in ships, and in this it did not differ much from the "oriental" empires. Finally, it is not true that "the Greeks" liquidated the Persian Empire, which was in fact master of Greece in the fourth century, but it was the Macedonian monarchy of Alexander the Great that first subjected the Greek world and then conquered the Persian Empire.

My critical remarks, it will be clear, do not diminish my deep admiration for this groundbreaking work, recommended to all economic historians who want to broaden their horizon.

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The Great Divergence Reconsidered: Europe, India, and the Rise to Global Economic Power. By Roman Studer. New York: Cambridge University Press, 2015. Pp. 231. $80.65–101.96, hardcover; $28.99, paper; $82.00, kindle.
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State, Economy, and the Great Divergence: Great Britain and China, 1680s–1850s. By Peer Vries. London: Bloomsbury. Pp. 504. $148.00, hardcover; $39.95, paper; $18.14, eTextbook.
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Economic success, like any other, has a thousand fathers: many schools of thought want to claim it as support for their preferred theory of long-run growth. The debate that ensues is then usually informative, in that we learn a great deal about the economies involved, but it is rarely conclusive. This is because the variables involved—capital accumulation, increases in productivity, better governance, etc.—influence each other and it is difficult to distinguish cause and effect. Also, real-world economies do not map neatly into any theoretical model, so every contender can find some evidence for his or her case. The "East Asian Miracle" is a good example of this. For some scholars, the rapid growth of Singapore, Taiwan, Hong Kong, and South Korea in the late twentieth century was evidence that the Neo-classical Counter-Revolution's critique of excessive state intervention was correct; that growth was launched by removing the heavy hand of the state. Others argued the state had been extensively involved in economic activity, but that this intervention had been effective and judicious. The debate continues to this day. The "Washington Consensus" of the 1990s, which advocated limited government as a key element of development strategy, has largely broken down, and it is not clear what will replace it.

If we cannot agree on the primary causes of growth in four East Asian economies over the last few decades, how much harder it will be to understand the causes of economic transformations that happened (or did not) a few hundred years ago, in economies spanning continents, in an era for which economic statistics are scarce compared to the present-day. This is the fate of the debate on the Great Divergence, an expression coined by Kenneth Pomeranz to describe the parting of ways, in terms of economic growth and standard of living, between Western Europe and other parts of the world, including China and India. Is it an exercise in futility to try to identify "primary causes" when societies differed in so many fundamental ways—in behaviors, in social, family, and state structures, and in technological capacity? Perhaps we can ask a more modest question: When did the divergence begin? But even this smaller question has been difficult to answer. In particular, a group of scholars who are sometimes called the "California School" has argued that the Great Divergence began much later than we have thought. In 1700 (say) China and Britain had roughly similar levels of economic development. The Great Divergence began later, in the late eighteenth century or early nineteenth century. Broadly similar arguments have been advanced for India by Prasannan Parthasarathi who has argued that real wages were higher in South India than in Britain in the eighteenth century.

Of the two books I am reviewing *The Great Divergence Reconsidered* has a greater emphasis on dating, and is more cautious on the question of causality. Studer's focus is market integration. The author begins with the intuition that market-integration is good for growth, as argued by Adam Smith. He then compares India to Europe in the period 1600 to 1900. The data for Europe are more readily available. For India, the author argues there is little data for the period before 1860, which he calls "pre-statistical." An important contribution of the book is the assembly of data for this earlier period, from a wide range of sources.

Roman Studer argues, persuasively, that the right way to examine market integration is to look at the extent to which prices have converged across regions for products with a high bulk-to-value ratio (when this ratio is low it is profitable to trade even when transport costs are very high). He examines grain markets. Studer uses a variety of techniques, ranging from simple graphs and correlations to sophisticated econometric methods which allow both an investigation of the long-run co-movement of prices (cointegration) as well as the speed of the return to the long-term relationship when deviations occur. The evidence is clear: Indian markets were less integrated than European markets well before the Industrial Revolution. India catches up to some extent after the arrival of the railways in the second half of the nineteenth century, but still lags behind.

Studer discusses geographic disadvantages which prevented market integration in parts of India. In some regions it was difficult to make roads because suitable paving material was not available. There were few naturally navigable waterways in South India, in contrast with much of Europe. This is a fascinating discussion, but it does raise questions regarding the use of market integration comparisons to date the Great Divergence. To the extent European markets were better integrated than Indian markets for geographic reasons, this advantage would likely have been present even much earlier, in (say) the year 1300. If that is the case, differences in market integration will not necessarily help us to date the Great Divergence. Studer partly addresses this concern by comparing two landlocked regions, Switzerland and the Pune region in western India. By the late eighteenth century, markets in Switzerland were much better integrated.

Studer does not limit himself to documenting the greater degree of market integration in Europe; he does suggest that this opened the door for greater "Smithian" growth in Europe than in India. Still, he is careful not to overstate his case. For instance, he makes a useful distinction, suggesting that the potential benefits of integration of internal markets can be greater than those of integration with external markets (say, those on another continent). Also, market integration in nineteenth century India (including the expansion of railways) seems to have led to modest increases in economic growth. Studer is explicit in his intention to avoid mono-causal theories, and identifies himself with a view of long-run economic growth as a "seamless web" in which various factors interact. The book thus persuasively identifies and documents an important fact (greater market integration in Europe as compared to India well before the Industrial Revolution), and is appropriately careful in its discussion of its causes and consequences.

