Using classical dynamics as a case study, I proposed an analysis of reformulation claims in terms of counterfactual interpretative judgments, which was aimed at how we could make sense of such claims in the context of false theories. Then, using that analysis, I sought

to cast doubt on the claim that Lagrangian dynamics is a reformulation of Newtonian dynamics by suggesting a non-Newtonian interpretation of Lagrangian dynamics. Both the analysis of theory reformulation and the non-Newtonian interpretation of Lagrangian dynamics are outlined rather quickly, and clearly each could use more development. How, for example, might the non-Newtonian interpretation, and the mathematical structure associated with it, be extended to Hamiltonian dynamics?

In addition, an important way the central ideas in this chapter could be extended is by considering the implications my analysis of theory reformulation has on our assessment of the underdetermination argument. For reformulation claims bear directly on the alleged status of theoretical rivals; if the former are a matter of interpretative judgments, so are the latter. How does this affect whether underdetermination is taken to be a significant threat to scientific realism?

Chapter 4 is perhaps the most open-ended, aiming as it does to simultaneously re-formulate one of the most influential arguments against realism *and* suggest a new form of realism. My discussion is organized around the issue of whether competing ontological interpretations raise underdetermination problems for realism about foundational physics. I first argue that the underdetermination issue can be re-formulated so as to bring out the underlying threat to realism in a perspicuous way. This re-formulation turns on a general deference to the norms and methodologies internal to scientific practice. But can such norms and methodologies be made explicit enough to warrant an evaluation of the underdetermination argument?

By considering intuitively unproblematic cases of interpretative underdetermination, I then argue that the 'standard' realist position must be weakened to as to avoid rampant underdetermination generated by competing ontological interpretations. The resulting realist position I suggest – which lies somewhere between structural realism and standard realism – needs to be sketched out much more. In particu-

lar, can it effectively accommodate the presence of different mathematical frameworks that are widely judged to be variant formalisms of a single underlying theory? And, of course, chapter 4 is only intended to lay the groundwork for a broader investigation: What are we to now make of the underdetermination threat?

APPENDICES

APPENDIX A

The Lagrangian Formalism

This appendix provides a brief summary of the Lagrangian formalism for classical dynamics. My discussion is restricted to finite-dimensional point-particle systems. To help make salient some of the novelties of this approach, I begin with Newtonian dynamics.

Motion on the Newtonian approach[1] is represented in 3-dimensional Euclidean space $\mathbb{E}^3$, taken to be an arbitrary inertial frame.[2] A system consisting of N particles (an "N-particle system") is characterized by N distinct curves through $\mathbb{E}^3$, corresponding to spatial trajectories for each particle. The dynamical problem is solved once those trajectories are determined as explicit functions of time; that is, once the functions $\mathbf{r}^i(t) = (x^i(t), y^i(t), z^i(t))$ for all $i \in N$ have been determined, where $\mathbf{r}^i(t)$ is a vector-valued function on $\mathbb{E}^3$ representing the ith particle's trajectory.[3] The real-valued parameter t represents time, as it does in analytical dynamics.

---

[1]My discussion is restricted to discrete systems, such as point particles, as that's the context in which claims of theoretical equivalence are often made. See Wilson (2009) for an interesting take on the relationships between classical formalisms from a rather different angle than the one developed here.

[2]There are significant questions about how exactly inertial frames are to be defined or characterized. I'll have little to say about these issues here. For a recent take on this issue, see Brown (2005).

[3]Additional complexities are involved if such curves are allowed to intersect, i.e., if particle collisions are permitted. We will not be dealing with such complexities here.

It is often convenient to represent the N trajectories as a single $3N$-tuple $(x^1(t), \ldots, x^{3N}(t))$, where

$$\mathbf{x}_i(t) = \sum_{j=1}^{3} x^{3(i-1)+j}(t) \; \hat{\mathbf{x}}^j$$

for a given set of orthogonal unit vectors $\{\hat{\mathbf{x}}^1, \hat{\mathbf{x}}^2, \hat{\mathbf{x}}^3\}$ spanning $\mathbb{E}^3$. Intuitively, $\mathbf{x}_1(t) = (x^1(t), x^2(t), x^3(t))$, $\mathbf{x}_2(t) = (x^4(t), x^5(t), x^6(t))$, etc.

Each particle (as represented in an inertial frame) is governed by the single dynamical equation:

$$\mathbf{F}_i = m_i \mathbf{a}^i \quad \text{(``Newton's second law'')}$$

(where $\mathbf{F}_i$ is the net force exerted on i, m_i its mass, and $\mathbf{a}_i$ its acceleration) along with the additional principle that the force i exerts on j be equal in magnitude and opposite in direction to the force j exerts on i:

$$\mathbf{F}_{ij} = \mathbf{F}_{ji}.^4$$

The specific forces whose vector sum constitutes the net force $\mathbf{F}_i$ are determined by *constitutive force equations*, such as the equation for the gravitational force. A central task facing classical dynamics for most of the 19th century was the determination of exactly what those constitutive force equations were. In principle, no constraints are placed on the functional form such force equations can take.

