Article:
Wilson, A (2017) Science's imagined pasts. Isis, 108 (4). pp. 814-826. ISSN: 0021-1753

https://doi.org/10.1086/695603

Viewpoint: Science's Imagined Pasts

Science's Imagined Pasts

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Abstract: Science entails history writing: scientists are continuously engaged in creating "imagined pasts" for their own specialisms, both on the small scale of the ubiquitous literature review and on a much broader scale. This aspect of science has been considered in very different ways in decades-old, yet largely neglected, contributions by Thomas S. Kuhn, Augustine Brannigan, and Simon Schaffer. Inspired by these pieces and by the missing dialogue between them, this essay argues that their concealment is itself an instance, on the broadest possible scale, of the power of "imagined pasts"—in this case the imagined continuity, inscribed in the very name of our discipline, between Isaac Newton and ourselves.

I shall argue here for three claims. First, that science (or, rather, each of the individual sciences) necessarily constructs—as an integral aspect of its endeavors—its own *imagined past*, a fanciful genealogy of itself. Second, that this characteristic of science has been half-recognized, yet ultimately elided, in the historiography of science. And third, that the creation and roles of such imagined scientific pasts will richly repay closer and wider study.

It is helpful to distinguish at the outset between what I shall call "interior" and "exterior" forms of the imagined scientific past. The two are related, yet distinct, and what distinguishes them is *audience*: the interior audience comprises scientists themselves, the exterior audience is the wider public. My argument pertains specifically to the *interior* form—that is, to the imagined pasts that science creates *for its own inherent purposes*. Every scientific paper begins with such an imagined past, in the opening literature review through which scientific authors anchor their work; yet remarkably little has been made of this well-known characteristic of scientific rhetoric.[1] In contrast,

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Acknowledgments. I am grateful to participants in the HPS Work-in-Progress Seminar at Leeds for comments on a preliminary presentation; to Greg Radick both for encouraging me to publish and for very helpful substantive suggestions; and to Tim Alborn, Jonathan Buehl, Berris Charnley, Floris Cohen, Jackie Duffin, Jeanne Fahnestock, Steven French, John Henry, Jon Hodge, Nick Jardine, Mark Jenner, Chris Kenny, Simon Schaffer, and two anonymous referees for invaluable advice.

[1] But see Charles Bazerman, "Modern Evolution of the Experimental Report in Physics: Spectroscopic Articles in *Physical Review*, 1893–1980," *Social Studies of Science*, 1984, *14*:163–196; Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Milton Keynes: Open Univ. Press, 1987), pp. 21–61; John M. Swales, *Genre Analysis: English in Academic and Research Settings* (Cambridge: Cambridge Univ. Press, 1990), pp. 114–115, 125–126, 137–166; and Alan G. Gross, Joseph E. Harmon, and Michael Reidy, *Communicating Science: The Scientific Article from the Seventeenth Century to the Present* (Oxford: Oxford Univ. Press, 2002).

Isis, volume 108, number 4. 0021-1753/2017/0108-0004$10.00.

increasing attention has been paid in recent years to the exterior counterpart, as exemplified, for instance, by studies of Isaac Newton and Charles Darwin as historical celebrities.[2] I shall touch on that asymmetry toward the end, but in other respects my focus will be restricted to the interior mode of science's historizing.

The notion of "imagined pasts"—widely used, yet never hitherto applied to science—derives by analogous extension from Benedict Anderson's *Imagined Communities*.[3] Anderson's justly-famous book was published in 1983, and, as it happens, the 1980s seems to have been the key decade for interest in my theme. But such interest began a generation earlier, in 1962, with Thomas S. Kuhn's *The Structure of Scientific Revolutions*, which I shall accordingly take as my starting point. That book was both inspiration and target for my second and pivotal point of reference—namely, Augustine Brannigan's *The Social Basis of Scientific Discoveries*, which appeared in 1981; and my other key texts—by Simon Schaffer and Timothy Alborn—were also published in the 1980s. I shall be suggesting that it is high time their lead was followed.

KUHN

The relevant chapter of Kuhn's *Structure* is Chapter 11, "The Invisibility of Revolutions," which is one of the portions of the book that is particularly indebted to Ludwik Fleck and is all the better for it.[4] The chapter is full of wisdom about the way science constructs a history for itself, a history that suppresses revolutionary change and puts in its place a very different picture, a picture of continuous cumulative development. Here is the essence of the argument; it's framed with reference to textbooks, which is one of the ideas that Kuhn owed to Fleck:

> Textbooks . . . begin by truncating the scientist's sense of his discipline's history and then proceed to supply a substitute for what they have eliminated. . . . The textbook-derived tradition in which scientists come to sense their participation is one that, in fact, never existed. . . . Partly by selection and partly by distortion, the scientists of earlier ages are implicitly represented as having worked upon the same set of fixed problems and in accordance with the same set of fixed canons that the most recent revolution in scientific theory and method has made seem scientific. No wonder that textbooks and the historical tradition they imply have to be rewritten after each scientific revolution. And no wonder that, as they are rewritten, science once again comes to seem largely cumulative. Scientists are not, of course, the only group that tends to see its discipline's past developing linearly toward its present vantage. . . . But scientists are more affected by the temptation to rewrite history. . . . Why dignify what science's best and most persistent efforts have made it possible to discard? . . . Whitehead caught the unhistorical spirit of the scientific community when he wrote, "A science which hesitates to forget its founders is lost." Yet he was not quite right, for the sciences, like other professional enterprises, do need their heroes and

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[2] Ludmilla Jordanova, "Presidential Address: Remembrance of Science Past," *British Journal for the History of Science*, 2000, 33:387–406; Janet Browne, "Charles Darwin as a Celebrity," *Science in Context*, 2003, *16*:175–194; Rebekah Higgitt, *Recreating Newton: Newtonian Biography and the Making of Nineteenth-Century History of Science* (London: Pickering & Chatto, 2007); and Christine MacLeod and Jennifer Tann, "From Engineer to Scientist: Reinventing Invention in the Watt and Faraday Centenaries, 1919–31," *Brit. J. Hist. Sci.*, 2007, *40*:389–411. A rare attempt to deal with both the internal and the external forms of the imagined scientific past and to explore their interrelationship is Bernadette Bensaude-Vincent, "Between History and Memory: Centennial and Bicentennial Images of Lavoisier," *Isis*, 1996, 87:481–499, esp. pp. 494–497.

