Communications to the Editor

Editor's note: Readers of this exchange between Philip Huang and Kenneth Pomeranz might also be interested in "Integrating China into World Economic History" by R. Bin Wong, which is posted on the AAS website, http://www.aasianst.org. We are publishing Wong's contribution to this debate on the website rather than in this issue because of production issues involving both time and space.

Further Thoughts on Eighteenth-Century Britain and China: Rejoinder to Pomeranz's Response to My Critique

PHILIP C. C. HUANG

LET ME START BY ADMITTING to an error I made, so that we can set it aside and move on to the substantive issues. Pomeranz mentions this particular error no less than one dozen times in his response (see Pomeranz 2002a). He almost makes it his central theme. I must say that I was taken quite by surprise, since I could not find it in my critique to which he was responding. I did find it eventually in my 1990 book (*The Peasant Family and Rural Development in the Yangzi Delta, 1350–1988*) in a passing reference that rice sold for 0.06 *tael* per catty, when the figure should have been 0.006. But, fortunately the error was an isolated one; even Pomeranz grants that the rest of what I said about prices is correct. My arguments are, in any case, based not on price data but on what I would call "conditions of production," such as farm size, labor input, crop mixes, animal and fertilizer use, techniques, yields, and the like. That was true of my 1990 book, as it is of my critique; the latter, in fact, does not mention prices at all.

Let us go back over the important points I make about handicraft production. First, weaving was the highest paying part of handicraft cloth production, bringing returns comparable to grain farming. This is unlike spinning, which brought only one-third to one-half of the returns to grain farming. Pomeranz now accepts these facts. He, to his credit, also accepts the criticism I made of him (Pomeranz 2002a, 559 n. 28): he mistakenly had assumed that weaving took up three of the seven days required to produce a bolt of cloth, when it in fact took up just one, while low-paying spinning took up four (the remaining two days being spent on fluffing, sizing, and other miscellaneous tasks) (Pomeranz 2000, 322; see also 102, 319–20; Huang 2002b, 513, 517). He now acknowledges this to not be the case.

The fact that the largest part of cloth production, spinning, took up four of the seven days and paid just one-third to one-half of what farming paid makes it obvious that for a peasant population to give more and more land to cotton and cloth production in favor of grain meant reduced returns per unit of labor. Indeed, while I myself would refrain from attempting any exact quantitative estimate, given the many imponderables involved (especially the composition of household labor), Pomeranz in his response attempts a precise measure of the differences in returns between the two activities. He

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arrives in the end at a difference of 1:3 and/or 1:2, after converting child labor used in cotton and cloth production to adult equivalents (2002a, 548). His figures seem to be within the ballpark. They serve to underscore the basic point here: that cotton handicraft production represented a reduction from farming in returns to labor. That is in fact the heart of my case for involution in my 1990 book, as in my present critique, and Pomeranz tells us that now he agrees with it: "A decline in average returns to labor as more people move from grain to cotton farming is still evident" (547). Pomeranz grants, in addition, that a second crop of wheat, which became more prevalent in the Yangzi delta in the eighteenth century than earlier, also meant diminished returns to labor (543). He and I, then, have really no disagreement about the basic fact of involution in the Yangzi delta.

His agreement with me on this now, I should point out, is somewhat camouflaged by his attack on what he terms my exaggerations of the differences between grain production and cotton-cum-yarn and cloth production. He goes on at some length to claim that I confuse differences in total labor input with differences in output. Although I pointed out that one *mu* of cotton and cloth production required eighteen times the labor for one *mu* of rice and twenty-seven times the labor for one *mu* of wheat, I never suggested such a differential in returns. I specifically said in the section titled "Labor Intensification" that I was talking about just that, labor input, to set the stage for the discussion of involution, or diminished marginal returns to labor, to follow in the next section. After all, as I pointed out repeatedly, spinning was the lowest-return part of cloth production, just one-third to one-half of farming. It would be ridiculous to suggest that the differential in returns could be as much as eighteen to twenty-seven times. Those who have read my article will see that there is no ambiguity in my presentation on this point.

Another diversionary issue should be clarified. Pomeranz makes much of what he considers an inconsistency in my use of figures for consumption requirements (2002a, 560). What he has done here is fail to notice the difference between what I say is the average grain consumption requirement per capita (two *shi*) (Huang 2002b, 509), including children, and what I say is the average grain consumption per adult (three *shi*) (517). That is a small point, but there is a more important substantive issue here. What I am referring to in the two *shi* and three *shi* figures (320 and 480 catties of rice) is grain that actually is consumed, and that alone, literally catty for catty of rice actually eaten. What Pomeranz mistakenly does with the figure is to confuse it with subsistence requirements, forgetting what peasants would need in the way of foods supplementary to grain and also of other necessities, such as clothing, oil, and salt. I would like to see anyone survive on just what Pomeranz would allot to subsistence: that is, only the grain actually consumed. Grain alone, in fact, typically amounted to just 50–60 percent of the budget of a peasant household (Huang 2002b, 523). If we want to talk about subsistence, rather than just the amount of grain consumed, we need to add at least two-thirds to the two/three *shi* figure. This point, while not centrally important in Pomeranz's response to me, is central in his response (Pomeranz 2002b) to the article by Robert Brenner and Christopher Isett (2002).[1]

