

STEVEN

LEVIATHAN
HOBBS, ROYLE

INCLUDING A
DIALOGUE

QC166
.B47
1785
c.1

COPYRIGHT © 1985 BY PRINCETON UNIVERSITY PRESS
PUBLISHED BY PRINCETON UNIVERSITY PRESS
41 WILLIAM STREET, PRINCETON, NEW JERSEY 08540
IN THE UNITED KINGDOM: PRINCETON UNIVERSITY PRESS
CHICHESTER, SUSSEX

ALL RIGHTS RESERVED

LIBRARY OF CONGRESS CATALOGING IN PUBLICATION DATA
WILL BE FOUND ON THE LAST PRINTED PAGE OF THIS BOOK
ISBN 0 691-08299-5

THIS BOOK HAS BEEN COMPOSED IN LINOTRON BASKETBALL
GLOUDBOUND EDITIONS OF PRINCETON UNIVERSITY PRESS BOOKS ARE
PRINTED ON ACID-FREE PAPER, AND BINDING MATERIALS ARE
CHOSEN FOR STRENGTH AND DURABILITY. TAPERBACKS,
ALTHOUGH SATISFACTORY FOR PERSONAL COLLECTIONS,
ARE NOT USUALLY SUITABLE FOR LIBRARY BINDING.

PRINTED IN THE UNITED STATES OF AMERICA
BY PRINCETON UNIVERSITY PRESS
PRINCETON, NEW JERSEY

Every afternoon Father Nic
preaching in Latin, but José
rhetorical tricks and the tra
manded the daguerreotype
canor then brought him me
duction of the Veronica, but
as artistic objects without an
that Father Nicamor gave up
continued visiting him out o
was José Arcadio Buendía; w
down the priest's faith with re
when Father Nicamor brought
and invited him to a game, José
because according to him he
a contest in which the two adv
GAUREL GARCÍA MARQU

What a blessing to mankind, in
ingenious, humble, and pious
society was the fallacious, proud
we find the former banished to
honour, and hope; while the ot
an odium on his name, as well
an unknown future.

W. Dean, *The Beauties
Virtues and Vice Dramas
Eminent for Their
Their V*

Seeing and Believing

Seeing Double; Hol

The Trouble with

Boyle's Adve

Replication and Its

Natural Philosophy at

viii · CONTENTS

· VIII ·
The Polity of Science: Conclusions
332

· APPENDIX ·
Translation of Hobbes's *Dialogus physicus*
345

BIBLIOGRAPHY
393

INDEX
427

· II ·
Figure 1 Boyle's

Figure 2 Frontispiece showing

Figure 3 Vignette of Thomas Birch's edition

Figure 4 Frontispiece essays in

Figure 5 Portrait of

Figure 6 Francis Bacon's

Figure 7 Boyle's revision of Newton

Figure 8 Illustration of his exhausted air

Figure 9 Illustration of marbles

Figure 10 Map of distance

Figure 11 Huygens' figure

Figure 12 Huygens' diagram (De

Figure 13 Huygens' diagram (De

Figure 14 Huygens' experiment

Figure 15 Huygens'

- Figure 16a* William Faithorne's drawing of Boyle (1664). 258
- Figure 16b* William Faithorne's engraving of Boyle (1664), showing his original air pump. 259
- Figure 17* Revised Boyle air pump; enlargement of detail of figure 2. 260
- Figure 18* Huygens' diagram of second air-pump sent to Montmor (July 1668). 268
- Figure 19* Huygens' air pump at the Académie Royale des Sciences (May 1668). 270
- Figure 20* Huygens' revised air-pump at the Académie Royale des Sciences, depicting an imaginary visit by Louis XIV and Colbert (1671). 275
- Figure 21* Otto von Guericke's second pump (1664). 279
- Figure 22* Otto von Guericke's first pump (1657). 335

for citations of sources in a convention similar to the *Printing Press as an Agent* kept to a minimum in the of date of publication where text and is germane. Full in the Bibliography. Co-sources, seventeenth-century and parliamentary papers repeated in the Bibliography.

We have made liberal not published in the seven been with knowledge that has affected the extent of interested in material intended to be restricted material is corresponding.

During the period with Isles employed a calendar continental countries, especially Julian (old style) calendar (new style) calendar etc. British new year was recorded in some detail with mental countries, we give but we adjust years to our January. Thus, the English 1662; the Dutch (who Protestant) 24 July 1664.

We have endeavoured century orthography, preferred with *sic* indications.

In our usage, "Hobbes" Hobbes as an individual his real or alleged follower sent (upper case) and in

permission to use this material. For permission to quote from manuscripts in their care we should like to thank the Syndics of Cambridge University Library and the Trustees of the British Library. For permission to reproduce pictorial material in their keeping, we thank the National Portrait Gallery, London (figure 5); the Sutherland Collection of the Ashmolean Museum, Oxford (figure 16); Cambridge University Library (figures 17, 20, 21, and 22); the British Library (figures 2 and 4); and Edinburgh University Library (figures 1, 2, 6, 7, 8, 9, 11, 12, 13, 14, 15, 18, 19, and the diagram in the translation of the *Dialogus physicus*). For permission to use the epigraph to chapter 1, we thank the holders of the original, copyright to Umberto Eco's *The Name of the Rose*: Gruppo Editoriale Fabbri, Bompiani, Sonzogno, Einaudi S.p.A., Milan (American edition published by Harcourt Brace Jovanovich).

January 1985
Aldbrough, Derbyshire

Unders

Ab

William of Baskerville

UMBERTO

Our subject is experime
status of experimental

These are the question
experiment? How is an
means by which exper
fact, and what is the rel
explanatory construct?
and how is success disti
kind this series of partic
does one do experimen
experiment a privileged
knowledge of nature, or
results the experimental

We want our answers
we will deal with the his
as a systematic means o
which experimental pr
which experimentally pr
foundations of what co
start, therefore, with th
dure: Robert Boyle's res
of the air-pump in that

Boyle's air-pump exp
ence texts, in science pe

the history of science. Of all subjects in the history of science it might be thought that this would be the one about which least new could be said. It is an oft-told tale and, in the main, a well-told tale. Indeed, there are many aspects of Boyle's experimental work and the setting in which it occurred that have been sufficiently documented and about which we shall have little novel to say; our debt to previous historical writing is too extensive to acknowledge adequately. It is entirely appropriate that an excellent account of Boyle's pneumatic experiments of the 1660s constitutes the first of the celebrated series of *Harvard Case Histories in Experimental Science*.¹ This thirty-five-year-old study admirably establishes our point of departure: it shows that Boyle's air-pump experiments were designed to provide (and have since provided) a heuristic model of how authentic scientific knowledge should be secured.

