

REVIEW ARTICLE

An Imaginary City State against Its Imaginary Big Bad Other

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The Jiankang Empire in Chinese and World History is an ambitious book that attempts a revisionist account of the period known as the Six Dynasties—Wu (222–280), Eastern Jin (317–420), Song (420–479), Qi (479–502), Liang (502–557), and Chen (557–589), whose capital was all based in the city of Jiankang (modern Nanjing)—and aspires to reorient Chinese and world history with its revisionist agenda. It engages with hot contemporary issues in Chinese historical scholarship, namely ethnicity and nationalism, and makes provocative arguments about the period in question. One of the few book-length historical studies in English devoted to the early medieval southern dynasties with a focus on the fifth and sixth centuries, it broadly draws upon previous scholarly works in various fields and sub-fields to tell a synthesized story within “an alternate framework and terminology” (p. 4). The book is highly commendable for trying to paint a big picture and to reach out to historians of later periods and of other cultures. However, the wide, yet selective and decontextualized utilization of secondary sources runs the risk of superficiality and partiality, especially when an author does not read extensively and deeply in primary sources or strive to give a fair, accurate representation of primary sources. Dominated by the author’s pursuit of an *idée fixe*, this book has a serious problem with evidence throughout.

The Jiankang Empire in Chinese and World History. By Andrew Chittick. New York: Oxford University Press, 2020. Pp. xi + 411. £55.00.

I wish to thank Sarah Schneewind for her feedback and Lai Kuen Cheng for her careful editing.

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The Jiankang Empire contains twelve chapters, the first and last being Introduction and Conclusion, and three appendixes (“The Population of the Jiankang Empire,” “Migration,” and “Geographic Distribution of Officeholding”). The middle ten chapters are divided into three sections, respectively entitled Proto-ethnic Identities (Chapters 2–4), Political Culture (Chapters 5–7), and Repertoires of Legitimation (Chapters 8–11). Section 1 argues that the most politically important proto-ethnic groups in the Jiankang Empire are the Wuren 吳人 (Wu people), the Churen 楚人 (Chu people), and what the author calls Zhongren 中人, the people of the Central Plains or the Yellow River plains. Section 2 discusses the political culture of the southern regimes in the fifth and sixth centuries in three aspects—frontier strategy, court institutions, and political economy—arguing that the Jiankang regime “operated quite differently from the traditional conception of ‘Chinese’ political culture” (p. 36). Section 3 contends that the Jiankang elites used local vernacular culture, Sinitic universalism, and Buddhist universalism to construct a “regional exemplar state” paradigm. The section also argues that in the sixth century the Jiankang regime began to prioritize the Buddhist repertoire over the Sinitic and, thus, moved even further away from the Central Plains political model and closer to a Southeast Asian model, which the author sees as the ultimate justification, among other things, of his calling the Jiankang regime a “Southeast Asian state” (p. 343). All these arguments require substantial evidence; but before the author gets to that, he devotes considerable space—43 pages, one-eighth of the book proper—to outline the “alternate framework and terminology” in the introduction. Since the introduction brings up a series of important issues and concepts, both for the book itself and in a general sense, it deserves a detailed discussion.

The introduction begins with the premise that “hardly anyone” has ever heard of the Jiankang Empire and proceeds to state that answering these questions—why that is the case, what was the Jiankang Empire, and what significance it holds—is “the primary objective of this research” (p. 1). If the premise refers to the term “Jiankang Empire” per se, it is certainly accurate, because this is a term coined by the author in this book; but clearly the author is referring to the historical period in question, claiming “the early medieval period is the least-studied, least well-understood, and most skipped-over period in Chinese history” (p. 7). He acknowledges the “ample and vigorous scholarship on the cultural accomplishments of the Jiankang Empire’s elite,” but claims that the cultural history is being studied and “celebrated” as part of “the tradition of Chinese civilization” that is “routinely shorn of its unique geopolitical context” (pp. 8–9), considering the work of literary and religious scholars as merely reinforcing the binary framework of “south-north/*wen-wu*.” The author finally pinpoints Jiankang Empire’s political and military history and economic influence as the real areas that are “all but invisible on the

world stage” (p. 9). Their “invisibility” is attributed to a problem with “Chinese nationalist history” that prizes large “unified” empires or dynasties because “they bear the closest resemblance to the extent of the modern nation-state” (p. 6), and the Six Dynasties is seen as embarrassing and insignificant, a mere “precursor to the glory of the ‘unified’ Sui and Tang dynasties” (p. 7). The author elaborates:

The razing of its dynamic and prosperous capital city, and the transportation of its elites to servitude in the north, is glossed over as a “unification” that allowed “China” to once again achieve imperial greatness. This means that the military and institutional history of the Jiankang Empire is understood to have been an *impediment* to the flourishing of China’s civilization; only with its elimination and utter destruction could “China” rise again. The narrative makes it difficult to express pride in Jiankang’s political and institutional legacy, and no subsequent major regimes did so. It has also led to a paucity of modern historical scholarship, especially by historians outside of China, on the Jiankang Empire’s military and political culture; both are seen as a “dead end” that did not obviously lead to any period of great glory for the Chinese people, or have decisive impacts on later Chinese, much less world, history. (p. 8)

This book is thus written with the objective of “rescu[ing] the history of the Jiankang Empire from that of the Chinese nation” (p. 9).

A major flaw in the proposed rescue mission is the author’s account of *what* the Jiankang Empire is being rescued from. Like the (mostly unspecified) scholars he criticizes for measuring the Jiankang Empire with a “Chinese ideal” and finding it a failure (p. 108), he himself espouses a notion of the “Sinitic paideia,” “Sinitic repertoire,” “Sinitic ritual and literary culture” that is rigid, static, unitary, and largely imaginary, confusing a version of the past with the cultural past as it existed in the fourth through sixth century C.E. and earlier. What the author calls “Zhongren”—the “people of the Central Plains”—is a straw man, used to define what the Jiankang Empire is not. This “Zhongren culture” gets characterized by a set of clichés that dissolve on examination into a sphere of Sinitic cultures through history that are diverse, contradictory, and always changing. The author is, in a way, trapped by the logic of his own thesis when he says that Jiankang’s political culture was “*different from* that of any other large medieval East Asian ‘Chinese’ empire” (p. 110, my italics). In the course of making his case, the “Jiankang Empire” itself is reduced to a much smaller realm consisting of Jiankang and the Wu region; gone from it are the Jing Chu 荆楚 people, the Ba Shu 巴蜀 people, or the Jiao Guang 交廣 people; or, speaking of ethnicity, the numerous, diverse Man 蠻 peoples in the south, to whom we will return below.

