

Dear reader, now that you have completed reading chapter 7 of *The Scientific Revolution. A Historiographical Inquiry*, you naturally expect to move on to the final chapter. 'The Structure of the Scientific Revolution' is how I entitled it, by way of an allusion to Thomas Kuhn's world-famous book. However, you are not going to find that chapter here. In course of my exchanges with the translator, Dr. Zhang Butian, I proposed to him to replace it with a Postscript. Both he and the American publisher (the University of Chicago Press) kindly went along at once.

What moved me to make that proposal? I opened the final chapter thus:

In the sketch that follows the origin of the ideas that underlie it is taken for granted, since these ideas have been treated at length in Parts I and II. ... The sketch is no more than an afterthought to the whole of the argument that has preceded, and the most suitable test for it would be to have it written out in book form to see whether it can encapsulate the succession of historical facts that together make up the birth of early modern science. But that would fill another book, which, indeed, I hope to write next. For now, what follows is no more than just that — a highly provisional sketch.

When the book came out in 1994, I had already embarked on this 'other book which I hoped to write next'.

This other book finally appeared with Amsterdam University Press in 2010 under the title *How Modern Science Came Into the World. Four Civilizations, One 17th Century Breakthrough*. I opened it thus:

Once upon a time 'the Scientific Revolution of the 17th century' was an innovative and inspiring concept. It yielded what is still the master narrative of the rise of modern science. The narrative has meanwhile turned into a straitjacket — so often events and contexts just fail to fit in. In the classroom we make the best of the situation; in our researches most of us prefer just to drop the concept altogether, regarding it as beset by truly unmanageable complexity. And yet, neither the early, theory-centered historiography nor present-day contextual and practice-oriented approaches compel us to drop the concept altogether. Instead, in the present book I provide a narrative restructured from the ground up, by means of a comprehensive approach, sustained comparisons, and a tenacious search for underlying patterns.

Key to my analysis is a vision of the Scientific Revolution as made up of six distinct yet tightly interconnected revolutionary transformations, each of some twenty-five to thirty years' duration. This vision equally enables me to explain how modern science could come about in Europe rather than in Greece, China, or the Islamic world. [...]

In a book published in 1994, *The Scientific Revolution: A Historiographical Inquiry*, I subjected to critical scrutiny some sixty views on the event selected from the vast literature for their boldly creative, interpretive sweep. I now present my own view. It has taken shape in critical dialogue with those sixty and several more-recent interpretations and also with many more narrowly focused studies. In good measure, it also rests upon firsthand familiarity with the subject. In the final chapter of my historiographical book I presented a preliminary sketch of my own budding view. That sketch has served me well as a stepping-stone, but my thinking has taken many a new turn in the meantime. I hereby discard that final chapter, with thanks for the encouragement it once gave me.

In the Postscript that now follows, I do two things. I bring you up to date (briefly and only in very rough outline) on such professionally responsible studies of the Scientific Revolution as have appeared since 1992 — the year I completed my manuscript. That done, I set forth how certain ideas discussed at length in the historiographical book that you have been reading inspired those 'new turns' I took in my ongoing thinking on the subject, leading in the end to *How Modern Science Came Into the World* and to a shorter version thereof, *De herschepping van de wereld*. I wrote this shorter version (not in English, to be sure, but in my mother tongue, Dutch) for the benefit of a wider, non-professional audience. It came out in my own country in 2007; a German translation appeared in 2010, and a Chinese translation, also by Dr. Zhang Butian, in 2012.

Books on the Scientific Revolution published 1992–2012

To the best of my knowledge, fourteen books have come out since 1992 whose main concern is not just one or another aspect of the Scientific Revolution (very many such books have in the meantime appeared) but

the Scientific Revolution *as such*, that is, as a Europe-wide event that involved an at least in good part novel understanding of the natural world and that, whatever its precise periodization, included the 17th century in any case. One of these fourteen books is a large encyclopedia, edited by Applebaum. One is an anthology, edited by Hellyer. Ten more are brief to very brief textbooks meant for the classroom in the first place. Finally, two lengthy studies have also come out. The earlier one serves at the same time as the first volume of a range of later studies (all written, or yet to be written, by Stephen Gaukroger). The other is my own *How Modern Science Came Into the World*. I shall now first discuss Applebaum's encyclopedia and Hellyer's anthology; then the ten textbooks; then Gaukroger's book, and (in the next section) my own, with special consideration for how much in the conceptual equipment I have employed there flowed from my earlier work on the historiography of the Scientific Revolution.

Wilbur Applebaum (ed.), *Encyclopedia of the Scientific Revolution from Copernicus to Newton*. New York / London: Garland, 2000; xxxv + 758 p.

In his Introduction (p. xi-xiv), Applebaum lists several grounds for the many objections raised in (by then) recent years to the very idea of 'The Scientific Revolution', and goes on to defend his continued use of the term. He has distilled from the recent literature seven significant objections. Usage of the term is held to imply (1) absence of cultural context; (2) a triumphalist account in terms of humanity's liberation from "ignorance, superstition, and error" due to final attainment of our present-day scientific truths; (3) the often piecemeal pace of events; (4) the long periods over which "opposed theories were simultaneously held"; (5) insufficient credit given to the preceding recovery of Greek texts and to "the questioning of traditional principles in the late Middle Ages", involving many continuities between late medieval times and 17th century developments; (6) absence of revolutionary developments in all other disciplines than physics and astronomy, notably in chemistry and the life sciences; (7) even more radical turnabouts in the scientific enterprise taking place in the 19th and 20th centuries, leading to the conclusion "that there were two or more scientific revolutions".

As against this, Applebaum argues as follows:

Whether or not events during this period represent the beginnings of what we call modern science, they certainly constituted a decisive break with the past. Prime axioms of ancient and medieval sciences, their modes of investigation and scientific explanation, however modified over the centuries, were overturned. The sometimes slow processes by which old scientific theories were challenged and new ones emerged and debated do not invalidate the concept of the Scientific Revolution, which was not an event, but a complex of events. [my italics. HFC]

After this curt rejoinder Applebaum dedicates the remainder of his introduction to outlining those vast changes that he finds most significant. As the title of the encyclopedia indicates, he takes the Scientific Revolution to run from 1543 to ca. 1700, with the proviso however that "the decisive events determining the nature of the Scientific Revolution were focused in the first half of the seventeenth century This uniquely creative segment of the Scientific Revolution was followed by one in which its achievements were absorbed and developed further."

Of course, many of the entries that make up the body of the encyclopedia concern individuals. But numerous topics have been given an entry as well, and for their selection Applebaum used a fivefold categorization: (I) Philosophical schools, worldviews, and associated concepts; (II) Disciplines (scope,

branches, methods, discoveries), (III) Institutions, organizations, and communication; (IV) Social and cultural contexts; (V) Historiographical issues and interpretations. Category (II) alone takes up by far the largest number of entries – clearly, besides individuals it is scientific disciplines that take center stage in the encyclopedia. Luckily, many entries were written by the most authoritative historian Appelbaum could find, as for instance Richard S. Westfall, who contributed all entries concerned with Isaac Newton. I myself contributed the entry ‘Scientific Revolution’ (p. 589-593) which (written in 1996) provides a slightly updated abstract of my historiographical book.

Marcus Hellyer, *The Scientific Revolution. The Essential Readings*. Oxford: Blackwell, 2003; viii + 264 p.

In his ‘Editor’s Introduction: What Was the Scientific Revolution?’ (p. 1-15), Hellyer seeks to demonstrate why the Scientific Revolution has become so contested a concept. After all, whereas some scientific disciplines were fundamentally altered in 16th/17th century Europe, many others (chemistry, the life sciences, medicine) underwent far less consequential changes. To meet with the difficulty historians have sought for alternative features that, unlike with disciplinary upheaval, *do* cover just about all major change at the time. These shared features, so Hellyer goes on, have been sought in five processes internal to science: (1) recovery of ancient knowledge leading to innovation; (2) shared metaphysical underpinnings; (3) a new method; (4) guidance by authority replaced by observation and experiment; (5) mathematics merged with natural philosophy and (6) (the one shared feature of an ‘externalist’ nature) a background in practical concerns that derived from capitalist commerce. For each of these six features Hellyer lists why they cannot really be counted as truly shared across disciplines, in the following manner: (1) with Aristotelian texts, biting criticism was already in the 16th century far more wide-spread than innovation; (2) natural history remained largely outside the province of the mechanical philosophy; (3) methods varied greatly across disciplines; (4) rejection of authority was in good part imported from antiquarian circles, hence, from outside the domain of science; (5) many scientific domains remained outside the precinct of mathematics; (6) the theoretical/conceptual aspect of changes wrought by protagonists like Galileo or Descartes was too important to warrant a wholesale ‘theory vs. practice’ dichotomy.

