

regional politics, economics, and society. Given that the family of shaykhs was linked to the Kartid Dynasty through marriage as well as economic and cultural projects, *The Sufi Saint* lends important depth to *A History of Herat* that would not emerge from a study based entirely on sources explicitly about the Kartids and their city.

Mahendrarajah writes in a unique style that is less formal than most academic prose. At its best, this brings a levity to the work that keeps the reader engaged through passages that might otherwise become tedious. At other points, the book reads as unfinished—a collection of reading notes not yet fully composed. This impression is enhanced by the inclusion of extensive tables in chapters One, Four, Six, and Ten and Appendices 2 and 3 that catalogue the evidence on which the book is based but which do not necessarily lend information beyond what is in (or should be in) the main text.

One real shortcoming of this book is its index. While poor indexing often goes unremarked, the particular nature of this book makes certain indexing choices regrettable. Since the two parts of the book deal with two aspects of the same history, the reader needs a tool to help draw connections between them. The index should be this tool, but it is not. Large bodies of material are subsumed under generic headers ('citadels', 'gateways', 'honyms', 'toponyms', etc.), so that the reader must first divine the indexer's categorisation to find a particular item. For example, the Kartid redoubt at Iskilchih, which gave the family refuge in times of political turmoil and which formed an important link in their regional security network, is listed only under 'Citadels > Fort Iskilchih', with no cross references to help the reader find it there. In a book about medieval Iranian economy and society, there is an entry for Charles Tilly, but not for caravansarays.

On the whole, *A History of Herat* takes an important step in scholarship, but it stumbles in some crucial ways. It will be a helpful resource for students of the political, economic, social, and cultural history of Iran in the thirteenth and fourteenth centuries, but it is not as comprehensive or internally consistent as it might have been, leaving the final impression that it was rushed to publication.

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## Uncertainty in the Empire of Routine: The Administrative Revolution of the Eighteenth-Century Qing State

**By Maura Dykstra. xxxv, 262 pp. Cambridge, MA, Harvard University Asia Center, 2022.**

Macabe Keliher

Clements Department of History, Southern Methodist University, Dallas, TX, USA  
Email: [macabe@smu.edu](mailto:macabe@smu.edu)

State centralisation stands as one of the most significant developments of the early modern world.<sup>1</sup> As empires swelled, rulers worked to extend personal power and enact policy

<sup>1</sup> For overviews of early modern trends, see Jerry Bentley, 'Early modern Europe and the early modern world', in *Between the Middle Ages and Modernity: Individual and Community in the Early Modern World*, (eds.) Charles Parker

throughout their territories, often by bureaucratic expansion and rationalising administrative procedures. In exploration of this phenomenon, scholars have traditionally focused on military and administrative capacity but, in the past few decades, have turned their attention to record-keeping and archives, illustrating how the control of information helped facilitate expansion abroad and consolidation at home. In this latter genre, known as the archival turn, scholars find early modern states not only generating more paperwork and information about their territories and populations, but also using it as a technology of power. In this way, the archive has come to be seen as part of the ruling repertoire for imperial expansion, consolidation, and governance.<sup>2</sup>

Late imperial China was no exception to global trends.<sup>3</sup> Historians have mapped developments that began in the Song and shown how rulers increased their power over the local elite, expanded administrative functions, and further rationalised reporting and resource extraction. Some of the most significant bureaucratic and archival developments of the Qing—such as the Grand Council, Lifayuan, and Imperial Household Department—enabled conquest, expansion, and fairly effective rule. The book under review takes as its theme these developments, and does so with a focus on the archive as a site of power that facilitated Qing centralisation.

