The Emergence of a Scientific Culture

Science and the Shaping of Modernity, 1210–1685

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Science and Modernity

> What is the significance of science viewed as a symptom of life? . . . Is the resolve to be so scientific about everything perhaps a kind of fear of, and escape from, pessimism? A subtle last resort against—truth? and, morally speaking, a sort of cowardice and falseness?
>
> Friedrich Nietzsche, *Die Geburt der Tragödie*[1]

One of the most distinctive features of the emergence of a scientific culture in modern Europe is the gradual assimilation of all cognitive values to scientific ones. This is not merely a distinctive feature of Western scientific practice, it is a distinctive feature of Western modernity: a particular image of the role and aims of scientific understanding is tied up in a fundamental fashion with the self-image of modernity. A striking illustration of this is the way that the West's sense of what its superiority consisted in shifted seamlessly, in the early decades of the nineteenth century, from its religion to its science.[2] As late as 1949, Herbert Butterfield, in his influential *The Origins of Modern Science*, could argue that civilized ideals, which had previously been transmitted by Christianity, were now transmitted by science, and that Christianity had evolved into a new secular science of faith.[3] While what is at issue would perhaps no longer be put in quite these terms, it remains the case that, over the last fifty years, a particular image of the role and aims of scientific understanding has been promoted by the West, and internalized by its recipients, as an essential element in the process of modernization.

A crucial ingredient in the plausibility and success of this notion has been the idea that science, by contrast with religion for example, appeals solely to reason and experience, and is as a consequence untinged by historical or cultural factors, which can therefore be ignored, making science something that in essence has no context, historical or otherwise. Science is thereby protected in advance from the historicization and contextualization that, coming to a head in the middle of the nineteenth century,

[1] Friedrich Nietzsche, *Basic Writings of Nietzsche*, trans. and ed. Walter Kaufman (New York, 1968), 18.

[2] See Michael Adas, *Machines as the Measure of Man: Science, Technology, and Ideologies of Western Dominance* (Ithaca, NY, 1989). Cf. Lewis Pyenson, *Cultural Imperialism and Exact Sciences: German Expansion Overseas, 1900–1930* (New York, 1985); idem, *Empire of Reason: Exact Sciences in Indonesia, 1840–1940* (Leiden, 1989); idem, *Civilizing Missions: Exact Sciences and French Overseas Expansion, 1830–1940* (Baltimore, 1993).

[3] See the discussion in Regis Cabral, 'Herbert Butterfield (1900–79) as a Christian Historian of Science', *Studies in History and Philosophy of Science* 27 (1996), 547–64.

eventually undermined Christianity's claims to *sui generis* legitimacy. The problem is magnified by the cultural standing that science has taken on in virtue of this image. In particular, the notion of science as something answerable to nothing but reason and experience has done much to encourage the otherwise somewhat unlikely association between scientific values, morality, and democracy.

This association began in earnest with the Darwinism debates of the late nineteenth century, and it became a dominant cultural theme in the twentieth century. In the Anglophone world, this development starts with Herbert Spencer, who set out explicitly to derive ethical principles from scientific ones,[4] and from the late ninteteenth century onwards there have been recurrent attempts to guide morality scientifically. In 1916, for instance, Richard Gregory, the editor of *Nature*, singled out the scientific values of selflessness and love of truth to act as the basis for morality.[5] He was followed in 1923 by the contributors to the volume *Science and Civilization*, who called for moral values based upon science to replace those based on religion, with Julian Huxley's contribution identifying the next great task of science as the creation of a new religion.[6] By 1931, the science columnist John Langdon-Davies was taking up the defence of the moral values of science with an attack on the use by religion of emotionally loaded words to describe abstract concepts.[7] At the same time, the Vienna Circle had decided that the best people to do philosophy were not philosophers but scientists. Reminiscing about his time with the Circle in 1926–31, Carnap tells us:

> The task of fruitful collaboration, often so difficult among philosophers, was facilitated in our Circle by the fact that all members had a first-hand acquaintance with some field of science, either mathematics, physics or social science. This led to a higher standard of clarity and responsibility than is usually found in philosophical groups, particularly in Germany. . . . The common spirit was one of co-operation rather than competition. The common purpose was to work together in the struggle for clarification and insight.[8]

That such an approach is not restricted to an outdated positivism is clear, for example, from Barrow and Tipler's more recent announcement that:

[4] See e.g. Herbert Spencer, *The Principles of Ethics* (2 vols, New York, 1892), i, pp. xv–xvi.

[5] Richard Gregory, *Discovery: Or the Spirit and Service of Science* (London, 1916). Gregory was assistant editor of *Nature* from 1893 and editor 1919–39. See the discussion in Peter J. Bowler, *Reconciling Science and Religion: The Debate in Early Twentieth-Century Britain* (Chicago, 2001), 68–70.

[6] Julian Huxley, 'Science and Religion', in F. S. Marvin, ed., *Science and Civilization* (Oxford, 1923), 279–329: 279. See the discussion in Bowler, *Reconciling Science and Religion*, 68–75, from which I draw the examples here.

[7] See John Langdon-Davies, 'Science and God', *The Spectator*, 31 January 1931, 137–8.

[8] Rudolph Carnap, 'Intellectual Autobiography', in Paul Arthur Schilpp, ed., *The Philosophy of Rudolph Carnap* (La Salle, Ill., 1963), 3–84: 21. On the Logical Positivist approach to ethics, see Moritz Schlick, *Problems of Ethics* (New York, 1939). The earliest statement of the idea that scientists (in this case mathematicians) were best placed to pursue the humanities and *belles lettres* is Fontenelle's 1699 claim, in the Preface to his *Histoire de renouvellement de l'Académie Royale des Sciences en mdcxcix*: 'The geometrical spirit is not so attached to geometry that it cannot be taken and applied to other knowledge. A work of morals, politics, and criticism, perhaps even of rhetoric, would be improved, other things being equal, if written by a geometer': Bernard le Bovier de Fontenelle, *Œuvres de Monsieur de Fontenelle . . . nouvelle édition* (10 vols, Paris, 1762), v. 12.

Whereas many philosophers and theologians appear to possess an emotional attachment to their theories and ideas which requires them to believe them, scientists tend to regard their ideas differently. They are interested in formulating many logically consistent possibilities, leaving any judgement regarding their truth to observation.[9]

There is a moral dimension to this view of the standing of science. In the USA during the 1930s and 1940s, for example, scientific values were contrasted with those of facism, communism, Catholicism, and McCarthyism in particular. While Charles Morris was identifying the strength of pragmatism as lying in the fact that 'it is essentially the marriage of the scientific habit of mind with the moral ideal of democracy',[10] Robert Merton was explicitly setting out to establish the correspondence between scientific ideals and those of democracy,[11] and the Yale social scientist Mark A. May was proposing a 'morality of science' as a basis for world culture, whereby everyone would eventually live by the code of the scientist, which consisted in a devotion to honest, free, critical, evidence-based enquiry.[12] It is worth reminding ourselves that at the time that May was writing, many scientists were not only failing to show themselves as more moral than anyone else in the population, but being a good deal less moral, willing and occasionally enthusiastic collaborators in barbaric atrocities.[13] Yet this did not prevent an uncritical idea of the 'morality of science' being taken up again after the Second World War by Richard Hofstadter and Walter Metzger, in their 1955 attack on McCarthyism, as the theoretical foundation of academic freedom for all disciplines, including the humanities.[14] In 1957 we find a member of the Mental Health Research Institute at the University of Michigan arguing that 'the ethical system derived from scientific behaviour is qualitatively different from other ethical systems—is, indeed, a "superior" ethical system'.[15]

But the connections between scientific and ethical progress are in fact at best fragile, a fragility manifest in a revealing way in the development of a 'scientific', i.e. laboratory-based, medicine. In the 1940s, two great successes

[9] John D. Barrow and Frank J. Tipler, *The Anthropic Cosmological Principle* (Oxford, 1986), 15.

[10] Charles Morris, *Pragmatism and the Crisis of Democracy* (Chicago, 1934), 8.

[11] See his essay 'The Normative Structure of Science' (originally entitled 'A Note on Science and Democracy') in Robert Merton, *The Sociology of Science*, ed. N. Storer (Chicago, 1973). See the discussion in David A. Hollinger, 'The Defense of Democracy and Robert K. Merton's Formulation of the Scientific Ethos', *Knowledge and Society* 4 (1983), 1–15.

[12] See David A. Holliger, 'Science as a Weapon in *Kulturkämpfe* in the United States During and After World War II', *Isis* 86 (1995), 440–54: 442.

[13] See e.g. Benno Müller-Hill, *Murderous Science: Elimination by Scientific Selection of Jews, Gypsies, and Others, Germany 1933–1945* (Oxford, 1988); John Cornwall, *Hitler's Scientists: Science, War, and the Devil's Pact* (New York, 2003); Daniel Barenblatt, *A Plague upon Humanity: The Hidden History of Japan's Biological Warfare Program* (New York, 2005). Cf. Edwin Black, *War Against the Weak: Eugenics and America's Campaign to Create a Master Race* (New York, 2001).

[14] Richard Hofstadter and Walter Metzger, *The Development of Academic Freedom in the United States* (New York, 1955). See the discussion in Holliger, 'Science as a Weapon in *Kulturkämpfe*', 447.

[15] Anatol Rapoport, 'Scientific Approach to Ethics', *Science* 150 (1957), 796–9: 797. The call for a 'scientific ethics' may have diminished since the 1950s, but it has certainly not disappeared: see e.g. Michael Ruse and Edward O. Wilson, 'Moral Philosophy as Applied Science', *Philosophy* 61 (1986), 173–92, and the reply by Antony Duff, 'Moral Philosophy as Applied Science?', *Philosophy* 63 (1988), 105–10.

in pharmaceuticals—the discoveries of penicillin and cortisone—helped fuel an enthusiasm for scientific medicine. In some respects this was ironic, because neither of these was the outcome of any scientific programme: they were 'gifts of nature', discovered wholly by accident under highly contingent and unlikely circumstances.[16] Nevertheless, there were clearly fundamental consequences for medicine. Penicillin, a naturally occurring non-toxic compound, along with the other antibiotics that followed in its wake, cured many lethal and chronic infections at a stroke, and suggested limitless possibilities for medicine pursued in the laboratory.[17] In the wake of these developments, various attempts were made to put medicine on a 'scientific' footing. From the 1960s onwards, clinical trials were well funded, and they dominated the field: when they failed, as they routinely did, for example in the attempts to extend chemotherapy to a wide range of cancers, researchers told themselves they had not got the mix quite right, and tried again on a new set of patients, only to fail again. As Le Fanu notes, 'the results were predictably appalling, with those receiving chemotherapy dying more rapidly and with much worse quality of life than those receiving no therapy'.[18] The divide between clinical and scientific approaches to medicine became an issue of public concern with the publication in 1967 of Maurice Pappworth's *Human Guinea Pigs*, in which Pappworth—the author of a standard clinical textbook and a defender of the diagnostic superiority of clinical skills over the tests and trials pursued by proponents of scientific medicine—while not doubting the huge advances that had been made in synthetic chemistry over the previous thirty years, convicts the medical profession of ruthless, cruel, dangerous, and often purposeless experiments on infants, pregnant women, the mentally ill, prisoners, and the old and dying, subjecting them to what is in effect a form of torture.[19] It is not possible to dismiss this episode as one concerning the misuse of science, rather than being about science itself, for the procedures followed not only were those prescribed by the most conservative canons of scientific

[16] See James Le Fanu, *The Rise and Fall of Modern Medicine* (London, 1999), Pt I.

[17] In fact, not only did hopes that the drug might be synthesized chemically turn out to be unfounded (despite a huge amount of well-funded research) but no understanding of how antibiotics work has been developed. The commonly accepted explanation that they are 'chemical weapons' produced by bacteria to maximize their chances of survival is quite false, and Selman Waksman, who was awarded the Nobel prize for his discovery of streptomycin, concluded that they were a 'purely fortuitous phenomenon', that 'there is no purposeness behind them', and that 'the only conclusion that can be drawn from these facts is that these microbiological products are accidental': quoted ibid. 15.

[18] Ibid. 156. He continues: 'The blindness of oncologists to what they were doing is well exemplified by a 1983 report claiming that chemo was no more toxic to the elderly than to the young, so they should receive chemo at maximum doses. Curiously the author of this report . . . felt it unnecessary to make any reference to the results of treatment, where only 20% of elderly patients have any response to treatment. . . . In Britain Tim McElwain of London's Royal Marsden Hospital commented on "the confusion of busyness with progress . . . with nasty drugs being thrown at unfortunate patients with very little evidence of gain".'

