The Historical Journal, 42, 4 (1999), pp. 1125–1145 Printed in the United Kingdom
© 1999 Cambridge University Press

IDENTITY, PRACTICE, AND TRUST IN EARLY MODERN NATURAL PHILOSOPHY

ADRIAN JOHNS
University of California, San Diego

ABSTRACT. *Historians of early modern science face a serious problem, in that there* was *no science in early modern society. There were, however, other enterprises in the early modern period devoted to the understanding and manipulation of the physical world. This review identifies important trends in historians' attempts to comprehend those enterprises. In particular, it identifies four leading currents. The first is the move to characterize these different enterprises themselves, and in particular to understand natural philosophy and the mathematical sciences as distinct practical endeavours. The second is the attention now being paid to the social identity of the investigator of nature. The third is the attempt to understand the history of science as a history of practical enterprises rather than propositions or theories. The fourth, finally, is the understanding of natural knowledge in terms of systems of trust, and in particular in terms of the credit vested in rival claimants. In a combination of these, the review suggests, lies a future for a discipline that has otherwise lost its subject.*

I

Some time around 1652, John Evelyn, newly returned from the Grand Tour, sat down and composed a taxonomy of the practices and techniques pursued in early modern society. His classification scheme carved up the world of craft into nine categories that he considered rational and distinct: 'Usefull and purely Mechaniq'; 'Meane & Less honourable'; 'Servile'; 'Rusticall'; 'Femall'; 'Polite'; 'More Liberall'; 'Curious'; and, finally, 'Exotick and very rare Seacrets'. The classification must have seemed an appropriate one, since it soon became rather a sought-after asset. When, a year later, the instrument-maker Ralph Greatorex introduced Evelyn to Samuel Hartlib, he did so as a dedicated 'chymist' who kept 'many furnaces a going'. But what really attracted Hartlib's interest was the rumour that this potential collaborator had 'collected a great Worke of all Trades'. This 'Worke' apparently ran to several volumes, and was almost complete. In effect, the nine categories formed its table of contents. For Hartlib, who had already dedicated years to recruiting gentlemen, craftsmen, and scholars to his own various projects of 'improvement', this promised to be a substantial prize. And he was not at first disappointed, for Evelyn had indeed compiled such a document. But it was not quite as extensive and definitive as Hartlib had hoped. On the contrary, it was a distinctly patchy composition. Although running-heads had been written in for a volume of 605 pages, text filled only 44 of them. Most of that material related to 'polite' and 'exotick' practices – techniques fascinating to a virtuoso like Evelyn, including a method for making marbled paper, a way of using shells in the construction of grottoes, and a recipe for gilt varnish. It also contained detailed descriptions of a lime kiln and

https://doi.org/10.1017/S0018246X99008729 Published online by Cambridge University Press

the calculations necessary to plan a naval vessel. But coverage of the productive crafts to be seen in action along any street in Hartlib's London was at best thin, and in most cases non-existent. Clearly, much work remained to be done.[1]

Like Hartlib, a modern historian encountering Evelyn's list for the first time is likely to respond with enthusiasm. It invites obvious comparison to the fantastic Borgesian classification scheme that Michel Foucault used to introduce *The order of things* – a work that proved immensely influential in shaping the field in the 1970s. Like the taxonomy of animals in the *Celestial emporium of benevolent knowledge*, this classification of techniques provokes reflection on what seems to modern minds 'the stark impossibility of thinking *that*'.[2] But if anything it packs a heavier punch, because it is both historically real and, for us at least, closer to home. Evelyn, although no Newton, was a considerable figure in the realignment of intellectual culture that took place in the seventeenth century. His list was the blueprint for a project that was similarly central – one that deserved state patronage, and that might take decades to complete. The project to create a 'history of trades' continued to consume the attentions of many virtuosi, a generation after it had first been advocated by Francis Bacon. Evelyn's plan proved a lastingly influential redefinition of that project. Preserved through the years of Cromwellian rule, it emerged after the Restoration to form the basis for the Royal Society's own investigation of trades. If scholar and craftsman were ever finally to meet, it would be by following a map like this.

Evelyn's schema underwrote a planned inquiry into a world of practice to which the learned were increasingly looking for knowledge of and power over nature. As such, it provokes reflection on the issues that consume today's historians of science, who find themselves faced with making a similar inquiry into Evelyn's own culture. There are four such issues. The first of them, and in a basic sense the most problematic, relates to the identity of science itself. Like Evelyn, we need to sort out the categories with which we are dealing. We are coming to accept that modern science is an enterprise characteristic of the industrial age: while *scientia* did exist in the early modern period – in the sense of certain, demonstrative knowledge – there was no such thing as a scientist.[3] This means that historians interested in natural knowledge have an identity crisis of their own to face. Are they historians *of science* at all? In what sense? They are being forced to pay closer attention to how early modern people defined their own enterprises, in a bid to address, if not overcome, the implications of such a challenge. Like Evelyn, they find themselves facing a world of enterprises that are apparently mysterious, but which they must classify for the knowledge they promise. Future historians may well end up with a classification of pre-scientific enterprises that to us would seem as odd as Evelyn's own.

[1] John Evelyn, cited in M. Hunter, *Science and the shape of orthodoxy: intellectual change in late seventeenth-century Britain* (Woodbridge, 1995), pp. 75–8 ('Polite' and 'More Liberall' were later joined to form one category).

[2] M. Foucault, *The order of things: an archaeology of the human sciences* (London, 1970), p. xv. Foucault's source is 'The analytical language of John Wilkins', in J. L. Borges, *Other inquisitions, 1937–1952*, trans. R. L. C. Simms (Austin, 1964), pp. 101–5: see p. 103. I am grateful to Simon Schaffer for helping me locate this.

[3] For programmatic remarks, see A. Cunningham and P. Williams, 'De-centring the "big picture"': *The origins of modern science* and the modern origins of science', *British Journal for the History of Science*, 26 (1993), pp. 407–32. See also S. Ross, '*Scientist*: the story of a word', *Annals of Science*, 18 (1962), pp. 65–85; and R. Yeo, *Defining science: William Whewell, natural knowledge, and public debate in early Victorian Britain* (Cambridge, 1993).

https://doi.org/10.1017/S0018246X99008729 Published online by Cambridge University Press

The second problem is very closely related to the first. It concerns the identity, not of enterprises, but of people. If early modern investigators were not scientists, then what were they? In their own terms, they included physicians, mathematical practitioners, natural philosophers, and gentlemen. But this multiplicity is not the only issue. In fact, whilst such identities were both established and largely stable, men and women do seem sometimes to have gained remarkable freedom to move between them, and even to use available resources to construct new ones. The most sustained examination of a practitioner doing this is Mario Biagioli's of Galileo. Biagioli tracks Galileo's progress through patronage regimes in Florence and Rome as he sought to make for himself a position as 'philosopher and mathematician' to the court. He was the first European to hold such a title, and, Biagioli insists, the fortunes of his claims and theories rose and fell with his efforts. There are now several studies that exemplify similar points. We have not only mathematicians becoming philosophers, but astrologers becoming experimentalists, statesmen becoming naturalists, and alchemists becoming entrepreneurs. All of them helped to re-sort the species of early modern practitioners, and in the process to unsettle and reshape the knowledge they professed.[4]

The third issue is then the need to appreciate not just the conclusions reached by these workers, but the means by which they reached them. There is a new focus on the practical character of their activities, and on how such practices served to create and secure natural knowledge. As Evelyn's list confirms, the characterization of practical skills, as much as that of knowledge itself, was seen by early modern virtuosi themselves as an important and difficult task. Modern historians too are focusing upon the same subject. Moreover, how could practical skills be made common property, in a world full of secretive guilds and riven by confessional and dynastic commitments? Yet common property they would have to become, if individual experimental claims were to advance to the level of universally accepted knowledge. In raising such problems, historians of science are showing themselves dedicated to sustained reflection on their own practice, with results that should command attention from their fellows in neighbouring fields. Whether *skills* can be captured by an interpretative practice honed on the exegesis of *texts* is a much-argued point, and one central to the enterprise of political, military, or religious history quite as much as that of science.[5]

The fate of Evelyn's project itself suggests a fourth issue. Ultimately, the effort to write a history of trades showed few results. Despite repeated exhortations, the Royal Society found it very difficult indeed to persuade craftsmen to reveal their 'rare Seacrets' to a body of gentlemen, since the very openness that the virtuosi so lauded told the craftsmen that the Society could not be guaranteed to honour confidentiality – a serious matter of livelihood to, say, instrument-makers or weavers. The history of trades came to nothing

[4] M. Biagioli, *Galileo, courtier: the practice of science in the culture of absolutism* (Chicago, 1993); see also P. Curry, *Prophecy and power: astrology in early modern England* (Cambridge, 1989), J. Martin, *Francis Bacon, the state, and the reform of natural philosophy* (Cambridge, 1992), and P. H. Smith, *The business of alchemy: science and culture in the Holy Roman Empire* (Princeton, 1994).

