

Communications to the Editor

A Reply to Lieberman

Victor Lieberman in the August 1980 (39,4) issue of this journal has reduced and changed the emphasis of my article (*JAS* 38,4 [August 1979]: 671–88). Then, by criticizing that reduced version and changed emphasis, he depicts his paper as a major *alternative* to mine when in fact it fills a gap I had acknowledged needed to be filled. The essential difference between our articles is one of emphasis: I give major weight to institutions in historical causation, and he stresses events. However, when he views my model—a complex, flexible, and accommodating one—as “mono-causal,” it is clear that he is confusing events with institutions. No one denies that events are important in their own right—that history is linear as well as cyclic (indeed, I am satisfied with Vico’s concept of history as “progressive spirals”)—but it is misleading to suggest that the exclusion of this linear dimension was a serious oversight on my part rather than a conscious attempt at isolating, identifying, and selecting a dimension closer to my interests. This is, in any case, the type of broad issue that will never be resolved. What is more serious is Lieberman’s uncritical reliance on secondary source material and manipulation of primary data to serve his conclusions with regard to my article.

Essentially, his critique rests on an obvious strategy: *Minimize* the amount of endowed religious lands, *maximize* the total cultivated area, to arrive at a much reduced percentage of lands held by the *sangha*. The latter depends upon two sets of figures: mine, which show approximately 301,796 acres; and Than Tun’s, which show 42,149 acres. Lieberman clearly accepts *prima facie* Than Tun’s estimate without subjecting it to the elaborate scrutiny mine had to face. Then, by increasing the estimated total cultivated acreage in Upper Burma, he was able to “show” that the amount donated to the church was unimpressive.

Let me choose but two inscriptions out of a total 500 originals I used as my data base: the Dhammarajika Inscription of 1196–98 and the Natonmya Inscription of 1207. The first records the donation of approximately 11,832 *pays* (about 20,706 acres) of *productive* land. (Of that, more than 5,682 acres were of the best, perennially irrigated crown lands in Kyaukse). The second inscription records approximately 27,000 *pays* (about 45,250 acres), again of *productive* land.¹ These two inscriptions alone, representing barely three years, have already surpassed Than Tun’s total estimate for 200 years by 3,515 acres. Lieberman further attempted to minimize my total estimate by distinguishing between the types of land donated to the *sangha*: irrigated, nonirrigated, garden, etc. But it is a distinction without much significance, for land donated to the Buddhist church was rarely unproductive, especially toward the end of the dynasty—which is, after all, the point. It is extremely rare to find wasteland (*mle ruiñ*) in gifts to the *sangha*, and for a good reason: One’s merit was

¹ U Aung Thaw, ed, *Shwe Haung Myanmar Kyauksa Mya* [Ancient Inscriptions of Burma], 1 (Rangoon: Ministry of Union Culture, 1972): 65–69; 83–86.

equivalent to the quality of one's gift. Any type of productive land given to the church, therefore, became a drain on state revenues, especially when labor was invariably attached to such endowments.

Lieberman's suggestion that I might have duplicated records is pertinent but highly improbable. Each inscription normally possesses a date, the donor's name and rank, the names and relationships of the attendants assigned to the endowment, the quantity of land and its boundaries, the names of people who witnessed the ritual, and, often, even the name of the clerk or official who "wrote the document." Moreover, of the relatively few inscriptions recorded in the same year, I know of none recorded on the same day. When arranged chronologically then, duplication is easy to spot. The safeguards imposed by the state on legal donations further reduce duplication "in the field," i.e., at Pagan itself. Donors go to great lengths to show clear title, and one could not simply steal land to redonate it for merit; indeed, if one stole, it would be to keep it—in which case (unless the thief confessed), the official files would continue to show such land as religious and therefore still tax-exempt.