[19] See ibid. 204–5. Cf. the report of Peter Medawar's view that the real science in medicine is the thorough understanding of the medical problem that comes from talking to the patient and performing a physical examination, from which it is possible to infer what is wrong in 90% of cases, by contrast with 'the technological gizmos and the arcane tests that pass for the "science" of medicine, which are frequently wrong' (ibid. 253).

method, but also yielded benefits in some cases. The episode effectively put an end to any notion of some intrinsic, superior 'scientific morality',[20] but it also raises the issue of the appropriateness of exporting procedures from one discipline to another, and indicates that the appropriateness in question can be complex, involving ethical as well as technical issues, and highlighting concerns about how we compare very different kinds of scientific practice each of which might be successful in some areas but not others.

One lesson to be learned from the 'scientific medicine' episode is that the question of the unity of science is highly overdetermined, and any attempt to pose it as if it were a purely abstract question is going to be fruitless.[21] The issues are not confined to medicine, and open up the contentious question of the extra-scientific sources of and motivation behind the notion of the unity of science. In mid-nineteenth century Germany, for example, scientists such as Virchow, DuBois Reymond, and Helmoltz explicitly saw the unification of science and the unification of the German state as being indissolubly linked, and nearly a century later Dewey was talking of the unity of science in political terms as a bulwark against intolerance.[22] This is an issue that has a very significant practical dimension for, just as in medicine, the future funding of a range of scientific disciplines depends on decisions made about the unity of science: the debates over the building of an extremely expensive supercollider in the USA in the 1990s rested in part on whether condensed matter physics was, as its advocates argued, autonomous from the guiding principles of particle physics, or whether all physics in some way follows from the fundamental laws of particle physics, which should therefore be given priority (in this case something approaching carte blanche) in funding.[23] The fundamental question of the 'unity of science' is one that we shall be turning to in a number of contexts. Its primary importance for us lies in the fact that any attempted assimilation of cognitive values generally to scientific ones works on the assumption that science is unified.[24] If science is simply a loose grouping of

[20] Ethics committees to oversee scientific and medical research, committees that included members from outside science and medicine, were finally established around this time.

[21] See Ian Hacking, 'The Disunities of the Sciences', in Peter Galison and David J. Stump, eds, *The Disunity of Science* (Stanford, 1996), 37–74; and Philip Kitcher, 'The Ends of the Sciences', in Brian Leiter, ed., *The Future for Philosophy* (Oxford, 2004), 208–29.

[22] See Peter Galison, 'Introduction: The Context of Disunity', in Peter Galison and David J. Stump, eds., *The Disunity of Science* (Stanford, 1996), 1–33: 1–8. On the German case, see e.g. Timothy Lenoir, 'Social Interests and the Organic Physics of 1847', in Edna Ullmann-Margalit, ed., *Science in Reflection* (Dordrecht, 1988), 169–81. In the case of Dewey, see his 'Unity of Science as a Social Problem', in Otto Neurath, Rudolph Carnap, and Charles Morris, eds, *Foundations of the Unity of Science* (2 vols, Chicago, 1970), i. 32–3.

[23] Galison, 'Introduction: The Context of Disunity', 2.

[24] In the last century, this has been most explicit and most marked in the writings of the Logical Positivists. The flavour of the project is well captured in Otto Neurath, 'Unified Science as Encyclopedic Integration' (first published 1938), in Otto Neurath, Rudolph Carnap, and Charles Morris, eds, *Foundations of the Unity of Science* (2 vols, Chicago, 1970), i. 1–27. Even this unification project was not straighforward, however: see Richard Creath, 'The Unity of Science: Carnap, Neurath, and Beyond', and Jordi Cat, Nancy Cartwright, and Hasok Chang, 'Otto Neurath: Politics and the Unity of Science', both in Peter Galison and David J. Stump, eds., *The Disunity of Science* (Stanford, 1996), 158–69 and 347–69 respectively. On the cultural transformation of

disciplines with different subject matters and different methods, tied in various ways each of which work for some purposes but not for others,[25] then there can be no modelling of cognitive values generally on scientific ones. As James Clerk Maxwell—with Newton the greatest unifier of physical theory that physics has known—remarked in an 1856 paper:

> Perhaps the 'book', as it has been called, of nature is regularly paged; if so, no doubt the introductory parts will explain those that follow, and the methods taught in the first chapters will be taken for granted and used as illustrations in the more advanced parts of the course; but if it is not a 'book' at all, but a magazine, nothing is more foolish to suppose than that one part can throw light on another.[26]

A century later, one of the other great unifiers of physical theory, Paul Dirac, suggested that the appropriate goal in making fundamental connections between disciplines in physics was that of removing inconsistencies, not attempting to unite theories that were previously disjoint. The former, he argued, led to brilliant successes, as in Maxwell's investigation of an inconsistency in the electromagnetic equations, Planck's resolution of inconsistencies in the theory of black-body radiation, Einstein's resolution of inconsistencies between his theory of special relativity and the Newtonian theory of gravitation. By contrast, the top down method of attempting to unify physical theories that were previously disjoint had produced nothing of significance.[27]

Since the assumption of the unity of science underlies not only reductionist programmes, but the assimilation of cognitive disciplines to science, and/or their modelling on science, it inevitably plays a key role in understanding how science has taken on a particular foundational standing. My aim in what follows is to examine the origins of—and examine the rationale for—a self-image of science whereby it purports to serve as a model for all forms of purposive behaviour, providing cognitive norms for everything from morality to philosophical dispute,[28] from

Logical Positivism from a model of a culturally and socially engaged philosophy (in which the unity of science plays an absolutely central role) to a model of philosophy as a narrow technical theory of induction, inference, and semantics see the comprehensive and intriguing account of the development of Logical Positivism in George A. Reisch, *How the Cold War Transformed Philosophy of Science: To the Icy Slopes of Logic* (Cambridge, 2005). Reisch's book builds in part on the picture sketched in Peter Galison, 'Aufbau/Bauhaus: Logical Positivism and Architectual Modernism', *Critical Inquiry* 16 (1990), 709–52.

[25] For an account of how this works in contemporary microphysics, see Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago, 1997).

[26] James Clerk Maxwell, 'Are There Real Analogies in Nature?', in Lewis Campbell and William Garnett, *The Life of James Clerk Maxwell, with a Selection from his Correspondence and Occasional Writings and a Sketch of his Contributions to Science* (London, 1882), 235–44: 243.

[27] Quoted in Hacking, 'The Disunities of the Sciences', 54.

[28] See e.g. Hans Reichenbach, *The Rise of Scientific Philosophy* (Berkeley, 1951), and the discussion in Ronald N. Giere, 'From *wissenschaftliche Philosophie* to Philosophy of Science', in idem, *Science Without Laws* (Chicago, 1999), 217–36.

political organization[29] to religion,[30] and even being considered part of the process whereby we progress to the next stage of evolution.[31] We shall see that these origins were forged in the debates over the purpose and standing of natural philosophy in the early-modern era, although we cannot understand the novel developments of the late sixteenth and early seventeenth centuries unless we understand the profound transformation of Western philosophical culture of the thirteenth century. In particular, we shall see that the distinctive feature of the Aristotelianism that was introduced then was that it made natural philosophy the point of entry into the whole philosophical enterprise. This included the role of providing foundations for a systematic theology, thereby giving natural philosophy a cognitive priority that was to become one of the central features of early-modern scientific culture. In sum, from the thirteenth century onwards in the West, the understanding of natural philosophy was transformed from a wholly marginal enterprise into the unique model for cognitive enquiry generally. It is with this phenomenon, on which hinge some of the central questions of modernity, that we shall be concerned. Our first task will be to reflect on what exactly needs explaining and why.

THE ENLIGHTENMENT INTERPRETATION

Since classical antiquity, there have been a number of civilizations that have witnessed a form of 'scientific revolution': rich, productive scientific cultures in which fundamental and especially intractable mathematical, physical, medical, astronomical, or other problems are opened up and dealt with in an innovative and concerted fashion, producing cumulative results over several generations. Among these,[32] we can

[29] See e.g. J. D. Bernal, *The Social Function of Science* (London, 1939) and, at the other end of the political spectrum, Karl R. Popper, *The Open Society and its Enemies* (2 vols, London, 1945), and *The Poverty of Historicism* (London, 1957).

[30] See e.g. Julian Huxley, *Religion without Revelation* (London, 1927). The idea of a scientific basis for religion underwent many transformations in the twentieth century. Note Paul Davies' claim that 'science offers a surer path to God than religion . . . science has actually advanced to a point where what were formerly religious questions can be seriously tackled' (*God and the New Physics* (Harmondsworth, 1983), p. ix) and Stephen Hawking's claim that he expects cosmological theory to tell us 'why it is that we and the universe exist. If we find the answer to that, it would be the ultimate triumph of human reason—for then we would know the mind of God' (*A Brief History of Time* (London, 1988), 175). The view that the new physics made a religious view of the world possible goes back as far as Eddington: see Bowler, *Reconciling Science and Religion*, ch. 3. There is competition from biology, however: see Richard Dawkins' claim that, with the advent of modern biology, 'we no longer have to resort to superstition when faced with the deep problems; Is there a meaning to life; What are we for? What is man?' (*The Selfish Gene* (London, 1978), 1).

[31] Popper maintained that the evolution of the argumentative function of language, because 'it has led to the evolution of science', 'has created what is perhaps the most powerful tool for biological adaption which has ever emerged in the course of organic evolution': Karl Popper, *Objective Knowledge* (London, 1972), 237.

[32] Cases where there were significant scientific developments, but where one might question whether there was something that could be identified as a scientific revolution include Mesopotamia, Egypt, India, Japan, and Mayan civilization. On these see the topical essays in vol. xv of Charles Coulton Gillespie, ed., *Dictionary of Scientific Biography* (New York, 1981), especially: David Pingree, 'History of Mathematical Astronomy in India', 533–633; B. L. van der Waerden,

include classical Greece and the Hellenistic Greek diaspora;[33] Arab-Islamic North Africa/Near East/Iberian peninsula in the ninth, tenth, and eleventh centuries;[34] thirteenth- and fourteenth-century Paris and Oxford;[35] and China from the twelfth to the fourteenth century.[36]

The scientific revolution with which we shall be concerned—*the* Scientific Revolution—is quite different from these. It is sometimes asked why the Scientific Revolution occurred in the West in the modern era and not, say, in China, or medieval Islam, or medieval Paris or Oxford. But it is the Scientific Revolution that requires explanation, not these developments: what is peculiar and exceptional is the nature of scientific development in the West in the modern era.[37] Scientific developments in the classical and Hellenistic worlds, China, the medieval Islamic world, and medieval Paris and Oxford, share a distinctive feature. They each exhibit a pattern of slow, irregular, intermittent growth, alternating with substantial periods of stagnation, in which interest shifts to political, economic, technological, moral, or other questions. Science is just one of a number of activities in the culture, and attention devoted to it changes in the same way attention devoted to the other features may change, with the result that there is competition for intellectual resources within an overall balance of interests in the culture.

The 'Scientific Revolution' of the early-modern West breaks with the boom/bust pattern of all other scientific cultures, and what emerges is the uninterrupted and cumulative growth that constitutes the general rule for scientific development in the West since that time. The traditional balance of interests is replaced by a dominance of scientific concerns, while science itself experiences a rate of growth that is pathological by the standards of earlier cultures, but is ultimately legitimated by the cognitive standing that it takes on. This form of scientific development is exceptional and anomalous. The question is, then, not why the Scientific Revolution didn't occur in any of the other cases of rich, innovative scientific cultures, but why it occurred in the West. The core issue here is this: how was scientific practice in the West so

'Mathematics and Astronomy in Mesopotamia', 667–80; Richard A. Parker, 'Egyptian Astronomy, Astrology, and Calendrical Reckoning', 706–27; Shigeru Nakayama, 'Japanese Scientific Thought', 728–58; Floyd G. Lounsbury, 'Maya Numeration, Computation, and Calendrical Astronomy', 759–818.

[33] See G. E. R. Lloyd, *The Revolutions of Wisdom* (Berkeley, 1987).

[34] See Roshdi Rashed, ed., *Encyclopedia of the History of Arabic Science* (3 vols, London, 1996).

[35] See Marshall Clagett, *The Science of Mechanics in the Middle Ages* (Madison, 1959).

[36] See Joseph Needham, *Science and Civilisation in China* (7 vols in 50 sections, Cambridge, in progress, 1954–).

[37] This was first pointed out in Joseph Ben-David, *The Scientist's Role in Society* (Chicago, 1984). See also Nathan Sivin, 'Why the Scientific Revolution Did Not Take Place in China—Or Didn't It?' in E. Mendelsohn, ed., *Transformation and Tradition in the Sciences* (Cambridge, 1984), 531–54. Similar considerations hold in the case of technological development: intensive bursts of technological innovations are both rare and isolated, and there is no reason to suppose that technological innovation will continue if unimpeded. In this case, what needs explaining is the peculiarity of the Industrial Revolution of the late eighteenth/early nineteenth century in Western Europe: see Joel Mokyr, *The Lever of Riches: Technological Creativity and Economic Progress* (New York, 1990).

transformed in the course of the modern era that it was able to establish cognitive priority for itself, so that it was able to shape other cognitive values around its own?

