

[../toc](#)

Muse

Search

Journals

This Journal

Contents

---

## The Scientific Revolution Is Undead<sup>\*</sup>

[Mario Biagioli](#)

---

The changing fortunes of the Scientific Revolution and its historiographic narratives have usually been connected to *macroscopic dynamics external* to the discipline of history of science. For instance, the warm reception in the fifties and sixties of Koyré-style intellectual history that privileged metaphysics over social context has been linked to Cold War era concerns with maintaining a safe separation between science and its socioeconomic dimensions. On the other hand, the emphasis on the role of artisanal practices (and of the economic interests behind them) originated from a historiography of the Scientific Revolution informed by left-wing politics or even straightforward Marxist commitments. Similarly, skepticism about grand, logophallogocentric, or simply progressivist narratives about early modern science may be seen as an expression of more recent sociocultural climates. Arguably, postcolonial, multicultural, and gender-aware sensibilities may have informed the reevaluation of the role of women in early modern science, the critique of the Scientific Revolution as a myth of origin of Western modernity, and the shift in focus from theory to practice or from universal to local notions of knowledge.

I do not wish to criticize these macroscopic contextualizations of the Scientific Revolution as a historiographic category, but rather to complement them with a more internal and microscopic analysis of **[End Page 141]** our professional and institutional practices as historians of early modern science. Having witnessed an analytical shift from the theory to the practice of science, it may be time to apply the same perspective to the history of science itself. Accordingly, we may treat realist or nominalist stances about the Scientific Revolution not only as methodological and theoretical issues, but also as expressions of our mundane practices as an academic tribe. I propose to look not only at what we *think* about the "Scientific Revolution" but also at how we *use* it in constituting our disciplinary identity as historians of science in the context of increasingly professionalized academic environments, protocols of scholarly credit, and

teaching responsibilities.

### **Tenure and the Mundane Demise of Grand Narratives**

The style and broad scope of books like Alexandre Koyré's *From the Closed World to the Infinite Universe* are rarely encountered in today's scholarly works. One could argue that this is because of all the bad things we and our students have heard or thought about the role of grand narratives and about historiographies of science driven by metaphysical commitments. But, more simply, one could also say that, despite its brilliance, *From the Closed World to the Infinite Universe* is a book that would not be likely to win tenure for its author in a modern university department. By today's standards, its footnote apparatus is on the light side, its scope too broad, and its generalizations a bit too adventurous.

To continue this thought experiment, a picky modern reviewer would be likely to see Koyré's "A Documentary History of the Problem of Fall" as a proper scholarly text, but might raise some doubts about, say, "Galileo and Plato." Still revered as quasi-sacred, Koyré's works are no longer presented as exemplars for young professional historians to imitate. The bureaucratic, academic standards that have evolved alongside the professionalization of our discipline have made Koyré look outdated, not so much because of the specific content of his interpretations, but because of his grand historiographic style--a style we now associate with textbooks and more popular works.

What has changed is our "form of life," not just our interpretive dispositions. Similarly, given that most professional historians of science do not dwell obsessively on issues of narratology or methodology, the emergence of more-circumscribed studies as the *modus operandi* of our discipline may not be the necessary result of the theoretical reframing of knowledge as local, contingent, contested, and so forth. Local studies of early modern science are now the predominant genre of our field, and they are not produced just by those **[End Page 142]** who fell in love with Clifford Geertz or SSK in graduate school. More mundanely, history of science has developed its academic niche (often within empirically minded departments like history), and its scholarly style reflects the usual trend of professionalization and specialization that we find in other "mature" fields. Grand historiographic narratives are more likely to have been felled by the ax of tenure committees than by the fine analytical scalpels of poststructuralist theoreticians.

### **New Knowledge and New Constituencies**

Of course, professional standards are not the only thing that has changed since Koyré. Historically, the Scientific Revolution used to be exemplified by the physical sciences and particularly by the "Copernican Revolution."

But today we know much more about previously less-studied practices (electricity, the "occult" sciences, medicine, natural history, cartography, engineering, etc.) and we have a much thicker picture of the social systems of early modern science and of their sites. As a result, it would be very hard to fit all this newly available knowledge into a grand narrative, even if we wished to do so. Simultaneously, the history of early modern science is no longer the exclusive domain of white men primarily interested in the work of European male scientists. Many of the current historians of early modern science are women, and the study of non-European traditions is also picking up new momentum.