Given a set of N masses and the force laws governing their interactions, Newton's law determines a system of $3N$ differential equations, one for each (spatial) degree of freedom per particle. In conjunction with the initial positions and velocities of each particle, these equations uniquely determine the resulting spatial trajectories and the

---

[4]There is some question as to whether additional, logically-independent principles are needed in accounting for rigid-body dynamics, such as principles concerning the moments of forces (torques). That further question will not be relevant here. See Truesdell (1968) for discussion.

dynamical problem is solved.[5]

Newton's second law requires each solution to be a spatial trajectory, or at least a vector component of a spatial trajectory, but in principle any collection of $3N$ independent variables given as functions of time, $(q^1(t), \ldots, q^{3N}(t))$, could be used to describe the system's motion. The Lagrangian approach exploits this fact by representing the motion of an N-particle system as a curve through a higher-dimensional *configuration manifold M*. Points of M are specified via *generalized coordinates* $(q^1, \ldots, q^n)$, related to the ordinary spatial parameters (x^i, y^i, z^i) through the coordinate transformations

$$q^\alpha = q^\alpha(x^1, y^1, z^1, \ldots, x^N, y^N, z^N, t), \qquad \alpha = 1, \ldots, n.$$

Here n is the number of degrees of freedom of the system, which may be less than $3N$ if the system as a whole is subject to m constraints of the form

$$f_j(x^1, y^1, z^1, \ldots, x^N, y^N, z^N, t) = 0, \qquad j = 1, \ldots, m.^6$$

It can be helpful (but not necessary) to think of M as a hyper-surface embedded in the space $\mathbb{E}^{3N}$, parameterized by the coordinates $(x^1, \ldots, x^{3N}) = (x^1, y^1, z^1, \ldots, x^N, y^N, z^N)$. Unlike the x^i, the q^α need not be spatial in any literal sense, although given an arbitrary point $p \in M$ the spatial positions of all N particles can be recovered.[7] A curve $q(t) = (q^1(t), \ldots, q^n(t))$ in M, representing the dynamical evolution of an N-particle

---

[5]But see Norton (2008), Norton (2003), and Malament (2008) for a recent discussion of Newtonian systems for which this result appears to fail – systems for which specifying the initial conditions fails to determine uniquely the resulting trajectories.

[6]Constraints of this form, which can always be expressed as finite relations between coordinates, are *holonomic* and always reduce the system's degrees of freedom. Many constraints, though, can only be expressed as differential relations between the coordinates. Such *non-holonomic* constraints do not reduce a system's degrees of freedom. See Lanczos (1986, pp.24–27, 48–49, 146–147). For ease of exposition my discussion in this section is restricted to holonomic systems.

[7]Here we require that the Jacobian of the coordinate transformation be non-singular and thus that the transformation be invertible. M is identified in $\mathbb{E}^{3N}$ by specifying $3N$ generalized coordinates $(q^1, \ldots, q^{3N})$ such that M corresponds to the hyper-surface $q^{n+1} = 0, \ldots, q^{3N} = 0$. See José and Saletan (1998, p.56).

system with n degrees of freedom, can also be considered as a curve in $\mathbb{E}^{3N}$, albeit one restricted to lie in the hyper-surface M.

By representing the instantaneous configuration of all N particles as a single 'system point' in M, we effectively treat a system whose motion is constrained as an unconstrained system moving freely in an n-dimensional manifold. This is often a significant practical advantage. Constraint forces are generally quite complicated and difficult to determine, but their *effects* may be obvious – e.g., that an object is confined to a surface or grooved track. Representing a system as a point in a configuration manifold allows us to compensate for this ignorance kinematically. If we know the ball stays on a warped surface, for example, we need not explicitly treat the forces keeping it there, as the Newtonian approach requires; its motion can be represented as a curve in a configuration manifold whose coordinates are adapted to the warped surface itself.

$\mathbb{E}^{3N}$ possesses a natural metric structure derived from the metric structure of physical space, and this induces a corresponding metric on M:

$$d\bar{s}^2 = \sum_{i,k=1}^{3N} g_{ik}dx^i dx^k = \sum_{\alpha,\beta=1}^{n} a_{\alpha\beta}dq^\alpha dq^\beta,$$

where the metric tensors g_{ik} and $a_{\alpha\beta}$ are functions of the x^i and q^α, respectively. Unlike g_{ik}, however, $a_{\alpha\beta}$ is generally non-Euclidean.

The use of generalized coordinates $q^1, \ldots, q^n$ allows "generalized functions" to be defined on M, which are needed to complete the Lagrangian picture. If the net force $\mathbf{F}_i$ on particle i is given, *in our Newtonian description*, as $\mathbf{F}_i = F_i^1 \hat{\mathbf{x}}^1 + F_i^2 \hat{\mathbf{x}}^2 + F_i^3 \hat{\mathbf{x}}^3$, the *work* done by all forces in an infinitesimal displacement of the system (where

work = force × distance) is given by

$$\bar{dw} = \sum_{i=1}^{N} \sum_{j=1}^{3} F_i^j dx_i^j.$$

Here dx_i^j is the infinitesimal displacement of the ith particle in the jth spatial direction. The coordinate transformations between the spatial coordinates x^i and the generalized coordinates q^α (where $\alpha = 1, \ldots, n$)[8] then allow us to write $\bar{dw}$ in terms of the dq^α,

$$\bar{dw} = \sum_{\alpha=1}^{n} F^\alpha dq^\alpha.$$

The F^α together constitute the components of an n-dimensional vector on M called the *generalized force*, which we can think of as acting on the 'point particle' in M that represents the state of our N-particle system at a given instant.[9] In a similar way the kinetic energy of each particle, expressed in Newtonian terms as $T_i = \frac{1}{2} m_i \dot{\mathbf{x}}_i^2$, allows us to construct the *generalized kinetic energy* function $T(\dot{q}^1, \ldots, \dot{q}^n)$ on M.[10] The function $T(\dot{q}^\alpha)$ represents the total kinetic energy of the system, but expressed as a function on the n-dimensional abstract configuration space M.

