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SPECIAL ISSUE INTRODUCTION

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Rethinking the Histories of War in Modern China

War creates intense experiences, and can be a powerful engine of social transformation. Yet it can also epitomize "sound and fury, signifying nothing": Either pure destruction that contributed to no large project, or a stage which merely displayed the inevitable outcome of a balance of forces already determined by social, economic, or political processes unfolding elsewhere.

Since China has a long tradition of regarding civilian affairs as more worthy of the scholar's attention than military ones, it is not entirely surprising that military history—of either old or new varieties—has been something of a step-child in Chinese studies. Moreover, because the 100-plus years of maximum military activity were also China's "Century of Humiliation"—an era of maximum weakness vis a vis foreigners, and of maximum domestic turmoil—they represented a period that, more or less by definition, had to be closed for Chinese history to resume its "normal course." That framing is most insistent in the People's Republic, but it has been influential elsewhere, too, and has reinforced the tendency of academic historians in the last few decades to wall off military history, handicapping efforts to see the full historical significance of war. While the "new military history" has done much to reverse that trend, its influence on the China field has thus far been limited.[1] Much good work has been done, but the larger picture is just beginning to come into focus.

These essays began as a panel on "Disrupting Histories of War and Peace in China, 1839–1951," at the 2017 Association for Asian Studies meetings, for which I served as discussant. While all the presenters have done original research about some aspect of war during this period, these papers are explicitly historiographic. Writing a synthetic introduction to four essays that are already syntheses may suggest the proverbial project of adding feet to the picture of a snake; but the papers have enough interesting areas of overlap upon which the authors themselves

[1] For a quick introductory summary, see Peter Paret, "The History of War and the New Military History," 209–26.

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did not have space to reflect that I have hopes of producing something more useful than those imaginary feet. Moreover, war has been so prominent a feature of 19th and 20th century Chinese history—while being the principal, explicit, focus of such a small portion of the academic histories of this period—that there are more intersections with other literatures than any one of us, or even all of us together, could point to. There should, consequently, be some value in juxtaposing these essays, and seeking some overall sense of where we stand in weaving histories of warfare back into the large history of modern China. One benefit of doing so is that it gives us another way of breaking through the 1949 barrier and making earlier history relevant to our understanding of the People's Republic: In this case, by noting that while China's "Century of Humiliation" and civil wars may have been over by 1949, and its engagement in high-stakes foreign wars by 1953, that does not mean that the influence of actual or contemplated war-making on modern Chinese history ended then. On the contrary, "militarization" of various sorts is an important strand in the social, political, and cultural history of the years since 1953 as well as the century before it.[2]

Xin Zhang invites us to think about many ways in which our understanding of the Opium War in particular has evolved in sync with larger currents in the field. This parallels what Stephen Halsey does with respect to the historiography of modern Chinese wars more generally. Both Zhang and Halsey describe what Halsey calls a *wai-nei-wai* (external-internal-external) movement, with the emphasis shifting to a China-centered, more *nei*, approach in the 1970s and then back to a more global approach later.[3] The result, both show us, has not been a simple oscillation, but a movement that has led to at least a partial synthesis.

Zhang points out that part of what happened with the movement from the early *wai* era to the *nei* period was a shift from the early historiography's nearly obsessive focus on why the Opium War started to more focus on what the war meant, and how it fit into a larger Chinese trajectory. Part of that obsession with origins, of course, came from wondering to what extent China actually had a *trajectory* to the

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[2] Micah Muscolino, *The Ecology of War in China: Henan Province, the Yellow River, and Beyond, 1938–1950*, 5–9, 56–58, 120–25, 237–46; Diana Lary, *The Chinese People at War: Human Suffering and Social Transformation*, 169–211; Covell Meyskens, "Maoist China's Hinterland War Machine: The Cold War, Industrial Modernity and Everyday Life in China's Third Front, 1964–1980." For some of my own thoughts on "militarization" and "technocracy" as organizing concepts of one kind of governance (water control) see Kenneth Pomeranz, "Water and Power in Twentieth Century North China," 157–72.

[3] Paul Cohen, *Discovering History in China: American Historical Writing on the Recent Chinese Past* both described the move from *wai* to *nei* and made an influential case that this represented historiographic progress.

Opium War at all, or was simply a stagnant entity waiting for a collision—though few people ever stated the case quite as simplistically as that. In this movement from origins to significance, Frederic Wakeman's *Strangers at the Gate* (1966) stands as one early landmark, shifting us towards thinking about domestic dynamics even while assigning foreign actions a crucial catalytic role. Likewise, Philip Kuhn's *Rebellion and its Enemies in Late Imperial China* (1970), though not about the Opium War, fit militia like those which Wakeman described into a longer-run pattern of regional and local militarization. Kuhn also pointed out—especially in the Cambridge History chapter he wrote with Susan Mann—that "dynastic decline" was in part a story about a dynamic society, marked by population growth, on-going commercialization, and other structural changes, outgrowing governing structures that did not keep up with social changes.[4] Thus, this work not only helped switch our focus towards internal, Chinese-driven dynamics, but pointed out that even internal dynamics that led to bad outcomes were not necessarily signs of inherently degenerative processes.

James Polachek's work—mostly written in the 1970s, though published much later—likewise serves as a key moment in Zhang's narrative, and also reminds us that intellectual and social movements in the real world are rarely purely progressive or reactionary. In some ways, Polachek combined *wai* and *nei* concerns, as one might expect of a work first conceived during the shift toward more internally focused work. His book certainly spoke to the old question of Opium War origins, but it did so by looking mostly within China. Moreover, it emphasized longer-term patterns of change—the rise of Han elites vs. Manchus; of statecraft studies and Hunanese vs. old Jiangnan literati, and so on—rather than either looking for answers exclusively in the immediate run-up to the war, or suggesting that given the basic nature of China and Britain, war was inevitable. Consequently, the Opium War becomes less a major "turning point" in his account than a point of relatively slight inflection—a speed bump in the rise of the literati networks and intellectual schools on which he focuses—or even a point which is most significant in that history did not turn more sharply.

Lest this last suggestion seem an exaggeration, it is worth remembering that Polachek framed his book partly by asking why the Qing did not manage a project of accommodating the foreigners while engaging in a crash program of top-down state-building, as Meiji elites did.[5] This kind of comparison and counterfactual, in

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[4] Frederic Wakeman, *Strangers at the Gate: Social Disorder in South China, 1839–1861*; Philip Kuhn, *Rebellion and its Enemies in Late Imperial China: Militarization and Social Structure, 1796–1864*; Philip Kuhn and Susan Mann Jones, "Dynastic Decline and the Roots of Rebellion," 107–62. Though Wakeman's book preceded Kuhn's, Kuhn's dissertation (1963) preceded Wakeman's (1965) so that influence is likely to have flowed in both directions.

