EPILOGUE

A DUAL LEGACY

Soon after his visit to the Academy of Lagado (p. 481), Captain Lemuel Gulliver availed himself of the opportunity granted him on the isle of Glubbdubdrib to converse with the dead. In 1726, one year before Newton’s demise, he reported as follows:

I then desired the Governor to call up Descartes and Gassendi, with whom I prevailed to explain their systems to Aristotle. This great philosopher freely acknowledged his own mistakes in natural philosophy, because he proceeded in many things upon conjecture, as all men must do; and he found, that Gassendi, who had made the doctrine of Epicurus as palatable as he could, and the vortices of Descartes, were equally exploded. He predicted the same fate to attractions, whereof the present learned are such zealous asserters. He said, that new systems of nature were but new fashions, which would vary in every age; and even those who pretended to demonstrate them from mathematical principles would flourish but a short period of time, and be out of vogue when that was determined.405

As before with the extraction of sunbeams from cucumbers, Jonathan Swift had it both right and wrong. With unfailing precision he now pointed at the very thing that, pace the captain, had altered for good between Aristotle and Newton, or even between Descartes and Newton. For the first time in history, the pursuit of nature-knowledge was no longer helplessly subject to change of fashion. ‘Attractions’, as embodied in Newton’s discovery of the first of just four forces that ultimately underlie all nature’s phenomena, have not ‘flourished but a short period of time’. Other than with Aristotle’s ‘mistakes’ or Descartes’ ‘vortices’, universal gravitation has come to stay. So have the ‘mathematical principles’ that, in Newton’s book of that name, establish ‘attraction’ and define its measure. It has now been more than three centuries since mathematical principles, in ongoing interplay with rigorous experimentation, have proven themselves capable of deciding in the end what in scientific research is just a matter of ‘vogue’ and what transcends fashion such that it can provide us with enduringly valid knowledge of nature.

‘Enduringly valid knowledge’ – is that article actually available on the market of ideas?

The 1980s have seen a return to ghostly ‘Aristotle’s’ conception of scientific change as running on fashion. As subjective preferences shift, and the power relation between negotiating practitioners alters, so, allegedly, do our views of how the natural world is constituted
This particular variety of the ‘fashion’ conception was first developed by ‘sociologists of scientific knowledge’, with an extreme wing claiming that the rules that scientists follow are just as arbitrary as those of baseball. Out of this came the so-called Science Wars, fought out between those who adopted this conception, albeit most often in some watered-down variety, and practicing scientists who felt their commitment to a lifetime of sometimes successful struggle with the very realities of nature to be unjustifiably wronged thereby.

Numerous historians of science for their part have since preferred in sophisticated accounts of some artfully contextualized episode to take a seemingly neutral position and just bracket the possible validity of the claim to truth so often made by our own protagonists, the scientists whose achievement we investigate. Unfortunately, the practice entails our losing sight of a distinctive, not to say decisively important aspect of our chosen subject: the apparent human ability to gain a reliable grasp of natural reality. After all, as Steven Shapin observed on the final page of his 1996 *The Scientific Revolution*, albeit without drawing any consequences from the observation, science is “certainly the most reliable body of natural knowledge we have got”. Is it not our historians’ duty, then, to find out when and how science became so reliable in the first place?

Indeed, it is about time once again to look our protagonists’ claim to truth squarely in the face. A priori reaffirmation of the validity of that claim, however, will not do. In the present book I have deliberately refrained from accepting without more ado the position of either party in the Science Wars. Instead, I have chosen to historicize the issue. For each of those six revolutionary transformations that I have found to make up together the sum total of the Scientific Revolution – Alexandria-plus, Athens-plus, fact-finding experimentalism, and three post-1660 blending efforts – I have investigated the empirical way whereby protagonists actually sought in their own practice to make their claims about natural reality stick. The outcome has turned out to be highly differentiated among these revolutionary episodes. Notably, far more robust procedures emerged in realist-mathematical than in fact-finding experimental science (p. 619). The culmination point of both was attained by Newton, in his ongoing fight against others’ and his own ‘fansying’, in *Principia* and in *Opticks* (p. 713).

The differences between the various ways in which practitioners of each revolutionary mode of nature-knowledge sought to curb fancy are vastly intriguing. The inevitably groping manner in which the pioneers and their immediate successors probed such ways reveals the Scientific Revolution to have served, as it were, as a laboratory in which this wholly unknown terrain was first explored. How, between the Scylla of dogmatic assertion and the Charybdis of all-pervasive, skeptical doubt, to ferret out ways and means to anchor their novel findings in reality? And yet, for all the vast differences between these ways and means that remained over the 17th century, some ground was shared between them. Out of the Scientific Revolution came something of still wider import: procedurally and institutionally established self-correction.
Significant cases of self-correction manifested themselves right from the start of the Scientific Revolution. Very well known are Kepler’s one-man decision to drop his own unprecedentedly accurate, painstakingly arrived-at hypothesis on the orbit of Mars in view of the tiniest of empirical discrepancies (the famous eight minutes of arc; p. 171) and Galileo’s one-man realization that his original supposition of speed in vertical descent increasing with distance ran into a dead end and had to be replaced with an increase in proportion with time (p. 194). These events, to be sure, did not reflect changes of mood, let alone changes of fashion; rather, both men drew radical consequences from a dilemma that their own investigations presented them with. They were of course at liberty to face the dilemma or evade it or even ignore it, whatever way out might please them best, yet if faced it compelled the very correction both men went on to apply.

What was still a matter of private decision with the pioneers (albeit prepared to some extent in the handling of models in Ptolemaean astronomy) quickly became accepted procedure as revolutionary transformation caught on. Not that self-correction need remain confined to the investigator himself, as with Torricelli’s vacillations over water outflow, taken up by Huygens and then again by Varignon (p. 340). Gilbert’s large-scale winnowing of facts and lore regarding amber and the lodestone is an early example of systematic correction of possibly erroneous facts maintained by predecessors, in ways that did not (as with the scholastics) oppose opinion to counteropinion but genuine fact to apparently spurious fact.

Here, too, there were incidental precedents (as with Castro), but this time the practice, now followed more systematically, caught on at once. Within decades of the onset of the Scientific Revolution correction of oneself and/or others was built into regular procedure and subsequently enshrined in the first institutions of science, where ‘provando e riprovando’ (the Accademia del Cimento’s motto; p. 487) became standard practice.