Interestingly, the Harvard history has itself acquired a canonical status: through its justified place in the teaching of history of science it has provided a concrete exemplar of how to do research in the discipline, what sorts of historical questions are pertinent to ask, what kinds of historical materials are relevant to the inquiry, what sorts are not genuine, and what the general form of historical narrative and explanation ought to be. Yet it is now time to move on from the methods, assumptions, and the historical programme embedded in the Harvard case history and other studies like it. We want to look again at the air-pump experiments, to put additional questions to these materials and to rephrase traditional questions. We did not initiate our project with a view to criticizing existing accounts of Boyle's experimental work. In fact, at the outset we were doubtful that we could add much to the work of distinguished Boyle scholars of the past. Yet, as our analysis proceeded, we became increasingly convinced that the questions we wished to have answered had not been systematically posed by previous writers. Why not?

A solution might reside in the distinction between "member's accounts" and "stranger's accounts." Being a member of the culture one seeks to understand has enormous advantages. Indeed, it is difficult to see how one could understand a culture to which one was a complete stranger. Nevertheless, unreflective membership also carries with it serious disadvantages to the search for understanding, and the chief of these might be called "the self-evident

¹ Conant, "Boyle's Experiments in Pneumatics"; idem, *On Understanding Science*, pp. 29-64.

method."² One reason for searchingly pressed the mental practices is that it accounts endorsed by the method the presuppositions are not regarded as pre-ordinarily, our culture's both ambiguous facts of nature of how people just do things ally"). A lay member of a bird, will probably tell or he will point to unpro of classification by which this lay member will thi bear upon a culture that In the case of experim particularly noticeable in why this should be the in identifying Boyle as which scientists now live the assumption that the with Robert Boyle, and t and the seventeenth-ce The historical career of support of this assump alternatives and objectio rapidly, largely aided an licity of the Royal Socie mental programme is c Even so, the usual way itself in historical practi

² See, for example, Douglas

³ A classic site for relativist world is Balzer, "Why is the asymmetrical: only cultures th curiosity. For asymmetrical re Marx Revised"; idem, Keos "Relativism, Rationalism and

⁴ For a powerful nineteenth century discourse on the study of N century examples, see L. T. M universally accepted, diverge immediately proclaimed as the

claims about the rise, acceptance, and institutionalization of experiment, but as a disposition not to see the point of putting certain questions about the nature of experiment and its status in our overall intellectual map.

The member's account, and its associated self-evident method, have great instinctive appeal; the social forces that protect and sustain them are powerful. The member who poses awkward questions about "what everybody knows" in the shared culture runs a real risk of being dealt with as a troublemaker or an idiot. Indeed, there are few more reliable ways of being expelled from a culture than continuing seriously to query its taken-for-granted intellectual framework.¹ Playing the stranger is therefore a difficult business; yet this is precisely what we need to do with respect to the culture of experiment. We need to *play* the stranger, not to *be* the stranger. A genuine stranger is simply ignorant. We wish to adopt a cultivated and an informed suspicion of our taken-for-granted perceptions of experimental practice and its products. By playing the stranger we hope to move away from self-evidence. We want to approach "our" culture of experiment as Alfred Schutz suggests a stranger approaches an alien society, "not [as] a shelter but [as] a field of adventure, not a matter of course but a questionable topic of investigation, not an instrument for disentangling problematic situations but a problematic situation itself and one hard to master."² If we pretend to be a stranger to experimental culture, we can seek to appropriate one great advantage the stranger has over the member in explaining the beliefs and practices of a specific culture: the stranger is in a position to know that there are alternatives to those beliefs and practices.³ The awareness of alternatives and the pertinence of the explanatory project go together.

Of course, we are not anthropologists but historians. How can the historian play the stranger to experimental culture, a culture we are said to share with a setting in the past and of which one of our subjects is said to be the founder? One means we can use is

¹ See the "experiments" of Harold Garfinkel, on questioning taken-for-granted rules of social interaction: *Studies in Ethnomethodology*, esp. chap. 2.

² Schutz, *Collected Papers*, Vol. II, p. 104.

³ The relative advantages of the member's and stranger's perspective have been debated by sociologists and taking participant observation of modern science. Latour and Woolgar, *Laboratory Life*, chap. 1, are wary of the methodological dangers of identifying with the scientists they study, whereas Collins, "Understanding Science," esp. pp. 272-274, argues that only by becoming a competent member of the community under study can one credibly test one's understanding.

the identification and explication. Historical instances of intellectual practices offer us the opportunity of entities or propriety subsequently taken to be metaphor, institutionalized the ship in the bottle, and offer us the opportunity of sticks and string, and the advantage afforded by a frequently play a role in the course of controversy-granted quality of the practices, and they do this by conventional status of those participants in controversy-stranger. It would, of course, simply to appropriate a entific controversy, and found it valuable to note strategies employed by both participants' accounts, and interpretative work: the

The controversy with Boyle in the 1660s and 1670s (1627-1681) and appears as the major practical one of the most important experimental practices in nature Boyle's most vigorous particular claims and searches and, crucially, experimental program Boyle recommended. The Hobbes-Boyle disputes a historian to analyze. One of Hobbes as a natural philosopher Kargon rightly says that important mechanical phil

⁴ Collins, "The Seven Senses"

along with Descartes and Gassend.¹⁹ There is no lack of evidence of the seriousness with which Hobbes's natural philosophical views were treated in the seventeenth century, especially, but not exclusively, by those who considered them to be seriously flawed. We know that as late as the early eighteenth century Hobbes's natural philosophy attracts formed an important component of the Scottish university curriculum.²⁰ Yet by the end of the eighteenth century Hobbes had largely been written out of the history of science. The entry on Hobbes in the 1797 third edition of the *Encyclopaedia Britannica* scarcely mentions Hobbes's scientific views and totally ignores the tracts written against Boyle. Much the same is true of the *Encyclopaedia's* 1842 *Dissertation on the History . . . of Mathematical and Physical Science*: Hobbes is to be remembered as an ethical, political, psychological, and metaphysical philosopher; the unity of those concerns with the philosophy of nature, so insisted upon by Hobbes, has been split up and the science dismissed from consideration. Even Mintz's article on Hobbes in the *Dictionary of Scientific Biography* is biased heavily towards his moral, political, and psychological writings.²¹ Fortunately for us, since Brandt's 1928 monograph on Hobbes's mechanical philosophy, this situation has begun to improve. Our indebtedness to recent work on Hobbes's science by scholars such as R. H. Kargon, J. W. N. Watkins, Alan Shapiro, Miriam Reik, and Thomas Spragens will be evident in what follows. Nevertheless, we are still very far from appreciating Hobbes's true place in seventeenth-century natural philosophy, and, if this book stimulates further research, one of its functions will have been fulfilled.

