

# Communications to the Editor

## TO THE EDITOR:

We, the undersigned, as contributors to David Lorenzen (ed.), *Studies on Asia and Africa from Latin America* (México City, El Colegio de México, 1990), reviewed in *The Journal of Asian Studies* 50.3 (August 1991):644–45, wish to register our specific disagreement with that review as well as our concern in general with the treatment that scholarship generated outside the United States and Europe receives in the pages of the *Journal*.

The review in question deals almost entirely with alleged faults of copyediting and nowhere with an analytic critique of even one of the ten articles represented. Worse, five articles, that is, half the book, are not even mentioned. It is our opinion that a “review” of this sort—which itself is full of errors relating to Latin American institutions and the very names of the authors cited—has no place in a serious scholarly publication.

Our major concern, however, is not this review but the basic policies of *The Journal of Asian Studies* with respect to Third World contributions to our various areas of professional interest. Very, very few of the serious studies published annually outside the United States and Europe are ever mentioned, and, when they are, all too often are dismissed out of hand (cf. Sumit Ganguly’s put-down of Partha S. Ghosh’s *Cooperation and Conflict in South Asia* in the same issue). It seems to us that this represents a paternalistic attitude on the part of the *Journal* that borders on neglect and contempt for Third World scholarship. What we are seeking in this letter is a ventilation of this problem with an eye to its thorough discussion and solution.

No one publishing the results of his or her research in our or any other area of academic interest can or should expect special treatment. We all benefit from serious criticism. And this, it appears, is precisely what Third World scholars in general and the contributors to *Studies on Asia and Africa from Latin America* in particular are not getting in the pages of *The Journal of Asian Studies*.

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## DAVID D. BUCK REPLIES:

As editor, I appreciate these concerns, but I am convinced the letter writers have missed the real trend toward internationalization of scholarship in the pages of *JAS*.

That trend shows up in many ways. It is obvious in articles, such as that by Sanjay Subramanyam of the University of Delhi in the *Journal’s* May 1992 issue, and in the many book reviews that scholars from overseas institutions contribute to our pages. The advent of the fax machine and electronic mail makes it possible for the editors to obtain timely referee reports from all over the world, and we

regularly avail ourselves of such advice. In 1991 I looked at the country of origin for submissions—a fair, but not completely reliable proxy for the homebase of the contributors—and found that sixty-five percent of our manuscripts came from the U.S. and thirty-five percent from Canada, Europe, Asia, or Australia. All these elements indicate a strong degree of internationalization.

In terms of reviewing, *JAS* is trying to broaden its coverage of significant scholarly books on Asia published around the world. We will not allow this instance to deter us from improving *JAS* in this respect.

TO THE EDITOR:

The ships and ports of Tokugawa Japan were probably more important in the expansion of interregional commerce than the packhorses and post stations mentioned in Kären Wigen's case study (*JAS* 51.1 [February 1992]:3–29), since it is likely that sea transport was less expensive and involved a greater volume of domestic trade than land transport (Funabashi 1992:50; Wakabayashi 1963:34–36; Makino 1963:36; Furuta 1961:105–8; Crawcour 1966; Flershem 1966:182, 184–85; 1964:405, 407). There were significant differences in the conveyance and marketing of goods by land and sea, and it is anticipated that study of marine transportation and trade would lead Wigen to modify if not abandon some of her present opinions. It therefore seems necessary for her to look into the maritime trade routes: the Hokkai route from Hokkaido through the Japan and Inland Seas to Osaka; the Nankai route from Osaka to Edo; the Tōkai route from Hokkaido through the Tsugaru Straits to Edo; and the Seikai route from Nagasaki to Osaka (Crawcour 1964:381).

These multiregional trade routes should certainly stimulate “geographic imagination” and provide ample material for study of regional interrelations and structures. The trade routes might become an invigorating locale for discussion about whether “regional formation in early modern Japan” was “shot through with politics” or determined by geographic factors (Wigen 1992:24). Professor Wigen's ascendancy in any such debate may be foreshadowed by the ease with which one can find in Tokugawa maritime trade-route history examples of political and social forces producing different outcomes in communities, subregions, and regions which had similar or identical geographic environments.

I either agree or seem inclined to agree with most of David Howell's ideas (*JAS* 51.2 [May 1992]:269–86). His analysis of the “transformation to capitalism” in the Hokkaido herring fisheries before the Restoration is interesting and valuable, but not surprising. It might be more surprising to be told that there was no such transformation. Howell's discussion of rural industry, commercial agriculture, capitalism, and proto-industrialization seems acceptable. His assertion that political and institutional factors are “as important as geographical” ones in proto-industrialization is obviously correct. He is on ground which is familiar and obvious to local historians when he argues that Tokugawa commercialization and proto-industry could be hampered and retarded by official policies, which varied in different regions (Howell 1992:273–81).

