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Response to Comments

Response to Comments on "Science's Imagined Pasts"

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Abstract: The four generous responses to "Science's Imagined Pasts" here commented on fruitfully extend its argument. In particular, all of them resonate with—even though only one of them explicitly addresses—its concluding theme of the "second scientific revolution."

With the aid of these thoughtful and wide-ranging responses, I can see that each of the three claims put forward in "Science's Imagined Pasts" either fell into (the first two), or invoked (the third), a distinct domain. What makes that visible is the fact that different respondents have focused on different domains, albeit with some overlap.

The first claim concerned *the microprocesses of the sciences themselves*: the argument here was that the construction of a past was an intrinsic and distinctive aspect of the sciences, since "science does this to a peculiar degree." The essay did instance the literature review as a key example of how this happens, but for the most part it evidenced this claim only indirectly—for instance, via Bruno Latour's asymmetrical Janus-face depiction of science. Thus the essay was invoking, but not exploring, the ensemble of activities that go to make up the sciences. This domain is discussed by Alexander Blum and by William Thomas.

The second claim pertained to the past *historiography of science*. "The theme of science's imagined pasts," I argued, "seems never to come into sustained focus": the essay suggested that historians of science have repeatedly touched on that theme yet have never succeeded in systematically addressing it. That argument was illustrated in various ways, such as (to remain with the same example) the fact that Latour's Janus picture has received no scholarly discussion. This domain has been explored by William Thomas (again) and by Massimiliano Badino.

The third claim again referred to the historiography of science, but this time in the future: "the creation and roles of such imagined scientific pasts," the essay proposed, "will richly repay closer and wider study." While various research possibilities in this area were sketched in an in-principle way, what stood out—and served as the punch line of the essay—was a single specific example, namely the so-called "second scientific revolution." The discussion of that theme, which rested heavily on papers by Simon Schaffer, Timothy Alborn, and Andrew Cunningham, put together a historical claim (Schaffer, Alborn) and a historiographic one (Cunningham). The historical claim was that nineteenth-century science differed profoundly from its eighteenth-century predecessor natural philosophy; the historiographic claim was that this difference had been suppressed at the outset—for instance, by William Whewell—and remains

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so to this day, obscured by the very term "history of science." That example belonged to a third domain, which might be characterized as *the relationship between science's history and its historiography*. And this is the domain addressed by Kathryn Olesko: as we shall see, it emerges in or from all four commentaries, but hers alone takes this as its central focus.

The natural sequence in which to discuss these responses, then, is Blum–Thomas–Badino–Olesko.

Blum opens with a delicious observation: "Wilson has, somewhat ironically, been led astray by his own construction of an imagined past for the history of science, more specifically by taking Chapter 11 of Thomas S. Kuhn's *Structure of Scientific Revolutions* as a starting point for his discussion." I'm not sure whether in using Kuhn I was constructing an imagined past, but the irony would be so neat that I rather hope I was! In any case, what Blum stresses is that "Kuhn's Chapter 11 is a problematic starting point"—and this "for two reasons." The first of those reasons is Kuhn's focus on textbooks, rather than on literature reviews and citation practices. The second, which arose from Kuhn's specific polemical needs, was a focus on explicit, reflective engagement with history, which is not "integral" to science but occasional—found in contexts (including textbooks) that draw forth "an explicit, self-conscious presentation of a field's history." In contrast, what is far more important (Blum argues) is "the implicit construction of that history through literature review and citation behavior," which, as he rightly remarks, "most scientists do not think of as a historical activity at all." This latter activity he calls the "intrinsic history writing of science."

I suspect that those two anti-Kuhn considerations are in fact one—but the point is powerful and persuasive, and to my mind radically improves the argument of "Science's Imagined Pasts." As Blum goes on to argue, the literature review is specific to science and integral to science, and it is through this, along with citation practices, that the sciences construct their imagined pasts. In expounding this picture, Blum makes a point that I'd like to appropriate for a different purpose. "One may rightly interject," he remarks, "that the literature review as I have invoked it here is a rather recent phenomenon. . . . But I would argue that this hardly invalidates my claim. Indeed, I find it quite plausible that science would establish its own unique, intrinsic form of writing its own history only in the later stages of its development." Blum's proposal here is that the literature review itself has a history—and one that is worth exploring. I agree, and I suggest that we might stretch that theme back in time as yet another aspect of what separated the sciences from natural philosophy.