To help orientate ourselves, it is worth noting that, in one sense, this mode of development is compatible with Kuhn's model of scientific development in terms of the emergence of new paradigms, which usher in periods of intense scientific activity.[38] But in another sense, it is antithetical to Kuhn's account, for what it suggests is that the development of science in the West since the sixteenth century follows a pattern wholly different from that of any other scientific culture. I know of no concerted attempt to investigate this phenomenon. But since such an investigation is precisely what we shall be setting out to provide, it will be helpful, in understanding the approach that I shall be taking, to have some alternative account with which to contrast it. I suggest such an alternative account is to be found in an implicit general thesis underlying much traditional writing in the history of science and in the philosophy of science which, if it were an accurate account of what was at stake, would explain the peculiarity of scientific development in the West. The core claim of the thesis, as I shall reconstruct it, is that science in the early-modern era was so spectacularly successful that not only could it displace competing accounts, but it was able to extrapolate from the method by which it achieved these fundamental results to all cognitive domains. The first thing to do is to flesh this thesis out a little, and in the process give some account of why it has been thought to have such a plausible ring to it.

Let us start with some historiographical flesh, for it is from this that a good deal of the plausibility of the thesis derives. From the point of view of understanding science as a cultural product, the two most formative scientific events of the modern era have generally been taken to be Copernicanism and Darwinism, the first marking the beginning of this era more decisively than any other scientific development, the latter marking the transition from one cultural idiom to another—very different—cultural idiom. The triumphs of Copernicanism and Darwinism, as they are usually construed, were twofold. In the first place they were successful in the face of fierce opposition from established religion. In the second, they replaced firmly held philosophical views that had persisted since antiquity, and which had the authority of two millennia. If we think of Copernicanism as marking the beginning of the struggle with non-scientific disciplines, and Darwinism marking the start of the final stage of this struggle, then it is tempting to think of their triumphs as indicating the *sui generis* nature of scientific values. That is, what they seem to indicate is that, unlike the cognitive values and norms of theology or the humanities, basic scientific values and norms are open to no refutation from outside.

A fundamental question arises here. If there is something unique about the Scientific Revolution that marks it out from other transformations of scientific cultures,

[38] See Thomas Kuhn, *The Structure of Scientific Revolutions* (2nd edn, Chicago, 1962). This kind of approach had been anticipated independently by Ludwig Fleck, *Entstehung und Entwicklung einer wissenschaftlichen Tatsache* (Basle, 1935) and by Gaston Bachelard in *Le Nouvel Esprit scientifique* (Paris, 1934) and *La Formation de l'esprit scientifique* (Paris, 1938). On Bachelard see Stephen Gaukroger, 'Bachelard and the Problem of Epistemological Analysis', *Studies in History and Philosophy of Science* 7 (1976), 189–244.

is this because its practitioners hit upon the only really successful way of pursuing science, or is it that, owing to a variety of contingent reasons, it was able to present its own model of scientific practice (or perhaps an idealized version of it) as the only viable one? I shall use the term 'the Enlightenment Interpretation' for the view that holds that what marks the Scientific Revolution out from other transformations of scientific cultures is that its practitioners hit upon a uniquely successful way of pursuing science, and that the scientific practice that was produced in the Scientific Revolution represents the only way in which scientific practice could have developed with any long-term viability.[39] On the Enlightenment Interpretation, there are two features of scientific development since the Scientific Revolution that are identified as distinctive, and mark it out from other scientific programmes, especially its medieval predecessors. These are its autonomy and its method.

In its most straightforward form, the claim with respect to autonomy is that, unlike medieval natural philosophy, for example, seventeenth-century science gradually breaks free of religious considerations and follows an autonomous path. The claim with respect to method is that the distinctive feature of this autonomous path is a method of investigation that is quantitative and empirical, as a result of which it is able to produce results of lasting value in a way that its medieval predecessors were not. This twofold process of securing autonomy from manifestly inappropriate considerations that are independent of physical evidence, and the establishment of an appropriate and viable method of producing reliable results, then opens up the way for a consolidation of scientific results which marks the scientific enterprise out from other forms of enquiry. This gives us another feature of the Enlightenment Interpretation: that it is not just its remarkable consolidation of results, but the very fact that it is capable of such consolidation, that marks modern scientific practice out from other enterprises and sets new standards of cognitive success by which disciplines that purport to make advances in our knowledge must be judged.

In reflecting on the adequacy of this view, it is crucial that we begin by distinguishing clearly between the kind of factors that might have played a part in the emergence of the Scientific Revolution, and those that might have played a part in its consolidation. In the Enlightenment Interpretation, there is an implicit assumption that a satisfactory account of the former is a satisfactory account of the latter, that the story about the establishment of Copernicanism from Kepler and Galileo onwards, culminating in Newton's *Principia*, for example, explains the subsequent consolidation of science, because these developments finally point us in the right direction—in

[39] We shall be dealing with some specific versions, and specific claims, of the Enlightenment Interpretation, but as a general indication of its role as the default position, at least before the influence of Kuhn began to be felt, some of the following general histories may be taken as indicative: Edwin Arthur Burtt, *The Metaphysical Origins of Modern Physical Science: A Historical and Critical Essay* (London, 1924); Butterfield, *The Origins of Modern Science* [1949]; E. J. Dijksterhuis, *The Mechanization of the World Picture* (Oxford, 1961 [orig. pub. 1950]); Charles Singer, *A Short History of Scientific Ideas to 1900* (Oxford, 1959). The Enlightenment Interpretation is still current in some popular histories of science—e.g. John Gribbin, *Science: A History 1543–2001* (London, 2002)—as well as in more specialized ones—e.g. Julian B. Barbour, *Absolute or Relative Motion: A Study from a Machian Point of View of the Discovery and the Structure of Dynamical Theories* (Cambridge, 1989).

the direction of truth—whereas earlier developments, or developments outside the West, were less successful in this respect. The sign of this assumption, where it is only implicit, is the absence of any serious consideration of how the Scientific Revolution was consolidated, as if explaining how it came to be established is *ipso facto* an explanation of its consolidation.[40] But once we consider the question of consolidation seriously, then a moment's reflection shows that such developments could not possibly explain how the Scientific Revolution subsequently came to be consolidated, if only because of the sheer contingency involved in consolidation. Very significant scientific advances, in optics, astronomy, the theory of machines, medical advances, and technological advances had been made in earlier cultures, yet after a period of relatively intense scientific activity things had come to a stop in all these cases.

One can perhaps think of these as mini-scientific revolutions, but what we cannot do is to think of them as failed scientific revolutions. What distinguishes them from *the* Scientific Revolution is their apparent failure to consolidate scientific gains. By consolidation here I do not mean the ability to build up and strengthen particular scientific results or theories or even programmes of research (for they clearly had the ability to do that), but rather to consolidate the scientific enterprise as such. The latter is a legitimatory venture: its concern is with the credentials and standing of a particular kind of activity. Such consolidation aims to establish science as a model of cognitive activity. We should not assume that large-scale consolidation of this type was ever part of the programmes of Alexandrian, Arab-Islamic, or Chinese science, for example. Quite the contrary, the evidence indicates that the solution of a limited range of specific problems seems to have been the rule, and success in this enterprise usually brought an end to significant attention paid to scientific problems. The idea of large-scale consolidation is not something inherent in the scientific enterprise as such, but it is inherent in the scientific enterprise after the Scientific Revolution. Without this kind of consolidation, we would simply not have had the Scientific Revolution: we would have had a development on a par with what happened in early medieval Baghdad or Andalusia, or in Sung and Ming dynasty China. Successful consolidation, of a kind that aims at the promotion of the cognitive claims of science and builds a legitimatory scientific culture around them, is the characteristic feature of the Scientific Revolution. But such consolidation is not simply a question of success, it is a question of success in achieving an aim, an aim absent from earlier scientific cultures, and from those outside the West.

Why and how this aim was generated in the Scientific Revolution is a question at least as worthy of our attention as that of how it was successfully achieved. If large-scale legitimatory consolidation had never played a significant role in scientific

[40] The failure to distinguish between the emergence of modern science and the emergence of a scientific culture which legitimates that science also lies at the heart of much of the kind of criticism of science that began with Max Horkheimer's statement in 1946 that 'the collapse of a large part of the intellectual foundation of our civilization is to a certain extent the result of technical and scientific progress': 'Reason Against Itself: Some Remarks on Enlightenment', in James Schmidt, ed., *What is Enlightenment? Eighteenth-Century Answers and Twentieth-Century Questions* (Berkeley, 1996), 359–67: 359. Cf. Stephen Toulmin, *Cosmopolis: The Hidden Agenda of Modernity* (New York, 1990).

programmes, and if their aims had, with some rare exceptions, been largely dictated from outside, how did it come about that an internally generated programme of consolidation developed, and when and under what conditions did this happen? Two key issues here are ones that we have identified in setting out the Enlightenment Interpretation: could the autonomy of science—with respect to religion—and the methodology of science explain the ability of science in the wake of the Scientific Revolution to shape a culture in which it gradually came to provide the models and norms for cognitive enquiry generally?

SCIENTIFIC AUTONOMY

Consider the autonomy claim first, that is, the view that the success of Western science lay, at least in part, in its ability to dissociate itself from religion.[41] It is certainly true that the relations between religion and natural philosophy shifted quite radically in the sixteenth and seventeenth centuries, but, as we shall see in some detail in the chapters that follow, these shifts are by no means straightforward, and the outcome is by no means a turn away from religion, but rather in many respects a turn towards it. We must remember here that the sixteenth and seventeenth centuries were the most intensely religious centuries Europe has known. A range of exacting moral standards, accompanied by demands for self-vigilance, which had been the preserve of monastic culture throughout the Middle Ages, were transferred wholesale to the general populace in the course of the Reformation and Counter-Reformation.[42] Religious

[41] This is something more characteristic of nineteenth-century and early twentieth-century treatments. See e.g. J. W. Draper, *History of the Conflict between Religion and Science* (London, 1875); A. D. White, *A History of the Warfare of Science and Theology in Christendom* (2 vols, New York, 1896). David S. Landes, in *The Wealth and Poverty of Nations*, esp. ch. 14, is careful to talk of 'organised religion' rather than religion *simpliciter*, and this has been the general view, at least since Robert Merton's *Science, Technology and Society in Seventeenth-Century England* (New York, 1970, first published 1938). The trouble is that this claim is compatible with, yet masks, all kinds of assumptions. At one extreme 'organised religion' might effectively be acting as a euphemism for Roman Catholicism, so that a Weberian Protestant ethic might be assumed to be projecting science forwards (Draper's *History* offered a trenchant criticism of Catholicism but saw a significant role for Protestanism in the early development of science). At the other extreme, it might be being assumed that early-modern natural philosophy is an essentially secular enterprise and is inhibited significantly by organized religion, but not significantly by religion that is not of a centralized authoritative form, perhaps on the assumption that organized religion places constraints on scientific enquiry which religion that is not organized does not. Before we can even begin to ask in what sense (if any) it is true that the success of Western science lay, at least in part, in its ability to dissociate itself from organized religion, we need to ask what form of guidance or constraint organized religion provided that is absent in enquiry not motivated in this way. These are questions to which we shall be devoting considerable attention.

[42] For details see Jean Delumeau's tetralogy, *La Peur en occident (XIV^e–XVIII^e siècles): Une cité assiégée* (Paris, 1978); *Le Péché et la peur: La culpabilisation en occident, XIII^e–XVIII^e siècles* (Paris, 1983); *Rassurer et protéger: Le sentiment de sécuritée dans l'occident d'autrefois* (Paris, 1989); *L'Aveu et le pardon* (Paris, 1992). See also idem, *Le Catholicisme entre Luther et Voltaire* (Paris, 1971); R. Po-Chia Hsia, *Social Discipline in the Reformation* (London, 1989); Gerhard Oestreich, *Neostoicism and the Early Modern State* (Cambridge, 1982), ch. 11; Phillipe Ariès, *Religion populaire et réforme liturgique* (Paris, 1975); and Lucien Febvre, *The Problem of Unbelief in the Sixteenth Century: The Religion of Rabelais* (Cambridge, Mass., 1982).

sensibilities in the secular population were deep and intense in the early-modern era, as deep and intense as anything in monastic culture, and—a crucial point for our concerns—these religious sensibilities motivated a great deal of natural-philosophical enquiry well into the nineteenth century.

