

Communications to the Editor

The Question of a Slave Society in North Korea

Yōng-Ho Ch'oe, "Reinterpreting Traditional History in North Korea" (*JAS* 40, 3 [May 1981]: 503–523), in the beginning of his interesting study states: "Ever since Karl Marx attempted to interpret history as a lineal progression through primitive communal, slave, feudal, capitalist, and socialist stages, Marxist historians have applied this periodic scheme in their studies" (p. 503).

This is, however, not the case. As for the antiquity, Marx to the contrary acknowledged the existence of the antique and Asiatic ways of production, respectively, but did not mention the "slave stage" at all. His ideas on the subject had been developed in the study, "Formen, die der kapitalistischen Produktion vorhergehen," a part of his *Robentwurf* of the Grundrisse der politischen Ökonomie (1939), unfortunately published only in Moscow.

The theory of the slave stage or slave society was developed in the 1930s during extensive discussion before the publication of the *Robentwurf*. This theory has never been fully accepted by the Marxist historians as the protracted discussions on the Asiatic way of production during the 1960s and 1970s in U.S.S.R. and elsewhere showed. For the background and application of the theory of the slave society in China, see, e.g., my study, "Existierte in China eine Sklavenhaltergesellschaft?" published in 1963 in *Archiv orientální* 31: 353–63.

My answer to the question of the existence of the slave society was and remains negative.

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The Segmentation of Monastic Fraternities in Sri Lanka

May I offer a few comments on Steven Kemper's paper, "Reform and Segmentation in Monastic Fraternities in Low Country Sri Lanka" (*JAS* 40, 1 [Nov. 1980]: 27–41), which he has presented as a continuation of, and improvement on, the discussion of segmentation in the first part of my book, *Buddhism in Sinhalese Society, 1750–1900* (Berkeley: University of California Press, 1976).

Kemper begins his initial references to my work by complimenting me on "a masterful job in discussing the rise of Low Country *nikāyas*" (p. 28); but he soon gives a strange interpretation of that discussion—on which I shall comment later—and ends by making a claim that I had left "the impression . . . that segmentation in the Amarapura fraternity came to an end in 1865" (p. 29). I am baffled by this claim because, in the introduction to my book, I made it clear why I chose to end the discussion of monastic fraternities in 1865, and I noted that the "same discussion could have been continued for the post-1865 period" (p. 7).

Kemper points out that only six of the Amarapura fraternities emerged before 1865, whereas nineteen more have appeared since then; his own aim is to take on all

twenty-five of them: “when we have understood the diverse origins of the Amarapura *nikāyas* as a whole,” he promises his readers at the end of the introductory section of his essay, “we will have a different idea of the nature of monastic segmentation, as well as a more problematic conception of the early groups that Malalgodā discusses” (p. 30).

In effect, Kemper undertakes to do three things in the ensuing discussion: (a) examine the origins of nineteen post-1865 *nikāyas*; (b) reinterpret the origins of pre-1865 ones; and (c) put forward a new explanation of segmentation in general. Clearly, (a) is an ambitious research undertaking—and the success of the other two are dependent on that—but the reader who expects to see the results of Kemper’s investigations into these previously unstudied *nikāyas* will be disappointed because all that he is given in the end is a table (p. 33) containing some embarrassingly elementary facts and figures: the names of five *nikāyas* (tucked away in a note); the names, dates of founding, and ordination traditions of the other fourteen *nikāyas*.