At this point Hellyer introduces such novel questions as (so he claims) have arisen from the 1970s onward. Together with a novel, more critical attitude taken toward present-day science, these questions have led to a new interest in the social-cultural context of science over time. Attention has since been given to how practitioners were situated in society and to how they conceived of their own role therein; to institutions; to practices, notably experimental ones; to an awareness that early modern disciplinary hierarchies differed from ours, with a quite different assessment of, for instance, magic and alchemy. Also, Hellyer argues, the alleged opposition between science and religion has given way to a more nuanced understanding.

As a result of these studies [so Hellyer infers from the preceding], the Scientific Revolution as concept and narrative is in turmoil. In fact, all three elements of the phrase are questionable. ‘The’ implies that there was just one revolution in the early modern period, which was not the case. Moreover, it also implies a hierarchy of significance, yet if the events of the sixteenth and seventeenth centuries were ‘the’ Scientific Revolution, then the chemical revolution of the eighteenth century was merely ‘a’ revolution and necessarily of secondary importance, something that historians of other eras would dispute.

Furthermore, with greater sensitivity to actors’ categories [...], historians now accept that the word ‘scientific’ is also misleading and anachronistic. The association between science and the study of nature was not one made in the early modern period ...

... 'Revolution' seems somehow inappropriate for a process that lasted at least a century and a half or, by many accounts, even longer. Furthermore, the lens of revolution is distorting: whatever doesn't measure up as a radical advance from our standpoint is tossed out of the historical story. ... Thus the term 'Scientific Revolution' has fallen out of favor without being replaced. 'Early modern science', which brings less intellectual baggage with it, has had some success.

Hellyer concludes that, whether or not we feel a need to retain the concept, the selected articles that fill the remainder of his anthology show at least that "the study of nature was fundamentally transformed between 1500 and 1700, not just in its theory but in its methods, institutions, and everyday practices."

So much for these two expositions, published in 2000 and in 2003, respectively, of what partly novel currents in history-of-science writing have induced wide-spread doubt about the viability of the very concept of the Scientific Revolution. I shall now provide brief characteristics of the ten textbooks. I present them in their order of appearance. Points to be addressed (where applicable) are the following:

- Does the author accept or reject the concept or at least the term 'Scientific Revolution'? If the former:
 - what does he or she take it to stand for?
 - how does he or she date the event?
 - does he or she distinguish between the Scientific Revolution and a preparatory period, and, if so, which one?
- What questions and/or what unit or units of analysis govern the account?
- Does the author seek to assign causes to the event or portions thereof?
- How do the author's scholarly specialty or specialties affect the book?

Steven Shapin, *The Scientific Revolution*. Chicago: University of Chicago Press, 1996; xiv + 218 p.

The book opens with a paradoxical statement that has often been quoted: "There was no such thing as the Scientific Revolution, and this is a book about it." In his Introduction, Shapin lists six reasons for making historians, himself definitely included, uneasy with the term, and in the end he appears to settle for the following program:

We can say that the seventeenth century witnessed some self-conscious and large-scale attempts to change belief, and ways of securing belief, about the natural world. And a book about the Scientific Revolution can legitimately tell a story about those attempts, whether or not they succeeded, whether or not they were contested in the local culture, whether or not they were wholly coherent.

What remains of the Scientific Revolution in Shapin's book, then, comes down to "a diverse array of cultural practices aimed at understanding, explaining, and controlling the natural world, each with different characteristics and each experiencing different modes of change". Consequently, nothing remains here of the idea that the Scientific Revolution signified a unique watershed between pre-modern and modern ways of understanding the natural world — any connection with the rise of modern science has been rigorously severed. On the final page of his book Shapin calls science "certainly the most reliable body of natural knowledge we have got", but how it has become so reliable in the first place is a question neither raised nor answered in its 163 previous pages. It is consistent with all this that Shapin has nothing to say about causes of a Scientific Revolution in so toned-down a variety. No 'why' questions come up; the book moves from one local context to the next.

Although no periodization is being argued for, the book remains in effect confined to the 17th century. Chapter 1 opens with Galileo's telescopic discovery of sunspots, and is entitled 'What Was Known?'; the next, 'How Was It Known?', and the final chapter, 'What Was The Knowledge For?' The first chapter provides a succinct and fairly non-controversial rendition of narratives often treated in earlier works, too (from the geocentric to the heliocentric universe; from Aristotle's purpose-guided world to a world filled with moving particles; etc.), but the two later chapters largely built forth upon episodes and views familiar to the reader of *Leviathan and the Airpump*. By what means practitioners sought to convince others of their views and their conclusions (Shapin prefers to speak of 'beliefs') is the core question throughout.

John Henry, *The Scientific Revolution and the Origins of Modern Science*. Hampshire/London: Macmillan; New York: St. Martins, 1997; x + 137 p. (a third, revised edition came out in 2008 with Palgrave MacMillan).

The series in which this book appeared, is "designed to present the 'state of the debate' on important themes and episodes in European History since the sixteenth century." Consequently, Henry takes up the points he wishes to raise in ongoing dialogue with the literature, and he regards the Scientific Revolution as taking place between Copernicus and Newton. He acknowledges current doubts about the concept, but prefers to stick to it, even ending his book with a curt "The Scientific Revolution was complete." The book's subtitle is quite clear about Henry's taking the Scientific Revolution in the sense it was originally intended for, as the origins of modern science indeed – precisely this is what, in his view, makes the event so uniquely significant and worthy of our attention.

Among the many changes that the Renaissance period carried in its wake, Henry regards "the reformist ideas of the humanists" as the foremost trigger of the Scientific Revolution. The event itself is marked above all, in Henry's treatment, by processes of fusion between mathematics, natural philosophy, and experimentation. Throughout his career as a historian of science Henry has paid much attention to magic and to religion as important aspects of 17th century science, so here he deals at some length with both. He does so by way of balancing various explanatory theses available in the literature (e.g., Merton's and Yates') against such data and views as have been revealed by later research. Significant differences between England and the European Continent also come to the fore at many places.

Rienk Vermij, *De wetenschappelijke revolutie*. Amsterdam: Nieuwezijds, 1999; 128 p. The Dutch title translates as 'The Scientific Revolution'; reissued since as Part I of a larger book, *Kleine geschiedenis van de wetenschap* ('A Short History of Science'; 2006; same publisher).

Vermij distinguishes between three periods. In his first chapter, conceptions of the world arising in Antiquity and settled in the European Middle Ages set the stage. In the next, the 16th century Renaissance is marked by a wide-spread unease with the ancient and medieval legacy, which leads to a groping search for possible alternatives in a large variety of directions. In the seventeenth century, finally, these Renaissance feelers consolidate in the Scientific Revolution itself. At bottom, Vermij makes this concept stand for the replacement of the Aristotelian by the Cartesian conception of the world, subsequently refined but not substantially altered during the second half of the century by Huygens and by Newton (ch. 3). So to Vermij Descartes is the central figure in the Scientific Revolution – an expression he sticks to without more ado,

and which he also appears to regard as covering the origins of modern science. Besides the preparatory work of the Renaissance and how it triggered the Scientific Revolution, Vermij neither raises nor answers any ‘why’ questions.

James R. Jacob, *The Scientific Revolution. Aspirations and Achievements, 1500–1700*. Amherst: Prometheus, 1999; xviii + 148 p.

As with Vermij, Jacob, too, dedicates a chapter to the classical legacy and the next to the ‘cosmological renewal and corrosive doubt’ of Renaissance developments. Here, too, such phenomena as Renaissance magic, the invention of print, the rise of modern skepticism, and the Voyages of Discovery serve as a prelude to the Scientific Revolution proper, which here, too, covers the 17th century. Jacob’s treatment of the Revolution is largely focused on individuals, with special attention given to scientific and religious developments and their interconnection in France and in England, respectively. Jacob’s earlier work on Boyle is reflected in how he discusses the Royal Society, its practical concerns, and the impact that a variety of Protestant denominations had upon its experimental and other researches. Throughout his book Jacob neither affirms nor denies nor questions the status of the Scientific Revolution as the event that gave rise to modern science roughly as we know it.

