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SYMPOSIUM

Enlarging the picture, enlarging the audience: response to my three critics

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'Could they have known? Did they even suspect—the great Revolutionary conflagration that would soon sweep over… much of the western world?'

This, so I argue in the book here variously anatomized by three reviewers, is the very question decisive for whether or not some given historical episode deserves the epithet 'revolution'. 'Could they have known?', that is, were people around, or could there realistically speaking have been people around, who suspected (albeit perhaps in broad outline only) the direction that events were soon to take? As Lesley Cormack rightly observes, it is for this reason that I have introduced in my book a non-existent person to whom I attribute full knowledge of the pursuit of nature-knowledge in ancient Greece, in the Islamic world, in medieval and also in Renaissance Europe up to the year 1600 when an equally non-existent Eurocommissioner for Science Policy instructs him to watch all pertinent trends and then to extrapolate those trends so as to sketch the likely future. This trendwatcher's dismal failure at prediction becomes readily apparent when you compare, on the one hand, what given the meanwhile established pattern of rise and decline of nature-knowledge he is bound to predict with, on the other hand, what, in a radical break with that pattern, actually happened in decades to follow at the hands of, notably, Kepler and Galileo, of Beeckman and Descartes, of Bacon, Gilbert, Harvey and van Helmont. It is the trendwatcher's utter failure at prediction that signals the truly revolutionary character of those events (and of several more in their immediate wake). For all that, the passage quoted is *not* about the Scientific Revolution. Rather, it forms the two opening lines of ch. 1 of Timothy Tackett's *The Coming of the Terror in the French Revolution* (Harvard UP, 2015), and the two words that I replaced with an ellipsis are 'France and.' At the heart of that instructive book is a careful examination of the correspondence of a range of Frenchmen who became

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active participants of the Revolution. On the basis of letters written by each of these figures prior to 1789, the answer that Tackett soon enough gives to his 'Could they have known?' question comes down to a clear-cut 'no, they could not and they did not'. Before that fateful year Tackett's main characters are dedicated to thinking up ingenious ways to improve the society they live in, yet none of their wishes and proposals comes even close to what no more than some three years later many among them are to think up, to embrace, and to defend with all their might in the name of ideals that literally no one has even conceived of prior to 1789. And so it is with the Scientific Revolution, the sole difference being that unlike Tackett I had to make do with a fictional person to make the exact same point.

The reason why I want to stick to 'the Scientific Revolution' as a viable concept, then, is hardly nostalgia for the 1930s–1950s when Alexandre Koyré coined it. Rather, it is an awareness of the event's utter unpredictability in the face of well-established historical patterns, *plus* a felt need to reconceptualize from the ground up what was radically innovative about it. Yes, the uncomplicated equation 'Scientific Revolution = mathematization of nature' pioneered by Koyré, Dijksterhuis, and Burtt has in its one-sidedness become untenable as our understanding of the period has become wider, subtler, and methodologically more sophisticated. Precisely that is why it has seemed crucial to me to rethink (on the basis of that newer literature without throwing the older literature overboard *in toto*) a more spacious and more sophisticated conception of that truly revolutionary event (for more extensive discussion of this point I refer readers to "The 'Mathematization of Nature': The Making of a Concept, and How It Has Fared in Later Years" (in Remmert et al. 2016, 143–160).

My effort at reconceptualization, as my critics quite rightly note, has gone along with an effort to explain the origin of recognizably modern science in Europe rather than elsewhere. Not any allegedly Western, inherent superiority, let alone (*pace* William Eamon) some teleological unfolding of a pre-set destination is the primary explanation that I have come up with. Babak Ashrafi and Lesley Cormack have seen quite well that my primary explanation is, instead, the presence or absence of a 'latent developmental potential' furthering in new cultural topsoil chances for enrichment or even, just possibly, for revolutionary transformation. Several specific features of European civilization around 1600 that I adduce are meant to assist my primary explanation in the subsidiary guise of *secondary* causes. They are meant to show how what, if no large-scale invasions had intervened, *might* have happened earlier and elsewhere (notably in the Islamic world), *could* and indeed *did* happen at the particular time and place when and where it did. I go on from there to show that even in Europe it was touch and go whether the readily apparent strangeness and sacrilege that came with newly realist-mathematical science and with a mostly novel natural philosophy of moving particles would or would not doom the entire enterprise after all, by causing it to lose momentum first (as it did in fact in the 1640 s and 1650 s), and in the end to run aground in a manner quite similar to how (in a persistent pattern) decline had marked every previous resurgence of the Greek corpus in new or partially new surroundings.

