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Reconceptualizing the Scientific Revolution[1]

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Academics all over the world rightly desire to understand how modern science has come about. Indeed there was a time when historians of science had on offer a clear-cut conception of how that happened. But ongoing innovation in historiographical approaches has rendered the period from Galileo to Newton ever more elusive. Its monolithic coherence has been dissolved, a mood of sceptical resignation reigns in the profession over the very possibility of treating seventeenth-century science as more than a string of loosely connected episodes. I argue that, without returning to a historiographical past definitively behind us, coherence may be restored at a higher level of sophistication. Cross-cultural comparison, and unusual ways of dealing with historical concepts and causes, are proper tools to revitalize the issue and come up with partly novel answers to a question that in any case refuses to go away.

The concept of the Scientific Revolution was coined in the 1930s, as one product of a major historiographical overhaul that took place between the mid-1920s and the early 1950s. It was meant to identify a period in European history that covers roughly the second half of the sixteenth century and almost the full seventeenth century (i.e. between Copernicus and Newton) as marking a uniquely radical, conceptual upheaval out of which modern science emerged essentially as we still know it. This view began to be articulated in the budding field of history of science. It did so in ways that turned the customary listing of one heroic scientific achievement after another into the careful reconstruction of the conceptual knots that those individuals who brought about the Scientific Revolution actually faced and strove to disentangle.

The concept-focused mode of history writing generated a number of path-breaking narratives. These minimally shared a focus on how the once self-evident conception of our Earth, stable at the centre of the cosmos, gave way to the core of the modern world-view, that the Earth together with the other planets

in the solar system was itself a tiny portion of an infinite universe. Historians viewed the 'mathematization of nature' as the major vehicle bringing about this fundamental reversal (along with several other major, closely related accomplishments such as a new conception of motion and the deliberate creation of void space). By this they meant the subjection of increasing ranges of empirical phenomena to mathematical treatment in ways suitable as a rule to experimental testing. Key figures in the process were held to be:

- Copernicus, who first computed down to the required detail planetary trajectories in a Sun-centred setting.
- Kepler, the man first to turn Copernicus' set-up into a previously unthinkable 'celestial physics', leading to his discovery of the planets' elliptical paths.
- Galileo, who first mathematized with success a significant terrestrial phenomenon (falling and projected bodies) in an effort to counter major objections to Copernicus' set-up.
- Descartes, the man to conceive the mathematical way of the universe and of the particle-governed mechanisms at work in it.
- Newton, who capped these developments by uniting terrestrial and celestial physics in his mathematically exact, empirically sustained conception of universal gravitation.

This is not to suggest that these men and their principal accomplishments were taken to stand for all there really is to the Scientific Revolution. Still, for decades historians were inclined to treat other noteworthy attainments of a modern-scientific nature, such as Harvey's discovery of the circulation of the blood or ongoing refinements in chemical testing procedures, as by-products, somehow, of this major development.

The master narrative challenged

Starting in the 1960s, a range of perspectives have been introduced that were meant to widen (or, in some cases, to replace) this 'master narrative'. An earlier generation of historians of science used unreflectively to identify the present-day definition and classification of scientific disciplines with their apparent seventeenth-century counterparts. This has been given up in favour of a still increasing awareness that, for example, what we now call 'mechanics' scarcely had a counterpart in the early seventeenth century, so different, and differently aligned, was the intellectual context in which problems of motion used to be considered from the ancient Greeks onward. Even 'science' as a general expression is on its way out. It carries too many associations far removed from

seventeen-century realities (e.g., a professional identity as a 'scientist' is a feat of the nineteenth century, not earlier).

Further, research subjects and/or people previously left wholly or partly in the margin have come to be included in the narrative. This concerns ranges of (at the time) non-mathematical, chiefly description-prone subjects like magnetism or illness, or subjects neglected in view of their scarcely being practised today anymore, like musical science, and/or held under grave suspicion, like alchemy, but also previously neglected contributors of lesser rank (e.g. hosts of able experimenters among the Jesuits).

