Who Owns the Twentieth Century? (And Is It Worth Owning?)

Stephen G. Brush with Ariel Segal. *Making 20th Century Science: How Theories Became Knowledge*. xx + 532 pp. illus., tables, bibl., index. Oxford: Oxford University Press, 2015. \$39.95 (hardcover).

John Agar. *Science in the Twentieth Century and Beyond*. x + 614 pp. index. Cambridge, UK: Polity Press, 2012. £15.99 (paperback).

The twentieth century enjoys a firm grip on our profession. Well over half the research articles published in this journal since 2000 devote significant attention to the period between the 1890s and the 1990s.¹ Similar trends prevail in other leading publications. But this outpouring of scholarship alone does not create a collective sense of how historians of science should confront the twentieth-century as an epoch. The synthetic reflection that established the scientific revolution as a historiographical category and lent the nineteenth century a sense of cohesion remains to be undertaken for the twentieth.²

A panoramic outlook on more recent historical eras is a pressing necessity. Publishing trends suggest that we will confront future methodological questions and navigate our evolving professional identity largely on twentieth-century turf. What, then, defines twentieth-century science? In some ways, this question is naïve, even trivial: it surprises no one that the currents of history disdain our thin calendrical embankments. But centuries, decades, and other conventional durations often prove useful for collating practices that shared a family resemblance and naming periods about which we might claim that continuity prevailed over discontinuity. Can the twentieth century offer such utility? The books under review can each be read as an attempt to use

¹ Including studies that focus on the final years of the nineteenth century, but excluding articles that extend into the early twentieth century when they focus on earlier decades, 107 of 187 research articles published in *Isis* between 2000 and 2015, inclusive, touch on this time period.

² For a representative example of each, see H. Floris Cohen, *The Scientific Revolution: A Historiographical Inquiry* (Chicago: Univ. Chicago Press, 1994) and David Cahan, ed., *From Natural Philosophy to the Sciences: Writing the History of Nineteenth-Century Science* (Chicago: Univ. Chicago Press, 2003).

the twentieth century in that way, and in so doing to claim it for a particular vision of how the history of science should be done.

Stephen Brush's *Making 20th Century Science: How Theories Became Knowledge* is both an extended argument that scientists value explanatory power over novel prediction when choosing which theories to accept and a plea for careful attention to the content of science in writing its history. Jon Agar, in *Science in the Twentieth Century and Beyond*, details how science connects to "working worlds," the social, cultural, political, and technological arenas that bind science to essentially human problems. If this recalls the split, often considered stale, between internal and external, conceptual and contextual approaches to the history of science, it is no accident. For all our efforts to transcend this distinction—if these works are any guide—it still holds considerable sway over our practice. Apart from their many intrinsic merits, these books present an opportunity to assess how that methodological legacy shapes understanding of the time period that now predominates in the history of science, and to ask what the definitive stories of that era should be. They suggest, I argue, that we still lack a cohesive picture of twentieth-century science and, furthermore, that we can expect it to remain elusive.

A Century of Theory Choice

Stephen Brush's *Making 20th Century Science* confronts one clear question: why have scientists accepted theories? Notwithstanding numerous reception studies of relativity, quantum mechanics, and Darwinism, Brush finds that historians and philosophers neglect this question. Historians, he charges, focus on the genesis of theories, or on the circumstances of their acceptance, without interrogating scientists' rationales for accepting them. Philosophers are too preoccupied with the question of why scientists *should* accept theories to examine how they actually *do*. Brush considers four potential explanations of how a new theory wins scientists' allegiance: it might anticipate hitherto unknown facts (novel prediction); it might provide a compelling account of previously known facts (explanatory power); it might be more beautiful than its competitors (aesthetics); or it might be favored by social, cultural, or professional factors (social construction). The bulk of the book presents, with scrupulous care, a series of case studies that test these candidate explanations.

