

Communications to the Editor

TO THE EDITOR:

Katherine Bowie's article, "Unraveling the Myth of the Subsistence Economy: Textile Production in Nineteenth-Century Northern Thailand" (*JAS* 51.4 [November 1992]:797–823), presents three inter-related arguments. First, that the peasant communities of premodern northern Thai society were not egalitarian. Second, that villagers were neither self-sufficient nor well-off. They thus, third, were compelled to engage in trade to meet their subsistence needs. While I generally agree with these points, I nevertheless find some confusion in these arguments. First, what does it mean for a society to be egalitarian? Much of Bowie's argument is aimed at showing enormous differences in wealth between lords and commoners. She is certainly right in this, but this point hardly seems novel or relevant. Of course the ruling classes were richer than the ruled in northern Thailand. At some points in her discussion, however, Bowie argues that there were important economic differences among peasants within the villages. This seems, on the face of it, far more relevant to a critique of the egalitarian-subsistence romance of the Thai peasant. A second dimension that is lacking, however, is a consideration of more complex status differences of a type that are socially and symbolically constructed and cannot be reduced to considerations of wealth.

The notion that the lords in the old northern Thai kingdoms were richer and had nicer clothes than the peasantry does not in itself contradict the common picture of the region as one of egalitarian, self-sufficient peasant communities. The concept of the Asiatic mode of production as formulated by Marx, for instance, sees the peasants as living in communities that approach a state of primitive communalism and the lords as a superposed, external group (Marx 1965:69–71). This is not to say that Bowie is therefore wrong in her picture of rulers and subjects (indeed, she is surely right), but rather that the relevance of the evidence she cites on the lordly standard of living to her argument about the villages is not clear. Far more important would be evidence on the internal differentiation of wealth and status within and between peasant villages. We know, for instance, that some villages (notably those close to a capital town) were directly ruled by an aristocrat holding a princely title, but that most were not. People holding "noble" titles (that is, non-hereditary state titles) could be found almost everywhere. This can be read, however, either as an extension of the state down to the village level or as a symbolic incorporation of local leadership into an external state structure. In either event, it tells us little about what we really want to know: How marked were economic and status differences within the villages? Bowie's brief discussion of rural wage-labor and poverty points in this direction, but one wishes that this issue had received more attention. Particularly important would have been statistical data showing village economic differentiation. As it is, we do not know if the misery she describes was the rule—in which case the free peasantry was self-sufficient, but only at the meanest level—or the particularly vivid memories of a relatively small number of poor peasants who lived to tell the tale.

This brings us to a consideration of self-sufficiency and social class within the village. Bowie brings some evidence to bear on the question of differential access

to cloth, citing this as both an index and a substantive aspect of wealth differential. The examples she cites, such as endemic theft of cloth and clothing, are telling. (I can say from personal experience, by the way, that the theft of clothing remains a living tradition in Chiang Mai.) When we consider, however, that cloth served as a form of money, the theft of cloth appears in a somewhat different light. It may well have been stolen for its exchange value as much as its use value, so to speak. This does not contradict the point that cloth was a scarce good, but it does require us to consider cloth as more than a practical item in the narrow sense. A case in point is the presentation of robes to monks in various ritual contexts and of new clothing to elders at New Year, two examples cited by Bowie. She takes this as a simple indication of cloth's value. I take it as an indication of cloth's complex social signification, which certainly cannot be separated from its practical value, but also surely cannot be reduced to it. The presentation of specific types of clothing to specific persons on specific occasions is part of an ongoing process of symbolic social reproduction and not simply a form of utilitarian economic exchange.