Derived apparently from just two sources (the Secretary of the Amarapura Nikāya and the 117th edition of Ferguson’s *Ceylon Directory*), the accuracy and precision of even these basic facts and figures leave a lot to be desired; but, more important, Kemper has no specific information on the issues and personalities involved in the origins of any of the (nineteen) post-1865 *nikāyas*, and one wonders how he will convince his readers that he has made an attempt to understand “the diverse origins of the Amarapura *nikāyas* as a whole.” I find no evidence in his paper that he has done the basic historical research that such an understanding presupposes. I am not even sure whether he knows where some of these *nikāyas* are situated. He takes it for granted that all twenty-five Amarapura *nikāyas* are relevant to the discussion of what the title of the paper calls Low Country Sri Lanka, and the abstract of the paper refers to “the establishment of twenty-five such *nikāyas* in the Low Country of Sri Lanka” (p. 9). In fact, about a third of those *nikāyas* were not established in the Low Country (but in the Kandyan provinces).

With this record of research, the level of discussion in the essay drops from the definite promise made at the end of its introductory section to the miscellaneous generalities in the following three sections, until the penultimate section presents a kind of case study of one fraternity: the Dhammarakkhita fraternity (which had its origin in the pre-1865 period). Although the quality of information in this section is improved, it has nothing to do with segmentation. Other fraternities, which began at about the same time as the Dhammarakkhita fraternity, experienced real segmentation in the sense that *groups of monks* broke away from their original fraternities and formed *new fraternities*. This phenomenon, as Kemper’s own account shows, has never occurred in the Dhammarakkhita fraternity; instead, only *individual monks* have left the fraternity and joined other *already existing fraternities*. That is not segmentation; Kemper could not have chosen a worse case to study than the Dhammarakkhita fraternity.

On this case study, in contrast to his “general study” (of the nineteen post-1865 *nikāyas*), Kemper has devoted some time and effort although it is not so much a study of the Dhammarakkhita fraternity as such as of its one-time headquarters—a monastery at Dondra. Even in this limited form it would have constituted a useful addition to the literature on Sinhalese Buddhism, but Kemper has adulterated it with superficial inquiries into other fraternities, and he has tried to extract from this mixture new insights into wider issues. Thus, while describing a dispute that took place over the incumbency of the monastery at Dondra, Kemper interjects: “What

followed was the kind of confrontation that has led to the creation of a number of the later Amarapura *nikāyas*—the squaring off of two or more candidates for the office of chief monk. Here the office in question was that of chief monk of a monastery, there the office has been that of chief monk of a *nikāya*" (p. 38). But if Kemper's interest was in segmentation should he not have been "there" rather than "here" and found out which particular *nikāyas*, how many, and why? Almost all *nikāyas* have had conflicts over their office of chief monk, but not all those conflicts have resulted in the creation of new *nikāyas*. Is it not necessary, therefore, to find out the circumstances in which segmentation did—and did not—occur?

Having commented on the new evidence that Kemper has brought into the discussion of segmentation, I now turn to his new interpretation of the phenomenon. His standpoint here is that segmentation should be seen as resulting from a variety of factors (such as caste and regional differences, and conflicts over ecclesiastical offices) and not from just one factor. This is a sensible standpoint, but it is not new; and the only way Kemper can hope to prove its supposed novelty is by arguing that others have preferred a one-factor explanation. Accordingly, Kemper constructs such an explanation—with reference to what he calls reform (and which he defines as "return to orthodoxy")—and he donates it to two parties: to the community of monks and to me.

Concerning the views of monks, Kemper states: "They too have an answer why small groups of monks have frequently broken away from the established monkhood. The answer is *Vinaya* (the monastic code for conduct), each new group asserting that *its* interpretation is the orthodox one" (p. 28).

It is laughable to suggest that there are as many different interpretations of *Vinaya* current among Sinhalese monks as there are fraternities among them (twenty-five interpretations among Amarapura monks alone!), but, more to the point, the history of the monastic order is a subject on which monks have written considerably, and these works—with none of which Kemper shows any familiarity—reveal their awareness that diverse reasons have produced divisions within the order at different times and places. (Several of these works are cited in my book, and there are others, published since then or dealing with the more recent period.) Even though Kemper's paper displays a concern with some 175 years of monastic history, only two monks are quoted in it (one through an interview and the other through a newspaper report), and one of those two monks smuggles in an explanation of *nikāya* differences (p. 34) that contradicts the view that Kemper attributes to monks in general. That is not the only internal contradiction on this point. If, as Kemper claims, the monks do explain the origins of all new fraternities in terms of disagreements over *Vinaya*, then, with increasing segmentation, there should also occur more and more discussions and debates on rules of monastic conduct. Kemper tries to demonstrate the opposite of such a trend when he asserts that "the monks stopped talking so much about *Vinaya* as the nineteenth century progressed" (p. 29).