Before exploring these examples, Brush issues a brisk dismissal of social construction in part 1. He does credit social constructionists for undercutting Whiggishness, promoting a much-

needed emphasis on scientific practice, and exposing some genuine ideological biases. He finds them open to criticism, and their work quite fun to read. But Brush's prevailing sentiment is that social constructionist claims are rarely supported by hard evidence, and that the approach's roots in 1970s counterculture opposition to Cold War technocracy make it ill-suited to a context in which anti-evolutionism and climate change denial, rather than the Strategic Defense Initiative and Agent Orange, dominate public scientific controversy. To understand why scientists accept theories, he contends, we need to inspect the documentary record individual scientists leave, and most of the time it shows that social factors play but a small and limited role.

With social construction shunted aside, Brush turns to the bugbear that bothers him more: novel prediction. The notion that theories are either more likely to be adopted, or more worthy of adoption, when they predict facts not yet known is an old philosophy of science standby.³ It makes intuitive sense. What is surprising about a theory assembled from available evidence explaining facts we already know? The *real* question, the argument goes, is how a theory's untested implications stand up to experimental scrutiny. But Brush, eyeing the documentary record, is skeptical that this surface-level plausibility tells us much about how theories actually won scientists over. The book's three remaining parts scrutinize the physics and chemistry of atom and molecules, the physics of space and time, and evolutionary biology, respectively. Brush concludes that scientists buy theories for explanatory power; novel prediction comes as *lagniappe*.

Brush's treatment of the general theory of relativity (GTR) reflects his general pattern of argument. GTR is often presented as the exemplar of a theory established by predictive success. Arthur Eddington's photographic plates of the 1919 solar eclipse validated the novel prediction about the degree to which the Sun's gravitational field bends passing star light, and cemented GTR's superiority to Newtonian mechanics—or so the story goes.

The confirmation of GTR's light bending prediction was psychologically powerful, made news worldwide, and transformed Albert Einstein into a celebrity.⁴ But the fact that a prediction captured the popular imagination reveals little about why *physicists* accepted GTR. Brush asks how physicists weighted light bending vis-à-vis other evidence, and how much of that weight derived

³ Heather Douglas and P. D. Magnus, "State of the Field: Why Novel Prediction Matters," *Studies in History and Philosophy of Science Part A*, 2013, 44:580–589.

⁴ Marshall Missner, "Why Einstein Became Famous in America," *Social Studies of Science*, 1985, 15:267–291.

from the theory anticipating a previously unknown fact. From physicists' and astronomers' published statements, he concludes that although light bending counted in GTR's favor, it was no more important than other available evidence, and many thought it less so. The theory's explanation of existing anomalies—the forty-three seconds of arc per century difference between Newtonian calculations of Mercury's perihelion advance and observed values, Michelson and Morley's null result, the redshift of spectral lines—often counted for more.

In fact, Brush argues, it is rational that explanation of existing anomalies should attract scientists' theoretical allegiances more strongly because the scientific community has already tried, and failed, to account for them. Novel predictions have not endured rigorous scrutiny. A good skeptic should remain receptive to a better explanation. In an extensive survey of physicists' writings from the 1920s, Brush finds only one who claimed that light bending provided better evidence for GTR because it was a novel prediction. The majority praised the theory for explaining known phenomena, and doing so beautifully. Success accommodating existing evidence emerges as scientists' principal motive for theory choice in most of Brush's examples. The elegance with which it does so often plays a supporting role. Novel prediction rarely, on Brush's analysis, holds special status. Successes like the prediction of new elements from the periodic law draw disproportionate attention, but are the exception, not the rule. And only in rare and aberrant cases, such as Lysenkoism in the Soviet Union, do social factors serve as primary motivators of theory choice.

The examples that establish this pattern cover a noteworthy range, especially considering the depth in which Brush reconstructs conceptual developments. They include Dmitri Mendeleev's periodic law and August Kekulé's theory of benzene's structure, natural selection and molecular genetics, and quantum mechanics and big bang cosmology. Each case mobilizes some mixture of textbooks, which show when a theory enters the canon; review articles, which often explain why a theory is useful; research articles, which, with simple quantitative methods, can reveal what proportion of scientists is using a theory at a given time; scientists' popular writings, which sometimes argue for accepting theories more directly than research publications; and prize citations, which track the prestige associated with theoretical accomplishments. These sources prove apt for answering the question of when and why individual scientists accepted a theory. They nevertheless leave some questions. I will address two: what it means to accept a theory, and the difference between individuals accepting a theory and a community reaching a consensus.

