Review Essay/ Essai Bibliographique

Garbage In, Garbage Out: Challenges of Model Building in Global History, A Military Historical Perspective

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Abstract. This paper examines two prominent recent attempts to explain the phenomenon of the "rise of the West," Ian Morris's model of "Social Development" and Philipp Hoffman's model of military power (Morris 2010, Morris 2013, Hoffman 2012, Hoffman 2015). Whereas most recent scholarship on the rise of the West has focused on economics, Morris and Hoffman widen the scope of comparison to other areas, in particular focusing on the measurement and explanation of divergences in military effectiveness. By drawing on recent work in China's military history, the author shows that both models – but particularly that of Morris – are inadequate, falling back on older narratives of Western military superiority that have been challenged or disproven by recent scholarship in global military history. The article suggests, however, that the two models – and especially that of Hoffman – do raise significant new questions for future research, and it concludes by noting that what social scientists need more than new models at present is a closer attention to the rapid and ever increasing proliferation of scholarship in non-Western countries, and in particular that of the Sinophone world.

Keywords: Military History, The Military Revolution, The Rise of the West, The Great Divergence, The Revisionist Model

Introduction

How do we account for the "rise of the west," that astonishing process by which the once-marginal states of Western Europe rose to global dominance? The puzzle is as old as social science itself, and in recent years it has become a key part of the discourse of global history, generating hundreds of articles and scores of books, including a seminal discussion in *The Canadian Journal of Sociology*.[1] This paper examines two new attempts to solve it – Ian Morris's model of "Social Development" and Philipp Hoffman's model of military power (Morris 2010, Morris 2013, Hoffman 2012, Hoffman 2015). Whereas most recent scholarship on the rise of the West has focused on economics, Morris and Hoffman widen the scope of comparison to other areas. In particular, both authors have a preoccupation with measuring and explaining divergences in military effectiveness. This is a commendable pursuit, because in all the recent work on East-West comparisons, the military balance has largely been left out.[2]

This paper evaluates the Morris and Hoffman models in light of recent findings in global military history. These findings, particularly new scholarship from the Sinophone world, has begun to upend long-held stereotypes about "West" and "East," and it forces us to reject key parts of both models. Yet that does not mean that the authors have written in vain. As Max Weber noted a century ago, no model is perfect, especially in the social sciences (e.g., Weber 1949). A hypothesis is useful to the extent that it's good to think with, helping us generate new research questions. The Morris model is not very good to think with, at least as regards military history. The Hoffman model, however, despite being wrong in certain key respects, does raise significant new questions for comparative research.

Yet what we most need now in global history is not so much new models as more data. Fortunately, there is an explosion of research in non-Western history, particularly in China, where for the past two decades new journals, university programs, and research centers have proliferated wildly. Much of the scholarship generated there is of very high quality, and it promises to revolutionize our understanding of global history.

1. Among the most important works are Pomeranz 2000, Wong 200, Rosenthal and Wong 2011, Frank 1998, Marks 2007, Parthasarathi 2011, Duchesne 2011, Bryant 2006, Landes 1998, Landes 2006, Ferguson, 2011, Huff 2011, Lin 2012. Many of these works are discussed in the seminal debate in The *Canadian Journal of Sociology*, which was touched off by the thoughtful but highly critical Bryant 2006: see especially Goldstone 2008, Elvin 2008, Bryant 2008, Andrade 2011.
2. But see Bryant , "A New Sociology"; Bryant, "The West and the Rest"; Goldstone, "Capitalist Origins"; and Andrade, "Accellerating."

Morris

Ian Morris's book *The Measure of Civilization* and its companion volume *Why the West Rules, For Now* attempt to explain the rise of the West by assessing its level of "social development" relative to "the East" (Morris 2010 and 2013). By social development, Morris means the aggregate effectiveness of a social group: its ability "to master [its] physical and intellectual environments and get things done in the world" (Morris 2013: 3). By "West" he means Europe and the near East; by "East" he means China and sometimes Japan.

Morris believes that most existing work on the topic is too qualitative, so he attempts to introduce a quantitative "Social Development Index," which assigns a single score to each area – "East" and "West" – for each period from 14,000 BC to 2000 AD. On the basis of these scores, he concludes that the "West" had a higher social development capacity until around the year zero of the western calendar, after which the "East" became gradually more ascendant. Starting in 1500 or so, the "West" rose again, catching up with and then surpassing the "East" by 1800.