I would argue, then, that unifying pictures of the Scientific Revolution have not only been penalized by modern protocols of academic credit, they have also been quietly undone by the quantitatively and qualitatively new professional makeup of our field. That little noticeable fracas has accompanied the demise of grand narratives about the Scientific Revolution strengthens my claim that changing academic demographics--not theoretical and narratological debates--have been the prime mover. What we find, again, is that the Scientific Revolution has not been "deconstructed" conceptually, but has fallen by the wayside, largely ignored by a new generation of historians busy at analyzing specific and previously unstudied aspects, actors, and actresses of early modern science.

I would add that, by and large, the very question of the revolutionary character of early modern science is no longer a lively research topic. Not only have we learned how messy the demarcation between the so-called ancients and moderns can be, but, more generally, the definition of "revolution" has been vastly complicated by the fact that we no longer think of early modern science only in terms of its theoretical dimensions (and this, again, is the result not [End Page 143] just of a methodological shift but of our new empirical understanding of the period). In short, I am not sure we could tell a Scientific Revolution if we saw one, and even if we did, we could hardly represent it in the historiographic genres we have adopted as professional historians of science.

But if the Scientific Revolution has become a ghost even before theoretical critiques could be effectively aimed at it, large unifying historiographic categories still play a crucial role elsewhere in our academic ecologies. There are institutional practices (such as undergraduate teaching) that seem to keep the "Scientific Revolution" going, if only as an "undead" category.

### **Course Titles and "Reality Effects"**

The teaching of the Scientific Revolution has been the topic of a few well-attended sessions at History of Science Society meetings. On these

occasions, questions about the definition of the "Scientific Revolution" were usually sidelined in favor of more pragmatic discussions about the contents and structure of the syllabi of undergraduate lecture courses. I assumed at the time that if my colleagues were discussing how to teach courses bearing "Scientific Revolution" in their titles, they believed that some version of that category (however amended, expanded, or contracted) was still historiographically relevant. With hindsight, however, I think I was wrong in assuming that such a discussion reflected implicit realist positions.

In fact, the syllabi and reading lists of lecture courses do not necessarily present and try to canonize *specific* interpretations of the Scientific Revolution, but rather create a "reality effect" for that category *in general*. What is being canonized in undergraduate teaching is the "box," not its contents. We end up with a category that looks quite real (in the sense that it appears in university course catalogs, students' transcripts, grade rosters, etc.), and yet it is almost empty because the "Scientific Revolution," like a capacious empty container, can be filled by many different narratives--narratives that do not necessarily offer "exemplars" of the Scientific Revolution, but often question its revolutionary character or even its existence. I would say that it is precisely the "emptiness" of the Scientific Revolution--its status as mere container or course title--that allows for its institutional stability. It is by having become a ghost that it has gained permanence--the permanence of immaterial things.

Therefore, the question of whether we are realists or nominalists about the "Scientific Revolution" is largely irrelevant to our professional practices. What is more interesting is that the category of the Scientific Revolution works well within our institutional ecologies [End Page 144] precisely because it can be treated as "dead" or "alive" depending on the professional context at hand. The clear distinction between the domains of undergraduate teaching and research allows us not to ask probing questions about its epistemological status. Perhaps undergraduates think we really believe there was such a thing as the Scientific Revolution, but colleagues from our own field tend not to pose that question because it is usually irrelevant to discussions of the disciplinarily or geographically circumscribed research topics that we pursue today.

In sum, it is not that the Scientific Revolution is dead and that it shows fictitious signs of life only as a course title. Rather, it really does not matter whether it is dead or alive insofar as we can avoid that question and *treat* it as dead *or* alive depending on the circumstances. In some ways, such an indeterminacy allows us to have our cake and eat it too.

### **Academic Cakes and Myths of Origins**

Today it is easy to say that the Scientific Revolution is one of modernity's

myths of origin. Like all truisms, such a statement does not have much of a bite, and, in any case, it applies only to very broad and stereotypical notions of the Scientific Revolution--notions that are hardly reflected in current scholarship. But it may be less trivial to say that the Scientific Revolution is not so much a myth of the origin of that grand entity called modernity, but rather the origin of something much more local and tangible: the history of science as an academic discipline.

Studies of the Scientific Revolution and of the "fathers" of modern science have been crucial for establishing the relevance of history of science and for developing its niche in modern academia. The "Scientific Revolution" has cast early modern science not just as another period of scientific activity, but as one in which science "really" changed and assumed its "modern" features. In turn, such a characterization helped legitimize the Scientific Revolution as a period that "needed" to be studied.