[5] For suggestive comments, see: O. Sibum, 'Reworking the mechanical value of heat: instruments of precision and gestures of accuracy in early Victorian England', *Studies in History and Philosophy of Science*, 26 (1995), pp. 73–106; A. Warwick, 'Cambridge mathematics and Cavendish physics: Cunningham, Campbell, and Einstein's relativity, 1905–11: part I: the uses of theory', *Studies in History and Philosophy of Science*, 23 (1992), pp. 625–56; and P. Galison and A. Warwick, eds., *Cultures of Theory* (*Studies in History and Philosophy of Modern Physics*, 29, 3 (1998), pp. 287–434). An important critique is S. Turner, *The social theory of practices: tradition, tacit knowledge, and presuppositions* (Chicago, 1994).

https://doi.org/10.1017/S0018246X99008729 Published online by Cambridge University Press

because two communities found the problem of establishing a relationship of mutual trust to be intractable. Trust thus stood at the nexus of knowledge and social order. And only with trust in people could come credibility for their observations and theories. As a result, the issue of trust offers a potential key to many of the problems now being encountered by historians interested in that combination of nature and culture. Together, the issues of identity, practice, and trust are reshaping the history of science. As a result, science may lose some of its sheen as the site of a rationality transcending place and time, but it stands to gain a much more central place in the historian's craft.

II

Perhaps the most evident characteristic of recent work in the history of science is a concern to appreciate that important distinctions were once made between enterprises that today are easily subsumed under the label of science. Roger French's reappraisal of William Harvey is the most explicit and thoroughgoing attempt to pursue this effort to the full.[6] French's book is not a biography of Harvey, nor an exhaustive survey of his work. Instead, it provides a new account of how his central claim that the blood circulated in the body fared at the hands of its first European readers. Harvey initially made this claim in lectures at the Royal College of Physicians, then issued it in print in 1628 in his *De motu cordis et sanguinis*. French is interested in how Harvey and his readers then argued for and against circulation. In other words, he wants to know not how the truth of the theory was accepted, but how people came to attribute truth to the theory. Or rather, theories; for there were soon many different kinds of circulation on offer. Before long, authoritative physicians and philosophers from all over Europe were publishing their own circulatory claims, every one of them different from the others in respects that their proponents could represent as crucial. French reviews in full the arguments that could be brought forth to defend all these possible views.[7] And what a variety there was: all conceivable stances were adopted, from a refusal to countenance any circulation at all, through any number of theories of 'lesser' circulations, to Harvey's own audacious scheme. French's survey of these is a reminder, by turns surprising, fascinating, and gruesome, of just how polyphonic the world of early modern natural knowledge could be.

But this is not just a rehearsal of a classic series of arguments in medical history. French is emphatically revisionist, in precisely the sense that that term has been employed by political historians writing about the same period: he insists that we assess the responses to Harvey with respect only to preceding events and contemporary concerns, and discard questions of a 'revolution' visible only with later eyes. But his more specific interest is in the identity of the enterprise that Harvey was pursuing. This enterprise was called 'natural philosophy'. It is commonly elided with 'science' in modern analyses, but French insists that this elision is a profound mistake. In fact, natural philosophy was a distinct undertaking, with its own aims, objects, and approaches. It is vital to appreciate this, because what was original about Harvey was his transfer of the enterprise of natural philosophy into the traditional place and practice of medical anatomy. Interpretations prior to French's have therefore misconstrued the significance of Harvey's work because they have assumed that both his own enterprise

[6] R. French, *William Harvey's natural philosophy* (Cambridge, 1994).

[7] Compare R. G. Frank, *Harvey and the Oxford physiologists: a study of scientific ideas* (Berkeley, 1980), a classic that adopts a more local perspective.

https://doi.org/10.1017/S0018246X99008729 Published online by Cambridge University Press

and the traditional one were, at root, the same thing. The point is similar to that repeatedly made by R. G. Collingwood (although Collingwood himself is not a writer whom French cites). The historian who insists on calling Harvey a 'scientist' is like the figure of Collingwood's nightmare who maintained that Athenian triremes were actually steamships, just not very good ones (their inadequacies, he explained, simply demonstrated how far we had progressed since classical times).[8] But the error in the case of the trireme/steamship seems to us obvious. Why is it, then, that in the case of natural philosophy/science the fault is not equally clear? Presumably because science holds a far stronger and more central place in modern sensibilities than steamships do. Steamers are not scientific laws, and elements like facts and reason have taken on the aura of an objectivity that can appear timeless. They look to us like acultural preconditions for knowledge, rather than elements of knowledge itself. We are all too easily tempted to locate them in the distant past. If we recognize that these concepts are themselves historical in character, however – that there is a history of the fact, and another of objectivity, and a third of reason – then science itself begins to look far more like Collingwood's steamer. It may look efficient and unsurpassable, but it was built by human beings, and there have been other ways to cross the water.[9]

Having argued this, French is then concerned to apply what was the central question of Harvey's natural philosophy recursively, to natural philosophy itself. A historian's Hannibal Lecter, he asks of natural philosophy, 'what is it *in itself?*' To understand the force of this question, one really needs to read French's argument in conjunction with *Before science*, a work he has recently published in collaboration with Andrew Cunningham. *Before science* presents a thesis of great originality about the historical origins, not of science, but of natural philosophy.[10] The argument is that natural philosophy, as one among many distinct enterprises devoted to the physical world, was invented in the twelfth and thirteenth centuries as part of a campaign dedicated to the eradication of heresy. In what is now southern France and northern Italy, the Cathars had achieved notable success in arguing for a theology of two coeternal principles of good and evil, with the material world being the creation of the evil principle. To do this, they had appropriated methods from philosophy, and in particular a way of arguing that traced chains of causation back from physical changes seen in the world to their ultimate causes. This seemed to provide a rigorous demonstration of the evil character of a creator capable of perpetrating matter. In response, Catholic authorities seized upon the works of Aristotle now becoming available in translation from Greek and Arabic, and from them created a discipline dedicated to a correspondingly demonstrative argument for an orthodox creation and a good creator. This discipline, called 'natural philosophy', became a centrepiece of the new universities then being founded in Oxford, Paris, and Toulouse. It was characterized by the analysis of

[8] R. G. Collingwood, *An autobiography* (Oxford, 1978), p. 64 (first published in 1939); see also N. Jardine, *The scenes of inquiry: on the reality of questions in the sciences* (Oxford, 1991).

[9] See especially L. Daston, 'Baconian facts, academic civility, and the prehistory of objectivity', *Annals of Scholarship*, 8 (1991), pp. 337–63; L. Daston, 'The factual sensibility', *Isis*, 79 (1988), pp. 452–70; L. Daston and K. Park, *Wonders and the order of nature, 1150–1750* (New York, 1998), pp. 230–53; M. Poovey, *A history of the modern fact: problems of knowledge in the sciences of wealth and society* (Chicago, 1998), esp. pp. 1–16.

