COPYRIGHT NOTICE
Michael A. Aung-Thwin/The Mists of Ramanna

is published by University of Hawai’i Press and copyrighted, © 2005, by University of Hawai’i Press. All rights reserved. No part of this book may be reproduced in any form by any electronic or mechanical means (including photocopying, recording, or information storage and retrieval) without permission in writing from the publisher, except for reading and browsing via the World Wide Web. Users are not permitted to mount this file on any network servers.

NB: Illustrations have been deleted to decrease file size.
In 1479, when King Dhammazedi of the kingdom of Pegu declared on his Kalyani Inscriptions that the legendary Suvaṇṇabhūmi of Buddhist tradition was the Mon kingdom of Rāmaṇādesa in Lower Burma, he inadvertently created a twentieth-century historiographic issue that I have called the “legend that was Lower Burma,” still with us today. Suvaṇṇabhūmi, “the land of gold,” was, of course, the region to which the two most famous Buddhist missionaries, Saṅa and Uttara, were said to have gone from the Third Buddhist Council of Aśoka in the third century BC to propagate the faith, an event long celebrated as the introduction of Buddhism to Southeast Asia. This council was perceived by Theravāda Buddhists as the most orthodox of Buddhist councils, so the version of the scriptures the missionaries carried with them to Suvaṇṇabhūmi, and therefore also to Rāmaṇādesa, was also considered the most orthodox. By thus linking Lower Burma with the sacred geography, sacred genealogy, and sacred chronology of Aśoka’s Buddhist India, King Dhammazedi, in one stroke, gave Rāmaṇādesa an antiquity, orthodoxy, and legitimacy it never had. Then for nearly four hundred years Dhammazedi’s attempt to link Aśokan Buddhist India with Lower Burma and the legendary foundations of his own kingdom was all but forgotten in the historiography of the country.

Two and a half centuries later, between 1712 and 1720, a private individual named U Kala wrote the most comprehensive chronicle of Burma’s monarchy that has survived, the Mahayazawingyī. In it he recounted for the first time the most complete version of the now-famous story about the conquest of Thatōn by King Aniruddha of Pagan in 1057. It begins with Shin Arahan, the celebrated monk who was said to have come to Pagan in the mid-eleventh century and converted the king to the orthodox version of Theravāda Buddhism. Desiring to promote the religion, Aniruddha asked Shin Arahan how to proceed. Shin Arahan told the king that if he wished to establish the faith in Pagan—which at the time was said to be rampant with the Aris, a heterodox sect—he must have possession of the
orthodox texts. To get them, the king should request a copy from the Mon King Manuh of Thatôn in Lower Burma, as he possessed many sets of the “pure” Tipitakas.

When Aniruddha approached Manuh with this request, he was rudely refused, so Aniruddha attacked and conquered Thatôn, taking back to Pagân not only the Tipitakas, on thirty-two white elephants, but also King Manuh, the royal family, and the country’s entire population of 30,000, among whom were myriad artisans and craftsmen, learned clergy, and other people of letters. Upon his return to Pagân, Aniruddha placed the texts in the specially constructed Pitaka Taik (library), a building that still stands today. Thereafter, the “true” religion shone radiant in his kingdom, and, lamented one late Mon chronicle, Pagân flourished “like unto a heavenly city.”

In the nineteenth century, over two hundred years after U Kala’s account was written, Dhammazedi’s fifteenth-century claim that ancient Suvannabhûmi was Râmaññaadesa and U Kala’s eighteenth-century account of the conquest of Thatôn—two temporally, causally, and textually unrelated narratives—were combined for the first time by colonial scholarship and synthesized into a new theory: that the Mon Theravâda Buddhist culture of Lower Burma “civilized” Burman Upper Burma. This is the thesis that I call the Mon Paradigm.

The historiographic and pedagogic implications of the Mon Paradigm are enormous. Because Pagân is considered to have been the “golden age” of Burma’s culture and therefore also the foundations upon which the country’s subsequent culture was built, the Mon Paradigm implies that the Mon people and the culture of Lower Burma were the ultimate origins not only of Pagân civilization, but also of Burma’s culture in general. To the Mon of Lower Burma have been attributed Pagân’s orthodox Buddhism of the Mahâvihâra school; its indigenous elements of the conceptual system (including even the Cult of the 37 Nats); its ideologies of leadership, legitimacy, and authority as reflected in the idealized organization at court; its pantheon of patron-saints, including Upagupta, Maitreya, and Gavanipati; its writing system (hence, that of the entire country); its fine arts and crafts; its unique temple architecture; the immediate source for its literature; and even its irrigation technology. All these, in turn, were said to have been implemented during a “Mon period” in the history of Pagân under the champion of Mon culture, King Kyanzittha, who almost single-handedly accomplished this “civilizing” process.

