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Symposium Review

The Gunpowder Age: China, Military Innovation, and the Rise of the West in World History. By Tonio Andrade. Princeton: Princeton University Press, 2016.

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INTRODUCTION

Tonio Andrade's *The Gunpowder Age* is a big book. It spans roughly 800 years, in both China and Europe. Its boldest claims concern China, but Andrade delves into European history as well, making it a challenge for any one scholar to assess his evidence and arguments. Because China specialists would want to know how historians specializing in European warfare and in Western science and technology evaluate Andrade's challenges to received wisdom, the *Journal of Chinese History*'s editor and editorial board invited historians outside the China field to contribute to a joint review. We succeeded in recruiting a distinguished panel, all of whom have written extensively on these issues: David Parrott, author of such books as *The Business of War: Military Enterprise and Military Revolution in Early Modern Europe*; Philip Hoffman, author most recently of *Why Did Europe Conquer the World?*; Stephen Morillo, author of *War in World History*, among other books; and Ian Inkster, author of *Science and Technology in History: An Approach to Industrial Development*, among other books. This introduction provides an overview of the discussion so far, and a few additional observations from a historian who has also tried his hand at Sino-European comparisons.

Andrade's principal argument is that—contrary to the myth that China invented gunpowder (ca. 1000) but did little to develop its military uses—China actually had the world's best gunpowder weapons and tactics until the late 1400s. It then fell behind, largely because it fought relatively few wars between roughly 1449 and 1550, while Europeans fought incessantly, especially after about 1490. When frequent fighting returned to East Asia, from roughly 1550 to 1700, Chinese again focused on gunpowder-related innovation, and more or less caught up with Europeans—even as Europeans themselves greatly increased their military capacities. But after 1700—perhaps especially after 1759, when Qing expansion into Central Asia ended—China fell behind again. This time, Europe's military advances relied on new kinds of math and experimental science, making them more difficult to copy. Moreover, though China began trying to catch up soon after losing the Opium War (1839–42)—belying notions of Confucian

cultural complacency or indifference to military matters—they were also handicapped by the late Qing state's fiscal and organizational weaknesses. Thus, despite considerable successes—evident in the reports of foreign military attachés, who expected China to win its war with Japan in 1894–95—China did not regain military parity this time until after the Communist victory in 1949.

In some ways—as Andrade himself notes—his intervention resembles those of the "California School," who argued that China did not fall behind the West economically until well into the eighteenth century, and then for reasons that owed much to contingencies.[1] However, comparing military capacity is easier in some ways and harder in others. On one hand, the standard for comparison seems clear—ability to win battles—while economic history suggests various criteria that do not always co-vary: Income per capita? Life expectancy? Head to head competition for specific markets? Ability to provision a state that guarantees security? On the other hand, economies can, at least theoretically, be measured at any date (if appropriate data can be found); but in periods when China fought few wars, and none against the west, we cannot readily track changes in its armies. A society with enough military capacity need not pursue all possible military improvements, especially in peacetime; but no early modern society was in any danger of being too prosperous. We should beware, then, of linking these "divergence" debates too closely, or assuming that they will parallel each other; yet the attractions of juxtaposing them are obvious.

Andrade quickly upends the truism that Chinese did little to develop gunpowder's military uses. The Northern and Southern Song Dynasties (960–1127, 1127–1279) lived within an intensely competitive multi-state system; both they and their Liao and Xixia rivals extensively used gunpowder bombs and other new technologies. True guns—with bullets that filled the entire barrel, and so were efficiently propelled by expanding gases—are only confirmed for the Mongol period (after 1279), but may be several decades older (51–54). The Ming dynasty, having overthrown the Mongols in 1368, stipulated that 10 percent of soldiers should have guns; by 1466, that figure was 30 percent—a level not reached in Europe for almost another century (54–55). Perhaps even more striking, the Chinese seem to have trained soldiers to keep a steady volley of fire by arranging themselves in rows and running to the back of the formation to reload while other shooters stepped forward (85). The elaborate choreography this required was greatly facilitated by having a standing army, suggesting that—contrary to the "military revolution" thesis that gunpowder-based warfare largely drove early modern state-making—some centralized state-building had to precede the effective use of guns. At any rate, it appears that Chinese armies mastered these techniques first because they maintained a continuous tradition of collective drilling from ancient times forward; such drilling largely disappeared from the West between Roman times and the 1500s.

Still, early modern guns had many limitations. The most effective early gunpowder weapons hurled larger projectiles, designed to sink ships and batter down walls. With

[1]Definitions of the 'California school' vary, but generally include my own work, especially *The Great Divergence: China, Europe, and the Making of the Modern World* (Princeton: Princeton University Press, 2000) and that of R. Bin Wong, especially *China Transformed: Historical Change and the Limits of European Experience* (Ithaca: Cornell University Press, 2000). Others often considered part of this group include Jack Goldstone, James Lee, Robert Marks, and Richard Von Glahn.

China fighting relatively few wars between 1449 and 1550, it fell behind; Portuguese cannons, and the warships carrying them, were initially better than any they encountered in East Asia. So were European fortresses and techniques for besieging fortresses.

Then, Andrade argues, as Chinese, Koreans, Japanese, and Manchus fought more wars again, they caught up. Foreign weapons were copied, then improved; tactics changed to reflect new technologies, and (like in Europe), new military manuals poured forth from presses (300). Ming loyalists drove Dutch forces off Taiwan; the Qing captured Russia's fortress at Albazin. Chinese musket fire—and especially that of their Korean allies—was more accurate than Europeans could produce. Dutch ships remained superior—though Ming loyalists apparently copied some successfully—and the Chinese learned the techniques for besieging European-designed fortresses incompletely. But other advantages gave them victory, allowing Andrade to assert that parity prevailed circa 1700. This will probably be the book's most contested claim, though only one of our reviewers directly challenges it.

Then, Andrade tells us, relative peace returned to East Asia (especially after 1760), while Europeans kept fighting: and this time, they pursued military power with increasing aid from math and experimental science. This produced, for instance, accurate tables for calculating how air resistance affected projectiles, improving artillerists' aim (245). Experimentation also led to dramatic improvements in gun design, and in gunpowder itself. Meanwhile, Chinese military readiness deteriorated. When the Opium War revealed China's militarily weakness, catch-up efforts began immediately—contrary to myths about Confucian indifference to things foreign and/or military. But because matching Europe's new advances required improvements in engineering, math, technical drawing, and other fields (257, 279), the process was slower than before. Administrative problems also interfered.

Nonetheless, Andrade emphasizes, China had made impressive changes by the eve of the Sino-Japanese War—which most observers expected China to win. Other scholars have already noted this point and suggested that China's defeat in 1894–95 should not lead us to dismiss the previous decades of reform.[2] But Andrade nicely synthesizes the case that inferior weapons were not the problem (284). He also shows that leadership and discipline at the unit level was probably not deficient: in short, the contrast between Japan's "samurai spirit" and a more pacific "Confucian culture" that figured in many post-mortems was probably a red herring. Instead, Andrade emphasizes high-level political and administrative failings, such as not establishing a unified naval command (291). This leads him to a more general contrast between Japan's ability to start fresh after the Meiji Restoration, abolishing many old institutions (293), and the inability of Chinese reformers, serving an enduring dynasty, to jettison old units and capture their budgets for new ones (275). In other words, Chinese pursued appropriate military innovations, but politics protected too many vested interests and too much costly dead wood. This is an important claim, to which I will return.