[3] Benedict Anderson, *Imagined Communities: Reflections on the Origin and Spread of Nationalism* (London: Verso, 1983).

[4] See Ludwik Fleck, *Entstehung und Entwicklung einer wissenschaftlichen Tatsache: Einführung in die Lehre vom Denkstil und Denkkollektiv* (Basel: Schwabe, 1935); and Fleck, *Genesis and Development of a Scientific Fact*, trans. Frederick Bradley and Thaddeus J. Trenn, ed. Trenn and Robert K. Merton (Chicago: Univ. Chicago Press, 1979).

> do preserve their names. Fortunately, instead of forgetting these heroes, scientists have been able to forget or revise their works.[5]

Kuhn went on to give examples from John Dalton (misinterpreting his own early work by seeing it through the frame of his later findings), from Newton (depicting Galileo as proto-Newton), and from modern textbook treatments of Robert Boyle (installing him as inventor of the concept of chemical elements). Perhaps the most forceful of these was the case of Newton: "Newton wrote that Galileo had discovered that the constant force of gravity produces a motion proportional to the square of the time. In fact, Galileo's kinematic theorem does take that form when embedded in the matrix of Newton's own dynamical concepts. But Galileo said nothing of the sort. His discussion of falling bodies rarely alludes to forces, much less to a uniform gravitational force that causes bodies to fall."[6]

Here Kuhn, with Fleck's help, had accurately captured what is undoubtedly a widespread phenomenon, of which other examples could be given: for instance, physiology, in the very act of transforming itself from a speculative discipline into an experimental one, made strenuous efforts to construct for itself a supposed experimental past. Yet it has to be said that Kuhn's chapter was guilty of exactly what it accused science of—namely, a highly selective approach to the facts. For the suppression of revolutions is only half the story, the other half being the exact opposite: the exaggeration of change, the claiming of revolution to conceal continuities, of which the conspicuous example is Antoine-Laurent Lavoisier with his self-proclaimed "revolution in chemistry" or "new chemistry." (Kuhn tucked Lavoisier into his discussion of Boyle, thereby conveniently sidelining him.)[7] Another instance is the supposed discontinuity between so-called "classical" physics and its self-alleged successor quantum physics: there was no such thing as "classical" physics until Max Planck defined it as the Other against which to pitch his own claims.[8] A third such instance is the much-discussed "Darwinian revolution"; here too science, far from obliterating discontinuity, manufactured it.[9] And yet a fourth has passed unnoticed right before our eyes, and before Kuhn's, in the form of Alfred North Whitehead's metaphor of amnesia ("*forget* its founders"), which Kuhn quoted; Kuhn rightly distanced himself from that metaphor but did not inquire as to why Whitehead had deployed it.[10] In fact, Whitehead—speaking in 1916 as President of Section A, Mathe-

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[5] Thomas S. Kuhn, *The Structure of Scientific Revolutions* (1962), 2nd ed., enlarged (Chicago: Univ. Chicago Press, 1970), pp. 137–139. Cf. Fleck, *Genesis and Development of a Scientific Fact*, pp. 112–113.

[6] Kuhn, *Structure of Scientific Revolutions*, pp. 139–143, on p. 139.

[7] Regarding physiology see Andrew Cunningham, "The Pen and the Sword: Recovering the Disciplinary Identity of Physiology and Anatomy before 1800, I: Old Physiology—The Pen," *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences*, 2002, 33:631–665, esp. pp. 636–637. On Lavoisier's "revolution" see, e.g., Robert Siegfried, "The Chemical Revolution in the History of Chemistry," *Osiris*, 1988, N.S., *4*:34–50, esp. p. 37; Bensaude-Vincent, "Between History and Memory" (cit. n. 2); and Hasok Chang, "We Have Never Been Whiggish (About Phlogiston)," *Centaurus*, 2009, *51*:239–264. For Kuhn's dismissal of Lavoisier see Kuhn, *Structure of Scientific Revolutions*, pp. 141–143.

[8] Richard Staley, "On the Co-Creation of Classical and Modern Physics," *Isis*, 2005, 96:530–558; and Graeme Gooday and Daniel Jon Mitchell, "Rethinking 'Classical Physics,'" in *The Oxford Handbook of the History of Physics*, ed. Jed Z. Buchwald and Robert Fox (Oxford: Oxford Univ. Press, 2013), pp. 721–764. On the associated problem of public management see Imogen Clarke, "How to Manage a Revolution: Isaac Newton in the Early Twentieth Century," *Notes and Records of the Royal Society*, 2014, *68*:323–337.

[9] Jonathan Hodge and Gregory Radick, "The Place of Darwin's Theories in the Intellectual Long Run," in *The Cambridge Companion to Darwin*, ed. Hodge and Radick, 2nd ed. (Cambridge: Cambridge Univ. Press, 2009), pp. 246–274; and Hodge, "Against 'Revolution' and 'Evolution,'" *Journal of the History of Biology*, 2005, 38:101–124, esp. p. 103. See also Michael Ruse, "The Darwinian Revolution: Rethinking Its Meaning and Significance," *Proceedings of the National Academy of Sciences*, 2009, *106*:10040–10047.