We shall see that a good part of the distinctive success at the level of legitimation and consolidation of the scientific enterprise in the early-modern West derives not from any separation of religion and natural philosophy, but rather from the fact that natural philosophy could be accommodated to projects in natural theology: what made natural philosophy attractive to so many in the seventeenth and eighteenth centuries were the prospects it offered for the renewal of natural theology. Far from science breaking free of religion in the early-modern era, its consolidation depended crucially on religion being in the driving seat: Christianity took over natural philosophy in the seventeenth century, setting its agenda and projecting it forward in a way quite different from that of any other scientific culture, and in the end establishing it as something in part constructed in the image of religion. We shall be investigating the complex processes by which this accommodation occurred, and how both natural philosophy and theology were transformed in the process. By the nineteenth century the two had started to come apart, but the intellectual causes of this phenomenon do not lie in any conflict or incompatibility between natural philosophy and theology. Quite the contrary, materialistically inclined atheists (at least before Diderot) were forced to ignore recent developments in natural philosophy, and reverted to the radical naturalistic conceptions that were prevalent immediately prior to the Scientific Revolution.[43]

The case of nineteenth-century Anglicanism is instructive here. The causes of Anglicanism's decline in authority from the 1840s onwards are complex, but the reasons given by those who had 'lost their faith' in Victorian England hardly ever included advances in science.[44] Rather, at least some of the difficulties for Christianity arose because of the emergence, from the seventeenth century onwards, of a historical understanding of, first, the Bible, and then Christianity as a whole, a development that gradually undermined the credentials of Christianity, as it was historicized and then relativized, from Bacon through to Hume and Gibbon. The final blow in the British Isles came with the publication of *Essays and Criticisms* in 1860, where the contributors, predominantly Anglican clergymen, urged the replacement of an inspirational reading of the Bible with a historical one, arguing that the Bible had to be read like any other book.[45] It was primarily biblical criticism and history rather than science that were the external causes of the intellectual rethinking of religious

[43] See Winfried Schröder, *Ursprünge des Atheismus: Untersuchungen zur Metaphysik- und Religionskritik des 17. und 18. Jahrhunderts* (Stuttgart-Bad Cannstatt, 1998).

[44] See the detailed discussion in Susan Budd, *Varieties of Unbelief: Atheists and Unbelievers in English Society 1850–1960* (London, 1977), 104–23.

[45] Victor Shea and William Whitla, eds., *Essays and Reviews: The 1860 Text and Its Reading* (Charlottesville, 2000). See also Ieuan Ellis, *Seven Against Christ: A Study of Essays and Reviews* (Leiden, 1980); and Peter Hinchcliff, *Benjamin Jowett and the Christian Religion* (Oxford, 1987), ch. 4.

sensibilities and sources of authority.[46] As Owen Chadwick points out, in the 1860s 'theologians were busier with the consequences of Biblical criticism than with the consequences of the natural sciences', and after that 'their new historical knowledge made them shrink away from basing the revelation of God upon documents which without doubt contained historical truth but no one could yet say how much truth'.[47]

However, as science sloughed off the religious ideologies that gave it its rationale, it took upon itself the mantle of religion in many respects, while at the same time trying to forge a new rationale for itself. In a lecture in 1853, the most influential advocate of applied science in his time, Lyon Playfair, declared that 'science is a religion and its philosophers are priests of nature', and Huxley referred to his own lectures on science as 'lay sermons'.[48] Beatrice Webb, reflecting on what she refers to as the 'religion of science' of her adolescence in the 1870s, defined it as 'an implicit faith that by the methods of physical science, and by these methods alone, could be solved all the problems arising out of the relation of man to man and of man towards the universe'.[49] 'Who will deny', she asked,

> that the men of science were the leading British intellectuals of that period; that it was they who stood out as men of genius with international reputations; that it was they who were routing the theologians, confounding the mystics, imposing their theories on philosophers, their investments on capitalists, and their discoveries on medical men; whilst they were at the same time snubbing the artists, ignoring the poets, and even casting doubts on the capacity of the politicians?[50]

In 1875 Francis Galton was calling for 'a scientific priesthood' to tend to the health and welfare of the nation,[51] and in 1889 the French Darwinian Alfred Giard was claiming that 'among the most happy public expressions of opinion toward the end of this century must be counted the tendency of science to replace gradually the role hitherto enjoyed by religion',[52] an assessment confirmed by the view of the English historian Alfred Benn, who writes in 1906 that 'a great part of the reverence once given to priests and to their stories of an unseen universe has been transferred to the

[46] On the impact of German biblical criticism in England in the nineteenth century see John Rogerson, *Old Testament Criticism in the Nineteenth Century* (London, 1984).

[47] Owen Chadwick, 'Evolution and the Churches', in C. A. Russell, *Science and Religious Belief: A Selection of Recent Historical Studies* (London, 1973), 282–93: 288 and 289 respectively.

[48] Quoted in John Hedley Brooke, *Science and Religion* (Cambridge, 1991), 31.

[49] Beatrice Webb, *My Apprenticeship* (London, 1926), 83.

[50] Ibid. 130–1.

[51] Francis Galton, *English Men of Science: Their Nature and Nurture* (New York, 1875), 195. The idea of a secular version of religion had effectively begun with Comte's idea of 'the religion of humanity', fostered by a Comtean 'Positivist Society' founded by Littré and others in the late 1840s, where the idea of a secular religion was promoted. See Frank Manuel and Fritzie Manuel, *Utopian Thought in the Western World* (Cambridge, Mass., 1979), ch. 30; and Leslek Kotakowski, *Positivist Philosophy from Hume to the Vienna Circle* (Harmondsworth, 1972), ch. 3. What we are concerned with here is very different from the idea of a 'scientific priesthood' which might be associated—e.g. in H. Fisch, 'The Scientist as Priest: A Note on Robert Boyle's Natural Theology', *Isis* 44 (1953), 252–65—with those natural philosophers who saw natural philosophy as a way of pursuing natural theology.

[52] Quoted in Brooke, *Science and Religion*, 298.

astronomer, the geologist, the physician, and the engineer'.[53] The trend to see scientists as having a religious standing had begun with Newton's sanctification by his hagiographer John Conduitt, who describes Newton as 'a Saint & his discoveries might well pass for miracles'.[54] Nor was the symbolism of Newton's supposed discovery of the law of universal gravitation by means of a falling apple lost on Newton's admirers. As Patricia Fara points out,

> In religious iconography, the infant's apple indicates that Christ, the Second Adam, will redeem humanity. For Bacon's followers, Newton became the new Adam who would uncover God's mathematical laws of nature. [James] Thompson presented him as the saviour who would explain the cosmos to 'erring Man', the fallen human race, and such scriptural metaphors were still widely prevalent in Regency England.[55]

The phenomenon was certainly not confined to England, and it showed no sign of abating over the next two centuries. At the end of the nineteenth century, the great German physicist and physiologist Helmholtz could reflect, autobiographically, on his life in science as being something 'everlastingly sacred', and the work of the scientist as being 'sanctified'.[56]

Internal factors also caused a rethinking of religious sensibilities and sources of authority, however, and if we fail to understand the role of these from the outset we may find ourselves advocating a view of the emergence of a scientific culture which, because of a mistaken understanding of just what we are being called upon to explain, tries to account for local and contingent developments as if they were the logical outcome of large-scale historical forces. Since this is a misunderstanding to which the kind of project in which we shall be engaged is especially susceptible, an example may serve to warn us of the dangers. We shall come across a number of such cases in the course of our investigation of the early-modern development of natural philosophy,

53 Alfred W. Benn, *The History of English Rationalism in the Nineteenth Century* (2 vols., London, 1906), i. 198.

54 Quoted in Rob Iliffe', ''Is He Like Other Men?'' The Meaning of the *Principia Mathematica* and the Author as Idol', in Gerald Maclean, ed., *Culture and Society in the Stuart Restoration* (Cambridge, 1995), 159–76: 176.

55 Patricia Fara, *Newton: The Making of Genius* (London, 2002), 199.

56 *Science and Culture: Popular and Philosophical Essays*, ed. D. Cahan (Chicago, 1995), 392. See also David Cahan, 'Helmholtz and the Civilizing Power of Science', in David Cahan, ed., *Hermann von Helmholtz and the Foundations of Nineteenth-Century Science* (Berkeley, 1993), 559–601; and Irmline Veit-Brause, 'The Making of Modern Scientific Personae: The Scientist as a Moral Person? Emil du Bois-Reymond and His Friends', *History of the Human Sciences* 15 (2002), 19–50. At the end of the nineteenth century, there was a circle in Germany (the *Monistenbund*) that fostered the idea of a 'religion of science', led by Ernst Haeckel and Friedrich Wilhelm Ostwald, with Ostwald telling us that 'we expect from science the highest that mankind can produce and win on this earth . . . Everything that mankind, in terms of its wishes and hopes, its aims and ideals, combines in the concept God, is fulfilled by science': Friedrich Wilhelm Ostwald, *Monism as the Goal of Civilization* (Hamburg, 1913), 37. See the account in H. Schipperges, *Weltbild und Wissenschaft: Eröffnungsreden zu den Naturforscherversammlungen 1822 bis 1972* (Hildesheim, 1976). The movement was represented in the USA by Paul Carus in his *The Religion of Science* (Chicago, 1893) and numerous later books. On the religious image of the scientist in the twentieth century, see Gerhard Sonnert, *Einstein and Culture* (Amherst, 2005), 144–83.

but the best example—one deeply ingrained in popular culture—is the case of the nineteenth-century abandonment of religion in favour of science in England.

The simultaneous combination of the decline in the fortunes of religion and the increase in the fortunes of science has often encouraged the assumption that the abandonment of religion in favour of science was the inevitable outcome of the progress of science, finally bringing to a head and settling a question over the relative authority of science and religion that had been simmering since the condemnations of Copernicanism. But to suppose this is to make assumptions about the role of intellectual factors in the abandonment of Christianity that are wholly unjustified. The well-attested 'crisis of faith' in England from the 1840s onwards was provoked not by the success of science but rather by a complex combination of contingent events, paramount among which are political reaction to the French Revolution, denominational rivalry, and evangelicalism.[57] In the first case, Burke's polemic against the French Revolution had identified atheism and materialism as the main contributing factors, and his work linked resistance to the Revolution with the protection of religion.[58] This certainly led to a deep suspicion of science on the part of some in England, with Patrick Colquhoun, magistrate and police reformer, encouraging elementary education in morals but making it clear that 'science and learning, if universally diffused, would speedily overturn the best constituted government on earth'.[59] Significant resistance to universal scientific education, even on the part of scientists, is clear from the opposition to the Parochial Schools Bill of 1807.[60] On the other hand there was also a sudden explosion of publications expounding natural religion, and two strategies were in operation: clergymen appealed to natural theology to refute the atheism and materialism of radical religious, philosophical, and scientific writers; and scientists (many of whom were clergymen) set out to show that science and rational thought did not lead to atheism and materialism, but to reverence for God and the existing political and social structure.[61] Second, the Anglican Church gradually lost its religious-political

[57] For a good summary of the issues, to which I am indebted here, see Frank M. Turner, 'The Victorian Crisis of Faith and the Faith That was Lost', in Richard J. Helmstadter and Bernard Lightman, eds, *Victorian Faith in Crisis: Essays on Continuity and Change in Nineteenth-Century Religious Belief* (London, 1990), 9–38.

[58] See Ursula Henriques, *Religious Toleration in England, 1787–1833* (London, 1961) and V. Kiernan, 'Evangelicalism and the French Revolution', *Past and Present* 1 (1952), 44–56.

[59] Patrick Colquhoun, *A Treatise on Indigence* (London, 1806), 148–9.

[60] See D. S. L. Cardwell, *The Organisation of Science in England* (London, 1972), 38.