Most important of all, a striving to put the history of scientific ideas in institutional and other socio-cultural contexts has become a fixture of much work on the subject. History writing in the vein of 'this major thinker brought about one particular conceptual breakthrough, then that thinker that one' has not come to an end, but historians have learned to recognize that proper understanding of scientific accomplishment requires an awareness of how it was situated in time and place. For example, this is how the dependence of practitioners upon patronage in Europe has been revealed. In addition, an influential argument has been made for a constitutive link between the contested viability of instrument-aided experimentation *per se* as articulated in Boyle's and Hobbes' dispute in the early 1660s over the void, and the politics of the Stuart Restoration. As a result, local particularity has in recent decades been gaining the upper hand over claims for the universal validity of the most seminal outcomes of the Scientific Revolution. One genuine accomplishment of this context-oriented approach is a heightened concern for the day-to-day practice of experimental research and for the trustworthiness of results attained that way. Another has been a heightened sense that there is room for contingency in the story, not everything that happened was bound to happen, or was bound to happen the way it did happen. Historians of science have further become aware that contemporary perceptions of modern-science-in-the-making as innately strange and disturbing were due to much more than sheer backwardness and/or superstition.

Resignation

With respect to the concept of the Scientific Revolution, the net effect of this plurality of mostly productive novel viewpoints has been resignation. Numerous historians of science have in the meantime given up the very idea that, deeply underneath the surface of particular events, something identifiable holds so complex an event as the Scientific Revolution together. And it is certainly true that no one-sided formula of the kind 'Scientific Revolution = mathematization of nature', for all the enlightenment it once provided, can now be accepted. But

is this conclusion tantamount to giving up the quest for underlying coherence altogether?

In everyday practice, it surely is. True, publishers keep inviting authors to produce classroom texts. The up-to-date surveys to come out of such requests are of as great a use to the students taught as they are vital for the health of the profession, but inevitably they obey a format with requirements of its own. That format precludes as a rule a concentrated effort to seek an underlying coherence at the depth of the issues, whether recent or ones once vividly debated and now abandoned.

The reigning atmosphere of sceptical resignation does not stem solely from the new perspectives brought to bear over past decades upon the Scientific Revolution, or solely from despair at the ever more apparent complexity of the event. Resignation stems in perhaps equal measure from the apparent elusiveness of all those big 'why?' questions once raised about the origins of modern science. The seeking after causes, enthusiastically embarked upon in the 1930s, has ended in failure and disillusion and by the 1980s it has gradually petered out. But should it have?

The difficulty with much causal debate at the time rested neither in its liveliness nor in the vital nature of the questions asked, but rather in the peculiar habit acquired by historians of science in attacking them. Explanation almost invariably took shape as a thesis, usually named after the historian to put it forward as *the* one and only, all-encompassing cause of *the* Scientific Revolution. For instance, the 'Duhem thesis' explained the Scientific Revolution out of an alleged, fourteenth-century revolt against Aristotle, whereas the 'Merton thesis' was made by adherents and opponents alike to explain the Scientific Revolution through the contemporary adoption of Puritan values in a capitalist setting. Acute critics found failings in each thesis to clinch the all-encompassing case made for it. Even so, theses in such a vein kept piling up without much exchange taking place over their respective merits. Small wonder that explanatory habits so unsubtle have in the end induced a sense of resignation. Kuhn once proposed to cut such causal theses down to size by restricting their scope to one identifiable portion of the Scientific Revolution rather than to the whole of innovative seventeenth-century science. And in my own overview of the historiography of the event I widened his constructive proposal into a plea for 'judicious combination and cross-fertilization leading to consciously applied transformation' of available conceptions of the Scientific Revolution.[2] But the drift of history writing has gone in another direction giving up the causal quest altogether.

The message given to outside scholarship is that the advent of modern science, a decisive event in world history, really the most outstanding among prime motors of our modern world was in effect due to chance. But, as the great pioneer of cross-culturally comparative history of science, Joseph Needham, remarked in

another context half a century ago, 'to attribute the origin of modern science entirely to chance is to declare the bankruptcy of history as a form of enlightenment of the human mind'.[3] As globalization gains pace, scholars from other places want to understand even more than they already have how it is that modern science arose in Europe rather than in any of the other great civilizations of the past. These scholars have a manifest and rightful interest in being served with accounts of how modern science arose and fared in Europe that seek a coherent pattern and order rather than offering an array of episodes arranged to meet the needs of the classroom.

