

ʿAbd al-Ghānī al-Nābulusī, fully embraced the theory of “the unity of existence.” These scholars were affiliated with the Shaṭṭāriyya and the Naqshbandiyya, Sufi orders that had their origins in India, but with the example of the Ḥanbalī Khalwatī order, the chapter also introduces the potentially surprising description of Ḥanbalīs who supported the thought of Ibn al-ʿArabī (p. 264). Here El-Rouayheb makes the point that present-day scholars need to work harder to understand the ways in which early modern Muslims drew in varied ways on Ibn al-ʿArabī’s thought, and not to reduce them to pro- or anti-Ibn al-ʿArabī camps. The second chapter continues to unravel the link between Ḥanbalism, Ashʿarism, and Sufi writings inspired by Ibn al-ʿArabī, drawing attention to interesting ways in which prominent thinkers such as al-Kurānī rejected aspects of Ashʿarism such as occasionalism, in favor of a belief in secondary causality that was professed both by Ḥanbalīs and Ibn al-ʿArabī (pp. 297–99). Perhaps provocatively, El-Rouayheb argues in this chapter’s conclusion that the complex Salafi movement of the nineteenth to twentieth centuries has its origins in part in this confluence of Ḥanbalism and Sufism that had made inroads into Sunni thought in the previous centuries (p. 311). The third chapter addresses the ways in which al-Kurānī and al-Nābulusī defended the theory of *waḥdat al-wujūd* theologically against Saʿd al-Dīn al-Taftazānī’s (d. 1389) critiques, arguing that these two thinkers were just as radical in their adherence to the full implications of the concept as Ibn al-ʿArabī’s early followers had been.

El-Rouayheb’s conclusion gives a concise and useful overview of the book’s trajectory and makes an eloquent case against measuring debates in the early modern Muslim world according to Muslim scholars’ engagement with European texts. He similarly brands as irrelevant other past attempts to find an index for Muslim creativity in this period, such as the debate on the closing of the doors of *ijtihād*.

As I stated above, this is an important book and one that scholars of the early modern period will no doubt read avidly. El-Rouayheb has been writing on this subject for almost a decade now, and the content of several of his book chapters and articles has been incorporated here. Those who follow his work will be satisfied that this is more than the sum of these previous writings and that it goes well beyond them in its overall analytical ambition. But there is a good chance that scholars of Islamic history who work on the formative or modern periods will pass over this at times dense book, perceiving it to be of only marginal relevance to their own research and teaching. That would be a mistake. Pressed for space, I cannot present a rounded argument and so will only claim that how we understand the intellectual trajectory of the early modern Muslim world has had substantial implications for both how we understand what is still often called the Golden Age of Islamic thought and how we approach the so-called reformist movements of the nineteenth and twentieth centuries. In its nuanced and detailed readings of the long seventeenth century, El-Rouayheb’s book contains numerous insights into the significance and nature of both earlier and later periods and should be read by intellectual historians of Islam, regardless of the period in which they specialize.

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Witchcraft and the Rise of the First Confucian Empire. By LIANG CAI. Albany, N.Y.: STATE UNIVERSITY OF NEW YORK PRESS, 2014. Pp. xii + 276. \$85 (cloth), \$27.95 (paper).

Liang Cai’s book concerns the history of the classically trained officials during the Western Han period. As her title suggests, the Han empire became a “Confucian empire” only after the witchcraft affair of 91 B.C.E. In Cai’s view, this event, which started when accusations of black magic were brought against the heir apparent, wiped out the hereditary groups that until then had enjoyed a virtual monopoly on the highest political offices and created opportunities for classically trained scholars to claim a larger share of high political offices. Huo Guang, who had a stronghold on the central court after Wudi (r. 141–87 B.C.E.) died until his own death in 66 B.C.E., was a key figure in this process. He bolstered his own power by employing and promoting several *ru* willing to use their knowledge of

the classics—and particularly a new kind of omenology based on classical texts—to support his cause. These *ru* who, due to the difficulty of their craft, formed intricate teacher-disciple networks, then used the continuously evolving recruitment system for officials to place more and more members of their group into high positions of power.

