NEWTON AND EMPIRICISM

Edited by

ZVI BIENER

ERIC SCHLIESSER

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Zvi Biener
and
Eric Schliesser

OXFORD
UNIVERSITY PRESS

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Oxford New York
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New Delhi Shanghai Taipei Toronto

With offices in
Argentina Austria Brazil Chile Czech Republic France Greece
Guatemala Hungary Italy Japan Poland Portugal Singapore
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Published in the United States of America by
Oxford University Press
198 Madison Avenue, New York, NY 10016

Cataloging in Publication data on file with the Library of Congress

ISBN 978–0–19–933709–5

1 3 5 7 9 8 6 4 2
Printed in the United States of America
on acid-free paper

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INTRODUCTION

Zvi Biener and Eric Schliesser

Sir Isaac Newton's association with "empiricism"—or rather, the set of traditions that constitutes "empiricism"—was clearly recognized during his lifetime and enshrined by Enlightenment *philosophes* as ideology after his death. Voltaire, for example, famously identified Newton's physics and Locke's metaphysics as the intellectual framework for the Age of Reason. The association became such a significant feature of the intellectual landscape that in the eighteenth century a thinker's relation to Newton was often a matter of self-definition; by way of affinity or difference, it was a means of locating one's advertised position in the philosophical spectrum. The influence of naturalistic and experimentalist thought on Newton was similarly well known. Roger Cotes highlighted it in his polemical preface to the *Principia*'s second edition. And Newton himself, although he cited sources only sparingly, explicitly affiliated himself in the *Principia* with the mathematical-experimental tradition of Galileo and Huygens. Moreover, from the 1690s onward Newton used language borrowed from the Baconian/Boylean experimental tradition; and, as the first Part of this volume demonstrates, his first optical works were set in a Baconian natural-historical mold and were read as such by his contemporaries and successors.

Yet the coupling of Newton and empiricism is not without problems. Some of the best-known "classical Empiricists" (with a capital "E"!) were prominent critics of Newton: Berkeley, for example, famously rejected the Newtonian fluxional calculus. Recent and ongoing scholarship has focused not only on *substantive* differences between Empiricists (e.g., Locke, Berkeley, Hume) and Newtonians (e.g., Newton, Clarke, MacLaurin), but also on the *polemics* exchanged between them.[1] Moreover, there were sharp differences among prominent eighteenth-century "Newtonians"—many of whom held a variety of Leibnizian metaphysical commitments—and Empiricists regarding questions central to empiricism: Euler and d'Alembert, for example, debated the limits, if any, of applying mathematics to nature,[2] and Hume demurred from the natural religion and physical theology espoused by the likes of Berkeley, Clarke, and

[1] E.g., Domski (2011), Schliesser (2009, 2011).

[2] Iulia Mihai has taught us this.

Newton. These problems challenge Voltaire's facile historiography to such a degree that explicating Newton's relation to "empiricism" is not a matter of adding minutiae to a broadly well-known narrative, but of constructing the narrative itself.

There is also a related, more reflexive difficulty: Why is the question of Newton's relation to "empiricism" as open as it currently is? The proximate cause may be the revival of philosophical interest in Newton's philosophy and its impact during the last forty years on English-language history of philosophy and philosophy of physics.[3] But it is also undoubtedly a consequence of a diffuse process in which categories inherited from Kantian-Hegelian historiography are interrogated and challenged, while at the same time Kantian-Hegelian first-order positions are giving way to what is sometimes called 'knee-jerk realism' in analytical philosophy or 'speculative realism' in continental philosophy.

By reflecting on these historical trends, we can offer two main reasons for the openness of the "Newton and empiricism" question. These reasons both motivate and structure the present volume. First, as already suggested, there is no single tradition that is "empiricism." Although there is no touchstone work in early modern studies that proclaims the death of the singular 'empiricism' as Charles Schmitt's "Renaissance Aristotelianisms" did for the singular 'Aristotelianism,' scholarship in the past decades has increasingly recognized an untidy heterogeneity of empiricist philosophical positions. Contrary to the implicit message of (particularly Anglophone) undergraduate courses and the more erudite, older reconstructions of philosophy's history on which they are based,[4] there is no body of doctrine in early modernity that was "empiricism" and no set of thinkers who self-identified as 'empiricists.'[5] For example, the temporal third of the classical Empiricists—Hume—certainly acknowledged profound debts to and engaged critically with Locke and Berkeley, but scholars have long known that Malebranche and Bayle were also very important sources.

Rather, our contemporary 'empiricism' refers to a mélange of related ideas that privilege experience, but in manners diverse and often indirect. These ideas may be overtly semantic or epistemological (concerning the origin of mental contents or the ultimate sources of justification), but they can also be methodological (concerning the proper method of discovery and use of evidence), practical and technological (concerning

[3] The revival and incorporation of Newton was lead by J. E. McGuire, I. B. Cohen, Howard Stein, Margaret Jacob, Mary Hesse, Ernan McMullin, Michael Friedman, Alan Shapiro, George Smith, and Bill Harper. Obviously, many other historians of physics and mathematics have made seminal contributions to the study of Newton, and there are many second- and third-generation Newton scholars now making significant advances.

[4] E.g., Burtt ([1932] 1954), Russell (1945), Copleston (1959).

[5] There were, of course, empiricks, but they do not answer to the undergraduate/great book use of 'empiricist.'

rules-of-thumb or procedures for developing knowledge through real-world manipulation, but also real-world manipulation for non-epistemic, e.g., medical, ends), or political and moral (concerning the social norms that govern knowledge creation, the sources of authority, and/or the broader goals of human life).[6]

In fact, the very label 'empiricism' has come under attack in recent years by Peter Anstey and his collaborators in the so-called Otago School.[7] They argue that 'empiricism' is an ahistorical category that should be replaced with 'experimental philosophy,' an actor's category whose contrast with 'speculative philosophy' captures more precisely what empiricism's contrast with rationalism was traditionally supposed to capture, but failed. And certainly, Otago has something right. We wholeheartedly endorse their core claim: 'empiricism' is a late eighteenth-century label, and we should take care not to attribute it to any earlier actors. However, 'experimental philosophy' does not do any better at accounting for the multiplicity of historical positions, alliances, and developments. Or, to put it more accurately, it only does better when the scope of 'empiricism' is artificially limited to the undergraduate/great book semantic/epistemological use. If 'empiricism' is understood broadly (as it is understood in this volume), it poses challenges as great as those posed by 'experimental philosophy' and, more importantly, opens the same interpretive possibilities.