[2]See for instance R. Bin Wong, "Self-Strengthening and Other Political Responses to the Expansion of European Economic and Political Power" in *The Cambridge World History*, Volume VII: *Production Destruction, and Connection, 1750–Present*, Part 1, ed. J.R. McNeill and Kenneth Pomeranz (Cambridge: Cambridge University Press, 2015), 366–94; Stephen Halsey, *Quest for Power: European Imperialism and the Making of Chinese Statecraft* (Cambridge: Harvard University Press, 2015).

David Parrott seems generally comfortable with Andrade's central claim of East–West military parity circa 1700. Indeed, he adds further reservations about the effectiveness of European infantry fire even in the very late 1600s, noting the enduring importance of hand to hand combat. He also points out that while early modern European artillery fire was deadly, this required that the batteries be well-supplied and well-maintained —which they frequently weren't. By qualifying claims for Europe's military revolution, he bolsters Andrade's case.

Stephen Morillo likewise seems largely in sympathy with Andrade. He particularly emphasizes their agreement that—in contrast to arguments that the military revolution and its resource demands drove early modern state-building—a fairly effective state was necessary to initiate the characteristic changes of early modern warfare. That improved guns, ships, and fortresses were expensive is not news, of course. But if Andrade is correct that China, unlike Europe, did not need to "re-discover" intensive group drilling because it always had a state (or states) large enough to maintain a standing army, this would mark a significant contribution of comparative history to our understanding of how, in Charles Tilly's phrase, "states made war, and war made states." Nevertheless, this seems to me a debate where splitting the difference may make sense. Few would deny either that it required states of some sophistication to successfully adopt gunpowder weapons, or that once some states were using gunpowder effectively, competitive pressures would drive attempts at state-building elsewhere. Tilly's aphorism, in which states are present even before "war made states," suggests just such a chicken-and-egg relationship.

Philip Hoffman, while praising Andrade's contributions, is more skeptical of his thesis. In particular, he questions whether China really regained military parity during the seventeenth century. He notes that Andrade himself acknowledges a European lead at sea (with China perhaps catching up briefly in the 1660s, but only briefly), in fortress design, and in tactics for besieging advanced fortresses. He also doubts whether China truly had parity even in infantry weapons and warfare. Here it seems to me that Andrade has enough evidence to place the burden of proof on the skeptics, particularly if one considers the marksmanship of China's Korean allies (193–195). The same, I think, is true with respect to land-based artillery and artillerists. But Hoffman's points about ships and fortress design seem indisputable—perhaps the most one can say about those areas is that circa 1700 the Qing probably could still have caught up quickly had that become a priority—which it didn't.

Hoffman's essay also suggests two broader issues, about the meaning of "parity" and "military capacity." First, he notes that the battles between Chinese and European forces that Andrade uses as evidence of parity were fought far from Europe and close to China. This is undeniable, but its significance depends on what "parity" means. If it means "the ability to do all the same things equally well," then the locations of these battles matters—but so would the vastly superior ability of the Qing to sustain large armies for long periods in landscapes that yielded few provisions.[3] Of course, no European state

[3]See Peter Perdue, "Culture, History and Imperial Strategy: Legacies of the Qing Conquests," in *Warfare in Chinese History*, ed. Hans van de Ven (Leiden: E.J. Brill, 2000), esp. 277–78; and more generally Perdue, *China Marches West: The Qing Conquest of Central Eurasia* (Cambridge: Harvard University Press, 2005.) This

except Russia cared much about supplying huge campaigns on the steppe, making their inability to do so unimportant; but it was equally true that the Qing were uninterested in projecting power thousands of miles from their contiguous territories. If "parity" instead means something like "a situation where any conflict that could plausibly have occurred, given each side's strategic concerns, could conceivably have gone either way," then Andrade's claims can probably survive Hoffman's point here. The former definition might be more appropriate if our larger question is "Are any factors contributing to Europe's nineteenth-century military superiority already detectable in 1700?" but that is a different question from either "Were European states in 1700 doing better at meeting their current and foreseeable military needs?" or "Were Europe's nineteenth-century military advantages over East Asia overwhelmingly due to eighteenth-century developments?" Here we come close to theoretical questions that have also arisen in debates over the economic "great divergence," where some European advantages that would later prove important appeared well before the totality of circumstances indicated either clear European superiority or the inevitable future emergence thereof.

Hoffman also broadens the definition of "military capacity" to include the ability to finance the military—where European states developed methods, based on borrowing (both at home and abroad) against future revenues, which had no close East Asian parallels. The early and high Qing faced no immediate problems in funding their wars, even without borrowing: a combination of saving up in advance, collecting low taxes across a vast empire, and periodic campaigns for "voluntary contributions" covered their decades of expansion quite satisfactorily. Indeed, they won those wars while taking a smaller share of GDP than any major European power, and spending a lower share of their revenues on war.[4] But Hoffman raises a different point, suggesting that lacking debt finance might have made it harder to continue upgrading military capabilities during peacetime—particularly, I would add, in a society where Mencian norms, embodied in Kangxi's 1713 tax freeze, made additional revenue demands not justified by an emergency particularly suspect. Thus, Andrade's very heavy reliance on the frequency of wars to explain rates of military innovation does seem excessively single-minded. This further suggests the need to think about different *kinds* of states facing qualitatively different challenges, rather than relying on a one-dimensional scale of their "strength," or of how much they fought.

A similar point seems implicit in Ian Inkster's review, which focuses less on when China fell behind than on why, and above all on the obstacles it faced in catching up again. He notes that the Qing had "huge difficult and very varied frontiers" to deal with, and thus needed to prepare for many kinds of war. This, he suggests, may have

logistical capacity exceeded that of Napoleon in his invasion of Russia several decades later, which was considered a remarkable achievement.

[4]Dwight Perkins, "Government as an Obstacle to Industrialization: The Case of Nineteenth-Century China." *Journal of Economic History* 27, no. 4 (1967), 492; Zhou Yumin 周育民, *Wan Qing caizheng yu shehui bianqian* 晚清财政与社会变迁 (Shanghai: Shanghai renmin chubanshe), 36–38. Compare Philip Hoffman and Jean-Laurent Rosenthal, "The Political Economy of Warfare and Taxation in Early Modern Europe: Historical Lessons for Economic Development," in *Frontiers of the New Institutional Economics*, ed. John Droback and John Nye (Academic Press, 1996), 36, and Sevket Pamuk, "The Evolution of Fiscal Institutions in the Ottoman Empire," in *The Rise of Fiscal States: A Global History, 1500–1914*, ed. Bartolomé Yun-Casalilla and Patrick K. O'Brien (Cambridge: Cambridge University Press, 2012), 325.

tipped the balance towards "institutional adaptation and social continuity" as opposed to the "more free-floating and discontinuous technological changes of Europe" (and later Japan). That military challenges varied in kind also matters in considering when Qing forces began to not only fall behind, but to deteriorate; and even more so in understanding why reversing this slippage proved so difficult.

