

Mikael S. ADOLPHSON, *The Gates of Power: Monks, Courtiers, and Warriors in Premodern Japan*. Honolulu: University of Hawai'i Press, 2000. xvii + 456 pp. \$29.95 paper, ISBN 0-8248-2334-6.

MIKAEL S. ADOLPHSON has produced the most in-depth non-Japanese study

and interpretation to date of Kuroda Toshio's *kenmon taisei* (system of ruling blocs, or system of "gates of power" as the author translates it). Kuroda's theory, one of the most influential interpretations of medieval Japanese history of the last half-century, claims that the religious institutions between the tenth and fifteenth centuries constituted as a whole a power bloc comparable to those of aristocrats (*kizoku*) and samurai warriors (*bushi*), typified respectively by the members of the imperial court and the bakufu military government. Adolphson attempts to take from Kuroda's theory what he sees as its most penetrating insights while at the same time offering a corrective to its tendencies to oversimplify the character and dynamics of power relations within and between the *kenmon*.

He stresses that there is a tendency to see warrior preeminence as having been achieved with the Kamakura era, but due to recent research "the age of the warrior has been pushed forward to the fourteenth century, when the second warrior government (the Muromachi or Ashikaga Bakufu) assumed all the responsibilities and qualities of a national government and warrior culture truly began to dominate Japanese society" (1). Most striking initially is Adolphson's use of narrative. He presents us with an engaging narrative in an effort to establish what he calls "a comprehensive theory that can advance general understanding as well as generate new questions regarding the era that has been known as the late ancient and the early medieval in Japan" (20).

And what a narrative it is. This work, while generally buttressed with ample citations, reads ironically like recent Western postmodernist-historians' renderings of subjects such as nineteenth-century American history, studies in which citations are avoided to stress the inherently interpretive character of historical understanding. The author, however, is trying to show us that there was, indeed, a "there" back there, and that religious institutions mattered. Moreover, the sources necessary for ample coverage of the religious institutions of the tenth to fifteenth centuries (let alone those required to connect those institutions with the larger history of society in the Japanese islands) are voluminous in scope and number. So Adolphson's book is, without question, an ambitious effort, and required several years of intensive research.

His immediate focus is on the "worldly aspect of religion" (2), which he sees as quintessentially demonstrated in divine appeals (*gōso*), mobilizations (*hōki*) and battles (*kasen*) of religious institutions that he identifies as *kenmon*: the Tendai complex Enryaku-ji near Heian-kyō (Kyōto), the Hossō complex Kōfuku-ji in Nara, and the Shingon complex Kōyasan in the mountains of the Kii Peninsula outside Nara. In the introductory chapter, Adolphson outlines the basic points of Kuroda's *kenmon* theory, making note of what he sees as one of its basic limitations—the assumption that "the religious establishment had the same kind of power as the court or the bakufu" (17). He points out the corrective of this limitation made by the inheritor of Kuroda's theoretical mantle, Taira Masayuki, who has stressed that the temples never held ultimate authority at the level commonly achieved by the warrior government or the imperial court (16; see TAIRA 1992, pp. 94–97).

The narrative plot of the story moves forward rapidly with Chapter 2, where he describes the early development of the Enryaku-ji, Kōfuku-ji, and Kōyasan complexes. Politics and religion were interrelated from the very beginning of these institutions; thus, developments such as the increase in the nobles resident in the complexes should not be interpreted in a “deprecatory” manner as having simply indicating monastic corruption, since “religious and political developments were interdependent from the very outset” (70).

Chapters 3 to 5, which form the central body of the study, are concerned with “the secular and religious roles of Kōyasan, Enryakuji, and Kōfukuji... from the late eleventh to the late fourteenth centuries” (74). The first of these chapters outlines the era of the retired emperor Shirakawa, emphasizing that the “imperial revival” attempted by Shirakawa was indicated primarily by his increased control over large numbers of manors (*shōen*) (81), but stresses that one important pattern for this increase was Shirakawa’s “use of religious institutions and head abbots to further the interests of the imperial family” (82). Moreover, Shirakawa also combined donations and pilgrimages to forge and renew connections with powerful religious institutions such as Enryaku-ji and Kōyasan; new temples of Shirakawa’s sponsorship were, indeed, not simply means to gain control of manors but also institutions where the retired emperor tried to establish novel religious rites (83–85), and even the innovation of appointing princes as the head abbot (*hosshinnō*) of Ninna-ji (86–87).

