

Nationalism and Religion in Southeast Asia

A Review Article

DR. VON DER MEHDEN'S BOOK, *Religion and Nationalism in Southeast Asia*,¹ is divided into two parts. In the first he "attempts to put the relationship of religion and nationalism" in Indonesia, Burma and the Philippines "into religious and historical perspectives." In the second he considers more especially the influence of religious personnel in these three countries—*hadjis* and *ulamas*, *pongyis*, friars, missionaries—on the nationalist movements. The book makes interesting and fluent reading but I find it difficult to evaluate for a number of reasons. Among these are my own attitudes and involvement in the field covered, and it may be as well to own up first to no small degree of interestedness in the issues at stake.

An initial difficulty is the identification of the public for which this book is written. To a British scholar, there are basically two potential publics: that of the "general reader" and that of the specialist. From this point of view the book falls between two stools. Insofar as it takes for granted a considerable amount of knowledge about the areas and is built upon the writings of other scholars without acknowledging their existence other than tangentially (e.g., pp. 17, 28, 46), the book is directed at the specialist without satisfying him that the requirements of scholarly respect and dignity have been complied with. It is as if the work of such writers as Furnivall, Cady, Tinker—to name but three in the Burma field—had not appeared or did not raise issues which need consideration before a new author can pronounce on the subject. The author knows his material and some justice has been done to it but justice is not shown to have been done. On the other hand, precisely because the book "replaces" others in this manner, it could be thought to aim at a general reader and to want to tell him all he wishes to know about the areas. Yet again and again this view has to be relinquished because the general reader could not be expected to pick up certain references or technical terms without much more help than he is afforded (e.g., on the Burma State Religion Act, especially at pp. 106-7). To invoke a third possibility (of applicability to the U. S.) in the persons of a large body of general "social scientists" who can

¹ *Religion and Nationalism in Southeast Asia*. By Fred R. von der Mehden. Madison: University of Wisconsin Press. 1963. 253 pp. \$5.00.

Nationalism and Religion in Southeast Asia

be expected to pick up a subject on the wing, as it were, does not really clarify the issue. Dr. von der Mehden has also clearly been the victim of a dateline: the book shows many signs of haste, including misprints and curious transcriptions such as "U Ottuma."

Two possible sources of confusion may be noted. It is hard not to feel that the book has been written back to front. Part II would have fitted naturally into first place insofar as the description of individuals and particular religious groups would seem to be the first task of an author describing relatively unfamiliar areas; after this the more generalized material in Part I would be more easily followed and digested. The second point, which evokes my deepest objections, involves the author's coverage of the three areas and the way in which this is done. At the outset the author claims that Burma and Indonesia will receive more or less equal treatment, with the Philippines as a border case owing to the vital fact that the colonial powers arriving there did not have a major or "Higher" religion to contend with, as with Islam in Indonesia and Buddhism in Burma. In fact, the first part of the book is primarily concerned with Indonesia, Burma being used here as illustrative of certain selected points. Only when the author is able to bring in his own field work, in Burma in 1959-60, does Burma really spring to life and one feels that the foregoing is scarcely more than a setting for this fieldwork—the original and valuable, though clearly insufficient, justification for the book. The Filipino material is rather repetitive and tends to fade from the reader's mind; the impression persists that elementary geography is a rather opportunistic basis for comparison and that relating the Philippines to Central America, for example, would have taken us just as far. On the other hand, a problem of method arises rapidly: the author is comparing uncertainties rather than certainties. We are told (pp. 36 and 45-46) that the Indonesian material is difficult to evaluate for a number of reasons. Yet this is immediately used to provide a *basis* for the Burmese material which follows. Such a procedure does not inspire confidence, and when it is considered that this very broad subject is fitted into about 250 pages, it is hard to escape the feeling that the author is skating like a virtuoso over three deep ponds on a fine summer day.

The unease deepens when we consider what it is that Dr. von der Mehden is building on. Would the work of other authors indicated in fact provide a strong enough basis? (I do not feel qualified to judge the Indonesian material [largely and probably usefully translated from the Dutch] and I deal here mostly with Burma.) The answer must be no, for the truth is that the sociology of religion in Burma does not yet exist.

No doubt, an acceptance of this fact would have involved the author in either writing a far longer book or in not writing one at all. Dr. von der Mehden is highly skilful in disguising the fact, for the general reader, that we have no Burmese religious sociology: he has a flair for the few available

sources to date, used with or without acknowledgement; he says just enough about the tragic lack of statistics and his coverage is wide enough in appearance for some to be persuaded (when so many disclaimers as to what the book *is not*, e.g., pp. xiii, 49, and formulae of the "partially due to" type are also considered) that there is more would he but tell it. He ably gives the impression that he is running after a good deal of walking: my contention is that it is as yet barely possible to walk. This brings me to the genuine evasions, visible to the specialist, which lie at the heart of much self-styled "analysis" of Southeast Asia of the type exemplified by this book. As an example, consider the superficiality of approach in a statement like "It is sufficient to state at present that the identification with the majority religion was the important political fact to be weighed and not the sophistication of the faith of the adherent" (p. 4). This opens the way to the use, yet again, of perfectly meaningless census tables such as those quoted on p. 6. To speak plainly, when the author says "analysis" we can read "description"—partial at that—in virtually every case, and the description is of a historical process which is more firmly implanted in the author's mind, his own *weltanschauung*, than in the facts of the case themselves.