When they finished conquering Xinjiang in 1759, the Qing certainly had reason to feel satisfied with their military—even if, taking a global perspective, we can identify areas where Europeans were stronger. The next few decades provided mixed signals. An invasion of Burma (1769–70) failed, but as much due to malaria as anything else; a conflict in Vietnam (1788–89) also went badly. However, a war with Nepalese Gurkhas over Tibet (1788–92) went fairly well, and various domestic uprisings were successfully crushed. Even the Qing navy—which had received little attention, and had trouble suppressing pirates—might have seemed sufficient as late as 1808, when the Qing successfully expelled British ships and soldiers from Macao (by suspending trade).

It was a dismal performance against the White Lotus Rebellion (1796–1805) that raised alarms in Beijing. But this was a counter-insurgency campaign in very mountainous terrain, won by militia after regular troops failed. Literati officials examined the campaign at length, but drew lessons, unsurprisingly, that mostly concerned improving civil government, rooting out supply-train corruption, and compiling improved maps, rather than, say, improving artillery that could not have been moved fast enough in such terrain anyway. The next 30 years brought mostly small actions against very varied foes—on the steppe, in the southwestern mountains, and once near Beijing; they mostly called for military capabilities other than those that Europeans were busily advancing. Hoffman also notes that the Qing probably suffered from having to wage different kinds of wars, emphasizing ones against nomads which provided "less of a chance to practice with guns, artillery, and fortifications, and less of chance to improve them"—and of course no impetus for naval development. The point, I think, could be pushed even further when we consider how rapid population growth, especially in the southwest (aided by the diffusion of American crops able to feed growing highland populations), was forcing the Qing to learn yet another kind of "pacification." Quite a bit was written about strategy and tactics between roughly 1800 and 1830, but it mostly concerned this type of warfare, rather than European techniques that the Qing had not yet encountered.

Divided attention persisted even after the Opium War revealed China's serious deficit in gunpowder-related technologies. The lengthy Taiping Rebellion (1851–64), fought mostly in the Yangzi Valley, stimulated interest in purchasing, using, and learning to make Western weapons. As Andrade notes, the new arsenals were more successful than has often been acknowledged, in some ways comparing favorably with their Japanese counterparts.[5] Nonetheless, they fell short of what was needed, partly because they were chronically underfunded. Andrade attributes much of this to the Qing being less

[5]See for instance Meng Yue, "Hybrid Science versus Modernity: The Practice of the Jiangnan Arsenal, 1864–1897" *East Asian Science, Technology and Medicine* 16 (1999), 13–15; Stephen Halsey, *Quest for Power: European Imperialism and the Making of Chinese Statecraft* (Cambridge: Harvard University Press, 2015), chapter 4.

able than Japan's new Meiji government to eliminate vested interests and de-fund outmoded units.

There is surely something to this. But I would not assume that all units that were not "modernized" along Western lines were dead wood which survived only because the Qing had insufficient freedom of action. The forces that defeated the Nian—who were essentially guerrillas—were not "modern": many were militia with very light equipment. Hunanese forces that were crucial against the Taiping—and later against Muslim rebels in the northwest—also owed much to home-grown ideas about how to win control of grass-roots society, even as they incorporated some modern weapons and drill. The decidedly low-tech Guangxi Army—scaled up from "Zhuang"[6] militias that had fought well against a Miao uprising—not only played a crucial role against the Panthay Rebellion (1856–73), but held their own when they fought French forces in 1884–85. In short, given the varied enemies late Qing armies faced, it is not clear, even in retrospect, that only the most modern forces deserved funding; many also mixed more and less modernized small units in less than fully legible combinations. (And even in World War II—where both sides anticipated a high-tech war[7]—the most modern units did not always prove the most valuable.) This is not to deny that the architects of the Meiji military benefitted from having a relatively clean slate; but the more limited range of missions they faced mattered too. So did the superior extractive power of Japan's fiscal apparatus.

Again, then, a scale of more and less modern armies, or states, takes us only so far without also considering different contexts and goals. But if Andrade has not fully settled questions about the military "great divergence," he has given us a new and exciting vantage point from which to study them. Much productive debate will surely follow.

Philip Hoffman

The technology of gunpowder weapons has long fascinated historians. Although it originated in China, gunpowder has been invoked to explain why Europe—so weak and so backward in the early Middle Ages—ended up dominating the world in 1914, with colonies, influence, or possessions in every inhabited continent. And the conquests that the technology made possible have in turn been linked to the Industrial Revolution, to enduring poverty in Africa and Latin America, and—last but not least—to the great divergence between Europe and China.

Historians of all stripes—world, military, economic, European, and Asian historians in particular—will therefore welcome Tonio Andrade's *Gunpowder Age*. Andrade gives us

[6]This is an ethnic label that has been applied retrospectively; it is not clear what the men would have called themselves. But they were clearly "minority" troops from a remote region, led by a descendant of "native chieftains" (*tusi*).

[7]Kwong Chi Man 鄺智文, *Minguohu? Junguohu? Dier ci Zhong-Ri zhanzheng qian de minguo zhishi junren, junxu, yu junshi biange* 民國乎？軍國乎？第二次中日戰爭前的民國知識軍人、軍學與軍事變革 (Hong Kong: Chunghwa Books, 2017). And on the modern nature of at least some of the oft-ridiculed warlord armies of the 1920s, see Arthur Waldron, *From War to Nationalism: China's Turning Point, 1924–1925* (Cambridge: Cambridge University Press, 1995), 53–71.

a new history of gunpowder, of early guns, and of the military use of gunpowder weapons in China from the Song dynasty on. But he does not limit himself to writing the early history of the gunpowder technology in China. He pushes on to the end of the Qing dynasty, includes naval warfare and fortifications, and extends his research to Korea and Japan, and to Europe as well, which he covers in considerable detail. With lucid prose, a bold argument, and research based on Asian and European sources in multiple languages (I counted nine), all historians should congratulate him on a wonderful book.

Andrade tells a fascinating story about gunpowder weapons in both Europe and East Asia, but what does his book bring to the broader debates about the rise of European power and about the great divergence? The gunpowder technology figures in these debates because it allowed small numbers of European soldiers and sailors to prey upon seaborne trade and conquer territory or at least gain a toe hold in foreign lands via forts that resisted sieges. It also helped small numbers of Europeans attract native allies, and it allowed them to occupy territory by using the threat of force to keep the native populations under control. European ventures of this sort were doomed to failure against China and Japan—at least before the nineteenth century—but they did succeed in the Americas, Australia and the Pacific, South and Southeast Asia, and Africa. And although other forces were certainly at work—notably disease in the Americas and the Pacific—it is clear that mastery of gunpowder weapons played a big role in explaining Europe's rise to power, as did continued advances in the gunpowder technology itself, from better guns and warships to improved forts and siege tactics.

