

Book Reviews

REVIEW ESSAY

The History Manifesto. By Jo Guldi and David Armitage. Cambridge: Cambridge University Press, 2014. Pp. x, 165. \$45.00, hardcover; \$19.99, paper.
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The History Manifesto [HM] is a call to arms for historians urging them to take on social scientists, tackle big questions, and master “big data” in order to return to the tradition of the *longue durée* as famously practiced by the Annales school. This is a worthy aim. And this short, engagingly written book will no doubt successfully inspire many young historians to do just that. Nevertheless HM is a deeply flawed book. Despite its promise, I cannot recommend that it be taken seriously either as an account of the relationship between history and the social scientists in recent decades or as a plan of action for historians in the future.

The ambitions of HM are twofold. On the one hand, it is an argument for historians to re-engage in more wide-ranging and ambitious historical work of the kind practiced by *Annalists* such as Marc Bloch and Fernand Braudel. The authors claim that historians are uniquely equipped to assess long-run trends due to their special skills in handling and interpreting evidence. On the other hand, the book also calls for political engagement by historians. Unfortunately, the latter objective mars much that is valuable in their call to historians. Like many economic historians, I am also critical of the narrowness that is characteristic of much academic history. Unfortunately, however, the problems of HM, including conceptual and factual sloppiness and partiality in engaging the research of other historians, prevent it from realizing its promise. It is precisely the political engagement they call for that leads them astray in their treatment of other scholars.

The History Manifesto comprises four chapters. Chapters 1 and 2 are the best. They provide a potted history of the rise of the *longue durée* and how the authors’ so-called Short Past came to dominate history departments after 1968. This account has been challenged by other reviewers (Deborah Cohen and Peter Mandler, “*The History Manifesto: A Critique*”, *American Historical Review*, forthcoming) but Guldi and Armitage’s version of the rise of detailed micro-history—the result of scholars spending years mastering previously untapped archives—rings true, at least to an outsider. As the authors explain, the shift towards micro-history has been fruitful, but it has also come at a cost. As historians abandoned ambitious Braudelian long-run studies, they ceded their influence in policy to either journalists working in think tanks or to other social scientists, notably economists who wrote what the authors call “dirty *longue durée* history.”

In Chapters 3 and 4, Guldi and Armitage focus on contributions of historians to current debates on inequality and climate change. Their aim is to carve out a conceptual space for historians on these important policy questions. This requires them to enter areas of research in which they are outsiders; a difficult task, but one that is essential for their case that historians are uniquely equipped to contribute to these fields. Unfortunately, this advocacy is a resounding failure, as the job of synthesizing research in fields outside history is one for which the authors appear signally ill-equipped. Criticizing a book that calls itself a “manifesto” on grounds of factual accuracy or scholarship risks accusations of pedantry. Still, this book celebrates the virtues of historians, among them careful scholarship. This virtue itself is too often absent in this book.

Chapter 3 deals with inequality and particularly the relationship between capitalism and inequality in the nineteenth century. It draws a distinction between economists, for whom the “numbers demonstrate conclusively that capitalism banished inequality during the nineteenth century, and could do so again,” and historians, who because of their greater sensitivity to historical evidence are attuned to the persistence of poverty through the nineteenth century (p. 58). Unfortunately, as the anonymous blogger Pseudoerasmus has documented, the “American economists” (p. 59) who concluded that the heights of British criminals rose in the nineteenth century are, in fact, Australian and British economic historians, and they actually found a decline in height during the nineteenth century (<http://pseudoerasmus.com/2014/11/10/history-manifesto-errors/>). So the entire paragraph summarizing the difference between historians and economists on the standards of living debate turns out to be false. It is false in its particulars and false in the impression it conveys as it does not acknowledge that the topic of the living standards during the British Industrial Revolution remains a fiercely debated subject not between economists and historians but among economic historians. Contrary to Guldi and Armitage’s depiction, economic historians have accumulated evidence to support pessimistic interpretations of the living standards during the Industrial Revolution up to the 1840s. Real wage data support more optimistic conclusions from the 1850s onwards (e.g., Charles Feinstein, “Pessimism Perpetuated,” this *Journal* 58, no. 3 [1998]: 625–58). Moreover, their summary bears no resemblance to the actual scholarly debate that took place among economic historians from the 1980s through to the 2000s in which evidence rather than ideology actually played a role in shifting scholarly opinion.