The book argues that, in order to discipline the bureaucracy, the Qing initiated an ‘administrative revolution’ by changing official reporting requirements. These changes, the author claims, generated more administrative paperwork and enabled the throne to collect more information and thus know more about its administrative staff and its operations. The more information the throne collected about its administrators, however, the author says, the more it came to know about incompetency and inefficiency, and the more information it demanded in an attempt to straighten out affairs. Yet, the author continues, this increase in information, while meant to root out corruption, ‘actually created evidence of corruption’ (p. 2) and ultimately ‘invented a crisis’ (p. xxii). Moreover, the author asserts, this administrative revolution has misled historians: scholars reading the Qing archive have mistaken the documentary evidence of a crisis for a real crisis. This is to say, historians have taken evidence of corruption as actual corruption, when in fact it was but an increase in Qing records on the matter. The crisis, she claims, is an invention—‘a paper ghost ... whose existence has been taken for granted’ (p. xxii).<sup>4</sup>

These are big claims. To be convincing, such an argument needs to both fully engage existing literature and be grounded in the empirical evidence, particularly the archive that historians are accused of misunderstanding. From the perspective of this reviewer, neither of these conditions has been met. The following first takes the author’s argument on its own terms and examines the claims of revolution, crisis, and historiography before turning to problems with the evidence.

In the author’s telling, the reporting reforms of the Qing constituted nothing less than an ‘administrative revolution’. The author does not discuss the meaning of an

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and Jerry Bentley (London, 2007), pp. 13–32; Luke Clossey, ‘Early modern world’, in *Berkshire Encyclopedia of World History*, (eds.) William McNeill and Jerry Bentley (Berkshire, 2005), pp. 592–98.

<sup>2</sup> The literature on the archival turn is large but mainly concentrated on Europe. For good overviews, see Ann Blair, ‘Introduction’, *Archival Science* 10.3 (2010), pp. 195–200; Alexandra Walsham, ‘The social history of the archive: record-keeping in early modern Europe’, *Past & Present* 230.11 (2016), pp. 9–48. Also see the Special Issues of *European History Quarterly* 46.3 (2016); *Journal of Early Modern History*, 22.5 (2018).

<sup>3</sup> For an overview of the Qing as an early modern empire, see Evelyn Rawski, ‘The Qing formation and the early-modern period’ and R. Bin Wong, ‘Did China’s late empire have an early modern era?’, both in *Comparative Early Modernities, 1100–1800*, (ed.) David Porter (New York, 2012), pp. 195–216.

<sup>4</sup> An alternative conclusion that the author could draw from her claims is that the Qing was always corrupt and ineffective; the reports just brought it out into the open. It is unclear why the author does not address this alternative in the face of her claims.

administrative revolution; the closest she gets to defining the perceived change is that it ‘fundamentally altered both the everyday work of provincial offices and the stakes of central review’ (p. 154).<sup>5</sup> From the author’s own evidence, however—and at one point own admission—nothing much came of the alleged reforms except more bureaucracy and more paperwork. The author has not shown that the reporting requirements changed the work of officials, nor that it altered how they related to the throne. To the contrary, the author points out that ‘many reports and cases piling up in the provinces ... led to no particular repercussions’ except occasionally for the unlucky official who was slow in processing his cases (p. 172). This is less a revolution and more an involution. Most tellingly is that the author’s key illustrative case (chapter 5) does not reflect changed governance or even altered routine practices. Rather, as previously illustrated by Hu Kuo-tai, whom the author does not cite, the case was not a consequence of ‘archival triangulation’ through the routine ‘information trap’, as the author claims (p. 200), but rather one of personal networks and non-routine communications, namely the Palace Memorial system, which the author never mentions throughout the book.<sup>6</sup>

The author does not delve further into the nature of the ‘administrative revolution’ and its effects, but instead turns to the inner anxiety of historical actors, which she claims invented crisis. As a result of more information, the author says, rulers grew ‘anxious’ about bureaucratic discipline. For evidence of anxiety, the author points to the growing number of demands from the throne but provides only one example at the end of the book: the Qianlong emperor grumbling that he had not received any reports from the provinces in three days (p. 225). One may question whether this constitutes proof of anxiety, especially since the emperor’s complaint here is about secret memorials (*zouzhe*) not routine reports (*tiben*), which is the subject of the author’s study.<sup>7</sup> Leaving aside this oversight, one still wants to hear the actors themselves articulate their thoughts and feelings on the matter and how this framed a crisis in their minds.<sup>8</sup> Indeed, what exactly does the author mean by an ‘invented crisis’? Certainly not the famines that plagued northern China, nor the rebellions that rocked the empire, nor the rot of Hesen, nor silting of rivers and failure of dikes, for these were very real crises that increased in intensity and frequency in the late eighteenth and nineteenth centuries.