Being at the forefront of this technology was therefore important, and Andrade aims to tell the story of who led its development from the very origins of gunpowder weapons in Song Dynasty China. In his story, which I greatly simplify here, the leader is either Europe or China, and at the outset China held the lead. But China fell behind after about 1450, as the Ming dynasty was stabilized and fought fewer wars. In Europe, by contrast, war did not abate, and having acquired the gunpowder technology from China, the Europeans used it heavily and pushed it further, by developing matchlock muskets and the first artillery. When these improved gunpowder weapons arrived in East Asia with the Europeans in the sixteenth century, China copied them, and as warfare picked up again in East Asia later in the century, the Chinese caught up with the Europeans, leading to an age of technological parity between Europe and China (and East Asia in general) between roughly 1550 and roughly 1700. Thereafter, Europe took the lead as fighting again subsided in East Asia and as the Europeans learned how to apply science (specifically the ballistics of air resistance) and industrial engineering to military technology.

This story raises two questions. The first involves Andrade's way of determining who had the lead in developing the gunpowder technology. Andrade's method (apart from looking at the remarks of contemporary observers) is to examine different battles between European and Chinese forces and then ask who won. For him, the victor is the leader, at least in the technology that was used in the particular battle. The battles, though, are all in East Asia, and that raises the question whether East Asian battles really are a fair comparison for the Europeans. As for the second question, it concerns his explanation behind the lead. For Andrade, the lead in improving gunpowder weapons is ultimately explained by the frequency of war, at least until science is applied to military technology in the eighteenth century. In a nutshell, when warfare

wanes in East Asia, China falls behind; otherwise it catches up until science becomes important. But are there not other factors at work?

The obvious concern with fairness of Andrade's method is that the Europeans were far from home or did not have all the resources available to the Chinese. And to judge from Andrade's account, the European leaders in Andrade's battles were not the best that Europe had to offer. In short, the comparisons may be stacked against the Europeans, and they may exaggerate the speed with which China caught up in the late Ming Dynasty or underestimate the size of any remaining technological gap in the period 1550–1700. Andrade acknowledges that in this age of parity Europe did still hold the lead in certain areas—in warships, fortification design, and siege tactics. But the Europeans may have still been ahead in other parts of the gunpowder technology too, if Andrade's method is misleading.

Ideally, one would like to correct for the bias of his method. Andrade could have done so by asking what would have happened if both sides in the battles had the same resources and leadership and were equidistant from home—in other words, by engaging in counterfactual analysis, something that is quite common in economic history. He might have also tried other ways of determining who was at the forefront of the gunpowder technology, such as examining the direction of intercontinental trade in military expertise and technology. From the beginning of the sixteenth century on, that trade nearly all went in one direction, from Europe to Asia, with the only exception being rockets from late eighteenth-century India. The flow of expertise, nearly all of it from west to east, raises some doubt about whether 1550–1700 really was an age of parity. And the European lead might have yawned open even wider if Andrade had considered still other factors that play into military domination, such as the ability to finance wars by borrowing. There Europe had a clear lead by the seventeenth century.

What about Andrade's singling out the frequency of war as the chief factor that determines who has the lead? It clearly plays an important role in explaining military advances, particularly before science starts to affect innovation, and it does seem to match Andrade's chronology of improved gunpowder weapons, provided we trust his method of measuring the lead. The problem, though, is that there were other forces affecting the pace of the advances with the gunpowder technology.

Among these other forces was the sort of enemies a country faced. In *Firearms: A Global History* (Cambridge: Cambridge University Press, 2003), Kenneth Chase (whose work Andrade does acknowledge) has argued that gunpowder weapons were less effective against the sort of steppe nomads that China often faced. The nomads had no cities to besiege, and they could ride away from infantry, who would usually run out of supplies on the steppe. The best tactic was to dispatch cavalry against the nomads and arm the cavalry with bows, because it was difficult to fire a matchlock musket on horseback. Not that guns were useless against nomads—far from it. They could target nomads from fortifications, and the nomads themselves began using guns in the seventeenth century. But even in the final campaigns against the nomads in the eighteenth century, archers on horseback proved important. So to some extent China continued to depend on archers. It therefore waged less gunpowder warfare and thus had less of a chance to practice with guns, artillery, and fortifications, and less of chance to improve them. In western Europe, by contrast, nomads posed no threat whatsoever,

and so nothing diverted attention from the gunpowder technology. The result would be a faster pace of improvements to the gunpowder technology in Europe.

The geography of power in East Asia had a similar effect, once the Qing Dynasty was firmly in power. As an East Asian hegemon, its size and military might would discourage rivals from attacking. The result would be the reduced frequency of war that Andrade notices, but not just for China. There would be less war throughout East Asia, and hence less innovation with gunpowder weapons everywhere in East Asia. Europe, of course, had no hegemon, so its wars continued unabated.

There was one other force that spurred on innovation with gunpowder weapons as well: spending on war. It was critical, as we can see by looking at the Ottoman Empire and South Asia in the eighteenth century. In both places, war was frequent, but improving the gunpowder technology was rare. The reason, as I have argued in *Why Did Europe Conquer the World*, is that the Ottomans, and the polities that battled in South Asia as the Mughal Empire disintegrated, lacked fiscal systems that could mobilize large amounts of revenue for war. If recurrent war is all that matters, they should have innovated, but by and large they did not. Andrade might object that the Ottomans and South Asians lacked the science needed for further improvements, but there were many military innovations in eighteenth-century Europe that preceded scientific progress by decades or had nothing to do with it at all—for instance, cannon boring machines or copper sheathing that increased the speed and useful life of warships.

In any case, spending was essential for innovation. Even in the nineteenth century, when the role of science was clear, spending played a key role. In fact, the Europeans actually fought much less between 1815 and 1914, but their military expenditures actually rose. They were in something like the cold war, and without all the tax revenue spent on the military, they too would not have innovated. Andrade himself seems to acknowledge the importance of military spending, particularly when he discusses how a lack of funds contributed to the Qing's military decline and hampered nineteenth-century efforts to modernize China's armed forces.

One reason why military spending may have been limited in Qing China is that per-capita tax revenues seem to have been low, far lower than in western Europe, if we can trust the figures gathered by Loren Brandt, Debin Ma, and Thomas Rawski.[8] And the imperial authorities may have devoted more tax revenue to public goods such as famine relief than their counterparts in Europe, whose sole task was to raise money for war. The result again would be less military innovation in China and more innovation in western Europe, simply as a result of the Europeans' profligate spending on war.