Kargon suggests that one of the reasons for the neglect of Hobbes by historians of science lies in the fact that he disagreed with the hero Boyle and, accordingly, suffered ostracism from the Royal Society of London.²² There is no doubt that Hobbes's scientific controversies in England, all of which his contemporaries considered he decisively lost, have much to do with his dismissal by historians. Within the tradition of "Whig" history, losing sides have little interest, and in no type of history has this tendency been more

apparent than in classic accounts of the seventeenth century concerned with Hobbes's mathematical disputes. These accounts cannot treat in any detail the disputes with Boyle, but have disappeared from the light with Boyle. In the *Encyclopaedia Britannica's* 1797 entry, Hobbes's "mathematical disputes"; CRONIN, *Robert Boyle*; and the 1928 edition of the *Encyclopaedia Britannica's* entry on Boyle, no historian dissents.²³

The situation is similar in the case of the controversies with Boyle's natural philosophy. In the history of science disputes, and even the history of philosophy, and even the history of ideas. For example, one of the most recent treatments of Boyle's natural philosophy, Aristotelian *horror vacui* sensitive, writer has argued that Boyle's natural philosophy for experimentation in the history of science. It pains to show to be wrong. For these errors, and the controversies, is difficult to determine, only two historians have opened the crucial text, *Dialogus physicus de vacuo*

¹⁹ The Whiggish tendency and Lucas is briefly noted in

²⁰ Stephen, "Hobbes," esp. pp. 249-250; "Hobbes," esp. pp. 249-250; esp. pp. 18-21, 40-41. See also geometry and the controversy "Hobbes: Geometrical Objects Revisited," *Les mathématiques* *The Mathematical Work of W.*

²¹ For the *horror vacui* claim p. 48; for a note pointing to

²² Watkins, *Hobbes's System*, vol. 1.

²³ The exceptions are G. Galilei's test Hobbes's Boyle's fifth advanced stage of Hobbes's natural philosophy. I do not see the *Dialogus* as development with the plenist physical writings (notably *De corpore* 271-278). But Gargani asks, c

¹⁹ Kargon, *Atomism in England*, p. 71.

²⁰ Shepherd, "Newtonianism in Scottish Universities," esp. p. 70; *idem*, *Philosophy and Science in the Nineteenth Century*, pp. 8, 11f., 151, 187, 215-217.

²¹ Anon., "Hobbes"; Mackintosh, "Dissertation Second," pp. 310-322 (on ethical philosophy); Playfair, "Dissertation Third" for mathematical and physical science, where Hobbes is scarcely mentioned at all; Mintz, "Hobbes."

²² Kargon, *Atomism in England*, p. 54.

has never been translated from the Latin original, and this may go some way to explain its neglect. (To remedy this state of affairs, we offer an English translation, by Schaffer, as an appendix to this book.) With these two exceptions, historians have been content to align themselves with the victorious Boyle and his associates, to repeat Boyle's judgment on Hobbes's text, and to keep silent about what Hobbes actually had to say. Even Brandt, who wrote the most detailed study of Hobbes's science, declined engagement with the *Dialogus physicus* and later natural philosophical texts. Brandt, too, accepted Boyle's evaluation of Hobbes's views:

We will not examine the works subsequent to *De Corpore* [of 1655, six years before the *Dialogus physicus*]. . . . No less than three times during these years Hobbes took up his physics for further elaboration . . . but it retains exactly the same character as the physics of *De Corpore*. This character becomes especially conspicuous in Hobbes' attack on Boyle's famous "New Experiments touching the Spring of the Ayre." Here again Hobbes shows how little he understands the significance of the experiment. In spite of the continual experiments on vacuity,

and pays no attention to the actual text or to the attack on Boyle's air-pump programme. Lupoli gives a full and valuable exposition of Boyle's response to Hobbes in the *Essays*. He places the controversy in the context of the earlier polemical trials in Italy and France in the 1640s, notably the Pascal-Neile debate. Lupoli suggests that Hobbes attacked Boyle because of his "disappointment at being excluded from the new scientific association, but above all the disillusion and preoccupation with seeing his foundation of physical science ignored" (p. 324). Lupoli highlights Boyle's priority as a response to Hobbes's attack on the "rhetoric of ingenuity," and Boyle's tactic of point by point refutation of empirical claims as a means of avoiding a direct confrontation with Hobbes's whole physical programme (p. 325). But Lupoli is much more interested in Boyle's asserences on method and on experimental philosophy, and does not give any detailed account of the sources of Hobbes's own polemic. We are grateful to Agostino Lupoli for a copy of his paper (received after our manuscript was written); it is the only source we have found that cites the *Dialogus* in detail. Other major recent sources for Hobbes's natural philosophy do not treat the controversy with Boyle in any detail, nor do they examine the contents of Hobbes's *Dialogus physicus*; see, for example, Spragens, *The Politics of Atom*, esp. chaps. 2; Reik, *The Golden Age of Hobbes*, chap. 7; Goldsmith, *Hobbes's Science of Politics*, chap. 2, although each of these is valuable in other connections. In addition, there are many allusions to Hobbes's science by mainstream Hobbes scholars. They have tended to mine his philosophy of nature because of the generally high evaluation that historians of ideas have placed upon the significance of Hobbes's political and psychological theories and because of their conviction that there must be an overall pattern in his thought. Historians of science, given their low evaluation of Hobbes's natural philosophy and mathematics, have not tended to search for such a pattern.

in spite of the invention to his view of the futuristic. He did not we English empirical sci And when the method perimental method o keep abreast of them

Here we see the germ dealing with the Hobbes dling rejected knowledge diments of a causal exp implicitly acts to justify dling of rejected and a that the rejected knowle the historian accomplish and using the victorious series' position as the b disposed of error, so th L. T. More notes that H of nonsense," and quote what Hobbes's position by saying that "Boyle d guments and very grace burst."¹¹ John Laird con criticisms [of Hobbes] a examine much of Hobbes claims that Hobbes's cri . . . who had himself don best way of seeking to c and value of experimen

¹⁰ Brandt, *Hobbes's Metaphysics*.

¹¹ For alternative sociologic see the contributions to Walker, *Frames of Meaning*.

¹² L. T. More, *Life of Boyle*, p. 105; but even less so say about

¹³ McKie, "Introduction," p. 19.