We do not know much about the internal social structure of premodern northern Thai villages, but we know that it was neither simple nor strictly egalitarian. Genealogies taken in villages today often go back to a nobleman or aristocrat, in a Southeast Asian version of what David Schneider called the "famous relative" phenomenon in *American Kinship* (Schneider 1980:67). Chronicles also reveal a complex set of relationships between court, capital, and village, sometimes mediated through Buddhist institutions. (See Rhum forthcoming, and also Calavan 1974.) The point here is that Bowie does not go far enough in her critique of the romantic Thai village. It is not simply that villages were forced by economic necessity to engage in trade and therefore were implicated in a larger world, but rather that they were both constituted by and constitutive of that world. The greater wealth of the lords is significant not because it proves that northern Thai society was not egalitarian (which is accepted by both the royalist and Marxist sources she cites), but because their status, and the trappings that went with it, were part and parcel of a total social system in which free peasants and slaves also figured integrally. This in no way implies that the old northern Thailand was a happy little world of contented peasants and benevolent lords. It does mean, however, that the hierarchical (and often brutal) nature of the system was constituted in its historical specificity and not in some material aspect of its "economy" that can be cleanly distilled out. In Marshall Sahlins's terms: "An 'economic basis' is a symbolic scheme of practical activity—not just the practical scheme in symbolic activity" (Sahlins 1977:37). It is interesting that as examples of the splendid clothing of the old lords, Bowie cites their "state robes" (*JAS* 51.4 [November 1992]:811). Granted that lords dressed better than commoners, it is still important to understand that state robes are more than just clothing. They are indicators of power and in some subtle way help to create power, as Geertz has noted of state pomp in general (Geertz 1980). If I go to the market and buy a state robe, then by that fact alone it is no longer a state robe. Wealth may be an instrument of status, but status cannot be explained in terms of its instruments.

MICHAEL R. RHUM
Northern Illinois University

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KATHERINE A. BOWIE REPLIES:

My *JAS* article challenged the prevailing characterization of Thailand as a "subsistence economy." I argued that "the subsistence economy paradigm has underestimated: (1) the degree of specialization of labor, (2) the magnitude of economic class divisions, (3) the scope of poverty, (4) the role of trade, and (5) the extent of changes in production and consumption" (1992:798). In his comment, Michael Rhum admits to a certain confusion, a confusion made evident when he reduces my five-point critique to a mere three. Nonetheless, given that my article attacks the hitherto pervasive self-sufficiency paradigm, I am pleased to read that Michael Rhum "generally agrees[s] with these points" and believes that I am "certainly right." Despite his apparent agreement with my argument, Rhum raises two issues of concern. First, he seeks clarification regarding my view of the extent of class stratification in northern Thai society. Second, he wants to emphasize that textiles should be understood as "part of an ongoing process of symbolic social reproduction" rather than "simply a form of utilitarian economic exchange."

Rhum agrees that northern Thai society was stratified into overall categories of lords, peasants, and slaves. He also accepts my argument that there were significant differences in wealth between peasants and lords. Instead, Rhum's concern lies with determining whether I am also arguing that villages were internally stratified as well. I am indeed arguing for the "whole nine yards"—that both the society overall and the villages internally were stratified along a continuum ranging from the desperate poverty of beggars to the silken luxury of the lords. Rhum does not make an argument either for or against internal village stratification. In fact, his very reluctance to conceive of internal differentiation can be construed as a testament to the remarkable hold of the subsistence economy paradigm. How can Rhum generally agree that "peasant communities of premodern northern Thai society were not egalitarian," "neither self-sufficient nor well-off," and "compelled to engage in trade," and still wonder whether there was significant internal village differentiation? Once it is conceded that villagers were linked to the process of textile production, distribution, and consumption through complex divisions of labor as part of a dynamic, market-oriented society, how could all villagers possibly acquire and maintain the same level of wealth? In my dissertation and in another article currently under revision, I consider the issue of internal village stratification in further detail (1988, n.d.). In the subsection of my *JAS* article addressed to the issue of class, due to space limitations, I simply focused on establishing the dramatic extremes in the continuum of socio-economic differentiation.

Rhum's second point is less clearly articulated, but in essence he is suggesting that I have reduced cloth to its material aspect and ignored its symbolic dimension.