[10] Kuhn had probably picked it up via Robert K. Merton rather than reading it in the original. It seems that Merton introduced Whitehead's metaphor in the 1957 edition of his *Social Theory and Social Structure*, which had first been published in 1948:

matical and Physical Science, of the British Association for the Advancement of Science—had done so specifically to celebrate the arrival of predicate logic and to depict that shift in the colors of revolutionary discontinuity.[11]

Further, Kuhn's picture is contradicted not only by these specific cases but also by a broader argument, anchored both in empirical instances and in theoretical reflection, that was first developed by Augustine Brannigan in his 1981 book *The Social Basis of Scientific Discoveries* and was taken up, and taken further, by Simon Schaffer in a paper that appeared three years later in Italian and in English another two years after that (that is, in 1986) in *Social Studies of Science*.[12]

BRANNIGAN

Brannigan's argument can be paraphrased by saying that scientific "discovery" always has to have quotation marks around it because a discovery is a retrospective construct designed to justify, rationalize, and systematize current scientific practice. His central example was the achievement and reputation of Gregor Mendel. According to the standard view, Mendel was the "founder of genetics," his work was "strangely forgotten" for thirty years, and then it was "rediscovered" in 1900. But Brannigan argued that Mendel did not found "genetics" (he was doing something different), that his work was not forgotten (it was widely cited in studies of hybridization), and that it was not rediscovered but reappropriated. Specifically, whereas Mendel saw hybridization as an *alternative* to evolution—that is, a different way of accounting for species—those who took up Mendel's work in 1900—most notably William Bateson—used it to provide a mechanism of inheritance that would *support* evolution. This was in a doubly new scientific context: on the one hand, "Weismann's cytological conclusions" (the distinction, which August Weismann had proposed in 1886, between "germ plasma" and "somatic" cells), and, on the other, a controversy between those who believed in continuous small evolutionary changes (the biometricians) and those who believed in sudden leaps or "mutations" (the saltationists). Mendel served the saltationists as a stick with which to beat the biometricians, and Weismann's work gave his findings a meaning that they could not have had for Mendel himself. Using language drawn—interestingly—from Kuhn, Brannigan summed up in advance his interpretation of the Mendel phenomenon as follows: "in 1866 Mendel's work figured as normal science in the hybridist tradition, while in 1900 the revival of Mendel's discovery of segregation constituted a relatively revolutionary achievement."[13]

What I have just offered is far too compressed a summary of an immensely nuanced and exceptionally fertile book—one of the great classics of our field, in fact. Where others had debated the supposed prior "causes" of discovery (individual genius or cultural maturity, according to taste), Brannigan showed that reinterpreting "discovery" as an honorific label awarded after the event made far more sense not only of actual discoveries but also of the theories surrounding

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Robert K. Merton, *Social Theory and Social Structure*, rev. and enlarged ed. (Glencoe, Ill.: Free Press, 1957). I rely here on Andrew Roberts, "Extracts from Robert King Merton," http://studymore.org.uk/xmer.htm (accessed 10 Apr. 2015).

[11] Whitehead's 1916 presidential address was entitled "The Organisation of Thought." Immediately after saying that "a science which hesitates to forget its founders is lost," Whitehead added, "To this hesitation I ascribe the barrenness of logic"—by which he meant the traditional logic whose overthrow he was endorsing. See Alfred North Whitehead, "The Organisation of Thought," http://www-history.mcs.st-and.ac.uk/Extras/BA_1916_1.html and http://www-history.mcs.st-and.ac.uk/Extras/BA_1916_2.html.

[12] Simon Schaffer, "Scoperte scientifiche alla fine del XVIII secolo," in *Intelligibilità e costruzione scientifica*, ed. F. Papi (special issue of *Materiali Filosofici*) (Milan: Franco Angeli, 1984), pp. 97–114; and Schaffer, "Scientific Discoveries and the End of Natural Philosophy," *Soc. Stud. Sci.*, 1986, *16*:387–420.

[13] Augustine Brannigan, *The Social Basis of Scientific Discoveries* (Cambridge: Cambridge Univ. Press, 1981), Ch. 6 (pp. 89–119), considers Mendel. Regarding "Weismann's cytological conclusions" see *ibid.*, p. 95; curiously, Brannigan mentioned this particular aspect only in passing, yet he implied that it played an essential role (as was indeed the case). For Brannigan's summation of his interpretation see *ibid.*, p. 90.

the subject: for instance, the famous notion of so-called "simultaneous discovery" was a polemical tool in the hands of those who favored cultural "explanations" and rested on an elastic and indeed vague notion of what counted as simultaneity. From the many insights in the book I select as an example this one, also from the Mendel chapter, this time from near its end: "Rather than treating the absence of Mendel's reputation in the wider community as a failure, the present argument has recommended that Kuhn's distinction between normal and revolutionary science, *conceived as a function of the attributional status of discovery*, explains the different values assigned to his achievement over time." At the start of the chapter it had looked as if Brannigan was simply applying Kuhn's categories, but it now turns out that—in almost a throwaway manner—he has radically relativized them: they belong within what he elsewhere calls "folk reasoning."[14]

That is probably why Brannigan didn't discuss Kuhn's Chapter 11, the one called "The Invisibility of Revolutions": he didn't accept the Kuhnian dichotomy between "revolutionary" and "normal" science as an analytic category (or pair of categories), and in the absence of that dichotomy the "invisibility of revolutions" is no longer a theme. But let us suppose that some latter-day disciple of Kuhn were to try to assimilate Brannigan's picture to the overall argument of *Structure*. Our Kuhnian might proceed along something like these lines: a scientific "discovery," as depicted by Brannigan, consists of a stylized and retrospective description of the change—in objects, methods, techniques, conceptual frameworks, presuppositions—that Kuhn called a paradigm shift. That might just about work, but the price it entails would be that the unfortunate Kuhnian would have to throw out Kuhn's Chapter 11. According to Kuhn, after the "revolution" (the change of paradigm) the new generation rewrites history to erase that revolution and depict change as gradual, incremental, cumulative, progressive. According to Brannigan, when scientists rewrite history they don't erase revolutions; on the contrary, they stylize them and celebrate them under the flag of "discoveries." The Lavoisier case—the kind that Kuhn suppressed—becomes the norm.

To sum up so far, these two classics have produced two incompatible pictures of the way that science constructs its own past: mythical continuities according to Kuhn, mythical discontinuities according to Brannigan. I shall come back to both of them, but first it's instructive to bring into focus Schaffer's development of Brannigan's argument, in a paper whose 1986 English version was entitled "Scientific Discoveries and the End of Natural Philosophy."

