

Li has published extensively in both English and Chinese on collecting and connoisseurship in the Ming-Qing period and on the literature of the time more broadly. This volume brings the two together seamlessly, framed by questions about how the human and the material interacted and how things still acted as sources of meaning even when they outlasted the world to which they belonged (for instance the vanished Ming, in early Qing memory) or when they themselves were gone, surviving only in memory or in writing.

The Cambridge Economic History of China. Volume II: 1800 to the Present

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This volume of *The Cambridge Economic History of China* covers roughly the last 200 years—a huge undertaking, although still smaller than the task of the first volume (which covers everything before 1800). Roughly two-thirds of the volume covers the period 1800–1949; most of the last third covers the Maoist period, although two essays are primarily concerned with events since 1978. Like all the Cambridge Histories, it is primarily a reference work, prioritizing synthetic overviews of what we know about various topics over strikingly new interpretations based directly on primary sources, or a single consistent interpretation spanning all the chapters. As such, it will probably be most useful to people seeking a general orientation to the topics covered: faculty and graduate students focused primarily on some other time and/or place, or on some different aspect of late imperial and modern Chinese history. Meanwhile, the editorial choices about what to cover and how are useful for thinking about the state of Chinese economic history as a sub-field: a sub-field which (like economic history generally) has since the “cultural turn” of the 1980s periodically been proclaimed to be disappearing. In fact, the field’s productivity has been so great as to make it hard to cover, even in twenty-one substantial essays.

The editors mostly opted for thematic essays, with a few (mostly post-1949) chronological ones. The volume includes very little on what we might call the penumbra of economic history: topics such as environment, labor history, and demography and migration that have obvious, material, links to economic activity. The only two chapters on specific sectors of the economy are a very helpful chapter on pre-1949 agriculture (by Debin Ma and Peng Kaixiang) and another good one on industry, including both handicrafts and mechanized production, by Linda Grove and Tōru Kubo; there is none on any specific sector—such as steel, housing, or textiles—for the period

My thanks to Jacob Eyferth for comments on an earlier draft of this review. I am, of course, solely responsible for any errors.

after 1949. The striking exception is deep coverage of the pre-1949 financial sector, including essays on “Money and the Macro-economy” (Dan Li and Hongzhong Yan), “Public Finance” (Elisabeth Kaske and May-li Lin) and “Financial Institutions and Financial Markets” (Brett Sheehan and Yingui Zhu). There is also very little on questions of distribution/inequality, the construction and decline of various social safety nets, or China’s frequent disasters, with the exception of a chapter on the Great Leap Forward by James Kai-sing Kung and some attention to distribution in Chris Bramall’s chapter on Mao-era living standards. Even a very fat volume cannot do everything.

Economic ideas and their instantiation in political-economic institutions receive much more attention, beginning with an opening essay by Debin Ma that argues for the centrality of ideological changes between roughly 1800 and 1937 for understanding economic performance over the long run. In Ma’s view, this is essentially a two-part story: a liberalizing phase, driven largely by foreign influences, from roughly 1842 to the 1920s, followed by a fateful turn towards a command economy as the Depression and war increased the appeal of central planning and of investment in heavy industrial sectors far from China’s comparative advantage.¹ As a very rough first approximation, this is hard to argue with, although nationalism also played a crucial role in reformist thinking and statecraft even before 1927, and often encouraged some state-led efforts in heavy industry, even by governments that were, simultaneously, strongly influenced by pro-market liberalism. More importantly, Ma probably overstates both the extent of Qing intellectual repression and (even more) its significance for economic growth, as suggested by William Rowe’s essay on “Economic Transition in the Nineteenth Century,” and some of the essays in Volume I. Rowe emphasizes the demographic and environmental pressures that China faced by the late eighteenth century; moreover, he suggests that if the economy underperformed its possibilities in the ensuing century (and Rowe is quite cautious about the extent of any actual economic decline), the explanation lay not in effective repression but, to the contrary, in “the declining leadership of the imperial state” (85). While also noting that some domestically generated reform efforts began well before 1842, Rowe sees no long-standing pattern of highly consequential government meddling, nor of anti-market policies even remotely comparable to what would come later. And if we look beyond the temporal focus of Rowe’s essay to the second half of the nineteenth century, it is hard to see how any government repression of the economy (which did, to be sure, exist) could possibly have mattered as much to the economy as the government’s inability to prevent the eruption of multiple, lengthy civil wars, suppress endemic banditry, or cope with a succession of unusually severe droughts and floods.²