What made these 17th-century developments so decisively important is the dual makeup of human cognition. We are blessed with a trenchant capacity to grasp realities with which the world confronts us. But that capacity is accompanied by an at least equally powerful capacity to enshrine the ideas in which we seek to capture those realities in allegedly timeless, dogmatically held truths or (with Swift’s ghostly ‘Aristotle’) to move from one ‘vogue’ to the next or – riskiest of all – to get lost in flights of fancy. The agents that enable us time and again to lose sight of reality have been identified by Francis Bacon as the ‘idols’ that so easily come to bewitch our minds (p. 486). We are subject to bias of the most varied kinds, from self-serving distortions to an even more powerful inclination to take our wishes for reality. In no human endeavor has our capacity to grasp reality in any of its varied aspects exceeded the level of a rare conjunction of individual gifts – except for recognizably modern science. Nowhere else have viable ways been found yet to turn the possible correction of our fondest assertions into standard practice that may serve, not just the exceptionally gifted and reality oriented, but everyone with a solid training in those procedures.
To be sure, a solid training in procedures does not guarantee the making of discoveries – for that, solid training must be accompanied by exceptional gifts. Nor does institutionally and procedurally established self-correction lead to valid results without fail. Recall how Newton, feeling in good empirical conscience compelled to adjust his mathematical model to new measurements of the speed of sound, spoiled the original, straightforward simplicity of the model by means of a ‘fudge factor’, and how it took almost a century until Laplace found the true grounds of the discrepancy to reside in the heat that sound waves engender (p. 494). The point is rather that, as one major outcome of the Scientific Revolution, institutionally and procedurally established self-correction has come to be built into how practitioners routinely operate.

Of course, with practitioners’ growing familiarity with the ins and outs of procedural self-correction, the practice has over time become more and more refined. The Scientific Revolution did not in any way complete the search for how to achieve self-correction in the most rigorous and productive manner possible – it only marks the very onset of that never-ending search.

And so it is in just about every other respect as well. To be sure, the Scientific Revolution was, first and foremost, a historical phenomenon. It was a clearly circumscribed episode, coherently made up of six distinct yet variously intertwined revolutionary transformations. With the completion of the sixth one, Newton’s, the episode came to a well-marked end. But from the point of view of well-nigh everything else, the Scientific Revolution set in motion a train of events that has yet to cease. With respect to the methods of science; its contents; its professional status; its institutional bearings; its societal preconditions and underpinnings; its capacity to revolutionize the arts and crafts; its expansion worldwide; claims made on its behalf for a monopoly on rational thought and for leading inexorably to a truly scientific worldview; and also the countercurrents provoked by science – in all these and many more respects vast extensions and huge changes have kept occurring. Together these have resulted in the Scientific Revolution bequeathing to the generations that followed a dual legacy: a legacy of gains attained and conquests made but also of losses suffered. To treat the legacy at the length it deserves would at least triple the length of this book and turn it into a survey of the entire history of science. But a few remarks on the major extensions and changes just listed may serve as a fitting epilogue to the conception of the Scientific Revolution here put forward.

Expanding modern science

Take first of all the way in which the prime culmination point of the Scientific Revolution, Newton’s dynamics and the law of universal gravitation as expounded in the *Principia*, has
over time undergone several major alterations. Starting in the 1720s, Newton’s works gave rise to an extended textbook tradition. Even earlier Varignon in France and a few British scholars with a penchant for mathematics began to reorganize the main message of the *Principia* and to modernize its mathematical language, with the latter sticking to Newton’s calculus of fluxions and the former preferring Leibniz’s differential calculus (p. 360). Later in the 18th century mathematical scientists like d’Alembert, Euler, and Lagrange made big strides in the same direction, while in addition pursuing a further differentiation between a variety of concepts of force (p. 622). This led among many other things to shearing ‘force’ of its remaining substantive connotations and enveloping it for good in a more abstract structure of space and time. The effort culminated in the mid-19th century in the coining of a clear-cut concept of energy destined to replace the primacy that Newton had envisaged for all those forces he mathematically prepared for in the *Principia* (p. 666).

But Newtonian force was still much more than a key topic for mathematical scientists. By the late 1720s it acquired wider meanings as the contested cornerstone of a closed system of natural philosophy, loaded with ideological connotations, that went by the name of Newtonianism. As such it played its part in a renewed process of compartmentalization. From the early 18th century onward, and due in part to the increasing consolidation and prominence of the states of England, France, and Prussia, mathematical science, fact-finding experimentalism, and speculative natural philosophy were once again pursued by and large in mutual isolation, with the latter being split up along national lines in three systematized philosophies, Newtonianism in England, Cartesianism in France, and, in German lands, a concoction of ideas first thought up by Leibniz. Not until the turn of the 19th century, when the French Revolution put so much else in flux as well, were the fences broken down all over again but now for good. This was just one major portion of a process of drastic change on many fronts that well deserves the label the ‘Second Scientific Revolution’ recently given to it. As that revolution unfolded, the term ‘science’ came to stand for the consolidated product of decompartmentalization, accompanied in its turn by its practitioners becoming ‘scientists’ and their vocation turning into the full-fledged profession it has remained ever since.

Regarding universal gravitation, for a long time all predictions entailed by the theory that came up for possible confirmation were confirmed indeed, as notably with the discovery of Neptune consequent upon the detection of small apparent deviations of Uranus’ actual from its calculated trajectory. Not until the early 20th century, when the advent of relativity theory at Einstein’s hands revealed universal gravitation to be a property of space-time curvature, did it turn out, at velocities near the speed of light, to be no more than approximately valid. Even so, the enduring validity of the mathematical law of universal gravitation but for extreme cases was resoundingly confirmed all over again when in the 1950s rockets became powerful enough to overcome the limitations imposed by the escape velocity that the law entails and to start the exploration of outer space. Take, for instance,
the calculated launch date and the fourfold ‘gravity assist’ that enabled a spacecraft named after Cassini and Huygens, which was launched on 15 October 1997 from Cape Canaveral, to swing by three planets in succession so as to accelerate it sufficiently to reach Saturn within seven years:

The primary launch period for Cassini, based on the alignment of the planets and the capabilities of the Titan IV/Centaur launch vehicle, was in October 1997. The launch boosted the spacecraft into a Venus-Venus-Earth-Jupiter Gravity-Assist (VVEJGA) trajectory toward its final destination of Saturn. 807

This feat was a fitting demonstration of the power and precision of what Isaac Newton began by 1685 to consider in earnest and then managed to maintain against three possibly lethal objections of his own devising (p. 656). When on the predicted date of July 1, 2004, Cassini smoothly slipped into its well-planned orbit around Saturn, even Jonathan Swift might have chosen to reconsider his ‘fashion’ judgment . . .