Further, Blum endorses the third claim of my essay, once again taking this in a direction I had not envisaged: his view is that a proper understanding of scientists' "intrinsic historiography"—his key term of art—will shed new light on the very nature of science. Here he argues that "scientists can actually be quite good at this kind of history, which is an essential part of their work"; and he illustrates this with the story of the Dancoff myth (promoted, it seems, by J. Robert Oppenheimer for his own purposes), which nicely exemplifies scientists (other than Oppenheimer) doing a better job than historians (who were taken in by the myth). Blum concludes by proposing that we investigate just how scientists go about making their histories, emphasizing a focus on "how scientists read their historical sources." With all of this I am in complete agreement.[1]

[1] My agreement includes the emphasis on reconstructing the reading of sources (rather than on analyzing the written product) as what most merits attention in the analysis of historiographic practice (in this case, the historiographic practice of scientists). See below, in my discussion of Badino's essay.

Thomas's essay both begins and ends by exploring this same theme—the characteristic practices of scientists—and what he says on this score fits remarkably well with Blum. I shall look at this part of his argument before turning to its other, historiographic, dimension.

Thomas starts by making a distinction between "what we can call epistemic pasts" and "what we could call mythic pasts." "Epistemic pasts" correspond closely to what Blum calls "intrinsic historiography"; "mythic pasts" resemble (and may perhaps be identical to) Blum's "explicit, self-conscious presentation of a field's history." Although Thomas doesn't have much to say about "epistemic pasts," what he does say is entirely consonant with Blum's picture. Thus it is "through their cited references" that scientists construct their epistemic pasts, which precisely matches Blum's characterization of intrinsic historiography as founded on literature reviews and citations; and as we shall see, his concluding proposals also harmonize with those of Blum.

The main focus of Thomas's discussion is on "mythic pasts," which of course means that his essay and Blum's are precisely complementary to one another. (Blum, it will be recalled, concentrates on epistemic pasts rather than mythic ones, or, in his own terms, on "implicit" rather than "explicit" historiography. Nevertheless, although Blum does not explicitly address the latter theme, what he writes about the Dancoff myth is in line with what Thomas argues on this score.)

The "primary function" of mythic pasts, Thomas explains, "is to forge identity, solidarity, and a sense of purpose"; and he goes on to suggest that there may be tension between mythic and epistemic scientific pasts and that this would be a worthwhile theme for historians to investigate. But the crucial step in his essay is to apply the notion of mythic pasts *to the discipline of history* and, specifically, the history *of science*. This is the core claim: "Interestingly and alarmingly, the primary mythic narrative of the history of science profession seems to be intimately connected with historians' mythic conceptions about the scientists they study. Specifically, historians are apt to regard scientists, and society more generally, as heavily burdened by misconceived and injurious ideas about the nature of science. In turn, historians regard themselves as destined to dispel such ideas through careful, critically informed scholarly research and advocacy." After running through a series of examples—Kuhn, Mulkay, Bloor, Shapin—Thomas comments: "Implicit within this notion that historians were freeing themselves from scientist-derived conceptions of science was the idea that scientists had indeed traditionally—and thus historically—misconceived or dissembled about their work."

This claim is ingenious, and highly so. It is ingenious in the literal sense: that is, it's a product of Thomas's genius—or, in other words, he has made it up. The clue is the giveaway word "implicit," which tells us that what follows is something that hasn't actually been said by any of the authorities just cited. And a glance at the presented examples confirms this. What is quoted from Kuhn is the famous remark about "the image of science by which we are now possessed" (not the image of science by which scientists are possessed); from Mulkay, his critique of Merton's norms (the work of a sociologist, not a scientist); what's said of Bloor is that he "cast prior generations of sociologists as lacking the 'nerve' to apply their analytical methods to the production of scientific knowledge" (nothing here about any illusions among scientists themselves); and of Shapin, that "he suggested that prior history had been built up around idealized conceptions of science meant to buttress its authority" (here the ignoramuses are historians, not scientists).

Conceivably Thomas, or someone else, can find a smoking gun to support his claim, but as it stands there is nothing going for that claim. And it may well be that historians-cum-sociologists of science are swayed by one or more mythic narratives; but if so, it remains to be seen just what those mythic narratives are.

Thomas's *bête noire* is Steven Shapin, and his culminating example is *Leviathan and the Air-Pump*, which he depicts as having set the seal on historians' myth-about-scientists by supplying it with exemplification. Yet, remarkably, he immediately places a wedge between that book's coauthors, for he goes on to extol the early work of Simon Schaffer (including "Scien-

tific Discoveries and the End of Natural Philosophy")—specifically, for demonstrating that eighteenth-century natural philosophers were "talented critics and reformers of their own enterprise." Thomas praises this work highly and at some length, proposing that such explorations should be extended from natural philosophy to science:

> The kind of attention that Schaffer dedicated to the intricacy and sophistication of natural philosophers' thinking is important not only for historians' understanding of what natural philosophy was and how it evolved and ended. There are certainly analogous situations permeating the entire history of the sciences. Historians are used to investigating the links between scientific knowledge, research practices, genre conventions, institutional structures, and cultural priorities. They are, I feel, less attuned to how scientists themselves have thought about, engineered, and re-engineered these links.