[61] Turner, 'The Victorian Crisis', 12–13. The most famous nineteenth-century works in this genre are William Paley, *Natural Theology: or, Evidences of the Existence and Attributes of the Deity, collected from the Appearances of Nature* (London, 1802) and *The Bridgewater Treatises, on the Power, Wisdom, and Goodness of God as Manifested in the Creation*, which appeared between 1834 and 1837. The Bridgewater Treatises covered everything from geology and anatomy to astronomy and the 'moral and intellectual constitution of man'. By the middle of the nineteenth century, evangelical writers in Britain had begun to move away from natural theology back to scriptural sources: see David W. Bebbington, 'Science and Evangelical Theology in Britain from Wesley to Orr', in David N. Livingstone, D. G. Hart, and Mark A. Noll, eds, *Evangelicals and Science in Historical Perspective* (Oxford, 1999), 120–41; and Aileen Fyfe, 'The Reception of William Paley's *Natural Theology* in the University of Cambridge', *British Journal for the History of Science* 30 (1997), 35–59.

monopoly from the end of the eighteenth century—the Catholic emancipation of 1829 came about largely because Parliament no longer believed that the Established Church had any unique claims to precedence on religious matters—and this provoked a very significant increase in theological controversy.[62] Third, with the rise of the evangelical movement from the late eighteenth century, we enter a period of sharp and intense public criticism of ecclesiastical institutions on moral, intellectual, and spiritual grounds. But the aim is to re-Christianize these institutions, not to replace them with something non-Christian. As Turner points out, 'Victorian faith entered crisis not in the midst of any attack on religion but rather during the period of the most fervent crusade that the British nation had known since the seventeenth century, indeed during the last great effort on the part of all denominations to Christianise Britain.'[63]

These developments belie the notion of a gradual but inevitable process of secularization, in which Western culture moves inexorably from Christian to secular. How the relations between science and religion were played out varied, often radically, from country to country, and there was no general movement of inevitable secularization brought about by science. While in France science had gradually been deployed by the *philosophes* in favour of atheism and materialism from the middle of the eighteenth century, in England there was a move in the opposite direction, as science was marshalled in defence of Christianity. The contrast between eighteenth-century England and France is particularly striking here. Priestley, at the radical cutting edge of dissenting thought in England, records his experience of a dinner in France:

> When I was dining at . . . Turget's table, M. de Chatellux . . . said the two gentlemen opposite me were the Bishop of Aix and the Archbishop of Toulouse, 'but,' said he, 'they are no more believers than you or I.' I assured him that I was a believer; but he would not believe me.[64]

Even Hume, often regarded as the archetypical British atheist of the eighteenth century, found himself in a similar position when dining with d'Holbach and others in Paris. Hume wondered whether there were in fact any atheists at all in the world, and remarked that he had never actually met one, only to be informed by d'Holbach that he was surrounded by fourteen of them.[65] By contrast, far from associating science and atheism, most British readers had learned what they knew about science from the new religious magazines that sprang up at the end of the eighteenth century, magazines that set out to incorporate scientific reading into the practice of Christian

[62] See in particular Jeffrey Cox, *The English Churches in a Secular Society: Lambeth, 1870–1930* (Oxford, 1982); P. T. Marsh, *The Victorian Church in Decline* (London, 1969); and Frank M. Turner, *Between Science and Religion: The Reaction to Scientific Naturalism in Late Victorian England* (New Haven, 1974).

[63] Turner, 'The Victorian Crisis', 11. See also Kenneth Hylson-Smith, *Evangelicals in the Church of England 1734–1984* (Edinburgh, 1989), and Michael R. Watts, *The Dissenters*, ii. *The Expansion of Evangelical Nonconformity* (Oxford, 1995).

[64] Quoted in Brooke', *Science and Religion*, 180.

[65] Interview with Boswell, dated 3 March 1777; the journal entry is given in David Hume, *Dialogues concerning Natural Religion*, ed. and introd. Norman Kemp Smith (Indianapolis, 1947), 76–9. There were actually seventeen at the table, other than Hume himself, but the three remaining had yet to make up their minds on the question.

piety.[66] Similar developments in the USA had quite different effects, as a free market in religious ideas encouraged a proliferation of evangelical sects which continues to this day:[67] despite the constitutional separation of church and state in the First Amendment, the teaching of natural selection theory in state schools could be seriously questioned in the courts, in the second half of the twentieth century, because it conflicted with a literal reading of the Old Testament, and indeed the Kansas Board of Education dropped the teaching of evolution from the state's science curriculum in 1999.[68] Yet at the same time scientific research, including evolutionary biology, has flourished in the USA. The one does not seem to have significantly impeded the other.[69] Early nineteenth-century England was very different from this: there, Christianity had significant difficulty in reorganizing itself to meet a number of challenges that came not from science, but primarily as a consequence of an intensified religious life, provoked in part by the rise of evangelical sects, in which science generally played a supportive role.[70]

Such local factors acted to induce an intensified religious culture in Victorian England, one in which new options were opened up, and choices forced.[71] The intense criticism that we witness in the mid-nineteenth century did not result in a consolidation and renewal of religious belief, however, but rather in a more general search for something that would meet these new moral, spiritual, and intellectual demands. The intensified religious context ruled out complacency in these matters, but it was

[66] See John Brooke and Geoffrey Cantor, *Reconstructing Nature: The Engagement of Science and Religion* (Oxford, 1998); Bernard Lightman', "The Voices of Nature": Popularising Victorian Science', in Bernard Lightman, ed., *Victorian Science in Context* (Chicago, 1997), 187–211; idem, 'The Story of Nature: Victorian Popularizers and Scientific Narrative', *Victorian Review* 25 (1999), 1–29; Jonathan R. Topham, 'The *Wesleyan-Methodist* Magazine and Religious Monthlies in Early Nineteenth-Century Britain', in Geoffrey Cantor et al., *Science in the Nineteenth-Century Periodical: Reading the Magazine of Nature* (Cambridge, 2004), 67–90; Aileen Fyfe, *Science and Salvation: Evangelical Popular Science Publishing in Victorian Britain* (Chicago, 2004).

[67] See George M. Marsden, *Fundamentalism and American Culture: The Shaping of Twentieth Century Evangelicalism, 1870–1925* (Oxford, 1980).

[68] The decision was overturned in 2001. The issue is not confined to Kansas, and has if anything been compounded in recent years. A November 2004 CBS poll found that 65% of Americans favoured teaching of creationism in science classes; 37% believed that natural selection should not be taught at all. Fifty-five percent of respondents believed that human beings were not the product of evolution but had been created in their present form. On 2 August 2005, President George W. Bush was reported in the newspapers as publicly supporting the teaching of a version of creationism—'intelligent design'—alongside natural selection in science classes in schools in the USA. There is a useful overview of American fundamentalism in Michael Ruse, *The Evolution-Creation Struggle* (Cambridge, Mass., 2005), chs. 8 and 12.

[69] This may change, however. At the time of writing, it does appears that stem-cell research may in fact be impeded in the USA, for example, on grounds that bear only very tangentially on genuine ethical issues and seem to be driven primarily by political and religious considerations. More generally, on the subordination of science to a political agenda by some conservatives in the USA, see Chris Mooney, *The Republican War on Science* (New York, 2005).

[70] In England, the development of a free market in religious ideas beginning with the repeal of the Tests Act had the opposite effect to that in the USA: see Cox, *The English Churches*.

[71] Cf. Matthew Arnold: 'Two things about the Christian religion must surely be clear to anyone with eyes in his head. One is, that men cannot do without it; the other is that they cannot do with it as it is.' *God and the Bible* (New York, 1893), p. xi.

less successful in dictating the range of possible solutions. This left scope for science, which had played a crucial role in the debates against atheism and materialism, to be seen as something that might meet these new moral, spiritual, and intellectual demands. It is worth noting here that in his *A Survey of the Wisdom of God in the Creation* (1763), John Wesley, the writer to whom the evangelical movement owed most, had both pursued and recommended the study of nature on the grounds that it inspired awe and humility in the face of the marvellous organization in the created order. He saw the wonderful fit between the functional anatomy of animals and plants and their environment as a pious alternative to the arrogance of theologians.[72] The move to science for those disillusioned with traditional forms of Christianity was by no means a radical one. As Turner notes,

> The activities of the advocates of scientific naturalism during the third quarter of the century in particular illustrate the actions of engaged laymen criticising the adequacy of religion in a manner reminiscent of evangelicalism. Their attack pitted what they regarded as real religion, honest in thought and morally beneficent in action, against the nominal religion of the Anglican Church. . . . Anglican clerical scientists and other interested clergy had claimed to resist the inroads of materialism, which was regarded as a political and social as well as a spiritual danger, through their . . . advocacy and support of natural theology. The scientists and scientifically-minded philosophers associated with scientific naturalism sought to beat the Church of England at its own cultural game. Like the evangelicals of an earlier day, the honest doubters and advocates of scientific naturalism demanded a truer and more genuine religion that was not an intellectual, political, and moral scandal.[73]

It is in this context that Huxley's invention of the notion of 'agnosticism' plays a key role in marking out his group from atheists and materialists, while at the same time offering an evolutionary form of natural religion.[74] The 'agnostics' could present themselves on the one hand as opponents of immoderate religious movements and on the other as opponents of Catholicism, at a time when factions within the established Church were prevaricating.

But, of course, this scientific movement had features that marked it out from religious movements. One of Huxley's inner circle, the mathematician T. A. Hirst, wrote of their 'X Club' founded in 1864, that its aim was 'devotion to science, pure and free, untrammeled by religious dogmas. Amongst ourselves there is perfect outspokeness.'[75] The new scientific movement was committed to a new ideology—unfettered

[72] John Wesley, *A Survey of the Wisdom of God in the Creation: or a Compendium of Natural Philosophy* (2 vols, London, 1827).

[73] Turner, 'The Victorian Crisis', 17–18. It is worth remembering, nevertheless, that in Galton's 1874 survey of English scientists, 70% of them regarded themselves as Anglicans: *English Men of Science*, 126–7.

[74] In fact, Huxley was not successful in controlling the term, which came to be associated with Spenser's philosophy of the 'unknowable': see Bernard Lightman, 'Huxley and Scientific Agnosticism: The Strange History of a Failed Rhetorical Strategy', *British Journal for the History of Science* 35 (2002), 271–89.

[75] Cited in Lightman, 'Huxley and Scientific Agnosticism', 272. See also Roy MacLeod, 'A Victorian Scientific Network: The X-Club', *Notes and Records of the Royal Society* 24 (1969), 305–22; and J. Vernon Jensen, 'The X Club: Fraternity of Victorian Scientists', *British Journal for the History of Science* 5 (1970/1), 63–72.

adversarial engagement—and with this came a distinctive kind of concern with scientific method. Method was not seen as something that one might devise to help one in particular scientific investigations, as it had been in the sixteenth and seventeenth centuries, but as a *post facto* rationalization of the success of science, in the work of Whewell, Mill, and others. This tied science into a new adversarial and democratic culture, which by virtue of these qualities was also meritocratic. The new theories of method combined the demands of quasi-religious authority, whereby there was a single authorized road to truth, and the demands of a meritocratic adversarial basis for science. It was these new theories of method that were considered to be what it was that provided science with its unique road to success. As Huxley put it in 1866:

> If these ideas be destined, as I believe they are, to be more and more firmly established as the world grows older; if that spirit be fated, as I believe it is, to extend itself into all departments of human thought, and to become co-extensive with the range of knowledge; if, as our race approaches its maturity, it discovers, as I believe it will, that there is but one kind of knowledge and but one method of acquiring it; then we, who are still children, may justly feel it our highest duty to recognise the advisableness of improving natural knowledge, and so aid ourselves and our successors in our course towards the noble goal which lies before mankind.[76]

A modern version of this project can be found in the work of Karl Popper. Popper also drew the connections between science and a democratic culture explicitly,[77] and he tied this in with the idea that the aim of the scientific enterprise is to try to falsify theories.[78] In Popper's account, the 'scientist as hero' reaches his apogee, taking on the role of the ascetic by a form of intellectual self-deprivation. Fighting his natural inclinations, the scientist must himself try to show the theories he has nurtured to be false. In compensation, however, he attains to a form of intellectual morality of a profound kind to which no one else can reasonably aspire: the scientist becomes the only truly intellectually honest person, for only the scientist is so concerned for truth that he works on the assumption that his own theories are false.[79] The parallels with the ascetic monk who considers himself permanently unworthy are too obvious to be pressed.

[76] T. H. Huxley, *Collected Essays* (9 vols, New York, 1893–4) i. 41.

[77] Popper, *The Open Society*, and *The Poverty of Historicism*. The theme goes back to John Stuart Mill, *On Liberty* (London, 1859). Paul Feyerabend, in his *Against Method* (London, 1975), took a more Millian view of modern liberal democracy than Popper, seeing its strongest feature as its pluralism, and he devised his scientific method accordingly, under the slogan of 'anything goes'.

[78] Karl R. Popper, *The Logic of Scientific Discovery* (rev. edn, London, 1968).

[79] Imre Lakatos, in his very influential paper 'Falsification and the Methodology of Scientific Research Programmes', in Imre Lakatos and Alan Musgrave, eds, *Criticism and the Growth of Knowledge* (Cambridge, 1970), 91–196, which set the agenda for a whole generation of post-Popperian thinking, is even more explicit. He writes: '*Sophisticated methodological falsificationism* offers new standards for intellectual honesty. Justificationist honesty demanded the acceptance of only what was proven and the rejection of everything unproven. Neojustificationist honesty demanded the specification of the probability of any hypothesis in the light of the available empirical evidence. The honesty of naïve falsificationism demanded the testing of the falsifiable and the rejection of the unfalsifiable and the falsified. Finally, the honesty of sophisticated falsificationism demanded that one should try to look at things from different points of view, to put forward new theories which anticipate novel facts, and to reject theories which have been superseded by more powerful ones' (122).