Nor are historians of science compelled by any necessity to keep their fellow-scholars waiting. A great deal of material, as well as numerous partial interpretations of penetrating profundity, is ready at hand to seek solutions to the big questions involved. No more is required for bringing these materials and these interpretations to bear upon them than a determination to shake off the reigning sense of resignation, and to rethink from scratch ways and means to go about such a quest. No return to the ways of the forefathers is desired or even possible, no monolithic answer is going to satisfy us. Instead, we must re-inspect our toolkit and refresh it from the bottom up.

A strategy for historical conceptualization

Not only in the natural but also in most human sciences, such as also in philosophy, is clear-cut conceptualization of the essence. A failure sharply to define one's concepts and carefully to delineate one's theories and hypotheses causes the investigation to go astray. Historians, instead, tend to avoid clear-cut conceptualization. We seem to be happiest keeping our concepts a bit fuzzy. Many academics, philosophers and sociologists in particular, uncharitably ascribe what they regard as our mindless fact-grinding to a congenital lack of capability for abstract thought ('history is sociology with the brains left out'). Still, there are some good reasons for the apparently lax habit. The prime commodity we are uniquely dealing in is change over time. But change over time cannot be captured well by means of fixed concepts imposed upon a subject in flux. If the concepts selected to handle it are just modern ones projected back upon the past, the investigation tends to end in accounts written as if history were on the move toward some pre-ordained purpose. If, on the other hand, the concepts selected are confined to contemporary ones in use with the protagonists themselves (so-called 'actors' categories'), the historian robs himself of the enhanced understanding that flows from his privilege to look beyond the temporal horizon to which his subjects were inevitably confined. Indeed, retaining some fuzziness is the middle way here needed.

Conceptual fuzziness is not a virtue in itself and may easily slip into a multiplicity of unreflective confusing meanings. Insofar as conceptual fuzziness goes beyond the historian's inevitable predicament, it ought to be combated. We ought to make our concepts as clear-cut as our sense of the past, which includes its complexity and the sheer richness and endless variety of human life over time, allows us.

To the extent that historians are sensitive to the issue, the solution often is to borrow a conceptual apparatus wholesale from elsewhere, and use it to inform the historical tale to be told. One major example is how social-constructivist conceptions that in the 1980s emerged from work in the philosophy and the sociology of present-day scientific knowledge were adopted in accounts of certain seventeenth-century episodes. Thus imported, the conceptual apparatus served as a useful searchlight, but for a while it also turned history-of-science writing into a battle scene for ideological contests of questionable relevance to the core business of the historian, which is to make sense of past events in their otherness from, *and* in their likeness to, our present-day concerns.

Rather than importing a conceptual apparatus from the outside, a better approach is to develop it from the inside. Such a procedure allows us to avoid current categories as much as an *a priori* limitation to actors' categories. At the same time the facts of history do not run the risk of being squeezed into concepts which, adopted wholesale from elsewhere, then impose themselves upon the narrative.

In the writing of history pattern must be discerned, not imposed. Wishing neither to press our causal accounts into pre-set conceptual schemata, nor on the rebound to refrain from conceptualization altogether, we might do well to coin our concepts and conceive the historical theories that bind them together as we go along, in ongoing dialogue with the empirical material that we find ourselves handling at every stage. 'Dialogue' is the key word here. This ongoing process of concept formation and theory building cannot be inductive only as if facts could speak for themselves. The process is deductive as well, insofar as the facts to go into the making of these concepts and theories ought to be selected with certain broad conceptions in mind that earlier accounts may alert us to.

Aligning anew the world history of science

It is about time to replace the still customary, Eurocentric account of the history of science, not only because in a globalizing world it is growing more inappropriate by the day, but also because it stands in the way of resolving the question of why modern science emerged in Europe, not elsewhere.