But it is good to pause a second and reflect on the claims in the paragraph quoted above. It is worth noting that, on the one hand, the Southern Dynasties were already denounced as corrupted and decadent by “subsequent major regimes” such as Sui and Tang (and the author also cites the Southern Tang and the Southern Song, p. 8, n. 17), which were certainly *not* writing a “Chinese nationalist history”; on the other hand, beginning in the Northern Song, the Southern Dynasties were also regarded as representing the line of historical legitimacy (*zhengtong* 正統), whereas the Northern Dynasties, which the author sees as laying the foundation for the “golden age” in the Chinese nationalist historical narrative, were not. In light of these factors, the attribution of “the Jiankang Empire’s invisibility”—and, by implied comparison, the Northern Dynasties’ visibility—to Chinese nationalist history does not quite work. The “paucity of modern historical scholarship, especially by historians outside of China” is also debatable, for there is surely no shortage of such writings in Chinese, or by Japanese scholars, who are decidedly outside of China. If the statement were modified to “a paucity of modern historical scholarship on the Jiankang Empire’s military and political culture in English language in the US, especially when compared with that on the Northern Dynasties military and political culture,” it would be much more accurate. The reasons for this phenomenon are, in my view, threefold—cultural, historical, and institutional—and could make an interesting topic for a study of the field, but that is beyond the scope of this book review. Suffice to say here that, while there is truth in the perception of the Southern Dynasties’ political and military culture being understudied outside of China, it seems to this reviewer a misleading oversimplification to blame everything on Chinese nationalism, as problematic and oppressive as that nationalistic discourse may be.

The criticism of Chinese nationalism, however, becomes a convenient lead-in for the author’s agenda. The introduction goes on to describe the way to accomplish the “rescue” mission: to disengage the Jiankang Empire from “China” and “Chinese” as much as possible in terms of political geography, culture, and ethnicity. A series of terms are regarded inadmissible. The terms include “China” and “Chinese,” which are saved in this book only for designating “the modern nation and its people” (p. 12). The author also proscribes “Period of Division,” arguing that the language implicitly assumes that “the region we now call China was already ‘one thing’ that *could* be divided” (p. 16), or the “Northern and Southern Dynasties,” seeing it as framing the period as “a binary split or schism, in which both sides ceaselessly sought to ‘regain’ their lost wholeness through ‘unification’” (p. 17).

Are the assumptions and prohibitions all justifiable? It would seem to depend on how *historically emic* we wish to be. The term “Period of Division” has its origin

from the point of view of the Qin, Han, or Western Jin Empires, and from the phrase “divided and fell apart” (*fenbeng* 分崩) that was used frequently from the end of the Eastern Han through Sui, as in various formulations like “[the realm] within the four seas / all under heaven / all the land, etc., was divided and fell apart” (*hainei / tianxia / sihai / shuaitu fenbeng* 海內/天下/四海/率土分崩). “Division” is not necessarily from the modern nation-state’s point of view. The author is entirely correct to state that the period was not always a time of chaos, but it is highly questionable to argue that during this period “the emphasis was on the permanence, indeed, the *naturalness* of the division between them [people of the era]” (p. 17). To speak of a land “broken apart” hardly seems to be an acknowledgement of the “naturalness” of the state of things. Military expansion and conquest for the purpose of reproducing a unified empire like the Qin and Han were avidly sought after by more than one ruler on both sides. More importantly, “north and south” are neutral terms of orientation designating two directions relative to each other and do not necessarily imply that they are two sides of a “whole.” Once again, people of the era frequently used these directional terms, *nan/bei* 南/北, or *nanren/beiren* 南人/北人, to refer to their rival state and people.

Instead, Chittick proposes to use the Sino-steppe zone and the Sino-Southeast Asian zone to respectively conceive of the two sides. He strenuously asserts similarities between the Jiankang Empire and the Southeast Asian states, citing their foodways (even though he admits that the Japanese and Koreans were also “rice-growing, fish-eating peoples,” p. 76), regarding Jiankang political leaders as equivalent to “men of prowess” in Southeastern Asian political cultures (p. 173) (but were the Northern Dynasties or any dynastic political leaders not “men of prowess?”), and comparing southern rulers’ extravagant public Buddhist worship to theatrical spectacle and ceremony in “Southeast Asian *mandala* states” (p. 295). The specifics of these points are dubious, but an even more fundamental problem here is that we know so little about Southeast Asian states from this historical period, and what little we know is largely from Chinese sources. Indeed, what we can call a Southeast Asian state at this time is pretty much only Funan 扶南, and even the author acknowledges that “we have very little written evidence from Funan itself” (p. 302, n. 32). Except for wresting it away from China/Chinese, the repackaging of the Jiankang regime as an outright “Southeast Asian state” (p. 343) is dubious, and does not do anything substantial to deepen our understanding of the regime or the period itself.

One particularly problematic move is the proscription of the word “Chinese” to describe the culture of this period. The author divides culture into high culture, “the ‘civilization’ of literate elites,” and the vernacular culture, “the lived experience of ordinary people in a society,” and maintains that “in neither sense can we identify

an early medieval culture that corresponds well to the modern conception of ‘Chinese’” (p. 19). It is not clear to me what this “modern conception of ‘Chinese’” is, or if there is indeed a single such conception. Regarding the high culture, the author argues that to call it Chinese “links it inextricably to the modern nation-state of China, when in fact it is the common heritage of all of East Asia” (p. 20), and thus adopts the term “Sinitic” in place of “Chinese” and the “Sinitic paideia” to designate the classical Chinese textual corpus. “Sinitic” has indeed become a common term nowadays in English academia, and it makes perfect sense when being used to designate the cultural heritage shared by the modern East Asian nations including China, Japan, Korea, and Vietnam. Nonetheless, if scholars of Japan, Korea, and Vietnam continue to refer to the early history and culture of these modern nation-states as Japanese, Korean, and Vietnamese, one wonders why scholars of China should be barred from doing the same. Regarding vernacular culture, the author states that “the lower classes of East Asia have always been much more culturally diverse than the upper classes, since they lacked the shared discourse of the Sinitic literary tradition as a common binding agent” (p. 21). This sweeping statement is problematic at many levels, including its spatial and temporal generality and vagueness, its assumption about the homogeneity of the “upper classes” or “lower classes,” and its supposition about a rigid division between “high” and “low” cultures; but a fundamental problem is that we do not know much about the “lived experiences of ordinary people” in the Southern Dynasties, and, apart from archaeology, whatever we know about it comes to us through its preservation in what the author would consider as “high culture.”