With both 'empiricism' and 'experimental philosophy,' the challenges are to articulate in what ways thinkers were empiricists or experimentalists, how their prima facie diversity nonetheless belies a philosophic or phylogenic commonality that merits classification as 'empiricist' or 'experimentalists,' and how such a commonality sheds light on their interactions with their contemporaries and their readings of and readership by their predecessors and successors. The devil, if you will, is in the details. The recourse to details, however, does not indicate the bankruptcy of the primary category—either experimental philosophy or empiricism—but rather suggests that either category is messy enough that a terminological shift cannot clarify it.

That said, there are several interrelated advantages in using 'empiricism' even if we grant that the speculative vs. experimental distinction does justice to important pre-Newtonian, seventeenth-century actors' categories. This volume is concerned with Newton, and Newton himself cannot be subsumed under the tradition of experimental philosophy without serious caveats. The most important of such caveats concerns the central importance of mathematics for Newton's natural philosophy and the fact that his understanding of mathematics and the relation between mathematical knowledge and evidential access to the real world was significantly influenced by

6 Waldow (2010), Wolfe (2010), Schliesser (*forthcoming*).

7 See Anstey (2005), Anstey and Vanzo (2012).

non-experimental thinkers like Descartes[8] and by thinkers that, while experimentalists, are more neatly located in the tradition of mixed-mathematics, like Galileo and Huygens.[9] A second caveat concerns Newton's own use of "experimental philosophy." Although one of the main advantages of the term is that it was an actor's category, it was not *Newton*'s category until after the publication of the first edition of the *Principia*. The phrase made its first printed appearance in the context of gravitational research only in the "General Scholium" of the *Principia*'s second edition (1712) and in the context of optical research only in draft queries to the Latin translation of the *Opticks* (1706).[10] It was likely introduced for polemical purposes—to defend Newtonian methodology against Cartesians and Leibniz—and its late introduction, despite Newton's engagement in similar methodological battles in the 1670s, indicates that Newton did not think that the character of his natural philosophy would be rendered obvious to his contemporaries simply by labeling it 'experimental.'

Another reason we favor 'empiricism' in the Newtonian context concerns the nature of system-building in the seventeenth and eighteenth centuries. In the seventeenth century, experimental philosophers were sometimes contrasted with system-builders. Experimentalists favored a more piece-meal approach to knowledge construction and inveighed against what they saw as the epistemic overreach in the architectonic systems of, say, Descartes. This "bottom-up" approach represents much of importance in seventeenth-century experimental philosophy, but it misses the mark when it comes to Newton. Newton was both a mathematical system-builder and an experimentalist.[11] When his system-building efforts were emphasized, it was even possible to put him in the same camp as Descartes (the arch system-builder) and apart from Boyle (the arch experimentalist).[12] Similar impulses can be easily seen in the negative reactions, say, by Leibniz, against Newton's inexplicable gravity. Was Newton, then, an experimental philosopher? We suggest that, phrased this simply, this is not a revealing question. Newton's way of systematizing observational data was sufficiently novel that it reoriented what one may have expected to conclude from experiments.[13] This is a crucial point about Newton's experimentalism that the emphasis on his continuity with earlier experimental philosophy (perhaps inadvertently) downplays.

[8] E.g., McGuire (2007), Gorham (2011).

[9] Murray, Harper, and Wilson (2011), Harper (2011), Garber (2012), Kochiras (2013). Newton was also guided by reflection on the ancients, e.g., Domski (2012) and the classic McGuire and Rattansi (1966) and commentary thereof.

[10] Shapiro (2004).

[11] E.g., Dunlop (2012) and essays in the first part of this volume.

[12] See, e.g., Otago's Gomez (2012).

[13] See Smith in this volume.

A final reason for our use of 'empiricism' is that one of our main interests is in philosophy itself and its self-constructed narrative history. That is, we are interested in the question of what Newton actually had to do with what eventually became known as 'empiricism,' either narrowly or broadly defined.[14] We believe that our terminology keeps this question firmly in mind. For, recent scholarship has shown that the authority of Newton's natural philosophy was deployed (and challenged) in a number of very important and highly charged eighteenth-century philosophical debates, several of which were crucial to the intellectual currents that drove apart "philosophy" and "science" and untangled "natural religion" and "natural science."[15] There were, of course, the familiar debates over the ontological status of key Newtonian concepts, particularly "absolute space" and "attraction." But there were also the Newtonian attacks on Spinozism,[16] the Humean attack on Newtonian natural religion,[17] and a whole variety of challenges regarding the mathematization of particular forms of inquiry (e.g., Mandeville in medicine, Buffon in natural history),[18] which included arguments from all three classical Empiricists.[19] In none of these debates was there a uniform "experimental" position that can be matched up to a canonical Newtonian stance. The same is true, of course, for the lack of uniformity of "empiricism." Even so, within these debates one can recognize "empiricist" constraints that are shared (or rejected) by participants, while this is not true of "experimental" constraints. In sum, when used with caution, the term 'empiricism' does not obscure any insight that might be gained from a careful study of the heterogeneous seventeenth- and eighteenth-century cultures of taking experience seriously. But there is plenty of work to be done. The essays in this volume exemplify some of the issues that make Newton's relation to these cultures far from well understood.

A second reason for the current openness of the question of Newton's relation to "empiricism" is that our picture of Newton himself has changed significantly in recent decades, and as our picture changes, our understanding of how Newton's contemporaries and successors read him changes correlatively.[20] Of particular importance here

[14] E.g., Fate (2011).

[15] Shank (2008), Schliesser (2011).

[16] Jorink (2009), Schliesser (2012), Ducheyne (2013).

[17] Hurlbutt ([1965] 1985).

[18] Hoquet, T. (2010).