It is at this point of my core explanation through latent developmental potential unfolding or not in thus far untried environments that Ashrafi has raised some very pertinent and intriguing questions.

His first question about the apparent inflexibility of coherent bodies of nature-knowledge—as compared to the transmission of material and cultural products—is intriguing, demonstrating the mind of a critic who relentlessly pursues the author's reasoning beyond anything the author himself has ever considered. Thank you so much, Babak! I wish I had a ready-made answer to your question, but I don't, and I promise that I shall keep thinking about it.

Then, there is the key concept of 'latent developmental potential' itself about which both Ashrafi and Eamon have things to say. To the latter, the concept with its inevitably Aristotelian overtones points at my 'Whiggish' inclination to see the past as working its way toward a pre-set goal actualized in our present. But to call a concept that, if applied to nature, is *ipso facto* teleological 'teleological' as well if applied to history, just replaces conceptual analysis with reasoning by association. Ashrafi makes a more valid and more interesting point when he asks how we may establish whether or not it makes sense in a given case to speak of a latent developmental potential. Not only can this be done by hindsight only (as with the water clock vs. mechanical clock example), but even then it inevitably involves some measure of speculation. At this point, Ashrafi suggests that the limits set to speculation rest in our imagination only. I agree, but only so far. Key to the kind of speculation I engage in for purposes of comparison at various points in my argument is the idea that in history there are three kinds of events. There is, of course, the primary dichotomy between those that happened and those that did not. Too many historians believe that, for viable history writing, we are necessarily confined to the former category only. But that is to overlook that there are two distinct possible reasons for why a certain event did not happen—because it could not possibly have happened, or because it might realistically have happened but did not. It is precisely the latter category that makes historical comparison (really the historian's own counterpart to experiment in science) possible. Please consider here that to make historical comparisons is what, in our everyday lives, we do all the time. 'If only I had done this, or not done that, would such or such have ended better' is precisely the kind of consideration that keeps our minds occupied for large stretches of our conscious life. How, then, could our everyday speculation about actions we failed to take but feel that we should have taken be wholly irrelevant to the larger events that, as a rule, we seek to subject to some form of historical understanding?

On pp. 463–6 of my historiographical book I have touched in passing on another excellent question posed by Ashrafi. I did so when addressing Joseph Needham's speculation about a Scientific Revolution unfolding, not as in Europe along a Newtonian route but in China along a more 'organic' pathway. As I was then, I am non-committal still, yet surely with a strongly skeptical undertone—although there seem to be no viable grounds for rejecting it a priori, there is also little historical material to recommend it. But Ashrafi is certainly right that there is every reason for historians of nature-knowledge in China or in the Islamic world to pursue their subjects without the question of 'why no Scientific Revolution?' looming over them all the time.

Ashrafi, and Cormack as well, raise another interesting and important point. What about those European peculiarities that I adduce as secondary causes? There is, of course, a huge body of literature on the topic of what, if anything, was so special about Europe, that late-comer among the world's civilizations. I have sought to deal with that question (albeit with unsatisfactory brevity) in a section 'Why Europe?' of the third chapter; it also comes up, still too briefly, in the second chapter of *How Modern Science Came Into the World*. My work on a new book (right now in process of being translated under the title *Troubling Knowledge*) has given me occasion to return to how Max Weber has dealt with the question in his comparative study 'The Economic Ethics of the World Religions'. For all that has become obsolete in his treatment, I still think that his far-flung investigation of precisely this question (far wider than just the so-called Weber thesis) is still the best source of inspiration that we have. But Ashrafi is also, more specifically, concerned about my Europe /Islamic world comparison in connection with the problem of decline. It is certainly true that I have elaborated the European horn of the comparison in more detail than its 'Islamic' counterpart. There are two grounds for this. One is that my invoking the effect of the tenth/eleventh century wave of invasions in the Islamic world is meant to explain, not decline as such (decline rather being part of a 'natural' pattern), but only why decline happened when it did. The other is that I wish historians of nature-knowledge in the Islamic world were more interested in large questions of this kind—for the effect of the invasions on the pursuit of nature-knowledge I had to rely solely on one article by J.J. Saunders (discussed in my historiographical book on pp. 405–9).