Scholars of China's early empires—Japanese, Chinese, or Western—have for some time been challenging the traditional view that Confucianism was elevated during Wudi's reign, to remain the dominant ideology for the rest of the imperial period. Several of these scholars (Michael Loewe, Michael Nylan, Hou Xudong, Fukui Shigemasa) have proposed, like Cai, that a "classical turn" occurred only after Wudi's death. So, is Cai's revisionism different from that of the other scholars, and if so, how?

The most obvious difference is methodological. Cai attempts to make numbers speak: the book is filled with various tables and graphs that seek to show that only 7.79% of the high officials during Wudi's half-century long reign were *ru* (6 out of 77), whereas that proportion rose to 32.43% during the half century that spanned the reigns of Zhaodi (r. 87–74 B.C.E.), Xuandi (r. 74–48 B.C.E.), and Yuandi (r. 48–33 B.C.E.); during this period 24 out of 74 identifiable high officials were *ru*. Whereas these numbers are persuasive, and indeed demonstrate an increase of *ru* among high officials, they are also highly dependent on how Cai defines her terms, on her interpretation of biographical data, and on inherently limited primary sources. Cai, early on in her book, recognizes the problematic nature of the term *ru*: she writes "those who called themselves *ru* in Han times were a heterogeneous group with varying intellectual orientations; some were not even followers of Confucius" (p. 9). This, of course, is not a definition one can use to separate *ru*-officials from non-*ru* officials, and Cai ends up adopting different criteria for the pre-91 B.C.E. and post-91 B.C.E. periods: during Wudi's reign they are simply, and somewhat circularly, "those called *ru* by Sima Qian and Ban Gu"; for the latter period she discerns a more self-conscious group, the members of which engage in serious study (for roughly ten years) of one or more of the five classics, and are linked with one another through master-disciple relations. (On p. 189 she opposes real *ru* to those "who merely took a class or two on Zhou culture or those who recite a couple of sentences from the Five Classics.") Cai's proposition as to who is included among the high officials is clearer and equally applies to both periods: they are the Chancellors (*chengxiang*), Grandee Secretaries (*yushi dafu*), Commanders-in-Chief (*taiweidasima*), Ministers, and senior officials serving in the capital area. Still we do not know the names of all the high officials, and for many of them we do not have enough biographical data available to decide what precisely allowed them to gain access to high office. For those who are included among the *ru*-officials, the question remains to what extent it was their classical training that won them promotion or whether other factors were at play. (Cai addresses this question head on in other chapters, but less so in the first chapter that provides most of the statistics.) Lastly, it is important to point out that, whereas Cai's tables and graphs do give us a valuable picture of what is going on at the highest echelons of power, they are, for lack of adequate source materials, completely silent about what is happening at the lower levels or away from the capital—even though important connections must have existed between what happened at court and in the realm at large. Were *ru* traditions being preserved away from the capital, as Hsing I-tien suggests when he describes how the Qin and Han capital area was exceptional in *not* featuring family cemeteries until relatively late? Was there a distinction between Confucian and non-Confucian officials at the lower levels of the bureaucracy, as Barbieri-Low and Yates posit in their introduction to their recent translation of the Zhangjiashan legal materials? (They understand the *Zouyanshu* as a collection of stories through which some "non-Confucian" legal clerks try to best and poke fun at their "Confucian" colleagues.)