Michel Blay, *La naissance de la science classique au XVII^e siècle*. Paris: Nathan, 1999; 128 p.

No book among those here discussed has been affected less by the various doubts about the concept of the Scientific Revolution recorded in the above. The French title translates as ‘The Birth of Classical Science in the 17th Century’, and indeed, to Blay this is precisely what the Scientific Revolution was about. All other authors are bothered to some extent by the inability of the concept to cover the non-mathematical sciences and by other often-signalized shortcomings, yet Blay’s vision closely resembles Koyré’s in that for him, too, the mathematization of nature was key to the entire event. Indeed, “the order of the world is no longer the same; the very frames of the one in which we at present live have by now been established” (“L’ordre du monde n’est plus le même; les cadres de celui dans lequel nous vivons aujourd’hui sont, pour l’essentiel, maintenant mis en place”), so Blay concludes at the very end of his booklet.

Inside the Galileo-inspired realm of a newly realist mathematics, Blay’s central concern is the history of thinking about the infinite. He outlines in some detail how the mathematical treatment of the infinite advanced from Galileo to Newton and from there to the first mathematicians (notably Pierre Varignon) who employed Newton’s and Leibniz’s calculus to resolve mathematical problems involving the infinite and the infinitesimal – many details of which Blay has dealt with in other books of his, and which he outlines here as the central event in the emergence of modern science.

Paolo Rossi, *The Birth of Modern Science*. Oxford: Blackwell, 2000; ix + 276 p.

Rossi († 2012) opens his book (written originally in Italian) with the story of Johannes Kepler’s strenuous, years-long efforts to save his old mother, accused of witchcraft, from the stake. Rossi’s scholarly career has been devoted in good part to showing how modern science developed out of Renaissance magic, so this

opening passage serves as a reminder of one of Rossi's main concerns — how in the 17th century superstition and other definitely non-modern viewpoints could and did exist side by side with the onset of modern science. Not that Rossi questions whether 'the Scientific Revolution' is still a viable expression, or whether it stands for the making of modern science at least in rough outline. In his account the Revolution starts with Copernicus, after a Renaissance prelude with magical currents paramount. In a more outspoken manner than with most other authors, Rossi lets individuals occupy center stage, with Galileo, Descartes, and Newton paramount, all the while his treatment of their and others' work follows the usual categorization by scientific disciplines. All this is supplemented with treatment of some selected special themes hardly or not at all encountered in other textbooks. One example is early ideas about how some pioneers came to regard fossils and certain other terrestrial phenomena as suggestive of the idea that not only human beings but nature, too, may have a history of its own. As with most authors, in all this Rossi neither raises nor answers any 'why' questions.

Peter Dear, *Revolutionizing the Sciences. European Knowledge and Its Ambitions, 1500–1700*. Basingstoke: Palgrave, 2001; viii + 208 p.

Dear acknowledges the term 'the Scientific Revolution' as a viable label for the "wholesale and profound restructuring of ideas about nature, of the proper purposes of knowledge, and of ways of acquiring that knowledge" that took place in the 17th century. However, since in his view "the historian has no stake in adjudicating the truth of past convictions", Dear abstains throughout from treating the event as modern-science-in-the-making. As also with Shapin, the only connection he makes with the present day is at the book's very end: "The modern world is much like the world envisaged by Francis Bacon." Indeed, this line encapsulates well the guiding thread of the entire book, which is that in course of the 17th century a 'contemplative' understanding of nature gave way to an 'operational' one — the exact opposite, that is, of how more than sixty years earlier Koyré conceived of the Scientific Revolution. In Dear's view, the upshot of the Scientific Revolution is that "knowledge of nature increasingly implied knowledge of how natural things worked and how they could be used".

Dear clearly and extensively distinguishes between a 'Scientific Renaissance' of the 16th, and the Scientific Revolution properly speaking, which he assigns to the 17th century. Although he finds it legitimate for historians to inquire into the conditions under which events of the past could come about, no 'why' questions of that or any other nature find much of a place in his book. One issue prominent not only in this book but also in other work by Dear is the kind of experience to which 'experiment' was increasingly taken to refer — not what we are all supposed to know to happen, but the careful narration (practiced at length by Boyle, in particular) of what the experimenter has at a specific time and place watched happening in a deliberately contrived, artificial setup. Solutions to the resulting problem of how to gain outsiders' trust in such a narrative indeed being truthful receive ample treatment in this context, along lines drawn earlier by Shapin & Schaffer.

Wilbur Applebaum, *The Scientific Revolution and the Foundations of Modern Science*. Westport (Connecticut): Greenwood, 2005; xxi + 245 p.

In keeping with the view on the Scientific Revolution that he expressed five years earlier in the introduction to the encyclopedia edited by him (see above), here, too, Applebaum sticks both to the expression itself and to the idea that this was the event in course of which the foundations were laid for modern science. The organization of his textbook is almost entirely by scientific disciplines, so that pioneers like Descartes and Newton keep reappearing under a range of distinct headings. Once he has completed this disciplinary survey, Applebaum surveys methods applied, the ‘mechanical philosophy’, magical currents, and the changing relation between religion and science during the period. He covers the 16th and the 17th centuries as one whole, jumping back and forth in the chronology of the period to an even larger extent than is true of the other textbooks here discussed.

The question of what may have caused the Scientific Revolution does not come up; instead, Applebaum ends with an outline of how the outcome of the Scientific Revolution influenced subsequent events, from the Enlightenment to present-day science.

Margaret J. Osler, *Reconfiguring the World. Nature, God, and Human Understanding from the Middle Ages to Early Modern Europe*. Baltimore: Johns Hopkins UP, 2010; xii + 184 p.

Margaret (‘Maggie’) Osler († 2010) takes care throughout her book not to speak of ‘the Scientific Revolution’. Consequently, she regards the full period covered (1500 to 1700) as one of ongoing change, even though 17th century developments take more space in her treatment than earlier ones. As another consequence of this decision, the question of ‘why’ is neither raised nor answered. Osler further insistently abstains from “searching the past to find the origins of modern science ... [Rather,] instead of discovering distorted reflections of our own preoccupations in the past” we must seek to understand how the natural world looked to those who lived in the period covered, and how and why their views changed. Not that these points of departure (no Scientific Revolution; no relation to modern science) lead to a narrative much different from the other textbooks here discussed. Here, too, the order of presentation leans chiefly on disciplines and on individual practitioners, with more than customary attention being given to Osler’s own research subjects – Pierre Gassendi, and (as also in Henry’s book) theological controversies, chiefly in regard of much-debated problems regarding God’s omnipotence.

Lawrence M. Principe, *The Scientific Revolution. A Very Short Introduction*. Oxford UP, 2011; xiv + 148 p.

In accordance with the series in which this booklet appeared, it is the shortest of all ten books here discussed. Principe acknowledges a variety of problems that have arisen about the expression ‘the Scientific Revolution’, but adopts it all the same. In so doing, he pays due attention throughout his book to the mixture of continuity and break that marks the episode, which he dates 1500 – 1700. In his Introduction as well as in his Epilogue he seeks to define what the episode has meant for us nowadays, in terms of laying the groundwork for modern science *but also* (a viewpoint not found in the other books) of what “rich worlds of beauty and promise that we have often forgotten how to see” got lost thereby.

Principe arranges the customary plethora of disciplines under three headings – ‘the superlunar world’; ‘the sublunar world’, and ‘the microcosm and the living world’. This is preceded by an account of what the

world looked like prior to the Scientific Revolution proper, which he characterizes as a “connected world”, and it is followed by a chapter under the heading ‘Building a world of science’. Here Principe shows how science turned from a marginal phenomenon in the larger society into an enterprise much more incisively present due to institution building, to the possible usefulness of a range of discoveries, etc. In opening his book with the comet of 1664 and the ensuing, Europe-wide debate over its properties and its significance, he encapsulates in one telling image all these leading topics, from a connected world on its way out but still vividly there, to a world of science already palpably in the making.

Naturally, the subject most prominent in Principe’s own research, the history of alchemy in the framework of what (using a contemporary term) he has dubbed ‘chymistry’, gets special attention in his chapter on the sublunar world. Nowhere in his book, finally, does he pose (or, naturally, answer) the question of what may have caused the Scientific Revolution.