Let's assume that the forces acting on our system – the Newtonian forces in ordinary $\mathbb{E}^3$ – are *conservative*, by which I mean that $\bar{dw}$ may be written as the true differential of a work function U such that $\bar{dw} = dU$, where

$$U = U(q^1, \ldots, q^n).$$

Setting $V = -U$, it follows that $F^\alpha = -\frac{\partial V}{\partial q^\alpha}$ and V can be interpreted as the system's *potential energy*.

---

[8]By convention, i runs $1, \ldots, 3N$ and α runs $1, \ldots, n$.
[9]Strictly speaking, the generalized force vector at a point $p \in M$ exists in the tangent space $\mathbb{T}_p M$ of that point, but this detail need not concern us.
[10]Here I adopt the standard convention that $\dot{q}^\alpha = \frac{d}{dt} q^\alpha$.

The *Lagrangian* $L(q^\alpha, \dot{q}^\alpha)$ of an N-particle system with n degrees of freedom is a scalar function defined simply as the difference between its generalized kinetic energy and potential energy:

$$L(q^1, \ldots, q^n, \dot{q}^1, \ldots, \dot{q}^n) = T(\dot{q}^1, \ldots, \dot{q}^n) - V(q^1, \ldots, q^n).$$

Given the Lagrangian of a system and its initial conditions $(q_0^1, \ldots, q_0^n, \dot{q}_0^1, \ldots, \dot{q}_0^n)$ at time t_0, there is a unique trajectory $q(t) = (q^1(t), \ldots, q^n(t))$ satisfying the (homogeneous) *Euler-Lagrange equations*:

$$\frac{d}{dt}\frac{\partial L}{\partial \dot{q}^\alpha} - \frac{\partial L}{\partial q^\alpha} = 0, \quad \alpha = 1, \ldots, n.$$

These equations provide a scheme for determining the motion of any N-particle system. Using L, they specify a system of n differential equations of motion – one for each degree of freedom – and the resulting solution is a curve $q(t) = (q^1(t), \ldots, q^n(t))$ on M. When re-expressed in terms of the x^i using the coordinate transformations noted above, we recover the individual 3-dimensional spatial trajectories of all N particles.

APPENDIX B

Fractional Calculus

Notation[1] On analogy with the notation for the ordinary n^{th}-fold derivative, $\frac{d^n f}{dx^n}$,

we define

$$\frac{d^{-1}f}{[dx]^{-1}} \equiv \int_0^x f(y)dy,$$

and

$$\frac{d^{-n}f}{[dx]^{-n}} \equiv \int_0^x dx_{n-1} \int_0^{x_{n-1}} dx_{n-2} \ldots \int_0^{x_2} dx_1 \int_0^{x_1} f(x_0)dx_0.$$

Invoking the identity $\int_a^x f(y)dy = \int_0^{x-a} f(y+a)dy$, the integral notation is extended

to non-zero lower limits as follows:

$$\frac{d^{-1}f}{[d(x-a)]^{-1}} \equiv \int_a^x f(y)dy$$

and

$$\frac{d^{-n}f}{[d(x-a)]^{-n}} \equiv \int_a^x dx_{n-1} \int_a^{x_{n-1}} dx_{n-2} \ldots \int_a^{x_2} dx_1 \int_a^{x_1} f(x_0)dx_0.$$

---

[1]The presentation in this appendix is adapted from Oldham and Spanier (2002).

In developing a unified notation for expressing derivatives and integrals, it's helpful to bear in mind that some identities only hold for the positive n case (i.e., differentiation) or the negative n case (i.e., integration), but not both. For example, $\frac{d^n}{[d(x-a)]^n} = \frac{d^n}{dx^n}$ only holds for non-negative n.

Differentiation   One standard definition of the first derivative is

$$\frac{d^1 f}{dx^1} \equiv \frac{d}{dx} f(x) \equiv \lim_{\delta x \to 0} \{ [\delta x]^{-1} [f(x) - f(x - \delta x)] \}.$$

By successive applications of this definition, we arrive at a general definition of the nth (integer) order derivative:

$$\frac{d^n f}{dx^n} \equiv \lim_{\delta x \to 0} \left\{ [\delta x]^{-n} \sum_{j=0}^{n} [-]^j \binom{n}{j} f(x - j\delta x) \right\},$$

where

$$\binom{n}{j} \equiv \frac{n!}{j!(n-j)!}.$$

In anticipation of constructing a unified notation for derivatives and integrals, we define a new expression, $\frac{d^n f}{[dx]^n}$, which consists of the above limit taken as $\delta x \to 0$ through discrete values only. More precisely, define $\delta_N x \equiv \frac{[x-a]}{N}$, $N = 1, 2, 3, \ldots$ and where a is an arbitrary number less than x. We then define

$$\frac{d^n f}{[dx]^n} \equiv \lim_{\delta_N x \to 0} \left\{ [\delta_N x]^{-n} \sum_{j=0}^{n} [-]^j \binom{n}{j} f(x - j\delta_N x) \right\}.$$