¹⁴ Laird, *Hobbes*, p. 117.

¹⁵ Peters, *Hobbes*, p. 40.

¹⁶ R. S. Jones, *Antients and* p. 117. Hobbes "Failed to ap deciding any question of nat

tionians go further in wiping the historical record clean of significant opposition to the experimental programme: Marie Boas Hall, though without mentioning Hobbes by name, says that "No one but a dedicated Aristotelian" (which Hobbes most certainly was not) "could fail to find Boyle's arguments powerful and convincing."²⁵ and Barbara Shapiro, in her admirable account of English empiricism and experimentalism, concludes that "Except for a tiny group of critics who joked fun at the virtuosi" (whose names she does not mention), "there was no serious opposition to the new philosophy."²⁶

Pervasively, historians have drawn upon the notion of "misunderstanding" (and the reasons for it) as the basis of their causal accounting and dismissal of Hobbes's position. The *Harvard Case Histories* relate that Hobbes's arguments against Boyle "were based in part on a misunderstanding of Boyle's views."²⁷ M. A. Stewart refers to Boyle's pneumatics as leading "Hobbes into ill advised controversy on matters he did not understand."²⁸ Leslie Stephen and Crook Robertson both attempt to explain Hobbes's misunderstanding by referring to factors that distorted his judgment or made him unfit to appreciate the validity of Boyle's programme: he was ill-qualified in mathematics and physics; he was too old and rigid at the time of his controversies with Boyle; he was temperamentally obstinate and dogmatic; he had ideological axes to grind.²⁹ (To the best of our knowledge no historian has ever suggested that Boyle may have "misunderstood" Hobbes.)

Since our way of proceeding will dispense with the category of "misunderstanding" and the asymmetries associated with it, some words on method are indicated here. Almost needless to say, our purpose is not evaluative: it is descriptive and explanatory. Nevertheless, questions relating to evaluation do figure centrally in this book, and they do so in several ways. We have said that we shall be setting out by pretending to adopt a "stranger's perspective" with respect to the experimental programme: we shall do this be-

²⁵ M. B. Hall, "Boyle," p. 279. Her *Boyle and Seventeenth-Century Chemistry* makes no mention of the Boyle-Hobbes dispute; cf. Burt, *Metaphysical Foundations of Modern Science*, p. 169.

²⁶ B. Shapiro, *Possibility and Certainty*, p. 79; cf. p. 68.

²⁷ Grant, "Boyle's Experiments in Pneumatics," p. 49.

²⁸ Stewart, "Introduction," p. xvi. Hobbes's "misunderstanding" of Boyle even crops into accounts written for young people; see Kusler and Stom, *Boyle: The Great Experimentator*, p. 26.

²⁹ Stephen, "Hobbes," p. 97; Robertson, "Hobbes," p. 58.

cause we have set ourselves experimental practices and these were considered to be the same exercise we shall be taking account" of Hobbes's attitude to put ourselves in a just programme seem plausible; rather, we shall be offering a point of view.³⁰ Our purpose is to resuscitate his scientific programme, which has been seriously undermined by an aura of self-evidence surrounding knowledge, and a tradition of experimentalism. Of course, our ambition is not to rewrite history: Hobbes's views were philosophical communications, not self-evident or inevitable in that context, which yields in favour of the experimental programme bearing upon that philosophy. We will have found a different method or believed—but then, to be correct—but there were different evaluations. Hobbes's criticisms were not aspects of Boyle's position and even sloppy. If the standards of present-day both Hobbes and Boyle's experiment of Boyle's experiment of convention, of practice, and positive evaluation of identity those features of intellectuals' decisions that such agreement was necessary for experimental knowledge to be preferred over alternatives.

Far from avoiding questions of method, we will be con-

³⁰ Gettier, "Concepts and Ap-

shall be treating them in a manner slightly different from that which characterizes some history and much philosophy of science. "Truth," "adequacy," and "objectivity" will be dealt with as accomplishments, as historical products, as actors' judgments and categories. They will be topics for our inquiry, not resources unreflectively to be used in that inquiry. How and why were certain practices and beliefs accounted proper and true? In assessing matters of scientific method we shall be following a similar path. For us, methodology will not be created solely as a set of formal statements about how to produce knowledge, and not at all as a determinant of intellectual practice. We shall be intermittently concerned with explicit verbal statements about how philosophers should conduct themselves, but such method-statements will invariably be analyzed in relation to the precise setting in which they were produced, in terms of the purposes of those making them, and in reference to the actual nature of contemporary scientific practice.¹⁴ More important to our project is an examination of method understood as real practical activity. For example, we shall devote much attention to such questions as: How is an experimental matter of fact actually produced? What are the practical criteria for judging experimental success or failure? How, and to what extent, are experiments actually replicated, and what is it that enables replication to take place? How is the experimental boundary between fact and theory actually managed? Are there crucial experiments and, if so, on what grounds are they accounted crucial? Further, we shall be endeavouring to broaden our usual appreciations of what scientific method consists of and how method in natural philosophy relates to practical intellectual procedures in other areas of culture and in the wider society. One way we shall try to do this is by situating scientific method, and controversies about it, in a social context.

By adducing "social context" it is routinely understood that one is pointing to the wider society, and, to a very large extent, we shall be concerned to show the connections between the conduct of the natural philosophical community and Restoration society in general. However, we also mean something else when we use the term "social context." We intend to display scientific method as crystallizing focus of social organization and as a means of regularizing social interaction within the scientific community. To this end, we

¹⁴ For examples of empirical studies which assess method-statements in these terms, see P. H. Wood, "Methodology and Apologetics"; Miller, "Method and the Microphysics of Science"; Yeo, "Scientific Method and the Image of Science."

will make liberal, but in a "language-game" and a scientific method as integrated with Wittgenstein "the term 'language-game' signifies the fact that the sense of a form of life," so that a method as disputes over organizing men to practice to the problem of knowledge solutions to the problem of practical solutions to the Hobbes-Boyle controversy.

It will not escape our notice to the sociology of scientific possibility of the sociology the job of doing the thing follows from our decision references to the theoretical that has been a major project. Nevertheless, we does bear sufficient. Our methodological debates and they are too deep edged. Among Hobbes J.W.N. Watkins (for his the natural and civic good on the issue of Hobbes's Skinner (for aspects of I from him over Hobbes's historians of science we studies of the actual particularly in mind the wo The particular orientatio judgment that we have fo

¹⁵ Wittgenstein, *Philosophical Investigations*, 17.81. Block, *Wittgenstein*, chap. similarities with Wittgenstein's of his stress on the primacy of socially. *The Archaeology of Knowledge*.