And he concludes by suggesting that scientists' imagined pasts are worthy of study as "one of many windows into scientific thinking."

This proposal—which is very much in line with Blum's but extends it in several directions—strikes me as very fertile indeed. I would add that such an exploration of the activities of *scientists*, when juxtaposed against the corresponding activities of *natural philosophers*, bids fair to shed considerable light on the transition from natural philosophy to science, as of course we should expect given that it was precisely Schaffer's "Scientific Discoveries and the End of Natural Philosophy" that, for an Anglophone audience, opened up that theme.

I turn next to Badino's engaging discussion of the historiographic theme. In a nutshell, I find myself welcoming both the question that Badino asks at the beginning and the exploration that he suggests at the end, while disagreeing with the premise that links these.

Badino begins by illustrating "imagined pasts" with a beautiful story—more properly, a story about a story—which, as it happens, could be seen as connected with the "second scientific revolution." Astronomers have come to believe that "the problem of the stability of the solar system was first posed by Isaac Newton in the late seventeenth century and then, after much labor, solved by Pierre Simon Laplace in 1773." Having demolished that picture, Badino sketches how it was put in place—by a surprising yet intelligible collusion between Laplace and (a generation or so later) Whewell. The excellent point that Badino thereby makes is that "the paths through which imagined pasts are established and accepted often make for interesting and revealing stories in their own right." And he goes on to ask, "Why have historians of science paid no heed whatsoever to this and countless similar stories?"

His answer is that were historians to embark upon analysis of "the process of imagining the past," this "would expose flaws they themselves are not immune to." Imagined pasts are "fictions," fictions with "multiple purposes" that all serve present interests and are far removed from the "serene and disinterested contemplation" to which historians are supposed to aspire; exploring the genesis of such fictions would bring to light the troubling fact that history itself is not so different. Drawing on William McNeill's notion of "mythistory," Badino argues that "the past . . . is not an autonomous entity to be unearthed and gazed at"; that "history cannot be neutral"; and that, as a corollary, "historians cannot be 'biased' with respect to an allegedly objective past 'out there.'" And all of this, Badino suggests, historians in general and historians of science in particular are unwilling to face: "Historians of science, I believe, do not deal willingly with science's imagined past for the same reason that historians in general are loath to deal with the fictional nature of history as a scholarly discipline . . .: because it lays bare intrinsic aspects of the very act of history writing that can potentially lead to its debunking as mere politics or rhetoric."

Badino presents this as what he believes, but for the reader it has the status of a hypothesis; and while that hypothesis might be true, it remains a mere hypothesis, since Badino offers no

evidence in support. For myself, I find it unlikely that the overlooking of imagined pasts arises from such timorousness; it seems to me that the theme has not been censored but, rather, has not been brought to mind, though I have no counterhypothesis as to why that should be so.

Further, the hypothesis leads Badino down an argumentative blind alley. He goes on to say that "this fear, albeit understandable, is . . . misplaced," because "even if narrating the past implies, partly and inevitably, constructing it, this does not entail that one cannot distinguish between good and bad fictions." This choice of words ("fictions") suggests that the criterion of historiographic worth has nothing to do with truth; yet the subsequent discussion entirely contradicts this, invoking as it does distinctions such as that between "interpreting and misinterpreting," which has to involve a truth criterion if it is to serve any purpose at all in this context. And of course the historian has to aim at historical truth in order to achieve results such as those that Badino laid out so well at the beginning of his piece, when he uncovered the making of the myth about the solar system stability problem.

Badino concludes, however, with a claim that I endorse: that just as it will be fruitful to explore science's imagined pasts, so also it will be worth investigating the nature of the historical discipline itself. While it is not entirely true that "we are still lacking empirical studies on the practices of history," it is certainly the case that more could be done in this regard, particularly on historical inference (the traditional theme, now long out of fashion) and its relationship to historical writing (the topic that has dominated since about 1980). But that would be, as they say, another story.[2]

So, finally, to Olesko, whose comments center on the "second scientific revolution." Olesko's opening point that imagined pasts "become constitutive of the present" is elegant; and her idea that "scientists' coming to terms with their collective past" resembles the process of "a nation's people coming to terms with its past" is intriguing. I suspect that there are differences, but there are certainly resemblances: famously, Ernest Renan argued in 1882 that "forgetting, and even historical error, are essential factor[s] in the creation of a nation," a form of words close to what Alfred North Whitehead was to claim about the sciences some thirty-four years later.[3] Olesko's main point, however, concerns the shift "from natural philosophy to physics," particularly in Germany and as illuminated by the work of Rudolf Stichweh, published as long ago as 1984: thanks to Stichweh and others, far more is known about that transition than my essay had suggested. The key process was "discipline formation." More specifically, Stichweh argued "that the transition from natural philosophy to physics was a movement away from a relatively unrestricted and unbounded system of communication and unsystematic means of reproduction toward more restricted, bounded, and controlled ones." In this and other respects, Stichweh's picture of German physics is entirely consistent with Schaffer's claims published, quite independently, around the same time.[4] Olesko goes on to demonstrate that, for all its importance,