All great books raise questions, but posing them should not detract from Andrade's achievement, for *The Gunpowder Age* certainly is a great book. It raises other questions as well. One might for instance ask what it says about global history beyond military technology. Does it have implications for the great divergence? It is hard to say that improved gunpowder weapons made anyone better off, either in China or Europe. The average European, for example, was taxed mercilessly by monarchs, who, for the most

[8]"From Divergence to Convergence: Reevaluating the History Behind China's Economic Boom," *Journal of Economic Literature*, 52, no. 1 (2014), 45–123.

part, were pursuing their own glory. True, some Europeans may have benefitted (wage earners in eighteenth-century London if we believe Robert Allen), but overall Europeans were worse off.[9] The same was true for British taxpayers in the heyday of the British Empire. Perhaps colonial trade contributed to industrialization (although many economic historians would disagree), and warfare may have facilitated the development of the iron industry or the creation of financial markets. But it is hard to argue that guns ushered in economic growth.

Stephen Morillo

Tonio Andrade has written a comparative history of the invention, development, and impact of gunpowder weapons, focusing on China (his academic area of specialization) and western Europe. This direct comparison sheds considerable light on several major debates in military and world history, particularly the so-called "Military Revolution" of early modern Europe and the origins of the "Great Divergence" between China and Europe in the nineteenth century. *The Gunpowder Age* should become a central piece in the "canon" of these issues.

Andrade plays the key card in his hand early. On page six, he presents a chart showing the number of wars fought in Europe and China between 1340 and 1910. As he points out, while the numbers do not match up exactly, they are remarkably similar between 1340 and about 1680. At that point, a "great divergence" sets in. In China, the successes of the Qing dynasty in establishing a "great peace" are clearly visible: except for a brief and moderate uptick in the 1740s, China saw minimal levels of conflict between 1680 and 1840s. Europe, meanwhile, witnessed during the long eighteenth century significant numbers of wars almost every year, including four of the top five years for numbers of wars on the entire chart. Andrade sees in this eighteenth century difference the cause of the vast disparity in military capability between China and (specifically) Britain that the First Opium War exposed starting in 1839. Simply put, after centuries of development of military capability—including especially the invention and improvement of gunpowder weaponry—that tracked closely in the two regions based in roughly equivalent challenges posed by war, China fell victim to its own success and got "out of practice" at technological development and conducting major wars during its eighteenth-century lull.

We should note several things about this thesis. First, Andrade applies a social science methodology—simply counting things!—to a question whose answers have recently come to be dominated by cultural analysis. In addition, the thesis fits into the materialist "challenge and response" dynamic often used by military historians to account for military innovation, whether technological or doctrinal. A clear and globally applied version of this sort of analysis is Wayne Lee's recent book *Waging War*, though Lee is careful to weave the role of culture into his analysis of how polities perceive challenges or threats.[10] Thus, Andrade returns a distinctly social science perspective back to the "Military

[9]Robert C. Allen, *The British Industrial Revolution in Global Perspective* (Cambridge: Cambridge University Press, 2009).

[10]Wayne Lee, *Waging War: Conflict, Culture, and Innovation in World History* (New York: Oxford University Press, 2015).

Revolution" debate, where it is much needed, and adds a different materialist perspective to the Great Divergence debate that began with the essentially economic analyses of Kenneth Pomeranz, though Pomeranz too rightly acknowledges some role for cultural differences in the ways that Chinese and European states responded to economic conditions.

This materialist, social science-oriented approach to comparative history allows Andrade to compare like things, avoiding the problem that can plague comparative cultural analysis of comparing apples and oranges (sometimes one culture's word for "apples" is *oranges* in translation, inviting misleading comparisons). Like Lee and Pomeranz, Andrade does not neglect culture, acknowledging that counting conflicts depends in part on how each culture defined and recorded them. But he considers this a minor problem, rendering his analysis more like Political Science than Comparative Literature. In this reviewer's view, his argument works. The "number of wars" comparison also allows Andrade to avoid the teleological traps that tend to emerge from cultural analyses, because the metric emphasizes the similarities between China and Europe up to a very late date, preventing the sort of "deep cultural difference from early on" argument that makes eventual European success appear inevitable. He further explicitly argues against such teleology by pointing out that the resurgence of Chinese military power in the later twentieth century is simply a recent instance of a series of Chinese modernizing responses to external challenges that date back to the fourteenth century, never mind that China sometimes led innovation in the first place. In addition, his comparative analysis works, as it should, in both directions, casting as much light on European developments as it does on China.

There is, however, more culture than meets the eye in Andrade's comparisons. His materialist frame, the "challenge and response" dynamic in military innovation, reveals the deep cultural assumptions that undergird his analysis. To begin with, I think he finds comparisons between China and Europe within this "challenge and response" frame fairly straightforward because he assumes (correctly) some basic similarities between the two military cultures. Many of these similarities are also materially grounded, but in ways that we need to specify. Most fundamentally, both regions created cultures of war that were characteristic of their relatively rich sedentary, agrarian economies—in contrast to the military cultures of, for example, pastoralist nomadic economies such as Central Asia or less productive agricultural economies such as West Africa. Compare, for example, John Thornton's analyses of Atlantic African warfare, which demonstrate the materially and thus culturally contingent reception of gunpowder in different economies.[11] The fundamental similarity of the Chinese and European geographic-economic regimes from early on can be seen in the basic similarities in strategic advice offered by the Chinese Warring States-era military literature represented by the *Sunzi* and the late-Roman military handbook authored by Vegetius.

Within that broad geographic-economic similarity, by the gunpowder age of Andrade's study China and Europe shared further similarities in the "scientific" and manufacturing sectors of economic activity that facilitated similar responses to military

[11]John K. Thornton, *Warfare in Atlantic Africa, 1500–1800* (London: University College London Press, 1999).

challenges, especially in terms of the development of military technology. The "scientific" resemblance was basic enough in terms of theoretical understandings of nature that cultural differences in what we would call applied physics and chemistry were still negligible. Indeed, Andrade argues that such differences remained effectively non-existent into the eighteenth century, explicitly countering interpretations of Chinese culture as "anti-technological."

Finally, to the extent that there were differences in the underlying *political* and economic contexts of military innovation, Andrade's analysis shows that Europe, over the course of the later Middle Ages and early modern period, was becoming more like China. This is centrally true of the role of the state and state power in military innovation, as European states became gradually more "Chinese" over the time period of the book. There remained differences, of course, mostly in terms of the interaction between the state and private enterprise in each region, but Andrade takes account of these differences and argues that they were less significant than is sometimes portrayed, along lines similar to those investigated by Pomeranz. Andrade's analysis of the role of state power implicitly argues that such power was in fact a precondition to the effective employment of gunpowder weaponry in war, an argument I have made about sixteenth-century Japanese warfare.[12] His argument therefore modifies the central thesis of the conclusion of Geoffrey Parker's *Military Revolution: Military Innovation and the Rise of the West* (Cambridge: Cambridge University Press, 1996), that "Warring States" eras—in China very early on and in Europe as well as Japan during this period—constitute crucial moments in the global history of warfare and state power. The modification, of course, is that for Adrade the growth of state power precedes military revolution, a position with which I fully agree, whereas the central point of Parker's book and much of the European "Military Revolution" literature is to show that changes in military technology caused (or at least stimulated) political change.