All in all, readers of *Metascience* who want to find out what is in my book would do well to consult the reviews by Ashrafi and Cormack, whereas I refer those who want to see listed what is *not* in it to Eamon's review. He attributes numerous views and approaches to me which are either crass caricatures of my real views or even wholly foreign to me. Not only does he list many people and topics that no one reading a book that ends its full treatment with Newton's *Opticks* can reasonably expect to find there. But he also manages somehow to overlook the co-constitutive role allotted in the book to empiricist science ('natural history') in the widest sense and on equal footing with mathematical science and with speculative natural philosophy. What else are substantial passages about Leonardo da Vinci, Vesalius, Paracelsus, Castro, Gilbert, Harvey, van Helmont, Boyle, Hooke *e tutti quanti* doing in a book allegedly confining itself to seventeenth-century changes in cosmology?

Take further Eamon's opening passage. It is easy to see what has inspired it. In the book's 6-page 'Introduction: the Old World and the New' I seek to draw the reader (not a professional historian of science, to be sure, but any academic with an interest in the key questions history has on offer) into the main story-line in the most attractive way I could think of while still remaining within the bounds set to a responsible historian. That is what making professional writing accessible to a wider audience minimally requires. To make a caricature of an introduction that seeks to remain just barely on the safe side of caricature itself is to miss entirely the difference between a book meant for the professional (like my *How Modern Science Came Into the World*) and a book meant for a broad academic audience (like the present, shorter version thereof). So I would have expected a critic, before attributing evident absurdities to me, to check in my larger book whether I really hold the views he has derived from a pretty careless reading of its smaller, deliberately popularizing and schematizing (as Cormack has perceptively noted) counterpart.

It would become tedious for the reader of *Metascience* if I were to seek to refute Eamon's numerous specific allegations point by point, particularly because to make the refutation clinching requires going into some detail. So let me confine myself to just a few telling examples.

My subject, as both Cormack and Ashrafi have seen well, is six closely connected revolutionary transformations that in their increasing intertwinement span the seventeenth century. Only in the 'Epilogue' do I bring together in the briefest of ways some characteristics of what I follow Kuhn and others in calling the Second Scientific Revolution. Also in the 'Epilogue' I point out in what decisive manner modern scientific knowledge of the existence of void space, of air pressure, and of the behavior of steam enabled the subsequent construction of steam engines, while making it crystal-clear that that frequently ignored narrative does not even come close to even half the 'explanation' of how the Industrial Revolution and, with it, basic features of our modern world came about. I state there in so many words:

> On the one hand, there is the emergence in 18th century Britain of the first viable pieces of science-based technology, conceptually prepared during the Scientific Revolution and actually created by engineers of a new type such as (to confine ourselves to the most famous and most significant) Harrison, Newcomen, and Watt. There is, on the other hand, a need to invest in these inventions and to market them—invention, after all, needs capital investment and sales efforts to be turned into true innovation. And indeed, economic historians have gone to great lengths to explain how it is that, by the second half of the 18th century, the state of the British economy had become such as to yield entrepreneurs both capable and willing to invest on a large scale in new machinery… The big question, then, is to explain how the mutually so different outcomes of two seemingly unrelated, long-term chains of events should coincide both in time (second half of the 18th century) and place (Great Britain). How is it that this world-historically unprecedented business climate… came about at the very moment when this new, potentially world-shaking equipment could be and actually was invented for the first time? *Whence, in short, the extraordinary confluence*? Without the Scientific Revolution, no Industrial Revolution, certainly; but also, no Industrial Revolution without a business climate like the one just sketched… Only much closer cooperation between economic historians and historians of science and technology can bring that enigma to a satisfactory resolution.

