
Reviewed by Vincent C. Pigott, Institute of Archaeology, University College, London

The excavation by Chinese archaeologists of naturally mummified Caucasoid individuals dating as early as the second millennium b.c. in the Tarim Basin in Xinjiang, China's westernmost province, are of unquestionable import in discussions of the movements of people across Eurasia in later prehistory. The cemeteries in which the Tarim mummies were found mark what is currently the easternmost presence of ancient Europoid peoples, representatives of the Eurasian steppe culture (see Barber 1999; Mair 1998; Mair and Mallory 2000). Metal artifacts in copper and its alloys stand among the most important possible archaeological markers of these wide-ranging movements. Jianjun Mei, in this publication of his doctoral thesis at the University of Cambridge, has opened the door on a wealth of hitherto uncirculated archaeological data both on the archaeology of Xinjiang province and on the coming of copper/bronze to this geographically and culturally pivotal region of desert and oases.

Mei seeks answers to four research questions: (1) when, where, and how copper and its alloys began to be used, (2) what metallurgical technologies were employed, (3) what the cultural context was for the beginning and early use of metals, and (4) what cultural connections and technological interaction existed between Xinjiang and its neighboring regions during the Bronze (c. 2000–1000 B.C.) and Iron Ages (c. 1000–300 B.C.). His overarching goal is to furnish an enhanced understanding of the archaeological and cultural contexts of late prehistoric Xinjiang, while at the same time offering a new perspective on how metallurgy spread into the province, and how this technology may have reached eastwards into the Chinese heartland.

In his introductory chapter Mei reviews both the background of the development of archaeology and the foci of pertinent prior research in Xinjiang. Crucial here is the role of external cultural influences in the development of settlement occupation. Moreover, it is clear from Mei's discussions that the current wave of archaeological research, much of it from Eurasia during the 1990s, has altered traditional thinking (even among the Chinese) about the development of Chinese civilization as an exclusively indigenous process, especially where metallurgy is concerned. This theme underpins discussion throughout the volume.

Mei divides the remainder of his publication into three major components. In the first component (Chapters 2 and 3) he reviews the archaeological evidence for at least fourteen Bronze Age cultures and a
similar number of Iron Age cultures from the various regions of Xinjiang (Chapter 2). He undertakes next a substantive typological investigation of six major categories of metal artifacts from these periods, e.g., implements, weapons, harness and chariot fittings, vessels, toilet articles, and ornaments (Chapter 3).

The second component (Chapters 4 and 5) consists of a review of previous analytical research on Xinjiang metal finds as well as an analytical program focused on metal samples supplied to him by local archaeologists. Chapter 4 presents the results from the battery of analyses performed to observe microstructure and determine composition of 58 metal samples with a goal of comparing the technologies of the various cultural groups he has identified. In Chapter 5 his investigation becomes site specific and focuses on the important finds from the mining and smelting site of Nulasai in Nileke which, on current evidence, appears to date to the first millennium B.C., but may well be earlier. This site, with its uncommon finds of mines and associated production debris, is one of but a handful of such documented sites currently known across Eurasia.

In his final component (Chapters 6 and 7), Mei turns to a synthetic overview of cultural interrelationships between Xinjiang and regions to the east, west, and north. He concludes with a discussion of the development of copper and bronze metallurgy in the region. Significant new archaeological data, much of it from Chinese sources, is presented in this volume, in particular that concerned with widespread contact between Xinjiang and neighboring regions. The initial occurrence of artifacts in copper (at Gumugou) and tin-bronze (at Tianshanbeilu) in Xinjiang takes place in the early centuries of the second millennium B.C.

Turning to the Iron Age, this period is marked by major changes including not only the coming of iron, but also the increasing use of gold and silver, and the practice of horse nomadism. However, as indicated by the unique evidence from Nulasi, copper mining and smelting continued unabated. Bronze artifacts, including socketed dagger-axes, and handled mirrors as well as items in precious metals, silk, and lacquer strongly suggest that Central Asia was linked by trade routes through Xinjiang with northwest China, i.e., the Gansu corridor. It is in Gansu that some of China’s earliest copper-base artifacts have been excavated (see Linduff et al. 2000).

In Mei’s detailed discussions of the typology of copper-base artifacts, there are repeated references to the similarities apparent between artifacts excavated in Xinjiang and those of known Andronovo type from neighboring Eurasian locales. These categories include shaft-hole axes, sickles, flanged adzes, and socketed celts. Through these artifact and site-specific discussions, Mei offers persuasive evidence for the impact of Eurasian steppe culture on Xinjiang. Nor can we ignore indications, though currently based on less substantive evidence, of contact between Xinjiang and bronze-using cultures to the east.