The scientific enterprise has kept expanding in other ways as well. Recall how speedily topics that prior to 1600 used to be treated in the wholly abstract manner of prerevolutionary Alexandria or of prerevolutionary, as yet scarcely experimental empiricism were absorbed into Alexandria-plus and into the practice of fact-finding experimentation (p. 600; 614). Since then, in stark contrast with the ‘world of the more-or-less’ from before the Scientific Revolution, ever more domains of natural phenomena and of human experience have come to be absorbed by what Alexandre Koyré dubbed ‘the universe of precision’ (p. 160). Its prime manifestation has been by means of numbers. For instance, the phenomenon of color, which natural philosophers like Aristotle treated as irredeemably qualitative, has proved analyzable in terms of wavelengths, with each color corresponding to exactly so many angstroms.

In due course, ever more natural phenomena, like those treated in chemistry and then biology, have similarly proven amenable to being handled in rigorously quantitative terms with great success. In at least partial contrast, in the human sciences it has become a perennial bone of contention whether across-the-board quantification can usefully supplement, or perhaps even replace, more qualitative analyses, or whether instead quantification tends to mask or even call into being the very events and states of affairs it is meant only to pinpoint. On the one hand, there is the perennial temptation eloquently expressed in 1793 by the marquis de Condorcet, and still adopted without much reflection by many a present-day scientist dabbling in the social sciences and the humanities:

If man is capable of predicting, with almost full assurance, the phenomena of which he knows the laws; if even when these are not known to him he is capable from past experience to foresee
with great probability the events of the future; why then would we regard it as a chimerical enterprise to draw with a certain amount of likelihood the table of future destinies of human-kind from the results of its history? The sole foundation of belief in the natural sciences is this idea that the general laws (be they known or not) that rule the phenomena of the universe are necessary and constant; and for what reason would this principle be less true for the development of the intellectual and moral faculties of man than for the other operations of nature?

On the other hand, there is the ‘revolt against positivism’ around the turn of the 20th century, engaged in by social thinkers like Weber and Durkheim and Croce and Freud, out of which came an armory of arguments against such a lawlike approach to social reality and also a range of tools for how instead to come to grips with the inner world and the culture and society that intentional human beings create in their own right. Overall, however, the prestige of the natural sciences and their successes on their own terrain are such as in our day to have their standards and methods bureaucratically enforced upon humanistic disciplines that by the very nature of their subject matter can profit from those standards and methods but little.

Outside academia, in everyday life, the scope of the quantitative likewise keeps expanding. Nowadays we no longer just spend (or idle away) time as students; rather, we fulfill a prescribed number of internationally standardized, administratively serviceable units called ‘study points’ or ‘credits’. Similarly, we no longer just contemplate nature but take part in the organized enjoyment of measurable ‘nature values’. It is a matter of personal preference whether in such quantification measures we value the orderliness and the semblance of unadulterated objectivity or rather resent the effacing of individual difference and the increase of control. Yet so much is certain that the invention and global spread of a wonderfully handy machine which in its obstinately binary way treats the entire universe as if it were composed solely of 0s and 1s has given a big boost to the predominance of the ‘universe of precision’ that was instigated in Europe some four centuries ago.

In tandem with such expansion into our daily lives of the apparent ways of modern science has gone an equally incisive, geographic expansion. In our time science has definitely become an operation spanning the globe, but the first vestiges of the process appear already in the very period of the Scientific Revolution or its immediate aftermath. What is meant here is not so much the ‘cultural transplantation’ of nature-knowledge of Greek provenance treated in chapters 2–5 but rather efforts undertaken in other civilizations to come to terms with certain wished-for attainments of Europe’s Scientific Revolution. Three instructive early examples concern the Ottoman Empire, China, and Russia.

The Ottoman Empire shared borders with Europe, and diplomats, merchants, and fugitives kept moving both ways, so contacts were close enough to enable the court at Istanbul to acquaint itself already in the 16th century with a carefully made selection of possibly useful
advances in, notably, gunnery and mapmaking as cultivated in Europe. The first product of the Scientific Revolution to be translated in this way into Arabic and then Turkish was a set of detailed tables compiled c. 1640 by a French astronomer on a Copernican basis. The translator, a native from present-day Hungary who in the 1660s presented his manuscript to the chief astronomer at the Ottoman court, rendered the book in accordance with a centuries-old tabulation genre, known as zij, to which al-Battani in the 10th (3rd) century and many others contributed and which was concerned almost exclusively with the ongoing correction and improvement of stellar and planetary data and parameters (p. 58). In his introduction, the translator did not address the conceptual issues involved but rather, in line with that tabulating tradition, treated Copernicus, Tycho, and Kepler as latter-day zij authors.

When in 1683 the army besieging Vienna was decisively defeated, the Ottoman perception of Europe changed. Apparently, Europe had more to offer than some useful bits and pieces, as it had meanwhile acquired a general superiority in the area of science and even more so in the military and naval arts. Consequently, translation and study began to cover a wider terrain, albeit still with a marked tendency to confine the science to its utilitarian and instrumental over its theoretical and conceptual aspects (p. 69). Early in the 19th century educational institutions were founded to supply their students with secular learning not on offer in the madrasas. Even so, the unbroken reliance on the centuries-old Golden Age of Islamic nature-knowledge made it possible for the principal of the Imperial Engineering School in 1834 to “explain the systems of Ptolemy, Tycho Brahe and Copernicus at length and in full technical detail”, all the while regarding heliocentrism as “quite possibly mistaken”.

An even more telling example of how much the new kept as yet being taken up in the framework of the old is the manner in which the same authority handled basic findings of post-Lavoisier chemistry; in his own words:

Some earlier scholars held that there were only four elements, but modern scholars of chemical science claim that there are more than four, and that water, air, and earth, previously considered to be elements by old scholars, actually consist of more than one element.