METHOD AND LEGITIMATION

It might seem that Popper has overplayed his hand here, but two points are worth noting. First, the idea that the natural philosopher must be someone who has personal qualities that enable him to act as a paradigm of intellectual honesty pervades early-modern natural philosophy, and can be found set out explicitly as least as early as Descartes.[80] It is one of the constitutive ingredients in the early-modern construction of the *persona* of the natural philosopher. Popper is simply one of the most recent to try to articulate—through his notion of falsification—just what is involved in this idea of intellectual honesty. Second, the claims of the Enlightenment Interpretation for the cognitive standing of science over religion thrive on the idea of the intellectual honesty of science, on its uncompromising probing of the evidence unhindered by dogma. If one wanted to defend the Enlightenment Interpretation on this question, Popper's programme offers a way of engaging the issues. Nevertheless, it soon becomes clear that this route is not a productive one. Questions of intellectual honesty do play an important role in the legitimation of the scientific enterprise from the early-modern era onwards, and they are crucial to the notion of scientific authority, but they are complex questions that go to the heart of what we might term the moral psychology of the natural philosopher, not the kinds of things that can simply be reduced to commitment to a particular methodology. Methodological questions are in any case highly overdetermined, and the connection between what drives the early development of the Scientific Revolution and what drives its consolidation as a model for cognitive enquiry is particularly problematic. Some of the problems here can be highlighted by considering the role of adversarial procedures in scientific enquiry.

In comparisons of the development of natural philosophy in the early-modern West with other cultures that initiated and supported successful scientific developments, two things have traditionally been claimed as the basis for its comparative success: its ability to dissociate itself from religion, and its independent adversarial approach. David Landes, for example, argues that economic and social progress in the modern world are due to Western civilization and its dissemination, and he identifies science as one of the crucial ingredients in Europe's economic growth. Science in the West, he argues, developed as an autonomous method of intellectual enquiry that successfully disengaged itself from the social constraints of organized religion and from the political constraints of centralized authority, and that, although Europe lacked a political centre, its scholars benefited from the use of a single vehicle of communication, Latin, which facilitated adversarial discourse in which new ideas about the physical world could be tested, demonstrated, and then accepted across the continent and eventually across the world.[81]

To give consideration of adversarial methods more substance, we need to add another dimension to the discussion, going beyond individual commitment to

[80] See Stephen Gaukroger, *Descartes' System of Natural Philosophy* (Cambridge, 2002), 239–44.

[81] David S. Landes, *The Wealth and Poverty of Nations: Why Some are So Rich and Some are So Poor* (New York, 1999), esp. ch. 14.

method. We need to consider the large-scale social and institutional context that shapes the possibility of an adversarial approach, as a number of writers in the Weberian tradition, notably Nelson and Huff,[82] have done. The Weberian approach takes its bearings from Weber's discovery that, in the East, a number of the pre-industrial cultures possessed the necessary technology and structures for the development of capitalism, yet (as he saw it) lacked the kinds of motives and sanctions needed to encourage the abandonment of traditional values. This raises the question of what provided these motives and sanctions in the West. In a comparison of Arab-Islamic, Chinese, and early-modern European scientific cultures, for example, Huff locates successful adversarial practice as occupying a central point between what we might characterize as an atomistic intellectual culture, namely that of Arab Islam, in which adversarial argument is possible but in which there is no network for following up the outcome of such argument, and the holist intellectual culture typical of Sung- and Ming-dynasty China, where there is an extensive network for the communication of results, but little role for adversarial argument. Islam had no corporate entities, China had only one, but the West had many, and this, Huff argues, enables the West to establish 'neutral zones of free enquiry' which both allow and encourage innovation, and grant science a measure of autonomy without which it could not survive in a hostile climate.[83]

On this account, Arab-Islamic science had two distinctive features. The first is that there was no institutional support available for scientific work that was not motivated by extra-scientific concerns; but mathematical astronomy, for example, received strong institutional support at centres such as the Marāgha observatory, because of the importance of determining the direction of Mecca at different locations if one was to face Mecca while praying. Islamic law did not recognize—indeed refused on religious grounds to recognize—corporate entities, with the consequence that no autonomous status groups, whether professional groups (with some qualifications in the case of medicine) or institutions such as universities were able to develop. And in the absence of such an institutional setting in a society in which daily life was regulated in a comprehensive way by religion, not only theories at variance with religious doctrine, but even those with no bearing on religious doctrine, were discouraged. Even though the teaching methods were adversarial, as a student one's interaction was limited to one's mullah. Innovation was at best frowned upon and at worst treated as a form of heresy. Second, the way in which the achievements of Greek philosophers, and Greek and Alexandrian mathematicians, were 'naturalized' in Arab-Islamic culture is distinctive. They were domesticated, incorporated into an indigenous cultural and philosophical system, rather than being institutionalized in such a way that they carried 'their own specific gravity of autonomy and legitimacy, independent of the moral and

[82] Benjamin Nelson, *On the Roads to Modernity: Conscience, Science, and Civilizations* (Towota, NJ, 1981), chs. 7, 8, and 9; and Toby E. Huff, *The Rise of Early Modern Science* (Cambridge, 1993).

[83] In some respects, this idea has affinities with Gibbon's typically Enlightenment view that the political pluralism of modern Europe allowed freedom of thought by preventing a return to the intellectual tyrannies of the Roman Empire and Christianity.

religious scruples of the surrounding culture'.[84] The consequence of these two institutional features of Arab-Islamic civilization is that while it was occasionally possible for innovations to be made in astronomy, optics, and even metaphysics, there was no way in which they could be followed up in a systematic manner.

The case of China is almost the reverse. If Arab-Islamic culture could initiate scientific developments but not follow them up, China had an extensive network of communications and this acted in such a way as to foster scientific and technological developments. In the Sung and Ming dynasties we find inventions such as mechanical clocks, movable type, and seismographs that predate developments in the West by at least a couple of centuries; and we find significant advances in observational astronomy and medicine, as well as an understanding and employment of a variety of products and devices from explosives to magnets. Yet there were serious obstacles to innovation that was not practically oriented. The bureaucratic structure of Chinese society is a crucial factor here. Not only was the office of emperor increasingly sacralized in the Ming dynasty, traditions of philosophical disputation are relatively marginal in Chinese intellectual culture.[85] Adherence to tradition plays a crucial role in all artistic and intellectual pursuits to a far greater extent than was ever the case in Arab-Islamic culture or the Christian West, with the result that there is a strong sense that scholarship rather than innovation is the path to wisdom. This was reinforced by the Confucian tendency to self-effacement and avoidance of contentiousness, as well as a strong commitment to outward obedience to public authorities.

The contrast between Arab-Islamic, Chinese, and Western culture in these respects is strikingly evident in the very different ways in which the legal process functions in these cultures, and Huff considers differences here to lie at the root of many of the others. Islam and the West both had adversarial systems of sorts, and legal reasoning, with its highly developed notions of proof and evidence, had an important effect on reasoning in general, particularly in the West. This is completely lacking in China. There were no private lawyers in China: the very idea of having someone help argue one's case before the authorities was alien to the whole system and in the rare cases where it happened it was punished severely.[86] As G. E. R. Lloyd notes,

> So far from positively delighting in litigation, as many Greeks seem to have done, so far from developing a taste for confrontational argument in that context and becoming quite expert

[84] Huff, *The Rise of Early Modern Science*, 63. This aspect of Islamic learning is contrasted with that of the West in Remi Brague, *Eccentric Culture: A Theory of Western Civilization* (South Bend, Ind., 2002), ch. 5.

[85] On this see Nathan Sivin, 'On the Word "Taoist" as a Source of Perplexity. With Special Reference to the Relations of Science and Religion in Traditional China', *History of Religions* 17 (1978), 303–30; and idem, 'Ruminations on the Dao and its Disputers', *Philosophy East and West* 42 (1992), 21–9. See also the discussion in G. E. R. Lloyd, *Adversaries and Authorities: Investigations into Ancient Greek and Chinese Science* (Cambridge, 1996), 26–41.

[86] Huff notes such behaviour was tantamount to challenging the word of a public authority and was an unforgivable sign of disrespect and dissension, 'the ultimate betrayal of filial piety, of family and clan, and, above all, the betrayal of the principle of *jang*, yieldingness'. *The Rise of Early Modern Science*, 269.

in its evaluation, the Chinese avoided any brush with the law as far as they possibly could. Disputes that could not be resolved by arbitration were felt to be a breakdown of due order and as such reflected unfavourably on *both* parties, whoever was in the right.[87]

This lack of an adversarial model shaped the way in which scientific practice was pursued in China, and Chinese respect for authority formed a sharp contrast with the Greek model of confrontational debate.[88] The situation in Islam was totally different. The judge of religious law—the *qadi*—was not in a position to innovate, but merely to interpret the law. Because this law had been handed down from God, interpretations could vary. There was no individual could decide between genuine, conscientious, and properly intentioned judgements, no mere mortal who could speak on behalf of God. Consequently, there was scope for shopping around—'forum shopping' as lawyers call it—for judgements, and one could then follow that more favourable to one's case. There are parallels in education here. In China, only the State could ever certify a student's competence. No individual could have this power, no matter what his skills or standing. In Islam, only an individual teacher could ever certify a student's competence. In neither case was there a corporate body of expertise—a professional body, a university, or whatever—to which those with special skills could make a contribution, and which could provide certification of competence. And in neither case was there a system that could provide group support for those who questioned tradition, or had heterodox views.

This, on Huff's account, is the essential difference between China, Arab Islam, and the West. What lies at its source is the Investiture Controversy (1050–1122), in which the Church was effectively formed as a corporation, declaring itself legally autonomous from the secular order. In the wake of this, Gratian's codification of canon law around 1140 began the construction of a new system of law. This not only harmonized various legal traditions but provided new foundations for the law, creating a new science of law which became a model of intellectual achievement, and establishing a principle of authority and legitimacy over discordant authorities. What this legal revolution did, Huff argues, was to create a 'neutral space for enquiry', and this is what allowed innovation to occur. Moreover, by instituting corporate bodies—towns, cities, guilds, universities, professional groups—on its own model, it brought about a kind of decentralization of responsibilities and expertise which created the kind of protected climate in which such innovations could flourish.

I offer this account as an illustration of how we might achieve a good deal more insight once we lift questions of method outside the realm of pure epistemology, and we can learn vital lessons from it. But the lessons lie in appreciating the value of the extra dimensions of analysis, and it soon becomes evident that we must go

[87] Lloyd, *Adversaries and Authorities*, 220. It seems to have been the Greeks who were exceptional in this respect, not the Chinese. As Lloyd notes elsewhere, 'The extant remains of Egyptian and Babylonian medicine, mathematics and astronomy can be combed in vain for a single example of a text where an individual author explicitly distances himself from, and criticises, the received tradition in order to claim originality for himself, whereas our Greek sources repeatedly do that' (*The Revolutions of Wisdom*, 57).

[88] See Lloyd, *Adversaries and Authorities*, ch. 2.

beyond what turns out to be the somewhat formulaic contrasts offered in this kind of Weberian approach. If we confine our attention to two issues—the existence of a neutral space for enquiry, and the role of an adversarial culture—we can glimpse the extent of the challenge.

First, the appropriateness of the idea of 'neutral spaces for enquiry'—and the idea of corporate entities such as universities providing such neutral spaces—is questionable. If there were any 'neutral spaces' in the first half of the seventeenth century, for example, they were not much in evidence in the universities.[89] Rather we must turn to the patronage of figures such as the Medici (Galileo), Maurice of Nassau (Stevin), Peiresc (Gassendi), the Earl of Northumberland (the circle that included Harriot, Hues, and Warner) and the Marquis of Newcastle (Digby and Hobbes), or to the use of private means (Descartes and Boyle), or some form of professional employment (Charleton) or public employment (Bacon) which left time to pursue natural philosophy. None of these were unique to the West. Moreover, even if the universities had provided the requisite neutral spaces, the new generation of natural philosophers could have learned little from them because they were teaching the wrong things. Neither Galileo nor Descartes, for example, was able to pick up the mathematical skills he so desperately needed during the course of his university education. Galileo had initially studied medicine at Pisa but left before completing his degree, and in 1583 started learning mathematics at his father's house from the Florentine court instructor Ostilio Ricci, who taught military fortification, mechanics, architecture, and perspective.[90] Descartes similarly learned his mathematics in a practical context: having studied law at Poitiers, he picked up and refined his mathematical skills in the armies of Prince Maurice of Nassau and Maximilian I, to which he was attached from 1618 to 1620.[91] More generally, it is far from clear that what we should be doing is trying to identify a single 'neutral space of enquiry'. Natural philosophy was not a uniform field, and the kind of conditions under which one might fruitfully pursue ballistics or mineral extraction, for example, are likely to be very different from those under which one might fruitfully pursue questions about the formation and age of the earth. Moreover, we shall see that some areas of natural philosophy were pursued in a way that was closely guided by natural-theological considerations, flourishing—and quite possibly only able to flourish—in an environment that was far from 'neutral'.