Mei, who is trained in metallurgy as well as archaeology, conducted his own laboratory analyses. They give us the first glimpse of the levels of sophistication attained in the metalworker’s craft as well as the multiple, alloying traditions being employed. Copper and tin-bronze artifacts are present in Xinjiang from the early second millennium B.C. while, interestingly, arsenical copper doesn’t seem to appear until the later centuries of this period. In the early first millennium B.C. artifacts in copper appear with more frequency due to what Mei suggests is the exploitation of local copper deposits near Urumchi. Thus, when compared to western Asia, copper and its alloys appear relatively late in Xinjiang and apparently not in the more time-honored sequence of copper, then arsenical copper followed by bronze as seen in the Near East, Central Asia, and Eurasia. The somewhat jumbled Xinjiang sequence, in Mei’s estimation, reflects the introduction of tin bronze and perhaps even arsenical copper artifacts and/or metallurgy from outside the region followed by attempts to produce metal locally. On the more technical side, Mei argues that the presence of sulfide inclusions in the microstructure of artifacts from the Tacheng region suggests that
copper sulfide ores were being smelted. He adopts the traditional ‘matte’ smelting model involving the roasting of sulfide ores prior to smelting to explain the production of the Nulasi ingots. In future research he might also consider the possibility of the co-smelting of sulfide and oxidic ores directly to copper in a one-step production process without roasting. Research by William Rostoker and colleagues (1989; Rostaker and Dvorak 1991) and that by Heather Lechtman and Sabine Klein (1999) has introduced co-smelting as a highly feasible alternative to the matte process and one which can yield arsenical copper.

When Mei turns his attention to east of Xinjiang, and to Bronze Age cultural contact with the Gansu-Qinghai region, he sees not only an influx of painted pottery into Xinjiang from this region, but also looks at the presently modest evidence for the spread of copper/bronze metallurgy into Gansu-Qinghai from external sources. One potential source is the possible interaction between the Machang (Gansu-Qinghai) and Afanasievo (southern Siberia) cultures in eastern Xinjiang. Arsenical copper appears late in both regions, but it is not clear if it has any direct links to Central Asian–Eurasian traditions.

Iron appears c. 1000 B.C. in Xinjiang, but the tradition of copper/bronze metallurgy continues with new forms being introduced during the Iron Age. It is from this period that the one major documented production site (Nulasai) comes with its rare find of smelting remains including five plano-convex ingots (an unusual copper-arsenic-lead alloy). Mei suggests that the high arsenic content may have been an intentional addition and not the result of smelting arsenical copper ores.

It would be of particular interest if, as suggested by scholars, Nulasai was a site which lay within the Saka people’s sphere of influence, but on current evidence this is a difficult attribution to make. The Saka may have been responsible for the production of a certain cauldron type, which Mei identifies and, given that all Xinjiang cauldrons are made with Chinese-style piece molds, this technology may have spread from China through the Mongolian steppe and into northern Xinjiang. Increasing contact with the Chinese heartland was occurring in the late first millennium B.C. as marked by the presence of silk, lacquer, and mirrors. Cast iron and its technology reaches Xinjiang from the central plains of China at this time as well, brought perhaps by the Saka.

What Jianjun Mei has achieved in this volume is a unique synthesis, from a variety of mostly new sources, of the critical information concerning copper-base metals and metallurgy in Xinjiang from the point of initial appearance shortly after c. 2000 B.C. down into the Iron Age. But this volume is much more than a study in a single technology, it is a harbinger of continuing revelations concerning the complex later prehistory of Eurasia. Mei’s research concretizes the crucial role played by Xinjiang in the transmission of cultural and technological traditions both East and West. He states rather decisively that “one thing appears quite clear: Andronovo expansion played a vital role in the transmission of copper and bronze technologies in Eurasia during the second millennium B.C.” (p. 74). Furthermore, Mei’s study does nothing to dispel the suggestion that this rapid cultural expansion may well have had an influential role in introducing metal and perhaps metallurgical technology into northwest China and ultimately to the Chinese civilization of the central plains.

In the Foreword, Colin Renfrew, Mei’s academic advisor at Cambridge, praises this volume as a “pioneering work,” “the first coherent study of later prehistory in this region,” and the “first in any language” to detail the region’s metallurgical evidence. Moreover, this newly opened window on what was transpiring technologically in Xinjiang, a geographical and cultural “shatter zone” between East and West, is crucial to understanding issues of fundamental interest to that substantial archaeological community whose interests lie in the Asian Old World.