SCHAFFER

Schaffer took Brannigan's thesis and radically historicized it. As Schaffer presented the matter, Brannigan's picture *became* true after around 1800, because it was specifically characteristic of science—as distinct from its diffuse, undisciplined predecessor natural philosophy. That is, the exaltation of "discovery" was part of what distinguished science from natural philosophy; and Schaffer mounted a powerful argument, anchored in four case studies (the planet Uranus, the substance called oxygen, the inverse square law of electrostatic attraction, the phenomenon of photosynthesis), that there was a historical change of just this kind, whose beginnings could be located in the late eighteenth century and whose consummation was effected by William Whewell and others in the 1830s. Typical of the power and insight of this article was the point that it was specifically in the context of the associated disputes that Whewell, under pressure from Samuel Taylor Coleridge, came to endorse what Schaffer nicely called "the new and outlandish term 'scientist.'"[15] The new set of practices that was already emerging, for which Whewell was busily providing the rationale, was distinct from what had gone before in two fundamental respects,

[14] *Ibid.*, p. 119 (emphasis added) and Ch. 8.

[15] Schaffer, "Scientific Discoveries and the End of Natural Philosophy" (cit. n. 12), pp. 387 (outlining the case studies), 410 (quotation).

which are precisely captured by the two senses of the word "discipline." First, science was divided into disciplines—geology, chemistry, botany, and so on—in a way that natural philosophy was not. Second, and as a corollary, its practitioners were *disciplined* in that they had to be trained in the specific skills, practices, and habits of mind appropriate to these various disciplines. That is, the practitioner of science—now to be called a scientist—is characterized by obedience, not originality; he (it was assumed to be he) is an underlaborer, tidying up the details left by the last genius (such as Humphry Davy) and preparing the way for the next one (Michael Faraday).

Here we approach Schaffer's point about discovery. Discoveries, in this Whewellian ethos, are specifically produced by geniuses like Davy; they result from flashes of insight that the humbler mere scientist—the disciplined underlaborer required by Whewell's vision of science—cannot hope to emulate or achieve. The genius, as free and untrammeled individual, produces the discoveries between which the underlaborers are to plod away, all adhering obediently to the procedures proper to their respective disciplines. The discoveries are epoch-making (literally, in Whewell's terminology) and are exactly what the nongenius scientist *cannot* aspire to. Thus, as Schaffer put it, discovery was about the "fixing of scientific practices."[16] In Whewell's vision, discovery was compressed (reduced from a process stretched out in time to an instantaneous act) and, above all, individualized—reduced from a complex social process to an individual act, an act of which only a "genius" would be capable.

It's surprising and disappointing how little has been done to develop, apply, test, and refine this bold and original picture in the thirty years that have now elapsed since it was published. There are plenty of citations (124 of them, at last count), but almost all of these take the form of a polite nod: hardly anyone has sought to put this thesis to actual work.[17]

The grand exception—which, we shall see, turns out to confirm the rule—was a remarkable response by Timothy Alborn that appeared in 1989. Alborn argued that Schaffer's historicization of the heroic conception of "discovery" had not gone far enough. The burden of his critique was that while Schaffer had identified a very important issue, he had oversimplified it by treating Whewell's model as definitive of science. In fact (Alborn contended), the picture that Whewell developed in the 1830s was contested at the time both by John Herschel and by David Brewster, each of whom had his own distinctive vision for the methods and social arrangements of science; furthermore, it would later emerge (through their responses first to Robert Chambers's *Vestiges* and then to Darwin's *Origin*) that all three of these eminent "gentlemen of science" persisted in yoking science to religion. In short, Whewell was not a representative spokesman for the "science" of his own era, let alone for that of the future. The corollary was that the creation of the "twentieth century scientific elite" was a protracted process, as was that of "its accompanying 'heroic' ideology." Alborn was inviting historians of science to embark on an exploration of that process, much as Frank M. Turner had done a decade earlier with respect to the closely related topic of the nineteenth-century debate over "science and religion."[18]

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[16] *Ibid.*, p. 397.

[17] Honorable exceptions include Jonathan R. Topham, "Scientific Publishing and the Reading of Science in Nineteenth-Century Britain: An Historiographical Survey and Guide to Sources," *Stud. Hist. Phil. Sci. Part* A, 2000, *31*:559–612; and Jed Z. Buchwald and Sungook Hong, "Physics," in *From Natural Philosophy to the Sciences*, ed. David Cahan (Chicago: Univ. Chicago Press, 2003), pp. 163–195, esp. p. 168. The most surprising instances that I know of are two classic papers on the science/natural philosophy distinction/transition, neither of which cites Schaffer's essay: Andrew Cunningham, "Getting the Game Right: Some Plain Words on the Identity and Invention of Science," *Stud. Hist. Phil. Sci.*, 1988, *19*:365–389; and Peter Dear, "What Is the History of Science the History *Of*? Early Modern Roots of the Ideology of Modern Science," *Isis*, 2005, 96:390–406. My "last count," which yielded 124 citations, came from a check of Google Scholar on 22 July 2016.

[18] Timothy Alborn, "The 'End of Natural Philosophy' Revisited," *Nuncius*, 1988, 3:227–250, on p. 230; and Frank M. Turner, "The Victorian Conflict between Science and Religion: A Professional Dimension," *Isis*, 1978, *69*:356–376.

I said that this was a remarkable piece and that it was the exception that confirmed the rule. It confirmed the rule in that, twenty-five years later, it has been cited in just three subsequent studies, none of which has added in any way to Alborn's claims.[19] And it was remarkable for the fact that it took Schaffer's argument further by the application of Schaffer's own approach. Indeed, the effect of Alborn's paper (although he didn't put it like this) was to depict Schaffer's story of the move from natural philosophy to science in terms similar to the way that Schaffer himself had portrayed the discovery stories that are characteristic of science. That is, Schaffer's account had reduced a *process* to an *event*.

I shall come back to the point about reception, but first I want to take stock of the larger theme.