[19] E.g., Domski (2012) on Locke; Jesseph (1993), Guicciardini (1993) on Berkeley; Meeker (2007) and Hazony and Schliesser (2014) on Hume.

[20] For example, Downing's essay in this volume discusses Locke's understanding of Newton's account of creation in *De Grav*, a document that was not widely available before 1962. Smith essay outlines the history of gravitational research in the past three centuries in light of the methodology implicit in the *Principia,* a methodology whose contours have only been fully understood recently.

is the hard-won understanding of the methodological nature of Newton's achievement in the *Principia*. While Newton's willingness to "stop short" of deep ontological commitments has long been recognized,[21] the complex evidential structure that allowed him to stop short—and crucially, to specify *where* to stop short—has only become clear relatively recently.[22] The importance of this development for understanding Newton's "empiricism" cannot be overstated. While reluctance to engage in ontological speculation is a hallmark experimentalism, Newton's reluctance was of a new sort: principled, highly mathematical, and borne of a deep commitment to the possibility of attaining *certainty* within a properly wielded natural philosophy. While elements of this stance were undoubtedly already present in Newton's early optical papers, the stance developed through the writing and rewriting of the *Principia*, particularly under pressure from hostile and friendly criticism, and given new empirical results.[23] There has also been an increasing body of scholarship on Newton's matter theory[24] and "chymistry."[25] Although no essays in this volume treat Newton's alchemical works, the subject is significant to understanding empiricism, as it provides a context distinct from the mixed-mathematical one and largely distinct from the Baconian one, at least in so far as it was intrinsically tied to a tradition of procedures that explicitly connected theory and experiment.[26]

Finally, the increased attention—cottage industry, if you will—centered on the renewed translation of the manuscript *De Gravitatione*—the most "philosophical" of Newton's works to modern eyes—has generated significant scholarly work on Newton's relationship to Descartes (and even Spinoza), his metaphysics, his theology (aided by the significant efforts of the Newton Project), his views on mathematics, as well as his broad methodological framework, a framework that combines conjectural and certain theses into a coherent natural philosophical and theological whole.[27] All of these certainly give impetus to a reevaluation of the association of Newton with Lockean classical empiricism.

This complexity in Newton's thought and in the nature of "empiricism" itself structures this volume. It is divided in three parts. The first part—"The Roots of Newton's Experimental Method" (by Gaukroger, Jalobeanu, and Hamou)—drives home three crucial points. First, empiricism as a doctrine about the sources and nature of the

[21] See the actors in Wolfe's study below.
[22] Cohen (1982), Stein (ms), Smith (2002) and below, Harper (2011), Belkind (2012).
[23] Biener and Smeenk (2012), Schliesser (2012).
[24] E.g., Brading (2012), Biener and Smeenk (2012), Kochiras (2011).
[25] E.g., Dobbs (1975), Westfall (1980), Figala (2002), Newman (2002).
[26] See Newman (2011); on the connection with optics in particular, Newman (2010).
[27] Works here are too numerous to cite, but special mention ought to be made of McGuire (1995), Stein (2002), and Janiak (2008).

understanding emerged from an earlier Baconian tradition of experimental natural philosophy, as it was practiced by mid-century thinkers in the Royal Society and, as Gaukroger stresses, as it was modified through interaction with the work of system-builders like Malebranche. Second, Baconian experimental natural philosophy was concerned with the discovery and composition of natural philosophical facts through the guidance of experiments whose reporting and interrelations were autonomous from the strictures of a predetermined fundamental ontology or privileged explanatory basis. Jalobeanu details the ways in which such experiments built on one another, formed elaborate experimental series, and, to use a much later phrase, came to have a life of their own. Nevertheless, as Hamou reveals, in certain instances this "experimental life" was tied to theory in surprising ways. Third, as all three essays demonstrate, Newton's work in optics was indebted to this mode of investigation, and his success provides an exemplar through which to articulate the practice of an autonomous, experimentally based natural philosophy.

The second part of this volume—"Newton and 'Empiricist' Philosophers" (by Downing, Gorham and Slowik, Hazony, and Demeter)—deals with Newton's impact on some of the classical Empiricists. Only the first two essays deal with empiricism as a semantic/epistemological doctrine, and both use Leibniz as a foil for Locke and Newton. Downing explores how Newton's success in establishing gravity as a property of matter seemingly challenged Locke's essentialism and the primary/secondary distinction. She shows how Newtonian discoveries occasioned significant philosophical work for Locke and were neither uncritically nor easily assimilated into the Lockean framework. Through this analysis, she further clarifies the nature of Locke's commitments. Gorham and Slowik further demonstrate the tensions between Lockeanism and Newtonianism by highlighting that in regard to space and time Locke and Newton employed importantly different types of "empiricism," what the authors term "sensationalist" and "scientific" empiricism. Locke's "sensationalist" empiricism lead him to believe that sensible measures of absolute space and time are doubtful, even if he did not doubt the existence of an in-principle empirically inaccessible absolute space and time. Newton's "scientific" empiricism allowed for empirically established physical theory to be a sufficient guide both to the existence and measure of inaccessible entities.

Hazony and Demeter discuss the tensions between Newtonianism and Humeanism, but their focus is methodological. They articulate how Newton's method—particularly his concepts of analysis and synthesis—influenced the Humean "Science of Man" and the system of sciences into which it was incorporated. Hazony echoes themes from Part I of this volume and connects Newton's vision of the sciences to Boylean ideals of explanatory reduction. He argues that Hume took from Newton these ideals of reduction and, despite the contrary appearance of the *Treatise*, successfully constructs a "system" that embodies them. Demeter also holds that Hume's affinity with Newton is primarily methodological and further argues that there is a distinction to be made between the methodology of the *Opticks*, particularly the "Queries," and that of the *Principia*. He argues that Hume's science of man is an application of the first of these to the human being *qua* moral being and that thus Hume's manner of "enlarging the bounds of moral philosophy" by way of the perfection of natural philosophy was radically different than the natural-theological path Newton had imagined. It should be noted that for both Hazony and Demeter, questions of semantics and epistemology take a back seat to questions of scientific method and scientific system-construction.