Scholars are indebted to Howell for his sound judgments and also for occasional tidbits of useful information about Nambu and other regions. I believe, however, that he would make a more stimulating contribution if he would advance from his interest in Hokkaido to serious contemplation of the whole vast

panorama of Tokugawa Japan with which Hokkaido was in commercial contact. This would require intensive examination of the Tōkai and Hokkai maritime trade routes that connected Hokkaido with Edo and Osaka, and encompassed most of the coastal areas of Honshu and the commercially important ones of Hokkaido. Howell might then find himself beneficially and happily enmeshed in dozens of regional, inter-regional, and intraregional situations, and in the social, political, and institutional ramifications of commercialization and proto-industry as well as the economic aspects.

It can be envisaged that maritime trade route enmeshment would first shock and then excite and inspire Kären Wigen, David Howell, and many other scholars. A plethora of often contradictory evidence might compel arduous reorganization of their thinking. This process would presumably be accompanied by enhanced understanding of problems embedded in the Tokugawa prelude to Japan's modernization. Study of the Tōkai and Hokkai routes, for example, would illuminate the importance of Hokkaido, despite its minuscule Japanese population, in the economic development of Tokugawa Japan as a whole.

Acquaintance with these trade routes should also improve scholarly response to the crucial question asked by Thomas C. Smith in 1959 at the end of his book on the rural background of modern Japan: "What social alchemy made of peasant boys men who could found international banks and trading companies?" (Smith 1959:213). This question has not been convincingly and conclusively answered, and it may never be, but each generation should try again to address it.

Smith said in 1959 that he did not know the answer, but "part of it is to be found somewhere in the history of change in rural Japan before 1868." The maritime trade which was carried on in village ports was an important factor of rural change, and the most enlightening evidence of the "social alchemy" sensed by Smith was perhaps buried before the Restoration, and has awaited excavation in hundreds of small rural communities that line the coasts of Japan. Many such places, which may be depopulated and all but dead today, were bustling local ports for the coastal trade 150 years ago. In the records kept by the Tokugawa period inhabitants of these village ports, one may occasionally find clues that help to explain why it was village tradesmen, perhaps more than city merchants, "who broke away from old ways and, in searching for new economic chances, became the harbingers of a new age" (Flershem 1966:321). Thus, unpublished manuscripts of the maritime trade route communities may be the best sources for struggle with Smith's question, and perhaps for some of the concerns of Wigen and Howell.

It would be desirable for non-Japanese as well as Japanese scholars to make a strenuous effort, before it is too late, to find all unpublished rural manuscripts that might be helpful in considering the problem posed by Professor Smith. This search does not seem quite so indispensable for the purposes of Wigen and Howell, but if they could mine this kind of material on a large scale, it seems safe to predict that their work would be profoundly enriched and probably revolutionized.

I have the impression that this comment may be more welcome in Japan than in the West. Japanese historians, at either the local or the national level, usually seem to understand the scholarly value of Tokugawa rural manuscripts better than their Western confreres. Japanese specialists in Tokugawa history are expected to discover and use unpublished manuscripts, and discovery is greatly admired, which is a healthy attitude, since there is still a lot to be found, and there may be even more that has been found but not published, and needs re-discovery. Tokugawa specialists of Western countries generally seem content to

depend on the published work (including published manuscripts) of their Japanese colleagues, and they usually seem unwilling to make a real effort themselves to find and use unpublished Tokugawa manuscripts in general and unpublished rural manuscripts in particular.

If I am right that, in comparison with their Japanese counterparts, Western scholars lack enthusiasm for unpublished Tokugawa manuscripts, it may be that this indifference is partly attributable to aversion to hunting for needles in haystacks. Obviously, in investigating interregional Tokugawa problems, needle-hunting should be avoided if possible by finding collections with interregional coverage. This is easier said than done.

Scholars should be aware that, for some problems, a private collection may have better interregional coverage than local and even national libraries and archives. For example, the best unpublished manuscript collection for research on the Tokugawa and early Meiji history of large sections of the Hokkai and Tōkai sea trade routes is a private collection whose contents are almost entirely unknown to both Japanese and Western scholars. If interested people want more information, Yoshiko N. Flershem, my wife and collaborator, and I hope they will get in touch with us.

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WIGEN, KÄREN. 1992. "The Geographic Imagination in Early Modern Japanese History: Retrospect and Prospect." *The Journal of Asian Studies* 51.1:3–29.

KÄREN WIGEN REPLIES:

I wholeheartedly concur with Dr. Flershem's substantive point about the importance of sea routes in the Tokugawa transport system, and have no doubt that investigation of coastal ports would reveal related patterns of development in similarly situated communities. The notion that such findings would shock me suggests a misconstruing of my argument. The intent of my article was to demonstrate that the spatial contours of the Tokugawa economy were socially *as well as* physically constituted. To say that regional formation in early modern Japan was shot through with politics is not the same as arguing that economic activities were located on the basis of political fiat alone; as I noted, circulation in an early modern economy was obviously highly responsive to the physical environment. My point was the more modest one that economic geography cannot be read *directly* from the physical landscape.