This volume is readable, rich in information and has numerous tables, maps, and figures. If it is read in concert with several recent studies of Asian metallurgy, e.g., Linduff et al. (2000) on China, Chernykh
(1991) on Eurasia, Pigott (1999) on Southwest Asia, Agrawal (2000) on South Asia, and Higham (1996) on Southeast Asia and the relevant papers in Mair (1998), a far clearer understanding of the development and spread of metallurgy across the vast geographical expanse that is Asia can now be achieved. Its title understates somewhat the wealth of archaeological information it contains as this volume is more than a study of metals and metallurgy—it is an insightful view of technology in cultural and historical context—one which will certainly be widely read.

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It does not seem to be too far from truth to state that the Mons have received relatively little scholarly attention so far, if compared to other Southeast Asian peoples. For whatever reason that this may be, the present study of the French philologist Emmanuel Guillon represents the first large-scale attempt to make good for this gap by writing the history of the Mons. In fact, both the author’s reputation as a leading scholar of Mon language, and the format of the book (349 pages, foolscap size, with many colored illustrations) suggest that we have a reference work in hand. The book consists of two parts. In the first part, Guillon approaches the phenom-
enon “Mon” by describing the components that shape their identity: language and script, ethnicity and belief system. The second part, entitled “A long history,” covers the period from the formation of the first polities in the third millennium B.C. to the present, including the first blossoming of Mon culture in central Thailand (the kingdom of Dvaravati), the “classical Mon period” during the Pagan times, and the Mon states of Lower Burma between 1281 and 1754; not to forget the Mon renaissance in seventeenth-century northern Thailand. Five appendices provide additional information on palaeography, chronology, a short history of Mon studies, and two glossaries on Buddhist texts and Mon words. The author has used a wide range of written texts including Mon historiographies and inscriptions, the oldest of which date back to the sixth century A.D.

The great expectations that are roused by the outer appearance of the book are however hardly met with by its contents. Basically, the book has three major weaknesses. First, it is, by and large, a compilation of dates and facts on Mon language, art, and culture without any theoretical reflection or methodological concept. This is certainly not enough for a study that undertakes to provide a general history of the Mon people.

Second, the book is quite outdated. The original French version of the manuscript was finalized in 1969, but apart from those few areas which were among Guillon’s main research interest (i.e., Mon epigraphy and language), it has hardly been updated since. On page 170, for example, it is stated that the Buddha reached parinirvana in the year 543 according to the tradition of the southern Buddhists, in contrast to Sanskrit sources which put it between the years 478 and 486 B.C., a statement that completely ignores the relevant writings of Bechert on this topic (apart from being wrong insofar as the 486 era was a creation of Indologists who tried to reconcile the 544 era with the date of the Indian king Ashoka). Or look at what is said about the kingdom Pagan, the early phase of which is earmarked nothing less than the “classical age” of the Mons.

Research on early Burma published after 1980 is completely ignored; even though studies by Michael Aung-Thwin, Janice Stargardt, and the present reviewer have, each one in his (or her) own way, contributed to a balanced view of the Pyus and Mons and their respective influence upon Pagan, by making thorough use of the available epigraphical, historiographical, and archaeological data. Instead, Guillon mainly quotes from Luce—who with full respect to his pioneering research on early Burma—seems to have overestimated the contribution of the Mons to the Pagan kingdom. Quite paradoxically, Luce is treated critically only on occasions when it appears to be unnecessary or even wrong. Thus, Sudhammāpura is translated as “Great Assembly Hall of the Gods of the Heaven of the Thirty-three” (p. 104) against Luce’s version “City of the Good Law” merely because of the long ā. If the lengthening of vowels had indeed to be taken that seriously, a good number of ministers in Pagan would have been female, as their names end with long ā (Satyā, Asankhya, etc.). Obviously, the long vowel is euphonic, and Thaton/Sudhammapura can still be considered as the “City of the Good Law.”