Here is the easy answer to that hoary question. The adventure of science started in the West – namely, in ancient Greece – and (but for a half-way holding action

by the Arabs) the adventure has *remained* of the West, namely, of Europe, where what the Greeks began was destined to come to full fruition. Still, that something is amiss with the West-centred view so readily taken for granted by preceding generations is already suggested by one apparent anomaly it contains: suddenly nature-knowledge in Islamic civilization turns up and then just as suddenly vanishes from the story without leaving a trace. Thus, the West-centred view implies that the entire development from the Greeks up to and including the rise of modern science was of the West, *yet somehow not quite*. This chink in the armour is accompanied by another, the broad picture rests upon an underlying presupposition which is that ancient Greece is where 'the West' took shape. And indeed, values and viewpoints that go back to ancient Greece have gone into the making of European civilization right from its inception in Carolingian times. Yet since the nineteenth century some sort of *identification* with Greece has taken place in European historical thought (especially German and British) that went one step further; the Greeks came to look very much like nineteenth-century Germans or Britons. *Almost*, the Greeks were already us, and the sole task left to Europeans was to take the final step and become *truly* us. Historians of ancient Greece have since broken with this still deeply ingrained picture, and sought to draw a different one in which not so much the extent to which ancient Greece was already like Western Europe, but rather the ways in which it had radical *otherness* were stressed.

However one-sided and overdrawn that new picture of Greece is, it does provide a healthy corrective to a conception of 'the West' that still lingers in historians' everyday thinking, the thinking of historians of science definitely included. The obvious way to overcome it is to re-conceive the issue in a world-historical setting. Two recent books have made efforts to provide such a setting. In *The Geography of Science* (1991) Harold Dorn outlines a world-historical panorama that, in effect, underwrites the standard view. He contrasts a state-run, narrowly utilitarian type of science characteristic of civilizations dependent on techniques of water management and on the organized mass labour force needed to keep it going, with a type of science largely autonomous, curiosity-driven, and unconnected to any state or state-like structure. The latter type marks those rare civilizations that are set in temperate climates with regular and moderate rainfall leading to an even spread of sufficient water; the principal incentive for a power monopoly of a central state is lacking here. Before modern times, Dorn argues, the bureaucracy-dominated type of science set the rule. In this alignment, Greece and Western Europe appear as the sole cases of the latter type, against Egypt, Mesopotamia, China, Islam civilization, India, the empire of the Khmer, and a range of pre-Columbian civilizations.

In *The Rise of Early Modern Science. Islam, China, and the West* (1993), Toby E. Huff tries out another alignment of the world history of science. Here, the science of China, of Islam civilization, and of medieval Europe appear as three,

by and large independent units of comparative analysis, with a view to showing how the indispensable conditions for modern science that first emerged in medieval Europe grew to full maturity in early modern Europe.

Both these accounts miss the profound likeness of nature-knowledge in Islamic civilization to that of Renaissance Europe – in the one as in the other it stemmed from what survived of the Greek corpus, subsequently enriched in quite similar ways. It is therefore indispensable to start with the Greeks. In books dedicated to the Scientific Revolution, Greece provides an unusual point of departure. E.J. Dijksterhuis' *Mechanization of the World Picture* (1950) is the exception; most other books confine themselves to seventeenth-century Europe, or take their point of departure in the European Middle Ages or in 1543 (in view of Copernicus' and Vesalius' revolution-inducing books both appearing in that year). In an alignment that seems to me more productive, Greece is seen to bring a definitely non-modern corpus of nature-knowledge that (unlike its Chinese counterpart) was enabled by certain military events to be revived and enriched three times in succession. This happened in Islamic civilization, in medieval Europe, *and all over again in Renaissance Europe*. In this view, medieval Europe no longer serves as the preparatory stage still customarily ascribed to it, but rather as the exceptional case of the three, in that this time the revival of the Greek corpus was a highly curtailed one and, as such, not only unrepresentative but also doomed to remain locked in its own framework. But the greatest advantage of this particular alignment is that the two other recipients now appear as almost independently engaged in broadly the same activity, to adopt, appropriate, and creatively enrich the Greek corpus. Phrased this way, the two civilizations turn out to have enough in common to make for a fruitful comparison.