Unlike with the Turks, acquaintance with products of European nature-knowledge came to China by way of the Jesuit mission, which the emperor first allowed to enter Beijing in 1601. In line with the current conversion policy (p. 495), its head, Matteo Ricci SJ, aimed to persuade the local intellectual elite of the unmistakable superiority of mathematics and astronomy as cultivated in Europe and to use this superiority, once acknowledged, as a ready means to convert the court mandarins, and then the population at large, to the Catholic faith. Together with an early convert he translated the first six books of Euclid’s Elements into Chinese, preceded by an extensive preface in which, following Clavius but also with a shrewd eye to the predominantly practical orientation of Chinese nature-knowledge (p. 45),
he extolled the many advantages for everyday life that a knowledge of demonstrative geometry had to offer. But Ricci’s prize asset was astronomy. The Ming ‘season-granting system’ (p. 41) had remained unaltered for the entire duration of the dynasty, and the prediction of eclipses was off by more than half an hour. Eager to point this out and to offer improvement from European resources, Ricci’s successor, Adam Schall von Bell, attained the very directorate of the emperor’s Astronomical Bureau (p. 42).

The job, however, also entailed responsibility for the establishment of lucky days. Accusations of having selected an inauspicious day and site for the burial of an infant prince, coupled to the political troubles that went with the forceful replacement of the Ming by the Ch’ing (Manchu) dynasty, caused the fall of ‘the second Adam’ in 1661. However, when seven years later the second Manchu emperor, young Kang Shi, in a move to get rid of his regents, wished to reinstall the Jesuits as reformers and guardians of the season-granting system, he set up a competition for the directorate between Schall’s successor as head of the mission, Ferdinand Verbiest, and native astronomers, who had meanwhile regained control over the bureau. Of course, ever since 1616 the Jesuits were compelled to work under the handicap of the official prohibition of Copernicus, which led to some planetary double-dealing that did not escape their Chinese opponents. Also, in his later account of the event Verbiest acknowledged that in executing the range of tests set for him he had been the recipient of several strokes of good luck (e.g., bad weather masked errors consequent upon his less-than-optimal tables). However, and this is a key point, for all the vast complexities of the local context in which the ‘duel’ took place, under the fair conditions the emperor set for it the superiority of the new astronomy, even if in a mutilated guise, was plain to see.

Not that the Jesuits’ mission succeeded in its principal aim of large-scale conversion. Among other setbacks, repeated accusations from the Jesuits’ own ranks that their errant brethren in faraway China were dangerously ‘going native’ proved decisive in the end for the Roman headquarters calling the mission back home. Also, the local repercussions of the far-reaching reforms instituted by the Jesuits in the Ming season-granting system proved too disturbing for ongoing adoption, so much did the system govern everyday life for the ordinary Chinese. All in all, the modes of nature-knowledge embodied in organic materialism, on the one hand, and in Alexandria and Athens in any of their varieties, on the other, proved too incompatible for large-scale adoption under the conditions of relative equality that as yet obtained. Not until the 19th century, when the pressure of Europe’s military superiority was brought home to China the hard way, did large-scale adoption begin in earnest.

Russia’s introduction to products of the Scientific Revolution began with the translation, under Czar Peter the Great’s personal auspices, of a posthumous work by Christian Huygens, Kosmotheoros. Before it came out in 1717, the latest exposition known in Russia of how the natural world is constituted was a 6th-century tract by a monk intent on explaining Genesis. Kosmotheoros (1696) is a running speculation about life on other planets, driven by
persistent analogical reasoning that takes its point of departure in the view of the universe
Huygens settled on during the final years of his life – filled with spherical whirlpools, yet
governed mathematically by what Huygens took to be the acceptable portion of Newton's
inverse-square law (p. 676). Czar Peter seized on the book as an engaging way to make “the
discoveries of Western science clear and intelligible to the still minuscule reading public.”411
The czar is likely to have met Newton in person during his 1698 stay in England, probably
at the Mint. At that time Newton served as its warden, and Peter consulted him primarily
on monetary matters so as to prepare for the large-scale reforms of the Russian polity he
began executing once he was back home in his newly founded capital of Saint Petersburg.
The abiding interest that sent the czar on his exploratory voyage to the Netherlands and
England was in advanced techniques (such as notably for shipbuilding) as the best means
to drive his planned reforms. Yet his lively interest in the latest science was not just deriv-
ative. In Amsterdam he arranged for the purchase of a well-stocked *Kunstkammer* (cabinet
of curiosities). In England he had the man who was to translate *Kosmotheoros*, a courtier by
the name of Jakov Vilimovich Bruce (of Scottish descent), purchase the *Principia* and other
books with mostly mathematical content and also construct reflecting telescopes and other
scientific instruments.

Bruce’s collection of instruments and books, soon enriched with many more works on
science and also with translations specifically ordered done by Czar Peter, was in due time
to serve as one kernel of the Russian Academy of Sciences that Peter founded but whose first
session he did not live to see. Benefiting from Leibniz’s counsel among others, he and his
successors on the throne managed to attract to Saint Petersburg leading luminaries from
all over Europe, such as Euler and several Bernoullis. By the late 1720s the respective merits
of Newton, Descartes, and Leibniz were discussed in Saint Petersburg as elsewhere on the
European continent – within less than ten years, that is, of the attempted sabotaging of the
Russian translation of *Kosmotheoros* by the man whom Peter dragooned into printing it, a
task that the printer detested in view of Huygens’ clearly apparent “Satanic perfidy”.412

The three cases just discussed have enough in common to invite an instructive compari-
son. In each case, policy considerations largely determine the course of events from the out-
set – in the Chinese case, policies on the receiving, as well as the supplying, end. In each case,
an autocratic court initiates events and keeps running the show, sometimes at one remove,
sometimes directly, yet invariably present and always decisive in the end. In two cases out of
three, both what portions of recent European science are selected for introduction and how
these are actually received are matters very much determined by local circumstances and by
constraints set by the locally predominant conceptual framework. Adaptation of European
work to the legacy of the Islamic Golden Age comes with considerable substantive impov-
erishment; the contrast with organic materialism even proves so vast as in the end to lead
to almost wholesale ejection. In the Russian case, by contrast, there is a blank slate. There,
Newtonian science is parachuted into a scholarly wasteland, and in the absence of any rival conceptual framework the science imported is very much received on its original terms.