As noted in the Preface, I accepted the Mon Paradigm when I began research on the role of Lower Burma in the Bay of Bengal. I had no intention of challenging the conventional view, and was, in fact, trying to prove, not disprove, the existence of Râmañña. I had no inking at the time that
my research would lead me in the opposite direction. Yet the more data I gathered on Lower Burma before and during the Pagán period, the more I realized that something was amiss. There was just no primary evidence—that is, authentically dated contemporary and original material—to support the belief that a civilization—as defined both in the popular sense of the term and more strictly as urbanization—existed in Lower Burma during the first millennium AD.

Even more unsettling, I had not yet begun to look at any new evidence, only the old data that have been around for many years, much of it originally uncovered by the scholars of the Mon Paradigm. In fact, this entire book could have been written without the most recent evidence, the bulk of which is relevant mainly to the Pyū, whom I discuss in Chapter Two. In other words, the viability of the Mon Paradigm does not hinge on dramatic new evidence that I recently uncovered but on old data that scholars have long known about. This, then, is not an indictment of evidence but of methodology: of the way data have been assessed and used to conform to a preconceived theory.

Throughout the twentieth century, respected scholars of Burma, not a few of whom were of Mon cultural background or otherwise intimately connected to it, continued to perpetuate and expand the Paradigm. It became the basis for virtually all scholarship on Pagán and early Burma, and has succeeded in dominating the study of early Burma for over a century. Its thesis also struck a responsive chord with other twentieth-century scholars of early Southeast Asia. In part this was because it involved the Mon people, who by then had become sentimental favorites. Colonial perception held the Mon to have been the oppressed victims of later-arriving, less civilized Khmer, T’ai, and Burmese speakers. These newcomers were thought to have conquered or otherwise integrated the Mon, absorbed their culture, and ignominiously ended their presumed great achievements at places such as Dvāravatī, which at that time had just been discovered. In the Mon-Burman situation especially, colonial scholars saw a replication of the Greek and Roman experience, in which the conquered had given their culture to the conquerors.

To be sure, there was at least one detractor among the few Mon specialists of the time. This was Pierre Dupont, an archaeologist and art historian whose research focused on Dvāravatī. He had serious doubts about at least one component of the Mon Paradigm: the alleged antiquity of Mon civilization in Lower Burma. Over half a century ago he suggested that the Mon of fifteenth-century Burma had probably recast their past “in a form that would bestow the dignity of age on their newly purified faith.” In part, his view was shaped by his research on Dvāravatī, whose Mon culture, he thought, preceded that of Lower Burma. But his assertion was more
than academic self-interest. Dupont was not a Burma specialist, and as a result he was unencumbered by its intellectual baggage or the ethnic nationalism of the era. He could, therefore, assess the situation more objectively. Dupont had actually hit the nail on the head, but did not pursue his thoughts much further, for, as a nonspecialist of Burma, he did not have the language tool (Old Burmese) to do so and had other priorities in any case. Also, he was apparently reluctant to contradict those who did know Old Burmese, specialists in the field who were, by then, totally convinced of the correctness of their theory.

Thus, the Mon Paradigm continued unquestioned. Eventually, it became so dominant and pervasive, both as an intellectual idea and in the number of prominent scholars of early Burma who subscribed to it, that it not only fed upon itself, but consumed virtually everything else in its path in order to perpetuate itself.\textsuperscript{13} Conflicting information was interpreted to fit, not to reexamine it, so that pertinent data were analyzed only within its framework of “truth.” The following rationalization is representative of the kind of reaction by advocates of the Mon Paradigm when faced with evidence that contradicted it. “When we consider how important the Mons must have been in the civilising of the Mranmä, it is surprising how rarely they are mentioned in Old Burmese.”\textsuperscript{14} And such statements were used to \textit{prove} not \textit{disprove} its case. Even fictitious individuals and places (like Makuta and Rakṣapura), about which I will have much to say, emerged virtually from thin air to sustain the thesis.