A final act of scholarly policing is the rejection of the term “indigenous” for describing the southern people, because to Chittick it implies that Sinitic influence is “nonindigenous or foreign” (p. 21). Instead, he prefers to think of Wu culture as having already integrated the Sinitic traditions into its own system and yet maintaining its distinctive cultural character (pp. 21–22)—in other words, he wants the Wu people to (so to speak) have the cake and eat it too. One problem is that, by focusing on Wu (and “Chu”—though this “Chu,” as we shall see, completely excludes what one might normally think of as “Chu,” i.e., Hubei and Hunan), the author ignores a large part of the diverse—and indigenous—southern peoples of the “Jiangkang empire,” implicitly treating them as *not* “politically important” (p. 35). One of the most glaring omissions is the Man peoples: the author denies their ethnic identity and dismisses them, in a footnote, as “an imperial administrative category” (p. 28, n. 74). The Man peoples spoke an array of different languages, had their own vernacular cultures, and had myths about common descent; they had violent clashes with the imperial state and were often slaughtered,

enslaved, or forced to become “military households.”¹ For the Southern Dynasties, even just the resources and energy invested by the state to persecute these peoples would make them “politically important” enough, and they deserve to be treated with more seriousness in a book claiming to rethink ethnographic categories in this period. It would seem irresponsible to claim that there was no ethnographic boundary between Man and non-Man simply because an unknown number of non-Man people from unknown regions reportedly escaped into the mountains to live amongst the Man peoples as a way to avoid the state’s heavy taxation.

This brings us to the last conceptual category that the author utilizes to disengage the Jiankang regime from China and Chinese: ethnic identity. The author relies heavily on the Austrian historian Walter Pohl, who, according to the author, suggests four levels of analysis for ethnic identification: (1) personal expression of allegiance to a social group; (2) a group’s collective self-representation; (3) the classification of a group by outsiders; and (4) scholarly identification of social groups as ethnic (p. 27). The author refers to the first two levels as *emic* evidence and the last two as *etic* evidence. The author believes the term “Wuren” 吳人 would ultimately evolve into “a true ethnonym” that is indicative of *ethnic* identity, but since he is hard pressed to find *emic* evidence for that (unless a native of the San Wu 三吳 [Three Wu] region refers to oneself as a Wuren in a literal sense), he draws exclusively on the *etic* criteria in his ethnic determination; in other words, he primarily resorts to the northern regimes’ references to their southern rivals, Wuren (“the Wu folks”) or Wu’er 吳兒 (“Wu lads”), as his measure of ethnicization. For this reason, he decides to call Wuren a “proto-ethnic group” (p. 28) rather than an ethnic group: this strategy allows the author to reference ethnicity without much (if at all) internal evidence, stressing how “ephemeral and transitory” or *proto* the group was on the one hand, and strenuously arguing for their “social and political coherence” on the other (p. 29).

¹ *Song shu* 宋書 (Beijing: Zhonghua shuju, 1974), *juan* 77, pp. 1996–98. I discussed the persecution of the Man peoples in my article, “Migration, Identity, and Colonial Fantasies in a Fifth-century Story Collection,” *Journal of Asian Studies* 80.1 (Feb. 2021): 117–19. The Man peoples of the early medieval period are a subject of considerable scholarship. See, for instance, Meng Mo 蒙默, “Wei Jin Nanbeichao shiqi de ‘Man’” 魏晉南北朝時期的「蠻」, *Journal of Chinese Studies* 中國文化研究所學報 19 (1988): 291–309; Lei Xiang 雷翔, “Wei Jin Nanbeichao ‘Man min’ de lai yuan” 魏晉南北朝「蠻民」的來源, *Hubei minzu xueyuan xuebao* 湖北民族學院學報 1 (Jan. 1990): 112–17; Yang Wenchun 楊文春, “Wei Jin Nanbeichao Huaihe shangyou de di yuan zhengzhi yu Manzu shili” 魏晉南北朝淮河上游的地緣政治與蠻族勢力, *Beifang luncong* 北方論叢 5 (2015): 78–81.

The real question here is the term “ethnic,” whether we add the prefix “proto” or not. How is this proto-ethnic identity differentiated from regional identity? The author is silent on this issue. The people of a region can have a strong sense of regional belonging, speak a distinctive regional language, share a vernacular culture, speak of their neighbours with disdain, and in turn are spoken of by their neighbours with disdain, which would be seen as an etic evidence for cementing their classification as a group. In fact, even two neighbouring villages or counties could enact just such a scenario. Yet, none of these needs to be taken as ethnicizing. In Chapter 2, “Discourse of Ethnicity,” the author dramatically downplays the importance of the idea of common descent groups and ancestral bloodlines in East Asian ethnicizing discourse and assigns these ideas to “European models” of ethnicity (p. 45), obviously because such an idea would not help the construction of Wuren as a proto-ethnic group, as the Wu elite always stressed that Wu’s founding kings, Taibo 太伯 and Zhongyong 仲雍, were the sons of the Zhou Taiwang 周太王—in fact, the eldest and second sons, thus having even more legitimate claim to the Zhou throne than their little brother who succeeded Taiwang; instead, the author claims that environmental and geographical determinism is “the more important sort of ethnicizing discourse” in East Asia (p. 45), without explaining why this should be the case or providing any evidence. Nor is there any attempt to explain the implications of environmental and geographical determinism for regionalism and local culture and how the implications may be different for ethnicity.

But even if we look to the etic criterion used to define Wuren as an ethnonym, the evidence is weak: there is no proof that “Wuren” in northern sources carries any ethnic implications. In fact, when Wuren appears in southern and northern textual records, in most cases it merely denotes a native of the so-called Three Wu region (Wujun 吳郡, Wuxing 吳興, and Kuaiji 會稽, in modern Jiangsu and Zhejiang). The author cites a single example of a generalized use of Wuren for the entire fourth century (p. 62). He states that the *History of the Wei Dynasty* 魏書, compiled by Wei Shou 魏收 (507–572), demonstrates the “most thoroughgoing summary of Central Plains attitudes toward the Jiankang regime and its people” (p. 63), and yet he finds only four uses of the term Wuren in it (p. 64, n. 62). It should be said that of the four, two specifically refer to the Wu region, and one is a reference to the State of Wu from the Spring and Autumn time, which leaves one instance as a *possibly* general reference to the “people of the Jiankang Empire.” The “plenty of other evidence” cited by the author (pp. 64–65) constitutes two mentions in tomb inscriptions (one of them is a historical allusion and does not apply to the southern regime but he fails to point that out) and one phrase, Wuren District (*Wuren fang* 吳人坊), in the *Luoyang qielan ji*

