Review: Scholarly Discourse and Peasant Discontent: Four Studies of Popular Contention in the Tokugawa Period

Reviewed Work(s):

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Deference and Defiance in Nineteenth-Century Japan by William W. Kelly

Peasant Protests and Uprisings in Tokugawa Japan by Stephen Vlastos

Social Protest and Popular Culture in Eighteenth-Century Japan by Anne Walthall

James W. White

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Review Section

SCHOLARLY DISCOURSE AND PEASANT DISCONTENT: FOUR STUDIES OF POPULAR CONTENTION IN THE TOKUGAWA PERIOD

Peasant Protest in Japan, 1590–1884. By Herbert P. Bix. Yale University Press, New Haven, 1986. xxxix, 296 pages. $30.00.

Deference and Defiance in Nineteenth-Century Japan. By William W. Kelly. Princeton University Press, Princeton, 1985. xvi, 322 pages. $31.50.

Peasant Protests and Uprisings in Tokugawa Japan. By Stephen Vlastos. University of California Press, Berkeley, 1986. xii, 184 pages. $20.00.[1]

Social Protest and Popular Culture in Eighteenth-Century Japan. By Anne Walthall. University of Arizona Press, Tucson, 1986. xviii, 268 pages. $19.50.

Reviewed by
JAMES W. WHITE
The University of North Carolina at Chapel Hill

The books under review here are, each in its own way, important contributions to our understanding of the ways in which commoners during the Tokugawa period contended both among themselves and with the authorities. They have markedly different objectives, adopt different approaches, focus on different data, evaluate their subject matter differently, and aspire to different degrees of generality in their conclusions, but each fills in part of the picture.

1. Editors' Note: The Vlastos book is discussed here only in its relation to the other three books because this reviewer has already reviewed it in *Monumenta Nipponica*, Vol. 41, No. 4 (1986). *JJS* policy precludes one scholar reviewing a book in more than one place but the Editors felt this book clearly should be discussed in conjunction with the other books on the same topic.

The Bix and Walthall books are what a Japanese colleague calls "forest" books: overviews of whole fields of phenomena about which they attempt to generalize. Both are broadly descriptive of the processes of popular contention—petition, litigation, appeal, communal conflict, confrontation, riot, and insurrection. But whereas Walthall accepts the people's own perception and interpretation of reality and meaning and seeks to infer popular culture and mentality from an analysis of popular contention, Bix seeks explanation of the causes of conflict and does so by imposing upon his data a received body of Marxian theory and deducing cause and effect therefrom. His perception of reality and meaning are to a large extent determined by his theory; for example, class is defined in terms of exploitation, unequal power, nonreciprocal relationships, and (by implication) intergroup hostility. This definition is loaded, to put it mildly, but it is consistently used and valid for Bix's purposes. On the other hand, the definition of *ikki* (the standard term for peasant protest) is not: to describe *ikki* as "full-scale . . . public protest in rural areas involving significant numbers of armed people. . . . in which some peasants invariably died" (p. xix) is simply inaccurate in terms of scale, armament, and casualties, nor does he hew consistently to it.

The Kelly book, by contrast, is a "tree" book: resolutely attentive to the complexities of individual cases of contention and highly resistant to generalization (although one may draw one's own generalizations). He too gets full marks for descriptive thoroughness; like Walthall, he accepts the commoners' perception and definition of reality and aims at inferring the meaning and significance of their behavior, at achieving deep (perhaps "thick" is better) but narrow understanding of society in nineteenth-century Shōnai domain through the lens of collective protest.

A fourth, rather in-between, study in this genre, worthy of note here, is Stephen Vlastos' *Peasant Protests and Uprisings in Tokugawa Japan,* which offers a general overview of popular contention throughout the era based on intensive analysis of one region: Aizu, today's Fukushima prefecture. He is less willing than Kelly and Walthall to rely completely on the peasants' own perceptions (especially as regards such broad processes as market development and class dynamics); like Bix he focuses and filters his data with a theoretical lens; unlike him, his lens does not predetermine the picture.

One might expect predetermination, by virtue of the sociology of knowledge, in an expressly interpretive study like Kelly's. However, it is not the rigorously theoretical Bix book but the more sensitive and nuanced Kelly book which avoids an evaluative stance. Kelly approaches the epitome (whether one likes it or not) of "value-free social science;" he is extraordinarily detached, as if neither peasants nor authorities earned either his respect or disdain. Bix is at the other extreme, characterizing peasant-

headman relationships as filled with "hatred" and Tokugawa villages as "prisons" (pp. 22, 109). Walthall occupies a middle ground; due perhaps as much to her approach as to her data, she evinces a positive view of the commoners: they are not noble or revolutionary but they are insightful and calculating, and not conned by some false consciousness foisted upon them by the ruling class.