[5] James Polachek, *The Inner Opium War*, 6–7.

which Japan represents success and China failure, is very unfashionable today, for understandable reasons, but it can be illuminating nonetheless; and it frames the counterfactual alternative to actual Chinese policy as a world in which a Qing Court unencumbered by resurgent literati might have undertaken very radical policy changes indeed. In the end, he give us a Hunanese movement that was anti-foreign, deeply conservative, and anti-centralization, and was set back only temporarily by the failure of its hard-line approach in 1839 to 1841. Polachek further suggests that we see the significance of that movement most clearly by contrasting it to Japanese forces that were also anti-foreign, but that were open to both radical reform and unprecedented centralization. Like Wakeman and Kuhn, then—albeit in different ways—Polachek straddled *wai* and *nei*, rather than firmly turning away from the former. There were, of course, scholars in the "China-centered history" movement who did turn completely away from *wai* concerns, but they did not write about the Opium War.

Xin Zhang then suggests that a third wave of interpretations of the Opium War—a second *wai* period, in Halsey's terms—came along with the so-called California School.[6] In this scholarship, China was again placed in a trans-national (or trans-imperial) frame, and questions about long-run trajectories again became critical; but unlike in the first *wai* phase, the frame was now less one of a binary East-West opposition (and still less a binary Sino-British opposition) and more one of a world of several intersecting and loosely comparable empires. Or, to look at things from a broader perspective, this scholarship aims to offer a global perspective in which Chinese agency matters more than it did in at least the more extreme versions of the impact/response model that was associated (perhaps not entirely fairly) with John King Fairbank, while acknowledging more than some China-centered scholarship did that Chinese often acted within a world they didn't make. These generalizations are, of course, not only true of the "California School" as it is usually described, but of other roughly contemporary work that is relevant here: Particularly James Hevia's work on the Sino-British clash of empires, which Zhang rightly emphasizes, but also much of the new Qing history.[7] Zhang does not cite this literature, as it has not produced work on the Opium War specifically, but we do learn something from including it here, as Halsey's paper notes. Since Zhang does not go in that direction, where he does turn is to more focused local and culturally oriented work by Julia Lovell, Robert Neil, and

[6] Definitions of who belongs to the "California School" vary considerably, and the group is hardly unanimous, but it is usually thought to include at least Jack Goldstone, James Lee, Robert Marks, Kenneth Pomeranz, Richard von Glahn, Wang Feng, and R. Bin Wong.

[7] James Hevia, *Cherishing Men from Afar: Qing Guest Ritual and the Macartney Embassy of 1793*; James Hevia, *English Lessons: The Pedagogy of Imperialism in Nineteenth-Century China*.

others.

Zhang thus ends with a call for historians to find and tell more personal stories of the war as experience: Something that resonates with Megan Greene's essay, as we will soon see. This call for more attention to the lives of ordinary people in wartime points to an area where we do indeed need more work. To reinforce that point, Zhang moves briefly outside the chronological frame of the rest of his essay, rightly pointing out how much good work has been done in recent years taking that approach to later conflicts, especially World War II. The question that follows from this is where to find the same sorts of testimonies for ordinary Chinese in the Opium War. Probably not in foreign archives, and probably not in the Ming-Qing archives in Beijing, either. These have been extensively searched in the earlier rounds of *wai-* and *nei-* oriented research, respectively, and were generally not oriented towards collecting this kind of material. In recent years, Zhang himself has found some very interesting material of this sort by digging hard in local libraries and archives in the Lower Yangzi region (though these materials were gathered for a project that goes well beyond the Opium War years, and many post-date this period). So have scholars working on the Taiping and its aftermath, also mostly in the Lower Yangzi.[8] Similar work in, say, the Pearl River Delta and the Beijing-Tianjin regions may or may not yield enough finds to give us a new history of the Opium Wars in particular. To paraphrase an authority from far outside our field, you can't always get what you want . . . or even what you need.

Though Zhang thus ends his article in a way that resonates with Greene's concerns, the bulk of his article aligns even more closely with Stephen Halsey's interests. Part of what those two papers, in combination, are showing us about the sociology of our field, I suspect, is how much generation does matter. In some sense, in fact, all the papers here are talking about how to combine *nei* and *wai*, but Zhang, having been trained in the era of "China-centered history," does put more emphasis on *nei*, while Halsey, having begun his career as the pendulum was swinging back, puts more emphasis on *wai*. Hayford, as we will see, notes that sometimes *nei* vs. *wai* is an artificial distinction.

Halsey also notes that the new *wai* phase differs from the old, among other things, in having a much broader understanding of the relevant scope of *wai*, which now includes Central Asia, Southeast Asia and (to a lesser extent) the Indian Ocean world, as well as the more familiar Euro-America and Japan. This is a point that is only marginally relevant to Xin Zhang's Opium War case study. Even for

[8] Tobie Meyer-Fong, *What Remains: Coming to Terms With Civil War in 19th Century China*; Stephen Platt, *Autumn in the Heavenly Kingdom: China, the West, and the Epic Story of the Taiping Civil War*; Christopher Heselton, "Reconstructing Order: Post-Nineteenth Century Chinese Civil War Society and Political-Economy."

that case, however, some of the late work of Joseph Fletcher—noting how dealings with Kokandis provided a model for the treaty ports, and how many officials were involved in dealing with both coastal and Inner Asia foreigners—opened the door for extending the relevant definition of *wai*.[9] Much more recently, Erik Schluessel's dissertation has returned the favor, showing how a specifically Hunanese vision of post-war reconstruction—promoted by the same literati networks that Polachek showed us were important to understanding the lead-in to the Opium War—influenced the reorganization of Xinjiang under Zuo Zongtang, and even beyond.[10]

In addition to noting this strong *wai-nei-wai* movement, Halsey's essay makes two other big points, both of which seem to me correct. First, he notes that we have used the results of war to evaluate the effectiveness of reform policies, and thus declared post-Taiping reform a failure based on China's defeat in the Sino-Japanese War of 1894–95. That war is, as Halsey notes, an event about which the existing literature is surprisingly thin, given how much importance we give it in most of our narratives of modern China. This is a mistake, he points out, because wars can be won or lost based on all sorts of contingencies that may have little to do with how well overall programs of reform are working, and also because some reforms may bear fruit over a time frame that is too long to affect the results of a particular conflict.