洛陽伽藍記. Because of this lone phrase, the author concludes: “This shows that the term Wuren was current in vernacular culture, to refer to the *colorful and exotic foreign* people of Jiankang” (p. 65, my italics). However, nothing about the term Wuren (or “Wu bandit” [*Wu zei* 吳賊] and “old Wu fellow” [*Wu’er laoweng* 吳兒老翁]) is discernably ethnic: with all its potential pejorative connotation, it is *regional*, much in the same way a New Englander in the US might say to a Nebraskan, “You cornhusker!” The author is aware how rarely the term Wuren appears in *Wei shu*, and tries to explain the rarity away by speculating that Wei Shou may be trying to “avoid colloquial ethnonyms” (p. 65), even though there is nothing “colloquial” about the term Wuren. By this point, the argument built on slim evidence has fallen into pure sophistry, along the lines of, “if a person does not use a certain word to designate something, the person must be avoiding that word”: this kind of insinuation only works if there is overwhelming evidence to show that everyone around the said person is using that word, and that word only, to designate that something. It is to the author’s credit that he observes Wei Shou’s much more frequent use of the term “southerner” (*nanren* 南人) in *Wei shu*; but once again it seems far-fetched when he speculates that it was perhaps because Wuren had developed positive connotations by this time and Wei Shou intended to vilify the southern regimes by avoiding the term (p. 65). The same lack of distinction between ethnic identity and regional identity goes for Chapter 3, “Agriculture and Foodways” (pp. 67–81), a chapter that uses the handful well-known anecdotes in *Shishuo xinyu* 世說新語 and *Luoyang qielan ji* to argue that “foodways and agricultural systems were an important ethnic marker in early medieval East Asia” (p. 67).

Chapter 4, “Vernacular Languages,” contends that spoken language is an important criterion for determining proto-ethnic groups. Here the author much overestimates the importance of differences in spoken language in the formation of ethnic identity or in the social and political life of the time. The claim that “the political and social system of the Jiankang Empire was largely oral and dominated by personal, face-to-face interactions” (p. 83) is largely without foundation; even a quick look at just one small area, namely the role played by an emperor’s handwritten notes to his ministers (*shouzhao* 手詔 or *shouchi* 手敕), copiously mentioned in the sources of this period, could undercut such a claim. This chapter focuses on three spoken languages that the author calls Wu vernacular, Jiankang elite vernacular, and Chu vernacular. Jiankang elite vernacular is the Luoyang-based speech spoken by northern immigrant elite while Wu vernacular was spoken by the natives of the Wu region: these are well-known. The author’s claim that by the fifth century the Jiankang elite vernacular was already changed by the many Wuren, Churen, and other southerners in court and “shifted” toward one with

Wu-inflected pronunciation is theoretically possible, but poorly supported. The single anecdote cited as evidence is one from *Shishuo xinyu* in which Huan Xuan 桓玄 asked Yang Fu 羊孚, “Why does everyone prize Wu sheng” 何以共重吳聲?² The author understands *Wu sheng* as “the speech of Wu” (p. 92), but *Wu sheng*, “the sound of Wu,” in Six Dynasties and Tang sources refers to *Wu music* without exception.

The so-called Chu vernacular is the most suspect. In describing pronunciation, *chu* had long been used as a word to indicate *generally* a southern accent. Hence Lu Ji 陸機 (261–303), a native of Wu, and Wang Dun 王敦 (266–324), a native of Langye 琅邪 (in old Lu 魯 region; in present-day Shandong), are both described as speaking with a *chu* accent, but it does not mean that they spoke the same vernacular. Yu Jiaxi 余嘉錫 has a detailed discussion of the meaning of *chu* in his commentary on the Wang Dun anecdote,³ which the author does not reference. The author’s declaration, based on the Wang Dun anecdote, that “we can be confident that there *was* a Chu vernacular language” (p. 96) has no basis in primary sources.⁴ Later, the author considers a slew of northerners from various regions of southern and southeastern Shandong as all “Churen” without any nuanced distinction between the old heartland of Chu (Hubei and Hu’nan) and the Warring States Chu territory or Han-dynasty administrative territorial division; he also disregarded how the so-called Churen would be identified by themselves or their contemporaries.⁵ This makes the assertion about the Song, Qi, and Liang rulers being “Churen,”

² Yu Jiaxi, *Shishuo xinyu jianshu* 世說新語箋疏 (Shanghai: Shanghai guji chubanshe, 1993), 1.104.

³ Ibid., 13.1.

⁴ While there are mentions of Wuyu 吳語 in the primary sources, there is no mention of a Chuyu 楚語; indeed, when this phrase appears, it almost always indicates the “Discourse of Chu” in *Guoyu* 國語.

⁵ If one is curious about the definition of a Churen from this era, one might want to take a look at Fu Tao 伏滔 and Xi Zuochi’s 習鑿齒 debate about “Qing and Chu personages” 青楚人物論 (Yu Jiaxi, *Shishuo xinyu jianshu*, 1.72), which is not referenced in this book. For instance, Cheng Youzi 承幼子, likely the Langye native from the Eastern Han Cheng Gong 承宮, was considered a man of Qingzhou 青州, not a man of Chu. More pertinent is the case of Yan Yanzhi 顏延之 (384–456), also from Langye like the eminent Langye Wang clan 琅邪王氏 that the author identifies as Churen, self-identifies with Qingzhou and the Qi 齊 region in his “Family Biography Inscription” 家傳銘, in Ouyang Xun 歐陽詢 (557–641), comp., *Yiwen leiju* 藝文類聚 (Shanghai: Shanghai guji chubanshe, 1999), *juan* 55, p. 994.

and the statics of the geographical distribution of office-holding in the Southern Dynasties, implausible and unreliable. In fact, should one argue for designating southern Shandong as “Chu” by following the Greater Chu territory of the late Warring States period, then the Wu region itself was also part of the Greater Chu, and so one should not claim “Wu vernacular” or “Wu people” either.⁶

Eager to show that the idea of “Chinese” was out of the picture in this period, the author pays scant attention to truly ethnic connotations of such terms as *Hua ren* 華人 and *Hua min* 華民, and states erroneously that when *Hua ren* emerged as an ethnonym in the fifth and sixth centuries, “it indicated only the Zhongren who lived in the Central Plains” (p. 59; see also p. 31).⁷ *Hua* 華 is translated as “civilized” to diminish its ethnic overtones (e.g., pp. 61, 135, 137); this itself is debatable, but the strategy does not work when *hua* is used to describe language: the terms *Hua yan* 華言 and *Han yu* 漢語 are used to indicate the Chinese language in contradistinction from foreign languages such as Sanskrit or Xianbei 鮮卑 in a variety of northern and southern sources.⁸

The turn away from available evidence to chase a shadowy proto-ethnic identity and “a locally distinctive elite political culture” (p. 330) leads to some

⁶ See *Shi ji* 史記 (Beijing: Zhonghua shuju, 1959), *juan* 129, p. 3267, for considering Wu as “Eastern Chu.”