¹⁶ The present state of the science is examined in Shapiro, "Hist-

of British and French micro-sociologists of science: H. M. Collins, T. J. Pinch, Bruno Latour, and Andrew Pickering, and from the pioneering Ludwik Fleck.

Since these debts are obvious and evident, it may be of some interest to acknowledge two pieces of empirical history whose connection with our own project may be less readily apparent, but which exemplify similar orientations to those employed here. John Keegan opens his magnificent study of the history of battle with the following confession:

I have not been in a battle, not near one, nor heard one from afar, nor seen the aftermath. . . . I have read about battles, of course, have talked about battles, have been lectured about battles. . . . But I have never been in a battle. And I grow increasingly convinced that I have very little idea of what a battle can be like.²⁴

It is a graceful admission of an ignorance that Keegan recognized in himself as a teacher at Sandhurst and in many military historians. Without this recognition, Keegan would have been unable to write the vivid and moving history that he ultimately produced. As we began the research for this book, we felt ourselves to be in a position similar to Keegan's. We had read much about experiments; we had both even performed a few as students; but we did not feel that we had a satisfactory idea of what an experiment was and how it yielded scientific knowledge. The parallel with Keegan's account of battle extends even farther. Keegan identifies a dominant variety of military history, shaped by Count von Moltke, which he refers to as "General Staff History." In General Staff History, what is of overarching significance is the role of the generals, their strategic planning, their rational decision-making, and their influence on the ultimate course of the battle. What is systematically left out of General Staff History is the contingency and the confusion of actual combat, the role of small groups of soldiers, the relationship between battle on the ground and the planning of the generals. It would not be a flight of fancy to recognize in General Staff History a family resemblance to "rational reconstructionist" tendencies in the history and philosophy of science. The von Moltkes of the history of science have shown similar disinclinations to engage with actual scientific practice, preferring idealizations and simplifications

²⁴ Keegan, *The Face of Battle*, p. 17; see also Keegan's more detailed account of a World War II series of battles, *Six Armies in Norway*.

to messy contingencies, standard conventions, references to independent criteria of scientific truth by real scientific actors.²⁵ Keegan's contribution to the history of experimental science is attributed to military historians, but the same historiographic entanglements are at work.

Our other unexpected connection is with our own objects of study. Unfortunately for us, Albert Alpert's work was substantially completed before we engage with it as extensively as we do. The parallels with our project are so brief that I can only briefly to point them out. Alpert's work is a fine art in the seventeenth-century sense of the word. To understand the assumptions of Alpert's work is to understand the assumptions of the scientific revolution, of the scientific painting and the construction of the modern world. She writes: "It was a century that finding and our crafting of it, are products of the same assumptions spread across the globe: the experimental projects, the experimental method, and that they were practiced in England. Both Dutch and English empiricist science involved a new way of thinking. This I mean a culture that through the mind's narrow knowledge was to be rationalized."

²⁵ The deep-rooted bias against the history of science has been noted by Alpert in *The Incomplete Scientist*, p. 1. Even the practice and pro-theory prejudice is noted in *Interpreting*, chap. 9, esp. pp. 149-150. Alpert always writes as a history of the philosophy of theory that the philosophy of theory that the experiments has been denied.

²⁶ Alpert, *The Art of Describing*, which resonates to the same extent as Alpert's *Painting and Experience*. Keegan's *The Face of Battle* is also a fine work.

²⁷ Alpert, *The Art of Describing*, a survey of the development of military science, esp. chap. 3.

and the art of the experimentalist, was, therefore, to make representations that reliably imitated the act of unmediated seeing.

There are two points in Alpers' account of special interest to us. One is the contrast she draws between Northern (and particularly Dutch) conceptions of the picture and those characteristic of Italian painting. In the latter the painting was conceived primarily as a gloss on a text; in the former the textual meaning of the picture was dispensed with in favour of direct visual apprehension of natural reality. Although the details of the contrast cannot concern us here, Alpers concludes that different theories of picturing expressed different conceptions of knowledge: the text versus the eye. The parallel between the Hobbes-Boyle controversy, and its underlying conflict over theories of knowledge, is far from exact; nevertheless, in the case of conflicts over the propriety of experimental methods we see a quite similar dispute over the reliability of the eye, and of witnessing, as the basis for generating and warranting knowledge. Seriously, Alpers adopts what we have termed a "stranger's perspective" to the nature of realist images. Their "mirroring" of reality is treated as the product of *convention* and of *craft*: "To appear lifelike, a picture has to be carefully made." The craft of realist representation is predicated upon the acceptance of Hooke's conventions for making realist statements in science: the "sincere hand" and the "faithful eye."¹⁸ With the acceptance of this convention for knowledge, and with the execution of the craft of representation, the artful nature of making representations disappears, and they acquire the status of mirrors of reality. Our project, therefore, is the same as Alpers': to display the conventions and the craft.

In the following chapter we examine the form of life that Boyle proposed for experimental philosophy. We identify the technical, literary, and social practices whereby experimental matters of fact were to be generated, validated, and formed into bases for consensus. We pay special attention to the operation of the air-pump and the means by which experiments employing this device could be craft to yield what counted as unassailable knowledge. We discuss the social and linguistic practices Boyle recommended to experimentalists; we show how these were important constitutive elements in the making of matters of fact and in protecting such facts from items of knowledge that were thought to generate discord

¹⁸ Alpers, *The Art of Describing*, pp. 72-73 (quoting Robert Hooke's *Micronographia* [1665], x, 227).

and conflict. Our task here is to display the experimental knowledge

In chapter 3 we discuss philosophy before the year 1660. Our major subject is philosophy and as epistemology. *Leviathan* was designed in order in the state. That was being, threatened themselves a share of civil. Their major resources in to Hobbes, a false ontology, deavoured to show the corporeal substances and ontology, and, in the knowledge in which the causes, and those causes entitled to the name of philosophy. And, crucially, its character. Assen was

Hobbes's philosophy, was already in place when came public in the year of Boyle's radical proposals for the framework of the air-pump. He argued that Boyle's air-pump, and that, therefore, its explanation of Boyle's explanation of Boyle's operational vacuum, was air. Plenist accounts of Hobbes attacked Boyle's of science on the vacuum epistemological hypothesis of matters of fact, the foundations of knowledge, the physical causes that accounted to the assertion of Boyle's programme was, it was not, and, as such, secure

partial assent at which Boyle aimed. Hobbes's assault identified the conventional nature of experimental facts.