[10] R. French and A. Cunningham, *Before science: the invention of the friars' natural philosophy* (Aldershot, 1996). French and Cunningham's work should be compared to similarly general accounts of the advent of various forms of persecution: see especially R. I. Moore, *The rise of a persecuting society* (Oxford, 1987), pp. 6–65.

https://doi.org/10.1017/S0018246X99008729 Published online by Cambridge University Press

causation, and especially by the aim of understanding the 'final causes' of natural things. That is, the natural philosopher strove to understand of any given thing what it was in itself – what it was its purpose to be. This had a ready Christian application, since for a human being the what-it-is-to-be-a-thing (as French calls it) could readily be identified as the soul. Natural philosophy remained a controversial practice, of course, not least because, although Christianized in this and other ways, it remained based in an artful appropriation of non-Christian sources. And before long there were other kinds of natural philosophy too. But the origin of the activity itself in opposition to heresy remained central to its historical identity. In the seventeenth century, natural philosophers who regarded it as part of their duty to inveigh against the heterodox, far from violating any principle of scientific freedom, were continuing what had been an essential part of the enterprise since the outset. The same inheritance was writ large in Harvey's debates over circulation.[11]

French's Harvey was renewing the traditional aims of natural philosophy, and applying this enterprise to the heart. The reappraisal of natural philosophy here has a major interpretative consequence. The chronology of natural philosophy is not that of science, and its historian perforce cannot rest content with the ring-fenced expertise of an authority on the 'scientific revolution', who might comfortably neglect practices pursued before the Renaissance. With a perspective extending back far into the middle ages, French himself is able to identify the perseverance and reappropriation of medieval practices where few other historians of medicine would have spotted them. For example, Harvey's adept use of the techniques of scholastic disputation is as evident to him as it must have been to a listener at the original lectures. With this in mind, French shows that Harvey's great originality lay in asking a question that in another field than anatomy would have been extremely hackneyed. In effect, Harvey wanted to know what it was to be a heart. Thinking of himself as a natural philosopher, he applied Aristotelian methods and aims in his anatomizing. And this was something new *for anatomy*. Anatomy had always been directed to the practice of medicine, not to the contemplation of philosophical knowledge; it had rested on the authority of Galen, not Aristotle. Its practitioners did not ask Harvey's type of question. For example, anatomists did not generally do comparative dissections – let alone vivisections – of non-human creatures. The inquiry that would have given such activities meaning – a search for characteristics common to various kinds of heart – simply did not arise. By leading him to raise this question, the enterprise of natural philosophy was largely responsible for Harvey's discovery of the circulation.

Many of the criticisms that contemporaries levelled at Harvey start to make sense in this light. Far from manifesting a stubborn refusal to recognize observed facts, critics often depended on them. But the observations they made arose within different practical regimes, and, more important, responded to different questions. Defences of Galenic anatomy, in particular, were defences not just of a theory of the heart and blood, but of a well-established practice of medical therapy in which bleeding was a standard technique. The basis of that practical art was challenged by Harvey's novel kind of questioning. But the most striking example is provided, not by the physicians conventionally seen as conservative, but by the mechanical philosopher Pierre Gassendi. Gassendi staunchly maintained that he had *seen* pores in the septum at a dissection performed at Aix en Provence. This act of witnessing, had it become accepted, would

[11] French, *William Harvey's natural philosophy*, pp. 194, 264–5.

https://doi.org/10.1017/S0018246X99008729 Published online by Cambridge University Press

have gone a long way to discrediting Harvey's claims. And it might well have been credited, since Gassendi was widely respected as a judicious and reliable interlocutor. But he was also that peculiar creature, a convinced sceptic. As such, he remained distinctly reluctant to advance 'systematic' claims with a degree of ambition sufficient to challenge Harvey's on their own broad level.[12] His observation was therefore personally convincing, and certainly anomalous, but collectively inconsequential.

French's book provides the most uncompromising statement of a case that is being made by many recent works in the history of the sciences. Two noteworthy examples are Paula Findlen's *Possessing nature* and Nicholas Jardine, James Secord, and Emma Spary's collaborative *Cultures of natural history*, which together restore to view the scandalously neglected practice of natural history.[13] The most fruitful comparison, however, is with Peter Dear's focus on what John Wilkins (a great ally of Evelyn) called 'Physico-Mathematicall Learning'.[14] Dear too is acutely aware of the variety of possible enterprises devoted to the physical world in the early modern period, and of the importance of distinguishing between them. He is particularly concerned with the mathematical sciences, as they were called – practices like navigation, astronomy, and surveying, that involved the manipulation of quantity to the end of mastering natural powers. In natural philosophy of the kind revived by Harvey, such 'sciences' were accorded low status. Aristotelians called them 'mixed', because, however effectual they might be in action, they instantiated a category confusion between the substantial and the mathematical that was illegitimate in principle. If the point of natural philosophy was to explain natural processes by their causes, and those causes resided in the substantial essences of things, then mathematics indeed had a marginal role to play. It dealt only in what were called 'accidents'. Yet by the late seventeenth century Newton could call his great work *The mathematical principles of natural philosophy* – a formula that in previous generations would have counted as a virtual oxymoron. Dear's *Discipline and experience* is the first clear account of how this change came about, and why it mattered.

In one sense, Dear is dealing with a subject as old as studies of the scientific revolution itself: the 'mathematization of the world picture', perhaps. But he, like French, has reconsidered the terms of the question – and to even more convincing effect. Dear restores to view thinkers who are now forgotten, but who were at the time of central importance. The Jesuit Josephus Blancanus is one example; his *De mathematicarum natura dissertatio* (1615) argued for the use of mathematics in the causal analysis of natural change at a turning point in the history of natural philosophy. Inheriting an innovative mathematical programme from Christopher Clavius, Blancanus and his brethren extended its scope and pioneered arguments for the natural-philosophical use of both experience and mathematics. Today it seems self-evident that natural knowledge is properly based on these. But that self-evidence is largely a result of their hard-won success, secured at the hands of Newton. Dear is the ideal guide to all their efforts, since he is at once both philosophically astute enough to spot the importance of distinctions that might otherwise pass the modern reader by, and clear enough in his exposition to make those distinctions apparent and consequential.

The sensitivity shown by Dear, it must be said, is still relatively unusual. Many scholars still blithely refer to early modern figures like Newton as 'scientists', and ignore

[12] Ibid., p. 329.

[13] P. Findlen, *Possessing nature: museums, collecting and scientific culture in early modern Italy* (Berkeley, 1994); N. Jardine, J. A. Secord, and E. C. Spary, eds., *Cultures of natural history* (Cambridge, 1996).

[14] P. Dear, *Discipline and experience: the mathematical way in the scientific revolution* (Chicago, 1995).

https://doi.org/10.1017/S0018246X99008729 Published online by Cambridge University Press

the practical, social, and economic gulf that generally existed between, say, natural philosophers and mathematicians. Moreover, the importance of the issue is not always brought out as effectively as it might be even by those who do attend to it. An example is Ann Geneva's unusual study of astrologer William Lilly. Geneva insists from the outset that the craft of astrology, too, be taken for what it was. In her words, 'astrology in seventeenth century England was not a science. It was not a religion. It was not magic. Nor was it astronomy, mathematics, puritanism, neo-Platonism, psychology, meteorology, alchemy, or witchcraft.' In short, 'it was only itself'.[15] A viable enterprise in its own right, it had its own history and traditions, and its own criteria of quality by which work could be appraised and compared. It was a craft at which Lilly could be supremely good without, for example, observing the heavens for himself, or doing much in the way of astronomical calculation. This is all uncontentious. The evidence is overwhelming that astrological prophecies like his were indeed taken very seriously, not for the functionalist reasons that modern readers tend to impute, but because astrological interpretation *itself* was convincing to them. Geneva's book sets out to demonstrate why. But instead of pursuing this interesting project to a consequential conclusion, she soon diverts to pursue a much more specific claim. Geneva argues that she has deciphered the astrological 'code' of Lilly's prophecies, to reveal a particular, politically radical message that was apparent to informed readers in the 1640s but has since been forgotten. The point might have been a valuable one, since it could remind us that the skills of a reader are themselves historical. But in fact the diversion into this rhetoric of revelation is unfortunate on several grounds. For one thing, it drives Geneva to disparage repeatedly the efforts of other historians, who are always cast, not as products of our own historical situation, but as wilful neglectors of valuable evidence. More materially, it leads her into a series of interpretative dead-ends, such as the long section devoted to 'validating' early modern testimony about parhelia (mock suns) by reference to a modern physics textbook.[16] Quite apart from the fact that parhelia are actually rather well-known phenomena, in need of no such validation, this recourse to modern science imposes an inappropriate standard of credibility on her own grounds. Warranting past claims in modern terms should be just as anachronistic as denying them. It gives the lie to the professed aim of recovering the astrological craft itself.