If this description of the standard of living debate fails to make a sympathetic reader doubt Guldi and Armitage’s ability to accurately summarize topics outside of their field, they follow it up with statements such as: “The data from economics tends to take one aspect of economic experience—wages, the price of grain, or height—and interpret them as a proxy for freedom, democracy, or happiness” (p. 59). Again there are numerous citations in the corresponding endnote, but not one of the papers cited uses the price of grain as a proxy for freedom, democracy or happiness. Nor can I think of a reason why anyone would. A paragraph unsupported by citations asserts: “Historians no longer believe in the mythology that the world was shaped dominantly for the good of economic well-being by the influence of western empire, but many economists still do” (p. 55). It is difficult to know who the authors are thinking of since they name no names. The most prominent defender of the British Empire is a historian: Niall Ferguson (*Empire: How Britain Made the Modern World*. London: Allen Lane, 2003). And while economists and economic historians have been busy gathering new data about the impact of colonialism it is difficult to think of a single prominent economist who takes the position attributed to “many economists.” To the contrary, examples abound of detailed, evidence-based research on the legacy of colonial empires, including those by Lakshmi Iyer (“Direct versus Indirect Colonial Rule in India,” *Review of Economics and Statistics* 92, no. 4 (2010): 693–713) and Melissa Dell (“The Persistent Effects of Peru’s Mining *Mita*,” *Econometrica* 78, no. 6 (2010): 1863–903).

The common thread linking many of these misconceptions and mistakes is ideology. Historians are celebrated. Economists and other social scientists are generally presented as either naive number-crunchers or outright villains. Therefore Richard Tawney, Eric Hobsbawm, Eric Williams, and other scholars they respect are historians rather than economic historians, while Gregory Clark, Simon Kuznets, and the other economic historians they dislike are economists and, as such, ideologically suspect.

The only thing worse than an economist is a “neoliberal” or “Cold War” economist. When we are told that Kuznets was a “Cold War economist” the implication seems to be that Kuznets’s research on inequality was politically motivated. Such a reading cannot be accepted by anyone familiar with either Kuznets’s presidential address on inequality or the body of Kuznets’s work in general. Many of his claims were speculative, and he was emphatic in cautioning against drawing too many inferences from the limited data at his disposal, admitting that it “came perilously close to pure guesswork” (“Economic Growth and Income Inequality,” *American Economic Review* 45, no. 1 (1955): 1–28, quote on p. 6). This political name calling is a barrier to genuine understanding. Following a discussion of Clark’s emphasis on Malthusian factors in *A Farewell to Alms* (Princeton: Princeton University Press, 2009), the authors tell us, “When neo-liberal economists measure one factor over time not many, they are involved in speculation not long-term thinking” (p. 60). However, it seems for the authors that when other economists measure “one factor over time” the results are ground breaking, as in the case of Thomas Piketty’s *Capital in the Twenty-First Century* (Cambridge: Harvard University Press, 2014) which they hail as exemplifying “the power of relevant historical studies, driven by data, to speak to policy and publics well beyond professional history” (p. 81).

What justifies this animus? Guldi and Armitage claim “Modern economists have removed the picture of an abusive God from their theories, but their theory is still at root an early nineteenth-century one, where the universe is designed to punish the poor, and the experience of the rich is a sign of their obedience to natural laws” (p. 109). I am unaware of any documented relationship between the Book of Job and classical economics. But apparently since economics is tainted by some kind of original sin, the authors of *HM* are allowed to dismiss it. Perhaps the most amusing statement is a quote they take from Geoffrey Hodgson saying that modern economics “has neglected the problem of causality” (p. 110)—a statement that might provoke outright laughter in many economics departments where countless seminars have been derailed precisely because of the obsession of some economists with identifying causal claims.