The third part of this volume—"Newtonian Method in 18th and 18th-Century Science" (by Nyden, Wolfe, and Smith)—deals with Newton's impact on diverse natural philosophical and scientific practitioners in the eighteenth, nineteenth, and twentieth centuries. Nyden shows how both "Continental Rationalists" and "British Empiricists" took up Newtonian experimentalism. Her essay directly undermines the distinction between semantic/epistemic rationalism and empiricism and shows that in order to understand the place of experimentation in the early eighteenth century, we must broaden our understanding of these categories. Wolfe shows how Newtonian methodology itself was variously understood by empiricists, vitalists, and other natural philosophers in the eighteenth century. He stresses the role that analogical transpositions of Newtonian method played in justifying eighteenth-century practices in the life sciences, as opposed to the direct incorporation of Newtonian metaphysical or physical tenets into theory. He shows that such transpositions united a variety of seemingly diverse schools, and thus offers a novel interpretive lens through which to understand Newtonian influence in the eighteenth century. Smith, on the other hand, shows how the research program established by Newton in the *Principia* was faithfully followed and developed into the twentieth century. Smith's essay is the longest in the volume. We believe it constitutes a major landmark in research on Newton and his reception and a capstone to a generation's worth of scholarly study of Newton's methods of inquiry in the *Principia*.

Smith's chapter is also noteworthy because many other chapters engage extensively with Newton's *Optics*. They do so for good reason: in the eighteenth century the optical works were celebrated and could be more easily understood.[28] Yet the *Opticks* is not simply more accessible than the *Principia*; it includes quite a bit of philosophical reflection by Newton, which framed and inspired eighteenth-century responses to him. While the optical works have certainly not gone unnoticed,[29] we hope our volume will further direct scholarly attention to Newton's optical writings, both in philosophical scholarship on Newton as well as in the history of early modern philosophy.

[28] Fontenelle singled these out in his influential obituary of Newton, Gillispie (1978).
[29] See especially the seminal work by Sabra (1981) and Shapiro (1993).

ACKNOWLEDGMENTS

We are grateful to the University of Pittsburgh and, in particular, the Center for Philosophy of Science, for hosting a "Newton & Empiricism" conference in the spring of 2010, organized by J. E. McGuire and ourselves. Although some of the authors represented in this volume were not present, and some speakers at the conference are not represented in this volume, it formed the core around which the volume developed. We thank the University of Pittsburgh, the Center, and the Center's director, John Norton, for their support.

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PART ONE

The Roots of Newton's Experimental Method

1

EMPIRICISM AS A DEVELOPMENT OF EXPERIMENTAL NATURAL PHILOSOPHY

Stephen Gaukroger

We do not know the substances of things. We have no idea of them. We gather only their properties from the phenomena, and from the properties [we infer] what substances may be . . . . And we ought not rashly to assert that which cannot be inferred from the phenomena.[1]

Since the beginning of the twentieth century, empiricism has been construed predominantly as a response to sceptically driven problems in epistemology, one that is the mirror image of rationalism, with which it shares an epistemological agenda, and with which it is in direct competition. This is not the only way of thinking of empiricism. From the perspective of the development of empiricism in the eighteenth century, it does not correspond to how those identified as its greatest proponents—notably Locke and Hume—conceived of its scope and aims. In what follows, I want to sketch an alternative account. I want to explore empiricism as a successor to, and philosophical refinement of, seventeenth-century 'experimental' natural philosophy, something that was intimately tied up with natural-philosophical practice, and was quite distinct from the speculative epistemology to which it was reduced in the 'rationalism/empiricism' debates. Moreover, as it matured, empiricism offered a form of naturalism that was distinctive in that it rejected the attempts to assimilate all cognitive enquiry to a form of physics-inspired natural philosophy. This is an important dimension of empiricism, one missed if it is viewed in rationalist/empiricist terms.

In reconstructing the emergence of empiricism from natural philosophy, I shall focus on three questions. First, I examine two formative developments in experimental natural philosophy: Boyle's account of pneumatics and Newton's analysis of the production of the spectrum. Second, I look at Locke's defence of some basic principles underlying experimental natural philosophy in his attempt to articulate the legitimacy of a form

[1] Sir Isaac Newton, draft of the "General Scholium" in the *Principia*, composed around 1712, in Newton (1962, p. 360).

of natural philosophy that is not grounded in, and does not require grounding in, a more fundamental underlying natural description. This development results in the transformation of experimental natural philosophy into a general philosophical account of the sources and nature of understanding—empiricism. Third, I examine the way in which Newton's work generally was read through Lockean eyes, and at how Hume developed this approach in the case of the 'moral sciences'. There are deep and difficult questions here, which I have explored in detail elsewhere.[2] My aim in this chapter is simply to provide a sketch of an alternative narrative of the development of empiricism, one which is more faithful to the concerns of early-modern thinkers, and which is far more engaging and plausible as an account of the sources and nature of understanding.

1.1 EXPERIMENTAL NATURAL PHILOSOPHY I: BOYLEAN PNEUMATICS

In his *New Experiments Physico-Mechanical, Touching the Spring of Air* (1660),[3] Boyle set out reports of forty-three experiments performed with his air pump. A year later, in his *Dialogus physicus de natura æris*,[4] Hobbes challenged Boyle's account of his experiments to produce a vacuum, initiating a dispute that was to last into the next decade, and which raised the fundamental question of what natural-philosophical understanding consisted in. In particular, it explicitly pitted 'speculative' against 'experimental' natural philosophy for the first time. How, asks Hobbes in the *Dialogus*, could Boyle have aroused 'the expectations of advancing physics, when you have not established the doctrine of universal and abstract motion (which was easy and mathematical)'.[5]

The experiment identified by Boyle as 'the principal fruit' of the air pump consisted in placing a Torricellian barometer in the pump and noting the change in the level of mercury as the tube evacuated.[6] The appearance of an empty space at the top of a sealed tube of liquid (the size of which depends on the liquid and its height), when the full tube is inverted and placed in a dish of the same liquid, was something that attracted a variety of explanations. It was a test case for competing natural philosophies, above all for the Aristotelian theory that 'nature abhors a vacuum'. One of

[2] In what follows, I draw extensively on the first two volumes of my account of the emergence of a scientific culture in the West, in Gaukroger (2005) and (2010). A fuller discussion of the issues raised here will be found there.