Brush makes a telling observation about declining interest in GTR after the 1920s: “there were no new convincing tests of the theory’s predictions until the development of better technology and advances in atomic physics.... In the meantime, quantum mechanics offered many more research opportunities for physicists” (350). This suggests another reason researchers might adopt a theory: it gives them something to do. C. Kenneth Waters distinguishes between a theory’s *explanatory scope* and its *investigative reach*.⁵ Brush emphasizes the former, but his analysis hints that investigative reach—the range of problems a theory allows scientists to undertake—also influences theory choice.

Quantum theory (chapter 8) is one example. The old quantum theory, represented by the Bohr model of the atom, was “not considered really satisfactory” (220–221) and “was never fully accepted before it was replaced by quantum mechanics” (488). Even so, it inspired copious publications from 1913 until 1926, when quantum mechanics offered a more satisfying alternative. Physicists might have considered the Bohr atom crude, but they needed a tool to hack through the jungle of spectroscopic data generated by new instruments and experimental techniques. Similarly, many scientists rejected the metaphysical implications of quantum mechanics, even as they published work that used the theory, thus accepting it by the standard Brush himself often adopts.

Brush skirts the slippery question of when we can say a scientist has accepted a theory, but it does not bear centrally on his case against novel prediction. Rather, it shows how his argument could have been sharpened by engaging more recent philosophy of science. Brush reprimands philosophers for making ahistorical claims about the potency of novel prediction. But the philosophical picture of scientific reasoning he attacks most forcefully is the hypothetico-deductive method, which is principally of historical interest for many philosophers. He praises social constructionists for highlighting scientific practice, but neglects a similar practical turn in philosophy, attention to which might have allowed him to build a more powerful case that his approach can be useful for philosophers. An agenda proposing “systematic study of scientific

⁵ C. Kenneth Waters, “Shifting Attention from Theory to Practice in Philosophy of Biology,” in *New Directions in the Philosophy of Science, The Philosophy of Science in a European Perspective 5*, ed. Maria Carla Galavotti, Dennis Dieks, Wenceslao J. Gonzalez, Stephan Hartmann, Thomas Uebel, and Marcel Weber (Dordrecht: Springer, 2014), pp. 121–139.

practice that does not dispense with concerns about truth and rationality,” as advocated by the Society for Philosophy of Science in Practice, accords closely with Brush’s mission.⁶

The second question concerns the difference between acceptance and consensus. Brush documents how *individuals* adopted theories. It is one thing to show that individuals, even a substantial group of important individuals, have accepted a theory, but quite another to demonstrate that theory’s consensus status. The latter requires that the theory is no longer competing against other serious alternatives.⁷ If scientists provisionally accept a theory because of its explanatory power, beauty, and/or investigative reach, might novel predictions, which take time to validate, help shut down alternatives and crystalize a consensus? What is the fate of theories that *fail* to produce novel predictions? Take string theory. Although lively and fashionable for over forty years, none of its predictions has proved testable, let alone been tested. This suggests the historical analogue of a gene knockout experiment: asking what happens in novel prediction’s absence might reveal something of its function.

Wanting for predictive success, string theorists turn to explanatory power and aesthetics. They seek to explain the large and powerful dataset telling us that the elements of the standard model of particle physics have the values they have and to resolve the persistent incompatibility between relativity and quantum mechanics. John Horgan crisply summarizes string theorist Edward Witten’s defense of aesthetic power as the claim that string theory is “too beautiful to be wrong.”⁸

The cumulative effects of four decades without an experimentally validated prediction, however, are weighing on string theory. It has suffered vigorous attack from philosophers and other physicists who cite its predictive failure as cause to reject it.⁹ Explanatory power and elegance,

⁶ “Mission Statement,” Society for Philosophy of Science in Practice, accessed July 3, 2016, <http://www.philosophy-science-practice.org/en/mission-statement/>.