As the above indicates, these books fill in a good range of the spectrum of recent studies of collective protest in Japan. Kelly's is anthropological in its attention to particular cases and uncolored by any of the major theoretical approaches to the phenomenon popular in the social sciences; Walthall falls into the *mentalité* niche; Richard Smethurst's recent book on rural contention in the post-Tokugawa period represents the rational-actor school;[2] and Vlastos's book, with its emphasis on economic development, class structure and dynamics, and popular mobilization, follows the broad outlines of Charles Tilly's widely adopted approach to popular conflict. Bix's book also fills a significant spot on the spectrum, being a straightforwardly Marxian analysis; many readers may find it even more profitable as historiography than as history.

Bix, Kelly, Vlastos, and Walthall all rely impressively on primary sources (although Walthall's use of chronicles and tales as a window into peasant mentality seems more prudent than Bix's use of them as historical documents); Kelly relies least on studies of protest (perhaps naturally, since his interest in nineteenth-century Shōnai is at least as strong as his interest in contention). Walthall's reliance is considerably greater, but it is Bix's book that stands out as an example of *Japanese* treatment of peasant protest. The Marxian approach has dominated Japanese scholarship until recently, and Bix fits comfortably into this genre, with his view of rapacious government, constrained peasant consciousness, and "progressive" popular contention which contributed crucially to the overthrow of the *bakufu* in 1868. However, he avoids the more doctrinaire Marxist tendency to denigrate any popular action that did not help advance the glacier of History and accords the people an intrinsic dignity, crediting them with real gains from protest. If you want to understand how Japanese scholars have viewed popular protest during the Tokugawa period, Bix is the place to find out.

One of the historiographical pluses of Bix's study is his focus: nationwide, era-long, and typologically complete. Temporally, he covers the entire era, concentrating on four periods of concentrated conflict (the 1720s, 1760s, 1780s, and 1860s), illustrating each with exemplary cases and link-

2. Richard Smethurst, *Agricultural Development and Tenancy Disputes in Japan, 1870–1940* (Princeton: Princeton University Press, 1986).

ing them with analytical historical elision. His omission of the conflict-wracked Tempō period is puzzling (Ōshio Heihachirō's rebellion, which is dealt with, is *sui generis* rather than exemplary of anything), but the cases used do illustrate the points essayed. The cases, as noted, run the gamut from peasant grumbling to mass insurrection and occur from southwest to northeast. I personally am persuaded that meaningful things can be said about popular contention in general, society-wide and across centuries (especially if the process of change is, as it is to Bix, a major focus of attention). The study of short periods and localized arenas can highlight detail which Bix must skim over, but his breadth of focus is preferable, at least to those of a nomothetic bent.

Walthall's geographical and behavioral foci are quite similar to Bix's. She wishes to see into Tokugawa popular culture; given the generality of this aim it is natural that she extends her perspective nationwide and looks sequentially at a wide variety of activities. Indeed, for one interested in a Cook's tour of the behavioral repertoires of Tokugawa peasants, Walthall is perhaps better than Bix, devoting separate chapters to petitions, village disputes, and riots in addition to substantial treatments of direct appeals and open confrontations with authorities. Her temporal focus—the 1780s—is short, however: like many Japanese historians she considers this period a key turning point. To her the era is particularly rich in evidence of a process of economic, social, and political change, and she uses examples of collective conflict that illustrate the evolutionary stage, in the 1780s, of a host of phenomena. The strength of her analysis rests, of course, on one's acceptance of the argument that the 1780s were as significant as she says; I tend to buy the argument in terms of *bakufu* economic and coercive resources, of the political consequences of economic change, of forms of contention, and of peasant consciousness (although I would accord the 1830s equal place). There are no clear turning points in any of these factors, and she does not assert that there were, but if one wishes to take a transverse section of Tokugawa history then the 1780s are one of the best places to stick in the scalpel.

Vlastos, rather than opening specific periods to view, lays bare the entire era. He does not dwell on, e.g., either Temmei or Tempō, but adduces well over a dozen cases of protest running continuously from the seventeenth to the late nineteenth centuries. His geographical scope, as noted, is restricted to Aizu, but it provides him with examples of all forms of contention, from petition to riot and suasion to coercion, and enables him to offer some national-level conclusions about the evolution of popular conflict over the centuries.

Kelly, by contrast, focuses temporally on the period 1840–1870s; he is interested less in how this period exemplifies some state than in how, and

to what extent, events during the period illustrate changes taking place then. Despite the major changes taking place during these years, he finds considerable continuity in patterns of popular contention: old injustices have new variations and old tactics are directed at new institutions, but comparison of events at the very beginning and end of the period enables only rather modest conclusions about change in the basic concomitants and characteristics of conflict.

Geographically and behaviorally, also, Kelly is reluctant to generalize—and with good reason, since both the region and the cases at hand are distinctive. Shōnai, as he notes, was a "monocropped rice bowl" (p. 60) with no significant protoindustrialization lying within a single domain and was, by inference, atypical in patterns of class differentiation and political structure. It was also (as he does not note) distinctively more conflictual than its neighboring region: Shōnai's Akumi and Tagawa counties saw far more contention during the Tokugawa era than either of the neighboring counties on the coast (Iwafune and Yuri) and as much as or more than three of the four closest inland counties (Okachi, Mogami, and Oitama; Murayama county was far and away the most conflict-ridden county in the region). Kelly stresses (rather humblingly, to those of us who tend to reify whole classes) the cross-class, coalitional nature of contentious movements, and resists typologizing these movements (again, his portrayal of the multidimensionality of contention is humbling to us theory-driven homogenizers); but he is wise to limit his conclusions to Shōnai, with its distinctive patterns of conflict and political economy.