[3] Boyle (1772, vol. 1, pp. 1–117).

[4] Hobbes (1839–1845, vol. 4, pp. 233–296), trans. as the appendix to Shapin and Schaffer (1985, pp. 346–391).

[5] Shapin and Schaffer (1985, p. 379).

[6] The first published mention of Torricelli's experiments in English was in Charleton (1654, p. 348).

the features of the barometer, and what made it so important, was that it could be manipulated to decide between competing accounts. Those who denied the existence of a vacuum, for example, had to provide some account of the space above the column of mercury in the sealed tube. Suggested explanations included the idea that a vapour was formed above the column, or that there was a bubble of air, but these contradicted the results of experiments.[7]

Boyle's insertion of a barometer into his air pump suggested that it was not a question of the weight of air, as others had believed, because although the height of the column of mercury was normal in a sealed container—once the container began to be evacuated—the level of the mercury fell; this could not be due to a balance of weights, as the weight of air in the container was negligible compared to that of the mercury. The variable factor, Boyle concluded, was not the weight of the air but air pressure, something reinforced in another experiment in which a bladder containing a small amount of air expanded as the container in which it was placed was evacuated. Air, he concluded, is an elastic fluid which expands as external constraints are removed.[8]

What was at issue in the dispute between Hobbes and Boyle was tied up with the question of just what kind of project the natural philosopher should be engaged in. In *De corpore*, Hobbes distinguished natural philosophy from natural history on the grounds that the latter 'is but experience, or authority, and not ratiocination'.[9] Boyle, by contrast, in his *An Examen of Mr. T. Hobbes His Dialogus Physicus de Natura Aëris*, takes Hobbes to task by asking 'what experiment or matter of fact' Hobbes has 'added to enrich the history of nature'.[10] The difference is a fundamental one. One of Hobbes' principal criticisms of Boyle's experiments, for example, is that, in invoking a restorative power in the air, Boyle is offering an account of the phenomenon which is not genuinely causal, and that such an account cannot be an explanation.[11] The basic problem for Hobbes seems to be that Boyle's proposed explanation would rule out more fundamental theories that are central to Hobbes' natural-philosophical system. Above all, Hobbes' physical optics required a medium for the transmission of light, and it was the normal transmission of light through the space at the top of the mercury that prompted his first doubts about the existence of a vacuum in 1648. For Hobbes, fundamental natural-philosophical issues had to guide one's explanations. But whereas Boyle had insisted that in addition to Hobbes' two criteria for the acceptance of a hypothesis—conceivability and necessity—we must also include 'a third, namely that it

7 Boyle (1772, vol. 1, p. 11).
8 Boyle (1772, vol. 1, p. 11).
9 Hobbes (1839–1845, vol. 1, p. 3).
10 Boyle (1772, vol. 1, p. 197).
11 Hobbes (1839–1845, vol. 4, pp. 247–248), trans. in Shapin and Shaffer (1985, pp. 356–357).

be not inconsistent with any other truth or phaenomena of nature'.[12] What he offers can consistently leave open the question of how light is propagated, since this is not a truth or phenomenon of nature but a hypothesis. Indeed, it is such hypotheses that the third criterion is directed toward, and he points out that Hobbes' notion of air as a homogeneous penetrative fluid is simply not consistent with the results of the experiment.

As far as Hobbes was concerned, Boyle's approach simply ignored fundamental natural-philosophical questions, offering what might be regarded as a one-off explanation for a phenomenon produced under conditions that can be questioned: any apparatus that produces variable pressures is particularly susceptible to leakage, for example, and Hobbes doubted that leaks had been avoided. If we see things in this way, we can appreciate Hobbes' frustration at Boyle's approach. The frustration was shared by, and similarly exasperated, some of Boyle's continental contemporaries, notably Spinoza and Leibniz. Yet the presentation of results in terms of bare facts, as it seemed to many of his critics, was not for Boyle a provisional record of research which was at a stage too early to merit systematization, or too early for appeal to fundamental causes.[13] In his earliest collection of papers, *Certain Physiological Essays*, Boyle makes it clear that he disagrees with 'some eminent Atomists' who maintain that 'no speculations in natural philosophy could be rational, wherein any other causes of things are assigned than atoms and their properties'.[14] As far as Boyle is concerned, Hobbes is treating an explanatory ideal as if it were a realistic goal. The trouble is that the explanatory ideal not only could do no real work here, but, *contra* Hobbes, actually prevents progress. For Hobbes, progress is guided by fundamental natural-philosophical principles which identify what needs to be explained, and indicate the appropriate type of explanation.[15] This was the level at which it was clear that a new start in natural philosophy had been made, because this was the level at which the fundamental assumptions and principles of the new natural-philosophical system were manifest. On this question, he was at one with Beeckman, Mersenne, Gassendi, and Descartes. Fundamental natural-philosophical principles do not play this role in Boyle, not because he did not subscribe to such principles—he was committed to mechanism no less than Hobbes—but because he has a different sense of the way in which natural philosophy can best be made to work.

[12] Boyle (1772, vol. 1, p. 241).

[13] See, for example, his comment in *Certain Physiological Essays*, that 'I am content, provided experimental learning be really promoted, to contribute even in the least plausible way to the advancement of it; and had rather not only be an under-builder, but even dig in the quarries for materials towards so useful a structure, as a solid body of natural philosophy, than not do something towards the erection of it'. Ibid., p. 307.

[14] Ibid., p. 308.

[15] See Gaukroger (2005, pp. 368–372).

Take the case of respiration. This was one of the great problems bequeathed by Harvey, and it was intimately connected with the 'spring of air'. The latter holds the key to the former: we will not understand how respiration works unless we understand the spring of the air. Now respiration is a complex empirical event. Extensive anatomical and physiological work is involved in the elucidation of the phenomena involved, as are chemical questions, but Boyle is able to focus on what might otherwise appear a diverse set of questions by thinking in purely mechanical terms about what is involved in the pumping action of the heart. The air pump not only allows him to bring a completely new focus to these issues, but also to vary conditions experimentally, and the focus is in turn constrained by what can be varied in this way. This is the kind of consideration that makes it look, to a critic like Hobbes, as if Boyle has taken a highly contingent, highly localized topic, centred around a highly specific piece of apparatus, reporting specific results of very limited natural-philosophical significance, yet all the while giving the impression that what he is doing is as legitimate as a 'core' form of natural philosophy.