This completes my overview of ten short to very short textbooks that have appeared between 1994 and the time of writing (2012). In addition to their text proper, which rarely exceeds some 100 to 150 pages, most authors provide illustrations; footnotes; suggestions for further reading or even fairly detailed bibliographical essays (many take care to mention my own historiographical book); time tables; a selection of source texts; indices, etc.

Further, all books (those by Shapin, Vermij, and Blay excepted) have been written at the invitation of the editor of one or another book series. These series are without exception dedicated to episodes in European history, in such a manner as virtually to exclude more than superficial treatment of nature-knowledge in ancient Greece, and in most cases confining their authors to a starting date no earlier than c. 1500. Similarly, insofar as a rare author mentions the non-Western world at all, it makes its brief appearance only by way of an Islamic stop-gap between the Greek legacy and the moment when medieval Europe takes it up.

Finally, and most importantly, the manner in which these ten authors have set up and arranged their respective narratives has been determined in good part by the primary challenge each had to meet, which was, simply: ‘how can I best serve the students to whom their teachers will assign my book?’ Indeed, all ten books have been written with a view to treatment in class, so the composition and pace of the narrative are quite rightly dictated by class-room needs in the first place. As a consequence, and whatever their mutual differences, each book without exception takes the ‘master narrative’ of the Scientific Revolution for its main organizing principle. That is, the account of the Scientific Revolution such as it has taken shape roughly between Koyré and Hall governs the main story line in all of them. To be sure, each author has made a personal choice out of available views and conceptions. Most authors have taken the trouble to indicate in what respects they felt the master narrative to leave insufficient room for a more truly satisfactory, up-to-date account of the Scientific Revolution. And yet, the format of a class-room text does not really provide a ready-made occasion to think up viable alternatives. Whether or not their authors would under less constraining conditions have grabbed the opportunity for more than marginal revision of the standard account (an issue I discuss in the next section), so much is certain that textbooks do not furnish a suitable opportunity for reconsidering things in a more fundamental way.

With the two books to which I turn now, things stand differently. Neither Gaukroger's book nor my own has been written as a brief text with the class-room uppermost in mind, but rather as a scholarly monograph well-placed in principle to rethink more or less from scratch the entire compound of problems meanwhile besetting the concept of the Scientific Revolution, and to come up, if possible, with a fresh view. I shall now first discuss (also in brief) the vision that Gaukroger has come up with.

Stephen Gaukroger, *The Emergence of a Scientific Culture. Science and the Shaping of Modernity 1210–1685*. Oxford UP, 2006; ix + 563 p.

For all its length and its vast scholarship, Gaukroger's book is only the first instalment in a multi-volume series entirely to be written by himself. One more volume has appeared so far. Each is to carry the same subtitle *Science and the Shaping of Modernity*. This defines quite adequately what the author aims to accomplish. A uniquely Western development, which started in medieval Europe, has eventually given rise to "the assimilation of all cognitive values to scientific ones", and the aim of Gaukroger's entire project is to arrive at an understanding of how this world-historically unique state of affairs has come about.

In view of the period covered in this first volume, there is of course a great deal of overlap with whatever period one may prefer to settle on for the Scientific Revolution. This large overlap is why, however briefly, I find it important to discuss the book here. But the author neither means to say that the Scientific Revolution began in 1210, nor that it came to an end in 1684 – he has selected these two dates for different reasons, both connected of course with the core problem he wishes to pursue. 1210 is the year when, so he maintains, controversy began in earnest over the acceptability, from a theological viewpoint, of the Aristotelian conception of the world that had just been imported into Christian Europe by translators operating chiefly in Andalusia. According to Gaukroger, this controversy opened an unprecedented period, which still has to cease, during which concerns over the constitution of the natural world moved from the cultural margin it habitually occupied previously and elsewhere to a place ever closer to the center of scholarly and, in the end, most other people's cultural concerns as well. The year 1210, then, is when Europe began to move in a direction ever more markedly distinct from all those other civilizations that went through periods of flourishing inquiry of a broadly natural-philosophical nature. The European experience, with the unique continuity that the scientific enterprise acquired in that specific part of the civilized world, differs fundamentally in Gaukroger's view from earlier episodes in ancient Greece, in China, and in the Islamic world, all characterized instead by a 'boom-bust' pattern.

Gaukroger rejects three standard accounts of what gave European science, such as by the mid-1680s it came to look like, the permanence acquired since. In what he terms the Enlightenment interpretation of the Scientific Revolution, the triumph of the science then arisen was (and still is) habitually attributed to (1) its allegedly breaking free from religion, (2) its alleged autonomy, and (3) the alleged, ensuing emergence of a new, science-based technology. Rejecting these three standard views, Gaukroger means his book to show what it really was that, against all precedent, made for continuity in the scientific enterprise such as it took shape in course of the Scientific Revolution.

Large parts of what answers he arrives at in the present volume coincide of course with the period of the Scientific Revolution, and several themes are quite recognizably informed by previous work on its what,

how, and why. His protagonist is neither ‘science’, nor ‘early modern science’, but ‘natural philosophy’ – the expression that in his treatment covers all inquiry into nature during the period. Inside this broad concept of natural philosophy Gaukroger distinguishes between three main currents, one concerned primarily with quantity, one with ‘mechanism’, i.e., a philosophical conception of corpuscles in motion, and one with ‘natural history’ in the sense of the investigation of phenomena by means of observation, be it unassisted or carried out with the help of experiments and/or instruments. What happened inside these three currents at the hands of Galileo, Descartes, Bacon, and many other familiar protagonists makes up large portions of Gaukroger’s account once he has arrived at the first decades of the 17th century.

Besides the various developments that took place in these three by and large parallel currents, Gaukroger’s other prime concern is with how the pursuit of a fundamentally changing natural philosophy was legitimated by those who pursued it. What made their methods and their conclusions acceptable in a culture driven primarily by Christian concerns and accepted verities? Gaukroger seeks to demonstrate how the role of the natural philosopher changed during the period. Not the pursuit of truth, but of objectivity and impartiality in a context of aimed-for utility becomes paramount in course of the 17th century, so he argues at length. He concludes that, far from breaking away from religion, the new modes of natural inquiry were marked by a

combination of revelation and natural philosophy [which] produced a unique kind of enterprise, quite different from that of any other scientific culture, and one that was largely responsible for the subsequent uniqueness of the development of natural philosophy in the West.

In this sense, then, it was indeed the period of the Scientific Revolution that in Gaukroger’s view marked for good the eventual emergence of the modern world we live in today.

My own evolving views on the Scientific Revolution

One unmistakable conclusion from the foregoing account is that the ‘master narrative’ of the Scientific Revolution, once a powerfully liberating account which opened all kind of excitingly fresh vistas upon science and its past, has in the view of most (though not all) historians of science turned meanwhile into a straitjacket. There is no comfortable fit any more between that narrative and the story that textbook authors on the Scientific Revolution would prefer to tell. And indeed, up to a point I go along with this view. My historiographical inquiry already persuaded me that no monolithic account, be it ‘mathematization’ or any other, is able to cover all by itself the highly complex manner in which numerous (yet by no means all) basics of modern science first emerged in the 17th century. But this conclusion has not stood in the way of my unshaken conviction that (1) in the 17th century the decisive difference was made in an already centuries-long search for a reliable grasp of the constitution of the natural world; (2) the turnabout then achieved counts among the truly revolutionary episodes in human history, and (3) there is very good reason not to give up the search for underlying coherence in the face of an event so decisive for humanity’s subsequent fate all the world over. The profession as such appears to have given in to a shared skepticism over the very possibility of ever mastering all that complexity by means of an effort at fresh conceptualization. To me, such all-pervasive scepticism came down to the throwing away with the bathwater of a far from lethally ill baby.

In ch. 1 I have called the concept of the Scientific Revolution a ‘secret treasure’. When I wrote that chapter I only meant to say that historians of science are in possession of one indispensable master key to the enigma

of the rise of the modern world so much debated by scholars in other academic niches, without these scholars taking much notice of our all-too-well hidden treasure. In the face of the rapid abandonment all around me of the very notion of the Scientific Revolution in any sense whatever, I was now facing a treasure we were meanwhile busily hiding from ourselves as well.