Thus, in terms of the Big Questions I noted at the beginning of this review, Andrade's book constitutes another nail in the coffin of the "Military Revolution of Early Modern Europe" thesis, which traces both the rise of the "modern state" and the origins of European imperial dominance in the nineteenth century to the results of the introduction of gunpowder weaponry in Europe from the fourteenth century. (This may suggest more finality than is warranted for the theory, given its zombie-like ability to rise from the dead. But the more specialists in non-European military history such as Andrade and Peter Lorge weigh in, the fewer lives it has left, I hope.)[13]

In addition, Andrade's focus on war fighting capacity and the role of being "out of practice" in a "challenge and response" military dynamic highlights the complexity of causation in explaining the "Great Divergence" between Europe and China (and indeed the rest of the world) in the nineteenth century. The divergence was not just an economic phenomenon, although Industrialization is centrally important and is inextricably tied up with military technology and capability. Furthermore, as noted above, the

[12]Stephen Morillo, "Guns and Government: A Comparative Study of Europe and Japan," *Journal of World History* 6 (1995), 75–106.

[13]Peter Lorge, *The Asian Military Revolution: From Gunpowder to the Bomb* (Cambridge: Cambridge University Press, 2008).

focus on military capability makes it even easier to see what a temporary phenomenon "western dominance" is in world history.

In sum, this is a very valuable book of world history because it takes China not just as a central topic, but because it uses the Chinese experience of gunpowder as a lens through which to view the rest of the world during this time, including Europe—which is too often the standard lens. The resulting picture is different and revealing in its difference.

David Parrott

Tonio Andrade has over the last decade become a key player in the development of a global perspective on the relationship between military change, technology transfer, and interstate competition. His work, based on direct knowledge of Chinese sources and scholarship, has challenged a range of assumptions about the respective military development of China and the European powers from the sixteenth to the nineteenth centuries. Andrade deconstructs a traditional, Europe-focused interpretation of the impact of gunpowder on warfare. The lineaments of this story are familiar. Gunpowder was made in Asia as early as the ninth century CE but was not originally used in warfare by the Chinese. Whether because they lacked the military stimulus of inter-state competition, or whether a Confucian-inspired contempt for the practical application of technology held them back, the Chinese placed themselves on a trajectory which consistently lagged behind European states in the application of gunpowder to warfare. If the military consequences of this backwardness only emerged in the Opium wars of 1839–42, the gap between military potential had been widening since at least the sixteenth century, when Europeans had started combining technology, organization, and resource mobilization in what has been described as a military revolution.

Andrade calls into question much of this story about the evolving superiority of European military capacity. A touchstone of the original military revolution thesis was the combination of infantry firearms, linear formations, and systematic drill to make possible the practice of counter-march and volley fire. To its most enthusiastic adherents, this turned European infantry formations into "production-lines of death," and transformed the early modern battlefield from one shaped by the brute clash of edged weapons into a competition to maximize firepower, in which drill and discipline would prevail. Andrade's account provides sources—down to carefully drawn diagrams dating from the eighth century—to show how the Chinese had centuries before developed and refined counter-march drills and volley fire for their crossbowmen. He adds some suggestive, though not entirely unambiguous, evidence that this had been adapted to Chinese handguns by the early fifteenth century, and both written and visual evidence that they were applied to troops equipped with European-style muskets by the 1560s.

Chinese, and indeed more widely Asian, traditions of military drill and discipline were autonomous, extended far back into the past, and were effective: Andrade's accounts of clashes between European forces and professional, drilled Chinese troops indicate that the Chinese were capable of the forbearance under fire that has been identified as the hallmark of European armies' success both on the European battlefield and in combat with numerically superior forces elsewhere in the world. Previous historians, most notably

Jeremy Black, have ranged widely over early modern warfare on a global scale to demonstrate that European military technology, drill and organization was by no means guaranteed success in encounters with non-European forces.[14] In general, however, the conclusion has been that other combat traditions had prevailed against European over-reliance on technology or rigid tactics. Andrade's case is that Europeans could be defeated on their own terms, by drilled, tightly-disciplined armies using the same repertoire of tactics and operational approaches to warfare.

These parallels were enhanced by technology transfers, and again, Andrade stresses, there was nothing in Chinese culture or the attitudes of governmental and military elites to impede the acquiring and deployment of European weaponry if this seemed to offer clear military advantages whether against Europeans, or more usually, against traditional Asiatic enemies. Both the arquebus and musket, and, on two separate occasions, different types of European cannon, were acquired, copied, and, in some cases, improved by better Chinese metallurgy. As in Europe, Chinese military innovation was driven by extensive and continuous warfare: Andrade shows how the patterns of intensive warfare from 1350 through into the 1700s are remarkably similar between China and Europe.

Yet this argument for military parity throughout the sixteenth and seventeenth centuries is presented with some significant caveats. In certain areas, Andrade argues, European technology stole a march on China, and this permitted the Europeans some crucial advantages. The first of these was the broadside-gunned sailing ship, and the second the "Renaissance fortress"—the bastioned, low-walled artillery-emplacements that had sprung up across Europe since the early sixteenth century and spread to extra-European colonies. The third, which became far more significant in the nineteenth-century as China faced the full onslaught of Western colonial powers, was experimental science and its impact in developing whole new levels of military technology.

What of the first two: the heavy gunned sailing ship and the Renaissance fortress? There seems some support from contemporaries for such a view: the superiority of European warships was acknowledged by the Chinese themselves, who in the early 1630s began to build a fleet "in the Dutch style." This was destroyed by a Dutch pre-emptive strike in 1633. Andrade speculates about why the Chinese made no further attempts to build Western-style vessels until the mid-nineteenth century: Qing China had no intention of contesting the Dutch presence in the Far East in general, and the European presence in China itself was contained and controlled; meanwhile far more pressing domestic threats and the strategy of pacification and annexation in Central Asia occupied military attention and resources. In Andrade's account, the superiority of the bastioned artillery fortress is equally clear: a means for Europeans to maintain a military presence while massively outnumbered by hostile local populations. A classic test-case of the utility of bastioned fortifications, the siege of Fort Zeelandia on Taiwan in 1661–62, which Andrade considered in detail in his earlier work *Lost Colony*, is central to the present account. It presents potentially ambiguous evidence: a Dutch artillery fortress that held out against a vastly larger Chinese army during a first siege, and only succumbed after nine months as the Chinese developed more sophisticated siege techniques. Similar

[14]Jeremy Black, *Beyond the Military Revolution: War in the Seventeenth Century World* (London: Palgrave Macmillan, 2011).

accounts are provided of Chinese military encounters with Russian bastioned fortresses in the later seventeenth century: the cost of taking them was high and would tie down armies that might be pursuing other objectives against an intrusive Russian presence. Yet taken they eventually were.