⁷ Naomi Oreskes explores consensus formation, and how to identify it, with respect to the scientific consensus on climate change in “The Scientific Consensus on Climate Change: How Do We Know We’re Not Wrong,” in *Climate Change: What It Means for Us, Our Children, and Our Grandchildren*, ed. Joseph F. C. DiMento and Pamela Doughman (Cambridge, Mass.: MIT Press, 2014).

⁸ John Horgan, *The End of Science*, rev. ed. (New York: Perseus, 2015), p. 65.

⁹ Nancy Cartwright and Roman Frigg, “String Theory under Scrutiny,” *Physics World*, 2007, 20(9):14–15; Peter Woit, *Not Even Wrong: The Failure of String Theory and the Search for Unity in Physical Law* (New York: Basic Books, 2006); Lee Smolin, *The Trouble with Physics: The Rise of String Theory, the Fall of a Science, and What Comes Next* (New York: Penguin, 2006).

even within favorable social conditions (in the 1980s and 1990s, some of the most high-profile physics departments became dominated by string theorists), have not secured consensus for string theory. Novel prediction might rarely be a primary motive for individuals to accept theories, but it does appear that its absence can inhibit consensus, indicating that it might have more import for communities than Brush allows it for individuals.

The above considerations do not detract from the remarkable achievement this book represents. Brush commands the technical details of a wide range of sciences and musters an impressive array of sources to answer to a clear question convincingly, and with only very few minor factual slips. The book's typographical errors, in contrast, are numerous. Oxford University Press has apparently done only cursory copyediting, leaving the text riddled with extra or missing spaces, misplaced punctuation, and, on occasion, typesetting errors that obscure the author's meaning. The confusion of "l" and "1" in Brush's discussion of the Pauli exclusion principle, for instance, leads to the absurdity "*l* may be either 0 or 1" (229). Such slipshod production is surprising from a press so esteemed as Oxford, whose authors deserve better.

Production aside, the book is a monument to scrupulous scholarship. As such, it advances an implicit argument about what it means to do the history of twentieth-century science. The twentieth century as a category, in fact, plays almost no role in Brush's exposition beyond the book's title. The bulk of the discussion concerns the period between the 1860s and the 1930s, with a few brief sorties into the Cold War. It offers scant defense of the twentieth century as a useful historiographical category. It does offer a vision of the history of science as responsible for the content of science, which allows us to ask questions that fall unambiguously and nearly uniquely within its remit and answer them by substantiating clear, causal historical explanations. It is field with a distinct identity. Like the best of the theories Brush discusses, it gives researchers an unmistakable mission.

Working with Working Worlds

Whereas Brush pays little heed to the twentieth century's temporal boundaries, Jon Agar addresses them explicitly in *Science in the Twentieth Century and Beyond*. The book's motivating conceit is to follow forward the threads that pass through a temporal plane drawn at 1900. These include many of the topics Brush addresses: molecular structure, relativity and quantum mechanics, and

evolutionary biology. But Agar ventures beyond the best-known stories from the physical and biological sciences. Breadth is the book's great strength. Alongside familiar accounts of chemistry, physics, and biology, it engages psychology, the social sciences, geology, operations research, virology, and other fields that might seem unjustly peripheral when placed beside more dominant narratives. In this epic work of synthesis, Agar makes a strong case that their stories are just as representative of twentieth-century science as the better known narratives of more visible fields.

The topical range Agar covers compounds the challenge of identifying a theme that can braid a sturdy narrative from the threads he follows across such a span of time. Agar settles on "working worlds," which he defines as "arenas of human projects that generate problems" (3). Working worlds are the realms of human activity we find when we ask whose interests science serves and why. The book unfolds roughly chronologically, articulating its examples in terms of some of the key working worlds science served throughout the twentieth century, most prominently those of agriculture, warfare, commerce, and medicine.