Behaviorally also Kelly's data are atypical. He chooses to focus on four episodes of non-revolutionary, non-ideological, modest contention, accurately stressing that such behavior was far more common than large-scale, ideologically-driven, violent, antisystem protest. These four episodes are, however, only four among over 40 such incidents that occurred in Akumi and Tagawa counties during the Tokugawa era, and they are by no means the only events that occurred during the nineteenth century. Nor are they representative of the whole: as Kelly notes, they were all sustained, enduring, collective, joint (i.e., cross-stratal or cross-class) movements; he has thus maximized class heterogeneity, community fragmentation, and organizational complexity in the cases chosen. One suspects that the unexamined residue of contention in Shōnai may have been more modest in magnitude, more *ad hoc,* and more rooted in class and village solidarity than the events Kelly examines. Thus he can, and does, tell us a great deal about relatively large-scale protest in Shōnai; those wanting to find out about contention in general (or of other types) in Japan in general (or in other regions) may feel unsatisfied.

Those seeking explicit causal analysis will also be unsatisfied by Kelly,

although some inferences may be made, and two striking factors emerge clearly. Such unsatisfaction is one's own affair, of course—his goal is not causal analysis, but rather how contention reflects social conditions and changes. But causality is a matter that cannot be entirely ignored; it is a point of difference between these books; and it is a surprisingly salient point in Kelly's work as well and thus of interest to us here.

For starters, Kelly expressly rejects certain common causal factors in contention: neither class interest, community ideology, nor popular rights is sufficient to explain contention in Shōnai, where motives were mixed, ideology was muted, and the class composition of movements was heterogeneous. The first causal theme Kelly does stress is vulnerability; not poverty, not deprivation, not exploitation, but the simple fact that prosperity in a preindustrial, agrarian society with an unaccountable autocracy at the top is tenuous for all. Weather and insects can devastate crops, and the common people suffer first from the ever-present tax burden and then from insufficient food and from the skyrocketing price of what food is left, and even merchants see the ultimately agrarian basis of their profits destabilized. The rapidity with which families achieved and lost wealth during the Tokugawa period should make us leery of any assertion that individual merchant or landlord houses (as opposed to their strata) tended to endure throughout economic fluctuations. The government was no less vulnerable: its revenues were rice-based or -denominated; if it tried to maintain revenue levels amid hardship it risked peasant protest, and if it lightened taxes it risked insolvency. For Kelly it was this universal vulnerability, and the "swings" and "intersections" of each type thereof, that created "moments of social protest, political crisis, and economic reform" (p. 49).

The equally salient, but unstressed, second causal theme of Kelly is the role of social and political elites. Over and over again they emerge: village and district headmen, merchant leaders, domain warriors or administrative agencies, and the *bakufu* itself mobilize, channel, manipulate, lead, exacerbate, reward exemplarily, or repress conflict in ways that go far to explain the initial possibility, catalytic process, magnitude, goals, means, and outcome of popular contention. A term Kelly does not use, but which jumps to mind (to mine, at least) repeatedly, is "opportunity structure:" the state, its agencies, its subordinate arms, and its commoner minions play a crucial role in limiting the realm of the politically conceivable, the practically possible, and the ultimately successful. Movements that reflect popular unity or face elite fragmentation tend to succeed; those that reflect disunity or face elite unity tend to fail. Above the social arena in which the anthropologist immerses himself stands (albeit with its hands plunged deeply therein) the state. Kelly makes this stance vividly clear—indeed, in several instances what goes on in Edo or Tokyo is at least as important as

what happens in Shōnai. Indeed, had he explicitly focused on the opportunity structure of protest he might have been better able to explain why, during the periods of intense dearth and upheaval of the 1830s and 1866 (when every group's vulnerabilities came home to roost, as it were) there was relatively little contention in Shōnai.

Walthall also takes a rather low-key approach toward cause and effect; for her too the aim is interpretation of meaning, not inference of causation. But she too has a vision of causation, and again the opportunities facing the people were important. She emphasizes more strongly popular consciousness; "her" peasants were pragmatic, shrewd, opportunistic, and self-interested—psychologically autonomous, if you will—and they took advantage of every chance to improve their situations under an avowedly rapacious system. She is not as consistently positive (some critics might say Pollyannaish) as, for example, Smethurst, but she clearly sees innate characteristics of popular culture as contributory to efforts to improve popular position vis-a-vis elites.