With regard to this point, Rhum does not do me justice. My discussion of textiles in the *JAS* article was in order to make a more expansive argument about the character of the northern Thai political economy: it was not intended as a study of the social significance of textiles per se. I readily agree that textiles are “part of an ongoing process of symbolic social reproduction” and I would refer the reader to my article in *American Ethnologist* where I consider exactly this issue. Nonetheless, I do not believe it is possible to understand the symbolic dimensions of a society without an accurate understanding of its economic structure. My *JAS* article was an effort to establish a more accurate material foundation for the analysis of the social fabric of northern Thai society.

KATHERINE A. BOWIE
University of Wisconsin-Madison

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TO THE EDITOR:

As the readers for Duke University Press in its consideration of Jing Wang’s *The Story of Stone* for publication, we were pleased to see this significant new book reviewed in *JAS* 51.4 (November 1992):910–12. We were also delighted that the reviewer, David Rolston, recognized that it is full of “important insights” and that it has the potential to “loosen up some of the frozen paradigms that exist in the ‘sinological’ study of Chinese fiction.” Mixed with the praise, however, is a certain amount of criticism which we consider to be unjustified and to which we would like to respond briefly:

1. Rolston asserts that a book that is comparative in perspective should make more than “random references to Western literature.” We believe that this is an antiquated notion of what the enterprise of comparison is all about nowadays, especially with its emphasis on interdisciplinary studies.

2. The reviewer complains about the abbreviated style of footnote references and states that it “is surely the cause for the high number of citation errors.” We are not persuaded that the book truly has a large number of citation errors. Furthermore, many style sheets (including that of this journal) recommend an even shorter form. In any event, it would be better to focus on important issues than on mechanical matters that are often out of the hands of the author.

3. The reviewer states that Wang “fails to bring out what is unique about the place of stone in Chinese culture.” The assumption of the utter uniqueness of any

element of a given culture is problematic. Wang quite responsibly shows how China shared certain aspects of stone symbolism that overlapped and intersected with that of many other premodern societies. Nonetheless, she wrote an entire section (pp. 109–21) on jade (a culturally specific symbol) that we consider to be a thoughtful, reasonably thorough treatment of the subject. Her primary goal, however, was to contextualize the stone imagery that surfaced in each novel with which she dealt. We believe she accomplished that goal admirably.

4. Rolston complains that Wang restricted herself to pre-Ming sources, but since she was writing about a cluster of three novels of which at least one dated to the Yüan-Ming period, it would have been unhistorical for her to do otherwise.

5. According to Rolston, “There are far too many misinterpretations (the story of ‘the stone of rebirth’ is taken to be about ‘karmic memory’ rather than foreknowledge), mistranslations, mistransliterations [*sic*], and plain old mistakes.” While there may be honest differences of opinion among scholars concerning the most suitable hermeneutic approach to a work of literature, after rereading the sixteen pages (177–93) that Jing Wang devoted to the careful analysis of this motif, we believe that “karmic memory” is actually a far more appropriate interpretive phraseology for evoking the Buddhistic implications of the “stone of rebirth” than is the Judeo-Christian notion of “foreknowledge.” With regard to the matter of alleged frequent mistranslations and mistranscriptions, the reviewer presents no evidence whatsoever, so it is impossible to evaluate his claims. We should note, however, that we checked Wang’s translations against the original Chinese texts and found them to be on the whole careful and conscientious. Nor did we find Wang to be prey to mistranscription. The complaint about “plain old mistakes” is so imprecise and unsubstantiated as to be of no value to anyone.

6. We question the justice of the reviewer’s allegation that Jing Wang’s style is afflicted by “jargon, repetition, and sometimes nonidiomatic English.” Even before the manuscript underwent rigorous scrutiny by the editors of the publisher, we as readers were both impressed by the remarkable precision and power of the author’s language. Rolston may be unfamiliar with the theoretical vocabulary of modern literary theory, but that does not entitle him to dismiss it as jargon. In any event, Wang uses such vocabulary sparingly and judiciously. The theoretical aspect of her book consists, rather, primarily in the critical assumptions and methodology that underlie her argument.