¹The principle of comparative advantage holds that a country should specialize in those things in which it is most efficient relative to its potential trading partners. Thus, if country A can produce wheat at one third of the price that B can, and steel at half the price, it should import steel from B (even though it can produce that steel more cheaply), and divert the resources it would have used to make that steel into growing more wheat that it can sell to B. Arithmetic can show that—in a frictionless world of perfectly fungible resources—this makes both A and B better off. Note, however, that (in addition to other simplifications) this assumes a world in which A and B might go to war (so that both countries would want domestic sources of both steel and wheat), and that, in this simple version, comparative advantage ignores ways in which “learning by doing” might make a country more efficient at producing something in which it initially has a comparative disadvantage.

²The bibliography here is almost endless, but I would particularly note Xia Mingfang’s observation that the death toll from natural disasters alone in the last five (or in a different estimate, seven) decades of the Qing exceeded by more than ten times the total number for 1644–1796. See Xia Mingfang, *Minguo shiqi*

For the most part, however, the other pre-1949 essays in this volume align not with Rowe, but with Ma. Some mention, for instance, low levels of public spending on education in the Qing and Republic, but things like the poor state of late Qing and Republican roads and even the state's frequent inability to suppress domestic bandits and foreign invaders are largely taken for granted, rather than analyzed either for their causes, or the size of their impact; certainly, such problems of "under-government" garner less attention than rent-seeking and repression by state actors. The volume's overall emphasis on an invariably overbearing state as an obstacle to growth stems, it seems to me, neither from China's long history of illiberalism, nor from Qing realities. Rather, it is rooted in a long-standing habit of reading backwards from the era of Mao—and now also from the era of Xi Jinping. Because this optic is crucial to the volume, this review will follow an unconventional course, jumping forward to look at the post-1949 essays before returning to some other aspects of how the volume treats the pre-1949 period.

Concerns about the likelihood that Xi's increasing authoritarianism will stunt future growth is most explicit in the volume's concluding essay, "China's Great Boom as a Historical Process" (by Loren Brandt and Thomas Rawski). Brandt and Rawski make a similar claim to Ma's about long-term, and economically crippling, continuities deriving from a state that lacked constitutional checks and that insisted on ideological orthodoxy—although the essay makes that case only briefly (777–79), since it is largely focused on the post-1949 era. Chenggang Xu goes furthest in insisting on the trans-historical centrality of a repressive state. His essay "The Origin of Chinese Communist Institutions," argues for "institutional genes" that have given Chinese political economy a basic sameness going all the way back to the beginning of the imperial era over 2,000 years ago, but he produces very little evidence for these huge claims, relying heavily on citations to his as-yet-unpublished book. Other essays that focus on big political-economic structures—Dwight Perkins on the "China's Struggle With the Soviet Growth Model, 1949–1978," Barry Naughton on "The Chinese Economy in the Reform Era," and the post-1949 part of Brandt and Rawski's "Great Boom" essay—also look back from a PRC (indeed, mostly early PRC) baseline, but they are empirically richer than Xu's and more attuned to the ways in which institutions did evolve over time, and they provide more information on actual economic performance under these institutions.