Second, Halsey notes that we have used military affairs to think about the internal stability of the late Qing state more generally. While that is not unreasonable, we have probably been too quick to see in those military affairs the seeds of the dynasty's ultimate fall and Republican warlordism. Doing so is not entirely unjustified, but it misses the extent to which these events were products of specific contingencies in the first two decades of the twentieth century. It also rests on taking too much on faith a sharp division between civil and military affairs which one does indeed see in many literati texts, but which does not seem to reflect a much messier reality. With respect to the first of these points, Halsey rightly notes that the more powerful state does not always win wars (a lesson the U. S. has had to learn repeatedly in the last half century), and that we need to be careful about what counts as "the state." He also notes that sometimes there are even bigger things at stake for a state (and a society) than winning or losing a war. In a nationalist narrative—in which resistance is always the proper response to imperialism—it seems like a big and obvious failure that not all Qing armies took

[9] Joseph Fletcher, "The Heyday of the Ch'ing Order in Mongolia, Sinkiang, and Tibet," 351–408.

[10] Eric Schluessel, "The Muslim Emperor of China: Everyday Politics in Colonial Xinjiang, 1877–1933." See also Stephen Platt, *Provincial Patriots: The Hunanese and Modern China*.

the field against Japan in 1894–95, or against the eight-country invasion of 1900; in fact, however, choosing not to participate in a probably futile battle could have been an intelligent way of protecting the state's interest in a nascent set of modern forces, rather thana betrayal of the country.

If we ever do get the richer history of the Sino-Japanese War that Halsey calls for, it will be very interesting to see how many people grasped this possibility at the time, as opposed to acting based on their more immediate interests and/or emotions. To this I would add that Japan makes an odd standard of comparison here, given its almost unique success in fending off 19th century imperialist intrusions: A bit like proclaiming your piano lessons a failure because you did not become Mozart. Or as Halsey puts it, echoing a recent essay by R. Bin Wong, state-making in both Japan and China looks relatively successful compared to the vast lands that became colonies, and to the empires (such as the Ottoman) that fractured beyond repair.[11]

In fact, as Halsey reminds us, there was quite a bit of successful state-building in post-1850 China: In fiscal affairs, arsenal construction, training of new-style troops, communications, and so on. Unfortunately for the Qing, however, it occurred against a backdrop of extraordinary challenges—a true perfect storm of everything from ethnic conflict to mass drug addiction, incursions by technologically superior foreign armies, and acute environmental crises—and also from a baseline situation in which the government's share of GDP (perhaps as low as 2-3%) was exceptionally small even compared to other "traditional" loose-reign empires like the Ottomans.[12] Moreover, we need to distinguish between failure from the Qing dynastic perspective—which was ultimately a matter of the state apparatus not remaining in Manchu hands—and calling the development of that apparatus itself a failure; the latter is a far less obvious proposition. There is also—especially as one moves into the Republican era—a distinction to be made between the relatively successful development along modern lines of various line agencies, chronicled by William Kirby, Julia Strauss, Morris Bian, and many others;[13] the at least partial

[11] R. Bin Wong, "Self-Strengthening and Other Political Responses to the Expansion of European Economic and Political Power," 366–94.

[12] On Ottoman revenues, see Sevket Pamuk, "The Evolution of Fiscal Institutions in the Ottoman Empire," 325.

[13] William Kirby, "Engineering China: The Origins of the Chinese Developmental State," 137–60; Julia Strauss, *Strong Institutions in Weak Polities: State Building in Republican China, 1927–1940*; Hans van de Ven, *Breaking With the Past: The Maritime Customs Service and the Global Origins of Modernity in China*; Morris Bian, *The Making of the State Enterprise System in Modern China*; Felix Boecking, *No Great Wall: Trade, Tariffs and Nationalism in Republican China, 1927–1945*.

success of municipal reforms (David Strand, Steven McKinnon, Frederic Wakeman, Ruth Rogaski, Kristin Stapleton, Di Wang, and many others);[14] and the story of field administration in the countryside (and for the most part, at the provincial level), where we have thus far found less reason to revise the old failure narrative, and where officials who held too strongly modernist a vision of what state-building should entail might be part of the reason why success proved elusive.[15]

As Halsey notes, for those who want to place late Qing military reform primarily into a story of failure, Exhibit A has been the alleged connection between the regional armies that arose to fight the mid-19th century rebellions and the slide into warlordism after 1916. If such a connection held up, it would, for instance, make a mockery of Halsey's suggestion that the commanders who held back from fighting foreigners in 1894–95 and 1899–1900 might have been acting in the interests of long-run state-building. Yet that connection has always been shaky, in part because the New Armies created at the end of the Qing were quite different from those regional armies.[16] One can also imagine rather different historical outcomes than full-fledged warlordism by altering some fairly specific matters of Qing policy during their last decade in power—particularly on railways and constitutional reform—or perhaps some different choices during the rise of the Boxers. But rather than speculate about such specific contingencies, let us return to

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[14] E.g., David Strand, *Rickshaw Beijing: City People and Politics in the 1920s*; Kristin Stapleton, *Civilizing Chengdu: Chinese Urban Reform, 1895–1937*; Ruth Rogaski, *Hygienic Modernity: Meanings of Health and Disease in Treaty-Port China*; Stephen MacKinnon, *Power and Politics in Late Imperial China: Yuan Shi-kai in Beijing and Tianjin, 1901–1908*; Di Wang, *The Teahouse: Small Business, Everyday Culture, and Public Politics in Chengdu, 1900–1950*; Frederic Wakeman, *Policing Shanghai, 1927–1937*.

[15] For four different, but potentially complementary versions of that skepticism about "modern" state-building and the countryside, see Prasenjit Duara, *Culture, Power, and the State: Rural North China, 1900–1942*; Kenneth Pomeranz, *The Making of a Hinterland: State, Society, and Economy in Inland North China, 1853–1937*; Ralph Thaxton, *Salt of the Earth: The Political Origins of Peasant Protest and Communist Revolution in China*; Wang Chen-cheng, "From Administrative Science to Chinese Statecraft: The Local Governance of the Central Politics School and its Political Modernity, 1927–1945." For a study of ultimately unsuccessful efforts to make the countryside "modern" while keeping some distance from national state-building efforts, see Kate Merkel-Hess, *The Rural Modern: Reconstructing the Self and State in Republican China*.

[16] Stephen MacKinnon, *Power and Politics in Late Imperial China: Yuan Shi-kai in Beijing and Tianjin, 1901–1908*; Ernest Young, *The Presidency of Yuan Shih-k'ai: Liberalism and Dictatorship in Early Republican China*.

the broader story of the military and state-building from the mid-19th through the early 20th century.

The regional armies of the middle and late 19th century were led by men who remained loyal to the Qing, and who took those armies (or portions of them) with then as they were rotated through posts all over the realm, from Taiwan to Xinjiang. Over time, as Halsey notes, at least some of those armies also began to recruit from a broader geographical base, and so became more like national/imperial armies. So, yes, there were trends towards regionalism, but it wasn't inevitable that they would prevail over various counter-trends.