[89] John Gascoigne gives a figure of 42% for university-educated natural philosophers born between 1551 and 1650 holding career university posts who were sufficiently noteworthy to be included in the *Dictionary of Scientific Biography*: 'A Reappraisal of the Role of the Universities in the Scientific Revolution', in David C. Lindberg and Robert S. Westman, eds, *Reappraisals of the Scientific Revolution* (Cambridge, 1990), 207–60: 209.

[90] See Stillman Drake, *Galileo at Work* (Chicago, 1978), ch. 1; Thomas B. Settle, 'Ostilio Ricci, A Bridge Between Alberti and Galileo', *Actes du XII^{e} Congrès International d'Histoire des Sciences* (Paris, 1971), 121–6; and Mario Biagioli, *Galileo Courtier, The Practice of Science in the Culture of Absolutism* (Chicago, 1993), 6–8.

[91] See Stephen Gaukroger, *Descartes, An Intellectual Biography* (Oxford, 1995), 62–7. More generally, see Geoffrey Parker, *The Army of Flanders and the Spanish Road, 1567–1659: The Logistics of Spanish Victory and Defeat in the Low Countries' Wars* (Oxford, 1972); and specifically on the professionalization of armies at this time, see Philippe Contamine, *Guerre, État et société à la fin du moyen âge* (Paris, 1972), 536–46.

This prompts a more general question, about whether it might be unhelpfully anachronistic to think about a 'neutral space of enquiry' in the early-modern era. Talk of a neutral space makes sense when we think of science primarily in terms of truth, because what such a space provides is a way of following arguments through to their conclusions without hindrance from prejudice or dogma. But although early-modern natural philosophers were of course concerned with truth, public discussion of the value of natural philosophy tended to turn on its usefulness rather than its truth.[92] This is important if we are to understand the disputes over the legitimacy of the natural-philosophical enterprise. The ultimately successful form taken by the development of early-modern natural philosophy was in no way determined from the outset. Quite the contrary, even the image of natural philosophy as an inherently worthwhile enterprise is not something that was secure in the Scientific Revolution. Within three years of receiving its charter, the newly formed Royal Society almost collapsed through lack of attendance at meetings and lack of funds.[93] While in seventeenth-century Italy, for example, one would be hard pressed to find a Tuscan prince and nobleman who did not at least feign an interest in natural philosophy, in Britain the Royal Society and its members were the subject of a great amount of sharp criticism and ridicule.[94] In a sermon preached at Westminster Abbey in 1667, the Public Orator of the University of Oxford, Robert South, refers to the Fellows of the Royal Society as 'the profane, atheistical, epicurean rabble, whom the nation so rings of, and who have lived so much to the defiance of God'. They are

> a company of lewd, shallow-brained huffs making atheism and contempt of religion, the sole badge and character of wit, gallentry, and true discretion; and then over their pots and pipes, claiming and engrossing all these wholly to themselves; magisterially censuring the wisdom of all antiquity, scoffing at all piety, and, as it were, new modelling the whole world. . . . The truth is, the persons here reflected upon are of such a peculiar stamp of impiety, that they seem to be a set of fellows got together, and formed into a kind of diabolical society, for the finding out new experiments in vice.[95]

King Charles II, the patron of the Royal Society, referred to the Fellows as 'my ferrets', and is reported by Pepys as laughing at Sir William Petty and others on a visit

[92] This is true of Renaissance conceptions as well, except that for Renaissance thinkers the utility of knowledge lay in its capacity to make us wiser and happier, whereas in the early-modern era there is a shift to seeing usefulness in terms of the improvement of our material conditions through increased control over nature.

[93] Contrary to its initial expectations, the Royal Society failed to secure an endowment from the King. This made it almost wholly dependent upon the joining fees and subscriptions of its Fellows, which explains why, by contrast with its continental counterparts, its membership consisted predominantly of men of wealth and high status, whose contribution to natural philosophy was often very small. See Michael Hunter, *The Royal Society and its Fellows 1660–1700* (2nd edn, Oxford, 1994), ch. 2.

[94] Satire and ridicule were not confined to the anti-Royal Society ranks, however. Sprat and Dryden dished it out from the Royal Society side and later Mandeville joined their ranks, and the Royal Society does not seem to have suffered more than its opponents. See Michael Hunter, *Science and the Shape of Orthodoxy: Intellectual Change in Late Seventeenth-Century Britain* (Woodbridge, 1995).

[95] Robert South, *Sermons preached upon Several Occasions* (7 vols., Oxford, 1823), i. 373–5.

to the Royal Society in 1664, 'for spending time only in weighing of ayre, and doing nothing else since they sat'.[96] Oldenberg wrote to Boyle in 1666 that Wren's plans for the rebuilding of London in the wake of the Great Fire should have been put to greater propaganda use since

such a modell, contrived by him, and received and approved by ye R. Society, or a Committee thereoff, before it had come to the view of his Majesty, would have given the Society a name, and made it popular, and availed not a litle to silence those, who aske continually, What have they done?[97]

The concern was still being echoed by Evelyn thirteen years later. ''Tis impossible to conceive', he writes, 'how so honest, and worthy a *design* should have found so few *Promoters*, and so cold a welcome in a *Nation* whose *eyes* are so wide open.'[98]

When Thomas Shadwell's comedy *The Virtuoso* was staged in London in 1676 (it was so popular it ran, on and off, for the next twenty years), natural philosophers were cruelly and publicly lampooned, and Hooke was singled out for special attention in the person of Sir Nicholas Gimcrack, 'who has broken his brains about the nature of maggots; who has studi'd these twenty years to find out the several sorts of Spiders, and never cares for understanding Mankind'.[99] Gimcrack remarks that ''tis below a Virtuoso, to trouble himself with Men and Manners. I study Insects.' At one point, on being discovered imitating the movements of a frog in order to learn how to swim, he declares that he contents himself with the speculative part of swimming, not the practical part, since knowledge, and not use, is his ultimate end.[100] Hooke was severely embarrassed on attending the play, recording in his diary: 'Damned dogs. *Vindica me deus* [God grant me revenge]. People almost pointed.'[101]

In 1709, William King published parodies of the Royal Society's *Philosophical Transactions* in which instructions were included on how to write unintelligibly.[102] Nine years earlier, he had attacked the editor of the *Transactions*, Sir Hans Sloane,

[96] Cited in Lisa Jardine, *On A Grander Scale: The Outstanding Career of Christopher Wren* (London, 2002), 185.

[97] Oldenburg to Boyle, 18 Sept, 1666: Henry Oldenburg, *The Correspondence of Henry Oldenburg*, ed. A. Rupert Hall and Marie Boas Hall (13 vols., Madison, 1965–75), iii. 231.

[98] John Evelyn, *Sylva; or, A Discourse of Forest-Trees, and the Propagation of Timber in His Majesties Dominions* (London, 1679), sig. A3v.

[99] Thomas Shadwell, 'The Virtuoso', in *Complete Works*, ed. Montague Summers (5 vols, London, 1927), iii. 113. Compare Meric Casaubon's comment on the Royal Society, eight years earlier: 'They therefore that would reduce all learning to natural experiments . . . how well they provide for Religion, the peace and tranquillity of publick Estates, the maintenance of truth, whether in matters Civil or Ecclesiastical; and what will be the end of such attempts . . . though such men cannot, or will not yet all wise men may easily foresee' (*Of Credulity and Incredulity in Things Natural, Civill and Divine*, 136).

[100] Richard Baxter, in an undated letter to Boyle, refers to his experimental philosophy as 'recreations', unlike card-playing, but recreations none the less: *The Works of the Honourable Robert Boyle* ed. Thomas Birch (6 vols, London, 1772), vi. 516.

[101] Robert Hooke, *Diary, 1672–80*, ed. H. W. Robinson and W. Adams (London, 1935), 235.

[102] William King, *The Original Works of William King, LL.D.* (3 vols, London, 1776), ii. 57–178.

telling his readers that Sloane,

> hath not so much as neglected an Ear-pick or a Rusty Razor, for he values any thing that comes from the Indies or China at a high rate; for were it but a Pebble, or a Cockle-shell from thence, he'd soon write a Comment upon it, and perpetuate its Memory upon a Coper-plate . . . there is not an odd coloured or an ill shapen pebble in the Kingdom, but the Secretary will manage it so as to make it contribute to the general heap of Transactions.[103]

Magnetism was a particular source of amusement, with the journalist Ned Ward in 1700 dismissing magnets along with the other 'Philosophical Toys' at 'Maggot-mongers Hall' (the Royal Society). His contemporary Thomas Brown describes how physicians, trying to cure a boy who had accidentally swallowed a knife, decided upon a 'more Philosophical' remedy, 'and therefore better approv'd; and that was, to apply a *Loadstone* to his *Arse*, and so draw it out by a *Magnetick* Attraction'.[104] Perhaps the best-known caricature of the Royal Society, however, is the Academy of Lagado on the island of Laputa ('whore' in Italian)—a caricature of Bacon's *New Atlantis*—in Jonathan Swift's *Gulliver's Travels*, first published in 1726.[105] Here the cranks of unworkable machines are turned, and attempts are made to store sunbeams in cucumbers, to be opened on cold days to provide heating. This hostility towards natural philosophy, which continued well into the nineteenth century,[106] was never confined to Great Britain. In 1740, Linnaeus wrote:

> one question is always asked, one objection always made to those who show curiosity about nature, when ill-educated people see natural philosophers examining its products. They ask, often with contempuous laughter, 'What use is it?' . . . Such people think that natural philosophy is just about the gratification of curiosity, just an amusement to pass the time for lazy and thoughtless people.[107]

[103] *William King, The Original Works of William King, LL. D.* i. 14–16. Cf. Alexander Pope's lines in the *Dunciad*: 'Impale a Glow-worm, or Vertu profess | Shine in the dignity of F.R.S.' King is not completely off-target on the interest in pebbles, and stones more generally. See the letter from Hooke to Boyle, 5 June 1663 in R. T. Gunther, *Early Science in Oxford* (15 vols, Oxford, 1923–67), vii. 132–3.

[104] Both quoted in Patricia Fara, *Sympathetic Attractions: Magnetic Practices, Beliefs, and Symbolism in Eighteenth-Century England* (Princeton, 1996), 159–60. It is just possible that Brown may have known of the Swiss physician Wilhelm Fabricius von Hilden, who, some time in the early decades of the seventeenth century, used a magnet to remove an iron splinter.

[105] The 'High Tory' satirical reaction to science in the eighteenth century is usefully summarized in Richard G. Olson, 'Tory-High Church Opposition to Science and Scientism in the Eighteenth Century: The Works of John Arbuthnot, Jonathan Swift, and Samuel Johnson', in John G. Burke, *The Uses of Science in the Age of Newton* (Berkeley, Calif., 1983), 171–204.

[106] The change in the fortunes of science in *Punch*, from a satirical attitude in the 1840s to a more respectful one as the the century progressed, is traced in Richard Noakes, '*Punch* and Comic Journalism in Mid-Victorian Britain', in Cantor et al., *Science in the Nineteenth-Century Periodical*, 91–122.

[107] Charles Linnaeus, *L'Équilibre de la nature*, trans. B. Jasmin, introd. C. Limoges (Paris, 1972). 145–6. Such 'lazy and thoughtless' people could, however, occasionally be awakened from their slumbers by natural-philosophical spectacles. A report in the *Gentleman's Magazine* for 1745 (vol. 15, p. 194) notes that demonstrations of electrical phenomena were 'so surprising as to awaken the indolent curiosity of the public, the ladies and people of quality, who never regard natural philosophy but when it works miracles'.