It is noteworthy that, despite this emphasis on the costs of a heavy-handed state, the volume seems to me quite balanced in its treatment of the Maoist era. Many of the essays broadly agree that China's performance in reducing mortality and morbidity and raising literacy levels was impressive; that basic physical security rose substantially after the nineteenth or early twentieth centuries (though with some spectacular interruptions); and that the early People's Republic built quite a bit of physical infrastructure, albeit often in places that made more sense from a military point of view than an economic one. However, there is also a consensus that overall productivity stagnated and consumption levels rose little, two factors indicating an economy that (given rising health, population, and literacy, and improved infrastructure) was drastically underperforming its potential on the eve of the post-Mao reforms (724, 797).

Chris Bramall's essay takes issue, to some extent, with the volume's generally pessimistic view of living standards under Mao. Bramall argues (against several other studies, and using new data) that average nutritional levels did rise, and that both intra-urban

ziran hai yu xiangcun shehui (Beijing: Zhonghua shuju, 2000), 78–79, 400–402. Estimates for the death toll of the mid-century civil wars vary widely but were certainly in the tens of millions.

and, especially, intra-rural income distribution became more equal (626–36).³ However, he does not contest that rural consumption beyond food improved little, with the result that the urban/rural divide in living standards was quite wide (636); nor does he doubt that what gains there were relied in large part on big increases in labor inputs, while productivity (as measured either per unit of labor or as total factor productivity) remained stubbornly low, as Brandt and Rawski emphasize (795–97). Indeed, Dwight Perkins' chapter shows that even in the cities, which were greatly privileged relative to the countryside, real wages essentially stagnated in the 1960s and 1970s; urban living standards did rise, but almost entirely because labor inputs per household rose, as women went from being less than 15 percent of urban employees in 1957 to almost half by the end of the Maoist period (589, 591). The lack of improvement in output per labor hour is particularly striking considering that industrial output per urban resident multiplied by almost seven times. (In addition to more urban people working for more hours, there was a significant reallocation of urban workers from services to industry.)⁴

The inescapable overall conclusion is that Maoist China raised its *potential* output quite a lot by developing both human and physical capital, but it squandered much of that potential (and the suffering that went into creating it) through heavy-handed repression and misallocation of resources. That conclusion provides a good baseline for the volume's last two essays. Both Naughton and Brandt and Rawski look at the massive gains that occurred when at least some of the state's counter-productive efforts were unwound after Mao's death. But the understanding of Maoism as having substantially increased China's potential material welfare, even though it failed to actualize most of that potential is in some tension with the volume's overall practice of using 1949–1978 as a lens that reveals how the late imperial economy was also greatly held back by a meddling state; for in the Qing, resource shortages (including gaps in technological knowledge) created far larger problems than any misallocations of the resources that were available.

Not surprisingly, repression and government errors stand out especially starkly in James Kai-Sing Kung's essay on the Great Leap famine—for the period 1958–61 constitutes an enormous and indisputable exception to any claim that Maoism at least ensured basic subsistence security for a growing population. Kung's approach is to review the wealth of studies that have attempted to measure quantitatively the many factors that may have contributed to this catastrophe, adding some regression analyses of his own, based on relatively new data disaggregated to county levels. This is very helpful

³Like most scholars of inequality, Bramall uses the Gini coefficient to provide a single summary measure—one that aggregates all observed units and compares the distribution of income (or sometimes wealth) to a theoretical world in which every unit was exactly the same (yielding a Gini of 0.00) and to one in which a single unit got all the income (yielding a Gini of 1.00). In this case, that means that an “intra-urban” Gini collapses into one measure both the inequalities between, say, wealth and poor residents of Shanghai, and those between residents of Shanghai and those of, say, Shijiazhuang. At least until recent years, it was usually the case that inequalities within a single PRC city were fairly modest by international standards, while those between cities were often strikingly large. The same was true of inequalities within and between different rural communities. Urban versus rural inequality has, however, been consistently high. For a book that usefully disaggregates inequality within cities and between cities, though mostly for the period after Bramall's, see Wang Feng, *Boundaries and Categories: Rising Inequality in Post-Socialist Urban China* (Stanford: Stanford University Press, 2008).