With regard to my views, Kemper states: "In linking reform and the establishment of new monastic groups, Malalgoda sounds like the monks themselves" (p. 28). He qualifies this statement later by adding that "Malalgoda points to other factors for the rise of new *nikāyas*" (ibid.), but he does not specify clearly what these "other factors" are, and, in moving toward the conclusion that "reform alone never seems to give rise to new *nikāyas*" (p. 40), he proceeds as if he had made no such qualification. (What is the purpose of arriving at that negative conclusion except to refute a previous or presumed affirmation that reform alone *has* given rise to new *nikāyas*?)

The “other factors” do turn up, however, in Kemper’s criticisms of the reform thesis. Thus, in a challenge to this thesis, purporting to come from his researches into the twenty-five Amarapura fraternities, Kemper says: “Some of these twenty-five *nikāyas* are linked to a specific region, caste-bound, and influenced by the one-time state-religious system only to the extent that after 1815 no *raja* existed to check their growth. Their establishment has nothing to do with reform” (p. 30). This challenge—directed at what he had set up as my explanation of segmentation—reads like a rehash of some of my own findings, summarized in the conclusion of my book as follows: “Once begun, and with no political backing to a central ecclesiastical hierarchy to hold the order together, there was no effective check to the process of segmentation. . . . It could proceed almost endlessly on the basis of caste, subcaste or regional differences, or clashes of personalities or doctrinal disputes” (1976:259).

This passage, for all its brevity, should make it plain that I too, “like the monks themselves” (but, I hope, somewhat more systematically than they have done), have explained the origins of new fraternities not in terms of one factor but a variety of factors; and, as far as tracing them all to reform or return to orthodoxy was concerned, I made my position clear when, arguing against a mid-nineteenth century observer who subscribed to such a view, I wrote: “Revival in such a literal sense as an attempt to take Buddhism ‘back to its early principles and doctrines’ does not provide an entirely satisfactory description of the rise and development of the Amarapura fraternity, nor of the other new fraternities of the time” (1976: 257).

Finally, Kemper has put forward as a methodological point that segmentation is best handled in local, not national, studies; “if we focus on the national level,” he claims, “the monkhood tends to look like a unitary organization, and the discourse of reform diverts our attention from the local social forces that shape monastic organization” (p. 29).

It is certainly important to examine “the local social forces that shape monastic organization” (and that precisely is what Kemper has failed to do with regard to any of the post-1865 *nikāyas*, whose very *locations*, as noted earlier, he has not identified properly), but nothing is gained by creating a methodological scare that “if we focus on the national level, the monkhood tends to look like a unitary organization.” The examination of local variations is not excluded from national studies; on the contrary, a nationwide perspective is essential to assess the nature and significance of local variations. What is impossible, on the other hand, is to study most fraternities, including the Dhammarakkhita fraternity, from the limited perspective of one small locality (like Dondra) because their monks and monasteries are distributed over several different localities.

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Reform, Segmentation, and Explanation: A Reply to Malalgoda

From the beginning of Malalgoda’s account in 1750 to the present, the Sinhalese monkhood has been characterized by the movement of monks between monastic groups. A new monkly group forms when monks leave their parent group, or an individual monk, with or without his students, breaks away. Individual monks usually join another group for logistical reasons, although the case of Attudawe Dhammarakkhita is an exception (he built his *nikāya* around his own students);