Historical comparison

In thus replacing a unidirectional alignment with a series of episodes of comparable structure, we throw open the gates towards a full-scale comparative approach. As 'the motor of historical thinking',[4] comparison is indispensable for coming to grips with the big questions we are dealing with here.

Historians rightly conceive of themselves as the academic guardians of the unique and the unrepeatable. We are almost innately inclined to regard comparative approaches to the past with deep and abiding suspicion. We do so in particular when the comparison is between Western and non-Western civilizations. We do so even more when the comparison is between what did happen – the Scientific Revolution in seventeenth-century Europe – and what did not happen but conceivably might have, a broadly similar Scientific Revolution elsewhere, at another time.

All kinds of objections have been raised against comparative research, particularly of the cross-cultural type. In my own book I have seized on Needham's and others' pioneering work in cross-cultural comparative history of science to ponder such sceptical questions at length. Suffice it here to state my resulting conviction that (for all the traps that the bold pioneer Needham fell into) there are ways to make cross-cultural comparisons produce viable insights – insights, moreover, that can be gained by no other means. Not that any individual's results can be definitive in any way. Not even to a historian of Needham's astonishing breadth is it given to attain a sufficiently deep grasp of so many culturally and linguistically distinct traditions. What we need instead is a plurality of historians to do the comparing; the more, the better. For so much is certain that (as Huff phrased it) '… viewed from a comparative and civilizational point of view, the rise of modern science appears quite different than it does when seen exclusively as an intra-European movement'.[5] Comparison is further indispensable if we wish, not only to describe and analyze events, but in addition to explain them.

Explanatory strategy

One brief textbook excepted (John Henry's *The Scientific Revolution and the Origins of Modern Science* of 1997), there is no full length treatment of the Scientific Revolution in which description and interpretation go intertwined with explanations of what is being described and interpreted. In the few places where attempts at explanation are undertaken at all, this is done by inserting a causal chapter that, inevitably, falls into the trap of seeking to explain the Scientific Revolution whole, as one monolithic unit. Equally inevitably, no cause or causes then appear to match so sizable and complex a series of events, thus contributing to the sense of resignation I have deplored above.

Instead, analysis and explanation may be interwoven in the following manner. If analysis proceeds by way of comparison, this is what yields the proper *explananda*, i.e. those components of the full story that require explanation. As carefully and precisely as we can, we must then seek to match each distinct *explanandum* with its specific explanation or explanations. We must show again and again that the operative causal mechanism invoked to link the *explanandum* to the explanation is causal indeed, not just linked arbitrarily to the *explanandum* but fit to clarify how what did happen could happen.

Here is an example of how to refrain from monolithic theses without missing the light these may shed upon specific events. In *The Printing Press as an Agent of Change* (1979), Elisabeth L. Eisenstein invoked the 'revolution in print' as the catch-all cause of the Scientific Revolution. But is it not analytically more

productive to ask at each stage of the Scientific Revolution and its Renaissance prehistory whether events were such as to require the printing press, or whether these might just as well have taken place in a manuscript culture? Sustained comparison with Islamic civilization provides ready-made empirical material to answer such questions. What, for instance, remains of claims made for the novelty of Copernicus' scrutinizing Ptolemy's printed *Almagest* at leisure in his home town once we realize that, in Baghdad about 700 years earlier, a hand-written copy could be had in exchange for one donkey?[6] On the other hand, comparison also suggests strongly that nothing in a manuscript culture would have made possible the blow-by-blow exchanges which, in the second half of the seventeenth century, enabled experimental scientists to improve upon each other's findings. In short, such comparisons enable the historian to determine with some precision what actually was transformed and how comparatively minor or major the transformation was, as an indispensable preliminary to the causal inquiry.