The Mon Paradigm continued unabated despite the fact that throughout these same years new archaeological data suggested that another culture, an ethnolinguistic group of Tibeto-Burman speakers popularly known as the Pyű had been present earlier and found throughout much of the country for an entire millennium. They had been centered in Upper Burma, with settlements also in Lower Burma.\textsuperscript{15} But the influence of the Mon Paradigm was so pervasive and dominant that scholars acknowledged this information in only the most perfunctory manner and continued as if the Pyű evidence had little or no bearing on their concerns. Part of the reason was probably their assumption that since Lower Burma “belonged” to the Mon, who were thought to have arrived earlier than the Pyű and subsequently overlapped with them, the Pyű (and later, Burmese speakers) must have been confined to Upper Burma. This produced an imaginary, ahistorical image (and map) of Burma as a land of discreet and absolute ethnic divisions that unfortunately became the basis for much scholarship (see Figure 1).\textsuperscript{16}

By 1983, another non-Burma scholar, the late Paul Wheatley, renowned historical geographer of East and Southeast Asia, again raised doubts about
the antiquity of Mon civilization in Lower Burma. After noting Dupont’s observations of nearly thirty years before, he wrote that according to later traditions of both the Burmese and the Thai, and of the Mon themselves, “the hearth of Môn culture was situated in Lower Burma, particularly in the neighborhood of the cities of Thatôn and Pegu.” But, he reasoned, had there been such a civilization, there should have been a rich harvest of related Buddhist materials and Mon remains at these avowed centers datable to the first centuries of the Christian era. Yet, he noted, “the opposite is the case.” It is a paradox, he concluded, “which in the present state of knowledge cannot be resolved.”

But as this study will show, it was a “paradox” not because of “the present state of knowledge,” but because the data already available at the time Wheatley wrote had never been considered independently of the Mon Paradigm. The issue could have been resolved then and there, but non-Burma scholars like Wheatley and Dupont found it difficult to push the subject any further when Burma specialists had accepted the Mon Paradigm so enthusiastically and more or less ignored or were unaware of what non-Burma specialists outside the country were saying. This lack of thoughtful attention to the research of outside scholars meant the unquestioned persistence of the Mon Paradigm for many more years.

Indeed, as I have seen nothing in Burma scholarship either inside or outside the country during the last century that has contested the Mon Paradigm, it is very likely that it would have continued for at least another generation. The bulk of even the most recent research on early Burma continues to perpetuate it, demonstrating to me at least, that the Mon Paradigm is still alive and well and quite sacrosanct. It has been difficult to challenge methodologically, not least because it has been entrenched for over a century, its arguments densely woven together, and its foundations buried in labyrinthine, subtle, and well-hidden tautologies extremely laborious to untangle. But it has been difficult to challenge conceptually as well, because several disciplines—from archaeology, history, and art history to epigraphy, paleography, and linguistics, not only of Burma but of other regions of Southeast Asia—have based their own works on it, giving the impression that the Mon Paradigm has a broad, interdisciplinary consensus. No matter what the data say, one always has to account for the presence of an early Râmaññadesa in Lower Burma first.

The institutional barrier in Burma studies has not been easy to breach either, for a personalized, patron-client colonial and postcolonial academic structure has made it extremely difficult for those who might want to contest the conventional view. One has to be at a distance from that academic setting—that is, to be located outside Burma and England, physically as well as intellectually—in order to successfully challenge the Mon Paradigm
without reproach; indeed, rewarded for doing so. But once these kinds of often “silent obstacles” had been overcome, all it really took to dispel the Mon Paradigm was a harder, closer look at the data already available, most of it the product of those very same scholars who had perpetrated the fiction in the first place. The positive reactions to my work that are beginning to come from both Burma and overseas scholars encourage me that the established ideology may at last be starting to crumble.20

**Approach to the Problem**

My initial approach to the discrepancy between evidence and conclusion was to offer alternate explanations for the evidence. But because I remained within the Mon Paradigm’s theoretical framework, I arrived at the same results. The antiquity of Mon Rāmaññadesa—the ultimate basis for the Mon Paradigm—had automatically made every coin, votive tablet, Buddha statue, inscription, potsherd, or settlement site found in Lower Burma to be Mon and earlier. That has led to numerous other tautologies which accepted only the conclusions and evidence conforming to the cherished premise. Those that were contradictory were rationalized as “improbable,” “unreliable,” or as Luce remarked, “surprising.” In other words, premise and proof had become synonymous.