While we know this story with respect to Zeng Guofan, Li Hongzhang and Zuo Zongtang, I think the point may emerge even more clearly from a lesser-known figure: Cen Yuying, whose troops were central to the suppression of the Panthay Rebellion in Yunnan, and who remained a dominant figure in Yunnan politics (rising to governor general) until his death in 1889. At first, Cen's very long period of political ascendancy in a remote region may seem like a particularly striking case of devolution and regionalism, particularly since Cen lacked a higher level exam degree, his younger brother also rose to be Yun-Gui governor general, and a lot of Cens (not a very common surname) turn up on lists of lower officials in the late 19th century southwest. None of that evidence should be dismissed. But consider also that Cen went to Taiwan when he was temporarily reassigned there, taking a bunch of his Guangxi Army troops with him; his aforementioned younger brother ultimately wound up spending most of his career in Guangdong; and four of his sons served as high officials in North China, far from their father's power base. [17]

So while the Cens certainly parlayed a capacity for local/regional mobilization (they were descended from a group of native chieftains in Guangxi and drew heavily on the region for troops), their upward mobility also put them in positions that gave the central bureaucracy leverage over the family, and thus over its base in the southwest.[18] Add further that one of Cen's major achievements while in

[17] On troop transfers, and the transfer of some members of Cen's personal staff into civil positions, see Cen Yuying, *Cen Xiangqin zougao*, 1777–80, 1783, 1865–69, 1887–88, 1897, 1903–5.

[18] On Cen's career and those of his close relatives, see Arthur Hummel et al., eds., *Eminent Chinese of the Ch'ing Period*, 742–46, which gives the story from the family genealogy. Jodi L. Weinstein, *Empire and Identity in Guizhou: Local Resistance to Qing Expansion*, 51–53, sketches the family's fortunes up to the 18th century, taking a more skeptical view derived from recent scholarship by Jeffrey Barlow, Shi Jizhong, and others. For the information on Cen Yubao (Cen Yuying's younger brother) specifically, see Qian Shifu, ed., *Qingdai zhiguan nianbiao*, vol. 2, 1501–2. The younger Cen also served as governor general of Guangdong and Guangxi, and acquired a Cantonese name by which he sometimes appears: Shum Yuk-po. For memorials showing that Cen Yubao served directly under his brother for a while, see for instance Cen Yuying, *Cen Xiangqin gong zougao*, 2445, 2631, 2671, 2843.

Yunnan was to stabilize the border with French-ruled Vietnam, and establish new garrisons near the new commercial towns that sprung up in the previously chieftain-ruled border region, and his career begins to look like a fairly successful partnership between the center and regional elites, not a zero-sum contest between them—just as Halsey suggests. Moreover, despite all the problems that post-1912 central governments had in holding on to the republic's peripheries—especially those with large non-Han populations—the area near the Vietnamese border where Cen established the provincial government's presence was relatively smoothly, if only gradually, incorporated into the Republic of China.[19] Finally, as previously noted, the reformed armies of the last Qing decade were in many ways more of an imperial (or, if you will, national, at least as opposed to regional) set of forces than those of Li Hongzhang, Zuo Zongtang, or Cen Yuying had been: More tied to the court by funding arrangements, broader and less personalistic in their recruitment patterns, and so on.[20] That they evolved in a very different direction after 1916 is indubitable, but it is not at all clear that this stemmed from the way the military had developed over the last 60 years of the Qing; the politics of the 1910s, including such truly exogenous events as World War I (which cut off non-Japanese sources of loans at a critical moment in Yuan Shikai's state-building project), may well be bigger factors in the Republic's decline into warlordism.[21]

The other point that Halsey takes up here is the gradual militarization of society under the Qing: A phenomenon that Kuhn et al. had already pushed back to the beginning of the 19th century, and that Joanna Waley-Cohen, looking at martial culture, had pushed further back into the "peaceful" 18th century (when there was plenty of fighting, but it occurred, for the most part, far from China's major population centers). Halsey asks for more attention to military symbolism: Something that we already see a bit of in Charles Wooldridge's dissertation of a few years ago, and in the more recent thesis of Christopher Heselton, both of which focus on the mid-19th century.[22] He also asks us to question whether the

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[19] Ling Yongzhong, *Minguo shiqi Yunnan bianjiang diqu teshu guodu xingzhengqu yanjiu*. On Cen's involvement in projecting government power into this area in the aftermath of the Sino-French war, see Cen, *Cen Xiangqin*, 2961–69, 2971–73, 2975–76, 3003–8, 23203–4, and Hummel et al., *Eminent Chinese*, 744–45.

[20] MacKinnon, *Power and Politics in Late Imperial China: Yuan Shi-kai in Beijing and Tianjin, 1901–1908*.

[21] Young, *The Presidency of Yuan Shih-k'ai*, 165, 179–82, 299 n. 3, points out that Yuan wanted to reduce foreign borrowing anyway—but the decline in funds reaching Beijing from overseas after war broke out is nonetheless striking, as is the limited number of options for future borrowing.

[22] William Charles Wooldridge, "Transformations of Ritual and State in Nineteenth-Century Nanjing"; Heselton, "Reconstructing Order."

supremacy of *wen* over *wu* was ever quite as strong as literati self-presentation would suggest. This, too, seems to me to be a question that points us in a useful direction: Certainly David Robinson's on-going work on the Ming suggests that we have over-emphasized civilian supremacy in that dynasty, and Charles Hartmann's Cambridge History chapter suggests that the fabled triumph of a civilian ethic in the Song was much stronger and came much sooner among the literati who wrote the (retrospective) Song History than it did in actual Song politics.[23] The Song did, after all, use about 80% of government spending for war, versus about 50% in the high Qing.[24] So there is a lot to chew on here: Between these two papers we begin to get a very different set of stories about war and the military in the 19th century, with potentially huge implications for how we understand the post-Qing world.

This brings us to the 20th century, and to the papers of Megan Greene and Charles Hayford. For Greene, the axis to think about is not so much a spatial *nei-wai* axis as it is a timeline: She raises important questions about how to divide that timeline into meaningful segments. She asks us to think about what is at stake in treating wars as "dividing lines" between periods, as opposed to moments in longer continuing processes—an issue that also arises, more incidentally, in the other papers. Going in the other direction, she also asks us to compare the strategy of focusing on moments within wars that may be important to particular people as experiences versus thinking of a war as a whole as a producer of material changes, and about the opportunities that histories of short "moments" within wars offer for getting away from an excessive focus on winners and losers, heroes and villains.