For all the differences between the local contexts in each specific case, the superior substantive quality of the science adopted is recognized in all three. In the Ottoman Empire and in Russia, European scientific superiority goes without saying, or is at least accepted without more ado by the respective autocrats involved (in the former case only after a crushing military defeat). In China the ruler deliberately sets up a competition and forthwith follows up its confidently foreseen outcome with the appropriate measure: reinstallment of an expert European as head of the emperor’s Astronomical Bureau. To be sure, in each case local acceptance of European superiority concerns only the portions of science actually adopted – not science as such, let alone the culture as such (here, too, Czar Peter’s Russia is a bit of an exception). Even so, the acceptance is all the more telling because all three episodes took place a long time before the as yet latent capacity of the science that came out of the Scientific Revolution to alter the arts and crafts from the ground up and thus help instigate the rise of the modern world became at all apparent. Neither to the Ottoman Empire nor to China or Russia did these early products of Europe’s Scientific Revolution come on the coattails of modern imperialism or on anything at all but their own substantive merits in the eyes of the mutually so different beholders.

Indeed, the Baconian vision, enthusiastically proclaimed in the 17th century, did not begin to work out in a manner visible to the public at large until the early decades of the 19th. As I have insisted at many places in the present book, the promised rise of a science-based technology, the obstacles to which were first encountered and then gropingly explored over the period of the Scientific Revolution, went through a creative incubation period during much of the 18th century. That is when ‘craftsmen of a new type’, like Newcomen, Harrison, Smeaton, and Watt, succeeded in overcoming all those obstacles arising from complex mathematics, from underestimated messiness, from basic miscommunication, etc. (p. 325). To be sure, the achievement of these men who managed to make novel science incarnate in novel machines forms only a necessary, not a sufficient condition for the onset of the Industrial Revolution. There was definitely more to the onset of industrialization than modern science alone. This is so in two major respects, quite distinct from each other.

In the first place, not all techniques that jointly made for the world’s first industrialization process owed much to science. The steam engine that came from Watt’s drastic transformation of Newcomen’s fire engine surely did. It was imbued to the very core with revolutionary scientific insights into, notably, atmospheric pressure (Galileo, Torricelli), the void (Torricelli, Pascal), latent heat (Black, Watt), and the expansion of steam (Watt). It was quite otherwise with the range of inventions that jointly made possible the mass fabrication of cotton (and soon enough other fabrics as well): the spinning machines invented by Hargreaves, Arkwright, and Crompton, plus Cartwright’s power loom. None of these machines
owed anything to modern science but the Baconian Ideology itself, even though by the turn of the 19th century the mass bleaching and dyeing that quickly came to present a production bottleneck was to benefit soon enough from contemporary work in advanced chemistry. In short, science did come in after the fact without fail (as also with the onset of thermodynamics in Sadi Carnot’s reflections on the motive power of steam), but only selectively as part and parcel of the original invention.

However, and this is my second major point, all this technical ingenuity would still have been wasted if technical invention had not been turned into veritable economic innovation due to a range of bold decisions actually to invest in these inventions and market the products to come out of those investments. What entered into the original investment decisions made by men like Boulton for the steam engine or Arkwright for the water frame that goes by his name was not just their capital or their visionary entrepreneurial foresight (although these particular components of the full equation are not to be neglected) but really an entire economic climate. In Britain the relevant economic parameters displayed certain special features, such as (to name but a few) a none-too-constricting patent law; an above-average public awareness of what was going on in science and in the crafts; reasonably dependable safeguards against arbitrary expropriation; a regularized money-lending system; and a unified national market (and the potential for an international market in the United States and in India) not encumbered by prohibitive interior or exterior tariffs. It is, then, in the seemingly miraculous confluence of, on the one hand, British craftsmen of a new type managing to overcome all the obstacles to a radical, science-based overhaul of the crafts and, on the other, British entrepreneurs seizing on the chance thus arising to invest, with hopes for vast long-term profit, in large-scale machines of a kind quite unheard of that the true riddle resides of the onset of our modern world in early-19th-century Britain.

In whatever manner that vast historical enigma is in the end to be resolved, early industrialization did give England, and soon enough all of western Europe and the United States, a decisive edge and then a hold on the world at large that in many ways endure to the present day. The making and the running of colonies in Asia and Africa came and went, yet in many respects Western predominance is still a fact of global life, and science has kept contributing to that predominance in large measure and without cease. This is not, however, a state of affairs that is bound to remain with us in any case. One major point of the present book has been that the events out of which modern science arose – a feat absolutely indispensable for the modern world to come into being at all – took place in Europe due to events and circumstances that, while well-explicable up to a point, nonetheless might have taken a somewhat different course in which the Scientific Revolution might not have happened at all or have occurred somewhere else or at another time (pp. 72; 204; 271). Just as there was no inherent necessity for modern science to emerge in the West, just so there is no inherent reason why its prime manifestations should forever remain an attribute of the West. There
is nothing *a priori* unlikely about a vision, recently promulgated by Kishore Mahbubani (a diplomat from Singapore), of Asian civilizations regaining their longtime predominance after an altogether fairly brief, two- to three-centuries-long interruption by the West.

For such a grand reversal actually to come about, it would nonetheless seem that – at least where science is concerned – certain conditions of an as yet unsettled status ought to be met. For how does modern science fare in civilizations other than the one that has given rise to it? The specifics of local context are highly consequential, decisive even, as the radically different outcomes of the early Chinese and Russian efforts at importing portions of revolutionary European science show – nearly wholesale ejection in the one case, almost wholesale adoption on its own terms in the other. Over the larger part of the post-1700 expansion of science, however, the Ottoman effort to adopt portions of science in the curtailed guise of ready-made usefulness at the cost of conceptual depth seems more of a harbinger of the future. To have just the science while leaving everything else intact has been a well-understandable reflex on the part of rulers who desire the useful goods but resist the wholesale modernization and its numerous discontents that all too easily come with it. But is such a strictly utilitarian manner of handling the cultivation of science really feasible? Does not modern science come with certain value-laden attributes which, although not directly involved in how research is actually done on a day-to-day basis, nonetheless co-determine the ongoing flourishing of the enterprise as such?

**Science and values**

Take first those values that have over time become anchored in stable institutions. Although scientific societies and academies remained significant sites of original research until some time into the 19th century, and although the Royal Society is still alive and kicking, laboratories maintained by universities and by private companies with R&D departments of their own have replaced them as the principal institutions housing scientific research. In addition, just as military and commercial interests were co-constitutive of the very foundation of the two earliest societies (p. 580), so have the 20th-century industrialization of warfare and the rise of the pharmaceutical industry, in particular, caused commissioned research to become a growing part of the landscape of science.