One minor allegation by Eamon is about the Jesuits. The 'mishmash' I attribute to them concerns solely, and quite explicitly so, the incoherent blend of assorted Aristotelian tenets, particle explanations, magical correspondences, and experimentally gained insights that for many decades around 1650 represented the centrally prescribed *worldview* of the Society of Jesus. That, and that alone, is what my curt qualification 'mishmash' stands for. How else could I have counted, on p. 216, the experimental work on electrical repulsion by Father Lana Terzi SJ among the peak achievements of 'fact-finding experimental' research in the second half of the seventeenth century?

And yet there is one thing that Eamon has diagnosed very well. It is the very thing that has caused his own review to go so curiously awry: 'We've tilted too far toward microhistory, and lost our nerve.' Indeed, if considered from a microhistorical point of view *alone* my book is not even wrong, it is just misconceived from the ground up. In the larger book from which it derives I have sought to select the best specialist literature on the numerous individual topics I was out to discuss, with a view to making what had naturally been written in a micro- or meso-historical vein serve my macro-historical purposes. Why did I do that? At least in part for a reason that Eamon expresses equally well: 'Thinking big, bold comparative ideas may be just the medicine our discipline needs.' Exactly! To me it seems that, in a fully healthy discipline, authors of research articles or monographs routinely make an effort to find out how their chosen, necessarily somewhat restricted topic stands related to the grand scheme of things. 'Does it fit in?' they would in my view be well-advised to ask themselves. 'If so, how? Or is it a misfit? If so, why? Maybe because on this point the current grand scheme of things has it wrong and deserves correction?' And so on. But is this how historians of science have routinely proceeded over, let us say, the last thirty or forty years?

Eamon brings up my current editorship of *Isis*. Let me take my own macro-work out of what I have now to say on the matter, and consider only another, even more ambitious macro-attempt to treat a big chunk of the history of science in a way meant to break new scholarly ground. This is Stephen Gaukroger's still incompleted, multi-volume effort to find out how science, utterly marginal in the world of medieval Europe, has become so central to our culture and our lives nowadays. In a fully healthy discipline I would have found the author of just about every manuscript that since I became *Isis*' editor has come my way busily seeking to find out whether and, if so, in what manner its approach and its conclusions fit in with what Gaukroger has to say about his or her chosen topic. That is how political historians do it, that is how economic historians do it, but that is not how historians of science do it—over some three hundred manuscripts meanwhile received (and let it be clear that I have found much to admire in those that it has been my privilege to select for publication in *Isis*) I do not remember even one bringing up the question of what Gaukroger has to say about its subject matter. Surely the authors of serious manuscripts submitted to *Isis* have taken ample care to embed their subject matter in the latest (not quite so often the earlier) historiography, but it is historiography of the micro- and/or the meso-variety only—the grand scheme of things appears to be absent from the authors' considerations. It is, however, precisely a plurality of grand schemes of things, not the still fashionable, post-modernist pooh-poohing thereof, that the history of science as a discipline is badly in need of.

It is so for two distinct reasons. One, as I have just argued, is the health of our discipline *qua* discipline, in that it should relearn to think about the big picture and how comparative approaches may help turn those big pictures into historically responsible efforts. The other is the health of our discipline in the view of others. The history of science has stories to tell that many people, once made aware of them, deeply care about. Most often we leave writing those stories to outsiders, to science journalists for instance, while standing ready in our learned journals to tell our fellow-insiders all about their surely numerous inaccuracies. What, instead, I have attempted to do with *The Rise of Modern Science Explained* is to write, not from the outside but from the inside where my larger book on the same subject situates me, a hopefully compelling story aimed at a larger audience. Indeed, historians of science are not the primary audience of *The Rise of Modern Science Explained*—the larger book is, resting as it does in its turn on a previous, large-scale survey of the historiography of the Scientific Revolution.

Eamon further suggests that I make the twelve men whom I treat as the protagonists of the revolution bring it about single-handedly. Why, so he complains at some length, does Cohen neglect that in recent decades artisans, travelers, merchants, women, etc., etc. have been restored to their proper place in the full story of the history of science?