How does he come up with single social development scores for East and West? By aggregating separate scores on four aspects of social power: energy capture; social organization; warmaking capacity; and information technology. Consider, for example, his scoring methods for social organization. That score is based on the size of the largest city within the geographical region in question. So the West's relatively high social organization score during the classical age and its subsequent decrease in social organization relative to the East had to do almost entirely with the population of the city of Rome, the largest city in the "West." Critics have pointed out that the size of a city may not correlate highly with social development. After all, today Kinshasa, in the Republic of Congo, has a greater population than New York but few would score it higher in social development. Morris understands this and admits that no scoring system can ever be perfect. He attempts to find objective numbers and feels that city size is a useful one. He also suggests that although there might be troubles with individual scorings, in the aggregate such issues will balance out because it is the overall score that is most important.

A more pertinent example for our purposes is his scoring for "war-making capacity." He assigns the East's "war-making capacity" circa 1500 a score of 0.11, as compared to 0.13 for the West. "This would mean," he observes, "that Chinese war making rose to match the peak Roman levels only around 1600" (Morris 2013: 203). He writes that by 1600 the East's war-making capacity rose to 0.12, while the West's rose to 0.18. By 1700 it rose to 0.15, while the West's rose to 0.35. He comments that this means that the Qing Emperor Kangxi's (r. 1661–1722) warmaking capacity "was midway between that of the Roman emperor Augustus [0.12 points] and that of the Habsburg emperor Philip II [0.18 points]" (Morris 2013: 204).

To anyone versed in Chinese history, this conclusion appears absurd. To say that Kangxi was, at the height of his military power, less militarily effective than the Habsburg Emperor a century before is an astounding claim that betrays a serious lack of understanding of Chinese warfare. It is the same with Morris's judgment that China's military capacity only reached "peak Roman levels" by 1600. It begs one to imagine how Roman legions might have stood up to hundreds of thousands of disciplined Ming troops armed with muskets and cannons. More importantly, this kind of scoring implies a consistent forward development, when in fact military capacity rose and fell depending on threats and challenges.

So how did Morris come up with the scores that underlie such judgments? Did he rigorously compare troop levels, weapon production statistics, military engagements? No. His scores are based on qualitative judgments. Consider, for example, his treatment of Ming dynasty warfare. He writes that "The Ming government may have had access to a few Western cannons as early as the 1520s, but if so, they remained curiosities until the 1540s" (2013: 203). Then he simply moves on to Japan. In this way he justifies assigning to the East a lower warmaking score than the West. But in fact his information is simply wrong. Evidence makes clear that Ming officials began adopting Western guns as soon as they encountered them, in the 1510s, and by the 1520s they were making large batches of Portuguese-inspired guns in central armories. There is strong evidence of significant production at the provincial level as well, which probably preceded that in the center. Moreover, if we look closely at the first significant battles between Chinese and Western European armed forces – the Sino-Portuguese War of 1521-22, we find evidence of technological parity. During the battles of 1522, Portuguese sources note, the Chinese artillery was devastatingly effective, and it contributed significantly to the Portuguese defeat (Andrade 2015a).

Another example concerns his treatment of muskets, which in the West became the mainstay of infantry warfare by around 1600. Morris writes that although the Japanese made "effective copies" of western firearms, Chinese did not effectively import them into their armies. "Even," he writes, "the celebrated Qi's Army that turned the tide in the mid-sixteenth-century pirate wars featured very few musketeers compared to contemporary European armies. Their guns were often amateurishly made and tended to explode, which discouraged gunners from getting close enough to their weapons to aim them properly" (Morris

2013: 203). This, too, is wrong. In fact, the Chinese General Qi Jiguang, who developed those celebrated Qi Armies, was a devoted partisan of the musket. The idea that his army had few musketeers has been effectively refuted by the work of historians like the great Wang Zhaochun and others, and if you look carefully at the sources, you find that in fact Qi Jiguang prescribed high percentages of musketeers for his infantry forces (Andrade 2015b). There is no credible evidence that his muskets were amateurishly made or particularly liable to explode. He certainly seems to have considered them a central weapon, one of the most effective guns available. As he wrote, the musket is "unlike any other of the many types of fire weapons. In strength it can pierce armor. In accuracy it can strike the center of targets, even to the point of hitting the eye of a coin [ie, shooting right through a coin], and not just for exceptional shooters…. [T]he musket is such a powerful weapon, and is so accurate that even bow and arrow cannot match it, and … nothing is so strong as to be able to defend against it."[3] Nor is there significant evidence that his musketeers couldn't aim or shoot their guns properly. In his detailed prescriptions for target drills, Qi Jiguang makes clear that standards for accuracy were quite high. Moreover, when it comes to tactical effectiveness, his firearms units were probably more advanced than those of Europe. Military historians of Europe have suggested that the musketry countermarch technique – by means of which musketeers kept up a constant hail of bullets by taking turns firing – was developed around 1600 in the Low Countries, but one finds very clear references to the technique in Qi Jiguang's writings as early as 1560 (Qi: 2001: 56).[4]