If $\frac{d^n f}{dx^n}$ exists, then so does $\frac{d^n f}{[dx]^n}$ and $\frac{d^n f}{dx^n} = \frac{d^n f}{[dx]^n}$. Our new expression for the nth (integer) order derivative can be re-expressed as:

$$\frac{d^n f}{[dx]^n} = \lim_{N \to \infty} \left\{ \left[\frac{x-a}{N} \right]^{-n} \sum_{j=0}^{N-1} [-]^j \binom{n}{j} f\left(x - j \left[\frac{x-a}{N} \right] \right) \right\},$$

109

where use has been made of the fact that, for integer n, $\binom{n}{j} = 0$ if $j > n$.

Integration A similar process is invoked for integration. We start with the standard definition of an integral in terms of a Riemann sum:

$$\frac{d^{-1}f}{[d(x-a)]^{-1}} \equiv \int_a^x f(y)dy \equiv \lim_{\delta_N x \to 0} \left\{ \delta_N x \sum_{j=0}^{N-1} f\left(x - j\delta_N x\right) \right\}$$

and by repeated application arrive at a general definition of the nth (integer) order integral:

$$\frac{d^{-n}f}{[d(x-a)]^{-n}} \equiv \lim_{N \to \infty} \left\{ \left[\frac{x-a}{N}\right]^n \sum_{j=0}^{N-1} \binom{j+n-1}{j} f\left(x - j\left[\frac{x-a}{N}\right]\right) \right\}.$$

Differintegrals We are now in a position to write a single expression for arbitrary n^{th} (integer) order derivatives *and* integrals. Recognizing that

$$[-]^j \binom{n}{j} = \binom{j-n-1}{j} = \frac{\Gamma(j-n)}{\Gamma(-n)\Gamma(j+1)},$$

where $\Gamma(x)$ is the *gamma function* defined as

$$\Gamma(x) \equiv \lim_{N \to \infty} \left[\frac{N!N^x}{x[x+1][x+2]\ldots[x+N]} \right]$$

and which exhibits the recurrence relationship $\Gamma(x-1) = \frac{\Gamma(x)}{x-1}$, we now define the *differintegral* expression

$$\frac{d^n f}{[d(x-a)]^n} \equiv \lim_{N \to \infty} \left\{ \frac{\left[\frac{x-a}{N}\right]^{-n}}{\Gamma(-n)} \sum_{j=0}^{N-1} \frac{\Gamma(j-n)}{\Gamma(j+1)} f\left(x - j\left[\frac{x-a}{N}\right]\right) \right\},$$

where n is an integer of either sign. This expression provides a unified notation for expressing ordinary derivatives and integrals.

As a result of using a unified notion we must be careful about invoking identities familiar from standard notations. For example, in our new notation the composition rule

$$\frac{d^n}{[d(x-a)]^n}\left\{\frac{d^N f}{[d(x-a)]^N}\right\}=\frac{d^{n+N}f}{[d(x-a)]^{n+N}}$$

always holds unless $N > 0$ and $n < 0$. (That is, it holds otherwise, and there are some circumstances in which it holds for $N > 0$ and $n < 0$.)

Differintegrable Functions The definition of a differintegral can now be extended to differintegrals of arbitrary order. Start by letting any real-valued function $f(y)$ satisfying the following conditions be a *differintegrable function*:

(1) $f(y)$ is defined on the closed interval $a \leq y \leq x$;

(2) $f(y)$ is bounded everywhere in the half-open interval $a < y \leq x$; and

(3) $f(y)$ is more well-behaved at the lower limit a than the function $[y-a]^{-1}$ (i.e., $\lim_{y\to a}\{[y-a]f(y)\}=0$).

The Fundamental Definition The differintegral of arbitrary order q is defined by letting the integer values n in the unified expression above range over arbitrary values:

$$\frac{d^q f}{[d(x-a)]^q}=\lim_{N\to\infty}\left\{\frac{\left[\frac{x-a}{N}\right]^{-q}}{\Gamma(-q)}\sum_{j=0}^{N-1}\frac{\Gamma(j-q)}{\Gamma(j+1)}\,f\left(x-j\left[\frac{x-a}{N}\right]\right)\right\},$$

where f is a differintegrable function and q is arbitrary.[2] When $q < 0$ (alternatively, $q > 0$) the expression is said to define the qth-order fractional integral (alternatively, derivative) of f. (The 'fractional' characterization is thus misleading, and reflects the fact that, historically, the first inquiries into arbitrary order derivatives and integrals

---

[2]My brief survey here treats real q, although the definitions carry over with only minor modifications to complex q.

were restricted to the fractional case.[3]) Note that the fundamental definition only requires an explicit evaluation of f, not of its ordinary derivatives or integrals.

It follows from this definition that the composition identity

$$\frac{d^n}{dx^n}\frac{d^q f}{[d(x-a)]^q} = \frac{d^{n+q}f}{[d(x-a)]^{n+q}} \qquad (*)$$

holds only for arbitrary q and positive integer n, an identity that's helpful in formulating an alternative definitions to the fundamental one given above.