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[2] Against the claim that we lack "empirical studies on the practices of history" see, e.g., J. H. Hexter, *The History Primer* (New York: Basic, 1971); Leon J. Goldstein, *Historical Knowing* (Austin: Univ. Texas Press, 1976); Albert Cook, *History/Writing* (Cambridge: Cambridge Univ. Press, 1988); and Anne Rigney, *The Rhetoric of Historical Representation: Three Narrative Histories of the French Revolution* (Cambridge: Cambridge Univ. Press, 1990). Further, the prescriptive literature generated by the historical profession is peppered with concrete examples of actual practice. See, e.g., C. V. Langlois and C. Seignobos, *Introduction to the Study of History*, trans. G. G. Berry (London: Duckworth, 1898); R. G. Collingwood, *The Idea of History*, ed. T. M. Knox (Oxford: Clarendon, 1946); Marc Bloch, *The Historian's Craft*, trans. Peter Putnam (Manchester: Manchester Univ. Press, 1954); E. H. Carr, *What Is History? The George Macaulay Trevelyan Lectures Delivered in the University of Cambridge, January–March 1961* (London: Macmillan, 1961); G. R. Elton, *The Practice of History* (Sydney: Sydney Univ. Press, 1967); and John Tosh, *The Pursuit of History: Aims, Methods, and New Directions in the Study of Modern History* (London: Longman, 1984).

[3] "L'oubli, et je dirai même l'erreur historique, sont un facteur essentiel de la création d'une nation": Ernest Renan, *Qu'est-ce qu'une nation?* (Paris: Calmann Lévy, 1882), pp. 7–8.

[4] But Stichweh, it appears, stressed the mechanisms of pedagogy, whereas Schaffer laid more emphasis on the intended results of such pedagogy.

Stichweh's 1984 *Physik in Deutschland* was but part of a larger historiographic tradition that had begun "over a decade earlier," most notably in the work of R. Steven Turner, and has continued ever since. All of this I had wholly neglected: *mea culpa*!

Nevertheless, the difference is a matter of emphasis: as Olesko puts it, "I agree with Wilson's recommendation to pursue the topic, but . . . he views the glass as nearly empty, whereas I view it as half full." In fact, the German studies that Olesko discusses serve to bring into sharper focus a paradox that has lain implicit around this topic for some time. On the one hand, we already know quite a lot about what the "second scientific revolution" involved—for instance, specialization, disciplines (plural), discipline (singular), communication practices, pedagogy, organizations. On the other hand, we struggle to grasp what all of this added up to, what it meant and means, how it came about, and whether and in what ways it differed in specific national contexts. Hence the fact that work of the mid-1980s, by Schaffer and Stichweh alike, has yet to be fully assimilated and appreciated. Hence too, perhaps, the way that all three commentaries already discussed have abutted on this theme without actually confronting it.

Olesko goes on to suggest a change of terminology and a widening of focus. To the latter there can be no objection: it is surely correct that the shift from natural philosophy to the sciences needs to be related to the involvement of the "broader public," to state and empire, and to the desideratum that knowledge should be useful. But while I agree that "second scientific revolution" is an inadequate label, I don't see Olesko's terminological proposal catching on: she suggests "resurrecting a term used by R. Steven Turner: the 'great transition.'" This has already been used—or, rather, overused—with other and quite different meanings (medieval climate, the end of the Cold War, Chinese politics, the future environment), precisely because it is nonspecific—whereas whatever we call the "second scientific revolution," its new and better name will surely have to include some explicit reference to science.[5]

I am grateful to these commentators for the trouble they have taken. It is pleasing that they all endorse the argument of "Science's Imagined Pasts"—and especially that all of them have found ways of taking that argument further.

[5] For uses of "great transition" in the contexts noted see Bruce Campbell, *The Great Transition: Climate, Disease, and Society in the Late Medieval World* (Cambridge: Cambridge Univ. Press, 2016); Raymond L. Garthoff, *The Great Transition: American–Soviet Relations and the End of the Cold War* (Washington, D.C.: Brookings Institution Press, 1994); Hung-Mao Tien, *The Great Transition: Political and Social Change in the Republic of China* (Stanford, Calif.: Hoover Institution Press, 1989); and Paul Raskin *et al.*, *Great Transition: The Promise and Lure of the Times Ahead* (Boston: Stockholm Environment Institute, 2002).