'Mode of nature-knowledge' as unit of analysis

What unit of analysis is most appropriate to the task of explaining how modern science could come about? Ongoing historization of the past of science has made nearly unfeasible a practice that we were once accustomed to apply without even pausing, to organize our *historical* accounts in accordance with *present-day* disciplines. But Aristotle and Galileo were not two experts in mechanics, whose main difference was that the former had it mostly wrong and the latter had it almost right. Even though moving objects were a significant shared concern, such commonality as a stance in the same discipline is bound to mislead rather than enlighten. Aristotle was a philosopher and, as such, out to grasp the totality of the world, whereas Galileo, as a mathematical scientist, deliberately operated piecemeal. Prior to the nineteenth century, disciplines in their present-day alignment can hardly ever be employed as viable units of analysis. Neither would our ends be served by selecting research subjects, or individuals, or successive time segments for our unit of analysis – to do so would obscure rather than help uncover what underlying coherence the Scientific Revolution has on offer. The same is true of the six 'styles' that Crombie identified in his monumental *Styles of Scientific Thinking in the European Tradition* (1994) as timeless entities amenable neither to transformation nor to mutual interaction, thus missing the very dynamics that have propelled scientific advance from early in the seventeenth century onward.

Instead, a unit of analysis that is sufficiently flexible without turning fuzzy is *modes of nature-knowledge*. By this I mean consistent ranges of approaches to natural phenomena, each distinct for their scope (deliberately partial or comprehensive), for the ways the knowledge was attained (predominantly

empiricist or intellectualist), for the practices that went with them (observational, experimental, instrumental, etc), for ultimate objectives (knowledge sought as an end in itself, or with a view to improving certain practices) and for the interaction (or, for the largest part of history, lack thereof) to occur between one such mode of nature-knowledge and others 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, how knowledge is organized (e.g. wholesale or piecemeal), how it stands oriented in time (practitioners conceiving themselves as working toward an open future, or as reconstructing past perfection, or as personally completing all over again the schema of all possible knowledge). Further, how empirical facts are being handled (in their own right, or for serving some *a priori* schema; if the latter, by way of illustrative confirmation, or for *a posteriori* checking), etc. For instance, much conceptual confusion in the seventeenth century comes from the circumstance that seemingly similar or even overlapping conceptions of motion and force were handled quite differently in different modes of nature-knowledge (not only those of Aristotle and Galileo, but also those of Galileo and Descartes).

These modes of nature-knowledge lend themselves to being treated 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, but were at least potentially subject to being transformed in ways varying from enrichment inside a given framework, to such revolutionary transformations as came in time to mark the Scientific Revolution. If considered in such a vein, it turns out that, around 1600, three distinct modes of nature-knowledge almost simultaneously underwent revolutionary transformation and gave rise, by the early 1660s, to two more, also revolutionary transformations, and out of these two a (for the time being) final one came about by the mid-1680s. It is this assembly of six revolutionary transformations, each with causes of its own and paths of advance of its own, that makes up what there is good reason to keep calling the Scientific Revolution.

The proof of the pudding

So much for possibly viable ways to reconceptualize the Scientific Revolution. The benefit may be more than negligible. To follow present-day historiography and dissolve the process by which pre-modern nature-knowledge gave way to modern science into an array of loosely connected episodes is to render incomprehensible an event of world-historical proportions. An effort to search again for underlying pattern and coherence seems therefore well-worth undertaking. In this essay I have outlined ways and means to go about such a

search; in my book *How Modern Science Came Into the World. A Comparative History* I have sought to bring it to a successful conclusion.

References and Notes

1. This paper is an abridged and adapted version of the Prologue to my forthcoming book *How Modern Science Came Into the World. A Comparative History*. The historiographical points here made refer back to my earlier book (1994) *The Scientific Revolution. A Historiographical Inquiry* (Chicago & London: University of Chicago Press).
2. H. F. Cohen (1994) *The Scientific Revolution. A Historiographical Inquiry* (Chicago & London: University of Chicago Press), p. 9.
3. J. Needham (1969) *The Grand Titration. Science and Society in East and West* (London: Allen & Unwin), p. 216.
4. I owe the phrase to Dick van Lente (private communication).
5. T. E. Huff (1993) *The Rise of Early Modern Science. Islam, China, and the West* (Cambridge University Press), p. 321.
6. I have discussed her thesis in H.F. Cohen (1994) *The Scientific Revolution. A Historiographical Inquiry* (Chicago & London: University of Chicago Press), pp. 357–67, 415.

About the Author

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