Other questionable points raised by Rolston’s review merit discussion but at the request of the editor for reasons of space, and in the spirit of concise and constructive criticism, we limit ourselves to those we have addressed here. Our purpose in writing this response is to ensure that the highest standards of scholarship and fairness are maintained by the *Journal* and by the field in general. What is at stake is more than the reputation of a single author and a single reviewer.

ANTHONY C. YU
University of Chicago

VICTOR H. MAIR
University of Pennsylvania

DAVID L. ROLSTON REPLIES:

To fully answer the complaints against my review of Jing Wang’s *The Story of Stone* (JAS 51.4:910–12) brought forward by Professors Yu and Mair there is no

alternative but a complete accounting of the evidence I relied on when I made the statements to which they object. That accounting required fourteen typescript pages, too long to publish here. I have sent copies of my complete reply to Professors Yu, Mair, and Wang, and invite them to respond publicly or privately. I am also prepared to make copies of that reply available to anyone who writes to me.

The process of drawing up my reply did make me realize that, besides the problem of what can or cannot be done in a short review, there are two statements in my review that I would not now make in the same manner. (1) According to my notes, Professor Wang has only two citation errors for which the likelihood is that the citation style is to blame, and (2) it was unwise of me to use the word "misinterpretation" after praising Professor Wang's book for advocating that we get beyond either/or arguments about literary texts. For these, I offer my apologies. Reviewing my notes did not make me change my mind about anything else that I wrote about the book, positive or negative.

I would like, however, to make a few comments. I think Professors Yu and Mair have misread sections of my original review. I said nothing about the "utter uniqueness" of anything, nor did I criticize the section on jade in the book, nor does the fact that Professor Wang "responsibly" shows shared aspects of stone symbolism mean very much without comparison of how that symbolism shows up in the literature of the different "premodern societies," since her book is, after all, a treatise on stone lore in literature rather than on stone lore itself (complaint 3). Also, if jade is allowed to be "a culturally specific symbol," what is so absurd about the idea that stone might have "culturally specific" aspects that might be worthwhile to point out? Professor Wang *does* make some attempt to do this, she merely does not go far enough for my taste. I also did not criticize Professor Wang's "careful analysis" of the "motif" of the stone of rebirth or her evocation of the "Buddhistic implications of the 'stone of rebirth,'" but rather her interpretation of "the story of 'the stone of rebirth'" (see complaint 5). I do not allege that the book is "afflicted" by anything (complaint 6); furthermore, careful reading of the review would show that I was asserting that in my opinion "[u]ndergraduates and nonspecialists" will be "put off by the jargon, repetition, and sometimes nonidiomatic English." Jargon here is used in its most primary sense: specialist language.

As for the complaints themselves, there is no need to go through them one-by-one here. I merely offer some clarifications. What upset me about Professor Wang's endnotes is that traditionally recognized and meaningful internal divisions within works (i.e., chapters in full-length fiction and year and reign for the *Tso-chuan*) are left out of citations. This is not an example of "mechanical matters that are often out of the hands of the author." With regard to complaint 4, there is no record of a reasonably complete version of any of the three novels before the middle of the Ming; Professor Wang uses the Chin Sheng-t'an edition of *Water Margin* (1641 preface) as her text of reference; no claim is made by her that she is exclusively interested in the influence of stone lore in the earlier two novels on the later one (supposedly her goal was to "contextualize the stone imagery that surfaced in each novel with which she dealt"); and the latest of the three novels (middle of the eighteenth century) is clearly the real focus of the book (as the author herself admits). As for my use of the word "transliteration" (complaint 5), that is the terminology used in the "Book Review Guidelines" for China/Inner Asia for this journal. The major transcription error in the book is the consistent (with the exception of a couple of items in the bibliography) "misromanization" of *tsu* as *tzu*.