Another example of Morris's odd interpretation comes when he writes that "the garrison of Beijing … shifted from clay cannonballs to lead only in 1564, moving on to iron (like the Europeans) in 1568, and only in the 1570s did Qi Jiguang introduce light cannons on carts protected by wicker barriers, like those the Hungarians had used against the Ottomans at Varna in 1444" (Morris 2013: 203).[5] This is quite a bundle of misconceptions. First, it betrays a subconscious (or perhaps conscious?) linearity. Morris seems to believe that there a technological advance from clay to lead to iron, each better than the last. It's true that

3. The term I translate as "musket" is in Chinese *niao chong* (鳥銃). It might also be translated as "arquebus."

4. On the development of the countermarch in Europe, see especially Parker 1996 (esp. 18–23), Parker 2007, and Nimwegen 2010 (esp. 100-112). On the countermarch in China, see Andrade 2015c.

5. He doesn't cite a specific source for his data, instead providing a general reference, without page numbers, to Chase 2003. I couldn't locate any passage in Chase 2003 containing a reference to this apparent shift from clay to lead to iron.

iron is quite effective as a projectile, having a greater density than clay or stone and thus carrying far more kinetic energy by volume at a given velocity. But we also know that the Chinese and their neighbors were using iron ammunition by the 1200s. There's no doubt that the Ming Firearms Commandery, the central bureau that oversaw firearms production and training, had produced plenty of iron ammunition well before 1564, the date at which Morris says it began producing iron ammunition. Indeed, it had been doing so since its establishment in the early 1400s. So we shouldn't see this putative switch from clay to lead to iron as a progression or linear development. In any case, the Chinese and their enemies (such as the Khitan, the Jurchens, and the Mongols) were using iron ammunition well before European even had guns.

As for Morris's claim that China lagged behind the West in mounting guns on carts, Chinese precedents for the practice can be found as early as the twelfth century, three centuries before the Hungarians built their famous cannon carts. In 1163, for example, a Song commander named Wei Sheng prepared several hundred "at-your-desire-war-carts" (如意戰車), each of which mounted fire-lances (gun-like gunpowder eruptor weapons) whose barrels protruded through protective coverings on the sides. The Song court was impressed with the innovation and ordered that the carts be copied by other divisions of the army (Wang 1991: 28). Thus, the Chinese gun carts that Morris considers a tardy adoption compared to those of Hungary were part of a long tradition of armored battle wagons, about which a young Chinese scholar has recently written a very informative dissertation (Zhou 2008). Again, the data make clear that China was rather more precocious than retarded.

Another oddity in Morris's discussion concerns his treatment of the Song dynasty period (960–1279). He writes that during that period China's war-making capacity "did not equal imperial Rome's" (Morris 2013: 208). But he is maddeningly imprecise about his units of analysis. Whereas he freely ranges between various states in Europe, he oddly neglects the Western Xia Empire (1038–1227) and the Jin dynasty (1115–1234), both of which coexisted with the Song Dynasty. Indeed, most experts believe the Jin to have been more powerful than the Song. He similarly glosses over the differences between the Northern Song (960–1127) and the Southern Song (1127–1279) suggesting that the Song Dynasty in 1200 was more militarily effective than the Song Dynasty of a century or so earlier, even though it controlled far less territory. In any case, the great military power of the late 1100s was the Jin Dynasty, which, as I noted, he barely mentions, referring to it merely by the ethnic name of its ruling strata: the Jurchens (Morris 2013: 208).

Morris's discussion of military effectiveness is filled with many other examples of judgments based on poor understanding, and one might be tempted to object that it's not fair to harp on details like these. After all, as he himself admits, "directly comparing Eastern and Western war-making capacity before 1900 CE is a very rough-and-ready business" (Morris 2013: 202). It seems that he believes that small errors don't matter in the aggregate, because what is important point is how the overwhelming weight of the evidence adds up.

But is it true that little errors will be washed away as we move toward broader frames? No. Big conclusions are based on many small details. If the small details are wrong, they affect the overall picture. Indeed, the effects of these mistakes probably don't decrease as we move toward grander scales. On the contrary, they might have a multiplying effect. As economic historian Eric Jones noted, in a thoughtful and critical review of Morris's work, "The difficulty is that errors from pressing insecure evidence into sometimes uncertain boxes may not be additive, but multiplicative" (Jones 2013).