Chapter 4 examines the era of Go-Shirakawa, marked by the warriors’ rise to the apex of Japanese politics, rural samurai’s use of military means to increase their power locally and “intrafamilial competition for leadership and supremacy within the great blocs of power... which became the immediate cause for several violent incidents late in the twelfth century” (125–26). Adolphson offers a close narration of the events and motivations surrounding the mid-twelfth-century imperial family, Taira no Kiyomori, and Minamoto no Yoritomo. Adolphson notes that disputes between the capital elites and powerful temples in rural areas, such as Kōyasan, could often work to the latter’s advantage (e.g., the Ategawa example, 173–75). He stresses, however, that the methods by which the imperial, courtier, and warrior “elites” attempted to control “elite temples” in the capital region were marked uniformly by the effort to control the abbacy through kinship ties or other forms of alliance with powerful monks—albeit without much success in the case of Enryaku-ji—indicating that the establishment of “bakufu” military rule meant merely “the institutionalization of the warrior elite within the *kenmon* state” (183–84).

In Chapter 5, Adolphson turns from the twelfth century and the capital region to the late thirteenth century and the military government’s dominance of eastern Japan. Although Enryaku-ji and Kōfuku-ji were threatened by the activities of Shirakawa and Go-Shirakawa, increasing dominance by the bakufu in the thirteenth century meant that they and other temple elites maintained most of their political clout. Meanwhile, Kōyasan, although

plagued by internecine rivalries, consolidated its control over large areas of the Kii Peninsula. The bakufu, alleged by previous historians to have gained control nationally, rarely intervened in the areas of traditional Heian-kyō authority, attempting to maintain “shared rulership” with the old elite, as witnessed by its granting of control of Yamato province and Kōfuku-ji (239).

The final two chapters respectively explore the history of *gōso* and what Adolphson sees as the “collapsing” of the *kenmon* system with the rise of the Ashikaga bakufu. Adolphson offers a convincing correction to traditional interpretations of the demonstrations as “nothing short of violent attacks” attempting to undercut government authority (240) and the prototypical example of the “evil influence” of institutional Buddhism (286), focusing especially on those of Enryaku-ji and Kōfuku-ji. He argues that actions such as the leaving of *sakaki* branches achieved the goal of resolving conflicts because of belief in the power of sacred beings rather than an imminent threat of violent behavior (265–66).

The collapse of the *kenmon* system, for Adolphson, is equivalent with the consolidation of “warrior dominance” of Japan—the rise of the Ashikaga bakufu in the fourteenth century. It was especially the shogun Yoshimitsu (1358–1408) who gave the “final blow to cooperative rulership” (333), managing to install constables in Enryaku-ji who operated like provincial military governors, possessing even the right to levy special taxes (336–37); likewise, sponsoring the reconstruction of Kasuga Shrine, Yoshimitsu took charge personally, replacing the position of the Fujiwara and even increasing his own control of Yamato province (338–39). Shared rulership was thus replaced by “the beginning of a new rulership and an entirely new era one might call Japan’s medieval age” (288). Adolphson concludes his discussion by emphasizing what he sees as the character of the collapse: contrary to Kuroda, who claimed that the *kenmon* system continued until the late fifteenth century, the fact that the Ashikaga “not only favored but also controlled Zen ... [and] Enryakuji and Kōfukuji were simply excluded from the Ashikaga polity” (344) meant that the system had indeed come to an end.