The most serious accusation presented by Professors Yu and Mair seems to be that I did not present evidence or cite examples for my critical remarks about *The*

Story of Stone. This arose purely from the necessity of discussing a complicated book in a complete and useful fashion in 750 words. If offense was taken with me for not presenting evidence for some of my statements in the original review, then, according to this logic, I could as easily object to the next to penultimate sentence in the letter from Professors Yu and Mair in which some further unspecified "questionable points" in my review are mentioned but not elucidated. I, for one, am equally regretful that the required brevity of the review also prevented me from going into detail about the "important insights" in the book. I am unable to find any place in my review that I could have cut. Not to mention the flaws of the work would make me remiss in my responsibilities as a reviewer as I understand them. I *do* recommend the work to interested readers, but I also want them to be aware of certain problems with it. That is my understanding of "concise and constructive criticism."

DAVID L. ROLSTON
University of Michigan

TO THE EDITOR:

John Lie has presented a generally positive review of my book *The Chosen Women in Korean Politics: An Anthropological Study* (*JAS* 51:4 [November 1992]:941–43). However, I'm afraid that Professor Lie's statement of the "stated intentions" of my book ("to illuminate 'the systematic limits to female life in Korean culture' ") misses the point.

I wrote in the preface that "The experiences of Korean women in politics not only delineate the systematic limits to female life in Korean culture but also reveal some commonalities in social structural impediments to women in high office across cultures." The book as a whole presents a cultural account of the patterns of Korean women's participation in political life and, in so doing, it makes frequent comparisons with other societies and offers crosscultural perspectives in the interpretation of the data on Korean women in politics.

The stated aims of my book were to contribute to the growing literature on women in politics by providing ethnographic details on Korean women in national politics, and to shed light on the processes of change in the gender-role system in the cultural context of Korea. Thus, the book not only discusses the personal backgrounds and political socialization of Korean women legislators (which is quoted in Lie's review) but also analyzes the subtle changes in the patterns of male-female relations in contemporary Korea.

Several inaccurate statements by the reviewer should be corrected. First, women legislators, including those who are appointed, tend to come from the middle class, not "from upper-class backgrounds," as stated in the review.

Second, Kim Ok-son was proud of having fulfilled her mother's expectation that she behave like a son in place of her deceased only brother. The mother on her part treated her youngest child like a patriarch of the family: she did *not* refer to her as a son.

Third, the ruling party under President Park during the 1960s and 1970s had no elected female legislators, but it did produce twenty appointed women legislators, some of whom served for more than one term. Park's military regime may deserve to be blamed for hampering South Korea's political development in general and women's active participation in politics in particular (as mentioned in the book). However, the problem of male dominance in Korean society is much deeper and more insidious than the recent ascendancy of the military in political power or "any

systematic linkage" between the two, as suggested in the review. Nor should male dominance in Korean society be translated into the absence of both private and public power for most women, as stated in the review. Factors such as age, class, matrimonial connections, and motherhood of successful sons, for example, are the important variables affecting the degree of power a woman could muster in Korean society.

Fourth, the number of women I studied was admittedly small, but the sample itself should not be regarded as small because it included twenty-nine out of the thirty-nine who constituted the entire universe of Korean women legislators at the time of the study. At any rate, the essence of the anthropological method is that it deals with a relatively small number of people, so that the researcher can present an intimate and holistic interpretation of all aspects of their lives.

CHUNGHEE SARAH SOH
Southwest Texas State University

JOHN LIE REPLIES:

I am disappointed that Professor Soh found my review less than satisfactory. Let me respond to her central point: if the book in fact harbors a larger ambition, then it must be adjudged wanting as her comparative observations are perfunctory. As for her other points, I invite interested readers to judge for themselves.

JOHN LIE
University of Illinois at Urbana-Champaign

TO THE EDITOR:

Yasumasa Kuroda's review of my book, *How Policies Change: The Japanese Government and the Aging Society* (JAS 52:1 [February 1993]:163–64), demands a rebuttal in three respects. First, Professor Kuroda puts silly words in my mouth: "Except for Scandinavian countries, Japan's public programs should rank near the top of the industrialized nations, he concludes." They should not rank that high. What I said was that Japan's programs for the elderly today rank well with those of the *English-speaking countries*, but these clearly are substantially smaller than those of Germany and several other European countries (not just Scandinavia). I add that Japan "may well be in the midst of Europe before long" (p. 392).