⁷ See, for instance, Xie Lingyun 謝靈運 (385–433), “Da Faxu wen” 答法勸問, *Quan Song wen* 全宋文, *juan* 32, in Yan Kejun 嚴可均, comp. *Quan shanggu sandai Qin Han sanguo liuchao wen* 全上古三代秦漢三國六朝文 (Beijing: Zhonghua shuju, 1965), p. 2612; *Zhou shu* 周書 uses *Hua min* 華民 to indicate the southern Chinese people in contradistinction against the Lao 獠 people, whom they define as “a species of the Man peoples” 南蠻之別種 (*Zhou shu* [Beijing: Zhonghua shuju, 1971], *juan* 49, pp. 890–91).

⁸ For instance, Ming Sengshao 明僧紹, “Zheng erjiao lun” 正二教論, *Quan Qi wen* 全齊文, *juan* 14, in Yan Kejun, *Quan shanggu sandai Qin Han sanguo liuchao wen*, p. 2868; Gu Huan 顧歡, “Yi Xia lun” 夷夏論, *Quan Qi wen*, *juan* 22, p. 2914; *Wei shu* 魏書 (Beijing: Zhonghua shuju, 1974), *juan* 101, p. 2234; *juan* 114, p. 3026; Liu Xie 劉勰, “Miehuo lun” 滅惑論, *Quan Liang wen* 全梁文, *juan* 60, in Yan Kejun, *Quan shanggu sandai Qin Han sanguo liuchao wen*, pp. 3307–9. Shi Huijiao 釋慧皎, *Gaoseng zhuan* 高僧傳 (Beijing: Zhonghua shuju, 1992), *juan* 13, p. 507; *Nan Qi shu* 南齊書 (Beijing: Zhonghua shuju, 1972), *juan* 59, p. 1023. The author also adds regional colouring to phrases that do not have any regional connotation. For example, the term *chula* 楚刺, describing Song Emperor Wu, means “crude and contrary,” but is rendered as “of Chu, and perverse” (p. 95). The binome *ziju* 趙睢, describing “fish-eating” Wuren, is translated as “overweening fish hawks” (p. 55), but *ziju* is alternatively written as 趙趙 or 趙趣, meaning “insolent and fierce,” not to mention *ju* 睢 hardly means “fish hawk” without *jiu* 鳩.

untenable claims in Section 2, “Political Culture.” Chapter 5, “Marking Territory,” argues that the Jiankang Empire only sought to defend its territory but never tried to reconquer the north despite their rhetoric, which the author promises to get to in Section 3 but never really does, with only two cases of “possible exception,” namely Song Emperor Wu 宋武帝 (r. 420–422) and Liang Emperor Wu 梁武帝 (r. 502–549) (pp. 127–28). No mention is made of the northern campaigns undertaken or planned under Song Emperor Wen 宋文帝 (r. 424–453), Song Emperor Ming 宋明帝 (r. 465–472), Qi Emperor Wu 齊武帝 (r. 483–493), or even during the much diminished, weakened southern regime under Chen Emperor Xuan 陳宣帝 (r. 569–582). Ironically, the exaggerated description of the south’s defence policy perfectly fits the stereotypical image of the southerners being cultural but weak compared to the macho north—an image that the author does not want to endorse.

The same binary scheme of conception is also seen in the dichotomy of garrison culture and court culture—*wu* 武 and *wen* 文—in Chapter 6, “Making Hierarchy.” Because of the considerable fluidity between military headquarters and court/capital as well as the convergence of cultural and political forces in events and personages, one questions if such a dichotomy is the best framework for conceptualizing the history of this period. The army was, of course, always important, but the role of court officials should not be underrated, and that of military commanders should not be generalized as a bunch of uneducated men, either.

Past scholarship has had a tendency to underestimate the elite status of Song, Qi, and Liang ruling families or the continuing influence of the elite clans in the Southern Dynasties; this tendency is unfortunately exacerbated by the author’s classification of all of the Song, Qi, and Liang ruling families as “Churen” who belonged to a “Churen jituan” 楚人集團, based on Chen Yinke’s 陳寅恪 term of “Chuzi jituan” 楚子集團, but with a number of new claims added: the author asserts that these people all spoke a common vernacular language that was “relatively close to the Wu language” (p. 171), used “shared cultural signifiers” (p. 149; what they were is not explained as far as this reviewer can see), were “largely uneducated in Sinitic literary traditions,” and “indeed, . . . considerably less well educated than the Wuren elite” (p. 171). These claims are highly doubtful because there is no evidence, and the last declaration has plenty of evidence to the contrary, for these families include, by the author’s account, the Langye Wang clan, the Lanling Xiao clan 蘭陵蕭氏, and the Pengcheng Liu clan 彭城劉氏. At one point the author comments that none of the earlier scholars adopting the *jituan* approach would “contemplate the Churen or any other *jituan* as an ethnographic category,

nor consider how their distinctive vernacular language and culture affected their group cohesion” (p. 149, n. 34)—one wonders why indeed! And no scholar holding the *jituan* approach ever denies the Langye Wang or the Lanling Xiao as “River-crossing” northern immigrants, either.

In the introduction to Section 2, the author states that his objective is to prove that the Jiankang Empire did *not*, as “historians expect,” “exhibit a typical Chinese political culture” (p. 107). One question is whether there was indeed such a thing as “a typical Chinese political culture.” Chittick lists its six core elements—such as the ideal of “unification,” the dominance of civilian virtues over military ones, and general hostility to commerce and trade—and criticizes a tendency in scholars to reflexively use them “as a yardstick for how political systems *ought* to function” and hence to judge the Jiankang regimes, measured this way, as a failure (p. 108). This is indeed an important criticism to make, but it seems to be only partially true: how often do we see these southern regimes being accused of allowing military virtues to dominate their civilian virtues, or of endorsing commerce and trade on a discursive level? Since the author does not cite any scholarly works here to support his statement, it is difficult to assess the evidence for this criticism. In any case, when we leave behind these abstract, and thus ahistorical, elements of the “Chinese ideal” (p. 108) and begin to examine the specifics that constitute “a typical Chinese political culture,” certain assumptions do not appear to be well tested. For instance, to demonstrate the importance of garrison men, the author points to the problem of imperial succession in the Southern Dynasties, arguing that it deviated from “a ‘traditional’ dynastic system as modeled by the Han Empire” (p. 163). And yet, in the dynastic succession in the Han, especially in the Eastern Han, primogeniture was never a political reality. The “*typical* Chinese political culture” (emphasis added), just like the “Sinitic norms” (p. 264), seems to be largely an imaginary construct of an essentialized, monolithic, and ahistorical nature.