CONSPECTUS

In the light of what we've learned from Brannigan and from Schaffer, we might easily dismiss Kuhn's idea as special pleading—but we'd be making a double mistake in doing so. In the first place, there's no doubt that what Kuhn called the invisibility of revolutions does indeed come to pass in some contexts. The question to ask is, In what contexts does it occur? Second, the counterexamples we've been looking at, while they are fatal for the crude generalization that Kuhn put forward, by no means eliminate the bigger theme that's in play here: on the contrary, they strengthen it. That bigger theme is that either way—whether inventing discontinuity or suppressing it—science *invents a past for itself*. No doubt the same is true of other fields of human endeavor: a case in point is philosophy, in which context R. A. Watson has coined the useful phrase "shadow history" to denote the corresponding phenomenon. On Watson's account, the "shadow history" of philosophy is the false history that philosophers construct in order to do philosophy.[20] But there are several reasons to suspect that science does this to a peculiar degree.[21]

In the first place, as we saw at the outset, the scientific paper—the universal vehicle of scientific communication, recognition, and record—always begins by placing itself in a context of relevant *past* research. (In John M. Swales's three-move analysis of article introductions, this is the first move, that of "establishing a territory," and is characterized by maximum "rhetorical effort.")[22] That past may be very recent, but what is happening here has nothing to do with relative antiquity; rather, the point is that this introductory act *constructs*—by selection, arrangement, valuation, and so on—the past that it posits. The scientist, then, always begins her writing as a kind of historian, and this by force of convention—for if she failed to do so, no journal would publish her paper. Second, we have to reckon with the phenomena identified by Kuhn and by Brannigan: the suppression of revolutions on the one hand, the magnification of discontinuities on the other. We have seen these exemplified on the grand scale, in cases ranging from physiology to predicate logic and from chemistry to genetics, but both of them apply on the medium and small scales as well: Kuhn depicted revolutions and their invisibility as multiple and widespread, and, similarly, "discoveries" are not just heroic but also pervasive. Third, there may well be other forms of this phenomenon. For instance, M. A. B. Whitaker, writing in 1979 about the history of physics, observed that there was a tendency—for which he coined the term "quasi-history"—for the history to be "rewritten so that it fits in step by step with the physics," but the examples he gave cannot easily

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[19] Helge Kragh, "The Solar Element: A Reconsideration of Helium's Early History," *Annals of Science*, 2009, *66*:157–182, esp. pp. 158–159; Skuli Sigurdsson, "Equivalence, Pragmatic Platonism, and Discovery of the Calculus," in *The Invention of Physical Science: Intersections of Mathematics, Theology, and Natural Philosophy since the Seventeenth Century: Essays in Honor of Erwin N. Hiebert*, ed. Mary Jo Nye, Joan L. Richards, and Roger H. Stuewer (Dordrecht: Kluwer, 1992), pp. 97–116; and Elizabeth Green Musselman, *Nervous Conditions: Science and the Body Politic in Early Industrial Britain* (Albany: SUNY Press, 2006).

[20] R. A. Watson, "Shadow History in Philosophy," *Journal of the History of Philosophy*, 1993, *31*:95–109.

[21] Kuhn touched on this point: *Structure of Scientific Revolutions* (cit. n. 5), p. 138.

[22] Swales, *Genre Analysis* (cit. n. 1), p. 141.

be classed as cases of either continuity or discontinuity.[23] Fourth, and finally, it is precisely science's need to manufacture a past for itself that gives us all a job. History of science came into being in the first place as an extension of science's own history writing—what we now know as "practitioner history"—and even now it often remains connected, and sometimes in subtler ways than we might expect, with that same "practitioner history."[24] Thus our own professional existence obliquely attests to science's continuing need to construct a past for itself —a point I shall take up at the very end, where (to give the reader fair warning) Kuhn will be vindicated anew. In short, the phenomenon of an "imagined past" is a remarkable property of science, a characteristic of science, that deserves attention in its own right.

At this deeper level, the Kuhn picture and its contraries—we might call them the Brannigan-Schaffer-Alborn picture—are agreed. And here's a further convergence between them. Just as Schaffer's brilliant argument has gone largely unnoticed, and Alborn's development of it has been entirely overlooked, so Kuhn's Chapter 11 has received only meager attention. Google Scholar identifies some 58 citations—48 for the phrase "Invisibility of revolutions" plus another 10 for "Revolutions invisible"—as against 833 for "Gestalt switch," 1,630 for "Revolutionary science," and well over 5,000 for each of "Anomalies," "Paradigm shift," and "Normal science" (the winner at 8,020).[25] In the citation stakes, "anomalies" (which, by the way, Brannigan neatly exposed as a circular concept) outweighs "invisibility" by over 100 to 1.[26]

Although Kuhn can be said to have identified this phenomenon first (albeit with a considerable debt to Fleck), he didn't really develop the point, because the intent of his "Invisibility of Revolutions" chapter was critical, not constructive: he was merely eliminating an obstacle to acceptance of his picture of the pervasiveness of hidden "revolutions." And similar considerations apply to Bruno Latour's picture of science as put forward in *Science in Action*, published in 1987—that is, the year after the Schaffer paper on "discovery." In that book Latour depicted science as Janus-faced: his running visual joke was that this Janus was asymmetrical, its backward-looking face being old (as in the traditional image), its forward-looking face young. The old, backward-looking face represented "ready made science"; the young, forward-looking one corresponded to "science in the making" or, as the book title had it, science "in action"; and of course Latour's very first "rule of method" was that "we study science *in action* and not ready made science or technology."[27] The fact that Latour could only ever depict his favored *forward*-looking face of Janus in the company of the *backward*-looking one is further support for the picture developed here—that science is constrained to construct a history of itself, for itself, in order to achieve whatever it is that it achieves. But the point to observe is that Latour's use of the Janus-face image, like Kuhn's of "invisibility," was strictly methodological, not substantive: the backward-looking face came into play only as an obstacle to our understanding, never as a phenomenon in its own right.