Third, the author displays a notable lack of language skills whenever he ventures into languages other than Mon. This refers mostly to Sanskrit/Pali and Burmese. To give a few examples: the suffix “-dev!” in royal titles is always translated as “divinity” instead of “queen,” which is very common in Sanskrit and Pali. A complete perturbation occurs on page 32 where cākhi and cāriy are described as “two kinds of scribes.” The Burmese word cākhi indeed denotes the position of a scribe or clerk, but cāriy (lit. cā “letter” and riyw “to write”) usually occurs in formulas such as cā riyw so sā ka “he who wrote these letters.” It is a mere description and has nothing to do with the office or position of a scribe. In the footnote, moreover, cākhi is linked to Pali sakki (“witness”), which also occurs in early Burmese inscriptions, but as saksīy, which is a direct derivation from Sanskrit, saksin. Equally untenable is the derivation of the Burmese word mān (“king, ruler”) from
Mon smin (p. 160). This misinterpretation is obviously based on the pronunciation of man as “min.” While man is a pure Burmese word again, smin seems to be related to Sanskrit samit (“lord, master”). Similarly, the remarks on Burmese epigraphy and historiography (p. 96) cannot be substantiated at all. This list could be continued for a while, and moreover become extended by a list of typing errors such as arafinsi instead of arafinavasi, the forest monks (p. 79 and passim), and in the glossary of Mon words in Appendix A.

Finally, it has to be noted that the book was arranged in a rather thoughtless way: expressions such as “I shall come back to this point,” “more about this later,” “I shall have to say more about this,” and even the announcement to tackle a certain problem in a separate monograph (p. 164) occur on several occasions—it may be 30 or more—and run like an unbroken thread through the text. The question that immediately emerges after reading the book is for what reason or what purpose it may have been published. Full of mistakes, outdated, and barely original as it is, it can hardly be considered as a standard work on a “Southeast Asian civilization,” as stated in the title. Nor can we regard it as a political manifesto, as the relevance of Mons for the modern states is not discussed, be it their contribution to the early Thai states or their situation in modern Burma. The annoyance about a book that is expensively made but faulty, poorly written (and translated?), and unnecessary at the time, soon gives way to the simple insight that the history of the Mons remains to be written. Or, in Guillon’s words: More about this later.

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Reviewed by Ian C. Glover, Institute of Archaeology, University College, London

The authors describe this as the second, but still preliminary, report on the joint Australian–British–Indonesian OXIS project on the Origin of Complex Society in South Sulawesi, and it includes short reports on 29 excavated sites, and 56 sites which were surveyed in the ancient kingdom of Luwu at the head of the Gulf of Bone, Sulawesi, in central Indonesia.

The slim volume contains five sections: (1) A historical account of the kingdom of Luwu based on oral traditions, especially the epic ‘La Galigo’ cycle, and the historical lontara texts for which South Sulawesi is justifiably famous; (2) A summary of the field research program in different parts of ancient Luwu; (3) An interpretation of the research findings in the light of the historical traditions with suggestions for a new chronology based on radiocarbon dates and an analysis of the imported, largely Chinese ceramics found at many of the sites; (4) Four appendixes listing the ceramic finds and forty-two new radiocarbon dates; and (5) A six-page bibliography, useful in its own right since the abundant literature on the cultures and history of South Sulawesi is not always easy to track down as it occurs in Dutch, Indonesian, French, English, and German, not to mention sixteenth- and seventeenth-century descriptions in Portuguese.

The volume is perhaps best read in company with the book, The Bugis, by
the French anthropologist Christian Pelras (1996) on which in some ways it is a commentary. Pelras advanced a proposition, developed on the basis of a reading of the lontara and analyses of the La Galigo epic cycle by many people including himself and Ian Caldwell, that Luwu was the earliest of the historical Bugis kingdoms and its court, from the fifteenth century, was the source of much of Bugis elite culture and traditions. Luwu and its successors were essentially agrarian kingdoms in which external trade was important but not the main source of the wealth and power of the rulers. In contrast, the La Galigo cycle tells of an earlier society—perhaps between the twelfth and fourteenth century A.D.—with divinely descended rulers centered in the northern part of what is now southwestern Sulawesi, in which the political economy depended on maritime trade with other parts of the Indonesian archipelago. The La Galigo society came to an end in the early fourteenth century and after a period of anarchy, the ‘Age of Chaos’ mentioned in later Bugis chronicles, a number of new, sago and rice-based agrarian kingdoms arose, of which Luwu was the first and for long predominant.

In the OXIS project Bulbeck and Caldwell are attempting to test this reconstruction through excavating and dating ancient settlement sites in and around Luwu—an ambitious attempt and perhaps the first sustained such piece of research in Southeast Asia aiming to integrate historical and archaeological data within a closely defined region. Additionally, they have examined coastal-hinterland relationships around the Gulf of Bone within the framework of Bronson’s well-known (1977) model. To this end they set out (pp. 14–15) a number of hypotheses, which can be summarized thus:

1. That the Bugis kingdom of Luwu was founded at Malangkene by the fourteenth century A.D. on the basis of earlier, but not perhaps Bugis occupation of the region.