Or had it? These conclusions would seem to beg further analysis. The author admits within lines afterward that while the bakufu was able to control Enryaku-ji and Kōfuku-ji, “the Ashikaga hegemony did not last long enough to extinguish the power of the old elites.... [I]n the middle of the fifteenth century, the old sects, especially Enryakuji and Kōyasan, resurfaced as independent and powerful military centers” (344–45). The author wants to correct Kuroda’s insistence that the *kenmon* system continued until the end of the fifteenth century, but ends up telling us that within a few decades of Yoshimitsu’s death Enryaku-ji and Kōyasan reappeared as major independent centers of power. Admittedly, this is a very difficult problem to sort out, but what is the precise difference between an “independent” military center and a *kenmon* competing and/or cooperating with other gates of power? It is thought-provoking when we note that the author has invoked the scholarship of a single article by Nagahara Keiji from 1964 as the initial basis for his criticism of Kuroda’s view that the system continued under the Muromachi

bakufu (291),<sup>1</sup> while failing to take account of the recent impact of the work of Harada Masatoshi and others on the study of the relationship between Zen and the Muromachi shogunate. HARADA (1998) emphasized that not only did the *kenmitsu* schools continue to perform imperial rituals as before during the Muromachi era but that such rituals were also important for the bakufu. Indeed, while the early Muromachi bakufu showed favor toward the *gozan* temples, it continued to patronize *kenmitsu* practice alongside Zen; even in the case of Yoshimitsu, *kenmitsu* practice dominated his funeral ceremonies—illustrated, I might add, by the relic rites<sup>2</sup> performed—although, in the case of Yoshimochi (1428), *kenmitsu* monks only listened while Zen monks performed rites that had originally been their purview. Furthermore, the bakufu attempted to take over aristocratic patronage of *kenmitsu* while pairing it with Zen to its advantage; and Zen, while patronized by the bakufu, became a *jisha seiryoku* (temple-shrine power complex) that stood alongside the traditional *kenmitsu* schools, assimilating practices like the feeding of hungry ghosts and the *nenbutsu* in an appropriation of *kenmitsu* and other beliefs and practices (HARADA 1998, pp. 335, 354–58, 367, 190).

Finally, I would just like to make note of a few other issues raised by the author's method and argument. First, as one Japanese scholar of medieval Japanese religion said to me in this regard, on what basis could anyone reduce the number of complexes treated to three? And, I would add, while Enryaku-ji and Kōfuku-ji are justifiable choices as objects of study, why select Kōyasan? Two other candidates come to mind, and their absence as major topics for consideration would seem to call for an explanation. Tōdai-ji, for example, is a major complex of Nara that has left us a large record of its manors and activities, and a number of extensive analyses of its history and organization have been written in Japan; indeed, figures such as the abbot Kakunin (fl. 1132–1157) were controversial historically—the latter has been the object of academic study (HISANO 1999, 123–74)—and the complex connection between Tōdai-ji and Shingon is well known. At the same time, if the author wanted to account for specifically Shingon complexes, why choose Kōyasan rather than Tō-ji, which he himself describes as “the head of Shingon” (281) and “the center of the sect” (200)?

Scholars specializing in Shingon history know that statements to the effect that Tō-ji was “expected to represent the interests of its distant sibling in the capital” (281) are extremely questionable; Tō-ji was, if anything, a kind of Vatican of Shingon, the apex of the Shingon hierarchy (Kōyasan was actually under its jurisdiction for many centuries) and it was certainly not expected to represent anything other than its own status (and wealth) within that hierarchy—its complicated relationship with the powerful imperial-prince-dominated

<sup>1</sup> Taira Masayuki recently described the work of Nagahara (and Ishimoda Shō) as possessing the view of history that Kuroda's work overcame—an old argument that the manor lords (including the temple-shrine complexes) and the imperial court were “ancient” throughout the so-called “medieval” era, up until the fourteenth century or later (“Kenmitsu taisei ron ni suite no shiteki memo,” at “Nihon Bukkyō kenkyūkai: daijūkkai shinpojiumu,” 12.2.2001).