In chapter 2 we show how Boyle replied to Hobbes and to two other adversaries in the 1660s: the Jesuit Franciscus Linus and the Cambridge Platonist Henry More. By examining the different nature and style of Boyle's responses, we identify that which Boyle was most concerned to protect: the air-pump as a means of generating legitimate philosophical knowledge and the integrity of the rules that were to regulate the moral life of the experimental community. Boyle treated Hobbes as a failed experimentalist rather than as someone proposing a quite different way of constituting philosophical knowledge. He used the opportunities provided by all three adversaries to exhibit how experimental controversy could be managed, without destroying the experimental enterprise itself—indeed, to show how controversy could be used to buttress the factual foundations of experimental knowledge.

In chapters 3, 4, and 5 we discuss the central role of the air-pump in the experimental programme and how critics might use imperfections in its working to attack experiment itself. In chapter 5 we attempt to do two things. First, we look at how the pump itself evolved as a material object in the 1660s, arguing that these changes embodied responses to earlier criticisms, especially those offered by Hobbes. We uncover information about the small number of pumps that were successfully built in that decade, and we show that, despite Boyle's reporting practices, no one was able to build a pump and make it operate without seeing the original. This poses problems of replication of greater interest than historians have previously recognized. Replication is also central to the second task of this chapter. In chapter 2 we argue that the constitution of matters of fact involved the multiplication of witnesses, and that Boyle exerted himself to encourage the reiteration of his experiments. However, shortly after the *New Experiments* appeared, another philosopher, Christiaan Huygens in the Netherlands, produced a finding (the so-called anomalous suspension of water) that seemed to invalidate one of the most important of Boyle's explanatory resources. We examine how this important anomaly was treated, and we conclude that the successful working of the air-pump was calibrated by previous commitments to whether or not such a phenomenon could exist. We analyze response to anomaly as a manifestation of the experimental form of life and of the conventions employed in the experimental community to protect itself from fatal internal discord.

Boyle's experimental both offered as solution attempt to locate solution Restoration debate over in society. This debate programmes for the produced. We seek to show the history of natural thought and action. On natural philosophy in of drawing it from counter paired, the community legitimacy in Restoration to guaranteeing order. Union (Hobbes's) demands creating a demonstration between the natural, the for no dissent within it.

In the concluding chapter of this study for the history. We argue that the problem is a problem in politics, and order always involves s

. II .

Seeing and Believing: The Experimental Production of Pneumatic Facts

... *Facts are things that minus they,
Are known to be disputed.*

ROBERT BOYLE: A DREAM

ROBERT Boyle maintained that proper natural philosophical knowledge should be generated through experiment and that the foundations of such knowledge were to be constituted by experimentally produced matters of fact. Thomas Hobbes disagreed. In Hobbes's view Boyle's procedures could never yield the degree of certainty requisite in any enterprise worthy of being called philosophical. This book is about that dispute and about the issues that were seen to depend upon its resolution.

Hobbes's position has the historical appeal of the exotic. How was it possible for any rational man to deny the value of experiment and the foundational status of the matter of fact? By contrast, Boyle's programme appears to exude the banality of the self-evident. How could any rational man think otherwise? In this chapter we intend to address the problem of self-evidence by dissecting and displaying the mechanisms by which Boyle's experimental procedures were held to produce knowledge and, in particular, the variety of knowledge called "matters of fact." We will show that the experimental production of matters of fact involved an immense amount of labour, that it rested upon the acceptance of certain social and discursive conventions, and that it depended upon the production and prohibition of a special form of social organization. The experimental programme was, in Wittgenstein's phrases, a "language-game" and a "form of life." The acceptance or rejection of that programme amounted to the acceptance or rejection of the form of life that Boyle and his colleagues proposed. Once this point is made, neither the acceptance of the experimental programme nor the epistemological status of the matter of fact ought to appear self-evident.

In the conventions of is no item of knowledge our ways of making sense their place in our overpotheses, and our metaphers of fact stand under reject particular matters adds solidity to the category theory; there are "given" currently regarded as in any longer believes to be of fact, we take away it was a matter of fact at a

There is nothing so given as in the philosophy of matters of fact reside in coming to be. Human and human agents then fact are regarded as the ideal novel, matters of holding a mirror up to but what nature makes of human agency in the identify the possibility of our natural reality is the irrevocable assent.

Robert Boyle sought naturally generated matter knowledge much less so portaut actors in the so wards a probabilistic and knowledge. Before the Shapiro have shown, the were rigidly distinguished former one could expect exemplified by logic and had been to model the

For a discussion of the history and the task of philosophy, pp. 149ff.

* Hacking, *The Emergence of Certainty*, esp. chap. 2.

demonstrative sciences and to attain to the kind of certainty that compelled absolute assent. By contrast, English experimentalists of the mid-seventeenth century and afterwards increasingly took the view that all that could be expected of physical knowledge was "probability," thus breaking down the radical distinction between "knowledge" and "opinion." Physical hypotheses were provisional and revisable; assent to them was not obligatory, as it was to mathematical demonstrations; and physical science was, to varying degrees, removed from the realm of the demonstrative. The probabilistic conception of physical knowledge was not regarded by its proponents as a regrettable retreat from more ambitious goals; it was celebrated as a wise rejection of a failed project. By the adoption of a probabilistic view of knowledge one could attain to an *appropriate* certainty and aim to secure *legitimate* assent to knowledge-claims. The quest for necessary and universal assent to physical propositions was seen as inappropriate and illegitimate. It belonged to a "dogmatic" enterprise, and dogmatism was seen not only as a failure but as dangerous to genuine knowledge.

If universal and necessary assent was not to be expected of explanatory constructs in science, how then was proper science to be founded? Boyle and the experimentalists offered the matter of fact as the foundation of proper knowledge. In the system of physical knowledge the fact was the item about which one could have the highest degree of probabilistic assurance: "moral certainty." A crucial boundary was constructed around the domain of the factual, separating matters of fact from those items that might be otherwise and about which absolute, permanent, and even "moral" certainty should not be expected. In the vast metaphor of the mechanical philosophy, nature was like a clock: man could be certain of the hour shown by its hands, of natural effects, but the mechanism by which those effects were really produced, the clockwork, might be various.² In this chapter we shall examine the means by which the experimental matter of fact was produced.

² The usual form in which Boyle phrased this was that God might produce the same natural effects through very different causes. Therefore, "it is a very easy mistake for men to conclude that because an effect may be produced by such determinate causes, it must be so, or actually is so." Boyle, "Usefulness of Experimental Natural Philosophy," p. 45; see also Laudan, "The Clock Metaphor and Probabilism"; Rogers, "Descartes and the Method of English Science"; van Leeuwen, *The Problem of Certainty*, pp. 23-96; B. Sklar, *Probability and Certainty*, pp. 44-61.