As we know, periodizations are not just historiographic conventions, but carry academic market value. In particular, revolutions (cast as "crucial" events ushering new eras into existence) tend to be seen as deserving special attention. Ultimately, such a historical "value system" helps justify the allocation of teaching positions. It is much easier to convince a chair or a dean to come up with a position in the French Revolution, the Scientific Revolution, the Reformation, or the Darwinian Revolution than to hire someone to teach the late medieval period, the seventeenth century, or early modern [End Page 145] Balkan history (though one could easily argue that there would be plenty of interesting topics, events, and historical processes to be studied in any of those periods and places). Whether we like it or not, cultural capital is not evenly distributed among historical events and actors (or those who study them), and the construction of what counts as "interesting" and "relevant" (be it a topic, a person, or an entire academic field) could be an easy subject for a Bourdieu-style sociology of "academic distinction."

As an "undead" category, the Scientific Revolution works well in this economy of academic cultural capital. It gives us historians of early modern science a recognizable disciplinary identity and a sustainable academic niche within history departments, without forcing us to believe in the label we use to designate our research field. Moreover, it is such a well-established myth of origin that we can get academic credit even by criticizing it (or by organizing conferences and editing volumes such as this one). If we don't overdo it, we can safely bite the hand that feeds us.

### **Micro or Macro? Grand Narratives in Local Disguise**

I conclude by returning to the alleged demise of grand narratives in the history of early modern science. For simplicity's sake, I will use SSK-informed histories as examples of local analyses (though my argument

also applies to other kinds of microhistories, including my own work).

We have clearly learned a great deal from historical case studies broadly informed by the sociological models of Harry Collins and his colleagues. The question here is not about the heuristic value of these methodologies, or the scholarly quality of the historiography they have inspired, but about whether these narratives are really as local and microscopic as they seem. At one level, the answer is certainly "yes." The controversies between Boyle and Hobbes on the vacuum, or between Hooke and Huygens on the spring watch, are indeed local and, more importantly, their analyses can be used to make strong claims about the local character of experimental knowledge. As such, these works appear to be the antithesis of "grand narratives." However, a different picture emerges when we consider the *kind of plot* that structures these narratives.

The story line, in fact, is remarkably stable across space and time. It reiterates that the closure of a debate involving experimental evidence is inherently problematized by the so-called experimenter's regressus. Experimental claims are necessarily unstable, and can be made temporarily stable through intricate social negotiations and the mobilization of a range of different resources. These negotiations **[End Page 146]** and the resources they draw on are inherently local, and so is the knowledge they produce. As a result, knowledge cannot be transferred unproblematically from one site to another, but can be made to travel under certain circumstances, with appropriate "technologies," et cetera, et cetera.

In my view, SSK-informed histories of science do not really provide local narratives about scientific practices for the simple reason that their sociological framework is, in fact, a grand narrative about the locality of knowledge. Therefore, histories that draw on this general interpretive model deploy the same grand narrative, but dress it in different local clothes. Because these studies articulate their general model through local evidence, and because, as historians, our reading protocols privilege the empirical specificity of the story over the generality of its plot, these works end up looking (to us) very much like local histories. It is our "form of life" as historians of science that creates the "locality effect" of these narratives.

Things change as soon as we cross disciplinary boundaries. Sociologists of science, for instance, may read and use SSK-informed histories of early modern science not as microhistories but as examples of the broad (i.e., "grand") applicability of their model (thereby implicitly denying the very historical specificity that constitutes them as "good" histories of science). And a clever reader like Bruno Latour can treat *Leviathan and the Air Pump* neither as a local study of a seventeenth-century experimental philosophy, nor as an exemplar of SSK in (historical) action, but as a

poster child of his "amodern" anthropology of knowledge.

In sum, grand narratives about early modern science have not disappeared but have been locally constructed as local interpretations. And local narratives are not inherently local, either. Although I have limited my case to SSK-informed historiography, similar conclusions could be drawn from a range of "microhistories" that present themselves as local while gesturing (more or less explicitly) to their possible connections to grander narratives that may carry cultural capital in other neighboring academic fields. These connections can be expressed or suppressed depending on the disciplinary setting in which a work is presented and read. Then, what counts as "micro," "macro," "local," "mid-level," or "general" is not a matter of essential definitions, but is constituted by the habitus of a specific field and by the ways in which it construes cultural capital. As with the "Scientific Revolution," grand narratives are neither dead nor alive, but simply undead.

*Harvard University*

[Mario Biagioli](#) is Professor of the History of Science at Harvard University, and the author of *Galileo, Courtier* (Chicago, 1993). With support from the John Simon Guggenheim Foundation, he is working on a book concerning the author-function in contemporary science. His most recent publication is "The Instability of Authorship," *The FASEB Journal* (1998).

\* This essay is a reworking of my introductory remarks at the conference. In revising them, I have maintained their original discursive style, and have avoided footnotes.

[Muse](#) [Search](#) [Journals](#) [This Journal](#) [Contents](#) [Top](#) |