But an even more damning point is that when we look at Morris's scoring for military matters, we find it based on biases that are themselves encrusted in old, discredited metanarratives. It is quite clear from his discussion that he expects to find military developments that are in some sense unilineal, improving over time – except in the case of the West during the post-classical period, which is also a quite traditional metanarrative: classical florescence falling to the Dark Ages. Similarly, influenced by notions of the European renaissance and its early-modern florescence, he expects to find the military of the mid- and late-Ming periods more backward than Europe's, with Ming guns few and liable to explode. That is, after all, the standard view.

With his qualitative judgments are based on these standard narratives, it's no wonder that all the small details end up resulting in scoring that supports a large-picture perspective that seems so traditional, fitting the usual chronology of the rise of the West: a glorious classical era, when the West led the world; a less-glorious medieval period, when the East excelled; and then a period of western rise starting around 1500 and accelerating throughout the next centuries until the West's lead was obvious and undeniable circa 1800.

So we cannot excuse the errors in the details, because our large-picture perspectives are very much determined by those details. New details help build new narratives.

Given the problems with Morris's scoring, it is hard to take his model seriously. Yet that does not mean that the idea of a historical social development index is futile. With proper data, it should be possible to create a more successful index. As we learn more about non-Western warfare, for instance, we will be in a better position to compare it with Western warfare. For the early modern period we might compare the numbers of guns produced each year, the numbers of soldiers armed with guns, the percentage of these soldiers who trained regularly, etc.

The picture that will emerge will be far more nuanced than that painted by Morris. During the early Ming period, for instance (i.e. from the 1350s to 1450 or so), we would have to assign a far higher score for military development to China than to Europe because there were far more troops and far more guns and gunners in China than in Europe. In fact, during the 1400s, there were more gun units in Ming armies than there were soldiers of any kind in all of England, France, and Burgundy combined, and the ratio of gunners to traditionally-armed troops was much higher in China than in Europe. By the late 1400s, Ming armies had stopped growing and Ming military techniques had ceased developing, whereas European armies and techniques began developing particularly rapidly. This was, however, a temporary situation, and by the mid-1500s, Ming military innovation accelerated rapidly again, even as European developments also proceeded apace, and so military development scores for "West" and "East" would begin to run parallel paths, although I suspect that China's would likely still be higher through the late 1600s. Thus, new data will allow more rigorous comparisons of European and East Asian military effectiveness.[6]

Morris may point the way toward better models in the years to come, but at present his model is too flawed to be relied upon. Nor is he the only contemporary social scientific model builder to fall prey to discredited narratives of Chinese military stagnation. We see the same biases in Phil Hoffman's recent work, although, to his credit, Hoffman is rather more aware of recent historiographical developments.

Hoffman

Phil Hoffman's intervention in the rise of the West debate is more satisfying than Morris's partly because his focus is tighter (Hoffman 2015). He takes as his starting point the model of the Military Revolution that has been put forth by historians of early modern Europe, and in particular Geoffrey Parker (Parker 1996, Parker 2007, Roberts 1956, Rogers 1995). The military revolution thesis holds that European expansion during the period 1500–1800 was made possible by Europeans' superior military capacities, and that this military advantage is what enabled

6. This is a task I undertake in Andrade 2016.

Europeans to control 35% of the earth's lands by 1800. But whereas Parker and other military historians attribute Europeans' military edge to a congeries of military developments – technology, tactics, organization, logistics, statecraft, and fortification techniques – Hoffman reduces the European advantage to one factor: Europeans supposed superiority in "the gunpowder technology," by which he means firearms and artillery (Hoffman 2015).

Moreover, Hoffman disagrees with the causative aspects of the military revolution model. Whereas Parker and others argue that Europeans' military advantage arose because Europe was unusually warlike over a long period, from 1500 through 1800, Hoffman argues that this is too simple on the grounds that many other parts of the world had persistent military competition. He believes that areas outside Europe, despite much warfare, did not innovate as effectively in gunpowder technology as Europe (Hoffman 2012: 602). In fact, evidence suggests that non-Europeans were far more militarily effective in gunpowder technology than Hoffman suggests, but let us give him the benefit of the doubt for now and attempt to understand his argument (Andrade 2011b and 2016).