Consequent upon all this, one indispensable ingredient of scientific research, its autonomy in the sense of the tenability and scientific standing of an investigator’s conclusions being subject only to the judgment of his or her peers, has come to face novel challenges. Indeed, nothing remains of the very specific constellation of large-scale European power realignment, rapidly emerging royal confidence, Baconian optimism, and religious sanctioning out of which an unprecedented amount of autonomy for the investigation of nature emerged
in the 1660s. Suddenly, within ten to twenty years of revolutionary nature-knowledge running a grave risk of losing momentum for good, a second generation of practitioners could be seen strolling in the Jardin des Plantes or seated in London’s coffeehouses to discuss the latest findings made under the auspices of the two royal societies newly founded in the two principal cities of Europe (p. 513; 619). The scope and the underpinnings of this newly attained and still rather circumscribed autonomy have since changed almost beyond recognition. Even so, a serviceable amount of freedom of research irrespective of where it may lead has remained, by and large undisputed (except for a range of totalitarian onslaughts), in quite different constellations.

In the late 1940s the manner in which Nazi and communist regimes appeared to handle certain disciplines (efforts at constructing an ‘Aryan’ physics and the slightly later imposition of Lysenko’s ideas on heredity are well-known examples) inspired the sociologist of science Robert Merton to articulate the ethos of the scientific enterprise, which he took to be defined by four values. When the pursuit of science is made dependent on anything other than individual merit (such as race, class, or gender); when scientific knowledge remains secret; when results are affected or even determined by the investigator’s personal stake (glory, money) in the outcome; or when limits are set to research in view of what any given society may hold sacred – in all such cases (which appear to be less rare than one might hope for and also at present on the increase) practitioners collectively feel in their bones that a binding norm is being trespassed. What is minimally required for the ongoing cultivation of science even on a day-to-day basis is the presence of safeguards against wholesale infringement of the self-critical procedures on which the scientific enterprise thrives, and which it cannot do without in the long run. Moreover, the general public’s trust in science, while bound up to a large extent with an as yet unceasing readiness to acquire the goods that scientific research is widely taken to hold in store for us, is ultimately dependent on a certain confidence in precisely these self-critical procedures. For as long as we sense that, in the end, nothing needs to be taken on trust and that anyone who decides to take the trouble and acquire the needed expertise may confirm or repudiate for himself the validity of a scientist’s assertions, our trust seems sufficiently well founded. If, on the contrary, science were widely seen as overall up for grabs by the highest bidder or as giving in to the loudest accusations of trespassing some sacred principle, it would spell its effective demise. The machinery might still rumble on for quite a while, but the lifeblood would meanwhile seep away.

From the 1940s to the 1970s it used to be maintained without much risk of contradiction that only a democratic environment is capable of providing the safeguards embodied in the scientific ethos. In his thought-provoking book, Mahbubani suggests that the home-grown cultivation of top-notch science may also thrive in the absence of Western-style democracy:
the ‘magical’ development that propelled Western science and technological research ahead in the past few centuries has now penetrated the Asian cultural fabric. Asians no longer believe that they are inferior to the West in science and technology research. They believe that they can do equally well on their own.\footnote{413}

As yet it is an open question whether or not ‘the Asian cultural fabric’, permeated as it is for the largest part by comparatively mild forms of authoritarian rule and behavior, would be sufficiently capable of ensuring the institutionalization of regular, objectivized feedback procedures, although the example of Japan (for all its democratic elections a deeply hierarchical, authority-run society) seems to confirm it.

A closely related, perhaps more worrisome question is whether science can enduringly flourish in an environment that has not or not yet interiorized the principal values of the Enlightenment. Surely there are intimate historical connections between the rise of modern science and the emergence of the 18th-century movement known by that name. In his 1784 essay ‘What Is Enlightenment?’, Kant famously defined it thus:

\begin{quote}
Enlightenment is man’s emergence from his self-imposed immaturity [Unmündigkeit]. Immaturity is the inability to use one’s understanding without guidance from another. This immaturity is self-imposed when its cause lies not in lack of understanding but in lack of resolve and courage to use it without guidance from another. Sapere aude [dare to know]! “Have the courage to use your own understanding!” – that is the motto of enlightenment.\footnote{414}
\end{quote}

A century and a half earlier, Galileo’s Dialogo and those deeply ambiguous ‘everything up for doubt’ passages in Descartes’ Discours de la méthode and Meditationes had already begun, if not literally to express, then at least exude this very admonition to think for oneself unencumbered by anybody else’s prescriptions however much imbued with authority (pp. 282; 385; 435). Also, one fundamental presupposition shared by just about all protagonists of the Scientific Revolution save only the dogmatic natural philosophers was that, in spite of what the vast majority of scholars knew for sure, the constitution of the natural world was not at all known yet but was open to fresh discovery by mostly novel, as yet hardly tried-out means. The 17th century, with its religious wars and its persecution of witches, was as little an enlightened age as any preceding century was; but to the coming about of such an age the rise of modern science contributed greatly. In his Lettres philosophiques of 1734, Voltaire, in creating the persona of the ‘philosophe’ and wittily taking sides in thirty years’ worth of debate in the Académie on the problems of Newtonian attraction, initiated the French Enlightenment movement all the while seizing on Newton’s published work. He adopted it in its current, mostly British/Dutch interpretation as primarily experimental rather than mathematical in its approach and its implied methodology, as nearly materialist and deist in its ideological
underpinnings, and as a close ally somehow in the upcoming fight against all that the ‘ancien régime’ stood for. Newton, already dead for seven years, would have been horrified. To be sure, other 16th- and 17th-century events, notably the emergence of a public sphere and the rise of biblical criticism out of new methods of philology, also contributed their fair share to the coming about of the Enlightenment – as with the later Industrial Revolution, the rise of modern science is only part of the story, albeit certainly an indispensable part.