The Second Sino-Japanese War/World War II is a particularly rich field for exploring these questions. It gives us, probably for the first time in Chinese history, large numbers of individual accounts from participants at all levels, from Chiang Kai-shek's letters and diaries down to those of ordinary people touched either directly or indirectly by the war. It is also an interesting case because it blurs the line between foreign war and civil war(s), and because it involved such an enormous amount of human movement, both on and away from battlefields: The epic treks to Chongqing and Yan'an, the Henan famine victims who resettled in Shaanxi, and so on, which also serve to blur the line between soldiers and civilians. The Nationalist government estimated that at the end of the war, 42 million

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[23] See, for instance, David Robinson, *Bandits, Eunuchs, and the Son of Heaven: Rebellion and the Economy of Violence in Mid-Ming China*; Robinson, *Martial Spectacles of the Ming Court*.

[24] Charles Hartman, "Sung Government and Politics," 29, for the spending figure, 85–86, on the emperor's continuing involvement in military affairs.

Chinese were away from their homes; over 95 million had relocated at some point, often more than once.[25] Greene calls attention to these movements, and to many similar ones—e.g., movements between various cities and their hinterlands as the relative safety of urban and rural settings shifted—about which we so far know very little.

At the same time, the 1937–45 struggle may have been the first Chinese war to have a "home front" (or multiple home fronts) in the sense that we are used to: Large areas that saw little or no fighting but were deeply engaged in the war as sites of resource mobilization, propaganda efforts, constant exposure to war news, and so on. Armies in, say, the Qing wars of expansion or the Taiping had, of course, had rear areas, but it is hard to think of any that were as continuously and deeply engaged with the war as was the case with mid-20th century rear areas.[26] And in the first Sino-Japanese War and warlord struggles, whatever possibilities might have existed for that kind of intense mobilization far from the front were not realized, although, as Hans van de Ven points out, doing so became an ambition once China's two Leninist parties came on the scene.[27] The development of homefronts during the 1937–45 struggle, and of a library of techniques for managing them, are themselves important phenomena, as homefronts became crucial thereafter—in the 1945–49 civil war, the Korean War, and throughout the Cold War. Examples of Cold War episodes in which the concept of "homefront" seems apropos would include the building of the Third Front, the phenomenon of martial law in Taiwan, and the seemingly endless use of soldiers from the Vietnamese front for political purposes between 1979 and the mid-1980s, even

[25] Statistics cited in Diana Lary, *The Chinese People at War: Human Suffering and Social Transformation, 1937–1945*, 170, 175–76.

[26] On how the Qing conquests in Inner Asia relied on resources gathered in "China proper," and how those wars were presented there, see especially Peter Perdue, *China Marches West: The Qing Conquest of Central Eurasia, 1600–1800*, and Dai Yingcong, *The Sichuan Frontier and Tibet: Imperial Strategy in the Early Qing*. Dai, however, concentrates on resource flows and their implications, not on attempts (if there were any) to stir popular support for the wars. Perdue does address ideological as well as material efforts. He finds, however, much more material about how these conquests were presented after the fact than during the wars themselves—indicative, I would argue, that these rear areas were not a homefront in the modern sense, where people need to be made enthusiastic about the conflict they are supporting.

[27] Hans van de Ven, "New States of War: Communist and Nationalist Warfare and State Building (1928–1934)," 297–321; see especially, 322–23.

though that was actually a pretty small and largely unsuccessful incursion.[28]

Greene calls our attention to two distinct and important parts of the "homefront" experience, neither one of which has been extensively studied. On the one hand, we have development efforts in World War II homefront areas. Admittedly, many of these were not completed during the war years, or in some cases ever. The gap between stated ambitions and actual results—or even investment—was particularly large in the far northwest.[29] However, even those unfinished projects often had important implications for post-1949 planning. In some cases, they also had considerable influence on the places where they were supposed to happen, even if they didn't get built until later, and some projects, of course, were completed as planned.[30] In many cases, though certainly not all, these projects involved a significant influx of outsiders, whether they were technical experts or laborers. We have by now a fairly substantial literature on the more elite outsiders, including work by John Israel, Pingchao Zhu, and others.[31] On the other hand, the experiences of people who were already in these areas when the newcomers arrived have been much less studied, as Greene rightly points out. We have a little bit of research about this in the case of southwestern and (to a lesser extent) northwestern minorities facing an influx of both bureaucrats and social scientists during the war years, but relatively little for the local Han population.[32] Nicole Barnes' work in progress on public health in wartime

[28] On mobilizing rear areas during the Civil War, see Suzanne Pepper, *Civil War in China: The Political Struggle, 1945–1949*; on Korea, see Gary Rawnsley, " 'The Great Movement to Resist America and Assist Korea': How Beijing sold the Korean War," 285–315, and Lawrence Weiss, "Storm Around the Cradle: The Korean War and the Early Years of the People's Republic of China, 1949–1953," 96–101, 110–51; on the Third Front, Meyskens, "Maoist China's Hinterland War Machine"; for a very brief summary of martial law in Taiwan, see Denny Roy, *Taiwan: A Political History*, 88–95.

[29] Jeremy Tai, "The Northwest Question: Capitalism in the Sands of Nationalist China," 1–19.

[30] For instance, Judd C. Kinzley, "Crisis and the Development of China's Southwestern Periphery: The Transformation of Panzhihua, 1936–1969," 559–84.

[31] John Israel, *Lianda: A Chinese University in War and Revolution*; Pingchao Zhu, *Wartime Culture in Guilin, 1938–1944: A City at War*, 67–166.

[32] On non-Han populations affected by Chinese war mobilization in southwestern and northwestern frontier zones see Edward A. McCord, "Ethnic Revolt, State-Building and Patriotism in Republican China: The 1937 West Hunan Miao Abolish-Military-Land Resist-Japan Uprising"; Hsiao-ting Lin, "Nationalists, Muslim Warlords, and the 'Great Northwestern Development,' in Pre-Communist China," 115–35. Justin Jacobs, *Xinjiang and the Modern Chinese State*, 118–47, tells the story of mobilization for WWII in Xinjiang and ethnic politics, but suggests that the key outsiders were at least as often from the USSR as from China.