But why should we? If for several good reasons a monolithic concept no longer satisfies, the sensible solution is to come up with a more pluralist one. What we need, so I reasoned, is a fresh conceptualization that preserves what remains valid in its predecessor but widens the concept of the Scientific Revolution without, at the other extreme, making it drown in its own complexity. By 'pluralist' I mean that underlying coherence should be searched for and dug up in more territories than that of mathematization alone, and also one level deeper than so far. My historiographical book, deliberately set up as more than just an inventory of historians' ideas, already yielded a number of highly fertile but as yet underexploited conceptions, approaches, and ventures into untrodden territory, all well-suited in my view to widen, deepen, and in the act partly alter the master narrative. The time had come to exploit these ideas for all they would prove worth, and to see how far they would get me in my upcoming venture at long last to tackle the Scientific Revolution straight away. This is the upshot of chapter 7 which you have just been reading.

What, then, are the most promising ideas I encountered in the literature, and what did I find myself doing with them in course of the long drawn-out composition of *How Modern Science Came Into the World*?

In search of a new protagonist

What my historiographical effort had alerted me to in the very first place was that I had to make a clear-cut decision about what unit of analysis would best suit my new purpose. Who, or rather what, should be the protagonist of my new book? Should I stick to 'early modern science'?

My decision to get rid of that expression came in three steps.

In the first place, I ceased calling the *outcome* of the Scientific Revolution 'early modern science'. Not only does the term not denote adequately the remarkable and, on the whole, radically novel (albeit of course far from complete) similarity between the best procedures and products of the Scientific Revolution and certain basic products and procedures that mark fully modern, that is, present-day science. But the entire term 'early modern', used so often for the period 1500–1800, wrongly suggests that the advent of modernity was already implied as a necessary outcome in what happened next – a fatally teleological implication of the term 'early modern' that I had failed to capture earlier.

Next, the term 'science' as such. As with 'early modern', in *The Scientific Revolution* I still used it without much hesitation. But could, for instance, the Aristotelian corpus fairly be called 'science'? Or could Taoist conceptions, or Descartes' collected views on how our world is constituted? When you call, for instance, Aristotle a 'scientist', it is almost impossible for you *not* to visualize him in some remote corner of your mind as a white-coated laboratory worker engaged in a painstaking effort to accomplish the next breakthrough. To follow tradition in calling conceptions like those of Aristotle or the Taoists or Descartes 'science' evokes too many associations that reflect present-day science rather than any allegedly early counterpart. So I went along with a sentiment increasingly noticeable among present-day historians of science in search of a suitable replacement for the term 'science' during the full period covered, i.e., at least up to the end of the

17th century.

But (third and final step) what to replace it *with*? For many professionals it has meanwhile become almost standard usage to speak for premodern times, not indeed of ‘science’, but of ‘natural philosophy’. But this seems to me much too broad a term, particularly if used indiscriminately for any effort whatsoever to come to grips with nature or some identifiable part of it. Its usage obliterates basic distinctions between a variety of such efforts that more and more struck me as vital. Take, for instance, the well-known distinction between empiricist and intellectualist approaches, that is, between insights attained chiefly through observation or derived chiefly from thought. So I decided to coin an expression of my own. The activity that in *The Scientific Revolution* I still called ‘science’ I now call ‘the pursuit of nature-knowledge’ – a deliberately bland term, meant to attain proper meaning only when specified further. This decision quickly yielded in its turn the very unit of analysis that I needed for my new book. It is ‘modes of nature-knowledge’, and here is how in *How Modern Science Came Into the World* I came to define it:

... consistent ranges of distinct approaches to natural phenomena, which may differ in several dimensions. Their scope may have been comprehensive, with a view to deriving the whole wide world from first principles, or deliberately partial. The way in which knowledge was attained may have been predominantly empiricist or chiefly intellectualist. If any practices went with a given mode of nature-knowledge, these may have been observational, experimental, instrumental, etc. Knowledge may have been sought for its own sake or with a view to achieving certain practical improvements. Exchange may or may not have taken place between practitioners of distinct modes of nature-knowledge that were pursued at the same time and place.

Of particular concern in distinguishing a variety of modes of nature-knowledge is what I label their ‘knowledge structure’. By this I mean something that the principal difference between Aristotle and Galileo illustrates. For instance, we shall find in due course that in the 17th century much conceptual confusion emerged from the different handling of seemingly similar or even overlapping conceptions (notably those of motion and force) in different modes of nature-knowledge (those of Aristotle and Galileo, but also those of Galileo and Descartes). Was knowledge organized wholesale or rather piecemeal? How was knowledge conceived to be oriented in time – did practitioners see themselves as working toward an open future, or as reconstructing past perfection, or as personally constructing the truly definitive schema of all possible knowledge? At what level of abstraction did they seek to capture nature’s phenomena? How were empirical facts handled – in their own right or made to serve some a priori schema and, if the latter, by way of illustrative confirmation or for a posteriori checking?

I treat these modes of nature-knowledge as dynamic entities. What turns them into viable instruments of historical analysis is the additional category of *transformation*. Modes of nature-knowledge need not remain fixed over time. At least potentially they were subject to being transformed, in ways that varied from enrichment within a given framework to such revolutionary transformations as came in time to mark the Scientific Revolution.

One more, very useful service rendered by taking ‘modes of nature-knowledge’ as the unit of historical analysis throughout my new book was that it could provide me with the ideal basis for sustained comparison. *Inside* a given mode of nature-knowledge I could now meaningfully compare between various practitioners at work in basically the same mode (e.g., Galileo with Archimedes, or Hooke with Bacon). But I could also make productive comparisons *between* distinct modes of nature-knowledge. In this regard the ‘non-European’ chapter 6 of *The Scientific Revolution* had prepared me well. To write the chapter was an exhilarating experience in its own right. At the outset I knew nothing of the historiography of Chinese or Islamic ‘science’; everything I encountered along the way was fresh to me, and yet, when I had finally completed the chapter I had a sense that I had been walking around in its protagonist’s very brain – I felt that I just *knew* how Joseph Needham’s extraordinary mind worked. However critical I gradually turned of most of Needham’s views, it was his work above all that convinced me of the indispensable aid cross-culturally comparative history of science could give me in seeking to resolve the big problem that I was after.

To be sure, my ongoing identification of the numerous traps that Needham, the great pioneer, fell into helped me avoid them when my own turn came. Above all, my historiographical forays into the works of Needham and the other authors I discussed in that chapter enabled me to cut across the biggest knot I encountered there — *what exactly should be compared with what?* China with Europe (following Needham)? The Islamic world with Europe as such (following von Grunebaum)? Or with medieval Europe specifically (following Sayili)?

Here is how I cut my way through this historiographical jungle. Notwithstanding the vast contrast between the more than two millennia long, really unbroken pursuit of nature-knowledge in premodern China and the far shorter (one millennium at most) duration of a similar pursuit in ancient Greece, I began to realize that, for the Chinese case, here resides the one comparison it is sensible to make. Neither Chinese nor Greek civilization produced modern science, yet both produced a viable corpus of nature-knowledge in its own right that is very much worthy of mutual comparison, which I went on to make indeed, using for my empirical data and their interpretation mostly investigations by Sivin (his work on the Han-synthesis, on the season-granting system, on Shen Kua, and on alchemy) and by Graham (his work on Mohism).

Please note that in the previous paragraph I affixed the expression ‘really unbroken’ *only* to the Chinese tradition. The fact is that the Greek tradition *was* broken — unlike Chinese civilization, its Graeco-Roman counterpart did come to an end. To be sure, both civilizations were overrun by foreign invaders, but the Chinese one survived the Mongol and then the Manchu onslaught essentially intact, whereas between c. 400 and c. 650 Graeco-Roman civilization perished for good.

None of this is in any way new, of course, and yet, why did I suddenly find the contrast so fraught with explanatory significance for my core problem? The answer resides in a novel concept that at some point in the endlessly revised composition of the first chapter of my new book finally dawned upon me. I named that concept ‘cultural transplantation’. By this I mean the transfer of an entire body of knowledge to a civilization not previously touched by it, and the core idea is that such a transplantation unto so far untried soil provides the corpus thus transplanted with fresh chances for new and possibly superior growth, expansion, enrichment, or even transformation of a more or less radical kind.