Believing that military competition alone is not sufficient to explain Europeans' advance in "the gunpowder technology," he adduces additional factors. First, he argues, European states were small. He believes that distance was the primary barrier to the spread of innovation, so the small states of Europe easily borrowed from each other, whereas the large polities of Asia – and here he is referring primarily to China – were less permeable to innovation. Europe's small states also made possible the geopolitical balance that prevented any single polity from defeating all the others, allowing competition to persist. Moreover, smaller states had an advantage in assembling armies because, he argues, warfare grew increasingly more difficult and expensive as its scales increased. This greater expense acted as a disincentive to the leaders who decided whether or not to go to war.

Second, he argues, Europeans were primarily focused on warfare against territorial states, whereas many Asian states – including China – were focused on warfare against mounted nomads. He believes that gunpowder technology was of little of less use against mounted nomads, so polities that fought nomads didn't invest in gunpowder technology nearly as much as those that fought against territorial states. Europeans primarily faced infantry forces, which were increasingly armed with guns, and this infantry-on-infantry warfare stimulated the development of gunpowder technology.

Third, he argues that the costs of going to war were much lower for European states. He posits three main reasons for Europeans' lower costs for warfare. I have already noted one of those three reasons: that the small size of European states meant that warfare was on a smaller (and cheaper) scale than in areas where states were large. A second reason was fiscal: European states, he believes, were able to glean higher rates of taxes from their populations. His final reason is economical: he argues that guns were much more expensive in China than in Europe, and so European warmakers got more bang for the buck. The result of all of these phenomena was, he believes, a sort of virtual cycle: war was cheaper in Europe, so sovereigns there had a greater incentive to resort to war, which caused innovations that further reduced costs for war, and so on.

This is an elegant model, and in contrast to Morris's, it is testable. How does it stand up to data? Not so well.

For one thing, Hoffman's model relies on the Military Revolution theory. Like Geoffrey Parker and other military revolution theorists, Hoffman believes that European expansion before 1800 was undergirded by military superiority. He downplays, for example, the significance of disease in the European conquest of the New World. Yet evidence does actually point to the vital role played by disease in New World conquest. Of course Europeans' weapons and the ways they used those weapons also played a significant role, but we must keep in mind that of that 35% of the globe's surface colonized by Europeans, the vast majority was in the Americas. The only significant areas colonized (and held) by Europeans in the Old World (that is to say, in Africa, Asia, and Oceania) before 1800 were Siberia, parts of South Asia, and parts of Southeast Asia. And it's not clear how significant a role Europe's putative military superiority played in those Old World conquests. Historians are increasingly finding that within Eurasia Europeans may not have had the military edge over non-Europeans that has traditionally been ascribed to them, and even the classical works proposing the military revolution theory are much more cautious about Europeans' technological superiority over Asians than Hoffman is (see especially Parker 1996, 107–145).

Second, Hoffman's model – like many arguments about China – overstates Chinese political unity. When he contrasts Europe's small states with Asia's large states, his primary reference point is China, and it's of course true that for much of its Late Imperial Period (1368–1911), China was ruled by two huge unified dynasties. But the periods of war and disunity in China were not trivial, and they turn out to be of signal importance in China's military history. For example, when comparing the numbers of conflicts China fought to those fought by European states, Hoffman generally discounts intra-Chinese warfare (including warfare against rebels and pirates), but in fact that kind of warfare was highly significant, especially during times of dynastic transition, such as the periods surrounding the Yuan-Ming transition (1350-1450) and the Ming-Qing transition (1617-1683). These were times of military competition and innovation, when gunpowder technology evolved extremely quickly.

Third, the notion that China's warfare primarily focused on mounted nomads and that firearms were of little use against them is probably wrong. This notion is a key pillar of Hoffman's argument, marking the most significant distinction he draws between Chinese and European warfare. The notion has a distinguished pedigree (most importantly Chase 2003 and Allsen 2002), but historians, myself included, have found that from the 1300s on, Chinese considered guns very useful indeed against mounted nomads. On innumerable occasions in the fourteenth, fifteenth, sixteenth, seventeenth, and eighteenth centuries, imperial armies used guns to attack mounted nomads and to defend against them, with considerable success (Andrade 2016). Moreover, there is plentiful evidence of innovation in anti-nomad gunpowder warfare. Chinese officials designed guns for use against mounted foes, making them shorter and faster to fire, and adapting them for use on horseback (Andrade 2017 and Feng 2012: 59).