The Riemann-Liouville Definitions When n is a non-negative integer, Cauchy's formula for repeated integration expresses an iterated integral as a weighted single integral, as follows:

$$\frac{d^{-n}f}{[d(x-a)]^{-n}} \equiv \int_a^x dx_{n-1}\int_a^{x_{n-1}} dx_{n-2}\ldots\int_a^{x_1} f(x_0)dx_0 = \frac{1}{(n-1)!}\int_a^x [x-y]^{n-1}f(y)dy.$$

This formula suggests a natural extension from $-n$ integer to arbitrary negative q:

$$\left[\frac{d^q f}{[d(x-a)]^q}\right]_{R-L} = \frac{1}{\Gamma(-q)}\int_a^x [x-y]^{-q-1}f(y)dy = \frac{1}{\Gamma(-q)}\int_a^x \frac{f(y)dy}{[x-y]^{q+1}}, \quad q<0.$$

This is the *Riemann-Liouville* definition of the fractional *integral*.

The integral expression diverges for $q \geq 0$, although a suitable expression for $q \geq 0$ can be constructed via analytic continuation. However, the more common practice is to insist that the Riemann-Liouville integral expression satisfy (*) above. That is, to require that

$$\left[\frac{d^q f}{[d(x-a)]^q}\right]_{R-L} \equiv \frac{d^n}{d^n x}\left[\frac{d^{q-n}f}{[d(x-a)]^{q-n}}\right]_{R-L} = \frac{d^n}{dx^n}\left[\frac{1}{\Gamma(n-q)}\int_a^x \frac{f(y)dy}{[x-y]^{q-n+1}}\right], \quad n>q$$

---
[3]See., e.g., Miller and Ross (1993).

112

where $\frac{d^n}{dx^n}$ represents ordinary n-fold differentiation and n is an integer so large that $q - n < 0$. This expression defines the *Reimann-Liouville fractional derivative*.

It can be proven that the Riemann-Liouville definitions are equivalent to the fundamental definition, although in many applications they are significantly easier to use.

The Laplace Transform   The *Laplace transform* of a function $f(x)$ defined for $x \geq 0$ is given as

$$\mathcal{L}\{f(x)\}(s) \equiv F(s) \equiv \int_0^\infty f(x)e^{-sx}dx, \qquad \text{where } s \in \mathbb{C}.$$

For positive integer m, the Laplace transform of the m-fold derivative is given as:

$$\mathcal{L}\left\{\frac{d^m f}{dx^m}\right\}(s) = s^m \mathcal{L}\{f\} - \sum_{k=0}^{m-1} s^{m-1-k}\, \frac{d^k f}{dx^k}(0), \qquad m = 1, 2, 3, \dots$$

Similarly, for repeated integration:

$$\mathcal{L}\left\{\frac{d^m f}{dx^m}\right\}(s) = s^m \mathcal{L}\{f\}, \qquad m = 0, -1, -2, \dots$$

Unifying this notation, the following expression embraces both of the preceding identities:

$$\mathcal{L}\left\{\frac{d^m f}{dx^m}\right\}(s) = s^m \mathcal{L}\{f\} - \sum_{k=0}^{m-1} s^k\, \frac{d^{m-1-k} f}{dx^{m-1-k}}(0), \qquad m = 0, \pm 1, \pm 2, \dots \qquad (**)$$

For the purposes of extending this notation to arbitrary q, note that the upper limit in the summation (i.e., $m - 1$) can be replaced with any integer $l > (m - 1)$, as the coefficients of such extra terms are uniformly zero for any function whose Laplace transform exists.

We define the Laplace transform of differintegrals of arbitrary order q as:

$$\mathcal{L}\left\{\frac{d^q f}{dx^q}\right\}(s) \equiv \int_0^\infty e^{-sx}\frac{d^q f}{dx^q}dx,$$

where $\frac{d^q f}{dx^q} = \frac{d^q f}{[d(x-0)]^q}$. So defined, it can be proven that the unified expression (**) above generalizes to arbitrary q:

$$\mathcal{L}\left\{\frac{d^q f}{dx^q}\right\} = s^q \mathcal{L}\{f\} - \sum_{k=0}^{n-1} s^k \frac{d^{q-1-k} f}{dx^{q-1-k}}(0), \qquad \text{for all } q,$$

where n is an integer such that $n - 1 < q \leq n$. (The summation vanishes when $q \leq 0$.)

BIBLIOGRAPHY

BIBLIOGRAPHY

ABRAHAM, MAX. 1951. *Classical Theory of Electricity and Magnetism.* rev. Richard Becker, trans. John Dougall, 2nd ed. New York: Hafner.

ABRAHAM, RALPH and JERROLD MARSDEN. 1994. *Foundations of Mechanics.* 2nd ed. Boulder, CO: Westview Press.

ALBERT, DAVID. 1994. *Quantum Mechanics and Experience.* Cambridge, MA: Harvard University Press.

ALBERT, DAVID. 1996. "Elementary Quantum Metaphysics". In *Bohmian Mechanics and Quantum Theory: An Appraisal*, edited by James Cushing, Arthur Fine, and Sheldon Goldstein. The Netherlands: Kluwer, pp. 277–284.

ALBU, ION DORU and DUMITRU OPRIŞ. 2009. "The Geometry of Fractional Tangent Bundle and Applications". In *BSG Proceedings 16. The International Conference on Differential Geometry and Dynamical Systems.*

ARNOLD, V. I. 1989. *Mathematical Methods of Classical Mechanics.* 2nd ed. New York: Springer–Verlag.