Having attempted to reduce my total estimate, Lieberman moves on to maximizing the total cultivated area in Upper Burma, perhaps the most careless part of his analysis and the least convincing. The British report, from which his figures were derived, does give the total acreage of cultivated lowland in Upper Burma as 3,000,000, *but in 1889!* Lieberman states parenthetically that his source is the same report from which I had obtained my figures for the total cultivated area in Upper Burma. And herein lies the mistake: the estimate I used is a projection of the cultivable area during "Burmese times"; while Lieberman's 3,000,000 is the estimate for 1889. The year of the report and the estimate for that year are immaterial to other estimates dealing with earlier times, even if in the same report. Indeed, the total cultivated area in 1881–1890 for *all* of Burma, including the enormous and rapid increases of cultivated land in Lower Burma between 1852 and 1890, was 4,723,200 acres.² Even U Nu's 1956 four-year plan could not command more than 1.4 million acres of paddy from the *entire* country, lower than Lieberman's 1.5 million acres of paddy projected for thirteenth century Upper Burma alone! Lieberman's 3,000,000 acres of crops would mean, furthermore, that if Upper Burma in the thirteenth century had even one-half the population of *all* of Burma (a little over 3,000,000 in the nineteenth century), there would have been for every man, woman, and child in Pagan, two acres of crops or one acre of paddy.

Lieberman then gives uncharacteristically low figures derived from the Restored Taung-ngu period to show "a change" in patronage patterns. He estimates that between 10,667 and 27,773 acres were alienated to the church in 1550–1750. Part of the reason for these lower figures is that the zenith of the Taung-ngu dynasty, when most donations would occur, was in Pegu, in "monsoon" Lower Burma, where relatively few inscriptions have survived. A more obvious reason for these lower figures, however, is Lieberman's careful qualification of what he considers "relevant" data and the arbitrary rearrangement of dates to serve his conclusions. At first glance, his chart seems to support his contention that donations were indeed negligible in "his" period, but, when scrutinized closely, both his qualifications and dates assigned

² *Imperial Gazetteer of India*, new ed., 9 (Oxford: Clarendon Press, 1908): 235. For the high rate of increase after British occupation, see Michael Adas, *The Burma Delta* (Madison: Univ. of Wis-

consin Press, 1974), p. 22, and Cheng Siok-Hwa, *The Rice Industry of Burma, 1852–1940* (Kuala Lumpur: Univ. of Malaysia Press, 1968), p. 25.

to each period, two clear reasons for that reduced number emerge. It is Lieberman himself who has given the Ava period the dates 1365–1555; no historian of Burma has ever before (including Lieberman) extended the Ava period to 1555. (His own thesis shows the beginning of the Taung-ngu period as 1486.) The reason for extending Ava's dates is obvious to me: It excludes from his analysis (and, therefore, his conclusions) approximately fifty-five additional inscriptions recorded after 1486, now safely tucked away in his redefined (and, therefore, "inapplicable") Ava period. He then further qualifies the data by stating that all inscriptions of the First Taung-ngu period prior to 1551 are also excluded, because Upper Burma, he argued, "was still under Ava's control." In addition to being immaterial to the issue of land alienation, this arbitrarily excludes the records from the reigns of Min Kyi Nyo and Tabinshwehti, two of the most successful kings of the Taung-ngu dynasty. By doing this, he eliminates the very type of period in which one finds the largest number of donations to the *sangha*. So few inscriptions are left after such careful qualification that the Restored Taung-ngu period indeed gives the appearance of a significant exception to the rule.

In addition to all this, the source for his data is outdated: Charles Duroiselle's *A List of Inscriptions Found in Burma*, on which Lieberman bases his critique, states clearly in the introduction that it does not include about 300 inscriptions at the time of its publication in 1921, and it certainly does not include the many inscriptions found during the following fifty-nine years, thirty of them in one find during 1962–63.³ Most importantly, Lieberman forgets one important fact: Lands donated to the *sangha* were *in perpetuity*, which means that the total acres donated during the Taung-ngu period *must be added to the lands already donated in the past 500 years*. Indeed, it is precisely because land was given in perpetuity to the *sangha* that each new dynasty had less productive land in its control and explains why successive dynasties may have donated less land in absolute terms, if in fact they did.