To a professed skeptic about the tendency to technological determinism in early modern military historiography, the case for the Renaissance fortress raises small but nagging questions about what in all other respects seems a convincing, and overdue, revisionist thesis about the nature of the military "great divergence." Paradoxically, the skepticism might even serve to reinforce the broader outlines of Andrade's thesis. Here, as in the work of other early modern military historians, there is a tendency to over-estimate the lethality of gunpowder weaponry and to underestimate the logistical difficulties of maintaining its supply and functioning. The *Réveries* of the Maréchal de Saxe, and other writings by eighteenth-century military memoirists and theorists, precisely at the point when European musketry drill was at its apogee, are a salutary warning for those tempted to write about devastating hails of musket fire, and musket volleys sweeping away advancing forces. Of the Imperial defeat by Ottoman troops at the battle of Belgrade in 1717, Saxe wrote: "I had curiosity enough to count the dead: I found only 32 Turks killed by the general discharge of the two battalions (around 1500 men, who had fired when the enemy was 30 paces from them)—which has not increased my regard for infantry fire." The Chinese professional troops who stood their ground against volley-fire by Dutch musketeers, were courageous in addition to being well-drilled; but they were not suicidal. Focusing on musketry, counter-march, and volley has always threatened to distort the bigger picture of early modern battles: European troops would ultimately need to engage, as they did in Europe, in direct hand-to-hand combat; this was where most casualties would occur, and where the advantages of gunpowder technology were minimal.

If the alternative might be to take refuge in the Renaissance fortress, a second problem arises: the defensive capacity of such fortresses was intimately linked to artillery, and the covering fire that bastion-mounted artillery could provide. From a European perspective the besetting weakness of such defensive systems was the substantial gap between the number of artillery pieces required to provide full covering fire, and the number of guns actually available in a given fortress. Demand for cannon meant competition with burgeoning navies and with field armies' need for siege trains; bronze-cast guns were extremely expensive, even by the standards of other military expenditure. Artillery in working order, well-supplied with powder and shot, and in a variety of different calibers and ranges, was certainly capable of deterring an assault on a fortified strongpoint like those built by the Portuguese, Dutch, and others in the Far East. Unlike infantry firearms, early modern artillery was unambiguously effective, both in terms of range and the variety of projectiles that could be used. But we have innumerable examples of European renaissance fortresses and state-of-art fortified towns which were chronically short of artillery, and especially artillery in working order. And we have further examples of those for whom inadequate stockpiles of munitions or the inability to resupply with powder and shot, rendered the entire defensive system untenable. Artillery supply and logistical support can be assumed to represent an even greater challenge in a colonial context: how well-provisioned with artillery and munitions was any given fortress, given the greater difficulties of purchase, transport, and constraining financial priorities?

In the case of the Chinese siege of Fort Zeelandia in 1661, it would seem from Andrade's accounts here and in his previous work that we do not know how many artillery pieces the Dutch defenders actually possessed. Soon after the Chinese arrival on Taiwan, a smaller, dependent Dutch fort surrendered on terms, the commander maintaining that the place was unable to resist as it possessed no more than 200 pounds of gunpowder and equally limited ammunition.

In this context the question of whether the artillery fortress was an "engine of European expansion" might still be worth revisiting. As I have argued in a European context, the effectiveness of fortresses depended on factors other than the shape of bastions and the careful placing of outworks as facilitators of artillery firepower. Though it was tempting for minor European rulers to embrace the rhetoric of the invulnerable fortress and to savor the dynastic prestige of fortress building, the decision was highly political: it required both a recognition that there was a major opportunity-cost in allocating military resources to building and maintaining a fortress, and that the decision itself would have political consequences in the attitudes and responses of other, particularly larger, states. There were undoubtedly cases where the building of renaissance fortresses in locations in East Asia that were contested between local powers, or on territory where the local power was weak, may indeed have been a facilitator of European expansion. From the evidence in Andrade's account, when such fortresses were built in or within the sphere of influence of Ming and Qing China, the results were similar to examples in contemporary European contexts where secondary powers built fortresses: larger military and logistical factors would prevail, whether the fortress surrendered immediately or whether it took a siege to demonstrate the unsustainability of the wider military-political position in which that lesser power stood.

Ian Inkster

A central feature of Andrade's *Gunpowder Age* is the analysis of the period approximately 1660 to 1839, not benchmarks of the Chinese calendar but of the years between the military revolution in Europe and the British aggression against China in the so-called Opium Wars. Clearly bounded by military clashes, Andrade's thesis is that Chinese military power "atrophied" just as western military power was revolutionized through "increase in size, organization, and technological sophistication." Following Parker, Pomeranz, and Wong, Andrade thus issues in a Great Military Divergence at the center of analysis, which allows the focus of Chinese problems to lie in the relatively short term, not so much as a result of some general Chinese cultural failure or regress or of the general industrial transformation of the west, as of its very much more specific military technological rise.

In this sweep, Andrade frees his argument from any obvious Marxian, Weberian, or Toynbeean European long-term perspective, and even from the longish-term of Gunder Frank's silver flows and "American" windfall gains.[15] Thus, the addition of

[15]André Gunder Frank, *(Re)Orient: Global Economy in the Asian Age* (Berkeley: University of California Press, 1998).

the combined Americas to form an Atlantic system might well have loaded things in favor of European commercial and industrial forwardness from the sixteenth century, and this may have been an indirect result of an independent rise in the Chinese demand for silver. But, the addition of such windfall gains, the accumulative, overwhelming new world resources, could not as such determine the direction or regime of technological progress in Europe that actually evolved during the years from the late eighteenth century, nor could it generate the undernourishment of Chinese military assets which emerged at the same time. Indeed, the latter occurred independently of global commercial developments, lying much more with an earlier Chinese (high equilibrium) success, which meant that the "Qing's unprecedented hegemony removed the stimulus for military innovation," something that can only be starkly contrasted with the European arms race that occurred during the Napoleonic wars. Globally neither cultures nor gross national products ever did clash, but navies and armies surely did! It was war that did it.

Although Gunder Frank, Ken Pomeranz, and R. Bin Wong are acknowledged positively and fully, the global comparisons and analyses that they and others have developed are touched upon only very lightly. Perhaps as importantly, fundamental work by Norman Jacobs in 1958 and the astute work of Daniel Headrick since the late 1970s is omitted from discussion.[16] This is not to be pedantic but rather to suggest that critical discussion of such authors might well have served to further enhance the comparative military-technological perspective developed here, and to expose more clearly the knotty problems of distinguishing between economic, political, and cultural processes within the overall framework. As one exemplar, Jacobs asked the now well-worn comparative question: "Why did modern industrial capitalism arise in one East Asian society (Japan), and not in another (China)?" and answered it in terms of socio-economic organization rather than in terms of technological capacities, whilst also adamantly maintaining that social systems "which do not develop capitalism are distinctly and positively different in kind even in their pre-capitalist stage from social systems which do develop capitalism."[17] That is, the values of industrial capitalist societies are not only distinct from those of failed systems, but were so long prior to the inception of capitalism itself. The systems that do not develop capitalist industry are those in which there was or is an utter absence of such "positive" values or propensities. The only logical conclusion to all this for Jacobs was that Japan succeeded because its pre-industrial feudal guild system secured rights which rising capitalism then assumed or took over, and thus "capitalism fitted into the traditional social structure."[18] In contrast, Chinese guilds never developed such rights nor did their successor commercial organizations. This is an interesting break with the Weberian approaches of that time, in that it stresses the continuities rather than dualism between feudalist organizations and capitalist development, but

[16]Norman Jacobs, *The Origins of Modern Capitalism and East Asia* (Hong Kong: Hong Kong University Press, 1958); Daniel R. Headrick, "The Tools of Imperialism: Technology and the Expansion of European Colonial Empires in the Nineteenth Century," *Journal of Modern History* 51, 2 (1979), 231–63, and his *The Tools of Empire: Technology and European Imperialism in the Nineteenth Century* (Oxford: Oxford University Press, 1981).