The problem with evidence—from slim to none, or from suppression/partial presentation to misinterpretation/mistranslation—plagues the remaining chapters in the book. Chapter 7, “Managing Prosperity,” in an effort to showcase the southern empire’s commerce and cash economy, states, “All commercial taxes and remittances would have been paid in coin, . . . Government expenditures were all calculated in coin, and in the early sixth century the Liang regime implemented a policy of paying all official salaries in coin” (p. 185). This statement contradicts both primary and secondary sources: the *Sui shu* 隋書 says that in the early sixth century only certain prefectures used coin in commerce while the others used a mixture of coin, grain, and cloths, and the Jiaozhou and Guangzhou regions dealt only in gold and silver, and that in Chen dynasty, coin, grain, and cloth were

in mixed use in commerce;⁹ the secondary source on which this chapter heavily relies says that Liang official salaries still consisted of three parts, one of which was rice and cloth.¹⁰ In discussing maritime trade with South Seas states, the chapter has a rather romanticized representation of the Jiankang Empire's diplomatic and trading system, which the author claims emphasized "nominal equality" (p. 202), and mentions the memorials sent to the Jiankang throne as "evidence" (p. 203). Unfortunately, the "evidence" is not as "clear" as the author declares if one reads through those memorials closely.¹¹ It is not true that the term *huangdi* 皇帝 is only used in two memorials as the author claims (p. 202), but not that it matters, for the memorials liberally use terms such as "Son of Heaven" (*tianzi* 天子) and "Sagely Lord" (*shengzhu* 聖主) and are filled with expressions of subservience to the great Chinese king who presides over a Buddhist paradise (*wang* 王, "king," does not imply any depreciation in this context, as the author implies).¹²

Chapter 8, "Vernacular Repertoire," largely reduces Jiankang Empire's vernacular culture to Wu vernacular culture alone, with no regard for Jing Chu 荆楚, Ba Shu 巴蜀, or the far south. It argues that local gods, such as Jiang Ziwen 蔣子文 and Wu Zixu 伍子胥, had more influence in the Jiankang Empire than

⁹ *Sui shu* (Beijing: Zhonghua shuju, 1973), *juan* 24, pp. 689–90. The quotation from *Sui shu* on p. 185 also mistakenly stops mid-sentence and mistranslates the original text, which says, "Because people here [in the south] all vied to engage in commerce but not in agriculture, the state made them pay equal [percentage in commercial tax] as a way of punishment [of doing commerce] and encouragement [of doing agriculture]" 以此人競商販，不為田業，故使均輸，欲為懲勵。The author ends his quotation at "not in agriculture" and drops the last half of this sentence, and erroneously takes the original text to say that "due to" the fact that the state levied commercial tax, people all vied to engage in commerce—which does not even make sense unless one wants to think the *Sui shu* historian is implying that the tax rate was too low. This is merely one example of the many mistranslations in this book. Another example from this chapter is to translate *yihui yingji* 異賄盈積 as "ample bribes" (p. 199), without realizing that *hui* here simply means material possessions.

¹⁰ Chen Mingguang 陳明光, *Liuchao caizheng shi* 六朝財政史 (Beijing: Zhongguo caizheng jingji chubanshe, 1996), pp. 218–19.

¹¹ *Song shu*, *juan* 97, pp. 2380–86; *Nan Qi shu*, *juan* 58, pp. 1015–16.

¹² For the two memorials that use "emperor," Chittick references one from Heluotuo 訶羅陀 and one from Funan (p. 202). The ruler of the Kingdom of Lions (Sri Lanka) also uses the term "emperor." *Song shu*, *juan* 97, p. 2384. Of course, one should also consider the fact that these letters were translated into Chinese, and that any "nominal equality" that might have been present in the original letters would have been completely obliterated in the process of translation before they reached the throne through the proper venues and were recorded in the dynastic history.

deities rooted in the “Sinitic paideia.” It states that Jiang Ziwen, the god of Mount Zhong 鐘山 in Jiankang, was “a tutelary deity of the empire” for “much” of the four centuries, and that he was “routinely honored, worshipped, appealed to, and regarded as a powerful local protective deity by all of the long-serving ‘good’ emperors just as much as the short-lived ‘bad’ ones” (pp. 224, 231). This is not entirely accurate. Song Emperor Wu banned the worship of miscellaneous gods including Jiang Ziwen, and his son, Emperor Wen, certainly a “long-serving ‘good’ emperor,” continued the policy; in fact, before the last southern regime of Chen, all *fervent* royal worshippers of Jiang Ziwen are problematic (to put it mildly) figures, including the patricide Liu Shao 劉劭 and the notorious Marquis Donghun 東昏侯, the only Qi emperor who is recorded to worship Jiang Ziwen. The one known instance of interaction between Liang Emperor Wu and Jiang Ziwen is phrased in the emperor’s threat to burn down Jiang’s temple if the god did not send rain (who then did).¹³ Yet, only the second part of this incident is discussed by the author, minus any mention of the emperor’s threat (p. 229). The point is that, yes, emperors, governors, and magistrates often paid homage to a local mountain god and did not always tear down a local temple to discourage “excessive worship” (*yinsi* 淫祀), but we want to be careful not to overstate one incident or one facet of an incident with no regard for the *totality* and *nuances* of the primary sources.

This chapter does not discuss the worship of the Great Yu 禹 or Jizha 季札 in the south, presumably because they are too closely related to the so-called Sinitic paideia. It also forfeits an opportunity to discuss the interesting case of Chen Baxian 陳霸先 (Chen Emperor Wu 陳武帝, r. 557–559), the only dynastic founder in the Southern Dynasties who worshipped Jiang Ziwen in an ostentatious manner;¹⁴ or to compare the case of Marquis Jiang 蔣侯 (as he was called) with another local god, Marquis Su 蘇侯, an Eastern Jin rebel general of a northern descent who was worshipped along with Marquis Jiang as a pair of protective deities by the patricide Liu Shao (and by Marquis Donghun’s generals), as well as by Song Emperor Ming.¹⁵ The discussion of the southern vernacular culture would have benefitted from an analysis of the richly fraught relationship between northern émigré elite and the southerners in the case of Jiang Ziwen and his “third younger sister.”¹⁶ The chapter

¹³ *Nan shi* 南史 (Beijing: Zhonghua shuju, 1975), *juan* 55, p. 1356.