Yet the sheer, relentless pugnacity of Geneva's book is at least true to the period. She simply adopts for herself the manners of the most characteristic form of astrological communication, namely the pamphlet war. Whether pursued in astrologers' libels, surgical theatres, academic disputations, or mathematical challenges, the battles in natural philosophy and mathematics proved immune to any attempt at resolution. Long before 1600, Aristotelianism was just one among many natural philosophies, and all sought supremacy. The competition was profound, with no neutral ground from which to identify winners and losers. An approach like French's therefore does have its limits, if not quite the ones that spring most readily to view. Historians who keep watch for every whiff of essentialism will not have taken long to convict French of the offence. The criticism is accurate, but in one sense facile: Aristotelianism was by definition essentialist, and French's recursive Aristotelianism is true to its source. More to the point, however, it is vulnerable to a damagingly reductive response of *tu quoque*. If, as French insists, it is mistaken to elide the enterprise of natural philosophy with science,

[15] A. Geneva, *Astrology and the seventeenth-century mind: William Lilly and the language of the stars* (Manchester, 1995), p. 9.

[16] Ibid., pp. 99–118.

https://doi.org/10.1017/S0018246X99008729 Published online by Cambridge University Press

then is it not wrong to elide different versions of natural philosophy under that one label? In what sense were atomism, corpuscularianism, experimentalism, Harveian anatomizing, and paracelsianism the same thing? The polyphony of claimants to natural knowledge in Harvey's period, at least, means there simply could not be one answer to the question, 'what was natural philosophy?' Asking that question, therefore, can only be a first step. Useful as it is, it ends up directing us to a different problem altogether.

The problem to which it leads is a problem of credit. At a time when a multitude of different enterprises existed, with mutually irreconcilable standards of evaluation, why were some claims accepted and others rejected? Dear and French converge on this question toward the ends of their books, and they also suggest a common answer. They identify a new persuasive strategy deployed in the mid- to late seventeenth century in an attempt to provide common ground in philosophy through common-sense attributions of credibility. This was the practice known as 'experiment'. Experiments were staged practical demonstrations, designed to manifest 'matters of fact' before witnesses whom contemporaries would *just know* to be reliable.[17] Given this self-evidence, believing claims made on the basis of experiments became a relatively simple matter. But in a society without scientists, what kind of person carried such self-evident authority?

III

If the enterprise of natural philosophy needs to be seen as something different in kind from science, all the more must the natural philosopher be appreciated as something other than a scientist. Steven Shapin has asked in an important paper the apparently obvious question, 'Who was Robert Hooke?'[18] In a sense, we know the answer very well: Hooke was the notoriously jealous 'curator of experiments' at the Royal Society, on whose almost endless practical creativity depended the Society's programme of experimental investigations. But Hooke was never called a scientist, nor even, as far as we can tell, a philosopher. So Shapin's question opens a broad range of inquiries into the character of natural investigators in early modern Europe: what qualities should they possess? Where should they exist, both physically and in the strata of social ranks? How should they conduct themselves?

A similar question to Shapin's is brought to mind by by the new *Cambridge companion to Francis Bacon*. The *Companion* joins a rapidly growing series, and is on the whole a welcome and authoritative addition. Its component essays meet high standards of quality. But whether it is possible to find in this book a collective answer to the question, 'who was Francis Bacon?' is not so clear. Sachiko Kusukawa's exhaustive survey of Renaissance classifications of knowledge, Michel Malherbe's treatment of Baconian method, and Graham Rees's careful reconstruction of Bacon's eclectic and sometimes bizarre natural philosophy are all excellent discussions of their subjects, as is Brian Vickers's characteristically astringent treatment of late Renaissance rhetoric and its modern interpreters. They form the philosophical and natural-philosophical components of an answer. But there are both gaps and conflicts between the chapters when

[17] French, *William Harvey's natural philosophy*, p. 386; Dear, *Discipline and experience*, pp. 246–7.

[18] S. Shapin, 'Who was Robert Hooke?', in M. Hunter and S. J. Schaffer, eds., *Robert Hooke: new studies* (Woodbridge, 1989), pp. 253–85.

https://doi.org/10.1017/S0018246X99008729 Published online by Cambridge University Press

it comes to the personal and historical components. There is no extended discussion of Bacon's legal work and thought, for example. It would surely have been worth including a discrete chapter on the subject, especially since Julian Martin has argued convincingly that it is an important key to explaining his ambitions for natural philosophy.[19] Moreover, there is no discussion of how Bacon sought to make himself into the kind of person entitled to discourse authoritatively on matters of knowledge. Did being a statesman or lawyer count as sufficient qualification? Perhaps, but Bacon was not yet lord chancellor when he began issuing his philosophical admonitions, and historians have detected their origins back in the days when he was a lowly bencher at Gray's Inn. At almost exactly the same time, Galileo Galilei was forging a new 'socioprofessional identity' for himself as he angled for an appointment in Florence at the court of a grand duke.[20] Like Bacon, Galileo would at first succeed – by the 1610s he was receiving one of the highest rates of remuneration in the Medici realm – only to fall from grace into a retirement of philosophical investigation and writing. Yet no hint of a correspondingly dynamic and laborious *process* is provided for Bacon here. His achievements are rehearsed as so many propositions.

One underlying reason for this is that the *Companion* as a whole avoids acknowledging the problems that are so pivotal to French, Dear, and even Geneva. Some individual contributors do indeed buck the trend to acknowledge their import: Ian Box, for example, ingenuously concedes that his assigned subject of 'moral philosophy' did not really exist for Bacon. But less subtle contributors like Rose-Mary Sargent use 'science' and 'natural philosophy' interchangeably, and others are content simply to stick to 'science'. For moderns, being a scientist explains itself, so no history of the creation of a personal character is needed. But Bacon himself would not have recognized that conviction. He was concerned above all about the identity, extent, and import of enterprises devoted to natural knowledge, and was scarcely less exercised about the characters of their practitioners. Some ambiguity about the development of his methods and objectives would certainly be appropriate – but not *this* ambiguity.[21]

The sheer multiplicity of natural philosophies available in Bacon's age had personal implications as well as disciplinary ones. Bruno was burned, Campanella jailed, and Hobbes vilified. Yet as Bacon and Galileo in their different ways showed, there was opportunity in the chaos. Seizing that opportunity was a high-risk endeavour on an intimate scale as well as an international one. Its outcome might hang on securing a personal reputation for learning, veracity, or expertise. Samuel Hartlib was one of many working in the middle of the century to forge a new community of such individuals, called 'virtuosi'. Such a community might replace the 'cavilling' scholastics found in academia. The long-honoured disputations central to university education were now regularly castigated as teaching only pugnacity, and Hartlib, along with others, advocated replacing them with the processes of polite conversation. Collaboration might reshape the identity of the natural philosopher as a pillar of civil society. The ambitions of these self-selected improvers extended beyond natural philosophy itself,

[19] S. Kusukawa, 'Bacon's classification of knowledge', M. Malherbe, 'Bacon's method of science', G. Rees, 'Bacon's speculative philosophy', and B. Vickers, 'Bacon and rhetoric', in M. Peltonen, ed., *The Cambridge companion to Bacon* (Cambridge, 1996), pp. 47–74, 75–98, 121–45, and 200–31; Martin, *Francis Bacon*, pp. 141–71.

[20] Biagioli, *Galileo, courtier*, pp. 11–101.

[21] I. Box, 'Bacon's moral philosophy', and R.-M. Sargent, 'Bacon as an advocate for cooperative scientific research', in Peltonen, ed., *Cambridge companion to Bacon*, pp. 260–82 and 146–71.

https://doi.org/10.1017/S0018246X99008729 Published online by Cambridge University Press

and into the mathematical sciences, agriculture, and trade. That was why Hartlib found Evelyn's list so appealing: it offered a map of these territories to be discovered and conquered. He himself struggled to co-ordinate efforts, channelling correspondence between distant writers and angling for patents and other perquisites to finance the endeavour. For a while, projects and promises bloomed. But they came to virtually nothing. For all that historians can retrospectively identify Hartlib as a pioneer of the Royal Society and the public sphere, in his own day his plans without exception failed. By the early Restoration he was a rather sad figure, excluded from the ranks of the experimentalists and dying in poverty in 1662. Within a generation, all that was left of his schemes was a box or two of mouldering papers.