Chongqing and environs will plug one part of that gap, while already existing work by Joshua Howard, Morris Bian, Matthew Combs, and others tells us a lot about how people experienced nationalist production drives, price controls, and so on; but there is still a great deal more to do on the lives of people who were already in the southwest when the war began.[33] There is, of course, a larger literature on how the lives of people in the northwest—or, more precisely, those parts of the northwest that became CCP base areas—changed during the war years, but with few exceptions that literature tells their story through the eyes of the CCP activists who organized them rather than their own perspective.[34]

As Greene notes, recent years have also given us a number of works on development projects related to the war effort, and tied them to long-term studies of regional development: Especially of increased integration into what became the P.R.C. Examples include Judd Kinzley on Xinjiang and, separately, on Panzhihua; Andres Rodriguez on Tibet; Joseph Lawson on western Sichuan; Morris Bian on Guizhou; William Haas on Qinghai; and Micah Muscolino on the Huanglongshan area of Shaanxi.[35] Here, for the most part, the literature has looked to the long term to find significance—particularly for projects that were only planned or

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[33] Joshua Howard, *Workers at War: Labor in China's Arsenals, 1937–1953*; Morris Bian, *The Making of the State Enterprise System in Modern China*; Matthew Combs, "Chongqing 1943: People's Livelihood, Price Controls, and State Legitimacy," 282–322; Nicole Barnes, *Intimate Communities: Wartime Healthcare and the Birth of Modern China, 1937–1945*.

[34] Here the literature is vast, even if we restrict ourselves to the Northwest. See, for instance Mark Selden, *China in Revolution: The Yenan Way Reconsidered*; Selden, *The Yenan Way in Revolutionary China*; Pauline Keating, *Two Revolutions: Village Reconstruction and the Cooperative Movement in Northern Shaanxi, 1934–1945*; Joseph Esherick, "Deconstructing the Construction of the Party-State: Gulin County in the Shaan-Gan-Ning Border Region," 1052–79.

[35] Micah Muscolino, *The Ecology of War in China: Henan Province, the Yellow River and Beyond, 1938–1950*, 81–86, 163–70; Andres Rodriguez, "Building the Nation, Serving the Frontier: Mobilizing and Reconstructing China's Borderlands During the War of Resistance (1937–1945)," 345–76; Joseph Lawson, "Unsettled Lands: Labour and Land Cultivation in Western China During the War of Resistance (1937–1945)," 1442–84; Hsiao-ting Lin, "Nationalists, Muslim Warlords, and the 'Great Northwestern Development' in Pre-Communist China," 115–35; William Haas, "Qinghai Across Frontiers: State- and Nation-Building under the Ma Family, 1911–1949"; Judd C. Kinzley, "Crisis and the Development of China's Southwestern Periphery: The Transformation of Panzhihua, 1936–1969," 559–84; Kinzley, *Natural Resources and the New Frontier: Constructing Modern China's Borderlands* I; Morris Bian, "Redefining the Chinese Revolution: The Transformation and Evolution of Guizhou's Regional State Enterprises, 1937–1957," 313–50.

begun during the war years—following the lead of William Kirby's work in looking for continuities across the 1949 divide.[36] An interesting counter-current surfaces in Wang Chen-cheng's recent dissertation, which shows some Guomindang local administrators attempting to implement a highly rationalized form of administration in wartime counties in Guizhou and the mountains of western Zhejiang, but ultimately abandoning this approach and finding greater success by building on various local practices.[37] The contrast may have much to tell us about the above-noted difference between successful state-building in line agencies and municipal government, on the one hand, and a still-prevalent story of failure in rural field administration, on the other. Meanwhile, that even those officials whom Wang finds discarding more "modern" approaches often told their superiors that they were persisting with them may suggest a reason for caution about some stories in which wartime exigencies provided an opening wedge for post-1949 high modernism.

Both Greene and Halsey also note the potential for more environmental histories of Chinese warfare, citing in particular Micah Muscolino's fascinating environmental history of World War II in Henan. Greene adds several studies suggesting that "the Sino-Japanese war fundamentally changed the ways that the Chinese government and people engaged with the expansive territories of the northwest and southwest."[38] Pauline Keating's work on the CCP base area in Yan'an might be added here, as well as Elizabeth Perry's work on rebellion and revolution in Huaibei, though both focus on environment as a factor explaining different patterns of mobilization rather than, like Muscolino and the others cited here, also considering the impact of war-making on the environment.[39] Halsey suggests that a study of war and the environment in modern China might begin with the Taiping: Certainly that war deserves an environmental history, given the devastation that it wreaked upon water control infrastructure, forests, farms, and both animals and humans.

I would be inclined, though to begin a bit earlier, with the White Lotus Rebellion of 1796–1804. As is well known, most of this war consisted of a struggle for control of western highlands that had become densely settled over the previous

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[36] William Kirby, "Engineering China."

[37] Wang, "From Administrative Science to Chinese Statecraft: The Local Governance of the Central Politics School and its Political Modernity, 1927–1945."

[38] Muscolino, *Ecology of War*.

[39] Keating, *Two Revolutions*; Elizabeth Perry, *Rebels and Revolutionaries in North China, 1845–1945*.

century, but without the Qing installing adequate civil government.[40] Like most wars of attrition, the campaigns often focused on denying food supplies to enemies – with Gong Jinghan's strategy of "strengthening the walls [of villages] and clearing the countryside" prefiguring, among other things, the U.S. "strategic hamlets" program in Vietnam.[41] Arson was used by both sides, much as it was would be in some of the Taiping campaigns.

Equally important for the environment, however, was the aftermath of the fighting. Qing statesmen and statecraft thinkers, such as Yan Ruyi, came to the conclusion that the highlands could not be depopulated—though it had been the policy of the Ming and early Qing that people should not live there, since much of this territory would not support farming, and they worried about it serving instead as a haven for bandits, salt smugglers, and other criminals. The new policy asked instead how the mountains could be filled with "good subjects" (*liangmin*) who would, among other things, join *baojia* organizations and militia, keeping order where the over-extended state could not. The answer that people like Yan provided included accepting the area's non-farming settlers—miners, loggers, paper-makers, charcoal-makers, mushroom gatherers, and other users of the forest —as potential *liangmin.* Yan even speculated that such people might be better candidates for leading stable lives in mountainous areas than farmers were, given the likelihood of farmland eroding rapidly and the difficulty of concentrating farmers in villages where per acre yields were bound to be low.[42] While long-standing government suspicion towards such people—especially the large numbers of them who remained single men—did not change overnight, we can see in the wartime militia that incorporated such men, and in these post-war reconstruction efforts, some crucial beginnings of precisely those changes "in the ways that Chinese governments and people engaged with the expansive territories of the northwest and southwest" that Greene sees coming to fruition during and after World War II.

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[40] For overviews, see Wensheng Wang, *White Lotus Rebels and South China Pirates: Crisis and Reform in the Qing Empire*, 37–80; Cecily McCaffrey, "Living Through Rebellion: A Local History of the White Lotus Uprising in Hubei, China"; Kuhn, *Rebellion and its Enemies*, 36–50.

[41] Kuhn, *Rebellion and Its Enemies*, 42–43. See also Daniel McMahon, "Fortified Walls and Social Ordering in China's Late Eighteenth-Century Revolts," 244–56.