If the arguments of the book do not hold, do the criticisms of historians? There is an unremitting tone of chastisement running through the book, which proceeds by taking historians to task for not recognising the administrative revolution, then for mistaking the evidence of corruption for real corruption, and finally for misunderstanding this revolution in paperwork as the sign of a real political and social crisis. What historians often take as one thing, the reader is told, ‘in fact comprise’ something else (pp. 58, 61, 62, 94). This is a straw man. No serious Qing scholar believes their documents are untainted and

<sup>5</sup> Readers would be forgiven for turning to the body of literature on administrative revolutions, which refers to radical developments in bureaucracies and governance in the nineteenth and twentieth centuries; yet the author neither engages nor cites this literature and seems to have a far more subdued understanding of the meaning of revolution. See Fred Riggs, ‘Modernity and bureaucracy’, *Public Administration Review* 57.4 (1997), pp. 347–53.

<sup>6</sup> Hu Kuo-tai, ‘Huangquan, guanliao, yu shehui zhixu: Qingdai Qianlongchao Zhang Hongshun xiongdi zeian yanjiu’, *Zhongyang yanjiuyuan jindaishi yanjiu suo jikan* (June, 1996), pp. 85–116.

<sup>7</sup> The author frequently conflates the two types of reports without acknowledging the difference.

<sup>8</sup> A better explanation of the aforementioned citation and the spiralling effect of more information generating more requests is the institutionalisation of norms and practices. Anxiety is neither here nor there for, when reports fell off, actors would begin to question the discrepancy as a break in the norm. This has been well theorised in organisational studies. See Gary Hamilton and Nicole Biggart, ‘Why people obey: theoretical observations on power and obedience in complex organizations’, *Sociological Perspectives* 28.1 (1985), pp. 3–28; Harrison Trice and Janice Beyer, *The Cultures of Work Organizations* (Englewood Cliffs, NJ, 1993).

unproblematic, nor do they read them in the way that she claims. Indeed, the reader can only guess what historians the author wants to implicate (she cites none) or how they have misread the archive (she gives no concrete examples). Moreover, it is hard to know what the author thinks a proper reading of the archive would look like and how it would yield different insights or conclusions from those historians have already drawn. Does the author believe, for example, that the White Lotus rebellion (1794–1804) was invented by the Qing in its ‘quest for certainty’?

The above analysis assumes that the author has correctly read her own evidence and only overreached in its interpretation. A few spot checks of her sources, however, raise more questions. There are three fundamental problems. First, despite the author’s assertion that the book is based on archival work, the majority of her evidence comes from highly curated printed sources compiled by the court—namely the *Shilu* and *Huidian*—which do not necessarily reflect the proceedings under inquiry. Second, these sources show not blanket reforms in the reporting process, as the author claims, but rather Qing emperors simply tinkering and commenting on existing practices.<sup>9</sup> Third, a number of her citations refer to secret palace memorials, not routine reports, as the author thinks—indeed, the author fails to distinguish between these two very different communication streams.

Would reading sources correctly lead to different conclusions about Qing centralisation, namely that neither administrative work nor reporting was ‘fundamentally altered’—that is to say, there was neither the so-called administrative revolution nor even involution in routine communications? Space does not permit a thorough examination; a few examples will suffice.

The claim of chapter 4 is that the administrative revolution produced new types of reporting. This claim appears to be based on a misreading of the evidence. The author writes: ‘The sheer volume of reports demanded by the Beijing ministries soon grew so large that a new genre of reports began to proliferate: reports on reports. These documents were known as “summary memorials” (*huiti* 彙題)’ (pp. 156–57). Her evidence comes from the *Shilu*, which she reads as requiring provinces to submit summary memorials to Beijing with ‘yearly tallies’ of cases that could also be ‘compiled into empire-wide statistics [by the ministries] for perusal by the heads of state’ (p. 160). Upon examining the original passage, however, one finds nothing on provincial reporting nor statistics. Rather, in the said passage, a censor suggested that *only* ‘major affairs’ (重大之事) *might* be summarised ‘concisely and clearly’ (簡明彙奏) and done so NOT by the provinces, but by the Boards of Revenue and Punishments. Furthermore, these boards are to submit reports as *palace memorials*, showing again that the author may not understand the different communication systems—she even changed the quotation: the original passage does not use the term *huiti*, but rather *huizou* 彙奏.<sup>10</sup>