Likewise on the other side of the dispute: what defenders of the value of natural philosophy from Francis Bacon onwards defended and promoted was its usefulness. Indeed, it was these defences of the usefulness of natural philosophy that came first, and they set the terms on which questions of its value were disputed. Bacon tell us in *Novum Organum* that 'the true and lawful goal of the sciences is none other than this: that human life be endowed with new discoveries and powers'.[108] Christopher Wren takes up the theme in his draft of the Royal Society's charter:

The Way to so happy a Government, we are sensible is in no manner more facilitated than by the promoting of useful Arts and Sciences, which, upon mature Inspection, are found to be the basis of civil Communities, and free Governments, and which gather multitudes, by an *Orphean* Charm, into Cities, and connect them in *Companies*; that so, by laying in a Stock, as it were, of several Arts, and Methods of Industry, the whole body may be supplied by a mutual Commerce of each others peculiar faculties; and consequently that the various Miseries and toils of this frail Life, may, by the Wealth and Plenty be diffused in just Proportion to every one's Industry, that is, to every one's Deserts.[109]

This theme also forms the core of Sprat's vindication of the Royal Society, set out in detail in Part 3 of his *History of the Royal-Society* (1657).[110] The title page of Glanvill's *Plus Ultra* (1668) contrasts the 'practical, useful learning' of the new natural philosophers with 'the notional way'.[111] It continues throughout the seventeenth and eighteenth centuries, with Priestley, one of the great spokesmen for the value of science, writing in 1768 that

all knowledge will be subdivided and extended; and knowledge, as Lord Bacon observes, being power, the human powers will, in fact, be increased; nature, including both its materials, and its laws, will be more at our command; men will make their situation in this world abundantly more easy and comfortable; they will probably prolong their existence in it, and will grow daily more happy, each in himself, and more able (and, I believe, more disposed) to communicate happiness to others.[112]

Even Huxley's defence of science, a century later, still turns on it usefulness. He talks of a 'new nature' created by science and manifested 'in every mechanical artifice, every chemically pure substance employed by manufacture, every abnormally fertile race of

[108] Francis Bacon, *The Works of Francis Bacon* ed. James Spedding, Robert Leslie Ellis, and Douglas Denon Heath (14 vols, London, 1857–74), iii. 79.

[109] Text as given in Stephen Wren, *Parentalia: or, memoirs of the Family of the Wrens; viz. Of Mathew Bishop of Ely, Christopher Dean of Windsor, &c. but chiefly of Sir Christopher Wren, late Surveyor-General of the Royal Buildings, President of the Royal Society, &c. &c.* (London, 1750), 196–7.

[110] Thomas Sprat, *The History of the Royal-Society of London for the Improving of Natural Knowledge* (London, 1657).

[111] This contrast drove Meric Casaubon to write his *A Letter to Pierre Moulin . . . Concerning natural experimental Philosophie* (Cambridge, 1669), and to ask: 'What is it that these account *useful*, and *useless*?' (5).

[112] Joseph Priestley, *An Essay on the First Principles of Government* (London, 1768), 6. On this whole question of the usefulness of science in the eighteenth century, see Larry Stewart, *The Rise of Public Science* (Cambridge, 1992).

plants, or rapidly growing and fattening breed of animals'. This new nature, we are told, is

> the foundation of our wealth and the condition of our safety from submergence by another flood of barbarous hordes; it is the bond which unites into a solid political whole, regions larger than any empire of antiquity; it secures us from the recurrence of pestilences and famines of former times; it is the source of endless comforts and conveniences, which are not mere luxuries, but conduce to physical and moral well-being.[113]

To the extent to which the discussion of the value of science turns primarily on usefulness, the idea of a 'neutral space for enquiry' is irrelevant.

The second question is that of the role of an adversarial culture. This turns out to be a very complex issue, as we shall see in the chapters that follow, but the crux of what is at stake can be set out succinctly. Huff's argument is that for scientific innovation one needs an adversarial culture. However, when we start to look at how early-modern natural philosophers describe the circumstances needed to foster innovation, the first thing they criticize is an adversarial culture. If Huff's analysis is correct, the combination of a staunchly adversarial culture within a relatively autonomous corporate structure, the university, should characterize early-modern natural philosophy. But it does not. Rather, it characterizes the far less fruitful, radically adversarial, scholastic natural philosophy of the universities of Paris and Oxford in the thirteenth and fourteenth centuries. There can be no doubt this was an innovative natural-philosophical culture, but it was one that was not consolidated, ultimately following the standard boom/bust pattern. When natural philosophy was revived in sixteenth-century Europe, it was nurtured in a very different kind of culture, and predominantly outside scholasticism. Indeed, its distinguishing feature was an unqualified wholesale rejection of an adversarial approach, which was almost universally seen, outside scholastic circles, as characteristic of sterile, unproductive dispute for its own sake, without regard to use or truth. Far from encouraging innovation, key early-modern natural philosophers such as Bacon, Descartes, and Boyle explicitly saw adversarial method as representative of an especially fruitless form of argument which cut any progress and innovation off at its roots. Bacon sums up the situation nicely in his criticism of Aristotle in Book 2 of the *Advancement of Learning*:

> And herein I cannot a little marvel at the philosopher Aristotle, that did proceed in such a spirit of difference and contradiction toward all antiquity; undertaking not only to frame new words of science at pleasure, but to confound and extinguish all ancient wisdom; inasmuch as he never nameth or mentioneth an ancient author or opinion, but to confute and reprove.[114]

Glanvill, pre-eminent Royal Society apologist, puts the point even more dramatically. 'Peripatetick Philosophy', he tells us, 'is *litigious*, the very spawn of *disputations* and *controversies* as undecisive as needless. This is the natural result of the former: *Storms* are the products of *vapours*.'[115] Bacon's own recommended approach is in marked contrast with what he considers to be the Aristotelian one:

[113] Huxley, *Collected Essays*, i. 51. [114] Bacon, *Works*, iii. 352.

[115] Joseph Glanvill, *Scepsis Scientifica: or, Confest Ignorance, the way to Science; in an Essay of The Vanity of Dogmatizing, and Confident Opinion* (London, 1665), 118.

I like better that entry of truth which cometh peaceably with chalk to mark up those minds which are capable to lodge and harbour it, than that which cometh with pugnacity and contention.[116]

It would certainly be an exaggeration to say that adversarial culture plays no part at all in early-modern natural philosophy—Galileo's *Dialogo* employs adversarial techniques, for example, and not just at the dramatic level—but its role is so far from being straightforward that it is an unlikely candidate for one of the characterizing features of early-modern natural philosophy.

The gradual consolidation of a scientific culture in the early-modern era, to the extent to which it occurred before the second half of the nineteenth century, was, then, not due an adversarial approach, or to the existence of some neutral space for enquiry, or to an enthusiasm for science on the part of the public or an educated elite. Nor, it should be noted, was it due to any practical benefits. Unlike science in China, for example, where the practical benefits were often immediate,[117] or medieval Islam, where they were at least direct if not immediate, the Scientific Revolution produced very little of any practical benefit for a long time. Ballistics and reliable clock mechanisms were the outcome of work in kinematics,[118] but generally speaking where there was some practical pay-off, in military and public architecture and ship design for example, it was hardly ever the new mathematical physics that produced the goods but developments in traditional Alexandrian disciplines such as statics and hydrostatics.[119] Furthermore, the pioneers were rarely scientists. As Peter Mathias notes in connection with the Industrial Revolution in England, the country where the changes first became apparent:

by and large, innovations were not the result of the formal application of applied science, nor the product of the formal educational system of the country. . . . Most innovations were the products of inspired amateurs, or brilliant artisans trained as clockmakers, millwrights, blacksmiths or in the Birmingham trades. . . . They were mainly local men, empirically trained, with local horizons, often very interested in things scientific, aware men, responding directly to a

[116] Bacon, *Works*, iii. 363. It is worth pointing out, however, that there is sometimes a gap between his recommended and his actual approach: see Stephen Gaukroger, *Francis Bacon and the Transformation of Early Modern Culture* (Cambridge, 2001), 105–14.

[117] See Mokyr, *The Lever of Riches*, ch. 9.

[118] Nevertheless, the importance of scientific advances to early ballistics should not be overestimated. John F. Guilmartin, *Gunpowder and Galleys: Changing Technology and Mediterranean Warfare at Sea in the Sixteenth Century* (Cambridge, 1974), points out that gunners would have had little use for instruments such as quadrants, preferring their own experience in the field. It should also be noted that it was only in the nineteenth century that weapons manufacturers explored the implications of seventeenth-century ballistics in a systematic way: see A. Rupert Hall, *Ballistics in the Seventeenth Century: A Study of the Relations between Science and War with Reference Particularly to England* (Cambridge, 1952), 158. See also idem, 'Gunnery, Science, and the Royal Society', in John G. Burke, ed., *The Uses of Science in the Age of Newton* (Berkeley, 1983), 111–42.

[119] In the case of public architecture see James A. Bennett, *The Mathematical Science of Christopher Wren* (Cambridge, 1982) and Lisa Jardine, *On A Grander Scale*. Shipbuilders in the seventeenth and eighteenth centuries were highly skilled but had little formal education, and certainly little use for new developments in natural philosophy: see Larrie D. Ferreiro, *Ships and Science: The Birth of Naval Architecture in the Scientific Revolution, 1600–1800* (Cambridge, Mass., 2006), ch. 2.

particular problem. Up to the mid-nineteenth century this tradition was still dominant in British manufacturing industry. It was no accident that the Crystal Palace in 1851, a miracle of cast iron and glass like the great railway stations of the nineteenth century, was the conception of the head gardener of the Duke of Devonshire. He knew about greenhouses.[120]

Indeed, it is not just that scientists were not the innovators behind the Industrial Revolution, but that for much of its early years advances in science had little bearing on technological advances. As Mokyr has pointed out, 'most of the devices invented between 1750 and 1830 tended to be a type in which mechanically talented amateurs could excel. In many cases British inventors appear simply to have been lucky . . . When, after 1850, deeper scientific analysis was needed, German and French inventors gradually took the lead.'[121] It is also worth remembering in this context that machines, which began to be introduced in a limited way in areas such as mining and cotton manufacturing from the eighteenth century,[122] were so slow until the use of the steam turbine, the internal-combustion engine, and the electric motor, that engineers used statics to describe the action of machines before the 1880s.[123] The other significant developments—such as the mass production of fertilizers and glass, light engineering, the use of thermometers (two hundred years after their invention) by physicians, and the use of anesthetics, not to mention fundamental domestic

[120] Peter Mathias, *The First Industrial Nation: An Economic History of Britain, 1700–1914* (London, 1983), 124–5. See also A. Rupert Hall, 'What Did the Industrial Revolution in Britain Owe to Science?', in Neil McKenrick, ed., *Historical Perspectives: Studies in English Thought and Society in Honour of J. H. Plumb* (London, 1974), 129–51. A parallel point is made in the context of the Royal Society in the seventeenth century in Marie Boas Hall, 'Oldenburg, The *Philosophical Transactions*, and Technology', in John G. Burke, ed., *The Uses of Science in the Age of Newton* (Berkeley, 1983), 21–47. Cf. Thomas Kuhn, *The Essential Tension* (Chicago, 1977), who notes what he terms the backwardness of science in Britain around the time of the Industrial Revolution, concluding that it played an insignificant role in the technological changes (141–5). See also Neil McKendrick, 'The Role of Science in the Industrial Revolution: A Study of Josiah Wedgwood as a Scientist and Industrial Chemist', in M. Teich and R. Porter, eds., *Changing Perspectives in the History of Science* (London, 1973), 274–319.

[121] Mokyr, *The Lever of Riches*, 244.

[122] Such machines often had very limited purposes before the nineteenth century. Newcomen's engine, for example, was devised in 1714, but was confined to pumping water, which was of limited value until a coal economy took off in Britain in the nineteenth century, where (with the aid of steam power from the end of the eighteenth century) it became crucial in enabling the working of deeper shafts. Note also that there was often a significant gap between the invention of a machine and its full mechanization. Samuel Crompton's 'Spinning Jenny' was invented in 1775, and although it had an immediate impact, it was not fully mechanized until the mid-1830s. Still, the 1820s are the key period in mechanization, with the extensive introduction of power looms throughout textile manufacturing. On the 'industrial revolution' generally see chs. 2 to 4 of David S. Landes, *The Unbound Prometheus: Technological Change and Industrial Development in Western Europe from 1750 to the Present* (2nd. edn, Cambridge, 2003), and, for a broader perspective, Mokyr, *The Lever of Riches*. One important issue at stake in understanding the eighteenth- and early ninteenth-century development of capitalism is the relative importance and priority of organization of work practices rather than technological developments as such (in Marxist terms, relative priority of relations of production over forces of production): see Keith Tribe, *Genealogies of Capitalism* (London, 1981). It is of interest here that the great eighteenth- and early nineteenth-century political economists—Smith, Malthus, and Ricardo—think of production almost exclusively in terms of agricultural, not industrial, production.

[123] J. P. Den Hartog, *Mechanics* (New York, 1961), 2.

improvements such as piped water, underground sewage, better food, softer clothing, warmer homes, and street lighting—are likewise developments dating from the third decade of the nineteenth century at the earliest. Despite the claims of its advocates for the usefulness of natural philosophy, these were at best promissory notes before then.

To what, then, was the gradual consolidation of a scientific culture in the West due? It is to this nexus of questions—for, as we shall see, there are a number of fundamental questions involved—that we now turn, and we shall begin by looking at the basis on which natural philosophy came to occupy a central place in medieval and then early-modern culture.