[42] Yan Ruyi, *San sheng shengnei fengtu zazhi*, 17:54a–b, 17:64b–66b *San sheng bianfang beilan* 17 (xia): 18b–26b, 49b–50a, 58b. Yan, *Leyuan wenchao*, 6:3a–4a, 7:7a–8a. See also Daniel McMahon, "Restoring the Garden: Yan Ruyi and the Civilizing of China's Internal Frontiers, 1795–1805," 256, and McMahon, "Qing Reconstruction in the Southern Shaanxi Highlands: State Perceptions and Plans, 1799–1820," 85–118.

Greene further points out that the mobilization and development projects associated with World War II also offer an opportunity to think about the history of technology in 20th century China. Thus far we have little work that considers that connection—a situation that is particularly surprising since arsenals have been so central to the history of technology in 19th century China.[43] That linkage was perhaps seen as particularly strong during the first period of *wai* historiography, when Western scholars generally lacked access to the necessary sources for exploring other sources of 19th century technological change, but the contrast is nonetheless striking. This does seem like a promising area for more work—both along the lines suggested by Greene and along a few others.

War is often an impetus for technological change, and not only with respect to battlefield technology. There are, for instance, technologies that feed the desire for news in times of intense interest: Mid-20th century examples would include radio and high-speed printing presses, the adoption of which was greatly accelerated in Japan by the hunger for war news.[44] There are also technological innovations that occur in civilian factories cut off from spare parts: These dynamics might have been significant for the post-1949 growth of the machine tool industry, as they were in the origins of China's modern capital goods industry during and after World War I.[45] There were also adaptations to other shortages that may have had important long-run effects on technological choices: For instance Nicole Barnes' above-mentioned study of public health in wartime Chongqing suggests some ideas, which she plans to develop further in a future project, about how shortages of western bio-medicines affected the rapprochement between supporters of "modern Western" and "traditional Chinese" medical systems during the war, with lasting consequences for both.[46]

Indeed, in thinking about how wartime exigencies opened space for modernist technocrats to plant seeds that would grow into huge trees after 1949, we should not forget, as Greene's tong oil example suggests, that the resource shortages and

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[43] Adrian Bennett, *John Fryer: The Introduction of Western Science and Technology in Nineteenth-Century China*; Steven Leibo, *Transferring Technology to China: Prosper Giquel and the Self-Strengthening Movement*; Meng Yue, *Shanghai and the Edges of Empires*; Benjamin Elman, *A Cultural History of Modern Science in China*, 158–97; Shellen Xiao Wu, *Empires of Coal: Fueling China's Entry into the Modern World, 1860–1920*, 51–68; Hsien-chun Wang, "Discovering Steam Power in China, 1840s–1860s," 31–54.

[44] Louise Young, *Japan's Total Empire: Manchuria and the Culture of Wartime Imperialism*, 55–95.

[45] Thomas Rawski, *China's Transition to Industrialism: Producer Goods and Economic Development in the Twentieth Century*.

[46] Barnes, *Intimate Communities*.

short time horizons characteristic of struggles for survival also begat technical improvisations that probably would not have happened in peacetime. Some of those expedients disappeared after the war, creating a double discontinuity for those who were involved in them, while others, as with Barnes' example, may have lastingly inflected various arcs of technological history. The implication of Greene's argument, which I would heartily endorse, is that we need to study both. I would further hazard as a hypothesis—though one thus far based on just a few examples—that some of the most interesting technological changes may not have involved cutting edge technology. Many technological and developmental efforts involved important compromises. For instance, technocrats made grand plans for petroleum and mining development in the northwest, while much of what could actually get done in the wartime northwest was old-fashioned, labor-intensive, land reclamation. And as Michael Muscolino has recently shown for Huanglongshan, and as we have long known about the Shaan-Gan-Ning base area.[47] There were also spurts of low-tech non-state economic activity that responded to the war as an emergency requiring new ways of seeking a livelihood, provisioning local security forces, or both, rather than as an occasion for development. Ralph Thaxton's account of rural Henan salt-makers branching out into the production of ammunition is one such example, though not cast in terms of technological history.[48] Greene's own work in progress on tong oil sounds like it will fit neatly into neither a "seeds of development" nor a "successful temporary improvisation" story, and thus may help us re-think those categories.

Even when it comes to strictly military technologies, recent scholarship suggests that the technologies that mattered most may not have been the newest or most "advanced" ones. Kwong Chi Man's new book on Chinese military thinking and planning before World War II makes clear that Chinese officers were very much aware of the potential of the newest technologies, such as planes and tanks, and Chinese forces acquired as many of them as they could within their budget constraints. However, the usefulness of these new tools was severely undermined by the tendency of officers to plan (and train their subordinates) for using them in isolation, emphasizing radically new and implausible missions such as destroying Japan's industrial base through bombing, while neglecting preparation for using technology in combination with regular infantry forces. It took the painful experience of battlefield losses and a long war of attrition to make these new technologies effective parts of the Chinese military, even though various voices, both inside and outside China, had advocated a more hybridized approach well

[47] Muscolino, *Ecology of War*, 163–69; Peter Schran, *Guerrilla Economy: The Development of the Shensi-Kansu-Ninghsia Border Region, 1937–1945*, 118–67.

[48] Thaxton, *Salt of the Earth*, 238–67.

beforehand.[49] There is an interesting parallel here to Wang Chen-cheng's finding that the practicalities of wartime mobilization also turned some Guomindang field administrators away from their excessive faith in high modernist techniques. In short, while we sometimes think of wars as promoting forced-draft modernization, and many leaders clearly bet heavily on this approach, what emerged across many spheres during China's World War II experience was a creation of more hybrid systems, which drew heavily on various kinds of Chinese traditional knowledge.

In some sense, that brings us full circle. The old story of China from roughly 1839–1950, as all these papers remind us in one way or another, was one about how a resistant "traditional" China was *forced* to "modernize"—and since war is a great engine of necessity, it fit comfortably into the many variants of that narrative arc. We are now getting a different story—one much less certain, at both its beginning and ending points, and with choices, rather than necessity, mattering more and more in between. But wars are still at the heart of it.

Charles Hayford's paper bridges Zhang and Halsey's focus on the use and abuse of spatial borders—the *wai/nei/wai* shifts in focus—and Megan Greene's focus on temporal dividers, with a greater emphasis on the latter. That all the conflicts he lists were both domestic and international struggles is true, but no longer terribly surprising; his idea of considering all of these struggles as part of "a single wave" is much more radically experimental. In fact, it is a thought experiment that is in one sense diametrically opposed to Greene's suggestion to focus on shorter periods of time within wars; but it also complements her approach in getting us away from assuming that the period from the first to the last shot of a conflict (if we can agree on when they occurred) constitutes a natural and preferred unit of analysis.