¹⁴ *Chen shu* 陳書 (Beijing: Zhonghua shuju, 1972), *juan* 2, pp. 33, 39.

¹⁵ *Song shu*, *juan* 99, p. 2433; *Nan shi*, *juan* 32, p. 828; *Song shu*, *juan* 72, p. 1873.

¹⁶ The author claims the Third Younger Sister was “apparently worshipped as a fertility goddess” because her temple was “painted with images of lush grain and a bird giving birth” (p. 230). The “grain” was actually a large paper mulberry tree (*gu shu* 穀樹 or *gupi shu* 穀皮樹), growing on the temple ground, with a bird’s nest on it.

would also have benefitted from an examination of the Wu songs that were an important element of Wu vernacular culture, or that of the *zhiguai* 志怪 material from this period that, unlike dynastic histories, proffers us precious records of much of the “lived experience of ordinary people” of this era.

Chapter 9, “The Sinitic Repertoire,” is built on a flawed premise, namely, that the Wu vernacular tradition viewed the Eastern Jin as a continuation of the Kingdom of Wu from the Three Kingdoms period (p. 255). This assumption is based on a distorted interpretation of primary sources cited in Chapter 8 (p. 222). The *Song shu* / *Jin shu* historians record that, in the early fourth century, popular ditties predicting Wu’s rise were mistakenly regarded by Wuren as an omen of a Sun Wu 孫吳 descendant’s ascending to the throne in the south; the historians’ implication is clear: the omen was about the Sima Jin 司馬晉 regime’s rise in Wu, not literally about a Sun Wu revival.¹⁷ The author’s contention that the Sinitic repertoire conflicts with the political culture of the Jiankang regime thus has no basis. In fact, the author should have considered why Chen Min 陳敏, a native of Wu who rebelled in the early fourth century and claimed the re-founding of the Sun Wu Kingdom, failed in his undertaking: he failed because he did not have the support of the powerful Wu elite clans, who embraced none of the many local uprisings at this time of chaos and opportunities; instead, they threw in their lot with the Sima Jin dynasty, which to them represented the orthodox lineage from Han to Wei to Jin, rooted in the Chinese cultural tradition. Indeed, the Sun Wu Kingdom itself never considered themselves as a foreign “Other” to the Chinese tradition of the Central Plains: they saw themselves as a fully legitimate participant in that tradition,¹⁸ perhaps even more legitimate than others because the founding father of the old Wu kingdom, Taibo, was Zhou Taiwang’s rightful heir, who deferred to his little brother out of filial piety, brotherly love, and modesty—all virtues lauded in the so-called Sinitic paideia. In this chapter the author also asserts that “Sinitic ritual and literary culture offered relatively little appeal to men of garrison culture, who continued to have considerable influence over the selection of the sovereign and the conduct of affairs” (p. 245). This statement is largely unsubstantiated.

¹⁷ The text reads: 于時吳人皆謂在孫氏子孫，故竊發亂者相繼 (*Song shu*, *juan* 31, p. 914). The author’s translation: “At that time the Wuren all said that the descendants of the Sun house would succeed one another to secretly cause disaster” (p. 222). The correct translation: “At the time, Wuren all mistakenly thought that [the ditties] pointed to the descendants of the Sun house, and so one followed another to secretly start havoc.”

¹⁸ See discussion in Xiaofei Tian, “Remaking History: The Shu and Wu Perspectives in the Three Kingdoms Period,” *Journal of the American Oriental Society* 136.4 (2016): 705–31.

Chapters 10 and 11 in particular contain some glaring examples of misconstrued evidence and doubtful claims. Chapter 10 attempts to construct an alternative history of the Jiankang state—a Buddhist history—by tracing the lineage of the state to King Asoka (r. 268–232 B.C.E.). The author believes that a legend about the “rediscovery” of a Buddhist statue made by Asoka’s fourth daughter at Jiankang’s Changgan 長干 Temple “was developed no earlier than the middle to late fifth century” (p. 290), and a group of Qi royals and monks, including the Crown Prince Xiao Zhangmao 蕭長懋 (458–493),¹⁹ the prince Xiao Ziliang 蕭子良 (460–494), Monk Sengyou 僧佑 (445–518), and so forth, “was instrumental in promoting, if not wholly creating” (p. 290), this legend. The reason for their promotion (and possible forgery, as hinted at by the author) is given in an ingeniously spun narrative: the author states that King Asoka “was supposed to have ruled East Asia” “during the era of Zhou King Jing (r. 519–477 B.C.E.),” which the author relates to the time of the Wu kings Helu 闔廬 (r. 506–496 B.C.E.) and Fuchai 夫差 (r. 496–473 B.C.E.) and Yue king Goujian 越王句踐 (r. 496–465 B.C.E.); therefore, “According to Buddhist historiography, it was the emperor Asoka, not the Zhou king, who had ultimate dominion over their [Wu and Yue kings] courts, having invested them as kings in a *fengjian*-type system, as Huijiao’s telling [in *Gaoseng zhuan*] implies” (p. 291).

This remarkable story, repeated as a fact on p. 314, has a few problems: first, there is, in several *Gaoseng zhuan* editions, a textual variant for Zhou King Jing, which is Zhou King Xuan 周宣王 (r. 827–782 B.C.E.);²⁰ second, Huijiao’s telling of the Buddhist statue story is not, as the author claims, “the earliest complete written version” of it—at least one known earlier record is in Zang Rongxu’s 臧榮緒 (d. 488) *Jin shu*;²¹ third, there is no evidence that Xiao Zhangmao, Xiao Ziliang, and their coteries had anything to do with the statue’s rediscovery; fourth, Huijiao’s

¹⁹ Misspelled as Xiao Changmao in this book. Xiao Zhangmao is also erroneously called “the Wenxuan heir” (p. 310, n. 70)—Xiao Zhangmao’s posthumous title was “Crown Prince Wenhui” 文惠太子—and Prince Wenxuan of Jingling 竟陵文宣王 (Xiao Ziliang) is taken to refer to two different princes, the Wenxuan 文宣 and Jingling 竟陵 princes (p. 300, n. 18).

²⁰ Shi Huijiao, *Gaoseng zhuan*, *juan* 13, p. 479.