2. That the presence of high-grade iron ore, rich soils, and dammar (fossil resin) deposits in the region were sufficient to ensure the prosperity of the kingdom.

3. That another, more southerly kingdom of Cina located near the mouth of the Cenrana River was either a trade-based or agrarian kingdom (or both?), later absorbed for some time by Luwu.

Fieldwork was started in 1992 with an extended survey and trek across Sulawesi by Caldwell when he recognized many archaeological sites, which were identified by local informants with places known in the lontara or from oral traditions. Further surveys between 1994 and 1997 were followed by an excavation season in 1998, the scope of which was curtailed by unusually bad weather.

Excavation consisted essentially of small, 1-m-sq test pits, sometimes enlarged to 2-by-1 m, and sometimes several to a single site. Deposits were sieved and soil samples retained for more careful examination, and charcoal samples were taken where available. It could be argued that many small pits—the ‘telephone box’ strategy—are quite inadequate to give a reasonable sample of surviving remains at heterogeneous living sites. This is probably true, but the abundance of local and imported ceramics together with some iron, bronze, and gold artifacts and ornaments, iron slag, chert flakes, glass and stone beads, has enabled the authors to develop rough chronologies for most sites using radiocarbon dates and ceramic histograms based on the frequency, layer by layer, of identifiable and dateable imported sherds.

Drawing on the wealth of archaeological material, the authors feel able to comment on, and sometimes modify earlier historical reconstructions of South Sulawesi.

First they suggest that as an ethnic and linguistic group the Bugis are relatively late arrivals, not earlier than A.D. 1300 in the region north of the Gulf of Bone. Further they find no archaeological evidence for the fourteenth-century ‘Age of Chaos,’ which figures in Bugis oral traditions, but quite the opposite; that the period saw an increase of overseas trade and the rise of powerful kingdoms of which Luwu was one of many, but not the earliest. One remarkable finding was of buried brick structures at Malangkhe, which seem once (now
looted) to have been associated with Majapahit-Javanese-style gold arm bands, bracelets, earrings, and eye covers, and a great variety of ceramics. It is quite possible that this was a classic Javanese religious and burial site of the fourteenth century; and if so, it is the first to be recognized in Sulawesi.

There are many other points of interest in this brief, but information-packed report, which is surely a model "preliminary publication," lacking only in photographs of the archaeological sites and material. Looking for something to criticize I found that it was often quite difficult to find early sites and geographical features mentioned in the text related to the maps, which themselves are clear enough. Some pointers in the text would have helped here.

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Reviewed by Astri Wright, Department of History in Art, University of Victoria, British Columbia, Canada

Alessandra Iyer’s *Prambanan: Sculpture and Dance* brings together under one cover elaborations on several of the author’s articles on dance in Java published in academic journals since the mid-1990s. This small, compact, and attractively packaged book organizes the results of Iyer’s doctoral and post-doctoral research on the history, iconography, and practice of dance in Java into a consecutive narrative. Drawing on a combination of South and Southeast Asian art history, dance history, and recent theory in dance studies in general, Iyer presents her material here to a broader public, with interests (both academic and applied) in dance research, anthropology, and art history in Southeast Asia and beyond.

In just over 160 pages, divided into Part 1 and 2, Iyer offers the following. Part 1 includes an introductory chapter of disciplinary reflections and theoretical-methodological navigations; a second chapter describing the Prambanan temple complex, its main temple Chandi Shiva with its Ramayana and dance reliefs, interspersed with reliefs of deities and auspicious symbols, and a discussion of the history of restorations; a third chapter discussing the dance reliefs and their dance iconography in greater detail, giving a more detailed introduction to the Indian Natyasastra’s karana dance sequences as tentatively restored by Indian scholars; a fourth chapter discussing the Natyasastra’s (sanskrit dance treatise) possible presence in ancient Java, textually or dance-practice-wise, and a short fifth chapter with concluding remarks.