Agar begins by noting that the early 1900s witnessed foment in the physical, life, and human sciences, and so is a useful starting point. Part 1, after introducing the working worlds framework, traces the development of quantum theory and relativity, experimental genetics, and psychology and immunology, which aspired to emulate the successes of physics and biology. In each case, Agar emphasizes what might be called "external" factors. The new physics both sprang from and contributed to global communication and transportation networks. Genetics was of special interest to the agriculturalists tasked with feeding ballooning populations. The sciences of the mind were put to work classifying those populations, sorting schoolchildren and soldiers into categories validated by a battery of new psychological tests. When Agar sees politics, technology, and ideology, however, he does not see external forces buffeting science from without, but integral components of its function. Working worlds are features of science, not bugs.

Part 2 addresses World War I and its aftermath. Wartime demands forged strong ties between government, industry, and academia, raising the stock of physics and chemistry as they showed how their expertise could advance practical ends. Following the war, science responded to the instability of the Weimar Republic in Germany (chapter 6), hastened industrialization in the United States (chapter 7), and intertwined with the resources and demands of the United Kingdom's global empire (chapter 8). Agar suggests that Marxism guided the development of

interwar biology, both in the Soviet Union and among scientists with leftist sympathies elsewhere (chapter 9). He traces how ideology directed science in Nazi Germany—toward ends both applicable and not (chapter 10). He considers the birth of big science in the interwar period and the political and technological circumstances that made it possible (chapter 11).

The contrasts with Brush's approach should be coming clear. Brush would bristle at Agar's suggestion that the modern evolutionary synthesis was a triumph of a Marxist materialism. Brush's own analysis of why geneticists accepted natural selection rests on reconstructing how numerous lines of evidence gradually convinced key individuals that selection, not genetic drift or saltation, best explained the available data. He trusts in the series of textbooks the principle architects of the synthesis wrote, often in successive versions that chronicled shifting theoretical allegiances, from the 1930s through 1970. Agar, however, cautions, "if we understand the Evolutionary Synthesis to be just the publication and reception of texts, then we miss something very important" (208). Namely, we miss the working world of plant breeding and a potent materialist ideology. The difference is not one of simple emphasis; it is a foundational disagreement about the types of arguments and evidence that are acceptable when we substantiate historical claims.

That difference is also evident in these authors' discussions of relativity. Brush's emphasizes the rational processes scientists used to decide whether to accept the theory, as discerned through their writings. Agar turns to Peter Galison's depiction of Einstein's technological context.¹⁰ The suggestion that the prevalence of rail travel, and the clock synchronization procedures that it required, influenced a young patent examiner fits neatly with Agar's working worlds framework. But the precise weight we should actually place on sociotechnical factors when assessing their relevance to the genesis and adoption of relativity is a question Galison himself holds at arm's length. If sociotechnical forces do indeed propel theory genesis and choice, then they are often occult forces so far as cause-seeking historians like Brush are concerned. They might feature in a tableau containing suggestive sympathies, but documentary evidence that can demonstrate their workings is scarce.

Through part 3, examining developments during World War II and the Cold War, Agar's focus remains on the type of historical correspondences that rarely appear in scientists' writings. The working worlds theme leads him to downplay the discontinuities many historians have seen as

¹⁰ Peter Galison, *Einstein's Clocks, Poincaré's Maps: Empires of Time* (New York: W. W. Norton & Co., 2003).

definitive of World War II research. The relevance of science to military ends is a major continuity. Although the Manhattan Project represented science on a new scale, it grew from links between science and military needs forged during World War I and built on big science initiatives begun in the interwar era. Similarly, on Agar's account, industry-driven developments in computing and pharmaceuticals connected to the working world of Cold War economy and mirrored similar connections between science and early twentieth-century industries.

Agar's account of the diverse scientific enterprises that proliferated after World War II continues into part 4, on the science of today. Individual examples in these sections are necessarily brief, and connections to working worlds less direct and detailed. Later chapters struggle to contain a cornucopia of examples, all relevant, but also calling for elaboration. Chapter 17, on the long 1960s, touches on anti-nuclear sentiments, Rachel Carson's *Silent Spring*, demonstrations against university-hosted military research, scientific entrepreneurship, connections between social crises and Kuhnian crises, the birth control pill, cybernetics, China's cultural revolution, sociobiology, and the Vietnam war. This and other late chapters, although sometime discursive, are nonetheless edifying insofar as they bring historiographical strands that rarely meet into conversation.