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[23] M. A. B. Whitaker, "History and Quasi-History in Physics Education," *Physics Education*, 1979, *4*:108–112, 239–242, on p. 109. Whitaker added: "Because the description of the physics is logical and orderly, the impression is necessarily given that this was also the way in which the ideas emerged historically."

[24] See Paul Forman, "Independence, Not Transcendence, for the Historian of Science," *Isis*, 1991, 82:71–86; Bensaude-Vincent, "Between History and Memory" (cit. n. 2); and Chang, "We Have Never Been Whiggish" (cit. n. 7).

[25] "Normal science" 8,020; "Paradigm shift" 7,320; "Anomalies" 5,960; "Revolutionary science" 1,630; "Gestalt switch" 833; "Invisibility of revolutions" 48; "Revolutions invisible" 10. In each case the search, conducted on 22 July 2016, was for {"Thomas Kuhn" Structure "scientific revolutions"} plus the item specified.

[26] Brannigan, *Social Basis of Scientific Discoveries* (cit. n. 13), pp. 21–25, 37.

[27] Latour, *Science in Action* (cit. n. 1), p. 258. Cf. Thomas S. Kuhn, "The Function of Measurement in Modern Physical Science," *Isis*, 1961, 52:161–193, on p. 162: "Measurement's actual function—either in the search for new theories or in the confirmation of those already at hand—must be sought in the journal literature, which displays not finished and accepted theories, but theories in the process of development." So far as I am aware, this Kuhnian origin, or anticipation, of Latour's most basic premise has not attracted comment.

That, I think, explains the otherwise puzzling fact that Latour barely mentioned Brannigan's book or its themes (there was just a single token allusion, buried within his critique of the diffusion model).[28] It also makes it intelligible that this pivotal aspect of *Science in Action* has been wholly overlooked in the reception of the book—for there has not been a single scholarly discussion of what Latour's young-and-old Janus-face actually means. The indifference to its meaning both of Latour himself and of those responding to him echoes what we've already seen elsewhere: the theme of science's imagined pasts seems never to come into sustained focus.

The elusiveness of that theme is also embodied in the asymmetry I mentioned at the outset—that is, the historiographic tendency to focus selectively on the *exterior* historizing of science. This is partly a matter of the topics chosen: for instance, anniversary commemorations are typically directed at the general public, not at scientific audiences. But it is also manifested in the way that those topics are approached: thus John Henry has pointed out that nineteenth-century biographies of Newton were informed and motivated by specific scientific claims and concerns to a greater degree than has been appreciated even by an otherwise very fine study of the genre.[29]

So too that theme surely deserves theorization, but such ready-made resources of which I am aware seem only loosely fitted, at best, for the purpose. Our phenomenon can presumably be regarded as a species of what Maurice Halbwachs called "cultural memory"; but if so, it is a very distinctive species. Certainly the ruthless suppression and appropriation that science seems characteristically to perform on its own past is quite different from what Martin Heidegger called *Wiederholung*, an engagement with the past that has the sense of authentic retrieval. In fact, the most relevant theory—and even this doesn't get us very far, as we shall see—seems to be Michael Oakeshott's concept of the "practical past." Oakeshott published two accounts of that concept: the first in *Experience and Its Modes* (1933), the second a half-century later in *On History and Other Essays*, which appeared in 1983, when he was eighty-two.[30] For the sake of simplicity I shall just use the later one, which means that once more—as with Brannigan, Schaffer, Alborn, and Latour—we are in the 1980s.

To illustrate his "practical past" notion, Oakeshott's favorite example was the Trojans: when he and his brother were children, getting tired in the course of a long walk, their father would exhort them to keep going by saying that Trojans would not have flagged or subsided, Trojans would have kept going. Trojans were emblems of endurance. Oakeshott's elegant point was that the Trojans his father was talking about were not the actual historical Trojans, all of whom were long dead, but emblematic figures who were very much alive. The practical past, then, is *the past that is present in our culture*; Oakeshott also called it the "didactic past" and "the so-called living past."

Now on each occasion that he advanced this concept, Oakeshott was actually writing about something quite different: what he called the "historical past." The historical past is the realm of past human actions and is therefore irredeemably gone: it can never be observed, only inferred from its surviving relics, and such inference is precisely the job of the historian. This

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[28] Latour, *Science in Action*, p. 134.

[29] John Henry, rev. of Higgitt, *Recreating Newton* (cit. n. 2), *Isis*, 2009, *100*:176–177, on p. 176: "The book deserves to be read by all those with an interest in the development of the public image of science in the nineteenth century. I couldn't help feeling, however, that a focus on the relevance of these biographies to nineteenth-century science itself, rather than to a putative incipient history of science, would have been much more revealing."

[30] Maurice Halbwachs, *La mémoire collective* (Paris: Presses Univ. France, 1950); Astrid Erll, Ansgar Nünning, and Sara B. Young, eds., *Cultural Memory Studies: An International and Interdisciplinary Handbook* (Berlin: De Gruyter, 2008); Martin Heidegger, *Being and Time*, trans. John Macquarrie and Edward Robinson (Oxford: Blackwell, 1962), p. 437, translators' note; Michael Oakeshott, *Experience and Its Modes* (Cambridge: Cambridge Univ. Press, 1933), pp. 103–105; and Oakeshott, *On History and Other Essays* (Oxford: Blackwell, 1983), pp. 16–19, 34–44, 106.

pastness of the historical past was Oakeshott's core concern, because his aim was to understand historical knowledge as a distinct "mode" (his key term) of experiencing or of knowing. And the reason he identified and labeled the "practical past" was precisely to demarcate it from the historical past.

This meant that Oakeshott was in fact not much interested in the practical past itself, which had two baleful effects. First, he drifted in the direction of depicting "the" practical past as single and homogeneous, even though his own examples (including his family's image of the Trojans) point toward there being not one practical past but many. In fact, of course, there are as many practical pasts as there are components of our culture, and once one sees this Oakeshott's conception becomes far more powerful than he himself allowed it to be.[31] Second, he never asked *how the practical past is constructed*, which of course is the question that our theme presses upon us.