⁴See Kam Wing Chan, *Cities with Invisible Walls: Reinterpreting Urbanization in Post-1949 China* (Hong Kong: Oxford University Press, 1994), 79, 83.

for sorting through the many claims about why so many people (probably over 30 million) starved. Kung shows quite convincingly, for instance, that bad weather was a major factor in only one of the “3 hard years” (1959), and that communal cafeterias—said by some to have led to wasteful over-consumption in the Leap’s early optimistic phase—did not matter much, while high levels of grain requisition in spite of poor harvests were enormously consequential. (This, then, was one occasion on which government coercion and the stifling of dissent had an enormous, deadly, impact on economic outcomes, as Amartya Sen famously pointed out long ago.⁵) The main limitation, for non-specialists, of Kung’s very effective meta-study of econometrics is that it provides no narrative. Readers will get a good sense of important *factors* behind the famine, but not of important actors or events—there is not much here, for instance, about why Mao doubled down on the Leap even as reports that it was backfiring multiplied, or why the rest of the Party took so long to stop him.

The volume’s final two chapters focus on the post-Mao era. Barry Naughton, covering the years from roughly 1976 to 2010, emphasizes how uncertain and contested the early years of reform were. An initial attempt to jumpstart the economy based largely on technology imports failed; advocates of institutional reform, although they shared a preference for some degree of marketization, were divided both on ultimate goals and on how to reach their preferred destinations; and a conservative faction suspicious of broad marketization remained influential until roughly 1992, without ever articulating a coherent alternative vision (744–60). The period from Deng Xiaoping’s southern tour in 1992 until roughly 2005 represented the high tide of reform, with aggressive downsizing of state-owned enterprises (SOEs); closure or privatization of most of the township and village enterprises (TVEs) that had been central to the achievements of the 1980s and early 90s;⁶ China’s accession to the World Trade Organization; and significant (though far from complete) easing of restrictions on internal migration. A major mid-1990s fiscal reform recentralized control of government revenue, restoring Beijing’s leverage over its boom areas and its ability to consider providing significant aid to lagging regions. (As part of this bargain, local officials gained more freedom to undertake their own entrepreneurial initiatives, especially in property development (765).)

As Naughton sees it, the very success of these reforms reduced officials’ sense of the urgency of further changes that might risk the rents they received for providing access to opportunities, while growing revenues allowed them to expand SOEs again and tolerate the losses that many incurred. The result was a slowing of reform that was already evident by roughly 2005–2006. The massive stimulus program that China used to weather the global financial crisis of 2007–2009 then marked a firm end to the era of

⁵Amartya Sen, *Development as Freedom* (Oxford: Oxford University Press, 2001), 181–82. Detailed studies of the famine are now quite numerous, and many highlight excessive grain requisitions and the resistance at high levels to reducing that extraction even as it became increasingly clear that people were dying. For one concise example, see Thomas Bernstein, “Mao Zedong and the Famine of 1959–1960: A Study in Willfulness,” *China Quarterly* 186 (2006), 421–45.

⁶It is interesting that neither Naughton’s essay nor Brandt and Rawski’s pays much attention to TVEs, although both essays are otherwise admirably broad in their coverage of the reform era and both insist that the process of reform has been anything but linear. It is true that these firms did not become a lasting part of the Chinese landscape, but in the first fifteen years of reform they generated well over 100 million new jobs, playing a vital part in the linked phenomena of poverty reduction and moving much of the labor force out of agriculture. It may be that the sometimes awkward combination of entrepreneurship and local public ownership that they represented makes them hard to fit into any smooth narrative of “state” versus “market” driven growth, but for that very reason it would have been useful to see them discussed.

marketization. At about the same time, a combination of demographic change, slowing growth in the demand for Chinese exports, and the exhaustion of many of the opportunities for “easy” catch-up growth that had existed back in 1976 ended the “miracle” era of 10 percent annual growth. Naughton does not attempt to assign precise weights to different factors in either the boom or the slowdown—indeed he emphasizes that his goal is to show how the logic of decision-making looked to actors at the time, and how it evolved as the choices they made at one point shaped their options later on, rather than to demonstrate the economic coherence of a system that emerged for often highly contingent reasons.