This text, interspersed with photographic and line-drawn images, is followed by Part 2, which begins with a sixth chapter that is really a catalog of the 62 dance reliefs. These are located on the outside of the Chandi Shiva balustrade (on the inside of which is carved the Ramayana series of
In this section, Iyer gives us relief-by-relief photographs and descriptions, each matched up with its corresponding karana number and line-drawing taken from Subrahmanyam's 1978 reconstruction of the Natyasastra's sections on karana. This is followed by an Appendix in which Iyer's analysis is summarized in note form in parallel columns; footnotes to the text; a glossary of dance terminology; and the bibliography.

Iyer's analysis begins with the riddle noted by nearly everyone who has written about the Prambanan over the last sixty years: what might have been the sources of inspiration and practice depicted in the relief series of dancers and musicians on the outer balustrade of Chandi Shiva (Shiva temple), most of them suggesting the difficult-to-substantiate relationship between the Natyasastra (an ancient Indian body of teachings about dance techniques, postures and iconography put into writing some time between the first century B.C.E. and the fourth century C.E.). Iyer dismisses earlier scholars' positings of this very connection as intuitive but unsubstantiated by quantifiable analysis (p. 99), even while noting that Suhamir, in his 1948 report on the completed Dutch-initiated restorations of the Prambanan, knew enough about this Indian dance tradition to identify the reliefs as depicting angahara, sequences involving more than one karana (p. 13).

Then, through an intermittently acrobatic analysis that rests on a painstaking adaptation of Indian dance scholar Padma Subrahmanyam's reorganization of the Natyasastra to the reliefs in Java, Iyer concludes that these Chandi Shiva reliefs (their original sequence also no longer identifiable), can "unerringly be identified as sculptures showing karana, the units of dance movement described in the Sanskrit text on dance and drama known from India as Natyasastra" (p. 11).

In her introduction, Iyer writes how, upon undertaking her research, she realized that she was "studying an obsolete non-Western dance form which, if at all, had only elicited interest in past times in terms of finding out whether it was an indication of the presence of Indian dance styles in ancient Java rather than for its own intrinsic value" (p. 7). While this seems a somewhat reductionist view of (among others cited) Claire Holt's varied and pioneering interdisciplinary contribution to dance studies, Iyer then proceeds to discuss what to this reader still remains unresolved in her own book: the theoretical-methodological debate around "Indianization" versus a model of local assimilation of foreign (including Indian) influences. (Iyer's footnotes here, as throughout, give a good orientation to newcomers to the field on the different stages in these debates.)

While Iyer distances herself from the 'Indianization' view of Southeast Asia as passive recipient to Indian influences in statecraft and cultural forms (and no student-scholar in this age of post-colonial analysis could fail to make such a gesture), she also critiques the "localisation of Indian influence" view as too passive and polarized a model. Good, so far as the theory is concerned. But what happens in the rest of her analysis, as it rests on her choices of methodology? Iyer's discussion is based almost entirely on recent results of the last thirty years of Indian dance scholarship (and in particular that of the author's own Indian mentor, Padma Subrahmanyan, with whom she traveled to Prambanan in 1994). Leaning on Subrahmanyan's publications of the late 1970s, Iyer creates a closer marriage between ancient Indian dance traditions and Javanese sculptural reliefs than this branch of scholarship has ever encountered before. At the same time, she also presents data that allow the reader to study the full series of dance reliefs and to compare these with the line drawings of Indian reconstructions of that tradition. Hence, while Iyer's book offers new intraregionally comparative possibilities, her analysis maintains the old India-Java axis, demonstrating mainly a one-way flow of influence. The one possibility she cites as perhaps pointing to influence flowing the other way, is not developed and remains airily hypothetical (see below).

Within Indonesian dance studies, most of the early writers on dance in Indonesia
focused on dance in Bali. Claire Holt was among the first to study dance in Java and Indonesia and to investigate sociohistorical dimensions that link indigenous and imported traditions, and sculpture, dance, and aesthetics. While Holt has a fairly lengthy discussion of the reliefs of dancers and dance-scenes at the Borobudur (1967), she does not touch on the Prambanan dance reliefs in detail. This textual focus is illustrated by the inclusion of only three photographs of dance reliefs from the Prambanan: a reproduction of a dance scene of a woman performing a sword dance in a court scene, as part of the Ramayana series (Holt 1967, pl. 102, p. 120), a second photograph showing a drummer dancing with two other dancers (frontispiece), and a third photograph from the outer balustrade’s dance reliefs (Holt 1967, pl. 41, p. 57). These last two reproductions show the reliefs numbered by Iyer as P6 and P55, respectively (Iyer, pp. 111, 159). However, one of the two publications has reversed the reproductions of these reliefs—and the existence of the Holt reproductions is strangely not mentioned in Iyer (the frontispiece photograph in Holt belonged to the Dinas Purbakala, the Indonesian Archaeological Service; the one on page 57 was photographed by Holt herself). Furthermore, this comparison highlights how poor the quality of the black and white reproductions of Iyer’s own photographs of the dance reliefs are, perhaps a signal to White Lotus to improve their reproduction technology.