In this regard I eagerly seized upon David Landes’ idea of ‘a magnificent dead-end’ (*SRHI*, 6.4.). There it serves only to illustrate the vast difference between the European-medieval mechanical clock and Su Sung’s water clock, in an effort to get rid of Needham’s strictly unilinear, evolutionary approach to the history of science. The mechanical clock, originally no more accurate than, or rather vastly inferior to, Su Sung’s water clock, nonetheless as time went on proved capable of further development (quick, extended distribution; miniaturization; improvement, notably by means of the pendulum) whereas the water clock already reached its pinnacle right at the start, only to see its action deteriorating over time due to accumulating corrosion and dirt. The idea that an invention or a discovery may in retrospect turn out to possess certain *latent developmental potentialities* which only become manifest over time, doing so under certain locally favorable circumstances only, got hold of me to the point of extending it from individual inventions or discoveries to entire bodies of knowledge like those of the ancient Greeks or the ancient Chinese.

In the end, then, my comparison between the two came down to this:

The organic/correlative view of the world that, as the Mo Tzu text lineage faded away, came to permeate the Chinese corpus of nature-knowledge may or (much more likely) may not have had the potential that its Greek counterpart possessed in retrospect to be transformed in such a way as to lead to the emergence of recognizably modern science. In terms of actual accomplishment neither corpus was inherently superior. The decisive difference is that the one, but not the other, was to meet with opportunities for the latent developmental potential contained within it to become manifest over time.

With this particular solution to ‘the Needham question’ behind me, the vicissitudes of the Greek corpus of nature-knowledge became my next big concern. To what civilizations precisely was it successively transplanted?

Three cultural transplantations

Once that particular question had taken shape in my mind, the answer became clear as well – not only, obviously, to Islamic civilization and to medieval Europe, *but to Renaissance Europe as well*. No less than three big translation efforts had happened to the Greek corpus, not only those in 8th century Baghdad (Greek to Arabic) and in 12th century Andalusia (Arabic to Latin), but also a third and final one in Italy and beyond, due to the 1453 fall of Byzantium. Recall that Paul L. Rose in his *The Italian Renaissance of Mathematics* (SRHI 4.4.2.) makes the Renaissance period take shape as an episode in its own right, not just as an indistinct bridge between nature-knowledge in the Middle Ages and in the 17th century. Due in good part to this (to me) breakthrough insight, which I also found expressed in some of Noel Swerdlow’s work, I finally came to realize that one fundamental presupposition of the Duhem thesis (SRHI 2.2.4.) still lingers on in almost every historian’s mind. Whether the medieval episode is being regarded as itself revolutionary or as an indispensable run-up to the Scientific Revolution or not even that, just about every historian of science keeps treating it as the immediate predecessor to whatever revolutionary things happened in the 17th century. Rose’s account, in marked contrast, made me realize that the medieval episode was fairly short and self-contained, *and that by the middle of the 15th century Europe made a fresh start in many respects*.

From the point of view thus gained, I could now rephrase my core problem as follows. Three cultural transplantations happened to the Greek corpus, at first to Islamic civilization, then to medieval Europe, and for a third and final time to Renaissance Europe. Each transplantation was a wholly unintended consequence of some big military event – the first civil war in the Islamic world, due to which around 800 the reigning Ummayyad dynasty was replaced by the Abbasids; the Reconquista, in course of which the Islamic part of Spain was gradually taken over by Christian kings from the North of Spain; the 1453 conquest of Byzantium by the Ottoman sultan Mehmet II. Each time a translation effort was set up; each time it made for a renewed flourishing of the Greek corpus. Whether adopted in the Islamic world or in medieval Europe or in Renaissance Europe, recovery and appropriation led to enrichment – in each case certain developmental possibilities latent in the original corpus became manifest. Now what specific enrichments accrued to the Greek corpus in each distinct case, and what did these have in common?

I decided that I should not confine myself to listing and comparing individual accomplishments alone. In order to make my comparisons truly viable I had to make them within the frame of some overarching conception. Inspired once again by certain authors discussed in *The Scientific Revolution*, I hit upon three such conceptions – a general pattern of upswing and decline; the presence of a basic dichotomy inside the Greek corpus; distinction made, inside the sum total of nature-knowledge pursued in all three cases of

transplantation, between the Greek corpus itself and certain more locally colored efforts pursued around the edges of the Greek corpus.

Recall my comparison between historians' debates about the problem of decline in the case of the Graeco-Roman and of the Islamic pursuit of nature-knowledge (SRHI 6.2.5.). Their respective historians were agreed among themselves that decline had set in at some point, yet there appeared to be no agreement whatsoever over *when* it took place and *what* made it happen, nor had it occurred to them to compare the two cases with each other. I also noticed the presence of a trap each author who addressed the problem of decline had fallen into — every single *dating* of decline appeared to depend largely on its preferred *explanation*. That is how far I came in *The Scientific Revolution*, but now I faced the question of how to break through the vicious circle.

Here Sayili's work proved most helpful. Historians of the Abbasid period are wont to speak of 'The Golden Age of Islamic science', and Sayili defined that particular Golden Age in a manner fit to be expanded far beyond Islamic civilization. His criterion (SRHI 6.2.3.), shared more implicitly by a few others, was the presence of a relatively dense constellation of first-rate practitioners. Indeed, in both cases historians were near-unanimous in pinpointing their respective Golden Ages (even though in the Greek case the period of highest flourishing was rarely called by that name). Decline can then be defined quite simply, and irrespective of any preconceived explanation, as the coming to an end of the Golden Age. In the Islamic case decline set in c. 1050, right after the near-simultaneous demise of Ibn Sina, al-Biruni, and Ibn al-Haytham, none of whose works were followed up, let alone enriched, by the immediately succeeding generation, which just turned to other concerns.

Sayili's vision also opened the way toward resolving the problem of decline in the original, Greek case — authors seemed by and large agreed that the 3rd and 2nd centuries BC witnessed a uniquely dense cluster of first-rate practitioners (SRHI 4.2.). However, I was well aware that Lloyd refused to date the decline of Greek nature-knowledge that early (SRHI 4.2.3.) — how could we speak of decline prior to the appearance, centuries later, of such first-rate practitioners as, notably, Ptolemy and Diophantos? I felt rather at a loss to handle this particular objection until a conversation I had with F. Jamil Ragep, an expert in the history of nature-knowledge in Islamic civilization. He pointed out to me that my account of decline in *The Scientific Revolution* was by and large alright as far as it went, yet, so he insisted, *there had been a 'resurrection'*. After all, so he argued, there is this vast legacy of pertinent manuscripts dating from later times and located partly in Persia, partly in Istanbul, partly in Andalusia, and of which the sum total considerably alters any simple 'decline' picture.

Both Lloyd's and Ragep's objections, which I realized at once to be quite similar, stemmed from plain facts of history I felt I could not in good consciousness ignore or sidestep. Once their combined impact had sunk in, they opened the pathway toward a necessary *refinement* of the very concept of decline I had so far adopted without more ado. Here is how in my new book I came to define what happened in both cases:

In each case an upswing takes place that within two to three centuries culminates in a relatively short-lived 'Golden Age', and then a steep downturn occurs *that is nevertheless punctuated by some rare, individual achievements at a level of quality far above what has in the meantime become standard.*

In other words, there is a postlude to decline; there are episodes of renewed flourishing, with one major difference however — the direction has as it were been reversed, the original sense of fresh discovery has

gone, and questions stemming from the Greek corpus are now taken up in a different vein, guided by a backward orientation upon the original Golden Age. Ptolemy, or Nasir al-Din al-Tusi, may well have surpassed Golden Age astronomers, yet they no longer opened new perspectives but sought to improve upon the work of those who made their respective Golden Ages in the first place.

Why did decline set in? In *SRHI* (4.2.3; 6.2.) I listed a large variety of possible causes. Each explanation, whether given for decline in Islam or in ancient Greece, could claim some measure of plausibility for it, yet none looked decisively more persuasive than any other. More and more I inclined toward a consequence I came to draw from Ben-David's account. In his view (*SRHI* 4.2.3.), fairly short-term flourishing of the pursuit of nature-knowledge is the (as it were) natural state of affairs, whereas the continuous growth of the scientific enterprise we are accustomed to nowadays is the one big, almost "pathological" historical exception really in need of explanation. It follows from this brilliant insight that not decline itself requires explanation, since it is bound to occur anyway, but at most its timing – why did it occur *when* it occurred? In this more modest vein, Saunders' explanation in terms of the unceasing waves of barbarian invasions which started c. 1050 (*SRHI* 6.2.4.) appeared to fit in seamlessly.