Knowing that legitimacy was still part of religious patronage, Lieberman then suggests that cash expenditures must have replaced land endowments, thereby allowing the state to continue such patronage while retaining its landed resources. But cash expenditures in themselves were nothing new: The donors of Pagan paid for the actual construction of temples in cash. (It cost King Narapatisithu 44,027 *kyats* of silver for his Dhammarajika Pagoda mentioned above, in addition to the land endowed.) Thus, the use of cash itself need not imply an alternative method of religious patronage; it certainly does not suggest that the state's *relative* expenditure (relative to its wealth) was reduced. To be legitimate, the state had to continue to patronize the church; only, by the sixteenth century with its increased commercial activity, the state may have donated less in land simply because it had more cash and less land. Indeed, one of the reasons Burman kings shifted their center of political power in the sixteenth century from agricultural Upper Burma to commercial Lower Burma, and thereby adopted another mode of generating revenue, may well have been the pressures exerted by the loss of productive lands in Upper Burma to the *sangha*. That religious patronage continued at a relatively similar level into the nineteenth and even twentieth centuries is clear. Of many Konbaung dynasty examples, King Mindon's Kuthodawgyi Pagoda should suffice. Built for the fifth Great Synod which he convened, this temple, surrounded by 729 marble slabs on which were inscribed the entire *Tipitikas*.

³ *Shwe Haung Myanma Kyauksa Mya*, p. *ga*.

cost an estimated 226 million rupees.⁴ Melford Spiro's study of a small but typical Upper Burma village in the 1960s shows that 30 to 40 percent of its annual income went to religious giving, not to mention time and energy.⁵ In 1953, when land was to be nationalized, many landowners donated their holdings to the *sangha* rather than "give" them to the government, in order not to jeopardize their chances for a better rebirth—precisely the type of reasoning behind similar actions of the rich in premodern Burma.

When everything has been said, however, the real issue may well be neither 10 percent nor 60 percent, but the meaning of these figures. Can we say that 60 percent is more significant than 10 percent simply because it is larger? Gregory Bateson⁶ and others would say, "No." In a delicately balanced economy, 10 percent may well have far more impact than 60 percent would in an elastic and fluid economy. Being aware of this, the quantification in my article was used merely as support for the thesis by the type of evidence historians are inclined to accept as more valid. The point of my article had more to do with the *relationship* between the alienation of land and political ideology than with the amount of land itself.

Anthony Johns, almost twenty years ago, warned us not to allow the mere passage of time to become the criteria for establishing "periods" in history, because it creates artificial boundaries where none may exist.⁷ Lieberman's article is perhaps an example of how dates have dictated and confined his view of change and continuity, to the extent that his article seems to say: "Your model does not work in *my* period," which is the historian's equivalent of the anthropologist's, "not true in *my* village."

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The Use of *Pinyin*

It is hard to help feeling that the editorial decision announced in a recent issue of *JAS* (38:4 [1979], 649) to initiate a transition to the uniform use of the *pinyin* romanization of Chinese in future issues of our *Journal* is both unfortunate and premature. One would have thought that a question recognized to be "difficult and troublesome . . . for a great many people in the China field" would for that very reason have called for careful and reasoned discussion in the pages of *JAS* before a decision to "firmly promote" *pinyin* was made. To dismiss such discussion in advance as "wrangling" is hardly consistent with the attitude proper to a scholarly journal.

To be sure, much of the discussion that has taken place hitherto has been so marred by muddleheadedness and political posturing on both sides that impatience with it is understandable. At the risk of being branded a wrangler, however, I venture to raise the subject with you myself for two reasons. First of all, I do not see that the real reasons for using either Wade-Giles or *pinyin* are much considered in discussions of the problem; and, second, it seems to me that the approach that you are proposing to adopt is likely to prove destructive in several ways.

⁴ Ludu Daw Ahmar, *The World's Biggest Book* (Mandalay: Kyipwayay Press, 1974), p. 20.

⁵ Melford Spiro, *Buddhism and Society: A Great Tradition and Its Burmese Vicissitudes* (New York: Harper and Row, 1972), p. 459.

⁶ Gregory Bateson, *Mind and Nature: A Neces-*

sary Unity (New York: E. P. Dutton, 1979), Chap. Two, pp. 25–64.

⁷ Anthony H. Johns, "Sufism as a Category in Indonesian Literature and History," *Journal of Southeast Asian History* 2, pt. 2 (1961): 10–23.