Even more importantly, China faced many non-nomadic enemies. Hoffman, like many others, doesn't account for the tremendous range and diversity of Chinese warfare. Armies of southern China were organized and armed differently from those of northern China. In the south, where armies faced non-mounted enemies, infantry was the core force, and musketeers and cannon units drilled in tight formations with units carrying more traditional weapons, such as lances and swords, as was done in early modern Europe. Indeed, there is compelling evidence that Chinese musket units deployed countermarch tactics well before those tactics permeated European warfare, and the ratios of gunners to traditional units seem to have been as high or higher in Chinese infantry units through 1600 (Andrade 2015b). Even in the north, where Chinese armies faced mounted nomads, gun-toting infantry trained to operate in close conjunction with cavalry, even as cavalry divisions themselves often contained specialized firearms units. The difference between northern and southern Chinese warfare was frequently noted by the Chinese themselves. During the Japanese invasion of Korea, for instance, Chinese generals who fought on the Korean side soon realized that their northern troops were far less effective than southern troops, and so they increasingly adopted southern styles of training and armament (Swope 2009, 162–63).

Fourth, Hoffman's argument that warfare was cheaper in Europe than in Asia is also suspect. He provides just two data points about Chinese gun prices, which were gleaned from a personal communication with a Chinese scholar, who in turn pulled them from a single northern Chinese official source written during a time of tremendous conflict, during which guns were in great demand even as supplies were low (Hoffman 2011: 52). In fact, however, China's diversity of warfare extended to weapon procurement and manufacture. Guns manufactured in centralized weaponry bureaus in and near Beijing were probably more expensive than those produced in the provinces, especially the southern provinces of Fujian and Guangdong, which were known for their metallurgy. When Beijing sought to enhance central bureaus' gunmaking techniques, it recruited southern Chinese artisans, and evidence suggests that gunmaking was quite developed in the maritime provinces. Indeed, maritime Chinese such as traders, smugglers, pirates, and port officials, were often the first to obtain new styles of foreign guns, which reached their shores by way of the maritime trade routes that so firmly linked China to Southeast Asia and the Indian Ocean world.

This intra-Asian system of arms circulation is only now being understood, and Hoffman doesn't mention it at all. Although we have much to learn, it seems that speeds of transmission were quite fast, which casts doubt on Hoffman's assertions that Europeans benefitted from uniquely speedy diffusion speeds for weaponry because of the small sizes of their states. For instance, excellent matchlock-style guns were being manufactured in what is today India possibly before the arrival of the Portuguese themselves in the early 1500s, and those guns appear to have spread quickly throughout maritime Asia, to the shores of China and Japan, probably preceding the Portuguese themselves (Daehnhardt 1994; Udagawa 1990).

Similarly, China's huge size doesn't appear to have slowed diffusion. When Chinese officials captured Portuguese-style guns in southern China in 1522, they shipped them northward, 1500 miles along established communications routes. By the following year similar models were being produced in Beijing, whose armories were soon producing thousands of them (Wang 1991: 127–29, Feng 2012: 59).

We have much to learn about the economics of gun procurement in the Ming and Qing periods, but there is no doubt that it was far more complex and multi-layered than Hoffman suggests. It's not impossible that guns were more expensive in China than in Europe, but we need far more data. So far there is no significant study of Chinese gun prices, but I believe it highly doubtful that local foundries and production centers – particularly in the maritime provinces, had prices significantly above those of Europe, given how competitive other Chinese manufactures – including metal works – were in intra-Asian markets.

Like Morris's model of social development, Hoffman's model of military power recapitulates standard narratives of world history, suggesting that the military divergence between Europe and China began by 1500 and grew increasingly wide over the following centuries.

Yet this narrative is being rewritten by global military historians, who are instead adopting a far more nuanced one (Lorge 2008, Andrade 2016). Hoffman, like most other historians, skips over the early periods of gunpowder weaponry, on the theory that gunpowder weapons mattered little before they became significant in Europe. Yet evidence suggests that from the invention of gunpowder around 800 AD to 1450 or so, the various states that existed in what is today China were the world pioneers and leaders in gunpowder technology. Conflagrative weapons, bombs, mines, grenades, gunpowder eruptors (fire-lances), proto-guns, and guns evolved first in China, spreading thereafter to the rest of the world. Historians have tended to neglect this period or, when discussing it at all, have tended to suggest that these developments were slow, and that it was only when Europeans got gunpowder that innovations sped up and powerful guns were created. Hoffman generally accepts this narrative, but it isn't true.

In China, for example, from the period from the development of the gun in the mid-thirteenth century through the massive wars of the fourteenth and early fifteenth centuries, guns evolved rapidly, and in ways quite similarly to the way they evolved in Western Europe after arriving there in the 1320s. In fact, through the 1300s and the first half of the 1400s, guns were used on Chinese battlefields much more frequently and effectively than in Western Europe. To be sure, Europeans (and their neighbors, most notably the Ottomans), developed large artillery and the Chinese didn't follow that path, but there seems to be a simple reason for that: Europeans had thin, brittle walls relative to Chinese, whose earthen-core walls were an order of magnitude thicker. The Chinese didn't build large guns because their walls were immune to early guns (Andrade 2015b and 2016).