The history of trade and transport in the Ise Bay makes it clear that this principle applies to maritime as well as overland commerce. The declaration of a tax-free commercial zone (*rakuichi-rakuza*) in Matsuzaka in 1588, for instance, enticed major merchant houses to relocate their headquarters to Matsuzaka from the neighboring port of Ominato, generating regular traffic at a new point on the shoreline. The small village of Shiroko, farther north along the Ise coast, maintained a flourishing shipping trade despite a shallow harbor due to the backing of Wakayama domain, which allowed ships laden with cotton to take on a token amount of Wakayama tax rice at Shiroko and thereby pass through Bakufu customs checkpoints without paying duties on their cargo. Likewise, a convergence of political interventions transformed Atsuta from a modest port into a major fixture in Ise Bay shipping. Atsuta's boom began in the early 1600s, after the town was designated in quick succession as the official outer port for Nagoya, as the forty-first post town on the Tōkaidō (at a time when it was illegal for inns to operate in Nagoya proper), and as the official wharf for processing and handling Kiso lumber.

As these developments suggest, even the routing of maritime trade was not *solely* determined by economic cum environmental considerations. But the invigorating debate that Dr. Flershem anticipates, namely between one camp that sees regional formation as political and another that sees it as "determined by geographic factors," presupposes an either/or dichotomy to which I do not subscribe. Perhaps not coincidentally, it also reduces the term geography to its physical component—a restricted definition that is no longer current among geographers, and one from which I deliberately sought to depart.

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## TO THE EDITOR:

Well-done book reviews, at least in my view, are those that summarize concisely the scope of the book and the thrust of the author's arguments, and then add the reviewer's judgments, positive and negative. A poorly done review occurs when the reviewer concentrates on picking nits or on trumpeting his or her own views on the subject. In this regard, it seems to me, over recent years the Journal's reviewers have gotten much better.

It is annoying when a reviewer uses the occasion to bad-mouth others who are not academics or who are apparently not properly "politically correct." An example is Andrea Panaritis's review of Larry Cable's *Unholy Grail* (JAS 51.2 [May 1992]:450–51), which closes, "it is, indeed, the rare general or statesman who possesses the desire and ability to read." How does she know? Because their views of the world are not hers, they must not be able to read? This sort of snide comment does not seem to do much for a serious journal.

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## ANDREA PANARITIS REPLIES:

I couldn't agree more with John Sylvester's description of a good book review. Certainly the idea that political and military policymakers don't often carefully read the lessons of history is not new. It is, however, one of the author's most important concluding thoughts. Larry Cable expresses it particularly well regarding Vietnam:

The ultimate tragedy of the American intervention and its failures is that there had been no need to learn on the job. The correct and incorrect lessons of counterinsurgency and interventionary warfare were all to be read in the pages of history. The Americans had written the earliest pages in their own success in the first insurgent war of national liberation, the War of Independence. The Americans had written additional pages in their success against the Filipino insurrectionists at the turn of the twentieth century. More recent chapters had been written by the British in Malaya, the Americans during the Huk Insurrection and the French in Indochina. Thucydides had written the first work of history for the instruction of generations of statesmen and generals yet unborn. What he did not realize was the futility of pursuing history for that end when the generals and statesmen lack the wit and will to read (p. 242).

The sentences immediately preceding the one that so offends Mr. Sylvester state "*Cable believes* that a careful reading of history . . . could have prevented many of the disasters he details. Likewise, a careful reading of the lessons of the American involvement in Vietnam could be of great use to current and future policymakers." Perhaps I should have more explicitly delineated the view as Mr. Cable's. Alternatively, perhaps, it is not only the generals and the statesmen who would do well by reading more carefully.

## TO THE EDITOR:

Although I appreciate the basically positive tone of Valerie Hansen's review of my *Fortune-tellers and Philosophers: Divination in Traditional Chinese Society* (JAS 51.3 [August 1992]:658–59), I am a bit uncomfortable with her characterization of the work as a kind of encyclopedic "catalogue" or "narrative." Potential readers should

know that the book offers explicit interpretations of the social, political, and psychological significance of divination in Qing dynasty China, as well as a revisionist perspective on the so-called “decline of Chinese cosmology” in late imperial times. In addition, it argues at some length that *kaozheng* scholarship may not have exerted as profound an influence on Qing social and intellectual life as some writers have asserted.

Also, it is rather misleading to suggest that the book is based solely on “secondary sources in Chinese and English” and my own “first-hand experiences.” In fact, Yuan Shushan’s *Zhongguo lidai buren zhuan* (1948) is itself a primary source, since most of the over one thousand biographies of Qing dynasty diviners are drawn verbatim from official histories, local gazetteers, and the like. Other primary Chinese materials used extensively for the book include official compendia (e.g., the *Siku quanshu zongmu*), unofficial compilations (e.g., the *Qingbai leichao*), gazetteers, essays, diaries, legal casebooks, diviners manuals, ritual handbooks, medical tracts, collections of proverbs, and Qing vernacular literature (novels, plays, and short stories).

My aim was to write a book that would provoke serious academic discussion and debate, not simply serve as a reference work for “scholars in other fields” and undergraduate students.

RICHARD J. SMITH  
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