Agar's summary reflections identify four major themes of twentieth-century science. The first is the relevance of working worlds, particularly those of warfare. Second is the transition of scientific power from Europe to the United States. Third is the insistence that these trends should not overshadow the missing stories: failed theories, the non-Western world, and science conducted under the veil of secrecy all invite further study—and I would add solid state and condensed matter physics, materials science, and evolutionary developmental biology as narratives largely absent from Agar's account, and most others, that might yet feature prominently in the twentieth-century story. Finally, Agar identifies the shift of preeminence from the physical sciences in the early twentieth century to the life sciences in its closing decades as his fourth theme. Looking ahead, he singles out biomedical engineering, technology transfer, elementary particle physics of the type conducted without large accelerators, exoplanet research, and climate science as current threads that a historian of twenty-first century science might well decide to follow forward. Agar's closing line reflects his suspicion that future historians might conceive of the twenty-first century in ways similar to how he has understood the twentieth: "The conflict between the working worlds of

energy-intensive societies, international environmental governance and global commerce look set to shape the twenty-first-century sciences” (530).

Agar hopes the working worlds concept will bring an unruly and diverse set of narratives to heel. What does it offer that more staid terminology does not? Agar senses that “context” has become a cliché “in George Orwell’s sense of clichés as worn ways of writing that were once alive and are now dead” (3). Gesturing to context, Agar suggests, which once made historians sensitive to oft-neglected political, social, cultural, and technological motivators of scientific change, now lures us into lazy, ambiguous thinking about how science interconnects with those same factors.

But the ambiguity that plagues “context” also affects “working worlds.” First, working worlds include a similarly heterogeneous range of phenomena and scales. “Global change emerges as a dominant working world in the late twentieth and early twenty-first century,” Agar writes (484). On the median scale we see working worlds of industry, warfare, agriculture, and medicine. We also encounter local working worlds, such as those of electronics manufacture, coordinated clocks, and the horse-borne official. It stands to reason that human problems occur on and across many scales, but Agar does not adjust for resolution when he attaches his key term to different types of activities, leaving readers to ponder whether some important scale differences are missed.

Second, working worlds encompass various *types* of interactions. They provide impetus for research, furnish funding and institutional support, supply metaphors for scientific concepts, put knowledge and know-how to work, shape the content of science, direct emphasis to particular questions, justify research, and generate their own sciences. This collection of qualitatively different types of interaction would have benefitted from unpacking. Perhaps context as a metaphor has grown tired, but the alternative to metaphors growing clichéd is that they become straightforward components of our lexicon, their metaphorical resonances decaying into simple, commonly understood meanings. Context, as a workaday word, is ambiguous, but its ambiguity signals the need for more detailed exposition. The terminology of working worlds, on the other hand, sounds specific, even when it is anything but. As a consequence, the working worlds concept often preempts the impulse to describe the relationship between science and the arena of human problems in more explicit terms. The remedy for a weary scrap of cant has side effects almost as irksome as the condition it treats.

The limitations of working worlds illustrate the difficulties of developing a cohesive account of twentieth-century science, and although the term has its problems, it is fresh. It forces readers to think about well-known stories in new ways, validating the adage that the worst coinage is better than the best cliché. However protean, the working worlds concept focuses attention on the impressive variety of ways in which twentieth-century science was deployed in service of technical, economic, military, and social needs. I know of no other attempt to synthesize such a wide array of scientific and technical practices into a single narrative. That alone is a worthy contribution to our understanding of twentieth-century science.

Is the Twentieth Century Worth Owning?

These divergent pictures of twentieth-century science bring old methodological squabbles to new turf. One presents the twentieth century through the eyes of a historian asking one well-defined question about science's conceptual development. The other is the kaleidoscopic image generated by relaxing the boundaries of science and considering the myriad ways it connects with other features of human existence. These accounts imply very different answers to the critical questions of how we should delineate the scope of the history of science and how we should argue it. For Brush, the field remains coherent by engaging with the structure of scientific knowledge and the processes by which it develops, and we can document clear historical causation with careful attention to the written traces scientists leave. In Agar's more expansive view, science overlaps, often seamlessly, with technology and medicine, and establishing cause is not so much the historian's goal as is demonstrating connection.