So political prejudice and perhaps an unfamiliarity with the terrain means that Guldi and Armitage do not mention some of the biggest recent debates in economic history, many of which exemplify the virtues of *longue durée* history that they celebrate elsewhere in the book. This is both a shame and a missed opportunity. Though they cite David Graeber and Piketty as exemplars of such long-run historical thinking, they could also have mentioned the numerous recent ambitious and wide-ranging works by prominent economic historians such as Claudia Goldin and Lawrence Katz (*The Race Between Education and Technology*. Cambridge: Harvard University Press, 2008), Robert Allen (*The British Industrial Revolution in Global Perspective*. Cambridge: Cambridge University Press, 2009), Timur Kuran (*The Long Divergence*. Princeton: Princeton University Press, 2010), and Deirdre McCloskey (*Bourgeois Dignity*. Chicago: University of Chicago Press, 2010), but they choose not to. The implication seems to be that either they are unaware of these works or they think that they do not belong to the tradition that they associate with Braudel and Tawney.

These problems get to the heart of what *HM* is about. If historians are to play an important role in shaping attitudes towards inequality and climate change they must come to grips with what other social scientists say on these issues. Guldi and Armitage’s

failure to do so meant that, while they jumped on the Piketty bandwagon, they missed the biggest debate in economic history in the last 15 years. This is the debate surrounding Kenneth Pomeranz's *Great Divergence* (Princeton: Princeton University Press, 2000), which claimed that China possessed as developed an economy as Europe until the eighteenth century. If nothing else the large number of articles and books that followed in its wake testified to both the interest economic historians have in big picture topics and their ability to systematically marshal empirical evidence so as to test hypotheses and arguments against data. (Bizarrely, the authors cite Pomeranz on a topic completely unrelated to the topic of his book.) Economic history therefore possesses a model for historians to engage in the *longue durée* but it is a model that *HM* ignores.

The comparative advantage of historians, perhaps their greatest virtue, is their commitment to scholarship: their appreciation of where data comes from and how to deal with conflicting sources of information. This is an area where economists and other social scientists have much to learn from historians (and economic historians) as they are all too often happy to download whatever data are available on the internet without exploring its provenance and the biases it might embody. Unfortunately, while *HM* pays lip service to these virtues, the authors themselves too often treat their material in a slipshod way. The result does little to point the way for historians to engage in the history of the *longue durée*.

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ANCIENT TO MODERN EUROPE

The Power of Market Fundamentalism. Karl Polanyi's Critique. By Fred Block and Margaret R. Somers. Cambridge, MA and London, England: Harvard University Press, 2014. Pp. 312. \$49.95, hardcover.
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Scholars commonly explicate the work of significant thinkers by one of two methods: the first is to deploy close textual analysis to pin down what they actually said and meant; the second is to interpret texts as living documents relevant to issues of current concern. This book falls in the second class. The authors argue that Polanyi developed the concept of “socially embedded” or “socially constructed” market economy as a critique of the libertarian claim that markets are naturally self-regulating entities best left to do their work unimpeded by government intervention. They term this view “market fundamentalism”; Polanyi called it utopianism. This theme is developed through a brief sketch of Polanyi's life in Vienna as a politically engaged financial journalist and subsequently in England and America as an uncredentialed academic refugee, and by analyses of historical arguments developed in his one major work *The Great Transformation*. The final chapters consider these ideas in light of the pronounced turn toward market-based solutions to social problems in the United States and the economic crisis following the crash of 2008.

Polanyi's vision of the logic of economic organization rested on the belief that attempts to remove politics from markets to muzzle the power of “special interests” betray an unrealistic and ultimately self-defeating understanding of how economies actually