Now to what extent would my upswing/decline pattern remain valid once applied to the two remaining cases of cultural transplantation of the Greek corpus, that is, to medieval and to Renaissance Europe? For medieval Europe it was not hard to pinpoint its upswing in the 13th century (when Albert the Great and Thomas Aquinas produced their great summaries of Aristotelian doctrine), its Golden Age in the 14th (the Calculators, Buridan, Oresme), and a very steep decline that started with Oresme's death in 1380. Medieval scholasticism, henceforward oriented in the most sterile fashion upon its own Golden Age, ossified beyond repair – in this particular case no postlude of any significance took place. Finally, there is the case of Renaissance Europe. Here, too, an upswing may easily be defined, starting by mid-15th century and smoothly ushering in a Golden Age marked by a fresh constellation of first-rate practitioners such as manifested itself in the final decades of the 16th century (e.g., Benedetti, Vincenzo Galilei, Clavius, Ramus, Stevin). But what about decline?

Before setting forth how, instead of decline, the Scientific Revolution itself intervened, I must return to the Greeks and mention another overarching conceptualization of the kind I needed to work out my comparison between three successive cases of cultural transplantation. This particular conceptualization concerns the Athens / Alexandria dichotomy, and I owed it in large part to a certain passage by Samuel Sambursky of which I failed to pick up the import until after completion of *SRHI*. It began to dawn upon me that there had been a twofold followup to the pre-Socratic period. From Plato onward a range of all-encompassing philosophical systems were construed, with Athens for shared geographical center. Later, after the foundation of Alexandria, the Pythagorean tradition was refashioned into efforts to mathematize in a very abstract manner a quite limited range of empirical subjects (solid and fluid equilibrium, light rays, vibrating strings, and planetary trajectories). Apart from a few shared conceptions like (notably) the common notion of an Earth fixed at the center of the universe, hardly any effective intellectual exchange took place between 'Athenian' and 'Alexandrian' practitioners. The same situation was repeated in the Islamic world, also in medieval Europe (with a uniquely marginal, even subordinate, position left for mathematical

science), and all over again in Renaissance Europe.

So with all three transplantations there was, not one homogeneous corpus of Greek nature-knowledge, but a dichotomy that cut right through it, even in those rare cases where one and the same individual used to practice both natural philosophy and mathematical science.

And yet, each cultural transplantation witnessed something else as well, some efforts that did not go back to the Greeks but rather marked what was peculiar about the civilization itself in which, as an outgrowth of the translation effort, nature-knowledge now found itself pursued. In Islamic civilization, such coloring around the edges of the Greek corpus derived directly from exigencies of the faith, as with the usage of newly invented algebra for the calculation of legacies in accordance with rules prescribed in the Quran. In Europe, a similar coloring took place on a far wider scale. Work by my teacher in history of science, R. Hooykaas, but also some of Max Weber's insights (*SRHI*, 3.6.3.), helped me realize that the pursuit of nature-knowledge in Europe was colored right from the start by a peculiarly West-European, outer-directed variety of the Christian faith. No more than marginally manifest during the medieval episode, by mid-15th century such pursuit took shape as a third, by and large independent current beside efforts to recover the texts that contained the Athenian and the Alexandrian legacies. This third current was marked by an empiricism quite distinct from the predominantly intellectualist approach characteristic of the Greek corpus. It was also marked by an urge to turn the outcomes of empirical research to practically useful ends. So the Golden Age of nature-knowledge in Renaissance Europe comprised, not only authors working in an Alexandrian vein (e.g., Copernicus) or in the Athenian tradition (e.g., Clavius), but also numerous men given to empirical research aimed at practical improvement, such as Leonardo, Tycho, Vesalius, or Paracelsus.

The onset of the Scientific Revolution

At the point in time now attained, very roughly the year 1600, something truly remarkable happened – the onset of what I keep calling the Scientific Revolution. The widely diverging dates that historians have assigned to that Revolution form a recurrent theme in *SRHI* (I have listed them in the book's Index under 'Scientific Revolution; temporal frame of; dating of'), as also in the previous section of the present Postscript. It will by now be clear why I myself opt for the early 17th century as the analytically most productive date for the onset of the Scientific Revolution. Buridan and Oresme did not start it, but rather formed the culmination point of the medieval episode. Copernicus did not start it, either, as there is nothing revolutionary about his restoration to its pristine purity of Ptolemaean planetary theory. As Dijksterhuis and Kuhn in effect argued, the conception of a revolving Earth that Copernicus used for his backward-oriented effort did not become truly revolutionary until Galileo and Kepler became aware of the consequences and went on to expand these into a new kind of realist-mathematical science far removed from its Ptolemaean and Archimedean predecessors.

May we speak indeed of a Revolution, that is, of a radical break with everything that went before? Both in *SRHI* 2.5. and in the previous section I have shown that, and how, the 'continuity vs. rupture' opposition has kept occupying historians of the rise of modern science. My own solution to the perennial quandary comes in two parts.

In the first place, my idea of cultural transplantation involved the possibility (not the necessity, but,

indeed, the possibility) of enrichment of the Greek corpus on foreign soil, such as indeed took place in each case – in the Islamic world, in medieval Europe, and all over again in Renaissance Europe. But enrichment could take a variety of guises, from incidental improvement while leaving intact the Athenian and Alexandrian bodies of nature-knowledge to something that I call ‘revolutionary transformation’. The latter, so I now argue, is what *might* have happened with the Alexandrian legacy in the Islamic world c. 1050, but which *did* happen with all three Renaissance European currents near-simultaneously. So I define ‘revolution’ not in absolute terms, as if anything totally new could ever happen in history, but as something relative, that is, in relation to things changing more smoothly and at a slower pace. Indeed, I argue that of these three near-simultaneous transformations one was considerably less revolutionary than the two others.

Secondly, I picked up a historical thought-experiment by Donald Cardwell, who wondered what technological domain would, to a well-informed outside observer, have seemed by the early 18th century the most ready candidate for profitable investment and drastic change. The observer’s answer would in all likelihood have been ‘metallurgy and mining’, whereas, in historical reality, the spinning and weaving of textiles became the big agents of change until far into the 19th century. I for my part placed an equally well-informed observer in the year 1600, and let that wholly imaginary person report on present trends and on that basis predict the most likely future. That way I sought to demonstrate how radically unpredictable events after 1600 truly were – not the decline following naturally upon each preceding Golden Age, but a no less than threefold, truly revolutionary transformation.

‘Threefold’ indeed – in *SRHI* 2.4.5. I have demonstrated the presence of a historiographical movement, starting with incidental remarks by Koyré and Burttt but taking wider proportions with Westfall and with Kuhn, to split up the Scientific Revolution over two or (as subsequently with Hakfoort) three parallel currents – one mathematical, one natural-philosophical, one ‘Baconian’. For the period up to c. 1640 I go along with that broad schema. I have made an effort to pinpoint precisely what novel features of mathematical science and of natural philosophy were so revolutionary. The names I give to the three revolutionized modes of nature-knowledge thus arising are meant to express those decisively novel features. The one instigated by Kepler and by Galileo (‘Alexandria-plus’ for short) I call mathematical-realist or mathematical-experimental. The one instigated by Beeckman and Descartes (‘Athens-plus’ for short) I call kinetic-corpuscularian, that is, out to explain the world through moving corpuscles. In the third, Bacon, Gilbert, Harvey, and van Helmont transformed (in somewhat less revolutionary fashion, to be sure) practice-oriented empiricism into what I came to call fact-finding experimentalism.

However, the process of revolutionary transformation did not stop there. We come now to a somewhat curious aspect of just about the entire historiography of the Scientific Revolution. It marks, not only the works discussed in previous chapters, that is, up to 1994, but just about all later books on the Scientific Revolution as well. Since textbooks on the Scientific Revolution have been organized as a rule with distinctions between disciplines uppermost in mind, the reader may find himself jumping from Galileo to Newton in one context and then back to the Renaissance in another. If the author acknowledges any specific currents inside the Scientific Revolution (mathematical, experimental, etc.) at all, these are presented as static entities which persist over the entire event essentially unaltered. One exception in this regard is Westfall’s

Construction (SRHI 2.4.5.), which has for principal story line a dynamic tension between the Galilean and the Cartesian approaches and how at the end of the period Newton resolved it.