By some miracle, those papers survived. As late as the 1930s they lay neglected in a trunk in a London solicitor's office. But then they came to the notice of George Turnbull, a professor of education at Sheffield University. Turnbull, who already knew of Hartlib through an interest in early modern pedagogy, realized their importance immediately. He transported the papers to Sheffield and began single-handedly to inventory them. The initial results appeared in 1947 in Turnbull's *Hartlib, Dury and Comenius* – a ground-breaking book that still remains useful today.[22] The material was subsequently given its central place in the history of early modern natural knowledge by Charles Webster's *Great instauration* (1975). Then, in 1988, work began on the Hartlib papers project, dedicated to the transcription and electronic publication of the entire archive. This remarkable project has now culminated in the issuing of the papers on CD-ROM. It makes available a uniquely rich record of the daily conduct of natural philosophy, agricultural improvement, and intellectual communication in the mid-seventeenth century.[23] And the Hartlib papers project also found time to issue two volumes of essays based largely on the Sheffield material. Together they provide a collective portrait of natural knowledge and its seekers at a moment of profound uncertainty.

Michael Leslie and Timothy Raylor's *Writing and the land* is the earlier and more modest of these two volumes.[24] It deals with the changing relation between human work, culture, and the natural world during the period. The book's *raison d'être* arises in part from Anthony Low's *The georgic revolution* (1985). Combining attention to the literary culture of the day with discussion of the 'new science', Low argued that Virgil's *Georgics* became newly influential in a late seventeenth-century England that was witnessing radical changes in farming practice. This combination of literary representation and technical practice, Low claimed, 'was directly responsible for the well-known Agricultural Revolution of the eighteenth [century]'.[25] The contributors in Leslie and Raylor's collection sometimes endorse, but more often qualify, such pronouncements. What they share with Low is an admirable ambition to relate literary and philosophical representations *of* the land to agricultural and horticultural practices *on* the land, without reducing either to a mere reflection or application of the other. Joan Thirsk and Andrew McRae establish the theme at the outset, by analysing the subtle relationship between representation and practice in husbandry. Thirsk describes the uses of classical sources like Xenophon and Columella in the sixteenth century, McRae

[22] M. Greengrass, M. Leslie, and T. Raylor, eds., *Samuel Hartlib and universal reformation: studies in intellectual communication* (Cambridge, 1994), introduction, pp. 4, 7–11.

[23] *The Hartlib papers* (2 CD-ROMs, Ann Arbor, 1995).

[24] M. Leslie and T. Raylor, eds., *Culture and cultivation in early modern England: writing and the land* (Leicester, 1992).

[25] A. Low, *The georgic revolution* (Princeton, 1985), p. 119.

https://doi.org/10.1017/S0018246X99008729 Published online by Cambridge University Press

those of new husbandry texts in the seventeenth.[26] Later essays then focus more exclusively on practical endeavours, most notably the 'spiritual husbandry' of John Evelyn and John Beale. Evelyn composed over the decades an extraordinary manuscript treatise devoted to the creation of a perfect garden. Part technical manual and part rhapsody along the lines of Sir Thomas Browne's *Urne-buriall*, he called this work *Elysium Britannicum*. Unlike his history of trades, Evelyn's *Elysium* grew and grew. By the time of his death it filled over a thousand pages; publication had long been unthinkable. But it was not quite private, for Hartlib engaged his correspondents to peruse parts of it and provide suggestions. Perhaps the most useful came from the Herefordshire gentleman and advocate of cider, John Beale. Beale's signal contribution was a site for the garden itself. What he did not tell Evelyn was that this site, Backbury Hill, held a peculiarly personal meaning. He himself had experienced visions there - visions powerful enough to stop him from writing for years at a time (a major handicap for someone as garrulous as Beale).[27] Michael Leslie's account of this revelation highlights a brief enough incident in the evolving projects of the Hartlibians, but one that encapsulates as few others the spiritual, personal, and practical constitution of their hoped-for knowledge. That is the theme that distinguishes this book. It is anchored in place by the centrepiece of *Culture and cultivation*: a very elegant essay indeed by Timothy Raylor, recounting plans to exploit what Hartlib called 'the commonwealth of bees'. Raylor attends closely to actual farming practice, showing that Hartlib's enormously ambitious plan to create a bee-based economy was by no means unrealistic even at the level of the apiary itself. It is not only a model essay in its own right, but a fitting linchpin for the book as a whole.[28]

The other collection reflects more fully the broad range of interests held by those in Hartlib's circle. *Samuel Hartlib and universal reformation* derives from a major conference convened at Sheffield in 1992.[29] The book addresses not only the Hartlibians' projects for 'improvement', but their search for unity in both method and community, and the problems of communication they faced in their efforts. Its attention extends beyond the English Channel, and the need for a continental perspective becomes one of its more consistent and valuable messages. Howard Hotson's discussion of pedagogical practices in Germany, for example, includes much that will be unfamiliar and enlightening to British historians. Particularly useful, however, is Stephen Clucas's account of the difficulties the Hartlibians faced in formulating a 'logic' capable of organizing all the materials they had collected. Clucas makes explicit the ambiguities residing in the very concept of such a logic. Would it not constrain one's freedom of thought, restricting original ideas and imposing on the flexibility of civil conversation? It was a central question for Hartlib's contemporaries, but is one rarely raised by historians. Its answer had immediate and intimate consequences for the person of the investigator. Clucas

[26] J. Thirsk, 'Making a fresh start: sixteenth-century agriculture and the classical inspiration', and A. McRae, 'Husbandry manuals and the language of agrarian improvement', in Leslie and Raylor, eds., *Culture and cultivation*, pp. 15–34 and 35–62.

[27] G. Parry, 'John Evelyn as hortulan saint', M. Leslie, 'The spiritual husbandry of John Beale', and D. Chambers, '"Wild pastorall encounter": John Evelyn, John Beale and the renegotiation of pastoral in the mid-seventeenth century', in Raylor and Leslie, eds., *Culture and cultivation*, pp. 130–50, 151–72, and 173–94 (see esp. pp. 165–6).

[28] T. Raylor, 'Samuel Hartlib and the *commonwealth* of bees', in Leslie and Raylor, eds., *Culture and cultivation*, pp. 91–129.

[29] Greengrass, Leslie, and Raylor, eds., *Samuel Hartlib and universal reformation*. My comments here should be compared to those of Deborah Harkness in 'Entering the labyrinth: exploring scientific culture in early modern England', *Journal of British Studies*, 37 (1998), pp. 446–50.

https://doi.org/10.1017/S0018246X99008729 Published online by Cambridge University Press

argues that novel 'reading technologies' could be deployed to resolve this issue, through the adaptation of commonplacing and related techniques. These 'technologies' were already familiar to readers of Hartlib's time, and historians too are coming to realize the constitutive role they played in arriving at conclusions in arguments of all kinds.[30]

The disciplining of reading practices here offered a way of preserving freedom of thought while at the same time manifesting an organizing logic. It made reasoning, perhaps, into a kind of craft. Moreover, if disciplined commonplacing made thinking into a craft, then the Hartlibians' many projects sought to make craft into an extension of thinking. That was a major reason why issues of collaboration and confidentiality plagued the virtuosi: for those actually dependent on their craft skills, these were matters of livelihood. As William Eamon has demonstrated, virtuosi across Europe had developed an acute sense of the value of 'secrets' allegedly possessed by craftsmen and chymists. But getting these men to reveal their techniques was far from easy, and William Newman's essay here helps supply some much-needed texture to historians' overly broad notions of secrecy, openness, and authorship.[31] The projects for which the Hartlibians sought such assistance were astoundingly diverse, and each craft tended to maintain its own conventions of communication, collaboration, and concealment. Hartlib himself wanted to revitalize the economy through the farming of bees; Beale wanted to do the same by planting cider orchards. Hartlib's son-in-law, Frederick Clodius, laboured away at his furnaces in an effort at transmutation in Hartlib's own kitchen, while others in the 'invisible college' pursued ambitious schemes in medicine, mechanics, and mathematics. Their objectives ranged widely too, from the mundane to the providential. A man like Benjamin Worsley envisaged the transformation of knowledge and the 'improvement' of nature at once; the phenomena of magnetism offered a means to achieve both. An approach through personal practices of communication offers a useful way to understand both the difficulties facing such proposals and the labour of pursuing them.[32]

If these projects showed any unifying theme, it lay in the lifting of limits to the thinkable. In Hartlib's England, no certainties held. Criteria for discriminating the rational from the ridiculous were themselves likely to prove violently contentious. *Samuel Hartlib and universal reformation* ends with a fitting essay by the historian of medicine Mark Jenner that articulates the central question posed by such a predicament. Inevitably, perhaps, we now tend to view the plans of a Hartlib or an Evelyn somewhat charitably.