<sup>2</sup> See RUPPERT 2000, pp. 251–52, which is based on documents in Honchō bunshū.

Ninna-ji, Minamoto-dominated Daigo-ji as well as Kōyasan having yet to be adequately studied. Moreover, the abbot of Tō-ji, as the pioneering research of USHIYAMA (1990) demonstrated over a decade ago, was, with only one exception (938–955), from 872 on paired with one other Enryaku-ji, Onjō-ji or Kōfuku-ji monk in the *sōgō* office as the highest-ranking monk-administrator of the schools of Japanese Buddhism, the *hōmu*, and by the mid-twelfth century, monopolized control of the position, while the *sōgōsho* headquarters itself was relocated from Sai-ji to Tō-ji by that time (USHIYAMA 1990, 202–25). Moreover, the head administrator-monk of the Rokushō-ji imperial temple group was as a rule chosen from among the monks of Tō-ji, Enryaku-ji, Onjō-ji, and Kōfuku-ji.<sup>3</sup> Within the official clerical system, the Tō-ji abbot was thus arguably as powerful as any monk in Japan. Tō-ji, indeed, left large quantities of documents in *Tōji hyakugō monjo* concerning its extensive manorial holdings (especially from mid-Kamakura on) and imperially-sponsored relic and rain-making rites, and several volumes have been written by Japanese historians (e.g., Amino Yoshihiko) about its religious and economic history. How can one create a “comprehensive theory” of the *kenmon* without addressing the documents and research concerning Tō-ji? My only estimation is that Tō-ji did not suit the author’s effort because it was not the kind of *kenmon* that depended primarily on threats of violence to achieve its ends—thus difficult for the author to categorize within the framework of “incident”-driven history. The statement that “It is through the many appeals, demonstrations in the capital, and conflicts with other elites that the political power of Enryakuji, Kōfukuji, and Kōyasan is most visible in the sources” (8) reminds us that the narrative assumes power most commonly through expressing conflict, claims to an Annales school-style approach notwithstanding (19). Attention to Tō-ji would also, presumably, enable the author to address issues that KURODA (1992, pp. 20–21) and others have referred to as the “esotericization” (*mikkyōka*) of imperial rule—issues such as the esoteric *sokui* accession rite (*sokui kanjō*)—and of *kenmon* more generally. The other two issues, of which I will only make brief note, are (1) the author’s lack of clarification of his suggestion (1, 288) that the “medieval” did not begin until the apex of the Ashikaga regime (ca. late fourteenth to fifteenth century)—a view shared by virtually no Japanese historian in Japan, including Kuroda and Taira, who both argue for the tenth century—and (2) frequent errors in romanization such as in historical names (e.g., #7 on page 45 is actually Shōbō) and text titles (e.g., the genealogy reference on page 433 is, properly, *Sonpi bunmyaku*).

I make these comments because Adolphson’s book is, without question, one of the most thought-provoking non-Japanese works to be written on Japanese medieval history in years. It deserves to be taken seriously, particularly given its potential implications for the study of Japanese history. Moreover, the author’s goal from the start is, we might say, to give religion a chance—treat religious institutions, like Kuroda did, on a par with other or at

<sup>3</sup> In fact, the head of the entire system of Rokushō-ji was the prince-abbot of Ninna-ji. See TAIRA 1992, pp. 96–97. Adolphson refers briefly to this on page 87.

least as one of the major players during the development of warrior prominence—within the framework of the larger goal of explaining the development of warrior dominance. We historians of Japanese religion—we try to avoid the rubric of “religious studies,” for good reason—owe him a debt of gratitude. As a (so-called) mainstream historian, he chooses to take religious institutions of the Heian, Kamakura, Nanbokuchō, and Muromachi eras seriously, and thus places them where they arguably belong: within the larger narrative of Japanese history. No other non-Japanese work has attempted so broadly to cover the larger history of *kenmon* (nor, we should note, has any Japanese work as yet succeeded in such an effort), and demands that all historians of Japan take a new look at the place of religion as a “gate of power.”

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