Recall now that (much inspired by Westfall's account) I purposely defined my central concept, 'modes of nature-knowledge', as dynamic entities, that is, as subject to possible change, with the amount of change extending from enrichment to revolutionary transformation. I now began to realize that they were also subject to partial or wholesale *merger*. That is, the far-going mutual separation between such distinct 'modes of nature-knowledge' as the mathematical, the natural-philosophical, and the empiricist mode, which coexisted during the European Renaissance and then found themselves unpredictably transformed in revolutionary fashion, need not persist. Over the first stage of the Scientific Revolution (c. 1600 – 1645), 'Alexandria-plus', 'Athens-plus', and fact-finding experimentalism by and large remained thus separated, and yet, two more stages were to follow, with characteristics of their own. By 1660 a certain amount of fusion began to manifest itself. This novel feat of revolutionary transformation yielded two, once again partially novel 'modes of nature-knowledge', followed a quarter-century later by n° 6. In my final view, then, the Scientific Revolution was made up of six narrowly interrelated, revolutionary transformations. Three took place near-simultaneously 1600 – 1645, two more followed c. 1660 – 1685, and a final one (Newton's) c. 1685 – 1700.

After the 'how?' the 'why?'

With this result firmly in hand, I could turn my growing dissatisfaction with how historians of the Scientific Revolution were wont to explain the event to a constructive alternative. As chs. 4 and 5 have shown, explanations came almost without exception in the format of *theses*. Lengthy arguments were set up to maintain that the Reformation, or capitalism, or refinements in Aristotelian doctrine, or the printing press, etc., 'caused' the rise of modern science. Each of these manifold theses formed a monolith in itself – just one specific historical phenomenon was adduced as a catch-all cause. The outcome was fairly obvious: each thesis quickly found its stern critics, who pointed out its many shortcomings in view of the ever more apparent complexity of the sum total of events that constituted the rise of modern science. Nonetheless the production of 'theses', and their ongoing rumination in mutually isolated niches, went on unabated until the 1980s.

What came in their place since is, once again, nothing – the search for causes, felt to be hopeless anyway, was given up together with the concept of the Scientific Revolution itself. Historians of science failed to act upon the wonderfully rich suggestion that Thomas S. Kuhn made in his 'Mathematical versus Experimental Traditions in the Development of Physical Science' (1977; SRHI 2.4.4.; 5.1.2.). There he made a case for confining the validity of several well-known causal theses to just one of the two strands distinguished by him. For instance, he very sensibly suggested to limit the relevance of the 'Merton thesis' to the 'Baconian' strand of the Scientific Revolution only.

With the Scientific Revolution now defined by me as a tightly interconnected range of six revolutionary transformations, I seized upon the welcome opportunity thus created to get rid of this odd habit of explanation in the format of large-scale 'theses'. I could now point instead at certain events and circumstances that might plausibly serve as causal links with the specific feat of transformation under

scrutiny. In that altered format, I could of course employ what remained causally fruitful in those theses, but now cut down to proper size. For instance, the printing press, proclaimed by Elisabeth Eisenstein as the one and only cause of the Scientific Revolution in its entirety (SRHI 5.2.9.), turned out to be indispensable, not so much for revolutionary transformations (1) – (3), yet so much the more for those that followed.

Please notice that the distinct explanations I came up with for each revolutionary transformation apart were subsidiary to the answer I had already given to the basic question of why modern science arose in Europe, not elsewhere. Indeed, that answer followed from the various cross-cultural comparisons made earlier, which jointly led to the following core conclusions:

- (1) Modern science rested as a latent developmental possibility in the Greek corpus of nature-knowledge, probably not in its Chinese counterpart.
- (2) Unlike with China, the Greek corpus was subject to three successive feats of cultural transplantation.
- (3) The third and final cultural transplantation, in Renaissance Europe, turned out (due to a range of circumstances specific for European civilization) to be the most radical one of the three that occurred. It was the one best suited to produce indeed what rested inside as a mere potentiality – wholly unpredictable transformation of an, indeed, revolutionary kind.

Two further considerations make my various explanations, even if taken conjointly, less than exhaustive. In the first place, I do not subscribe to a view of history where everything that ever happens does lend itself to explanation. History, I think, is made up of concatenations of successive events that run on a retrospectively detectable logic of their own, but also of fortuitous or at least highly coincident events that at unpredictable moments (Dzhengiz Khan's conquests are obvious examples) cut right through the unfolding logic of more ordinary events.

Secondly, what about the c. 15-years' gap between revolutionary transformations (1) – (3) and the two later ones? For an answer, I benefited greatly from certain views of Ben-David, further from Shapin & Schaffer's *Leviathan and the Airpump* and from a good deal of more-recent literature devoted to the questionable credibility of experimental outcomes as perceived at the time from the outside. Also, already when I was preparing the Index for *The Scientific Revolution* I began to realize that in the main text I had failed (e.g., in what I wrote about Merton and Ben-David) to make a proper distinction between two entities increasingly conflated by historians of science, that is, between matters of *justification* and of *legitimation*. After all, to make a case for why you think your conclusions about some mathematical regularity or experimental outcome are right is not the same thing as making a case for your being engaged in a worthy activity, beneficial to or at least compatible with the values of the civilization you live in. All these considerations together helped me to the conclusion that in the 1630s and 1640s the legitimacy of those revolutionary currents (in particular those to which the names of Galileo and Descartes were attached) quickly eroded, with possibly lethal consequences for the entire movement of radical renewal. Those roughly fifteen years 1645–1660, then, were marked by a vast, Europe-wide crisis of legitimacy, which on further reflection I found composed of two distinct elements – *strangeness* and *sacrilege*.

By 'strangeness' I mean that all kinds of practices utterly familiar to us nowadays, like the airpump experiments Shapin & Schaffer focused on in their book, but also Galileo's very conception of a universe

governed in the final analysis by mathematics, was bound to look extraordinarily odd and far-fetched to those in the 17th century not accustomed to them, that is, to just about everybody but the pioneers themselves.

‘Sacrilege’ refers in particular to a range of conflicts over theological issues fought out between certain proponents of the new currents in nature-knowledge and several authorities, notably Galileo vs. the Vatican, Descartes vs. the city of Utrecht, and Descartes’ orthodox followers vs. the French king and the archbishop of Paris.

As a consequence of these two powerful sentiments, strangeness and sacrilege, so I came to argue, all over the European Continent the movement of revolutionary transformation quickly lost momentum in a manner fit to threaten its very survival.

This conclusion in its turn provoked a second principal problem, to be posed beside my first one of how modern science got going, to wit, *how did it manage against many odds to survive?* Clearly, viable answers had to explain how strangeness and sacrilege either ceased to be felt so acutely or were in effect neutralized. Here I could benefit from significant portions of ch. 5, notably Ben-David’s *The Scientist’s Role in Society* but also Weber’s, Hooykaas’ and Rabb’s work and even portions of the Merton thesis. Key to my answer were the Peace of Westphalia of 1648 and the spirit of compromise fostered thereby, and the rise of a ‘Baconian Ideology’. To set forth in any further detail how my exposition of what enabled the process of revolutionary transformation, so enthusiastically embarked upon around 1600, to regain its pace by the 1660s, would send me far beyond the proper boundaries of the present postscript.

It is time indeed to draw this Postscript to a close. Writing it has occasioned me to reconsider all those many things that happened in regard of the concept of the Scientific Revolution since by the early 1980s I set out to list, to summarize, and to compare what its historians had to say about it. By then, the most recent monograph on the subject was Rupert Hall’s *The Revolution in Science* (1983; *SRHI* 2.4.3.). By now, the latest contributions are the short textbooks by Osler and Principe that I discussed in the previous section, and my own monograph. Not quite three decades separate Hall’s book from Osler’s, Principe’s, and my own. The numerous, also vast *differences* between what was written then and what is being written now testify to the vitality of the profession of the history of science. Their even more numerous *similarities* testify to the penetrating vision of those three pioneers (Dijksterhuis, Burt, Koyré) who almost a century ago began to conceptualize the incisive upheaval known since, and for a long time to come, as the Scientific Revolution.

H. Floris Cohen
Amsterdam, July 2012