Stephen Vlastos continues in this vein: disavowal of causal focus and of the language of "independent variables" and "statistical correlations" (p. 4) combined with quite a bit of closet nomological explanation. His major goal—which he most adroitly achieves—is to depict the changing nature, form, and content of popular contention, especially as it illustrates the interplay of changing economic and social structures. But by the time he is done limning the political structure, the social organization of the peasantry, the constants and vagaries of weather and policy and administrative style, and (better than the other authors) the process of sericulture-based protoindustrialization, we also have a pretty good idea of *why* the peasants of Aizu (and elsewhere) were likely to protest.

The most intentionally causal of these studies is Bix's, but he is occasionally too astringent or, conversely, too luxuriant. In the former vein he simply skirts the question of spatial variation in conflict: "one can only speculate" why contention is frequent here and rare there (p. xxiii), beyond the co-occurrence of "ripe" political context and acute crisis. Elsewhere he does flesh out this model, characterizing "ripeness" as intense exploitation, the existence of clear targets, and a history of local contention; and as a combination of unilateral exercise of state rights without consideration (most essential in time of dearth) of the security of the people's livelihood. In the individual cases he treats, such factors are indeed intertwined skillfully and one can see why this explosion occurred at this time, in this place; it is at this middle level that Bix is most successful. But looming over the cases he creates an overdetermined, snowballing causal model: the merchants are always rising, exploitation is always intensifying, state cooptation of commoner officials is always spreading, relationships be-

tween rich and poor peasants are always worsening, villages are always fragmenting, urban-rural splits are always widening, elite-mass conflict is always growing, and anti-feudal ideology is always developing (if not maturing)—and all of this appears to be going on at a fairly even rate nationwide for the entire Tokugawa period. One's first inclination is simply to get the hell out of the way of this teleological juggernaut. I suppose that the Marxist answer is that you can't—that's History coming down the tracks—but surely, without adopting the extreme particularistic posture of Kelly, some spatial and temporal distinctions might be found. And a combination of processes that occurs universally explains nothing in particular, so the massive Marxist panoply of causes actually is of little help in explaining the specific cases upon which Bix focuses.

As intrinsically interesting as the causes of contention, but of less concern to the authors under review here, are the consequences thereof. This is a generally underemphasized topic in the study of popular contention, partly because people seem more interested in asking "why?" than "so what?"; partly because for a long time peasant protests were seen as goalless, anomic convulsions; partly because Marxist scholars have tended to dismiss any gains short of revolution; and partly because assessing consequences is so very difficult: just because a group seeks some goal and the goal ultimately appears does not necessarily mean that the protest led to the achievement. Kelly faces one such situation: the *bakufu* attempt to transfer the daimyo of Shōnai in 1840, which was resisted by both commoner and noble elements in Shōnai but which also became crucially entangled with intra-*bakufu* conflicts in Edo. It is impossible to weigh the different contributions made to the eventual recission of the edict by the various actors, and Kelly wisely avoids attributing relative causal influence. But overall he too makes some statements about the efficacy of popular protest.

In the case of the 1840 transfer edict, Kelly credits broad-scale popular protest with helping negate the edict; in 1844, however, a more fragmented and less populist movement (led by merchants) to prevent incorporation of some *bakufu* territories into Shōnai jurisdiction failed. Another attempt to move the daimyo in the late 1860s was also deterred, but again it had become substantively intertwined with intragovernmental conflict. A constellation of movements between 1869 and 1880 ultimately ended in clearcut achievement through litigation of the goals of the movement, but along the way certain stages of protest were successfully repressed by the state, and Kelly asserts that the final verdict actually represented as much a victory for the central government vis-a-vis recalcitrant local officials as it did victory for the protesters. The picture remains murky; not murky, perhaps—Kelly is too lucid a writer for that—but rather kaleidoscopically complex and resistant to sweeping conclusion. Clearly the people made

some real gains through their actions, but just as often they were successfully manipulated or otherwise utilized by elites for their own purposes.

Bix lies at the other extreme in assessing the consequences of contention. He laments the slow development of anti-feudal thought and the failure of peasants to create lasting "struggle organizations" (p. 127; a legitimate lament, in my view, but not a legitimate indictment), and is probably unhappy that the Meiji Restoration did not entail social revolution. But at the same time he does recognize the material improvements in power relationships won by the people: early risings achieved practically nothing (p. 52) because the people as yet had neither unity, stamina, nor much consciousness, but later protest did impose concrete changes on domain attitudes, personnel, and policy and represented "measurable political progress" (pp. 215–16). He soars a bit unduly, perhaps, at the end of the era, seeing the Restoration as "inconceivable" (p. 162) without the wave of popular contention of the 1860s, but his view (paralleled by Vlastos) that this contention seriously "undermined" (p. 225) domain and *bakufu* power and hastened the demolition of *bakuhan* institutions after 1868 rings true.