[30] H. Hotson, 'Philosophical pedagogy in reformed central Europe between Ramus and Comenius: a survey of the continental background of the "three foreigners"', and S. Clucas, 'In seach of "the true logick": methodological eclecticism among the "Baconian reformers"', in Greengrass, Leslie, and Raylor, eds., *Samuel Hartlib and universal reformation*, pp. 29–50 and 51–74. For the importance of commonplacing practices see esp. A. Blair, *The theater of nature: Jean Bodin and renaissance science* (Princeton, 1997), pp. 65–77, and A. Moss, *Printed commonplace-books and the structuring of renaissance thought* (Oxford, 1996), pp.269–75.

[31] W. Eamon, *Science and the secrets of nature: books of secrets in medieval and early modern culture* (Princeton, 1994); W. R. Newman, 'George Starkey and the selling of secrets', in Greengrass, Leslie, and Raylor, eds., *Samuel Hartlib and universal reformation*, pp. 193–210. Newman's chapter presents arguments now available in more extended form in W. Newman, *Gehennical fire: the lives of George Starkey, an American alchemist in the scientific revolution* (Cambridge, MA, 1994).

[32] C. Webster, 'Benjamin Worsley: engineering for universal reform from the invisible college to the navigation act', and A. Clericuzio, 'New light on Benjamin Worsley's natural philosophy', in Greengrass, Leslie, and Raylor, eds., *Samuel Hartlib and universal reformation*, pp.213–35 and 236–46. The range of these activities is captured in C. Webster, *The great instauration: science, medicine, and reform, 1626–1660* (London, 1975): see p. 303 for Clodius.

https://doi.org/10.1017/S0018246X99008729 Published online by Cambridge University Press

We attach them to the later successes of the Royal Society. But from a contemporary viewpoint it was far from clear that *any* of these schemes was worthwhile. For every Boylean plan to build an air-pump, there were dozens to clear a room of fleas by putting a bowl of warm horse-piss under the bed, or to create prodigiously powerful cranes small enough to fit in one's pocket. As Jenner rightly says, there is a hint of Hooke's later struggles for credibility in many of Hartlib's proposals – and Hartlib's exclusion from the ranks of the virtuosi after 1660 was complete enough to make Hooke look like an esteemed colleague. The question Jenner raises is thus once again the question of credibility. Why should anyone have believed any of Hartlib's promises?[33]

Trust, Hartlib's experience confirms, was central to the establishment of both learned communities and the knowledge those communities professed to hold. The Hartlib papers effectively constitute a long, exhausting record of two decades' attempts to secure belief in a diverse array of promises. All ultimately came to nothing. But they were not without consequences. After the depredations of the 1640s and early 1650s, Ireland became the sad testing-ground for many such projects. What in London or Herefordshire might be merely ridiculous was there tragic, and T. C. Barnard's account of how Gerard Boates's *Naturall history* 'slithered' from under Cromwell's conquest carries appropriate bite.[34] Trust, in this case, had real and severe effects. But the point is really a general one, extending far beyond the confines of Hartlib and his sometimes motley cohorts. It has become routine for historians of science to bemoan the lack of a coherent 'big picture' among the many micro-histories that today populate the field.[35] The quest for one may be futile, since it is hardly in the nature of academic disciplines to rid themselves of disagreement. But here the theme of trust runs through an array of enterprises and a range of characters sufficient to satisfy the most demanding historian of science. If any concept offers the opportunity to create a new 'big picture', then, it may well be that of credibility.

IV

Michael Hunter's *Science and the shape of orthodoxy* offers itself as one plausible candidate for a new 'big picture' around which the history of science could cohere. Hunter claims from the outset to be advancing a 'new theory of intellectual change' – one that is articulated in conscious opposition to some of the most influential interpretations developed during the last quarter-century, and presented as their plausible successor. The essays comprising the book, although written over a period of almost three decades, apparently exemplify this new theory.[36] Their most characteristic attribute is the repudiation, on two grounds, of what Hunter believes to be an excessively schematic historiography. First, and quite properly, he pays homage to complexity. Hunter attacks as 'unduly simplistic' many current representations of seventeenth-century

[33] M. Jenner, '"Another *epocha*"? Hartlib, John Lanyon and the improvement of London in the 1650s', in Greengrass, Leslie, and Raylor, eds., *Samuel Hartlib and universal reformation*, pp. 343–56.

[34] T. C. Barnard, 'The Hartlibian circle and the cult and culture of improvement in Ireland', in Greengrass, Leslie, and Raylor, eds., *Samuel Hartlib and universal reformation*, pp. 281–97.

[35] See the special issue of the *British Journal for the History of Science*, ed. J. Secord: 26, part 4 (1993).

[36] M. Hunter, *Science and the shape of orthodoxy: intellectual change in late seventeenth-century Britain* (Woodbridge, 1995), esp. introduction, pp. 11–18.

https://doi.org/10.1017/S0018246X99008729 Published online by Cambridge University Press

intellectual life. For example, he argues that Thomas Sprat's *History of the Royal Society* (1667), which Hunter concedes to be 'propagandist', cannot be taken as straightforward evidence of the Society's character, since within the Society itself there was continual disagreement about what that character was. Second, Hunter rejects what he calls 'a new polarization' in recent historical accounts, which has replaced the old struggle of Ancients against Moderns with an equally categorical war between conservative experimental philosophers and radical 'pagan naturalists'. He has a specific target in mind, in the shape of James and Margaret Jacob. And in his specific charges, he hits home. Hunter shows through half a dozen case studies – including one of John Evelyn – that it would be at least as difficult to fit most seventeenth-century figures into this new scheme as into the old.[37] His points, then, are to this extent well taken, and are unlikely to prove controversial.

For all his criticisms of the Jacobs, Hunter himself is not altogether untempted by the identification of polarized alternatives. It is his use of them that is different. He argues that such extremes did indeed exist, but only as 'idealized' positions. Few people adopted such a stance in practice, and no one at all did so with consistency over a long period. The actuality was composed of individuals forming their own diverse responses to these projections, on the basis of what they knew and felt at particular moments. Modern historians, in Hunter's eyes, have therefore chased after the wrong quarry. They have taken as their subjects the extremes constructed by seventeenth-century figures themselves in order to define their own philosophies, rather than analysing those philosophies themselves. So it is that histories have appeared of empiricism versus rationalism, radicalism versus conservatism, science versus magic, and words versus things. Even Steven Shapin and Simon Schaffer's *Leviathan and the air-pump* – easily the most influential work on early modern natural philosophy of the last twenty years, and far more sophisticated than the earlier work on which Hunter concentrates his fire – teeters on the edge of the trap, Hunter thinks. Shapin and Schaffer's own polarity of Hobbes versus Boyle declines to recognize that Hobbes represented no largely held position. He was an old man bereft of allies, and thus 'almost unique'.[38] In effect, Hunter charges his colleagues with falling for an early modern version of the favourite political tactic of our own day: triangulation.

In what sense is Hunter's a theory about intellectual *change*? The answer seems to be that it explains the dynamics of intellectual history by using its own process of triangulation. That is, intellectual change comes about when individuals chart new positions by distancing themselves from the various ideal-typical extremes perceived to exist in their culture. Change comes about, then, by the artful reconfiguration of existing resources. So it is that Hunter can and does propose polarities of his own. They include empiricism versus rhetoric, inductivism versus dogmatism, mechanism versus vitalism, Latitudinarianism versus Enthusiasm, and atheism versus orthodoxy. He is no less prone than other historians to think in such terms, and the polarities he uses are, in themselves, no more sophisticated. But his declared purpose is to write the history of individual responses to these polarities, not that of the polarities themselves. And this, he argues, is indeed a step forward. The argument evidently places great stress on the power of particular men and women to shape such individual responses. For that

[37] M. Hunter, 'John Evelyn in the 1650s: a virtuoso in search of a role', in *Science and the shape of orthodoxy*, pp. 67–98; see also introduction, pp.1–18.