To begin at the beginning. There is not yet in existence a body of information about the Burmese monkhood which comes anywhere near telling us what their role in nationalism may or may not have been. We have no reliable statistics on the monkhood whatsoever; nothing on the organization of the monkhood, its hierarchy, its sects (the latter highly important in my estimation but, significantly, completely overlooked in this book), its parapolitical bodies modelled on the sects—nothing in short which could lift a description of monkish activities between 1915 and 1960 above the journalistic, impressionistic level. The common assumption, picked up by Dr. von der Mehden, that the monkhood was better organized and more closely linked to the state in royal times than it was after the British came, requires much qualification. The equally common assumption that the monkhood lost its role in the governance of Burma, in education and social welfare—again picked up here—pays no regard to the fact that, through the sects, the monkhood was actually reorganizing itself on *different* lines against both the British *and* (later) against the independent State's designs upon it.

In short, by locating a few references in reports about the participation of some, barely adequately described monks (an admitted minority) in nationalist movements, the author makes it appear as if he is telling the whole story. For him, the fact of monastic participation, to some degree, is all important to his thesis. For me, the greater degree of monastic non-participation is far more crucial. But this would mean telling a different story and with quite a different viewpoint on the whole history of colonialism. To this I shall return below but may summarize by saying that whereas, for the "political scientist" the history of a "new nation" involves mainly the discontinu-

Nationalism and Religion in Southeast Asia

ities of the pre- and post-colonial situation, the "political sociologist," flying in the face of many superficially admitted "facts," is impressed by the basic continuities, by the lack of basic change in patterns of religio-political authority and social control evident in the history of Southeast Asia up to this very day. That this involves a radical reappraisal of Furnivall's important contribution is not seen by Dr. von der Mehden.

Coming to such a study with a predominant interest in religion rather than in politics may lead one into stressing one's own study of the former and leaving the latter to the political scientist. Tempting as this division of labour may appear, it must be resisted. For it must be clear that writing of religio-political relationships without knowing one of the poles of the debate is equivalent to not starting at all. The author does not evade the issue by claiming that his book is not a politico-religious study but one on nationalism and religion. I cannot see wherein nationalism can be defined outside the field of "politics" (cf. p. 96. The author's naiveté of approach can also be pinpointed at the second paragraph of p. 108 and at pp. 161-2 where the lack of a true grasp of the nature of Burmese ideas about authority and social control drives him back to locating a "paradox," stated but not explained by him, in the personality of a single politician!)

I have tried to show that the sociology of religion in Burma is not yet born. I would add that the nature of Burmese Buddhism itself is not yet understood. Throughout the book the author is content to take at its face value the understanding that there are in fact such abstractions as "Buddhism" and "Islam." The first thing this leads him into is disregard of "animism," though it continually exists in all the Buddhist situations we know of (e.g. on p. 3: "In spite of charges that these Asian peoples practice some animism. . . ."). This cannot be done because it is precisely the "animist" component in the almost inextricable blend of Buddhism and animism that gives us the key to the realities of Burmese religio-political behaviour. Broadly speaking—I have gone into this elsewhere at length—it is the animistic component present in Buddhism (*ab initio* as well as through historical overlapping in any particular context) that determines the individual search for power (or "individual autonomy," as a psychologist has defined it) in the teeth of Buddhist theoretical abnegations of power. It is this component which explains and justifies the motivations of Burmese politicians and, on this score alone, the author *should* have paid more attention to "theology"—to karmic theory, to merit-acquisition, to *nat* worship, to Messianic Buddhism and other such matters in which the will to power is exemplified. Dr. von der Mehden touches very briefly on Indonesian messianism, leaving Burma virtually out of account, and never comes to grips with the implications of its existence, *inter alia*, for right-wing nationalism. The work of Sarkisyanz alone *should* have taught him better. And one cannot help noticing either that even the items on the sociology of the *Sangha*

itself (Cady, Hobbs, Paw U) mentioned in the bibliography are not recognized in the text.

The illusion of certain political scientists seems to be that by overhearing what a prime minister says to his chief monk, or priest, he can ignore the vast and unknown majority of ordinary priests and ordinary laymen. Because Western progress and political models (with which he is constantly comparing the "emerging nations") engage his attention almost exclusively, because he is committed to a fundamentally ethno-centric task, he is apparently debarred from understanding that the history of a nation is not made through ministers and chief priests alone. A host of compilations, written mainly in the U. S. since 1945, attest to the fact that the same data can be served up again and again (the same research too!) without any progress being made in the grass-roots sociology of the areas concerned. To pile comparison on top of this, as is frequently done at the descriptive rather than analytical level, is to mislead the public still further. The task in hand is admittedly immense and is not aided by the cardinal sin of inefficiency which plagues the record offices of new nations, by the political insecurity reacting on the observer's own mental and physical health, and by the desperate need for quickly-forged "tools of policy" on the part of policy-hungry Western, and Eastern, governments. Yet the task was well begun by the 19th century chroniclers, writers of gazetteers and devoted scholars in the early twentieth century, and it is being continued in a modest and patient way by a number of anthropologists, British and American. Rather than floating on air, we need more true analysis, more genuine comparison and this can be done only by recognition of the virtues of walking rather than running—by real statistics, real local histories, real sociological studies of limited units, real analyses (and informed ones at that) of ideologies, and above all at this stage, by the knowledge that we do not yet possess such things.

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