Second is the reviewer's mention of "a few factual errors." No doubt there are a few, but not the example he picked. I said that "pension outlays are today the largest single item of government expenditure," and in the last sentence of his review he objects that "the budget for national debt servicing and grants to local governments is about 40 billion yen over the amount spent on social security in Japan." He is wrong.

I can only guess that Professor Kuroda looked at General Account budget figures for 1992, when debt service was ¥16,036 billion, local government grants ¥15,975 billion, and "social security" ¥12,212 billion. (This difference is actually 4,000 billion yen but it is easy to make decimal-point errors in reading Japanese numbers.) The problem is that these figures pertain to General Account spending on social security (*shakai hoshō kankei hi*). This is only the portion of the cost of pensions (and other social policies) that is subsidized from tax revenues. In Japan, like most countries but unlike the United States, individuals' contributions to and payments from pension

programs are included in Special Accounts that are “off-budget,” not added into the regular General Account budget.

My sentence referred to pension *outlays*, a term that always means total government payments to pensioners. The most recent year for which this figure is available is 1990, when it was about ¥25,000 billion (there are two slightly different estimates, by the Prime Minister’s Office and the Social Development Research Institute). So pension outlays were about 10,000 billion yen (over 80 billion dollars) higher than either local government grants or debt service. The figures are comparable for other years too—my sentence was completely correct.

Third, the phrase “he chose not to fully disclose his research methods” seems to impugn my scholarly integrity. I don’t know what is meant here. My sources of information are fully documented (other than some anonymous interviews with officials, as is common in decision-making studies), and I go on about theories and methods at—according to some readers—inordinate length. True, my sort of analysis is not exactly simple, but then my chief methodological argument is that decision making is so complicated that it requires a multifaceted approach. I did my best to synthesize earlier models into a coherent theory, and to explain consistently how the theory illuminates a variety of changes in old-age policy.

JOHN CAMPBELL
University of Michigan

YASUMASA KURODA REPLIES:

My review did question John Campbell’s methods and his failure to report operational definitions of his key concepts and interview-data collection procedures.

Campbell’s method was to divide all policy-making processes into two groups twice, once by the amount of energy exerted and the second time by the presence or absence of ideas, resulting in four different modes of policy making. Where was his cut-off point between the presence and absence of “energy”? How did he measure how much energy was exerted by any official or anybody else? He claims that all four modes of policy making were observed. How did he operationally make distinctions among the four? What did he do when conflicting data were obtained as a result of interviewing actors or observers of the policy-making process? How were his 237 respondents selected for the purpose of a “relatively formal interview”? Did he interview any retired officials or active officeholders? If so, how many? Was Seto’s claim substantiated (pp. 107–9) by other indicators? Whose opinion or data did he use for his final decision to classify a policy as one mode or another? The reader is kept in total darkness.

Knowledge claims in science must be accompanied by the disclosure of operational procedures without which no one can test their validity and reliability. The lack or paucity thereof also inhibits replication and constructive criticism of one’s work and invites misunderstandings, such as the one Campbell demonstrated in his response regarding the amount of “pension outlays.” Hence, I stand by my statement in the opening paragraph of the book review that Campbell’s book is seriously “flawed in the scientific rigor expected of a political science work.”

YASUMASA KURODA
University of Hawaii at Manoa

TO THE EDITOR:

The grandiosity of Masao Miyoshi's review of Jeffrey Mass's *Antiquity and Anachronism in Japanese History* (JAS 52.1 [February 1993]:169–71) was sufficient to offend not only the canons of good taste but the limits of credulity. Other than failing to conform to an agenda for historians, the definition of which Miyoshi somehow feels exclusively entitled to formulate, Mass's putative crimes against intellect and history hardly seem serious enough in themselves to merit such scathing opprobrium. A quest for order, for clarity, a relentless pursuit of data and the commitment to rendering them meaningful—these are entirely legitimate concerns of Mass that Miyoshi disdainfully finds so mundane as to merit consignment of a distinguished scholar to a newly created subclass of “institutionists.”