Walthall alone devotes space specifically to the consequences of contention, citing government policy and personnel changes, the influence of popular contention on nineteenth-century elite schools of thought which became increasingly anti-Tokugawa and inconsistent with (if not anti-) feudalism, the same sapping of *bakufu* strength in the 1860s that Bix cites, and Ōkubo Toshimichi's decision to reduce the Meiji land tax in the face of popular dissent. In all four books, however, the same picture emerges: when protest was caused by governmental actions that contravened official ideals, when the people could demonstrate unquestionably good cause, when the people were able to act in concert, and especially when they confronted a divided elite, then the people were able not only to win short-run, localized victories but also able to constrain the entire *bakuhan* system in ways that crucially decreased its viability. They never attempted revolution, never propounded radical ideologies, never created lasting organs of resistance, and (until the Meiji era) never even won the unquestioned legal right to protest at all. But they connived and acted as far as circumstances permitted and manipulated the elite to a surprising extent; and, really, who are we to fault them for not achieving more? In the absence of a millennial religious tradition, of mountain fastnesses free from official control, of sharp horizontal stratification which subjectively overshadowed vertical "pillarization," and (last and perhaps most) of elite allies, they persevered and won many small (but not insignificant) battles.

This last suggests my own orientation toward the Tokugawa peasantry, and our four authors also weigh in with their respective views. Perhaps the

closest to my own view is Walthall: we disagree over her view of popular contention as relatively ineffective in specific cases, achieving only temporary relief and often quite successfully repressed, but I do agree with her (and with Bix, albeit in a later time) that the peasants show little evidence of false consciousness. They were self-interested (though not atomistically or exclusively so, as the rational-choice vocabulary of self-interest implies) and psychologically autonomous, able to mock and occasionally even to sneer openly at authority, and to create their own standard of morality and measure the elite against it. They were constantly on the lookout for ways to turn official precepts against the elite, painting it into a corner by invoking its own notions of benevolent rule (*jinsei*) and interpreting statements of official prerogative as statements of the limits of official power. She may overestimate their conniving qualities—Vlastos observes the same behavior but does not seem to see as much self-conscious manipulation, or open defiance, of the system as she does. But both would agree that they were subjects of the system, not simply its objects. It is quite possible that this assessment is peculiar to the 1780s: had Walthall looked at earlier years her view might more closely have resembled Bix's negative view of peasant activities in the 1720s. Perhaps she has selected a relatively advanced stage in the people's transition from "deference to defiance," a transition that Bix notes, and Kelly focuses upon, in later years.

Bix evinces little admiration for the achievements of early Tokugawa peasants, but even at this stage he does not simply reduce them to mindless termites chewing utterly unawares at the foundations of the system. I think he would call them ignorant but not stupid, and their ignorance was overcome in the process of generations of contention. Bix's peasants (we all tend to have "our" peasants, simplistic as we may be) were practical, persistent, and actively against elite attempts to impose values and policy upon them. This positive image may derive from his assessment of their role in (or surrounding) the Restoration; in the 1930s, on the other hand, the common people were at best acquiescent, and at worst complicit, in the militaristic and imperialist transformation of Japan and receive (consequently?) much harsher treatment in Bix's recent review of Smethurst's book.[3] Are the common people accorded dignity and intrinsic significance only when their actions—in hindsight—have contributed to Progressive Historical Change? One hopes not.

Overall Bix traces a clear development in popular attitudes and actions from deference to (or at least toward) defiance. The only group he has difficulty with is the upper stratum of the peasantry: those who accumulated the

3. "Class Conflict in Rural Japan," *Bulletin of Concerned Asian Scholars,* Vol. 19, No. 3 (July 1987), p. 29.

greatest wealth and who became the government's commoner officials, presiding over villages and groups of villages and often siding with the government right down the line. But not always: the most common leaders of most forms of protest, throughout the Tokugawa era, were local officials. Bix, however, has trouble with this group: he sees them as government pawns or active allies, and professes difficulty (pp. xxxiff., 73–74) in explaining their leadership role against government. I think that it is in fact Bix's firm placement of black hats on this stratum that creates the difficulty: the village and district headmen were, as Kelly points out, in a Janus-like position, and it is precisely this which makes them so important. They did not lead as often as they pacified, but their own identification with their communities, their own vulnerability to popular sanction, and the danger of popular insubordination make their leadership roles easier to understand than Bix implies. And, as Vlastos points out, the evolution of their position is important: as time passed they became increasingly part of the rich-peasant/landlord/merchant stratum (the *gōnō*), increasingly integrated into the official framework of social control, and increasingly *un*able (regardless of their own will) to represent communities that were pluralizing apace under the impact of protoindustrialization.