It is, of course, understandable for a school of thought not to question its own premise. But that has also meant that those subscribing to the Mon Paradigm never asked some very basic questions. The situation is similar to the old myth that the Nanchao kingdom of Yunnan was T’ai.21 This assertion remained unchallenged for many years simply because no one asked the most basic question that would have immediately helped resolve the issue: what language did the people of Nanchao speak? It turns out to be Lolo from the Tibeto-Burman, rather than T’ai from the Austro-T’ai family. No one asked those kinds of questions of the Mon Paradigm either, questions that would have challenged it at the outset.

So I decided to ask them, albeit over a century later. First, if a Mon kingdom in Lower Burma called Rāmaññadesa existed from before the first century of the Christian millennium onward—as the historiography has it—then why are the Rmeñ (Mon), as a distinct ethnolinguistic group, not mentioned in the country’s original, contemporary sources until 1,100 years later? Second, why does the first evidence of a Lower Burma kingdom appear only 1,300 years later? And third, why does Rāmaññadesa and its putative center, Thatôn (Sadhuim), not materialize in original and contemporary domestic epigraphic sources until 1,400 years later? New evidence was not needed to ask these questions.

I therefore adopted a strategy of reexamining the evidence as if the
Chapter One

Mon Paradigm did not exist at all, that is, independently of those premises and assumptions, and hence outside its framework of analysis. To do this, I had to reassess the original evidence not only for evidentiary reliability, but, more important, to also remove it from the subtle influences of the nineteenth- and twentieth-century sociopolitical framework in which past and current generations of Burma scholars have worked.²²

Specifically, I had to reexamine the primary sources in the original language, or in translation when I could not read the language. In terms of epigraphic material, I had to reread every Old Burmese and Old Mon inscription of the period, taking care to distinguish editorial interpolations that have sustained the Mon Paradigm from the original text itself.²³ I analyzed most of these inscriptions in their published versions, but on several occasions had to scrutinize the actual rubbing or photograph of the rubbing, particularly when I needed to reread crucial words in their original or near-original state. With manuscript material, I used the published versions also used by the Mon Paradigm, for the most part. I looked at the originals on microfilm in those cases where I needed to reassess them afresh for the same kinds of reasons that I needed to investigate their stone counterparts more closely. Only as a secondary effort have I reconsidered the factual basis of the evidence, because it became clear almost immediately that the evidence itself was not the main problem. With regard to archaeological data, I deliberately went back to the raw data and reports, rather than using the interpretive conclusions, for these invariably assumed the validity of the Mon Paradigm.

Essentially, then, I studied the same archaeological, epigraphic, chronicle, and, to a lesser extent, art historical, and numismatic evidence used by the Mon Paradigm, along with whatever new information carbon-14 and thermoluminescence dating provided. To reiterate, the crucial difference in my approach was not so much reassessing the credibility of the evidence as it was reexamining it outside the analytical framework of the Mon Paradigm. This meant, ultimately, not assuming a chronological or cultural relationship between the data found in Lower Burma and Mon speakers—the fundamental flaw of the Mon Paradigm.

Results of the Approach

Once I used the above approach, an entirely new picture with several different options sprang up almost immediately. Perhaps most important, I found that neither the Old Mon inscriptions nor the earliest Mon texts of Burma supported the Mon Paradigm. Indeed, as we shall see throughout this book, the history of the Mon in Lower Burma, as told by the Mon
themselves, is not consistent with the Mon Paradigm, but with the archaeological, art historical, epigraphic, chronicle, and Chinese sources.

Thus there is no evidence to support: a) the presence of a Mon (or any other) kingdom in Lower Burma prior to the rise and development of Pagán, b) the conquest of Thatôn by Aniruddha, or c) the “civilizing” of Upper Burma by Lower Burma. In fact, the primary evidence suggests just the reverse: it was the kingdom of Pagán that was responsible for the demographic, cultural, and infrastructural development of Lower Burma, providing it with the wherewithal that turned a sparsely populated “frontier region” into an independent polity for the first time only in the late thirteenth century. In short, it was Upper Burma that was responsible for the civilizing of Lower Burma.