BELOT, GORDON. 1996. "Why General Relativity *Does* Need an Interpretation". In *PSA 1996.* 63 (Proceedings), Lansing, MI: Philosophy of Science Association, pp. S80–S88.

BELOUSEK, DARRIN W. 2005. "Underdetermination, Realism, and Theory Appraisal: An Epistemological Reflection on Quantum Mechanics". *Foundations of Physics* **35** (4): 669–695.

BOS, H.J.M. 1980. "Mathematics and Rational Mechanics". In *The Ferment of Knowledge*, edited by G.S. Rousseau and R. Porter. Cambridge: Cambridge University Press, pp. 327–355.

BOYD, RICHARD. 1973. "Realism, Underdetermination, and a Causal Theory of Evidence". *Noûs* **7** (1): 1–12.

BOYD, RICHARD. 1979. "Metaphor and Theory Change". In *Metaphor and Thought*, edited by Andrew Ortony. Cambridge: Cambridge University Press, pp. 356–408.

BOYD, RICHARD. 1981. "Scientific Realism and Naturalistic Epistemology". In *PSA 1980*, vol. 2 (Proceedings). Lansing, MI: Philosophy of Science Association, pp. 613–662.

BOYD, RICHARD. 1983. "On the Current Status of the Issue of Scientific Realism". *Erkenntnis* **19** (1–3): 45–90.

BOYD, RICHARD. 1984. "On the Current Status of Scientific Realism". In *Scientific Realism*, edited by Jarrett Leplin. Berkeley and Los Angeles: University of California Press, pp. 41–82.

BOYD, RICHARD. 1985. "Lex Orandi est Lex Credendi". In *Images of Science: Essays on Realism and Empiricism*, edited by Paul Churchland and Clifford Hooker. Chicago: University of Chicago Press, pp. 3–34.

BOYD, RICHARD. 1989. "What Realism Implies and What It Does Not". *Dialectica* **43** (1–2): 5–29.

BOYD, RICHARD. 1990. "Realism, Approximate Truth, and Philosophical Method". In *Minnesota Studies in the Philosophy of Science*, vol. 14, edited by C. Wade Savage. Minneapolis, MN: University of Minnesota Press, pp. 355–391. Reprinted in Papineau (1996, pp.215–255).

BOYD, RICHARD. 1992. "Constructivism, Realism, and Philosophical Method". In *Inference, Explanation, and Other Frustrations: Essays in the Philosophy of Science*, *Pittsburgh Series in History and Philosophy of Science*, vol. 14, edited by John Earman. Berkeley and Los Angeles: University of California Press, pp. 131–198.

BROWN, HARVEY. 2005. *Physical Relativity: Space-Time Structure from a Dynamical Perspective*. Oxford: Oxford University Press.

CASEY, JAMES. 1994. "Geometrical Derivation of Lagrange's Equations for a System of Particles". *American Journal of Physics* **62** (9): 836–847.

CHALMERS, A.F. 1973. "Maxwell's Methodology and His Applicaion of It to Electromagnetism". *Studies in History and Philosophy of Science* **4** (2): 107–164.

CLARK, PETER and KATHERINE HAWLEY, eds. 2003. *Philosophy of Science Today.* Oxford University Press.

COTTRILL-SHEPHERD, KATHLEEN and MARK NABER. 2001. "Fractional Differential Forms". *Journal of Mathematical Physics* **42** (5): 2203–2212.

DARRIGOL, OLIVIER. 2000. *Electrodynamics from Ampère to Einstein.* New York: Oxford University Press.

DEVITT, MICHAEL. 2005. "Scientific Realism". In *The Oxford Handbook of Contemporary Philosophy*, edited by Frank Jackson and Michael Smith. New York: Oxford University Press, pp. 767–791.

EARMAN, JOHN. 1993. "Underdetermination, Realism, and Reason". In *Midwest Studies in Philosophy*, vol. XVIII. South Bend, IN: University of Notre Dame Press, pp. 19–38.

EARMAN, JOHN and JOHN NORTON. 1987. "What Price Spacetime Substantivalism? The Hole Story". *British Journal for the Philosophy of Science* **38** (4): 515–525.

FINE, ARTHUR. 1984b. "The Natural Ontological Attitude". In *Scientific Realism*, edited by Jarrett Leplin. Berkeley and Los Angeles: University of California Press, pp. 83–107. Reprinted in Fine (1986a, pp.112–135).

FINE, ARTHUR. 1986a. *The Shaky Game: Einstein, Realism, and the Quantum Theory.* 2nd ed. Chicago: University of Chicago Press. Reprint 1996.

FINE, ARTHUR. 1986b. "Unnatural Attitudes: Realist and Instrumentalist Attachments to Science". *Mind* **95** (378): 149–179.

FOX, ROBERT. 1974. "The Rise and Fall of Laplacian Physics". In *Historical Studies in the Physical Sciences*, vol. 4, edited by Russell McCormmach. Princeton, NJ: Princeton University Press, pp. 89–136.

FRASER, DOREEN. 2009. "Quantum Field Theory: Underdetermination, Inconsistency, and Idealization". *Philosophy of Science* **76** (4): 536–567.