Kelly, as noted, clarifies the position of the headmen, but my own feeling is that (perhaps for reasons unique to Shōnai or, more probably, to his momentary rather than evolutionary focus) he may underestimate the ability of headmen to represent unified communities. Kelly's peasantry is a richly variegated bunch—so much so that unified action of any kind, led by anyone, is hard to imagine. Classes, strata, occupational groups, and communities were divided in the cases he studied; indeed, in nineteenth-century Shōnai the unified village was a myth. Protest movements were heterogeneous in the extreme, incorporating (to an extent I suspect is also distinctive to Shōnai) samurai elites as well. The commoners, for their part, were nonrevolutionary, nonideological, pragmatic, narrow, and modest; they did not behave rigidly as *homines economici* (although all four of the movements he studies do reduce pretty nearly to money, either in the form of taxes or market access) but they did not advance great ideological principles either. They seem (as they do to Vlastos) a bit more passive and reactive than they do to Bix or Walthall, responding to government impositions rather than engaging incessantly in what James Scott has called "small arms fire in the class war."[4] When aroused they are capable of more sustained action than those in Bix's or Walthall's cases, but these events

4. *Weapons of the Weak* (New Haven: Yale University Press, 1985), p. 1. Vlastos, *Peasant Protests,* pp. 4–5, and Smethurst, *Agricultural Development,* p. 348, also note the ongoing nature of this engagement.

seem to spring more out of a tranquil background than out of a context of (for Bix) constant struggle or (for Walthall) contentious traditions and never-ending attempts at self-improvement or (for Vlastos) social, political, and economic structures inherently conducive to official autocracy, economic vulnerability, and popular insubordination. Who is correct: have Bix and Walthall read too much into periods of non-contention, or has Kelly ignored an undercurrent of conflict potential which could surge through any fissure between the tectonic plates of the status quo?

What Kelly has certainly not ignored—gratifyingly, given the usual bottom-up perspective of anthropologists—is the role of the authorities, who get about as much attention as do the common people in his study. Indeed, all three of these books illustrate the importance of the state (or nonstate social and economic elites) to an understanding of popular contention. Both Marxists and pluralists tend to reduce the state to either the ruling class or a congeries of contending groups respectively, underemphasizing the discreteness, autonomy, and independent causal influence of the state. None of the authors at work here makes that mistake.

Take Kelly: he makes a passing commitment in his introduction to look at the relationship of protest to state development, and returns to it briefly in his conclusion, noting that by the 1870s popular protest had come to include a focus on governmental accountability (to law, not necessarily to the people) which it had not had before. In between, however, the state and its official and semi-official agents crop up literally everywhere, critically influencing the emergence (or non-emergence) of contention and its course and outcome. In the 1830s domain concessions are cited in explaining the paucity of contention, despite crop failures and dearth. The 1840 anti-transfer movement owed its comprehensive character (it was a samurai-merchant-peasant coalition, or at least a set of simultaneous movements with the same goal and occasional overt collusion) to the threat posed to all elements by the harsh, repressive, and inept prospective daimyo of Shōnai, and it owed at least part of its success to divisions in the *bakufu*. The 1844 movement to prevent incorporation of *bakufu* territories into Shōnai domain, on the contrary, failed largely because the domain and *bakufu* were in this instance allied—indeed, the whole movement was generated and led by upper-stratum merchants and brewers and hardly seems like popular protest at all.

Another elite movement occurred shortly after the Restoration, in opposition to another proposed transfer of the daimyo; this succeeded by virtue of a "contribution" of 700,000 *ryō* to the new Tokyo government. An anti-tax movement at approximately the same time elicited government rebuff and punishment; the people responded with escalated protest and the government in turn responded with administrative reshuffles and restructuring, but when the movement continued its actions it was suppressed.

The "Wappa Movement" of the early 1870s, which began largely as a demand for monetarization of tax payments, early on tried petitions which became embroiled in the governmental division of 1873 over the Korea issue; in the summer of 1874 (after governmental concessions and proliferation of issues) it was repressed by the government and (its form thus constrained by state action) turned to litigation. Simultaneously it became caught up in the national-level Movement for Freedom and Popular Rights; the subsequent litigation and petition action also became mixed up in central government debates and divisions. And the *dénouement* of the movement, a series of court verdicts in 1878 and 1880 granting most of the people's demands, is interpreted by Kelly as a state weapon designed to subordinate local officials (the target of popular protest) to new nationally-mandated norms of administrative behavior. It is an extraordinary picture; I am not sure that Kelly intended to paint it, but it should serve as a stern lesson to any social historian who thinks that history without the state is any more valid than history without the people.

Bix stands at the opposite pole from Kelly, explicitly focusing on the state and its minions at every turn, but his picture of state involvement in popular contention is less detailed than Kelly's. It rests as much on faith as on data: he basically posits (recall the definition of "class" offered above) that *bakufu,* domains, and upper-stratum commoners were rapacious, repressive, and generally dastardly, and it is only natural that popular protest flourished under such conditions. On occasion he does turn constant (and therefore non-explanatory) exploitation into variable (and thus explanatory) phenomena, pointing out the crucial importance of enhanced governmental extraction in contexts of deteriorating economic conditions (this combination could be either absolute or relative, e.g., raising *or* refusing to cut taxes after a crop failure), and the equally crucial importance of domain-*bakufu* relationships: in more than one of his cases a domain caved in to protesters because it feared *bakufu* retribution, and apparently the people were well aware that domain succession crises, for example, were a time of acute domain vulnerability to the *bakufu* and thus to anyone who could bring down the *bakufu*'s wrath on the domain. Overall, however, although he gives a general picture of, and more directly addresses, the role of the state, Bix's picture is less vivid than Kelly's.