Accordingly, Chapter Two describes the Upper Burma Pyu culture of the first millennium that was responsible for the subsequent rise of the Pagán kingdom by perhaps the ninth century. The chapter summarizes the current academic situation in Burma studies regarding this Pyu culture and its implications for the question of state formation in the country. By now it is quite clear that Tibeto-Burman language speakers dominated the general geographic region of Upper Burma known to its historians as the “heartland” for approximately the two centuries prior to, and for most of the first millennium AD. It is with this group that Burmese speakers made first contact and from whom they borrowed their Indic culture. There is no primary evidence of another polity or kingdom led by Austro-Asiatic, Mon language speakers in Lower Burma or anywhere else in the country during that same period of time.

Chapter Three examines the etymology and historicity of the entity and concept of Rāmaññadesa, the “Realm of the Rman,” employing contemporary and near-contemporary indigenous and external sources. Not a single contemporary external record mentions any polity in Lower Burma prior to the late thirteenth century, and not a single indigenous epigraphic source mentions it prior to the fifteenth.

That led, in Chapter Four, to the reexamination of the etymology and historicity also of Thatôn, the alleged center of the alleged Rāmaññadesa. As one might expect, Thatôn does not appear in original epigraphic sources either until the latter half of the fifteenth century; indeed, it appears in the same inscriptions in which Rāmaññadesa is also first recalled. As for the site alleged to have been ancient Thatôn, there is no scientific evidence of its eleventh-century existence or of its occupation at the time by Mon speakers, or that it is even the same site claimed to be the Thatôn of legend.

If there is no evidence of a Mon kingdom until the very late thirteenth century at the earliest, and no mention of “its” capital until the fifteenth
century, then the historicity of its conquest by Aniruddha in 1057 becomes highly problematic. Chapter Five, therefore, searches for the first mention of the conquest story in epigraphy, while Chapter Six considers the same issue in the chronicles. While the conquest does not appear in epigraphy at all, a short and convoluted version of it first appears not in Burmese, but in Northern Thai chronicles written in the sixteenth century. Indeed, as stated above, it was not until the early eighteenth century that an extended, “full-blown” version appears in Burmese chronicles for the first time in the Mahayazawingyi of U Kala. Why did the story appear only then? What function did it serve? If the story were not historical, what other purposes might it have served, and why at that specific time? I attempt to answer these questions, albeit superficially, in Chapter Six, for the context in which these chronicles were written is still not well understood and the subject by itself would require a monograph. 

Without the conquest of Thatôn, of course, the consequences attributed to it can no longer stand prima facie. But in order to dispel the Mon Paradigm thoroughly and completely, I show that the primary evidence does not support that claim in any case. One of the most important consequences of the alleged conquest involves the origins of the Pagán writing system, long attributed to the Mon of Râmaññadesa via Dväravati. But the theory proposed by the Mon Paradigm for the advent of that script is simply impossible, while paleographically and linguistically, it remains to be demonstrated, let alone proved. Tentatively, I hold that the Old Burmese (Pagán) script was adopted from the Pyû, who in turn had earlier borrowed theirs from a South Indian script. And it is from that Pagán Old Burmese script that written Old Mon, Arakanese (which is practically Old Burmese), and the main Shan scripts of Burma were subsequently derived. I address the issues and problems inherent in all this in Chapter Seven, but ultimately leave the topic open for linguists to resolve.

In order to prove the contention made in Chapter Seven, original, dated epigraphy must show that written Old Burmese in the Pagán script preceded written Old Mon in the same script, the two being virtually identical in Burma. Chapter Eight demonstrates that is the case; Old Burmese inscriptions in the Pagán script were indeed present well before the first dated evidence of written Old Mon in the country, possibly by as much as a hundred years. Therefore, the former could not have come from the latter; rather, the reverse is more likely.

In Chapter Nine I address another important ancillary conclusion claimed by the Mon Paradigm: that the style of what became one of Pagán’s most ubiquitous religious architectural forms, the hollow temple (or gu), was Mon. There is no evidence to demonstrate that the typical Pagán period gu, with its distinct engineering feature (a true vault), was a Mon contribu-
Neither the style nor this engineering feature on which the integrity of the style rests can be found in any other Pagan-period Mon site anywhere in Southeast Asia. The conclusion also assumes a very problematic link between artistic style and ethnicity. Once again, the evidence shows that the situation was likely to have been the reverse of what the Mon Paradigm asserts. It is likely that the kingdom of Pagan was the source for the most prevalent religious architectural form of Lower Burma, that is, the stupa, and that the genuine Pagan gu, along with its engineering knowledge, disappears in Burma’s history shortly after the decline of Pagan.