GALLAVOTTI, GIOVANNI. 1983. *The Elements of Mechanics.* New York: Springer–Verlag.

GOLDSTEIN, HERBERT, CHARLES POOLE, and JOHN SAFKO. 2001. *Classical Mechanics.* 3rd ed. New York: Addison-Wesley.

GOODMAN, NELSON. 1983. *Fact, Fiction, and Forecast.* 4th ed. Cambridge, MA: Harvard University Press.

HAND, LOUIS and JANET FINCH. 1998. *Analytical Mechanics.* Cambridge: Cambridge University Press.

HARMAN, P.M. 1982. *Energy, Force, and Matter: The Conceptual Development of Nineteenth-Century Physics.* Cambridge: Cambridge University Press.

HEILBRON, J.L. 1980. "Experimental Natural Philosophy". In *The Ferment of Knowledge*, edited by G.S. Rousseau and R. Porter. Cambridge: Cambridge University Press, pp. 357–387.

HEISENBERG, WERNER. 1955. "The Development and Interpretation of the Quantum Theory". In *Niels Bohr and the Development of Physics*, edited by Wolfgang Pauli. Oxford: Pergamon Press, pp. 12–29.

JOSÉ, JORGE and EUGENE SALETAN. 1998. *Classical Dynamics: A Contemporary Approach.* Cambridge: Cambridge University Press.

KUHN, THOMAS. 1962. *The Structure of Scientific Revolutions.* 3rd ed. Chicago: University of Chicago Press. Reprint 1996.

KUKLA, ANDRÉ. 1996. "Does Every Theory Have Empirically Equivalent Rivals?" *Erkenntnis* **44**: 137–166.

KUKLA, ANDRÉ. 1998. *Studies in Scientific Realism.* New York: Oxford University Press.

LAGRANGE, JOSEPH-LOUIS. 1997. *Analytical Mechanics*, *Boston Studies in the Philosophy of Science*, vol. 191. Dordrecht: Kluwer. Translated by Auguste Boissonnade and Victor Vagliente. Orig. 1788.

LANCZOS, CORNELIUS. 1986. *The Variational Principles of Mechanics.* New York: Dover Publications. Orig. 1970.

LANGE, MARC. 2002. *The Philosophy of Physics: Locality, Fields, Energy, and Mass.* Oxford: Blackwell.

LAUDAN, LARRY. 1981. "A Confutation of Convergent Realism". *Philosophy of Science* **48** (1): 19–49. Reprinted in Papineau (1996, pp.107–138).

LEPLIN, JARRETT. 1997. *A Novel Defense of Scientific Realism.* New York: Oxford University Press.

LEWIS, DAVID. 1973. *Counterfactuals.* Oxford: Blackwell.

LEWIS, DAVID. 1986. *On the Plurality of Worlds.* Oxford: Blackwell.

LEWIS, DAVID. forthcoming. "Ramseyan Humility". In *The Canberra Plan*, edited by David Braddon-Mitchell and Robert Nola. Oxford: Oxford University Press.

LEWIS, PETER. 2004. "Life in Configuration Space". *British Journal for the Philosophy of Science* **55**: 713–729.

MACH, ERNST. 1919. *The Science of Mechanics: A Critical and Historical Account of Its Development.* 4th ed. Chicago: Open Court Publishing.

MALAMENT, DAVID. 2008. "Norton's Slippery Slope". In *PSA 2006*. 75 (5) (Proceedings), Lansing, MI: Philosophy of Science Association, pp. 799–816.

MARION, JERRY and STEPHEN THORNTON. 1995. *Classical Dynamics of Particles and Systems.* 4th ed. Saunders College Publishing.

MAUDLIN, TIM. 2007. *The Metaphysics within Physics.* New York: Oxford University Press.

MAXWELL, JAMES CLERK. 1925. *Matter and Motion.* London: The Sheldon Press. Reprint. Orig. 1876.

MAXWELL, JAMES CLERK. 1952a. "On Faraday's Lines of Force". In *The Scientific Papers of James Clerk Maxwell*, edited by W.D. Niven. New York: Dover Publications, pp. 155–229. Orig. 1856.

MAXWELL, JAMES CLERK. 1952b. "On Physical Lines of Force". In *The Scientific Papers of James Clerk Maxwell*, edited by W.D. Niven. New York: Dover Publications, pp. 451–513. Orig. 1861–1862.

MAXWELL, JAMES CLERK. 1952c. "A Dynamical Theory of the Electromagnetic Field". In *The Scientific Papers of James Clerk Maxwell*, edited by W.D. Niven. New York: Dover Publications, pp. 526–597. Orig. 1865.

MAXWELL, JAMES CLERK. 1954a. *A Treatise on Electricity and Magnetism*, vol. 1. 3rd. (1891) ed. New York: Dover Publications. Reprint. Orig. 1873.

MAXWELL, JAMES CLERK. 1954b. *A Treatise on Electricity and Magnetism*, vol. 2. 3rd. (1891) ed. New York: Dover Publications. Reprint. Orig. 1873.

MILLER, KENNETH and BERTRAM ROSS. 1993. *An Introduction to the Fractional Calculus and Fractional Differential Equations*. New York: John Wiley and Sons, Inc.

MONTON, BRADLEY. 2002. "Wave Function Ontology". *Synthese* **130**: 265–277.