European guns did improve rapidly in the second half of the 1400s, whereas Chinese designs stabilized (one might even say "stagnated") from around 1450 or so. By the mid-1500s, however, Chinese firearms innovation was taking off again. Why? Because of warfare. Hoffman's model has a low resolution – when comparing levels of warfare in China and Europe, he compares large units of time, even as he leaves out much of the most important warfare in China on the grounds that it concerned rebels and pirates. Yet to understand the pattern of China's military past, we must look in greater detail. It's certainly true that European states fought frequent wars from the 1300s through the 1700s (with a significant decrease during the 1700s relative to the 1600s), but China did too, with some slight differences: China saw frequent wars in the 1300s and early 1400s. China experienced a period of lower levels of warfare during the second half of the 1400s. This was followed by a period of intense and sustained warfare, from 1550 or so through 1683. During the entire period from the 1300s through the late 1600s, China's gunpowder technology was either better than or equal to Europe's, with one small lag, between 1470 or so to 1520, when China briefly fell behind because of a period of relative peace. It caught up quickly by the 1530s and continued innovating through the 1680s.

Indeed, the only period during which China fell significantly behind Europe was during the eighteenth century, particularly from 1760 or so to 1839, when the Qing Dynasty, having achieved a position of unprecedented hegemony, its territory and authority greater than any previous dynasty, fought few wars (Wakeman 1986: 1125-26, Perdue 2005: 526–27, Andrade 2016). Quantitative data suggest that that period, which one might call the Great Qing Peace, saw less warfare than any other period in China's imperial history. With no significant external enemies and with relatively quiescent internal enemies (quiescent in the Chinese context, where rebellions and revolts could be enormously destructive), the Qing faced little military stimulus and its military institutions and technologies stagnated, even atrophied (Andrade 2016: 1–14 and 312–316).

So the simple narrative that pervades Hoffman's study – that Europe experienced a quicker pace of innovation than China and thus surpassed it – is overly linear. The real pattern was actually far more cyclical. China's periods of most intense gunpowder technology innovation coincided with its periods of most intense warfare. When wars were frequent and consistent — as in the periods 1350–1450 and 1550–1683 — China's military innovations were swift and effective. Government officials, gentry, merchants, and mariners innovated, adapted, and adopted, drawing ahead of or at least keeping pace with developments in Western Europe. In contrast, during periods of relative peace — 1450–1550 and, more importantly, 1760–1839 — military innovation in China slowed.

This more nuanced narrative of Chinese military history suggests that perhaps Hoffman's model, aside from being based on wrong assumptions, is unnecessary. Hoffman believes that a standard "states system" explanation, which focuses on levels of geopolitical competition alone, is inadequate because geopolitical friction seems to be present throughout the world, and he therefore adds other variables, most notably the stipulations that contending states must (a) be small, (b) be focused on warfare against territorial states (and not mounted nomads), and (c) experience lower costs for going to war (particularly costs for guns themselves). Yet Hoffman has simply not attempted to compare with any level of precision levels of warfare in Europe with levels in China or elsewhere in Asia. To be sure, he does mention other periods of geopolitical competition in Asia, most notably South Asia during the eighteenth century, but his level of analysis is extremely rough. Indeed, when he suggests that South Asia saw little indigenous military innovation during its warlike eighteenth century, he is probably not correct. Evidence suggests that there was in fact rapid military innovation in South Asia at that time.

Still, although Hoffman's model doesn't seem to answer the question of why Europe conquered the world, it is still useful because it raises good questions. Most importantly, it challenges us to conduct more research into the relative costs of gun procurement in Europe versus other areas of the world. Although his evidence about China's supposed high cost of gunpowder weapons is exceedingly scanty, he has put his finger on an important question, and examining gun procurement and production costs in China will lead to new insights.

Similarly, we should focus on identifying other "warring states" periods in Asian history. My own recent work looks at such periods in China's long history, including periods during which China was supposedly unified, as in the late imperial period, and as a result I come to a much more complex understanding of the military balance between China and the West (Andrade 2016). The idea that Europe had a consistent and growing lead over China is simply not true. We must look, however, beyond China, and in particular at South Asia. There is some wonderful scholarship being produced on this topic, and we can expect some intriguing and, likely, challenging findings (Roy 2005 and 2014; Gommans and Kolff 2001, Gommans 2002, Welsch 2010).