Compared to Studer's work, *State, Economy, and the Great Divergence*, comparing China with Britain (and often other parts of Europe), is framed more explicitly as identifying a major cause of long-run growth, as opposed to simply documenting and dating a difference between China and Britain. Eventually, though, for this reviewer, its main contribution is similar to that of Studer's book: a striking fact is identified and thoroughly documented, but its causal significance remains uncertain.

Vries' work reminded me strongly of *Governing the Market: Economic Theory and the Role of Government in East Asian Industrialization* (Princeton, NJ: Princeton University Press, 1990), Robert Wade's contribution to the debate on the East Asian Miracle, an intervention aimed at bringing the state back to center-stage as a major contributor to an economic success. Vries' formidable book, which can be fairly described as a *tour de force*, begins with a detailed discussion of existing work. It takes on two important schools of thought in British and Chinese economic history, respectively. In the case of Britain, Vries rejects the view, popular among economists writing about the Great Divergence, that economic growth occurred because the power of the state was weakened, and, to the extent states became stronger, growth may have occurred in spite of it. On the Chinese side Vries takes on the "revisionists." While the state in Qing China was once viewed as despotic and inimical to growth, the revisionist view highlights its role in promoting material welfare, such as, for instance, alleviating the misery caused by famines. Vries believes the revisionists have gone too far in overstating the reach and effectiveness of the state in China.

The bulk of Vries' book is devoted to demonstrating that the British state in period 1680–1850 was much larger than the Qing state in terms of various measures (taxes, expenditures, employment), permeated economic and social life to a far greater extent, and invested far more in the military. Moreover, the state in Britain was what we might today call "developmental," not merely creating an environment suitable for economic growth, but actively promoting it. This included protecting domestic industry from foreign competition when considered necessary. Vries also argues that the Chinese bureaucracy, often admired, was relatively small and that the training of officials was generalist, rather than specialized. Officials were transferred every few years (to discourage corruption) but this also prevented them from sinking local roots. Even with respect to famine relief, Vries shows that the British state was far more effective than the Qing state.

Vries' book will be rewarding for the patient reader. His arguments are developed slowly and carefully, and supported by masses of evidence, much of it statistical. These clearly establish the greater size and capacity of the British state as opposed to that of Qing China. But I must confess to remaining agnostic regarding his final conclusion, that mercantilism and the fiscal-military state were a necessary condition for modern economic growth in Britain. It does not surprise me that the state was interventionist in fast-growing Britain. Indeed, as we look around the world today, we can see that rapid growth creates assertive middle classes and industrial interests which demand state support. As I write this review, Indian newspapers are filled with descriptions of the Prime Minister's "Make in India" initiative. Economic growth in China and India even seem to have encouraged muscular nationalism and militarism of the sort Vries describes in nineteenth-century Britain. These appear to be the consequences of growth, and it remains to be seen if they will contribute to it.

I conclude with a provocative question: Is the Great Divergence still a useful "hook" or motivation for comparative historical research? On one hand it is clear that comparison between Britain and India or China (say) can draw scholars of different regions into productive conversations. On the other hand, when we compare regions that are vastly different, it is difficult to pin down why their economic growth trajectories diverged. For instance, after reading Vries' account of the importance of the state in the Britain-China contrast, I wondered whether, in India, a thin state with limited capacity was at the heart of slow growth in both pre-colonial and colonial periods. Limited market integration, which is the focus of Studer's analysis, may have been only one of many adverse consequences of modest state capacity. To address this question we might benefit, for instance, from a comparison of economic outcomes (including market integration) in Tipu Sultan's relatively more centralized "military-fiscal" Mysore (late eighteenth century South India) as compared to the regions more loosely governed by the Marathas. Alternatively, to understand the impact of market integration we might compare living standards in a region like Bengal, with many navigable rivers and a thriving export trade by 1700, with a landlocked interior region. Building on existing research on the Great Divergence, including the two fine books discussed here, we might benefit from now focusing on smaller divergences.

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Corruption, Party, and Government in Britain, 1702–1713. By Aaron Graham. Oxford: Oxford University Press, 2015. Pp. xv, 305. $110.00, cloth; £65.00, eBook.
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While handling the finances of the British forces during most of the War of the Spanish Succession, 1702–1713, the Duke of Chandos endured intense scrutiny, much of it hostile. Parliament's inquiry into his accounts as Paymaster of the Forces Abroad (1705–1713) took years to complete, but finally acknowledged, reluctantly, that his books were all in order, even if he had somehow become the richest man in the British Isles in the meantime. Later historians, as well as contemporary publicists, have pilloried Chandos as an archetypical representative of endemic corruption among the ubiquitous "moneyed men" or "proto-capitalists" who would arise in the following decades to undercut the legitimacy of British government, all the while enriching themselves. True, he was also a patron of the arts, known as "The Apollo of the Arts" for financing the composer Georg Handel as well as large numbers of paintings and sculptures that adorned his lavish stately home and estate, Cannons, in London. Did he also help in the creation of the British fiscal-military state in the eighteenth century?

Aaron Graham lays out the reasons he feels this is a plausible hypothesis in two introductory chapters covering the period 1660–1830. For Graham, state-building with the establishment of effective bureaucracies for administration over this (very) long eighteenth century would not have succeeded but for some form of ties within either the Whig or Tory parties that could loosely connect the multiple networks of merchants, politicians, and bureaucrats who financed Britain's overseas wars. Moreover, the Pay