Furthermore, in its concern for what we name the different wars within this wave, Hayford's paper is the most sharply focused on the historiography of war *per se*, as opposed to using war as a lens through which to view state-building or other processes. And it is probably not coincidental that his is also the paper that takes the most account of the vast amounts of military history that are created and consumed outside the academy. After all, whether we call the Taiping a rebellion, a civil war, or anything else matters little to our understanding of how suppressing it transformed the empire's fiscal system; nor, for instance, would renaming the Boxer uprising change Ruth Rogaski's assessment of how the resulting Allied occupation of Tianjin affected the history of Chinese medicine and public health.[50] Nor does what label we retrospectively apply to the "Opium War" necessarily matter to how we understand the way foot-soldiers, smugglers, or villagers

[49] Kwong Chi Man, *Minguohu? Junguohu? Di er ci Zhong-Ri zhanzheng qian de minguo zhishi junren, junxu, yu junshi biange*.

[50] Ruth Rogaski, *Hygienic Modernity: Meanings of Health and Disease in Treaty-Port China*.

experienced those years. But nomenclature matters a great deal when we try to sum up the overall significance of any one of those conflicts as a discrete event, to answer "What was the _____ War?"—or as Hayford's title puts it "What's the Story?" And those are, among other things, questions that are prominent when school curricula are written, anniversaries celebrated (or not), and people—especially those who are not academic historians—ask "Could X have won instead of Y? Would it have mattered?" In this sense, despite Hayford's invocation of "the new military history"—something the other participants also engage with, but do not name—his article is the one that works hardest to reinvigorate the concerns of an older military history, with wars as objects of study in themselves.[51] Consequently, he focuses less than others on the New Military History's concerns with how "war-making has remade states, shaped national identities, transformed organizational and managerial practices, and propelled important social, demographic, economic, and cultural developments."[52] His "wave" metaphor prioritizes linking wars (as individual "particles") to other wars to understand the phenomenon of a century of war, rather than focusing on what a given war meant to economic development, popular culture, or state formation.

As Hayford points out, names and language are particularly important when the goal of historiography is to praise and blame, or to reinforce national belonging. At the same time, words once used for some now irrelevant reason may have become so accepted over time that their incongruities can be ignored. Thus, as Hayford notes, insisting that the areas conquered by the Qing are integral parts of "China" does not necessarily entail referring to the campaigns involved as "civil wars"; but for many Chinese it does go with calling them campaigns of "unification," rather than "expansion," and calling the Dzungars "rebels." Conversely, however, Westerners who avoid that language are usually not questioning the boundaries of today's P.R.C., even if they are occasionally accused of doing so.[53] In other cases, a shared label can serve multiple purposes—both at one time and across time. Thus, Hayford notes, the label "Opium War" fits from the perspective of China, and that of Western critics of British aggression (who coined the term), since it highlights the morally dubious commodity at the heart of British interests; but it may have proved so durable, despite the number of Westerners who claimed the war was not about the drug, because it could fit their agenda, too—by summoning an image of

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[51] Peter Paret, "The History of War and the New Military History," 209–26.

[52] Hayford, citing Hans van de Ven, "War in the Making of Modern China," 737, 756.

[53] These issues led to a blistering polemic against the New Qing History by Li Zhiting of the Chinese Academy of Social Sciences. A link to that article, along with some responses from Western scholars, can be found at https://networks.h-net.org/node/22055/discussions/68052/chinese-social-science-web-article-new-qing-history.

an inert, deluded, China that needed to be roused from its "pipe dream." [54] And as Hayford notes, battles have also been fought over what to call the Taiping (Rebellion? Civil War? Revolutionary Movement?), Boxer (Rebellion? Movement? Uprising?), and 1911 (Revolution?) and so on—with, in each case, real political stakes.

When it comes to the war between China and Japan that ended in 1945, but could be assigned various beginning dates, there are even more options. The answers depend in large part on how much one wants to single out the fighting that began in July, 1937 (but did not become a declared war until December 9, 1941) from (a) their earlier conflicts, (b) Japan's wars with other powers in this period, and the conflicts of those powers with still other powers, and (c) the fighting among various groups of Chinese during the same years. These are somewhat different issues from the cases above, where at least the dates of the conflict being named (1839–42, 1851–64, 1899–1901, 1911–12) are not much in controversy, and where—despite the interventions of foreigners in the Taiping Rebellion, or of Chinese who assisted the foreigners in the Opium War—the identities of the primary participants (except perhaps in the Boxer case) are not that much in doubt. And given the overlapping CCP-GMD conflict, and the scale of collaboration (reluctant or not) with Japan, it is probably this war for which Hayford's concept of "a long internationalized (Chinese) civil war" works best.

But another question that emerges from Hayford's approach. Is it possible that "long internationalized civil war" isn't a necessary concept for dealing with most of the individual conflicts listed here—that we can assign each of them to a "primarily foreign" or "primarily internal" category, even if they always have some elements of the other kind of conflict—but is nonetheless a good description of the 110-year-long "wave" he posits, taken as a whole? And do we need a label for that "wave" as a whole, if only to provide an alternative to the polemical "century of humiliations"? Certainly some framing for this period that is neither as nationalistic as that, as restrictive as "China's response to the West," or "China's long revolution," nor as anodyne as "China's early modernization" would be a welcome addition to our toolkit.

Since Hayford suggests his wave metaphor as a heuristic stimulus to future thinking, rather than a statement of what existing work has shown us, it is too soon to tell how well it may fit this need. In closing, though, let me suggest that using

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[54] Here Hayford quotes from Julia Lovell, *The Opium War: Drugs, Dreams and the Making of China*. For an account of how the label "Opium War" was placed on this conflict by British opponents of the war (its supporters preferred to call it "the China war," see Song-Chuan Chen, *Merchants of War and Peace: British Knowledge of China in the Making of the Opium War*, 133–37).

that metaphor need not commit anyone to a wave of the particular length that framed our panel. We have long known—and Halsey's essay reminds us—that the intense militarization of the years from 1839 onward had roots at least as far back as the White Lotus Rebellion, and arguably much further; we also know that the impact of war on the making of modern China did not end with the Korean armistice.[55] We have come a long way in re-integrating war and its many ancillary activities into the history of modern China, but there is much more work ahead.

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[55] See, for instance, Muscolino's discussion of the militarization of river work, discussed above; Diana Lary's argument that the suffering and social ruptures of WWII prepared the way for the Tmplacability of high Maoism, in Lary, *The Chinese People at War*, 194–201, 208–11.

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