[17]Jacobs, *Origins of Modern Capitalism*, ix, 212.

[18]Jacobs, *Origins of Modern Capitalism*, 214.

remains fundamentally part of the liberal conventionalist corpus, as it stresses the decisive importance of underlying historical traits derived from euro-history.

Andrade does show that "cultural" factors might be enlisted not so much to explain lack of innovation but rather a tendency in China for induced innovations to follow patterns that were less likely to be mechanical and metallic and more likely to be institutional and procedural. An older but always problematic thesis did argue that the institutional innovations in the rural economy that allowed China to mobilize its labor resources for economic surplus from the latter half of the nineteenth century were induced by an economic stagnation and a search for more effective control over local and regional resources.[19]

Innovation without disruption was perhaps a far more ingrained world view of Chinese governance than was any search for technological change, but this might have been of little importance in determining the historical outcomes if such governance had not been imperial, or if physical reality had been closer to that of the west—the ubiquitous bamboo so readily substituted for most needs that were met by iron and coal in Europe, short but many-sided and contested borders between European states constantly invoked searches for competitive advantage, whether from extended foreign markets, improved weaponry or utilization of a wider range of natural materials, especially minerals. Securing any one of these desiderata induced changes in technology—market frontiers could only be broken through efficient transport, increasingly by sea; any revolution in weaponry required a cultural jump from wood and man-force to metal and gunpowder. In contrast, a bamboo economy could feed a growing population without much pressure upon mechanical contrivances, expansion of markets by sea or even imperial governance, and none of this demanded a populous, bustling world of metal-using artisans whose workshops could through time develop as the production sites of a new industrial manufacturing.[20]

Furthermore, the Andrade perspective does not require any argument that such Chinese governance was either a sole determinant of failure in the face of the rise of the west from the 1660s, nor that it was forged deep within an unshifting Chinese literati culture that either knew nothing of or refuted the western alternative—Andrade makes nothing of the Macartney Embassy of 1793. Throughout his development of these themes, in the present absence of any grand theory, there is an astute maneuvering of arguments that resonate off a social constructivist view of technology, to approach at one time the cultural, at another the economic, at another the political. It seems to me that in present world history no one is succeeding in doing much more than this.

[19]More realistic views of the Maoist component of this history have of course much modified the assertions of both actual economic surplus and the forces of inducement; see Barrington Moore, Jr., *Social Origins of Dictatorship and Democracy: Land and Peasant in the Making of the Modern World* (Boston: Beacon Press, 1996),162–227; Ramon H. Myers, *The Chinese Peasant Economy: Agricultural Development in Hopei and Shantung 1890–1949* (Cambridge, Harvard University Press, 1970).

[20]The prime reason for the inability of the Chinese to initially invent the classic Watt steam engine, was not failure of imagination, lack of theoretical knowledge or inhibitions from governance, but more simply an inability to envisage or reliably create a stable vacuum space enclosed in metal that acted as the prime mover of a pumping engine relying on atmospheric pressure: Ian Inkster and Patrick O'Brien eds, *The Global History of the Steam Engine*, Special Issue of *History of Technology* 25 (2004), 25.

Andrade's work is very much in line with recent tendencies in history of science and technology that aim to free the western story, in particular, from claims of cultural—even racial—particularity. If we adhere strictly to the book, we no longer need to seek a Chinese cultural argument (of failure to keep up in technology or governance) to juxtapose with a western cultural argument (of success in thrusting forward into industrial capitalism, technological superiority, and colonialism), and thus need not conclude as did Kurt Mendelssohn (1906–1980, FRS and emeritus of Wolfson College, Oxford) in 1967 that

> other races had no option but to imitate Western methods, since they had nothing of their own to offer the world of technological progress and success. Largely by their own choice, and certainly by their own default, they became economically second-class citizens of the world. Thus, the white man's domination was not primarily due to his cruelty or his aggressiveness, but to the long and patient development of his own natural philosophy.[21]

It is possible to draw some world analytical synthesis here from the history of technology if we are prepared to reflect back upon the core approach of the great Joseph Schumpeter.[22] If we acknowledge a version of the Schumpetarian model for technological change in history, then much of Chinese history not only becomes more "rational" in a western perspective, but may well be thought of as at least equally productive. Schumpeter always embraced within his notion of technological innovation those efficiency increases resulting from innovations in institutions, markets and governance. So, the fast growth of Japan in the 1960s and 1970s had much to do with technological change; but isolating that from the undoubted plethora of organizational innovations is no easy matter.[23] This is clearly much more of a problem with any nation or region for say the eighteenth or nineteenth centuries. It might be that imperial governance and the exigencies of huge, difficult, and very varied frontiers did direct the Chinese elite into institutional adaptation and social continuity, as against the more free-floating, discontinuous technological changes of Europe and then America. But measuring whether the one was more or less productive than the other is a difficult matter, and with recent work by O'Brien and Deng that casts doubts upon many of the statistical presumptions of the recent global accounts,[24] we might conclude as a reasonable if temporary position, that in China economic growth and its welfare outcome stemmed more from institutional innovations within a continuing governance, than in the west where technological change (in the more direct sense of machines and structures) was far more prominent and ad hoc. That one was far more growth-inducing remains in doubt for the long years into the eighteenth century, but from thereafter

[21]Kurt Mendelssohn, *Science and Western Domination* (London: Thames and Hudson 1976).

[22]Joseph Schumpeter, *The Theory of Economic Development* (Oxford: Oxford University Press, 1969; first edition, 1934 by Harvard University Press); for a synoptic application, see Ian Inkster, "Inertia and Technological Change: An Elementary Typology," in *Industrial History and Technological Development in Europe*, ed. Pascal Byé and Daniel Hayton (Luxembourg: European Commission, 1999), 343–48.

[23]Ian Inkster, *Japanese Industrialisation: Historical and Cultural Perspectives* (London: Routledge, 2001).

[24]Kent Deng and Patrick O'Brien, "Why Maddison was Wrong? The Great Divergence between Imperial China and the West," *World Economics* 2, 18 (2017), 21–41.

the ascendency of the west arose from her early choice of metal-based machinofactures that gave her the military advantage when systems clashed. Birmingham produced both small toys and very big guns. And when civilizations clashed, cultural conflict on one hand and varying levels of per capita income on the other meant far less than the variations in military capacities and in the natural and man-made resources upon which they could draw.