A second distinctive feature of Cai's book lies in the close ties she sees between her theme—*ru* in high official positions—and the study of the Western Han system more generally. She draws upon a wide range of secondary scholarship, mostly in Chinese, to provide a sketch of the Western Han recruitment system—usefully drawing our attention to some aspects of that evolving system that are often overlooked. She emphasizes how especially during the first century of Western Han the nobles ("marquises" in her translation) had privileged access to the highest offices. She understands that nobility as, by and large, a hereditary class consisting of the comrades-in-arms of the Han founder and their descendants, and maintains that this group, generally speaking, consisted of social upstarts averse to the

benefits of a classical education. Cai also points out how recent archaeological discoveries have helped us understand another avenue to office: clerks, even those of low background, were able to climb to high-ranking positions, either by leaps and bounds, or gradually, by being promoted from step to step due to seniority and merit. Some of the *ru* Cai studies climbed to the top as clerks. After 135 B.C.E., some won distinction via the Imperial Academy. Cai makes a strong claim that success in the Academy only guaranteed access to lower-level official positions (as *lang* or courtiers), and that incumbents needed to be as creative as others in their position in finding ways to climb up.

Cai's central thesis is that as long as the nobles had a stronghold on power, the chances of the *ru* to rise to the top were very slim, and that this changed in 91 B.C.E.. This was the year when the witchcraft affair erupted, causing the downfall not only of the consort families that dominated the last decades of Wudi's reign, but also of many noble families associated with them. In emphasizing this event as the decisive moment at which "Emperor Wu swept clean the entire political stage," Cai, is, once more, parting ways with other scholars. Michael Loewe, for example, has pointed to 112 B.C.E. as the year in which Wudi purged many of the surviving noble lines. It may be that rather than a single event, the elimination of the noble lines with their hereditary access to the highest official positions was a process that took decades to complete, and that Cai chose to highlight the witchcraft affair because the event—with foreign shamans scouring imperial grounds in search of magic dolls and the crown prince engaging in military battles in the streets of Chang'an—speaks to the imagination. Most scholars who have argued for a classical turn in the post-Wudi period have situated this slightly later than Cai proposes, in ca. 50 B.C.E. rather than in Huo Guang's era, but, as we are dealing with complex historical processes, the difference might be a matter of emphasis rather than substance.

Cai also presents an original reading of some of the collective biographies in *Shiji*, particularly those dealing with *ru*-officials (*Shiji* 121) and with harsh officials (*Shiji* 122). Indeed, if traditional scholars have, by and large, missed how unimportant the *ru* were until 91 B.C.E., much of the blame for that can, in Cai's view, be laid at the feet of Sima Qian, who "invented" a collective identity for the *ru* of Wudi's reign where none existed, and who often, misleadingly, made it seem as if training in a classic was the only factor propelling the career of a *ru* upward. The *ru* of Wudi's time were, in Cai's view, disadvantaged individuals with no incentive to cooperate (she draws here on the theories of the sociologist Michael Hechter). Sima Qian resented non-*ru* officials, particularly those who had achieved their positions because of their family background, because they had purchased a position, or because of clerical abilities. In the chapters of *Shiji* he constructed a utopia in which the *ru* were presented as a cohesive group, bound by a common allegiance to the figure of Confucius and to some early Han masters who rescued the classics from the Qin bibliocaust. Cai almost goes as far as to cast early Han classical masters such as Fu Sheng—according to Sima Qian and Ban Gu the founder of all Han traditions relating to the *Documents Classic*—as literary inventions, and claims that true *ru* cohesiveness, as well as real interpretative schools only developed after the watershed year of 91 B.C.E..

Cai's book might not be the definitive account of the *ru* in Western Han, but it certainly presents much food for thought. The book contains many interesting, inventive readings of individual passages. Cai is absolutely right to link the history of the classical scholars as officials to changes in the political culture and to the decline of the nobility. She is also spot on in her willingness to read Sima Qian and Ban Gu against the grain; and she does a great service to the field by pointing out the importance of surveying complete data sets as an antidote to the rhetoric contained in the transmitted histories. At the same time, some of her claims seem to be made in haste, and can appear a bit overstated at times. This book, therefore, should be seen as an important contribution to an ongoing scholarly dialogue on one of the central topics in Western Han history—a dialogue that will require further investigations into and refinement of the bold theses Cai put forward.

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