One of the most intriguing problems in the historiography of Pagan is the modern legend of King Kyanzittha. Created by G. H. Luce, it is very much a bulwark of the Mon Paradigm. The king was said to have been responsible for the establishment of Mon culture, from which arises the alleged and celebrated “Mon period” at Pagan. The king was also given the credit for introducing Sinhalese Theravada Buddhist orthodoxy to the kingdom, so that he, not Aniruddha (as the traditional view has it), was said to have been the one who really reformed the sangha and the religion. Upon closer scrutiny, it turns out that the modern legend of Kyanzittha and the consequences attributed to his reign cannot be supported, even by the same evidence used to sustain it. Chapter Ten deals with this issue.

These chapters address the three most crucial components alleged by the Mon Paradigm: the antiquity of Ramaññadesa, the mechanism by which the latter’s culture was transported to Pagan, and the “civilizing” of Burman Upper Burma by Mon Lower Burma. But the Mon Paradigm’s influence was not limited to the study of early Burma. It went well beyond that to shape the historiography of the “early modern” as well as the colonial and post-colonial periods. Chapter Eleven is, therefore, concerned with one of the most important issues in Burma studies shaped by the Mon Paradigm: the notion of the “downtrodden Talaing,” an alleged derogatory Burmese term for the Mon people. This phenomenon was said to have originated with King Alaungpaya and his reunification of Burma in the mid-eighteenth century. The chapter describes how this belief became embedded in the Mon Paradigm, and how it subsequently developed into the primary organizing principle of Burma’s entire precolonial and much of its postcolonial historiography. Most revealing, the notion of the “downtrodden Talaing” can be found initially only in the English-language scholarship of the colonial period; it exists nowhere in the indigenous literature of the time, Burmese or Mon. Only with subsequent colonial persistence did the idea of a downtrodden Talaing class become part of twentieth-century Burma Mon mythology.

These chapters virtually beg the question of how, when, and by whom, the Mon Paradigm was begun. Who were the scholars and officials responsible? What were the pressing issues of the time that may have motivated
them and shaped their ideas? Although this topic, the subject of Chapter Twelve, surely requires an entire book by itself, I nevertheless attempt to provide a general chronological and topical narrative of people and ideas. I describe the way in which the Mon Paradigm, entangling itself in the political issues of the day and missionary concerns surrounding ethnicity, emerged and developed during the early colonial era, and how the Paradigm subsequently became institutionalized as historical “truth” in the official and unofficial canon of Burma Studies. Thus, the intimate relationship between the colonial scholar and the colonial official is very much a part of the story of the Mon Paradigm and Burma’s historiography.

In the final chapter, I offer an alternative scenario, suggesting what Burma and early Southeast Asian history might look like without the Mon Paradigm. This discussion is woven around several well-known, more general topics: a) the formation of the state, b) “Indianization,” c) the rise of “classical” Pagán, d) the “crisis of the thirteenth century,” and the “decline” of the “classical” states, e) the actual role of historic Rāmaññadesa in the “long sixteenth century,” and f) the implications of all the above for the understanding, organizing, and periodizing of Burma’s history today.

Still, I wonder whether Southeast Asian scholars can genuinely accept the alternatives. Can we shed our modern, postindustrial, market biases that trade and commerce were the major causes for state formation in much of (especially Mainland) Southeast Asia, and consider instead that agriculture and the agrarian interior may have given birth to the states in question? Can we imagine the “Indic” development of Pagán, and that of other early Mainland Southeast Asian states, without the dominating influence attributed to the Theravāda Buddhist Mon culture of Lower Burma that early, so ingrained in the epistemology of the field? Indeed, can we accept just the opposite, that Upper Burma may actually have “civilized” Lower Burma in terms of its religious and conceptual systems, its script, its literature, its art and architecture, its physical and administrative infrastructure, perhaps even its codified law and legal system, with all the attendant consequences for other adjacent areas of Southeast Asia? Can we envision a late Mon Lower Burma that actually “belonged” to the “early modern” period rather than to the earlier “classical” era? And, finally, can we perceive a precolonial and postcolonial Burma in which ethnic conflict is not the dominating and determining factor? All this would require a paradigm shift, which is exactly what I am asking Southeast Asian scholars to consider.