Is this intemperance necessary, or even desirable? Regrettably, for some it apparently is. For if Mass's passion lies in a crusade against clutter, the absence of clarity, and the dazzling dances of the Grand Interpreters of History in his universe, Miyoshi's passion—nay, *requirement*—appears to lie in creating an entirely new universe and securing recognition for it as the only legitimate game in town. Creating new universes is exciting, seductive, and even gratifying; declaring them to be the only valid conceptualization of the space occupied by History is neither a scholarly act nor, as history instructs us, is it a terribly wise one.

Surely, mature scholars can live with competing visions of legitimate scholarship in the field of historical studies, can be critical without being dismissive. Many of us not only share Mass's passions but admire and learn from his work. I have no quarrel with those who criticize it, but I, and others, are deeply offended by the irresponsible arrogance of a review that dismisses him, and those of us who share his concerns as historians, from the world of “serious” historical scholarship. Such bravado regrettably says more about the reviewer than the material and the scholar he has undertaken to review.

GORDON M. BERGER
University of Southern California

MASAO MIYOSHI REPLIES:

For decades now, every intellectual discipline—from anthropology to literature, from law to biology, from history to philosophy—has been undergoing radical transformation and redefinition. Area studies are no exception, and even in Japanese Studies that have been as a whole obdurately resistant to outside scrutiny and criticism, signs of change are increasingly becoming visible. Professor Gordon Berger has missed out on such events: he fails to understand that what Jeffrey Mass dismisses as “clutter” is, in fact, a nugget of meaning(s). He, too, cannot escape from the naïve habit of self-projection that the Kamakura practice of naming is just another program of one-name-for-one-object identification—just like the U.S. Social Security or postal address system. Despite the famous joke about the Chinese encyclopedia by Borges and Foucault (*The Order of Things*), a separate system of difference and taxonomy is in operation here. And to ignore this particular mode of nominal signification is an act of “anachronism” that Mass presumably wrote the book in question to combat.

Gordon Berger admonishes us to be nice, to maintain “good taste.” At a recent scholarly gathering at Stanford, one of Mass's colleagues told me of being asked to rebut my review but declining because of not having read *Antiquity and Anachronism*. Then this scholar at once proceeded to berate my review as both *ad hominem* and excessively divisive. The point here is that the scholar had not read the book, nor

do Berger's comments in Mass's defense show any evidence of knowledge of the book. And yet the two do not hesitate to convert the issue of my substantive review into a sheer *ad hominem* attack. A judgment based on association and loyalty may have a place in a social club, but not in scholarship. As for the charge of divisiveness, criticism is, as I take it, predicated on the recognition and clarification of difference. Do we want to think and discuss, or join—or try to join—the club and relax? What is the reason for reviewing a scholarly book?

MASAO MIYOSHI

University of California, San Diego

TO THE EDITOR:

I am responding to Professor Edward I. Chen's review of my *Janus-Faced Justice: Political Criminals in Imperial Japan* (JAS 52.1 [February 1993]:171–72).

Careless reading is a cardinal sin for a book reviewer. Professor Chen writes: “. . . mere statements by high officials urging police to refrain from torturing prisoners does not warrant a conclusion that human rights were protected” (p. 172). My conclusion begins: “This study of the treatment of political criminal suspects concludes that their procedural rights [i.e., human rights] were as often violated as they were protected” (p. 155). Thus, I did not conclude “that human rights were protected,” but made the point that they were sometimes violated by police, preliminary judges, and procurators. Court judges, however, sometimes freed accused political criminals and often reduced the penalties demanded by procurators. In summary, court judges had a better record protecting procedural rights. Why did Professor Chen not see the book's many examples of court decisions?