²¹ *Taiping yulan* 太平御覽 cites from Zang’s *Jin shu* 晉書, *Taiping yulan* (Taipei: Shangwu yinshuguan, 1975), *juan* 657, p. 3066. The author’s only reason for dating the story to the middle to late fifth century is that Zong Bing 宗炳 (375–443) did not mention it in his treatise. But Zong Bing is speaking of a completely different topic—namely, Buddha’s bone relics in a temple called the Asoka Temple—to prove that Buddhist practice had long existed in the northern region of Qi and Jin.

telling of the story implies nothing about Asoka's investment of the Wu and Yue kings in a *fengjian*-type system.²²

In short, eagerness to prove that Jiankang Empire was not a "Chinese" regime but resembles a Southeast Asian state (about which we know very little and largely through Chinese sources in the early period) led the author to downplay, simplify, and essentialize traditional Chinese culture, political statecraft, rituals, and values. The author also tends to exaggerate the divide between Buddhism and Sinitic as irreconcilable ruling strategies, rejecting the possibility that an emperor could wear a different hat (so to speak) on different occasions (pp. 210, 212), even though that was exactly what Liang Emperor Wu did (and he did it as a response to the Northern Wei's fervent Buddhist worship as well). For instance, an entry from the ninth-century work *Youyang zazu* 酉陽雜俎 is cited to show how Liang Emperor Wu used Buddhist protocols to receive a northern emissary, Lu Cao 陸操 (p. 278), but the author neglects to mention that the entries from the same work describe Emperor Wu using conventional Chinese ritual protocols to receive emissaries (one being the same northern emissary Lu Cao) on two other occasions.²³ These entries show that Emperor Wu was indeed "switch[ing] modes" (p. 210), and which protocols he would follow largely depended on the occasion. The author is, however, so intent on demonstrating "a sharp shift to a Buddhist construction of state legitimacy in the early sixth century" (p. 212) that he greatly exaggerates Emperor Wu's Buddhist project but entirely ignores the emperor's projects on *Ru* 儒 rituals and classical scholarship.²⁴

Perhaps to circumvent such a critique, at the beginning of Chapter 9, "The Sinitic Repertoire," the author states that "The Sinitic repertoire for legitimation hardly needs an introduction" (p. 244). However, considering his rhetoric about the "invisibility" of the Jiankang empire, especially to scholars working on later periods or outside Chinese studies with whom he hopes to have a dialogue through this book, selective presentation of evidence can be quite misleading. Since so many scholars have written about Liang Emperor Wu's Buddhism-inflected political vision, it also seems unfair to say that "Sinitic universalism is assumed to have been the only way the elites of the Jiankang Empire ever conceptualized their identity and legitimacy as a state" (p. 244).

²² Shi Huijiao, *Gaoseng zhuan*, *juan* 13, p. 478 and p. 495. The author's translation of the *juan* 13, p. 495 passage is problematic (p. 291).

²³ Duan Chengshi 段成式, *Youyang zazu* (Beijing: Zhonghua shuju, 1981), *juan* 1, p. 7.

²⁴ For Emperor Wu's ritual programme, see, for instance, Liang Mancang 梁滿倉, *Wei Jin Nanbeichao wuli zhidu kaolun* 魏晉南北朝五禮制度考論 (Beijing: Shehui kexue wenxian chubanshe, 2009).

The tendency to distort historical record in order to prove a point culminates in the assertion, in Chapter 11, that the Buddhist statue made by Asoka's Fourth Daughter became, in the author's account, a "tutelary deity" for the empire. He cites Daoxuan's 道宣 (596–667) *Gantong lu* 感通錄 and concludes the icon "clearly is understood to be representative of the Jiankang throne itself," so much so that after Sui conquered Chen, it was brought to Chang'an, and "was installed facing north to symbolize its subservience, but every night it would rotate to face south, . . . thereby demanding proper respect from its conquerors. The statue in this way mutely lodged the Jiankang Empire's final claim to legitimacy" (p. 319). This is a moving account, but the author neglects to mention two things from Daoxuan's account: (1) after the statue was brought to Chang'an, Sui Emperor Wen 隋文帝 (r. 541–604) himself would stand in attendance on it and worship it; (2) when the statue was first placed in the temple, the monks found the hall too big to set the statue up in the south-facing direction, and that was why they left it in a north-facing direction until the statue righted itself twice and the monks apologized for their carelessness—so it was a very mundane logistical issue, meaning no disrespect. By leaving these details out, the author tells a touching story, but it is, alas, untrue.

* * *

The Jiankang Empire in Chinese and World History attempts to change some fundamental paradigms of studying the Southern Dynasties and "offers a sweeping re-assessment" (book jacket blurb), and so it is worthwhile to see whether the new framework and terminology proposed in their stead can pass fact-checking. The answer is largely no. A recurrent saying in the book is that "there is good reason to believe" (e.g., pp. 277, 313, 316). "Good reason" is never enough: we also need good evidence. Before "believing," we must read primary sources, widely and deeply, and to be conscientious in presenting *all* of the evidence, not just the part that serves one's argument based on a belief.

In recent years, there is much advocacy of "distant reading" in both historical and literary scholarship. There is an emphasis on mastering secondary sources at the expense of mastering primary sources, and in dealing with primary sources, some historians segregate sources and areas by a rigid sense of a modern disciplinary divide. The lesson we should learn from the failings of this book is that distant reading cannot replace close reading, that secondary sources cannot replace primary sources, and that no matter how attractive a conceptual framework sounds, it needs evidence. It takes patient and careful work to unpack a historical period, and it takes more than simply an interesting idea to successfully create a new paradigm.

It is worthwhile to state, for the record, that to study premodern Chinese history and culture and a dynasty's imperialistic ambitions is *not* the same as

celebrating or abetting modern nationalistic discourse as the author seems to imply. It is also worthwhile to reiterate that Chinese culture was constantly changing and being renegotiated throughout history. In fact, Chittick puts it very well when he states that the Sinitic repertoire “was not monolithic; it contained numerous alternatives that led to conflict, contestation, and change over time” (p. 244). The “Chinese” ethnic and cultural identity was, and is, a long process of becoming, and ethnic consciousness is always born out of the presence of, and pressure from, an Other, and even just for that reason alone, the mention of the brutally slaughtered, enslaved, and suppressed Man peoples in a book on “the Jiankang Empire” would have been crucial because the “Man barbarians” or the “Hu barbarians” are all negative indices of an ethnic awareness and an implicit affirmation of ethnic solidarity. Early medieval Chinese history is a fertile ground for studying these issues, and it is ultimately a good thing that we have a general historical study of the Southern Dynasties in the English language touching on many timely issues and concerns. It is also a good thing that we are able to put some of the problems on the table for an open discussion. It is my hope that there will be successors to this book that are inspired by it.

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