Vlastos's view of the state is relatively structural, like Bix's, in that he emphasizes institutions like the *kokudaka* system, the leveling of the *hyakushō* and urbanization of the samurai, and the ideology of *jinsei*—indeed, he has an entire chapter on the "Political Economy of Benevolence." But for him the state is a set of parameters, of structures conducive to contention, rather than an (overly reified?) agent of popular distress and outrage.

Anne Walthall is intrinsically least interested in the state. To her, the

most interesting aspect of the state was what happened to its own values, ideology, and principles when they fell into the hands of the people. Self-congratulatory official norms of benevolence became transformed into relief obligations; prior edicts were interpreted as proscriptions of new regulations; and statements of peasant obligation were used to justify remonstration ("It is *because* we are loyal and industrious that we are entitled to. . . ."). One need not wax hyperbolic about popular insolence and intellectual sophistication, but it is clear that she is not dealing with agrarian slugs unable to define, much less critique, their lot.

Walthall touches on other aspects of the state as well, but does little more than tantalize. She notes that the contention of the 1780s occurred disproportionately in *fudai* domains, which tended to be relatively fragmented and given to absentee daimyo, and raises the suspicion (which I share) that governmental regulatory and coercive capacity and consistency were inversely related to contention. Elsewhere she asserts that governmental repression was effective in the sense that few localities saw more than one "serious" protest during the Tokugawa era. This hypothesis I do not share: she does not define either "locality" or "serious," but a look at some county-level data reveals, *inter al.*, that Iwaki Shirakawa county, with 102 villages, had 88 incidents of contention during the era; Shinano Chikuma county, with 236, had 152; Tamba Taki, with 114, had 69; and Settsu Muko, with only 49 villages, had 98.[5] And in many instances counties with fewer than 10 incidents during the era were adjacent to counties with several dozen. Thus even such crude data as these actually strengthen an argument implicit in Walthall's study (and explicit in Bix's): they suggest the existence of subcultures of dissent, the vehicle for which must have been in large part the chronicles and tales of contention in which she is so interested. The spatial distribution of contention across Tokugawa Japan was highly variable, and neither political nor economic factors fully explain it: county A was obstreperous and county B, next door (and perhaps with a similar economic structure and the same overlord), simply wasn't. Neither Walthall nor Bix is eager or really methodologically able to explain why this was so, and Kelly's microscopic two-county focus is too fine-grained to do so. Vlastos, with a regional focus, does the best job of addressing this question, but his methods do not generate the interpretive

5. The data on contention are taken from Aoki Kōji, *Hyakushō ikki sōgō nempyō* (Tokyo: Sanichi, 1981); those on number of villages are from Kimura Motoi, ed., *Kyūdaka kyūryō torishirabe chō* (Tokyo: Kondō, 1954), Vols. 1–6. Kokushō Iwao, in *Hyakushō ikki no kenkyū, zokuhen* (Tokyo: Dōhōsha, 1971), pp. 31ff., noted the same geographical concentration of contention. Even the butchery involved in repressing the Shimabara Rebellion of 1638 did not lastingly cow the people of the area: Amakusa county was the scene of over 50 contentious incidents during the remainder of the era, whereas the county average for the *entire* era was only 12.

depth of Walthall and Kelly. The elephant of contention has enough organs and extremities to preoccupy a lot of blind persons (including myself).

It also presents a sufficient challenge to our understanding to stimulate various ways of attempting to grab an organ or two, if not the whole beast, and this is my final concern here: the variety of theoretical approaches represented by these three books. At one pole (as usual) we find Bix: militantly nomothetic, applying an accepted theory and deducing hypotheses which (unsurprisingly) we find confirmed across the board. This approach allows him to organize a mass of data and, as noted, elucidate a wide variety of patterns and processes. I do not agree with them all, and the approach is a bit too uncritical for me—it is hard for me to imagine any theory in the social sciences that can be applied to a case like Tokugawa Japan and fit with no revision at all. But my greater disagreement lies in what seems to me to be the teleological imperative of the theory—it leads Bix to see the overthrow of the Tokugawa foreshadowed in the contention of the late eighteenth century, a shadow I have missed—and its deductive imperative—if the theory implies it, it must have been there. Two examples will suffice: first, popular contention fell off sharply after the 1780s and remained at a relatively low ebb through the first quarter of the nineteenth century. The question that occurs to Bix is: "Why did the dissident peasants of the 1780s so quickly assume the yoke after having succeeded, momentarily, in throwing it off?" (p. 132). The answer dictated by theory is that repression and indoctrination succeeded; that suggested by other data (and even by Marxist historian Hayashi Motoi) is that agricultural and economic conditions improved, partly as the result of real concessions wrung from the government.[6] What his theory does not permit is the conclusion that class struggle ever substantially abated. Similarly, in a discussion of the radicalism of popular demands, Bix acknowledges that the development of "antifeudal consciousness" (p. 139) was slow; one might offer the alternative position that it never appeared at all, but that would probably lock one into dismissing any progressive role for popular contention, and Bix clearly does not wish to do this.