Another issue, which relates both to the author and the editors at White Lotus Press, is the occasionally confusing organization of the book. The most blatant example is in Chapter 3. While the text is organized into subsections, numbered 3.1 to 3.5, a ten-page section of the line drawings of dancers illustrating different karana (taken from Subrahmanyan’s publications, pp. 73–82) are also numbered with the same numbering system (starting with Figure 3.1). This is done without there being any apparent correspondence between text and image bearing matching numbers, and there are no references to individual drawings in the text.

This confusion is due to a major editing and layout oversight. After much cross-checking, the drawings are, in fact, revealed to be illustrations of the karana discussed in text section 3.4.1 (Karana descriptions); however, the existence of the line drawings that follow twenty pages later is not signaled in the written text. Furthermore, the small ‘catalog’ of line drawings have no subheading of their own either in the table of contents or on the page where they begin. Finally, the drawings are also numbered according to a second system, according to their karana number, all of which makes for a disorganized reading experience.

Since dance studies of non-Western traditions constitute the lesser part of that literature, historiographically Iyer’s book represents a welcome contribution to a small field. Through her detailed studies and analysis, Iyer is able to tie the Chandi Shiva dance reliefs more closely to the Indian sanskritic text tradition than anyone has before and perhaps this reflects more closely a historical reality than what we have known in this subject area until now.

However, this analysis has been done without the author adding any perspectives from what one might expect to be an absolutely necessary component in her research—reconstructions (in texts, image, or contemporary choreography) of indigenous (here, Javanese) dance traditions and iconography. Similarly, it is stated clearly that the Glossary of dance terminology (pp. 193–198) includes only those Sanskrit dance terms in the Natyasastra that Iyer has identified as relevant to the Prambanan dance reliefs. Rather than an informed challenge from a dance historian, the following is a sincere question from someone rooted in an interdisciplinarily informed, culture-specific art history: Is there, then, no Javanese dance terminology, as distinct from those in Sanskrit, used in Java in historical texts or even today (which are linguistically archaic) to describe any movements identifiable in the reliefs?
Perhaps the latter kind of material does not exist. But any sense of a social history of dance in Java is missing. Hence, I cannot help but wonder how Iyer’s study would have differed had she traveled to Prambanan with other mentors at other times, mentors from Java, Bali, perhaps Cambodia and Thailand, perhaps even from parts of the Oceanic world. I cannot help wonder what her conclusions would have been had she undertaken comparative local studies of tribal-primal dance forms, as far as they still exist, alongside the studies of the more obviously ‘internationalized court cultures’ dance traditions. The fact that many *karana* imitate animal movements or are inspired by elements of nature (p. 89) would seem to point to a link, at a deep historical (indeed, archaeological) stratum, between a primal way of thought and ritual and the high-Sanskrit brahmin literary culture that produced the written versions of the Natyasatras and other classic texts. After reading this book, I cannot help wonder at how text-based many historians, including art historians (which might or might not include dance historians), still are.

Adding first-person experience of culturally relevant kinetic movement to the art historical study of sculpture and space, urban and rural, interior and exterior, would greatly enrich insights into writings about a culture like Java’s where both dance and an appealing personal body-language for both men and women is so central. The way the discipline of art history is currently constructed and practiced, however, dance history, dance iconography, and movement analysis occupies no more than a tangential relationship to it. Indeed, it would appear that dance history stands on its own methodological feet as much as does the history of music, theatre, literature, and other areas of human endeavor that together constitute the arts of humanity but are generally not included under the rubric of fine arts. In the same way that music needs the aural text to display its data, I would argue that this book needs a video of dancers offering various possible interpretations of *karana* to accompany it. The drawings on pages 73–82 do provide a lively and communicative break from the static nature of print text. Since Iyer has been experimenting with digital video-imaging, perhaps this is a next stage in her research.