What guides Boyle's approach to natural philosophy is not what explanatory resources a micro-mechanical corpuscularian model offers. He is committed to such a model, and it guides the kinds of explanations he is prepared to propose. But it does not organise his *explanandum*. It is the air pump that shapes and brings unity and coherence to the field of enquiry. It acts as a focus for a number of contingent interests in physiology, chemistry, and mechanics, and its success lies in its ability to produce a rich range of highly controllable phenomena.

In brief, Boyle discovered that in order to account for certain phenomena in a satisfactory way, he had to suspend his commitment to corpuscularianism. Because corpuscularianism had acted not merely as a form of explanation, but also as a way of organizing the *explanandum* into phenomena that needed explaining and those that did not, and distinguishing real and apparent properties, this meant that he needed some alternative way of organizing the phenomena under investigation other than in terms of underlying micro-corpuscularian structure. This organization was effectively provided by the experimental apparatus itself. The apparatus produced a certain range of phenomena which defied explanation in fundamental terms, and indeed from a foundationalist mechanist perspective, the results produced showed no internal coherence: they were anomalous. The way in which they were generated was therefore crucial, not just because this is what legitimated them, but also because, if they were to have any coherence at all, it had something to do with the way in which they were generated, for it was this that held them together as connected phenomena, and excluded what might, on mechanist grounds, mistakenly or at least unhelpfully appear to be related phenomena. The way in which the results were generated was a function of the experimental apparatus, the way in which this apparatus was manipulated, and what one was able to do with it. Here a domain of investigation is brought into focus not through the constraints imposed by a postulated underlying structure, but by means of the experiment or instruments. Exactly the same considerations hold in the case of Newton's 1666 experiments on the optical spectrum.

1.2 EXPERIMENTAL NATURAL PHILOSOPHY II: NEWTON ON THE SPECTRUM

For the advocate of a systematic mechanism, ultimate explanations take the form of accounts in terms of underlying microscopic states, so that causation, and with it explanation, are always construed as vertical, as it were: causes and effects were not on the same level, because causes are always more fundamental. By contrast, Boyle and Newton postulated horizontal causal processes, those where cause and effect were on the same level, and where this was defended as a genuine and independent form of explanation. What is at stake here is explanation of phenomena in terms of their systematic relations with other phenomena, not in terms of some underlying reality. Opponents of this way of proceeding were completely non-plussed by the claims of experimental philosophy, construing it as at best a merely provisional stage on the road to explanation in terms of underlying principles. Leibniz and Spinoza both thought Boyle perverse in not offering a 'systematic' account of his views, for example. Likewise, in criticizing Newton's account of the production of a colour spectrum with a series of prisms, Huygens demanded that a hypothesis be offered as to how differences in motion were connected with differences in colour. But both Boyle and Newton saw the matter in a very different way. In effect, they rejected the idea that causes must be restricted to what underlies the phenomena, and in consequence that they must be located at a different level from the phenomena. Rather, their treatment implied that there is a way of understanding at least some phenomena that consists in exploring the causal connections between—as opposed to underlying—them.

Two principal sources of inspiration for Newton in his early optical work were Descartes' *Dioptrique* and *Météors*, and Boyle's *Experiments and Considerations*.[16] Descartes' view was that white light is homogeneous, but that under certain circumstances, such as refraction through a prism at a particular angle, the constituent corpuscles making up the light ray are caused to rotate at different speeds, and this in turn causes us to see different colours. Newton's view was that light is heterogeneous and that under certain circumstances, again such as refraction through a prism at a particular angle, the light ray is decomposed into its constituent rays, which are differently

[16] See McGuire and Tamny (1983, pp. 262–272) and more generally Hall (1993, ch. 2).

coloured. If we compare these cases on the question of the relation between *explanans* and *explanandum*, we see that the way in which Descartes and Newton come to their respective conclusions is radically different. Descartes builds up a geometrical optics and then shifts into a wholly different register, a micro-corpuscularian physical optics, to account for colour. Newton, by contrast, does not explain colour by reference to an underlying causal realm which produces effects at the phenomenal level, but remains in the realm of geometrical optics and explores causal relations between the phenomena themselves. As Huygens, commenting on Newton's account of the heterogeneity of light, puts it in a letter to Oldenburg, 'if it were true that from their origin some rays of light are red, others blue etc., there would remain the great difficulty of explaining by the mechanical philosophy in what this diversity of colours consists'.[17] Huygens demands that a hypothesis be offered as to how differences in motion are connected with differences in colour, 'for until this hypothesis has been found, [Newton] has not apprised us what the nature of and difference between colours is, only the accident (which is certainly very considerable) of their different refrangibility'.[18] But Newton sees the matter in a very different way. He does not accept the idea that causes are restricted to what underlies the phenomena, and therefore necessarily at a different level from the phenomena: explanation does not necessarily have to take the form of identifying 'underlying' causes.

Boyle's attempts to come to terms with the phenomenology of colours, in his *Experiments and Considerations*, is crucial in Newton's thinking here. Just as Boyle had used the air pump to organise and focus his *explanandum* in accounting for the elasticity of air and the action of the lungs, so Newton proceeds in a parallel way, using an instrument (a prism) in a very particular experimental arrangement to organise and focus the *explanandum*. Like Boyle, he is criticised for the narrowness of his treatment—he does not repeat the experiment many times over, he does not take into account numerous other experiments on colours, nor does he offer a natural-philosophical explanation in terms of the nature of light—and as a result, his account met considerable resistance.[19]

Newton's starting point is a practical problem, inherited from Descartes. One of Descartes' primary aims in his geometrical optics of the 1620s had been to produce lenses that brought parallel rays to a single focus. The spherical lenses used in telescopes were unable to do this, with the result that the image was significantly distorted (spherical aberration). Applying his newly discovered sine law of refraction to lenses by accommodating their curvature in terms of a series of prisms, he realised

[17] Huygens to Oldenburg, 17 September 1672; Newton (1959–1977, vol. 1, pp. 235–236).
[18] Oldenburg to Newton, quoting a letter from Huygens, 18 January 1673; ibid., pp. 255–256.
[19] See Shapiro (1996, pp. 59–104) and Schaffer (1989, pp. 67–104).

that hyperbolic and elliptical lenses would refract rays to a single point. But grinding aspherical lenses was a very difficult matter, as Descartes was well aware. It was this problem of grinding 'Optic glasses of figures other than Spherical' to which Newton devoted attention in the years 1663–1665.[20] At the beginning of 1666, Newton procured a prism and began experimenting with it. The prism allows one to isolate the process of image formation through refraction: to have used a lens, by contrast, would have meant one would have to deal with multiple refractions because the curvature of the lens causes incident rays to enter it at different angles.