MONTON, BRADLEY. 2006. "Quantum Mechanics and $3N$-Dimensional Space". *Philosophy of Science* **73**: 778–789.

MORRISON, MARGARET. 2000. *Unifying Scientific Theories: Physical Concepts and Mathematical Structures*. Cambridge: Cambridge University Press.

MOYER, DONALD FRANKLIN. 1977. "Energy, Dynamics, Hidden Machinery: Rankine, Thomson and Tait, Maxwell". *Studies in History and Philosophy of Science* **8** (3): 251–268.

NORTON, JOHN. 2003. "Causation as Folk Science". *Philosophers' Imprint* **3** (4). URL www.philosophersimprint.org/003004/.

NORTON, JOHN. 2008. "The Dome: An Unexpectedly Simple Failure of Determinism". In *PSA 2006*. 75 (5) (Proceedings), Lansing, MI: Philosophy of Science Association, pp. 786–798.

OLDHAM, KEITH and JEROME SPANIER. 2002. *The Fractional Calculus: Theory and Applications of Differentiation and Integration to Arbitrary Order*. New York: Dover Publications.

O'RAILLY, ALFRED. 1965. *Electromagnetic Theory*. New York: Dover Publications.

PAPINEAU, DAVID, ed. 1996. *The Philosophy of Science*. Oxford: Oxford University Press.

POOLEY, OLIVER and HARVEY BROWN. 2002. "Relationalism Rehabilitated? I: Classical Mechanics". *British Journal for the Philosophy of Science* **53**: 183–204.

PSILLOS, STATHIS. 1999. *Scientific Realism: How Science Tracks Truth.* London: Routledge.

PUTNAM, HILARY. 1975. *Philosophical Papers*, vol. 1: *Mathematics, Matter, and Method.* Cambridge: Cambridge University Press.

QUINE, W.V.O. 1975. "On Empirically Equivalent Systems of the World". *Erkenntnis* **9**: 313–328.

RABEI, EQAB, TAREQ ALHALHOLY, and AKRAM ROUSAN. 2004. "Potentials of Arbitrary Forces with Fractional Derivatives". *International Journal of Modern Physics A* **19** (17–18): 3083–3092.

RAILTON, PETER. 1989. "Explanation and Metaphysical Controversy". In *Minnesota Studies in the Philosophy of Science*, vol. 13, edited by Philip Kitcher and Wesley Salmon. Minneapolis, MN: University of Minnesota Press, pp. 220–252.

RIEWE, FRED. 1996. "Nonconservative Lagrangian and Hamiltonian Mechanics". *Physical Review E* **53** (2): 1890–1899.

RIEWE, FRED. 1997. "Mechanics with Fractional Derivatives". *Physical Review E* **55** (3): 3581–3592.

SANTILLI, RUGGERO MARIA. 1978. *Foundations of Theoretical Mechanics I: The Inverse Problem in Newtonian Mechanics.* New York: Springer–Verlag.

SCHAFFER, JONATHAN. 2003. "Is There a Fundamental Level?" *Noûs* **37** (3): 498–517.

SIMPSON, THOMAS. 1970. "Some Observations on Maxwell's *Treatise on Electricity and Magnetism*: On the Role of the 'Dynamical Theoy of the Electromagnetic Field' in Part IV of the *Treatise*". *Studies in History and Philosophy of Science* **1** (3): 249–263.

SKLAR, LAWRENCE. 1977. *Space, Time, and Spacetime.* University of California Press.

SKLAR, LAWRENCE. 2000. "Interpreting Theories: The Case of Statistical Mechanics". *British Journal for the Philosophy of Science* **51** (4): 729–742. Reprinted in Clark and Hawley (2003, pp.276–289).

SKOW, BRAD. 2007. "Sklar's Maneuver". *British Journal for the Philosophy of Science* **58** (4): 777–786.

SMITH, SHELDON. 2008. "Symmetries and the Explanation of Conservation Laws in the Light of the Inverse Problem in Lagrangian Mechanics". *Studies in History and Philosophy of Modern Physics* **39** (2): 325–345.

STANFORD, P. KYLE. 2006. *Exceeding Our Grasp: Science, History, and the Problem of Unconceived Alternatives.* New York: Oxford University Press.

SYNGE, J.L. 1927. "On the Geometry of Dynamics". *Philosophical Transactions of the Royal Society of London. Series A* **226**: 31–106.

TRUESDELL, CLIFFORD. 1968. *Essays in the History of Mechanics.* New York: Springer.

VAN FRAASSEN, BAS. 1980. *The Scientific Image.* Oxford: Clarendon Press.

VAN FRAASSEN, BAS. 1989. *Laws and Symmetry.* New York: Oxford University Press.

VUJANOVIC, B. D. and S. E. JONES. 1989. *Variations Methods in Nonconservative Phenomena.* Academic Press, Inc.

WILSON, JESSICA. 2007. "Newtonian Forces". *British Journal for the Philosophy of Science* **58** (2): 173–205.

WILSON, MARK. 1980. "The Observational Uniqueness of Some Theories". *Journal of Philosophy* **77** (4): 208–233.

WILSON, MARK. 2009. "Determinism and the Mystery of the Missing Physics". *British Journal for the Philosophy of Science* **60** (1): 173–193.

WORRALL, JOHN. 1989. "Structural Realism: The Best of Both Worlds?" *Dialectica* **43** (1–2): 99–124.