The ground already covered suggests two lines of attack on that question, which I shall sketch by way of conclusion. The first of these is the broader of the two in formal scope, but the second is much the richer in substance—and it will lead us, in a surprising twist, back both to Kuhn and to ourselves.

REVOLUTIONS RECONSIDERED

First, we could elaborate a typology—so to speak, a "natural history"—of the ways in which science constructs a past. Those ways will no doubt prove to be more complex than the simple continuity/discontinuity dichotomy on which I have focused here.[32] Indeed, these have sometimes been combined—as, for instance, by William Bateson, the chief rediscoverer of Mendel, who wrote in 1909:

> The words "evolution" and "origin of species" are now so intimately associated with the name of Darwin that we are apt to forget that the idea of a common descent had been prominent in the minds of naturalists before he wrote, and that, for more than half a century, zealous investigators had been devoting themselves to the experimental study of that possibility. Prominent among this group of experimenters may be mentioned Koelreuter, John Hunter, Herbert, Knight, Gaertner, Jordan, Naudin, Godron, Lecoq, Wichura—men whose names are familiar to every reader of *Animals and Plants under Domestication*. *If we could ask those men to define the object of their experiments, their answer would be that they were seeking to determine the laws of hereditary transmission with the purpose of discovering the interrelationships of species*.[33]

This passage would have served Kuhn perfectly, since Bateson here did to a tradition of hybridists (with the experimental anatomist John Hunter thrown in) exactly what Newton had done to

[31] See Adrian Wilson, "What Is a Text?" *Stud. Hist. Phil. Sci.*, 2012, *43*:341–358, esp. pp. 346–347.

[32] An indication of the complexities is supplied by the only instance known to me of a relevant case study that has itself been subjected to critical analysis: Paul Forman's 1969 paper on the "myths" surrounding the origins of X-ray diffraction. The main critique (though not the only one), that of L. D. Gasman, was motivated by a Lakatosian argument yet drew on historical evidence; and the most recent analysis, by André Authier, finds that both Forman and Gasman got some things right, some things wrong. See Paul Forman, "The Discovery of the Diffraction of X-rays by Crystals: A Critique of the Myths," *Archive for History of Exact Sciences*, 1969, *6*:38–71; L. D. Gasman, "Myths and X-rays," *British Journal for the Philosophy of Science*, 1975, *26*:51–60; and André Authier, *Early Days of X-ray Crystallography* (Oxford: Oxford Univ. Press, 2013), pp. 125–129. For a different angle on the subject (eschewing discussion of Forman's claims) see Jonathan Buehl, *Assembling Arguments: Multimodal Rhetoric and Scientific Discourse* (Columbia: Univ. South Carolina Press, 2016).

[33] William Bateson, *Mendel's Principles of Heredity* (Cambridge: Cambridge Univ. Press, 1909), p. 2 (emphasis added).

Galileo: that is, he projected his own concerns onto his predecessors. Yet Bateson was also a classic case—one might say the classic case—in Brannigan's argument.

Such a typological investigation should go on to identify the contexts in which particular variants have appeared. What disciplines, and in what circumstances, have opted for continuity? Where and when, on the other hand, do we find discontinuity being favored? Have there been cases where the two have been in contention? And in what settings have they been combined? [34]

Second, we could take up Schaffer's argument and subject it to refinement and testing, with Alborn's critique in mind. Here two sets of questions can be envisaged, one on each side of what Schaffer portrayed as the natural philosophy/science divide. First, what was the significance of "discovery" within natural philosophy? Since associated phenomena such as priority claims were a familiar part of the early modern natural-philosophical scene, it appears that there were elements of continuity as well as discontinuity in this respect (as in some others) between natural philosophy and science. And second, how did the transition from natural philosophy to science—which Alborn rightly redefined as a process extended in time—work itself out through the nineteenth century?

Now this latter theme is arguably the grand neglected topic of our entire discipline: it has become universally recognized as important, yet it has received only a tiny fraction of the attention bestowed on its predecessor transformation, the one that used to be called the "scientific revolution." That now-discredited term itself served to obliterate the later transition by fostering the illusion that "science" or "modern science" was formed in the early modern period, a category mistake from which we are still in the process of extricating ourselves.[35] Even when the later transition has been recognized, the profession has been unclear what to make of it and, indeed, what to call it. Kuhn himself (in a paper of 1961) dubbed it the "second scientific revolution"—a formulation that combines continuity ("scientific") and discontinuity ("revolution");[36] Brannigan did not discuss it. Apart from brief signs of interest in, yet again, the 1980s, "second scientific revolution" has never caught on; but nothing has replaced it.[37] Collectively, then, we are in a strange position. On the one hand, it is widely accepted that the natural philosophy of the eighteenth century (and before) was a radically different kind of activity from the science of the nineteenth (and since).[38] On the other hand, we have no name for the change from the one to the other, and we have no consistent story to tell about it.

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[34] Nick Jardine has remarked (personal communication, 2015): "I am struck by the many early-modern mathematicians, natural philosophers and medics who do both: for example Kepler who sees his work as fruit of a long descent from Pythagoras, but also as the first unveiling of the archetypal secrets of the universe; Freind, as expounded by Julian Martin, who presents his medical practices as continuously descended from Hippocrates and his medical theory as entirely unprecedented before Newton and Pitcairne; etc." This refers to Julian Martin, "Explaining John Freind's *History of Physick*," *Stud. Hist. Phil. Sci.*, 1988, *19*:399–418.

[35] Admittedly, a strong argument has recently been mounted for retaining the concept of a "scientific revolution" to describe the seventeenth-century transformation: see H. Floris Cohen, *The Rise of Modern Science Explained: A Comparative History* (Cambridge: Cambridge Univ. Press, 2015); and, for a more extended treatment, the same author's *How Modern Science Came into the World: Four Civilizations, One Revolutionary Breakthrough* (Amsterdam: Amsterdam Univ. Press, 2010). But Cohen also stresses the subsequent transformations of the eighteenth and nineteenth centuries (see *Rise of Modern Science Explained*, pp. 262, 269–278), which means that the immediate legacy of the seventeenth century was distinct from post-1800 science.