Newton describes what he did in these terms:

> I procured me a Triangular glass-Prisme, to try therewith the celebrated *Phænomena of Colours*. And in order thereto having darkened my chamber, and made a small hole in my window-shutts, to let in a convenient quantity of the Suns light, I placed my Prisme at his entrance, that it might thereby be refracted to the opposite wall. It was at first a very pleasing divertisement, to view the vivid and intense colours produced thereby; but after a while applying my self to consider them more circumspectly, I became surprised to see them in an *oblong* form; which, according to the received laws of Refraction, I expected should have been *circular*. They were terminated at the sides with streight lines, but at the ends, the decay of light was so gradual, that it was difficult to determine justly, what was their figure; yet they seemed *semicircular*.[21]

There is a significant contrast here between what Newton expected to see and what he did see. On the question of what he expected to see, the light entered the room in the form of a narrow beam through a small circular hole, so it is a circular beam that is refracted through the prism. In fact, a good deal depends on the angle at which it strikes the prism, and we would expect some elongation in most cases. However, Newton's optical lectures indicate that the angle at which the beam strikes the surface of the prism is what is called the position of minimum deviation.[22] If one were to rotate the prism in relation to the light source, there would be one orientation at which rays entering the prism would be refracted to the same degree as those leaving it, so that those parallel to one another before refraction, for example, will also be parallel after refraction, and this is the angle of minimum deviation. At such an angle, one would expect the light beam to retain its circular shape, especially if one treats beams of light

[20] See Hall (1948, pp. 239–250) and Hall (1955, pp. 27–43).

[21] Newton (1959–1977, vol. 1, p. 92).

[22] Newton (1984, pp. 53–59). See the very helpful account in Sepper (1994, ch. 3).

as if they were individual rays of the kind envisaged in geometrical optics.[23] Newton uses the angle of minimum deviation, but what he finds is a lozenge-shaped band which is about five times longer in length than in breadth.

He tests various possible explanations for this. One traditional explanation for the spectrum was that, because of the triangular shape of the prism, one side of the beam had to traverse a greater distance than the other, and hence the beam is disturbed or weakened more on one side than on the other, and this had led many natural philosophers to conclude that colours were a mixture of light and dark. Newton tests this by comparing the results of passing the light through the base, where the beam has to traverse the maximum distance, and near the apex, where it traverses a very short distance, only to find that the same spectrum is produced. Hence the amount of glass traversed by the beam is not an operative factor. Nor can the size of the hole through which the light passes be a factor since, he reports, changes to the size of the hole make no difference to the spectrum produced. Moreover, placing the prism outside the window so that it was refracted before passing through the hole in the shutters and entering the darkened room made no difference either.

Another possible explanation for the colours and the elongation is irregularities in the glass from which the prism is made. To test this, he takes two similar prisms, one upright and one upside down, and passes the beam through these. The thought is that 'the *regular* effects of the first Prisme would be destroyed by the second Prisme, but the *irregular* ones more augmented, by the multiplicity of refractions'.[24] But in fact what results is a colourless circular image, so irregularities cannot be the cause of the colour or the elongation. At this point, Newton begins calculating and measuring. The rounded edges of the spectrum suggest that it has been elongated from a circle. Measurement of the width of the image produced by an unrefracted beam shows it to be the same as that of the spectrum: it is simply a feature of the linear propagation of light.

What requires explanation, therefore, is the lengthening of the spectrum in a direction perpendicular to the refracting edge of the prism. One possibility is that the rays

[23] It is striking that Newton treats them in this way even after he is aware of, and has accepted, Römer's demonstration of the finite speed of transmission of light. In the *Opticks* (1704, p. 2) he writes: 'Mathematicians usually consider the Rays of Light to be Lines reaching from the luminous Body to the Body illuminated, and the refraction of those Rays to be the bending or breaking of those lines in their passing from one medium into another. And thus may rays and Refractions be considered, if Light be propogated in an instant. But, by an Argument taken from the Æquations of the times of the Eclipses of *Jupiter's Satellites*, its seems that Light is propagated in time, spending in its passage from the Sun to us about seven Minutes of time: And therefore I have chosen to define Rays and Refractions in such general terms as may agree to Light in Both cases'.

[24] Newton (1959–1977, vol. 1, p. 93).

coming from opposite ends of the sun enter the hole at different angles, and the sine law predicts that these will be refracted differently, but he calculates the difference to be very slight (about 31'), and by manipulating the prism around its axis, he shows that even a deviation of 4º or 5º makes no difference: the locations of the colours on the wall is unchanged. This suggests either that the sine law is flawed, or that something happens to the beam once it leaves the prism. One possibility is that the rays making up the beam diverge on leaving the prism. Consider, for example, the Cartesian model whereby the light globules acquire a degree of rotation in passing through the prism: Could not the differences in rotation cause the rays to bend? Newton remarks that he has

> often seen a Tennis ball, struck with an oblique Racket, describe such a curve line. For, a circular as well as a progressive motion being communicated to it by that stroak, its parts on that side, where the motions conspire, must press and beat the contiguous Air more violently than on the other, and there excite a reluctancy and reaction of the Air proportionately greater. And for the same reason, if the Rays of light should possibly be globular bodies, and by their oblique passage out of one medium into another acquire a circulating motion, they ought to feel the greater resistance from the ambient Æther, on that side, where the motions conspire, and thence be continually bowed to one another.[25]

But he can detect no curvature: the ratio between the length and breadth of the spectrum remains constant.