Another question that Hoffman's model raises is the importance of the size of political units. He believes that Europeans excelled in gunpowder weaponry partly because its states were small and distance was one of the most significant impediment to the spread of innovation. The Chinese case, however, casts doubt on this position. For one thing, one of the most rapid periods of military innovation in world history was during the Southern Song period (1227–1279), when behemoths battled each other – including the Southern Song, the Jin Dynasty, the Xi Xia Dynasty, and the Mongol State – resulting in very rapid military innovation. It was during that period that guns evolved. Similarly, even during times of relative unification, China's huge size didn't prevent it from innovating rapidly when it was facing military challenges. For instance, after the Ming fought against the Portuguese in 1521-1522, the Ming court very rapidly adopted Portuguese guns, producing thousands of them in Beijing and setting off a period of constant innovation, during which Portuguese gun designs were crossed with indigenous Chinese designs, Japanese designs, even Turkish designs. Chinese scholars have called this period the period of Sino-Western Hybrid Guncraft (Wang 2007: 159ff, Li 2012). Moreover, China's military production was also quite decentralized, with much production of guns occurring in the provinces. It's not clear to me that size is as significant as Hoffman suggests, but it is certainly a topic worth further study.

Conclusions

Both of these models – and especially Hoffman's – provide insights and stimulate further research, but both also suffer from the same problem, and it's in fact a deep structural problem that will increasingly affect historiographical developments in the twenty-first century: the challenge of keeping up with the explosion of research in non-Western languages. Most historians in the West do not appreciate the historiographical revolution occurring in the wider world. At the forefront of that revolution is China, a place where new journals, new research centers, indeed new universities, are founded each year. For decades in the second half of the twentieth century it was possible to keep up with research from the Sinophone world relatively easily. Journals and newsletters weren't always easy to acquire, but they were few – in fact, many ceased publication entirely during the 1960s and 1970s – and so one could keep abreast of the literature relatively easily. But the trickle of publications increased to a flow during the 1980s and by the end of the 1990s became a torrent. It is becoming a full time job to keep up with the latest research in one's subfield, and the sophistication, reach, and significance of that research continues to increase.

That new research does not merely press around the edges of current knowledge. It in many cases completely overturns it. This is certainly the case with military history. We now know that Chinese warfare was not merely focused on northern China, but that southern Chinese battlefields deeply influenced China's military culture, and this gives us a deeper understanding of the vast diversity of Chinese warfare. We now know far more about early Chinese guns, and we can say with relative certainty that through the end of the 1300s they were used more effectively on the battlefield than in Europe, and far more numerously. Moreover, we now know that the Chinese adoption of Western guns was more thorough, more rapid, and more innovative than was understood before. Chinese historians speak rightly of the nativization of western designs and describe how deeply those designs changed, adapting to the many various conditions of Chinese warfare (Feng 2012). They speak of a period of Sino-Western firearms fusion, during which traditional Chinese guncraft was influenced by western guncraft, to form a new synthesis (Wang 2007: 159ff). Sinophone historians have also shown very compellingly how in many ways Chinese guns were not just more suitable to Chinese conditions than Western guns, but more effective than Western guns, thanks to brilliant metallurgical techniques, which made possible bronze-iron hybrid guns that were marveled at by British observers of the nineteenth century (Huang 2011).

Morris and Hoffman both hope to avoid Eurocentrism, yet both of their models nonetheless propagate older Eurocentric narratives. Indeed, given the constantly increasing volume of data being generated among historians and social sciences, one might wonder whether it is possible to create any grand models that will truly be compelling enough to account for the disparate data. This is, however, too pessimistic, as the spectacular work of Victor Lieberman indicates (Lieberman 2007–2009).[7]

Indeed, the proliferation of new data will ultimately bring about a new generation of models, which promise to be more effective. Whereas previous grand models, from Montesquieu to Morris, have been built on shallow foundations, we are today building up a thick base of historical knowledge, much of which is in non-Western languages, a trend that seems likely to accelerate. New models will work better than old ones to the extent that they take into account the new data, especially data produced in China and other rising states. Otherwise naïve stereotypes will continue to proliferate: Chinese guns exploded; Chinese never used muskets in large numbers; Chinese were too focused on mounted nomads to invest in guns; Chinese gun production was too centralized and too expensive; etc.

It is a very exciting time to be a social scientist. Our understanding of human history is capable of being far more sophisticated and compelling than ever before in human history, but only insofar as we pay close attention to research from the rapidly-expanding scholarly communities of the non-Western world.

7. For a brief overview of Lieberman's arguments, see Andrade 2012.

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