Loren Brandt and Thomas Rawski emphasize that the early stages of reform not only did not necessarily unfold as Beijing had anticipated; often they were driven from below, and only ratified by the center when local experiments had yielded irreversible results (802–7). For the era of peak market reform, ca. 1992–2006, their account largely reinforces Naughton’s, although they place more emphasis than he does on the role of foreign trade and investment, and they make more definitive claims about the causal connection between aggressive marketization and rapid growth. They also devote considerably more attention to years between 2005 and 2020, emphasizing the reassertion of state control in many areas, the renewed preference given to SOEs, and the importance of particularistic bargains between officials and the firms to which they give favors; and tying all of this to the slowdown in productivity growth that occurred over this same period. The resulting system of state capitalism, they point out, is capable of impressive dynamism and has generated various showcase successes; but it also generates massive waste and reinforces an authoritarian political system. They close their essay, and the volume, by arguing that two enduring dilemmas are central to understanding China’s economic history (827–828):

China will continue to grapple with dilemmas that have bedeviled two centuries of modernization efforts. How can China embed a creative, freewheeling culture of economic and technical innovation within an authoritarian system whose leaders feel threatened by unorthodox thinking? How can China resolve the concern arising from fears that indiscriminate opening to Western technology and ideas endangers the edifice that Confucian and Communist thinkers have long seen as the foundation of authoritarian rule and social stability?

I am not sure that this is the best way of framing even the post-1949 period. Even if we agree that the state’s insistence on ideological orthodoxy has done considerable harm, it seems to me more useful to look at the origins and results of separate, specific campaigns and policies than to trace all cases in which the state did not opt for market-driven policies to a single (though changing) ideological root, and then to compare the spectacular successes and disasters of the last seventy years to the presumed benign results of a very different kind of regime. But rather than pursuing that debate here, I will loop back to the pre-1949 period, and consider the costs and benefits of viewing the pre-Communist past—Confucian and otherwise—through a lens that makes autocracy the main explanation of China’s economic problems in this earlier period, too.

Not surprisingly, the volume’s emphasis on repressive and counter-productive actions of the Chinese state, both before and after 1949, goes along with a largely positive view of foreign influences, which many essays credit with inducing crucial structural and ideological shifts reaching far beyond the usually small role of the foreign sector in GDP. Ma’s opening essay, as I said above, sets the tone here. Ma provides a

thoughtful overview of financial and institutional changes after 1842 and suggests that this points to a “new framework” for understanding this period. This framework places ideological transformations at the center of explanations of growth (28–31)—or its absence, since outside of the Yangzi Delta and Manchuria, growth was actually modest at best from the Opium War until the 1949.⁷

In her essay on Chinese business institutions, Madeleine Zelin highlights the adaptability of indigenous firms, and she is thus cautious about how much difference the western corporation in particular really made. But most chapters highlight positive foreign influence on finance, education, and business forms, and some also question the negative influence often attributed to foreign intrusion. Pushing back against the emphasis, both in many Western works of the 1960s to 1980s and in nationalist-inflected Chinese scholarship today, on the burden of late Qing and Republican China’s foreign debt, Kung points out that direct foreign investment significantly exceeded that debt (367), and the largest recipients of FDI were light industrial sectors such as food and beverages (369)—hardly Lenin’s commanding heights of the economy. Elisabeth Köll’s essay on the pre-1949 development of infrastructure analyzes the most obvious examples of positive foreign influence: the new technologies that began with railroads, steamships, and telegraphs. Further questioning negative views of foreign influence, she reminds us that (as Thomas Rawski argued years ago), the growth of railways and steam shipping stimulated demand for traditional transport (e.g., by increasing the total amount of freight, which needed to be moved to and from the railway station by carts or porters) much more than it displaced workers in this sector (465).⁸ The volume in general does not deny the costs of imperialism, but pays them relatively little attention: opium is mentioned only briefly, and the destructiveness of wars is mentioned but not calculated or emphasized. Elisabeth Kaske and May-Li Lin’s chapter on “Public Finance” does say more about the costs of imperialism. It notes, for instance, that a very high percentage of the new government revenues collected by the Maritime Customs Service went to service foreign debt, much of it originating in indemnities imposed at gunpoint. The Boxer Indemnity, in particular, had much to do with “chaotic proliferations of taxes and surcharges” (260) that had baleful consequences for political stability (259–65). By the late 1920s, however, Chinese governments had, of necessity, largely transitioned to relying on domestic lenders (272) and on more centralized and “modern” sources of revenue, such as income taxes (274), so that the essay ends by emphasizing how the Nationalist government eventually acquired “all the trappings of a modern fiscal state and skillfully used domestic borrowing and monetary policy for economic stabilization and development” (278), although the eight-year war against Japan ultimately created burdens too big for this state to manage.