Each of these books succeeds, and impressively so, in a narrower project, but they do not, singly or together, offer a cohesive picture of twentieth-century science. Each makes the shortcomings of the other evident. Against the rich contextual backdrop Agar provides, Brush's dismissal of social factors seems too quick. Even if we remain skeptical about the possibility that patent applications for clock synchronization devices catalyzed the genesis of relativity, or of Paul Forman's insistence that the Weimar Republic's instability shaped interpretations of quantum mechanics (an argument Agar also invokes),¹¹ we still have to confront the military and industrial

¹¹ Paul Forman, "Weimar Culture, Causality, and Quantum Theory: Adaptation by German Physicists and Mathematicians to a Hostile Environment," *Historical Studies in the Physical Sciences*, 1971, 3:1-115.

needs that directed attention and resources to particular sectors of science. Brush might claim that these contingencies are tangential to the question of how and why individuals accepted theories, but they remain critical pieces of the larger history that explores why those theories were available to be accepted at particular places and times, and we can appreciate that importance without making deep assumptions about the epistemic status of scientific knowledge.

Reading Agar alongside Brush reveals the value of careful attention to conceptual details. Agar largely eschews technical content, sometimes leaving an incomplete picture of why science unfolded the way it did. His book is more approachable for it, particularly to the undergraduates who might encounter it as a textbook. However, essential terminology often appears with little effort to explain it to the uninitiated, making it more likely to confuse than clarify and leaving the impression that the precise nature of the evidence available for scientific claims was relatively immaterial. Agar's otherwise edifying account in chapter 8 the petroleum industry's hastening of geophysical research, culminating in plate tectonics, for example, does not discuss the evidence that led geologists to accept plate tectonics, leaving readers wondering how it happened.¹²

These poles, the history of concepts and the history of contexts (or working worlds), reflect our methodological legacy, but they are not the whole picture. Much historical work has sought to unify conceptual and contextual approaches in order develop more complete accounts. Might this approach also provide a coherent picture of the twentieth century? I remain skeptical. Brush's book is indeed cohesive. But it uses the twentieth century little either as a temporal or historiographical category. Agar, in an attempt to give the twentieth century historiographical meaning, develops an analytical category so broad as to have little definitive about it. Any attempt to combine these approaches in a way that captures the diversity that Agar so aptly portrays is liable to either break the temporal bounds of the twentieth century beyond reason or be forced into assessments so general that they fail to tie it together.

The enormous increase of the size of the scientific community throughout the twentieth century suggests that we will have to develop new ways of thinking in order to appreciate it.¹³ And doing so might mean focusing on narrower temporal units such as the Cold War, where useful

¹² An exhaustive account is available in Henry R. Frankel, *The Continental Drift Controversy*, 4 vols. (Cambridge: Cambridge Univ. Press, 2012).

¹³ See David Kaiser, "Booms, Busts, and the World of Ideas: Enrollment Pressures and the Challenge of Specialization," *Osiris*, 2012, 27:276–302.

synthetic work has already been done.¹⁴ The twentieth century might indeed be too complex and varied to be worth owning. We would do better to consider it piecemeal, and to capture its diversity with a corresponding multiplicity of methodologies. Although genuine methodological integration is hard to achieve, we have much to gain by supporting multiple, parallel approaches to the complex and multifaceted world of twentieth-century science.

These books, individually, do the profession a great service. *Making 20th Century Science* is certain to become the definitive history of scientific theory choice. *Science in the Twentieth Century and Beyond*, by reaching beyond the core narratives of twentieth-century science, establishes an exciting agenda for future research. Together, these books do us an additional service by demonstrating the inadequacy of the methodological legacy we have inherited for taming the twentieth century.

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¹⁴ Audra Wolfe, *Competing with the Soviets: Science, Technology, and the State in Cold War America* (Baltimore: Johns Hopkins Univ. Press, 2012).