This is equally true of one more portion of enlightened thought: the aimed-at eradication of superstition in all its many guises. Here, too, what protagonists of the Scientific Revolution contributed forms rather a mixed bag. Debate about the very possibility of miracles was affected to say the least by the idea and the confirmation of the constant action of inexorable laws of nature like, notably, Newton’s; yet denial of miracles could and did emerge as well out of Protestant polemics against what was felt to be a popish overdose of miraculous events and cures. Belief in the reality of witchcraft and of demonic possession was by no means confined to opponents of the new currents of nature-knowledge such as, for instance, Voet; yet the remarkable shift in the debate that took place by the second half of the century owed much to a consistent failure to render urgently searched-for, experimental proof of demonic action in any of its numerous varieties (p. 480). Similarly, as debates for and against the portentous meaning of comets continued through the seventeenth century and beyond, opponents appealed less and less to just common sense endorsed by ancient authorities and more and more to a century’s worth of astronomical investigation that had revealed the regular calculability of their appearance in the sky and, indeed, of their entire trajectory, due, once again, to the inexorable operation of dependable laws of nature.

Thus was the beneficial rule of enlightened ‘reason’ opposed to the tyrannical rule of irrational authority on many fronts at a time, and scientists were quick to avail themselves of so advantageous an opposition. To what extent were they, and are they, entitled to do so?

They are very much so entitled insofar as Enlightenment feats and values like those of unencumbered inquiry and of a securely institutionalized public sphere which leaves ample room for critical, open-ended debate form the very lifeblood of the scientific enterprise. Even in totalitarian states attempts have been made to create carefully encapsulated domains where, in maximum insulation from society at large, scientists were given privileges along such ‘enlightened’ lines. Alexandr Solzhenitsyn’s novel *V kruge pervom* (The First Circle) provides a haunting, firsthand picture of such a privileged site during Stalin’s reign, while also intimating that artificial hothouses like these are doomed in the long run to collapse under their own contradictions. Whether, in the far milder circumstances of run-of-the-mill authoritarian regimes in Asia, the surely smaller amount of insulation needed there would be sufficiently limited to allow truly groundbreaking scientific research to flourish and survive in the long term is an as yet open question, albeit one to which within a few decades we are likely to find out the answer.
So much for the ‘freedom and critical openness’ side of the Enlightenment in connection with the scientific enterprise. But what about its ‘rationality’ and its ‘liberation from superstition’ aspects?

Surely scientists deservedly lay claim to rationality in the sense of a proven capacity to train the human powers of reason-guided action and understanding in a manner optimal for coming to grips with the realities of our natural environment – precisely there lies the greatest triumph of the Scientific Revolution.

But does it follow that scientists have a monopoly on rationality? On hearing some of their self-appointed representatives, one would almost believe it. For instance, in his book *The God Delusion* Richard Dawkins makes no secret of his conviction that any riddle in the world, if it can be solved at all, can be solved by the strictly rational, strictly empirical means which science alone is in a position to bring to bear on it. A delightful sentence in Dawkins’ book provides a neat measure for the degree of rationality and empiricism that mark his own argument, viz. “Newton did indeed claim to be religious”. That is, Newton said so, but he cannot have meant it, for how possibly could that prime icon of science have been a true Christian believer? And so Dawkins feels at liberty to ignore the tons of evidence in Newton’s own manuscripts and actions so carefully assembled and analyzed by historians of science over the past four decades. Did Newton risk his entire career for his religious convictions just on a whim? Was it just to mask his utter unbelief that he appended to the second edition of his *Principia* a ‘Scholium generale’ in which he set forth the publishable portion of the highly personal conception of the Deity that he had arrived at after decades of assiduous wrestling with theological issues (p. 710)? In short, if the facts of history do not fall in line with what Dawkins already knows for sure on sheer *a priori* grounds, then so much the worse for the facts …

Or take a cosmologist by the name of Vincent Icke, who recently, in his ongoing fight against superstition, wrote an op-ed piece attacking astrology. In effect the piece came down to propaganda for astrology – if there are really no better arguments against astrology than vulgar name-calling and misinformed accusations dished up without adducing one shred of evidence, then perhaps there may be something to it after all? For instance, Icke jeered at unnamed astrologers who thought they could ‘prove’ that there were only seven planets. As everyone with an inkling of the history of astrology knows, there has been no more than one astrologer intent on proving that there could only be a determinate number (six, not of course seven) of planets, and his name was Johannes Kepler (p. 164).

What all this amounts to is not of course that scientists are incapable of rational thought and action or of taking empirical realities into account. Quite to the contrary – even if not in person, then collectively they may rightly lay claim to one highly specific and as such most successful and admirable form of rationality. It means only that scientists, as soon as they feel at liberty to expound *ex cathedra* beyond the proper boundaries of science, are prone
to all the distortions that Bacon, in his analysis of the four idols, eloquently warned against (p. 486). Outside their own field of competence, they may all too easily fall back into the very habits of our naturally biased thinking that so often mar progress in other domains than the natural sciences – taking one’s wishes for reality and ignoring or even forgoing readily available evidence.

Behind such arrogance on the part of numerous self-appointed spokesmen of science often resides a sentiment far more interesting than mere arrogance, and a culturally very significant one to boot – *a sense of unity lost*. This, then, is my final topic.

Among all the many changes due to the Scientific Revolution was the loss of a by-and-large unified conception of the world. Human concerns, spiritual agents, and natural givens all formed coherent parts of one overarching whole, be that whole thought of as a seamless web held together by correlations, as in China, or rather as a multilevel construction held together by logic of the ‘if this, then that’ type, as predominantly in the Greek tradition. Albeit not without its scary aspects, this was a cosmos one could feel at home in. But the rise of modern science changed all that.

On the smaller scale of the ‘nature’ portion of that overarching unity, some practitioners adapted without any apparent difficulty to the apparent lack of a unified structure for all of nature. For all Galileo’s ‘opportunism’ in matters philosophical (p. 211), his work still displays a lively interest in how the whole of nature hangs together. One generation later, Huygens is the prime exemplar of a virtuoso practitioner who scarcely cared for anything beyond just the piecemeal and the readily graspable. His near-contemporary Isaac Newton was of a partly different temperament, or rather of two temperaments at once. In the preceding chapter I argued at some length that the very tension between his urge to go for the whole and his self-enforced confinement to the mathematically and experimentally securely proven was in good part responsible for making him an even greater discoverer than his so often unwitting rival Huygens (p. 675; 711). A little more than two centuries later, Lorentz and Einstein were to replicate much the same opposition, which indeed seems to run (albeit not as a rule in quite so plain a fashion) through the entire history of science.