As well as a tendency to downplay the harm of foreign influence and stress its benefits, the volume also stresses new developments, rather than continuities, across the 1842–1949 period. This appears in the relatively large amount of space given to late Qing and Republican era finance—a sector in which there was considerable innovation, but which did not necessarily make a large immediate contribution to the well-being of most Chinese people. Dan Li and Hongzhong Yan make a compelling case in their essay “Money and the Macro-Economy” that despite the enormous numbers of often dubious

⁷Thomas Rawski, *Economic Growth in Prewar China* (Berkeley: University of California Press, 1989).

⁸Wolfgang Keller and Carol Shiue do, however, point to the likelihood of foreign-induced technological unemployment in a few places (446–47).

currencies that circulated in late Qing period and in early twentieth century China (especially during the warlord period), there was an overall trend towards monetary and financial integration, and (until World War II) probably towards stability as well. They further argue that these trends contributed to a growth in domestic trade, although the size of that influence is hard to measure. Brett Sheehan and Yingui Zhu, in their chapter entitled “Financial Institutions and Financial Markets,” show that modern banks grew rapidly, even as traditional “money shops” (*qianzhuang*) persisted in large numbers (287–94), and that both kinds of institutions were important lenders to nascent modern industry (307–12). However, new-style financial institutions did not lend much to agriculture (299). The Guomindang did encourage rural credit cooperatives in the 1930s, but they never grew enough to reach most of China’s farmers (321). Beyond this book, some recent research suggests that Qing financial markets may have been a good deal more sophisticated and integrated than we thought, and did reach down to at least many property-owning villagers.⁹ It remains to be seen how this revised picture of China’s pre-twentieth-century baseline in finance may affect our understanding of the significance of twentieth-century developments, and of foreign-derived innovations in particular.

Finally, it seems worth reflecting on issues of method and argument that arise frequently in economic history, positioned as it is between two disciplines with very different customary approaches. The volume presents a good mix of more narrative chapters, which attempt to explain outcomes as the result of (causal?) sequences of events, and others that review a series of econometric attempts to assign weights to different factors; but there are inevitable tensions and omissions. Perkins’ chapter on economic policy during the Maoist years and Naughton’s on the unfolding of post-1978 reform lean heavily towards narrative; Kung’s chapter on the Great Leap famine and Wolfgang Keller and Carol Shiue’s on foreign trade and investment before 1949 lean strongly towards econometrics. In a volume like this, econometric chapters are great for guiding readers towards relevant historiography, and in some cases for calling attention to little-known sources or new ways of using them (as the Keller and Shiue essay does), but they will frustrate readers who want to understand how change actually occurred over time. Moreover, authors often do not explain why they prefer some studies to others, or why we should think that particular correlations really do represent causation. (Space constraints may, of course, explain some of these absences.) If, for instance, former treaty ports and self-initiated ports (cities that Chinese governments opened to foreigners under terms similar to treaty ports, but under continuing Chinese administration) have grown faster than other cities since the 1978 reforms, but did not outperform them between 1949 and 1978, as Kung finds in his essay “The Economic Impact of the West” (388–90), it seems possible that the post-1978 data does indeed represent some long-delayed effect of the pre-1949 foreign presence, but it is not hard to imagine various confounding factors that would make this a spurious correlation. Various attributes that made a place suitable for “opening”—whether