THE MECHANICS OF

Boyle proposed that negotiation of individuals' *habitu* had mutually to assure empirical experience was one of the process of habit to oneself, and assuring adequate. In that process experience was fundamental. experimental performance adequate to make a matter extended to many, and it be constituted as a matter to be seen as both an epistemological and foundational item of experimental communication and whatever sustain and enhance our

We will show that the experimental program *technology* included in the pump, a literary technology, induced by the pump were witnessed; and a social technology, experimental philosophy, and considering knowledge, "washing" the three technologies impression should not be categories; each embedded practices employing the realized specific forms of forms were dramatized in findings; the literary re-

³ Our use of the word *technology* and social relations may appear justified, as Carl Mitcham nicely esp. pp. 170-175. Mitcham deals of *techné*, one that combined and associated with speech. By using as well as to machines, we wish

tended an experience that was regarded as essential to the propagation of the material technology or even as a valid substitute for direct witness of experimental displays.¹ If we wish to understand how Boyle worked to construct scientific facts, we must consider how each of the three technologies was used and how each bore upon the others.²

THE MATERIAL TECHNOLOGY OF THE AIR PUMP

We start by noting the obvious: matters of fact in Boyle's new pneumatics were machine-made. His mechanical philosophy used the machine not merely as an ontological metaphor but also, crucially, as a means of intellectual production. The matters of fact that constituted the foundations of the new science were brought into being by a purpose-built scientific machine. This was the air pump (or "pneumatical engine," or, eponymically, the *machina Boyleana*), which was constructed for Boyle by the instrument maker Grotoreux and, especially, by Robert Hooke in 1658-1659. We have to describe how this machine was put together and how it worked in order to understand its role in fact-production.

Boyle intended to improve upon the design of Otto von Guericke's device, described by Caspar Schott in his *Magicka hydraulico-pneumatica* of 1657. According to Boyle, this earlier machine (see figure 24) had several practical disadvantages. (1) it needed to be immersed in a large volume of water, (2) it was a solid vessel, such that experimental apparatus could not be inserted in it; and (3) it was extremely difficult to operate, requiring, as Boyle observed, "the continual labour of two strong men 'for divers hours' to evacuate it." Boyle and Hooke sought to overcome these practical problems. Figure 1 is an engraving of their first spring-air machine, that was used to produce the forty-three experiments of *New Experiments Physico-Mechanicall*.³ The machine consisted of two main parts: a glass globe (or "receiver") and the pumping apparatus itself.

¹ Boyle, "New Experiments," pp. 6-7. Many of Boyle's essay titles began with "New Experiments . . . we use this short title to refer exclusively to the "New Experiments Physico-Mechanicall, touching the Spring of the Air" (1660).

² This account is drawn largely from that provided by Boyle in "New Experiments," pp. 6-11. One of the best modern descriptions of the pump and its operation is Frank Hoeny and the Oxford *Physiologists*, pp. 149-150. The most useful accounts remain the nineteenth-century essays of Wilson, *Edwards and Robert Boyle*, pp. 31-41, and, especially, his "Early History of the Air-Pump."



Robert Boyle's first air pump, *Physico-Mechanicall* (1660).

The receiver contained to be removed. It was, though Boyle would admit of his "glass men's" Boyle used a variety of sizes in volume, hoping (with little success) to be easier to evacuate.⁴

⁴ Boyle, "New Experiments"

the receiver through an aperture of about four-inch diameter at the top ("B-G"), and special arrangements could be made for instruments, like the Torricellian experiment, which were taller than even the big receiver, in which cases part of the apparatus extended through the sealed aperture above the receiver.

The receiver narrowed at its base so as to fit into a brass device ("N") containing a stopcock ("S"). This in turn was connected to a hollow brass cylinder ("3") about 14 inches long and about three inches in internal diameter. At the upper lip of the cylinder there was a small hole into which a brass valve ("R") could be inserted as required. Within the cylinder was a wooden piston (or "sucker") topped with "a good duck piece of tanned show-leather" ("4"), which provided for an exceedingly tight fit between piston and the inside of the cylinder. The piston was worked up and down by means of an iron rack ("5") and pinion ("7") device, the whole machine resting upon a wooden frame ("F").

This is how the engine worked to remove air from the receiver: with the stopcock in the closed position and the valve "R" inserted, the sucker was drawn up to the top of the cylinder; at this point there was no air between sucker and the top of the cylinder. Then the sucker was drawn down and the stopcock was opened, permitting the passage of a quantity of air from the receiver into the cylinder. The stopcock was closed, the valve was removed, and the sucker was forced up, thus expelling that quantity of air to the exterior. The process was repeated, each "exsuction" requiring progressively more force as the amount of air remaining in the receiver was diminished. (This account of how the machine worked to remove air, it must be noted, agrees with that provided by Boyle and modern commentators. As we shall see, Hohlus claimed that the receiver remained always full; therefore his view of how the pump operated, to be detailed in chapter 4, differed radically from Boyle's.) Later air-pumps of the 1660s and 1670s (described in chapters 5 and 6) differed from this original design in several respects: the cylinder and receiver were intimately connected, and, after Denis Papin's innovation of 1676, there were two pumping cylinders with self-acting valves. Although we shall be almost exclusively concerned here with Boyle's air-pump as a rarefying engine, it could also be used to condense air in the receiver, simply by reversing the operations by which air was withdrawn.⁸

⁸ As noted, for example, by Wilson, *Religio chemica*, pp. 197-198, and see Boyle, "New Experiments," p. 38.

The evacuation of air by pump was an extremely difficult task, and the problem of leakage was a constant one. Boyle's external air did not enter through a number of points and merely technical points of matter of fact crucially important, upon every all practical purposes, to seal the machine apparatus, the aperture at special contact called the reason of the exquisite of its texture, duty all provide the recipe for olive oil and other vegetable. He described how the it did not leak, using a ashes." And he took around the sucker was in the cylinder and to itself betwixt it and the "sallad oil" was poured and more oil was used. He noted that sometimes an effective seal and lubric to more spectacular assa state of the glass-blower receivers were likely to were not, in Boyle's view, pressure could act to pre for fixing them if requ cheese scrapings and wat and stinking smell," spec crack." Finally, the pressure and the force re affiat the gradness of t

⁹ Boyle, "New Experiments," p. 38. Boyle's use of the word "sallad" was somewhat unusual.

¹⁰ *Ibid.*, p. 9.