[36] Kuhn, "Function of Measurement in Modern Physical Science" (cit. n. 27), p. 190: "Sometime between 1800 and 1850 there was an important change in the character of research in many of the physical sciences, particularly in the cluster of research fields known as physics. That change is what makes me call the mathematization of Baconian physical science one facet of a second scientific revolution."

[37] For some 1980s considerations of a "second scientific revolution" see Enrico Bellone, *A World on Paper: Studies on the Second Scientific Revolution*, trans. Mirella Giacconi and Riccardo Giacconi (Cambridge, Mass.: MIT Press, 1980); and Stephen G. Brush, *The History of Modern Science: A Guide to the Second Scientific Revolution, 1800–1950* (Ames: Iowa State Univ. Press, 1988).

[38] Cohen's formulation addresses, rather, suites of activities: "modes of nature-knowledge" before ca. 1800 (see Cohen, *Rise of Modern Science Explained* [cit. n. 35]) and, presumably, the various scientific disciplines in the modern era.

Indeed, given how little attention the topic has received, it is remarkable how divergent are those views of it that have been articulated. Kuhn's "second scientific revolution" was restricted to the physical sciences; he said that one aspect of the change was the mathematization of what he called the "Baconian" sciences (such as electricity and magnetism), but he refrained from sketching the contours of the larger picture of which he was discussing merely "one facet." Schaffer's 1986 argument was very different: broader in scope (his examples embraced chemistry, physics, astronomy, and even botany); focused not on methods but on social relations; and, above all, characterizing science as disciplined—in the double sense of that word—where natural philosophy was not. In contrast, Andrew Cunningham, writing two years after Schaffer (yet again, be it noted, in the 1980s), depicted the shift from natural philosophy to science as a change in the object of knowledge—that object being God's Creation for natural philosophy but mere Nature for science. These three pictures were wholly at cross-purposes; and Peter Dear, in a paper of 2005, extended the pattern. Dear rejected the notion that natural philosophy was superseded by science; rather, he contended, natural philosophy persisted as one aspect of science, its other aspect being instrumental power over nature and its "ideology" the circular notion of justification linking the two.[39]

The most recent attack on the problem, that of Floris Cohen published in 2015, has departed from this pattern in that it has a place for each of those earlier pictures—yet Cohen himself acknowledges that our understanding of the topic is still in its infancy, proposing as he does that we return to Kuhn's term "second scientific revolution" while admitting that this is, so far, "an 'expression'—not yet a concept."[40] Taking all his cues from an unpublished "think-piece" in Dutch by Frans van Lunteren, Cohen proposes, first of all, that we need to take account of the contribution of modern industry (the conspicuous example, though not the only one, being the steam engine), an aspect that he argues differentiates the second scientific revolution sharply from the first. Second, and relatedly, he suggests that the new coupling of science with industry served as a source of legitimacy by actually delivering, at long last, on the former promises of Baconian and Enlightenment aspirations (and, unlike Dear, he does not depict this legitimation as "ideological" in the sense of mystifying). Third, he connects the second scientific revolution with that historicization of European culture that was opened up by the French Revolution—leading to the historicization of Nature herself and thus to Darwin. Finally, building on these foundations, Cohen manages to weave in both Schaffer's disciplines and Cunningham's focus on religion, also finding a place for Kuhn's mathematization. Despite its brevity, this is probably the most considered attack on the problem that has appeared so far—yet it has been offered only as a brief glance forward at the end of a book on the *first* "scientific revolution." The second one remains elusive even in the very moment that it has been brought into focus.

Thus the *substantive* theme to which Schaffer and Alborn led us raises a *historiographic* question that neither of them addressed: Why has that theme been obscured for so long? That question has also been overlooked by all but one of the commentators just discussed—the exception being Cunningham's 1988 paper entitled "Getting the Game Right" (1988). Cunningham argued that it was science itself—conceived as a new "game," a different "game" from its predecessor natural philosophy—that brought about the illusion of its own eternal character. Science, in Cunningham's picture, was "invented" in the early nineteenth century, and those who invented it immediately set about legitimating it by presenting it as eternal:

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[39] Cunningham, "Getting the Game Right" (cit. n. 17); and Dear, "What Is the History of Science the History *Of*?" (cit. n. 17).

[40] Cohen, *Rise of Modern Science Explained* (cit. n. 35), pp. 273–278, on p. 278. For Cohen's debt to van Lunteren see *ibid.*, p. 286.

> Writing the history of science was an early nineteenth century innovation (as it could only be, if the practice itself had only just been invented). The inventors of science and their immediate successors unselfconsciously rewrote the past in a way which showed themselves to be the heirs to a grand tradition. . . . The most general form that this history-of-science writing took was to write histories which were in practice actually making novel *assertions* about where the "natural" subject-boundaries of knowledge now lay: for instance, histories of the "inductive" sciences, of the "exact" sciences, or histories of "biology," of "geology," or of "physics."

Notice the allusion to Whewell ("histories of the 'inductive' sciences"), whose appearance here, albeit implicit, fitted nicely with what Schaffer had published two years earlier. The consequence of this new history writing, Cunningham argued, was that "the invention of science" became "historically 'invisible'"—and here he cited the very chapter from Kuhn's *Structure of Scientific Revolutions* with which we began.[41]

The immediate import of Cunningham's historiographic claim (though he did not spell this out) was that Whewell *et alia* recast natural philosophy as "science"—or, rather, as so many discrete "sciences." The longer-term import of that claim—which went to the very heart of his paper—was that the consequent distortion has been inherited by the "history of science" profession (as of course its name implies). And if that, or something like it, is the case, then the phenomenon of science's imagined pasts entangles us all, no doubt in ways that remain to be discovered. The happy corollary is that attacking this problem should illuminate every corner of our discipline.

[41] Cunningham, "Getting the Game Right" (cit. n. 17), p. 386.