⁹For just a few salient contributions in the last two years and focusing almost exclusively on the supposedly backward North China interior, see Matthew Lowenstein, “Financial Markets in Late Imperial China, 1820–1911” (Ph.D. diss., University of Chicago, 2021); Luman Wang, *Chinese Hinterland Capitalism and Shanxi Piaohao: Banking, State, and Family, 1720–1910* (London: Routledge, 2021); Cao Shuji and Xu Junsong, “Qingdai Shanxi de qiantie yu xinyong: yu Dongnan diqu bijiao,” *Shilin* 6 (2020), 79–95; Xu Junsong and Cao Shuji, “Qingdai Shanxi qiantie: fenlei, qi yuan, yu xinyong,” *Shehui kexue* 7 (2021), 155–66.

by foreigners or by domestic actors—in Qing times might also have pertained during “reform and opening.” Similarly, the correlation between localities with high rates of success in the imperial civil service exams and places with high educational attainments today *might* reflect a trans-historical persistence of positive attitudes towards education, as suggested in the essay on “Education and Human Capital” by Pei Gao, Bas van Leeuwen, and Meimei Wang (527), but regression analysis alone does not establish that conclusion.

Moreover, in both types of chapter, a focus on national policies—perhaps inevitable for a book that aims to cover so much—is bound to miss important aspects of how innovation and development actually occurred on the ground. And reliance on orthodox economics, in which bottom-up initiatives can easily be naturalized as profit-seeking “market responses” to opportunity that need no explanation in the absence of interference, can reinforce this tendency to overlook how successful entrepreneurship actually happened. The oversight becomes particularly significant in the many cases in which local entrepreneurship came as much from governments and collectives as from truly private sources. The TVEs discussed in a note above represent many important examples that get short shrift here. One thinks also of the emphasis on development “campaigns” in Yuen Yuen Ang’s prize-winning *How China Escaped the Poverty Trap* (2016) and of its focus on the kinds of local case studies for which synoptic chapters like the ones in this book rarely have room. That a recent book on economic change so acclaimed in one discipline (political science) finds no mention here—either positive or critical—testifies to the power of disciplinary silos. Those silos can be reinforced if we too quickly assume that we know what economically optimal institutions look like, and thus see failure to adopt them as a sign of a general ideological rigidity requiring disruption from the outside, rather than focusing on how existing institutions could develop. Starting from mainstream economics’ assumptions about optimal institutions, and emphasizing how real institutions deviate from them, also tends to preclude the exploration of alternative explanations from within economics itself, broadly construed. For instance, greater engagement with issues of distribution and China’s post-Mao safety net could open up other, demand-based, understandings of the recent slowing of Chinese growth. In this interpretation, rising inequality and the needs of most people to stockpile lots of savings for health, retirement, and other basic needs have weakened consumer demand, thus driving over-reliance on often dubious state-sponsored investment projects, especially since the global financial crisis brought the limitations of export-dependence into sharp relief.¹⁰ Arguments like these, based on specific conjunctures, should at least supplement assumptions about trans-historical resistance to markets and its consequences across many, many contexts.

This is not to suggest that economic history can or should dispense with the explanatory power of simplifying models from mainstream economics. And I do not wish to place too much emphasis on what a book does not do when it already does a great deal and represents a major effort. But a landmark publication like the *Cambridge Economic History of China* represents an important opportunity to think about where the field goes from here, and to get as many as possible of its very diverse participants talking to each other. By not explicitly raising questions about its choice of methods and its assumptions, this volume may do less to advance the field than it might have, given its many, often excellent, essays.

¹⁰For one version of such an analysis, see Ho-fung Hung, “Rise of China and the Global Over-Accumulation Crisis,” *Review of International Political Economy* 15.2 (2008), 149–79.