In the first place, I do not neglect this. For instance, I have dedicated substantial passages to finding out whether, and if so to what extent, a large variety of craft practices impinged on revolutionary developments in budding science (doing so by way of a brief summary of a systematic, dozens of pages long survey made in my larger book).

In the second place, my book is *not* a history of seventeenth-century science, but of something smaller because it is more pointed. It is an effort to demonstrate what was truly revolutionary about the period. To that end, an author has to concentrate on those who did revolutionary things, be it in theory or in practice or (as in most cases) both. It is trivially true that each innovator was steeped in learning and practice picked up in his immediate or wider environment. Without such a background, plus a good deal of interaction with numerous others inside and outside their own environment, no innovation is at all possible. Many a contributor to the current wave of microhistory has been admirably out to fill in all kinds of often important details about backgrounds of that very kind. But all these local details should not make us overlook the at times very big, very bold conceptual steps by means of which the great discoverers managed along some crooked pathway to arrive at their conceptions and their discoveries, and to novel ways and means to find out whether these conceptions and discoveries could survive a first reality check. My concentrating (certainly not exclusively so, but in the main) on the most radical innovators is *not* a sign of old-fashioned, Romantic hero worship. If it were, I would not have taken the trouble systematically and for each protagonist to compare the *status questionis* on, e.g., the nature of local motion prior to, and by the end of, Galileo's effort at radical innovation; ditto for the state of thinking about magnetic attraction before and right after Gilbert, and so on for each protagonist.

Speaking of which, a word now about my alleged patriotism as it reveals itself, so it has occurred to Eamon, in my allegedly appointing Christiaan Huygens the true hero of the Scientific Revolution. What little is true in this extraordinary comment is that, born and living on the European Continent as I do, I have always found our discipline overdoing its AngloSaxon orientation a little. I have, for instance, found with great astonishment how Huygens, rightly acknowledged by Richard S. Westfall on p. 240 of his Newton biography as the 'recognized leader of European science' in the third quarter of the 17th century, has been neglected in the large majority of overviews of the Scientific Revolution (look up, for one example among many, what little Steven Shapin has to say about Huygens on pages 11 and 147 of his *The Scientific Revolution*). The greatest Huygens expert now living, Joella Yoder, has made it clear in her writings that she feels quite the same. And sure enough, even as a Dutchman alone I *am* sensitive to Huygens' curious near-absence in too many accounts where, due solely to the significance of his achievement, he should have found an obvious place.

Even so, the idea that Huygens is my principal hero, and the added suggestion that patriotism is at work here, is… well, let us kindly call it remarkable. I distinguish in my book twelve main persons central to the bringing about of, altogether, six 'revolutionary transformations'—Ashrafi carefully lists the dozen. With one German, one Italian, one Frenchman, one Belgian, two Dutchmen, and six British I do not quite see what, or whose, patriotism has been steering my account.

At various occasions I have suggested that historians of science could do worse than to learn to practice what I have called 'responsible heroism'. As we all know, people love to read about people, more in particular about special people, admirable people. People also like to read about science, as well as about history. We, historians of science, deal with all of this: with people; with special, often even admirable people; with science, and with its history—why, then, are we so habitually failing those numerous potential readers of ours who do harbor these quite legitimate needs? If we do not at least once in our career write up our results in a way accessible beyond the profession, then others do it for us. And it is this consideration, in particular, that years ago moved me to sum up the somewhat involved argument of *How Modern Science Came into the World* in an almost three times as short, more widely accessible work that in naturally more schematic fashion presents the exact same argument. In the original Dutch it quickly sold 12,000 copies; what the English sales numbers will in due time turn out to be, I cannot say. But even apart from sheer sales, I have used the original Dutch text in class for many years, in ongoing comparison to be sure with other scholarly books on the same subject, and I can testify that, with proper teacher's guidance, the book seems to lend itself well to students' understanding. Again, whether it would survive class treatment in the English-speaking world I cannot say either, yet I take heart from the fact that, in her review, Cormack appears to think so.

I thank the *Metascience* co-editors for the opportunity to ponder the three reviewers' highly varied comments, all of them instructive in some way, and to respond to them in public. I also thank the reviewers themselves for their willingness to give to my work so much generous time and dedicated thought.

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