Professor Chen writes: “Even in those rare cases in which the offending police were on record as being prosecuted (p. 130), the author is silent whether penalties were actually carried out.” Again, the problem of careless reading. The number of police indicted, prosecuted, convicted, imprisoned, and released on probation (between 1926 and 1940 inclusive) is on the last page of chapter 3 (p. 116). Out of fifty-seven convicted, thirty-seven were placed on probation. I conclude “that authorities were not eager to imprison policemen. . . . The less-than-zealous prosecution of police lawlessness is also reflected in statistics collected by Judge Kawakami Kan for the years 1932–36 (see chapter 4)” (p. 116). Unfortunately, Professor Chen cites the Kawakami cases on page 130, neglecting to mention the complete statistics on page 116. How is it possible to miss twenty-one lines of type on the last page of a chapter?

My book concludes that court judges were able to maintain a considerable degree of independence (p. 175). This conclusion is based on a careful analysis of many trials. Professor Chen disagrees: “The Judiciary attained its independence only after the Occupation . . .” (p. 172). The kindest thing one can say about this viewpoint is that Professor Chen is unfamiliar with this subject. Many scholars have acknowledged that the pre-1945 judiciary maintained considerable independence. My contribution is to illustrate concretely how this autonomy functioned in a number of trials.

RICHARD H. MITCHELL

University of Missouri-St. Louis

EDWARD I. CHEN REPLIES:

Professor Mitchell's statistics only serve to cast doubt on his conclusion that the pre-1945 Judiciary in Japan maintained considerable independence. In fourteen

years since 1926, only fifty-seven policemen were convicted of brutality. Of those convicted, 65 percent were allowed to remain outside of jail. Professor Mitchell remains silent on how many of the twenty policemen convicted but not probated were *actually* sent to jail. How many of them were allowed to slip away from jail and live a normal life in civilian clothes? The author himself admitted that “authorities were not eager to imprison policemen . . .”

Professor Mitchell also confuses the effort of a few independent-minded judges with independence of the judiciary. Under the Meiji Constitution, a judge exercised his judicial power “in the name of the Emperor.” The Minister of Justice controlled the administration of the court system. He could cause disciplinary action against judges, as a result of which judges could be transferred, demoted, suspended, or even dismissed. Thus, in prewar Japan, independence of the judiciary was in theory at best.

It was only after 1947 that the independent status of judges was given the constitutional guarantee, and that the executive branch was unequivocally precluded from taking part in any disciplinary action against judges (Articles 76 and 78, Constitution; Article 48, Court Law).

EDWARD I. CHEN

Bowling Green State University

TO THE EDITOR:

Michael Leifer, in reviewing my two recent books on Cambodia in *The Journal of Asian Studies* (52.1 [February 1993]:216–18) strangely claims that ASEAN’s Kuala Lumpur of 1971 differs sharply from ASEAN’s goal to achieve a Zone of Peace, Freedom, and Neutrality (ZOPFAN). Perhaps he should reread the text of the 1971 communique, which states that ASEAN countries “are determined to exert efforts to secure the recognition of and respect for the neutrality of Southeast Asia as a zone of peace, freedom, and neutrality, free from any form or manner of interference by outside powers.”

MICHAEL HAAS

University of Hawaii

MICHAEL LEIFER REPLIES:

Michael Haas has failed to understand the distinction drawn in my review between neutralization and the Zone of Peace, Freedom, and Neutrality and also distorts my comment in his paraphrase of it. I pointed out “that Haas has confused a commitment in principle to neutralization by the governments of that Association [i.e., ASEAN] with their declared corporate policy of promoting a so-called Zone of Peace, Freedom, and Neutrality (ZOPFAN). If ever made operational, ZOPFAN would provide a very different basis for managing regional relations than neutralization.” Moreover, in his letter, Michael Haas has added words to, and so altered, the original text of the 1971 communique.

MICHAEL LEIFER

The London School of Economics and Political Science