[38] Hunter, *Science and the shape of orthodoxy*, introduction, pp. 13–14; S. Shapin and S. J. Schaffer, *Leviathan and the air-pump: Hobbes, Boyle, and the experimental life* (Princeton, 1985).

https://doi.org/10.1017/S0018246X99008729 Published online by Cambridge University Press

reason, Hunter's preferred mode is biography, and his preferred mood, 'ambivalence'. Hunter's new theory amounts to a call to pay close attention to what he calls 'the "bundle" of ideas which each individual formed' by reacting against constructed extremes. Individuals and idealizations come first, he insists. Then, and only then, would Hunter advocate moving to a collective analysis grouping the individuals together.[39]

The new theory Hunter recommends is thus an 'intensive' approach, for which the favoured idiom is the biographical case study. It is ultimately an uncontentious kind of method, and on the face of it rather traditional. But it has problems when excessive interpretative demands are made of it. That may explain why, even within *Science and the shape of orthodoxy* itself, only half the chapters adopt anything closely resembling the announced approach. The others are broader surveys – and sometimes surveys of surveys. Not a few, unfortunately, are old book reviews, the relevance of which to the purported theme is tenuous. At the more general level occupied by such discussions, Hunter's new theory loses much of its clarity. Its triangulation policy can then descend into a rather unsubtle rhetorical device for claiming the *via media* between alleged extremes of historiography – extremes that may be representative of positions genuinely held in the profession, but are necessarily articulated by Hunter himself. The result is frequently not a wrong interpretation, but an elementary truism that does inadequate justice to the quality of evidence and sensitivity of interpretation leading up to it. Hunter thus concludes at the end of one characteristically elegant assessment of primary sources that '[Christopher] Wren's intellectual milieu helps us to understand his choice of career'; after another, he arrives at the view that 'science and learning were not totally polarised'. More than one of the essays here finishes on such a note of *parturiunt montes*. Hunter's theory seems geared to produce bromides like these when it is applied outside the case study genre for which it was surely designed. As his own choice of essays tacitly admits, it is ill-suited to answering the broader questions that historians sometimes want to address.[40]

The salient question, however, is why Hunter feels the need to present himself as a theoretician in the first place. His real strengths lie in his archival knowledge and his empirical research skills, and in these respects he is simply unparallelled. Whether or not they really do exemplify his theory, it should be affirmed that the best of Hunter's essays are invaluable for their own sake. Finest of all is the piece on the Scottish 'atheist', Thomas Aikenhead, executed for his alleged blasphemy in 1697. In an interpretation of great subtlety, Hunter is able to dissect the fragmentary evidence for Aikenhead's views, the attitudes of his accusers, and the motives of his judges. He shows that 'practical' atheism of the kind that doomed this twenty-year-old student combined naturalistic ideas (such as eternalism), scepticism as to the source and authority of Scripture, and, perhaps most important, ribald public mockery of priests and priestcraft. His conclusion – which is reinforced in a second, more synthetic discussion of early modern heterodoxy – is that the prosecution was designed to attack not so much a set of heretical postulates as an attitude: a 'confident, unintellectual, common-sensical scepticism' that was both widely adopted by prosperous citizens and much denounced as leading to the erosion of Christianity. The fears raised by this style of conduct traversed political divisions, and were substantial enough that natural philosophy was called into the lists against

[39] Hunter, *Science and the shape of orthodoxy*, introduction, pp. 11–18.
[40] Ibid., pp. 65, 117, 134, 147, 179.

https://doi.org/10.1017/S0018246X99008729 Published online by Cambridge University Press

'practical atheism'. The Jacobs' influential argument that Boylean and Newtonian natural philosophies came to be promulgated in opposition to a consistent and potentially powerful political radicalism is here made to seem ill-founded and not a little romanticized. For Hunter, they were indeed weapons, but weapons directed against a social practice and a moral construct.[41] Nobody else can do this kind of reconstruction of past natural knowledge as skilfully as this, and its virtues exist irrespective of their relation to a specific theory of intellectual change.

It is possible that the reason why Hunter feels the need to appear in the guise of a theoretician may relate to the identity of the history of science itself. The field is a relatively young one. It was forged in the post-war years, and has never enjoyed consistently high status in its own right. It has always tended to exist in uneasy alliance with other, more or less proximate, approaches to the sciences – not to mention with the sciences themselves. Exactly which neighbouring approach has been deemed most relevant has varied. In the 1950s–70s, it was usually philosophy. At length, historians found themselves striving to create an autonomous legitimacy for their interpretations: they wanted to produce work that could still seem philosophically consequential, yet they also wished to insist that it be appraised by specifically historical criteria of quality, independent of its utility in a non-historical field. In the 1980s and 1990s, sociological and anthropological approaches have come to the fore, partly as a response to this perceived problem.[42] Oddly, it has remained comparatively rare for history of science to sustain a discrete disciplinary presence in university departments dedicated primarily to social, religious, or political history. For whatever reason, then, the history of science has never been securely sufficient unto itself. And this has created a corresponding imperative in its practitioners. They have found themselves constantly pressed to justify not just particular interpretations, but their very existence in the academic world. As a result, they are accustomed to appearing not just as practitioners, but as theorists of their own practices. And their disciplinary position has encouraged them to do this in ways unfamiliar to many historians working in more conventional university departments. It is a convention of reflexivity that has been efficiently passed on to each new generation of scholars. And Hunter's 'new theory of intellectual change', perhaps, is best seen as evidence of a continued pressure to follow that trend.

Steven Shapin is representative of the same imperative, but in another setting. Shapin is a sociologist by profession, with a background in the sociology of knowledge. His work on early modern natural philosophy is both elegant in its presentation and profound in its consequences. Historians may find the sociological idiom of *A social history of truth* strange on first acquaintance. They should persevere. Unlike most social scientists, who tend to write about the past only in order to provide premasticated fodder for their theories, Shapin knows the history of seventeenth-century England in great detail. Not

[41] M. Hunter, 'Science and heterodoxy: an early modern problem reconsidered', and '"Aikenhead the atheist": the context and consequences of articulate irreligion in the late seventeenth century', in *Science and the shape of orthodoxy*, pp. 225–44 and 308–32; see esp. pp. 243–4. (The dust-jacket actually gives a slightly different title, referring more modestly to *studies of* intellectual change in late seventeenth-century Britain.) Contrast J. R. Jacob and M. C. Jacob, 'The Anglican origins of modern science: the metaphysical foundations of the whig constitution', *Isis*, 71 (1980), pp. 251–67.

[42] These links are explored in J. V. Golinski, *Making natural knowledge: constructivism and the history of science* (Cambridge, 1998), and J. R. R. Christie, 'The development of the historiography of science', in R. C. Olby, G. N. Cantor, J. R. R. Christie and M. J. S. Hodge, eds., *Companion to the history of modern science* (London, 1990), pp. 5–22.

https://doi.org/10.1017/S0018246X99008729 Published online by Cambridge University Press

only that; he is also prepared to honour the historian's conviction that, as Oakeshott put it, 'to know only the gist is to know nothing'. The aim of his book is to present a historical argument about the fortunes of natural knowledge in early modern England, but one that will raise issues general enough to interest philosophers and sociologists. His subject, in general, is the conjunction of knowledge and social order. Specifically, Shapin claims that the success of experimental philosophy came about when norms of conduct traditionally appropriate to gentlemanly conversation were deliberately transposed into natural-philosophical practice. Or, to put it another way, philosophical practice was reformulated to make it recognizable as a form of polite conversation.[43]

Shapin's argument centres on trust. Experimental knowledge is often assumed to rest on personal experience. In fact, however, such immediacy is rare, and not infrequently impossible. Even when it does occur, an individual's assessments of what he or she is experiencing necessarily depend to some extent on prior knowledge, gained either by previous experiences or through the testimonies of others. No experience is an epistemic island. Even a decision to distrust it can only be taken by reference to a repertoire of experiences that *are* credited. Scepticism itself, often touted as a central feature of scientific virtue, is therefore parasitic upon trust. So a philosophy based on experiment had, in fact, to call upon the elemental forms of credit operating in the wider culture. And trust of that elemental sort is a profoundly social entity. In fact, it is quite possibly a necessity for any form of society to exist. Certainly, the bounds of trust are in effect the bounds of society itself, since anyone who exercised absolute scepticism would end up paying the absolute price, namely exclusion. On the other hand, the most trustworthy members of civil society are regarded as its mainstays. In early modern England, this meant gentlemen. Gentlemen may not in fact have been more truthful than any other participants in early modern society, and Shapin (*contra* some of his reviewers) never claims that they were. But honesty was a characteristic that was routinely attributed to them, and that was reckoned part and parcel of gentility itself. More specifically, the kind of conversation carried on by gentlemen was reckoned a model for civil society, in that disagreements were accommodated without descending into violence – and if violence did come to pass, it, too, was subject to norms. Truthful testimony was thus seen as a ready concomitant to gentility. And the role of gentility was therefore going to be central to the fortunes of experimental philosophy.