Such problems continue to drive the author’s analysis. She claims that the provinces engaged in ‘a massive end-of-year reporting ritual’ (p. 157) but provides neither citations nor evidence of such a practice. Likewise, she outlines a ‘ministerial summary memorial aggregating information’ (p. 158) but provides no information on the office of origin or who submitted it to whom for what purpose—or even if it was a secret or routine memorial. She says that ‘multiple summaries existed’ but gives no indication on where they might be found. In fact, in the next paragraph, she accuses Beijing’s First Historical

<sup>9</sup> For example, the *Shilu* passages that the author cites for key ‘reforms’ do not tell of pledges or the need for extensive reporting, as the author says, but rather give instructions for limited and specific situations (pp. 121–22). Similarly, a key Yongzheng-era reporting requirement for local magistrates (p. 129) turns out to have been a long-time practice, as seen in a well-known Kangxi-era handbook—so well known and important, in fact, that it has been translated into English: Liu-hung Huang, *A Complete Book Concerning Happiness and Benevolence: A Manual for Local Magistrates in Seventeenth-Century China* (Tucson, 1984), pp. 617–25.

<sup>10</sup> QLSL 9.12.1.4 vol. 230.

Archive (FHA) of having ‘obscured’ these materials but offers neither explanation nor citation that they even exist (p. 159). This lack of clarity is repeated for tables and statistics of tallies on infractions (pp. 194–96): ‘Reporting of local infractions increased over time,’ she says, referring to her own limited survey of FHA documents, again offering no contextualising information.<sup>11</sup> Computations of the use of the character ‘case’ (案) lack enough context to be meaningful or explanatory and rely solely on the problematic *Shilu* (pp. 183–84).<sup>12</sup> Lastly, discussions of the supposed increase in paperwork on all matters great and small veer into one claim after another with no evidence or citation: detailed reports generated (p. 162), no more false reports (p. 168), proliferation of documentation of cases (p. 185). The list goes on.

So what does it all mean? The author seems to have also felt something amiss, for she makes a surprising admission in the closing sentence of the book: that none of this can explain the Qing. ‘The question that now falls to historians of this era is: How did the pursuit of certainty continue to drive the Qing into ever more uncertain waters after the net had spread’ (p. 236)? Unable or unwilling to situate her study in either the historiography or a trajectory of the Qing, she tells readers to accept her claims and figure out the importance for themselves. We might turn to the late Philip Kuhn on the matter, with whom the author does not engage and cites only once. Writing about ‘[t]he Monarch’s control of bureaucrats’, Kuhn notes that Qing rulers

had to pick their way carefully between routine and arbitrary models of command. When rules were ineffective, the remedies included not only more rules but also procedures that rested upon arbitrary power. From early in his reign, [Qianlong] was impatient with rules that did not work. His remedies included both tightening the screws of the routine bureaucratic machine and finding ways to inject his own arbitrary power into it.<sup>13</sup>

It seems that historians of the last era already provided an answer to the question. A convincing book offering another answer—or even how the Qing used the archive to centralise power—still remains to be written.

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<sup>11</sup> In a footnote, the author says that others will not be able to replicate her results because the FHA flagged her account (and these materials?) for overuse (p. 197, note 8). If I understand correctly, the author appears to have postulated non-existent evidence, blamed the archive for hiding that evidence, then announced that no one else can look for it.

<sup>12</sup> The *Shilu* cannot be representative of affairs or developments for reasons stated above. In addition, any change in the frequency of appearance of any character would have to be mapped against its context as well as other characters and carried forward in time.

<sup>13</sup> Philip Kuhn, *Soulstealers: The Chinese Sorcery Scare of 1768* (Cambridge, 1990), pp. 190–91.