¹¹ *Ibid.*, p. 36.

the cylinder. The reasons for our detailed treatment of the physical integrity of the air-pump and the steps Boyle took to guarantee it will become clear below. For the present, we simply note three points: (1) that both the engine's integrity and its limited leakage were important resources for Boyle in validating his pneumatic findings and their proper interpretation; (2) that the physical integrity of the machine was vital to the perceived integrity of the knowledge the machine helped to produce; and (3) that the lack of its physical integrity was a strategy used by critics, particularly Hobbes, to deconstruct Boyle's claims and to substitute alternative accounts.

THE AIR-PUMP AS EMBLEM

Boyle's machine was a powerful emblem of a new and powerful practice. As Rupert Hall has noted:

The air-pump was the unflinching *pièce de résistance* of the incipient scientific laboratory. Its wonders were inevitably displayed whenever a grander graced a scientific assembly with his presence. After the chemist's furnace and distillation apparatus it was the first large and expensive piece of equipment to be used in experimental practice.

It was "the cyclotron of its age."¹⁸ Similarly, Marie Boas Hall:

... Boyle's air-pump together with Hooke's microscope constituted the show pieces of the [Royal] Society; when distinguished visitors were to be entertained, the chief exhibits were always experiments with the pump.¹⁹

As early as February 1661 the Danish ambassador "was entertained with experiments on Mr. Boyle's air-pump," and in 1667 Margaret Cavendish, Duchess of Newcastle, probably the first woman to be admitted to a meeting of the Royal Society, was treated to a similar display. According to Pepys, Margaret "was full of admiration, all

¹⁸ A. R. Hall, *From Galileo to Newton*, p. 294, and *idem*, *The Revolution in Science*, p. 262; see also Price, "The Manufacture of Scientific Instruments," p. 696; the pneumatic pump "was the first large and complex machine to come into the laboratory."

¹⁹ M. B. Hall, *Boyle and Seventeenth Century Chemistry*, p. 186.

admiration."²⁰ When the Society, it was anxious to then well-known to His the honoured guest. As

The solemnity of the our of the society, remarkable enough master experiment as is of new experimen servedly spent much

An experimental display be both edifying and sp air-pump:

And if you have any open new light into would better become possibly such would there ought to be so produce knocks only Karcher, Schottus, an become the gravity o tween both. luciferous advantage is obvious surprise with some u for the ingenuity of

²⁰ The visit of the Danish a that of Margaret in *ibid.*, pp. vol. viii, pp. 242-243 (entry 6 and the New Yorker, chap. 3. A for rationalistic, rather than Hobbes's patrons, and her a See Cavendish, *Observations on Excess*, p. 14 (also sig. 11). " rations arguments, which we and trusts, 1667 in the deca the perception of clear and re p. 218".

²¹ Wren to Trouncker, 30 Preparations for the King's May, 1684, but we have no ce 1684 place; see also Oldenbur 1687, vol. II, pp. 78-79. At pre

No new device had taken the place of the *machina Boyleana* as an emblem of the Royal Society's experimental programme.

The powerfully emblematic status of the air-pump is manifested in its contemporary iconography. Boyle and Hooke took an active interest in the production of drawings and engravings by William Faithorne that depicted Boyle together with his pneumatic engine (see figure 16).¹⁷ During the mid 1660s the Somerset virtuoso John Beale was sedulously involved in celebrating the Baconian works of the Royal Society, encouraging John Evelyn to produce an appropriate iconographic drawing which, after various vicissitudes, eventually appeared as a frontispiece in some copies of Sprat's *History of the Royal Society* (1667) (see figure 2).¹⁸ This engraving (by Wenceslaus Hollar) shows a redesigned version of Boyle's pump in the left background. (See figure 17 for an enlargement.) Through the last seventeenth and eighteenth centuries the Faithorne image was continually adapted and modified. Perhaps the richest in iconographic significance eventually appeared on the title page of the collected editions of Boyle's Works in 1744 and 1772 (figure 3).¹⁹ This vignette by Hubert François Gravéot, Bourguignon incorporated the Faithorne likenesses of Boyle and his original pump. The power of the pump is indicated by the conjunction of the Latin motto and the gesture of the classical female figure. Her left hand points to the air-pump while her right points to the heavens. The significance of the gesture is reinforced by the motto: "To know the Supreme Cause from the causes of things." It is the operation of the pneumatic engine, among all the scientific apparatus displayed in the engraving, that is going to enable the philosopher to approach God's knowledge.²⁰ The *ma-*

was a long time to be thought of as "pumpers" and royal display: "The works of God are not like the tricks of jugglers, or the juggles, and artificial prizes, where concealment is requisite to wonder; but the knowledge of the works of God proportionally our admiration of them." Boyle, "Usefulness of Experimental Natural Philosophy," p. 30 (1669).

¹⁷ For a full account of seventeenth and eighteenth century images of Boyle, see Maddison, "The Portraiture of Boyle." For correspondence relating to the Faithorne work, see Boyle, *Works*, vol. vi, pp. 438, 440, 441, 501, 503.

¹⁸ A detailed treatment of the circumstances attending the production of this image is in Hunter, *Science and Society*, p. 19, 207.

¹⁹ See Maddison, "The Portraiture of Boyle," p. 178.

²⁰ Such a motto might have been regarded as inappropriate for many mid-seventeenth-century experimental philosophers as apparently immodest sentiments seem to belong more to the mid-eighteenth century. Boyle agreed that one could never fully understand "from Nature up to Nature's God," yet we shall see that he set strict limits on the possibilities of natural knowledge.



Frontispiece to Sprat's *History of the Royal Society*, 1667, and transferred to Sprat in the center by background foreground are the president (last center, being eternal

- [Burial Rites pdf](#)
- [read Smart Skills: Negotiation](#)
- [download Bitemporal Data: Theory and Practice pdf](#)
- [The Big Fight: My Life In and Out of the Ring pdf, azw \(kindle\)](#)
- [read online Aesthetics of Sorrow: The Wailing Culture of Yemenite Jewish Women online](#)

- <http://junkrobots.com/ebooks/Arms-and-Equipment-of-the-Civil-War.pdf>
- <http://anvilpr.com/library/The-Black-Box.pdf>
- <http://korplast.gr/lib/Bitemporal-Data--Theory-and-Practice.pdf>
- <http://korplast.gr/lib/Green-Smoothie-Magic---132--Delicious-Green-Smoothie-Recipes-That-Trim-And-Slim.pdf>
- <http://monkeybubblemedia.com/lib/Aesthetics-of-Sorrow--The-Wailing-Culture-of-Yemenite-Jewish-Women.pdf>