Similarly theoretical, though more broadly and less rigorously so, is Vlastos. He locates his study in the theoretical debate between moral-economy and rational-choice, and between deprivation and resource-mobilization, explanations of popular contention, and non-Japanist readers can relate easily and comparatively to his approach. But his book is not an explicit "test of hypotheses" or confrontation of models; it is a theoretically-assisted inquiry and an attempt to refine theories in light of data.

In an in-between position we find Walthall, whose position may be

6. Hayashi Motoi, *Zoku hyakushō ikki no kenkyū* (Tokyo: Shinhyōron, 1976), pp. 33ff.

briefly described as atheoretical generalization. Hers is, as noted, a wide-ranging "forest" study, but it does not rely upon any expressly theoretical framework with which I am familiar. Theory is less important to Walthall than to Bix, since she is not engaged in causal analysis; she interprets from specific events, processes, legends, and records, and generalizes to Japan of the 1780s as a whole, and she devotes considerable time to descriptive generalization as she covers the major types of contention of the day. One may argue that her types are too general and homogenizing, or that her interpretations of contention as demonstrating peasant intelligence are too positive, or that her description is not thick enough, but she has documented peasant mentality in a way that such critics must refute empirically.

Kelly is as relentlessly idiographic as Bix is nomothetic, sacrificing breadth to depth at almost every turn. This in itself is a legitimate strategy, but I was a bit frustrated by Kelly's introductory enticements, because he does touch upon several of the better-known debates in Tokugawa historiography but never returns systematically to this level (although he does offer data one can relate by oneself to the initial theoretical discussion). The initial discussion promises to relate contention to the development of a constitutional state and a capitalist economy; the conclusion contents itself with the observations that the earlier contention focused more on minimizing tax burdens and the later more on market access (a statement that might constitute a valid comparison across the Japan of 1700 and 1800, or 1750 and 1850, as well), and that the later dissent aimed more at limiting the prerogatives of government and increasing its accountability (again, a statement that describes village-level reform movements found in many regions of Japan throughout the latter half of the Tokugawa period). It's not that he doesn't give one enough to chew on, and it's certainly not that he should have written the book as another would have, but I wish he had drawn out some of the implications of his findings a bit more than he does.

On the other hand, perhaps his caution in generalizing is wise, and my lack thereof too rash. Shōnai appears atypical, and the types of contention Kelly examines do also, and generalizing from such data is akin to closing one's eyes and taking a dive from the Kiyomizu Temple. And my concern with the typical may be itself misguided—Kelly might deny any "typicality" anywhere in Tokugawa Japan, and he may well be right. It is a reviewer's prerogative to be autocratic and to dictate an orthodoxy from which authors deviate at their peril. But, at least in the field of Tokugawa popular dissent, we all live—ontologically, epistemologically, and methodologically—in glass houses. Thus I see these books as synergistic, not as rivals to be weighed against one another. Together (and especially when combined with Vlastos) they constitute a multidisciplinary, multimethod, multilevel overview of the field, each with something to offer and some-

thing with which to take issue. Each (in my opinion) is incomplete, overemphasizing some things and overlooking others, as is also my own work in the field (as some of these authors have been helpful enough to point out); together, one hopes, we can get a firm grip on our elephant.

Visions of Virtue in Tokugawa Japan. By Tetsuo Najita. University of Chicago Press, Chicago, 1987. x, 334 pages. $37.50, cloth; $14.95, paper.

Reviewed by
Kate Wildman Nakai
Sophia University

Visions of Virtue in Tokugawa Japan, by Tetsuo Najita, is both a rewarding and frustrating work. It is rewarding for its provocative analysis of the intellectual relationships between a number of eighteenth-century thinkers associated with the Kaitokudō, an Osaka academy backed financially by a consortium of leading merchants and granted an official bakufu charter in 1726. It is frustrating because this illuminating analysis is embedded in an overly reductive interpretative framework in which the Kaitokudō thinkers are cast as champions of the right of commoners to pursue virtue in the face of a general assumption that such activities were the unique prerogative of the "aristocracy" (the *bushi* class).

The most original and interesting aspect of Najita's work is his skillful delineation of the interaction between the contrasting approaches to knowledge running through the history of the Kaitokudō. Najita depicts the successive generations of Kaitokudō thinkers and their immediate predecessors as sharing a deep interest in the fundamental Confucian question of how to establish reliable links between social life and virtue. They also shared the conviction that knowledge of virtue was within the grasp of the ordinary individual. However, Najita argues, disagreement about how such knowledge could be acquired led to an ongoing dialogue in which two different epistemological positions were repeatedly posed against the other. Najita terms one of these "historicist." The other he describes as based on a "natural ontology."

The "historicist" approach he sees as associated in the first instance with Itō Jinsai, who denied the validity of the Zhu Xi Neo-Confucian program for assimilating oneself with a timeless, absolute moral truth through systematic investigation of the principles governing the natural order coupled with introspection into one's own nature and the disciplining of