'The gradual removal of these suspitions', Newton writes in 1672, 'at length led me to the *Experimentum Crucis*'.[26] The role of the 'crucial experiment' in Newton's account varies, and it plays no role in his earlier report in the *Optical Lectures*, nor in later reports of the experiment, but it enables him here to bring out the kinds of considerations he considered would be decisive.[27] The new experiment is ingenious, and apparently straightforward, although, for various reasons, both good and bad, some critics were subsequently unable to reproduce it.[28] Two prisms and two boards with small

[25] Ibid., p. 94.

[26] Ibid., p. 94.

[27] The extent to which this experiment has been reconstructed and idealized by Newton in the letter, which was written five years after the original experiment, is discussed in Lohne (1968, pp. 169–199) and Shapiro (1996). See also Zemplén and Demeter (2010, pp. 640–656).

[28] Mariotte, in particular, claimed that he could not establish that colour is immutable as a result of a second refraction, although many of his problems with the experiment seem to have derived from the difficulty of producing well-separated violet rays, which Newton had in fact explicitly stated was a *sine qua non* of a successful result. On the adverse French reaction to the

holes in them are set up so that the sequence is: light source (hole in shutter), first prism, first board, second board, second prism, and the wall on which final images appear.[29] The boards and the second prism are in fixed position, whereas the first prism can be rotated to allow different parts of the spectrum to fall on the aperture in the second board. When violet light passes through this aperture, it is refracted by the second prism to a certain point on the wall; but when red light passes through the aperture, it is refracted by the second prism to a different point on the wall.

One important feature of this experimental setup is that the angle of incidence on the second prism cannot vary, because both it and the boards are fixed in place: only the first prism is allowed to move. What this means is that any difference in the position of the image on the wall can only be due to a difference in the refraction of the beam in the second prism. When he elaborates on the experiment many years later in the *Opticks* (1704), he notes that the images produced by the second prism are not elongated, but almost circular: this circular image moves across the wall as the colour produced by the refraction shifts from red to violet.

What Newton has done in this experiment is to isolate something that can display a fundamental feature of the behaviour of light when it is refracted through a prism. Refracted light behaves differently from unrefracted light: the iris displayed on the boards is very different from the image on the wall. The conclusion he draws is that the production of colour is not something that is due to a modification of light as it is refracted through the prism; rather, there must be components of the sunlight which behave differently, being refracted at slightly different angles along a continuous gradation from red to violet. This has immense significance for the construction of telescopes, as Newton notes,[30] for as well as the problem of correcting for the distortion produced by the wrong degree of curvature of the lens (the problem to which Descartes had directed his efforts), there was now the newly discovered problem that there will always be a small but significant difference between the refraction of the red and violet rays at the opposite ends of the spectrum, and at this point he shows how a reflecting telescope overcomes the problems of refracting telescopes in this respect.[31]

experimentum crucis, generally see Guerlac (1981, ch. 4). Newton was subsequently forced to offer detailed instructions as to types of glass, etc. See Schaffer (1989, pp. 85–91).

29 See Figure 3.1 of the present volume.

30 Newton (1959–1977, vol. 1, p. 95).

31 The solution was thought by some to lie in composite lenses. Euler claimed in 1748 that an achromatic combination should be possible, and there ensued a dispute between Euler and Newtonians, who denied this. In 1757 Dolland patented a new achromatic lense, but it had no rationale in terms of physical optics, relying on chemical properties of glass rather than physical properties. See Hutchison (1991, pp. 125–171).

What Newton has shown in his '*experimentum crucis*' is described by him, in a somewhat reconstructed and idealised way, in these terms:

> A naturalist would scarce expect to see ye science of those [colours] become mathematicall, & yet I dare affirm that there is as much certainty in it as in any other part of Opticks. For what I shall tell concerning them is not an Hypothesis but most rigid consequence, not conjectured by barely inferring 'tis thus because not otherwise or because it satisfies all phænomena (the Philosophers universall Topick,) but evinced by ye mediation of experiments concluding directly & without any suspicion of doubt.[32]

The first point here is that the treatment of colours is mathematical. Whereas Descartes had taken his geometrical optics and then shifted into a different register to account for it in physical terms, Newton remains at the level of geometrical optics. Colours, as Newton points out,[33] are produced in a variety of ways which cannot be accounted for in terms of anything that construes them in terms of mixtures of light and dark, or rotation of corpuscles. He notes for example that colours adjacent to one another in the spectrum can be mixed to produce an intermediate colour, but colours which are separated cannot be mixed in this way, yet when all the colours are mixed they produce white light; and that thin strips of the wood of the shrub *Lignum nephriticum* which have been soaked in water appear gold when refracting light, but blue when reflecting it. As well as those colour-producing phenomena with which Newton was familiar, namely refractive dispersion (as in diamonds), interference (soap bubbles), and fluorescence (*Lignum nephriticum*), there are others such as diffraction (feathers) and scattering (the sky), that make colour phenomena even more intractable.[34] It is worth noting here that Newton's focus in his account of his *experimentum crucis* is on the elongated shape of the spectrum rather than colour. The reason for this is that the elongation is subject to quantitative variation: Newton can keep it as part of an exercise in geometrical optics, thus ensuring that we do not leave the quantitative realm.[35] The

[32] Newton (1959–1977, vol. 1, pp. 96–77).

[33] Ibid., vol. 1, pp. 98–99.

[34] See Shapiro (1993, p. 99). Note also the problem of 'boundary colours', produced when lighter and darker objects which abut each other are viewed through a prism, producing a spectrum which lacks a green band and which is quite different from the normal one. Newton observed this spectrum: see McGuire and Tamny (1983, pp. 246–247).

[35] 'Colour' comes to stand in for refrangibility in Newton, but this is problematic. As Alan Shapiro (1980, pp. 215–216) notes: 'The problem of establishing the innateness and immutability of color is altogether different from that of refrangibility: first, Newton had no mathematical law to describe color changes; and second, the color of the sun's incident light appears totally different before the first refraction and ever after, once it has been resolved into colors. As Newton