Who was then a gentleman? It was not necessarily easy to tell, especially by mid-century, when shopkeepers and tradesmen were eager to lay claim to the qualification 'gent'. Shapin himself claims that 'English culture as a whole from the Elizabethan age to the Augustan was more profoundly shaped by inquiries into the nature of gentility than by any other concern' – a remarkable statement about an age that witnessed civil war, regicide, and revolution, but not an incredible one.[44] The extensive historical literature on this topic yields no single definition, but a series of criteria by which one might decide in particular cases whether one's interlocutor qualified. Birth was one such criterion, although by no means a decisive one. Wealth was another. Learning, too, might qualify one to be a gentleman. But more basic were habits of prudence and person. A gentleman was, essentially, his own man. He was disciplined by self-control, but not by external constraints such as the need to earn wages (or, conversely, the possession of such exorbitant wealth that he was overwhelmed by commitments). He was therefore above all *disinterested*. With no material stake in any particular outcome,

[43] S. Shapin, *A social history of truth: civility and science in seventeenth-century England* (Chicago, 1994), pp. xvii–xix.

[44] Ibid., p. 56.

https://doi.org/10.1017/S0018246X99008729 Published online by Cambridge University Press

he could pronounce freely upon the arguments in a conversation. In sum, three kinds of judgement were in play in deciding whether a particular person qualified as a gentleman: a secular, chivalric criterion, with regard to blood, reputation, and honour; a humanist criterion, stressing virtue and classical codes of conduct; and a Christian criterion, that emphasized the systematic self-interrogation needed to rein in the passions and sanctify the soul. These being satisfied, a gentleman was then likely to be trusted *because* of his gentility. Reliance on one's word went with recognition of one's honour, virtue, and godliness. These, then, were to be the attributes of the new philosopher.[45]

This sorting-out of social kinds – which, as we have seen, went on continuously in early modern society – extended to characterizations of physical capacity. Gentlemen were normally ascribed what Shapin calls 'perceptual competence'. That is, they were regarded as capable of experiencing what was in the world to be experienced, as well as of reporting on it accurately and truthfully. Other humans might not share this characteristic. Artisans, especially, were liable to be portrayed as possessing hard, gnarly bodies – as less sensitive than gentlemen as well as less honest. Ways of life increased these differences. Manual labourers coarsened themselves as they worked. In Galenic terms, over time custom and climate became nature. Servants were thus liable to be treated as second-rate witnesses because they were both physically and socially compromised. Shopkeepers too were notorious for the 'trading lies' their businesses obliged them to tell. Artisans and alchemists traded in 'secrets', and guarded their confidentiality with understandable determination. Courtiers were compromised by the servility they must display in their places at court. And Jesuits, of course, owed their allegiance to the pope, who licensed them to lie even on the scaffold.[46]

As Bacon advised, gentlemen should not be absolutely truthful on all occasions. The best 'composition', he warned, was 'to have openness in frame and opinion; secrecy in habit; dissimulation in seasonable use; and a power to feign if there be no remedy'. In practice, all participants in society had to learn to read each others' faces and gestures for signs of equivocation. Conduct manuals explained at length the degrees of reserve and dissimulation that were appropriate in different circumstances. There was a huge grey area between absolute truth and the explosive accusation of lying, within which most polite conversation moved. Elaborate performances of scepticism, admiration, modification, and questioning took place in that area. Gentlemen developed an extensive repertoire of techniques for keeping the conversation going. These techniques, Shapin suggests, were what they then put to use to solve problems of evidence, testimony, and assent in matters of natural knowledge.[47]

Since the Renaissance, various new philosophies had created a vast number of potential 'facts', as they were increasingly being called. In future, some means of sifting the valuable from the spurious, redundant, or fraudulent would be required.[48] The Cartesians, like Clucas's Hartlibians, sought a 'logic' to achieve this. But the maxims of gentlemanly conversation provided another possible sieve. The Royal Society would make itself into a venue for experimental demonstrations before gentlemen witnesses. Such witnesses were conventionally regarded as likely to deliver disinterested observations, deserving of assent by reasonable readers. Their 'matters of fact' were not indubitable, nor unassailable; but when properly managed, they did attain a level of

[45] Ibid., pp. 63–4, 68, 73. [46] Ibid., pp. 83–100. [47] Ibid., pp. 114–25.
[48] This problem is graphically described in Shapin's more recent introductory text, *The scientific revolution* (Chicago, 1996), pp. 85–9.

https://doi.org/10.1017/S0018246X99008729 Published online by Cambridge University Press

'moral certainty' that was effectively obliging on rational, peaceable citizens. Such proper management took place through the deployment of 'maxims' of prudence that guided, but did not compel, the conversations surrounding and defining experimental practice. Shapin provides a catalogue of these maxims – *A social history of truth* is in large part a modern's instruction manual for interpreting early modern civil conduct – with examples illustrating their use in the pursuit of knowledge. Most remarkable is a case in which two gentlemen's observations of a comet could not be reconciled without concluding that one or the other had submitted a false figure; the Society, faced with destabilizing either the natural or the social order, meekly concluded that there must have been two different comets all along.[49]

The most important maxim of all – the only one virtually immune to conversational counter-measures – was the rule that one should vest trust in people of integrity and disinterest. This rule provided a practical means of closure. Quite simply, in seventeenth-century England experimental philosophy could reach conclusions because 'in certain sorts of people credibility was *embodied*'. By this stage, 'every man whose judgment is free from prejudice' would assent to those conclusions. People who persistently refused to concur when faced by such reasonable testimonies would by their very refusal exclude themselves from the civil commonwealth as 'prejudiced'. Among the 'prejudiced' were most of the tradesmen, sailors, artisans, and alchemists repeatedly importuned by the virtuosi to reveal their secrets. They proved intensely reluctant to do so, and for sound reasons. What to men like John Evelyn seemed a self-evidently worthwhile subjection to rational inquiry was to them an economically disastrous revelation of valuable and proprietary techniques. The list with which this article began is thus a testament, both by its own structure and by its eventual futility, to the strength of the distinctions, simultaneously both social and epistemic, that Shapin addresses.[50]

One reason for the manifest differences between Shapin and Hunter is that they are explaining different things. Shapin's target is a practical enterprise: he wants to understand what one may call the 'life of science', and in seeking that end he illuminates social life in general. Hunter's is at its core intellectual: he wants to understand 'bundles of ideas'. Practical regimes do loom large in his work, but as context, not content. The contrast is not insignificant. It marks a fundamental shift in subject and approach. It currently seems that all works on seventeenth-century English natural philosophy are doomed to reach a similarly awkward juxtaposition of the intellectual and the practical. That is, they all conclude with the mysterious power of the experiment. Experiments were practical displays, performed before credible witnesses, with intellectual consequences. As elements in a conversation, they entertained, and in entertaining they exemplified a polite way of learning that could underpin a more stable society. In an age of all but unlimited contention, experiments alone proved persuasive. Roger French captures their effectiveness very well in describing a spectacular vivisection designed to reveal the structure of the heart. 'Its unlooked-for result allowed no dissent', he explains: 'there is a conviction arising from being covered in blood that recognizes no philosophical opposition'.[51] Trying to discern the sources of such conviction is a key part of the historian's enterprise in examining early modern natural knowledge in the making. Figuratively, it means reconciling the perspectives of Shapin and Hunter, and

[49] Shapin, *A social history of truth*, pp. 266–91. [50] Ibid., pp. 193–242, esp. 209, 238.

[51] French, *William Harvey's natural philosophy*, pp. 159–60. Compare Pamela Smith on the alchemist who displayed a transmutation before sceptical scholastics, relying on 'the creative, productive *action* of raw *ars*' to win his case: Smith, *The business of alchemy*, p. 45.

https://doi.org/10.1017/S0018246X99008729 Published online by Cambridge University Press

making intellectual history into a history of social practices. That is why the leading candidates for a new 'big picture' bring us back to the problem that confronted John Evelyn in 1652. We are again in Evelyn's position of having to classify human enterprises. We may eventually arrive at a new taxonomy of early modern natural knowledge, and that taxonomy may rival Evelyn's own for strangeness.

https://doi.org/10.1017/S0018246X99008729 Published online by Cambridge University Press