While Iyer claims dance history as a branch of art history, she herself displays limited art historical tools at times, such as when interpreting the famous early fourteenth-century statue of King Krtanagara as a fusion of Siwa and Buddha (p. 69) when it is well established as a *hari-hara* (Siwa and Vishnu) image and simultaneously an *ardhanarisvara* (Lord who is Half Man Half Woman) image. In addition, when challenging the old theory that the three structures facing the three Prambanan temples once housed the *vahana* (vehicles) of each temple’s main deity, Iyer cites the find of ‘a solar disk’ opposite the Vishnu temple as more likely evidence of Surya than of Vishnu. Here, she (and the unnamed staff member at the Prambanan Archaeological office she cites) misses the fairly basic point that a circular disk (a weapon) looks very similar (and at times identical) to the symbol of the sun, and such a disk is one of Vishnu’s main iconic attributes. Vishnu is classified a deity of solar lineage, and as such complements his theological rival Shiva who is of lunar lineage and powers.

This is not to say that dance would not be a major part of a culturally contextualized Indonesian art history more free of European biases and outmoded disciplinary limitations than it is today (or that a solid knowledge of sculptural-mythological iconography shouldn’t be part of a dance scholar’s tools). Holt’s *Art in Indonesia: Continuities and Change* (Ithaca, NY: Cornell University Press, 1967), divided into three parts (The Heritage, Living Traditions, and Modern Art, followed by rich appendixes of translations of inscriptions, scenes from epics and shadow puppet plays) still stands out as a pioneering (even prescient) work of post-colonial art history that has not been matched or recognized for its contribution to theories and meth-
odologies developed (and still developing) three-and-a-half decades after its publication. Among national or regional histories of art and cultural expression within Southeast Asia, Holt's remains a lone monument to a culturally embedded, multimedia and multivocal art history.

Like Iyer, Holt was trained as a dancer and knew the various aesthetics of different Indonesian (and other Asian and European) dance traditions through her body. However, most of us in Southeast Asian art history have not been able to include more than a smattering of dance studies (if any) in our training, which makes the work by Alessandra Iyer, and her colleagues Felicia Freeland-Hughes and Clara Brakel all the more valuable.

To me, the two most interesting hypotheses offered by Iyer are the following: (1) the possible presence of Shiva, Lord of Dance, on the Chandi Shiva and (2) the linking of all the individual karana into a specific tandava (dance associated with Shiva). While Holt writes that no image of Shiva himself as Lord of the Dance appears at Prambanan or at any other of Java's sanctuaries (Holt 1967:61), Iyer posits that some of the groups of three dancers may depict the Lord Shiva himself. While establishing the many variations in Shiva iconography from Indian to Cambodian and Cham art and how definite Shiva attributes may have gone missing, Iyer does not in the end point to any specific reliefs as candidates for Lord of the Dance status. This would be an interesting line of analysis to continue. Combining close scrutiny of the dance reliefs by eyes very familiar with Shiva representations throughout South and Southeast Asia (to me, based on the poor reproductions, reliefs P27, p. 132 and P31, p. 136 stand out as strong candidates for Shiva status here) with the idea that Javanese sculptors may have worked locally from imported workbooks with drawings of images not entirely familiar to them, one could arrive at a meaningful hypothesis of one likely scenario.

The other interesting hypothesis of Iyer’s to follow up on is the fact that, if the dance reliefs indeed are dated to around the same date as the construction of the temple, they are one to two hundred years older than the earliest representations of a karana series found in India today, the earliest known series being at the Chola Brhadisvara temple in Tanjore (Iyer, p. 37). Iyer posits that, while neither the idea of karana nor the Natyasastra text originated in Java, perhaps the idea of carving a series of karana as part of a temple’s sculptural program may have originated in Java and been transmitted back to India, and as such, constitute a case of cultural recycling—or the flow of influence going the other way.

However, before research like what Iyer outlines above can be taken any further, a well-overdue translation seems an absolute necessity. In Chapter 4, Iyer points out that a balanced analysis of the dance reliefs carved at Chandi Shiva or elsewhere in classical Java is not possible until the Old Javanese text Nawanatya (late thirteenth—fourteenth centuries), is fully translated and analyzed. The fragments of the Nawanatya translated by Pigeaud in an appendix in his 1963 translation and study of the Nagara Kertagama, include terms also found in the Natyasastra. Iyer points out that this suggests prior knowledge of the Natyasastra in Java (p. 92). It would seem clear, however, that the Javanese text would have to be read for all of its references, not only for all of its potential narratives of recycling but also for possible information of local invention, whether more hybrid or less so.

These, then, are some of the studies we can hope for in the next round of scholarship pertaining to dance and cultural transmission in historical Java, and toward which Iyer’s present book provides a stepping stone.