As specialization keeps remorselessly increasing, the longing for the whole has more and more difficulty finding a responsible outlet. It comes to the fore in admirable ways in the Santa Fe Institute, dedicated to integrating a variety of scientific disciplines by throwing “a crude look at the whole” (in Murray Gell-Mann’s wonderful catchphrase). But it also comes to the fore in adorning the aimed-for unification of the four basic forces of nature, which would of course be a formidable achievement in its own right, with the silly label ‘Theory of Everything’. It is as if using that theory we could henceforth not only explain relativity theory but also determine the causes of cancer and warfare and even what exactly made Mozart a composer superior even to Haydn – ‘everything’ after all meaning nothing less than, indeed, everything.
Nor have all scientists withstood the urge for reunification on even grander scale than just all of nature. Particularly prominent in our time are efforts to reduce without more ado all human phenomena, from ethical rules and conduct to literary criticism, to Darwinian evolution in one or another interpretation thereof. But proponents of a superficially quite other-directed mode of unification, which usually goes by the name of ‘New Age’, really drink from the same source. Scientists like Capra, Bohm, Prigogine, and Lovelock have aimed to show how this or that piece of scientific endeavor, from quantum physics to dissipative structures in molecular chemistry, constitutes nothing less than proof for some all-encompassing conception of the world and humankind – each to be sure a different conception.

In short, many scientists are united in the belief that there exists something like a scientific worldview, which most place in square opposition to religious belief, whereas others find the two readily compatible. But is there really a scientific view of the world? What can be said, at most, is that the advance of science over, by now, some four centuries has, if not straightforwardly ruled out, then at least made quite unlikely the tenets of any religious creed in its literal sense. As noted before (p. 430), I go along with Steven Weinberg’s idea of an ongoing retreat of the conception of a caring Deity in the face of what science has successively revealed to be the case. Whether that retreat is full scale in the sense of in the end leaving no legitimate room for any religious belief at all is another, ultimately personal matter, which goes far beyond the scope of the present book. Within that scope, however, fits the observation that to many upholders of a ‘scientific worldview’ – Dawkins is a prime exemplar – science has taken all the trappings of a worldly religion, just as has happened to many pronounced atheists of quite other stripes. Here, too, nostalgia for a lost unity seems to be at work.

What it all comes down to in the end is that certain limits are set to scientific knowledge. This is hardly a novel revelation – Kant made a formidable first step in laying down those limits. Since then, it has been sought time and again to overstep them, most often under the self-invoked aegis of science. One obvious example is Comtean positivism, with its ultimate aim of providing “one body of homogenous doctrine.” Another, much less known yet even more instructive example is Schopenhauer’s Kantian or, rather, brilliantly pseudo-Kantian effort to bring the latest science in harmony with an all-encompassing conception of the world at large.

The appeal that efforts to overstep Kant-like limits have nonetheless kept exerting on so many people, be they themselves scientists or not, testifies to the presence and the acuteness of the sense of loss that the Scientific Revolution has brought about. From a human point of view, the world that science reveals to us looks bleak – its richness rests solely in the incredible vastness and precision of understanding that modern science has enabled us to attain. The world that we ourselves experience on a day-to-day basis is much richer still, and science seems to do nothing but impoverish it.
All this is reflected in the modern ways of life that, due in good part to the coming into
the world of modern science, many of us have become accustomed to. No one who has
tasted the ways of modern life wishes to give them up, for very good reasons (higher life
expectancy, better health, greater wealth, a greatly enlarged choice of consumer goods). Even
so, modern life comes with a vengeance – it tends to lock us up in an alienation-inducing
technopolis that endangers our very survival in more ways than one. The challenges are new
– the Scientific Revolution and a range of events that more or less inadvertently followed in
its wake have called them up. Yet the predicament is as old as humanity itself. Ever since the
rise of consciousness we have been the objects and subjects of nature’s greatest experiment
to date. What exactly that experiment is meant to prove, and whether it is meant to prove
anything at all, or even whether it has been meant by Someone or something in the first
place is hard or even impossible to tell – quite possibly forever.

Here is a final image of science, of its triumphs as well as its limitations. I take the image
from music – that perennial source of illustration throughout my account of how modern
science has come into the world. For the longest part of history, music was just music. Or
rather, music was far more than music – it was communal sharing and bonding; it was
sacred ritual; it was medical cure; it was accompaniment to dance, warfare, worship. Music
was all these things, and of course pure relaxing or energizing beauty to boot, yet it was not
science. By now it is. Or rather, in addition to all these things that it still is, it has become
science as well. We know (yes, dear reader, we know) how the consonant and dissonant inter-
vals that music is made of are mathematically and physically linked up with the vibrations
of the air that make up their constituent sounds. We know the anatomy and physiology of
our sense of hearing, from the eardrum, across the hammer, anvil, and stirrup to the oval
window, the cochlea, and the auditory nerve. We are learning more and more about those
specific places inside the brain that, on investigating musicians’ heads, light up in the test
subject’s MRI scan. We further know how musical notes if sounded together produce beats
and combination tones, such that the man to make the single biggest stride in this domain of
inquiry, Hermann Helmholtz, could show that the spacing of chords in Palestrina’s masses
was precisely the optimal one from the viewpoint of what his own theory predicted.

But none of all this gets us (nor to be sure does it claim to get us) any closer to what
transcends even Palestrina’s expert craftsmanship – the capacity of his music to bring us to
tears fully four centuries after he first composed it.

In Palestrina’s day, music was still a vital part of the cosmos. It even served as the prime
expression of the harmony that was taken to bind humans and the cosmos together. That
unifying harmony has gone forever.

In Palestrina’s day, musical composition was given to exploiting the finest varieties be-
tween intervals tuned as purely as the elementary arithmetic of tuning and temperament
allows. Those subtle differences have gone forever. We at present experience every day how scientific knowledge of musical and other sound has homogenized our experience of music: that most deadening of all possible tuning systems, equal temperament, has now spread throughout the globe. Meanwhile technologies originating in scientific knowledge of the radio wave have blessed us with the opportunity to listen to the loveliest music we know of, whenever and wherever we wish for it. They have also given us the ubiquity of those really submusical sounds that go by the name of muzak, and by means of boom boxes they have made true Narcissus Marsh’s confident 1684 prediction of the advent of “microphones . . . contriv’d after that manner, that they shall render the most minute Sound in nature distinctly audible, by Magnifying it to an unconceivable loudness”. The Scientific Revolution, in short, has truly come with a dual legacy. It is one we had better face upright, with a mixture of delight, admiration, and revolt, and, above all, with all the courage we can muster.