Pomeranz does make in his response to me a point that is worth addressing

[1]There is a further distraction, in the appendix, in which Pomeranz tries to argue for a "fertilizer revolution" (and, by implication, an "agricultural revolution"), even though he has already conceded the point about involution. I address this question in some detail in my response (Huang 2002a) to Jack Goldstone (Goldstone 2002), written for the 3 June 2002 conference at the University of California, Los Angeles, on "*The Great Divergence?*" and thus will not do so here.

seriously here: namely, that rice cultivation in the delta was so productive that it compared quite favorably with English wheat. Rice was indeed among the highest-yielding crops in China, both in terms of output per unit of land and output per unit of labor. Pomeranz suggests that while rice output per workday in the delta might have fallen short of wheat output per workday in England, it was roughly comparable in terms of nutritional value (2002a, 543). He bases his per-workday computation on the figure used in my critique, of ten days of work for one *mu* of rice. My figure, in turn, was based on two main sources, John Lossing Buck's survey, which shows an average of ten days (1937, 314), and Jiang Gao's [1834] 1963 agricultural treatise, which gives the same figure after detailing the individual tasks.

All well and good—until, that is, comparisons with England bring out complications that demand a higher degree of precision in the data. Let me just point to some of the difficulties involved. In the appendix, I put together the information from the three most detailed accounts known to me of labor use in rice cultivation. It should be apparent that Jiang's count, and most likely also Buck's, did not include the work required for hulling the rice. This is an entirely understandable omission, since hulling generally was done at a mill and since peasants usually thought of and spoke of rice yields in terms of unhusked rice, *daogu*, rather than husked rice, *daomi*. Our discussions, however, have been based on husked rice, and Jiang points out that one person could husk at most one *shi* in a day ([1834] 1963, 11a). Further examination of the data in the appendix shows that Jiang also left out of his count the work required for irrigating the field—this because it was "indeterminate." As Fei Hsiao-t'ung (our second detailed source) made clear, generally one day was needed to water one *mu*, but the number of times a field had to be irrigated (or drained) depended very much on how much rain fell (1939, 163). Jiang chose not to give that part of the work a specific number. The Japanese Mantetsu investigators in Wuxi (our third detailed source), by contrast, counted six days for watering one *mu* six times in 1941, which accounts for a large part of their difference from Jiang. Thus, Jiang's figure of ten days arguably should be increased to thirteen if we were to add an average of three days for watering the field. If we further include hulling the rice, then we would need to add another 2.25 days (at a yield of 2.25 *shi* per *mu* and 1 day for hulling each *shi*) for a total of 15.25 days of work for one *mu* of husked rice. Fei's work suggests, moreover, that when oxen were not used for plowing, as appears to have been quite often the case in mulberry-rice areas near the Taihu Lake (e.g., Kaixiangong village), more time was needed to turn the soil. All of this, of course, still does not count other related work, such as accumulating fertilizer and feeding the animals.

The point I wish to make here is that if we want to compare the delta with England, then we need to pay close attention to the conditions of production and make sure that the specifics are matched up, so that work in the field is compared only with work in the field, and if work off the field is included, then it should be included in both. Similarly, if we are talking about unhusked rice in the delta, then we had better make sure that we are comparing it with unmilled wheat and if husked (ready-to-cook) rice, then milled (ready-to-bake) wheat flour. All of this will require original research into the source materials for both the delta and England. As yet, no such research exists to my knowledge; reliance on secondary scholarship only, in the manner of Pomeranz's response (and indeed of his entire book), just will not do.

There is another big problem with any England–Yangzi delta comparison. A truly meaningful comparison will have to take into full account English animal products along with grain. We need to establish precise and convincing grain equivalents for meat, milk, cheese, and fats. Output per unit of labor in animal products in England, I expect, will be substantially higher than in grain. It just will not do to compare only

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grain, as Pomeranz does, and treat the mixed husbandry-crops agriculture of England as if it were the same as the Yangzi delta's crops-only agriculture.

An England–Yangzi delta comparison, finally, should also take into account the implications of the very great difference in average farm size. I would like to examine one additional implication in particular thus far not explicitly spelled out in our discussions. Both English and delta farms, to be sure, were subject to seasonal time constraints in terms of their agricultural portfolios: rice in the delta, for example, had to be planted within a period of a few weeks. There is, however, a big difference between a 125-acre farm, as in England, and a 1.25-acre (7.5 *mu*) farm, as in the delta in 1800, or even between a 20-*mu* farm, as in the delta in 1400, and a 7.5-*mu* farm, as in the delta in 1800.[2] By the later period, most delta farms were pressed by land scarcity to cultivate fewer *mu* of grain than were allowable under seasonal time constraints alone. The tradeoff, as we have seen, was with lower-return cotton-yarn-cloth. That, too, was a manifestation of involution.

This brings us back to the main point here: that one cannot dispute the fact that the delta was undergoing involutionary change as peasant households turned more and more to lower-return (than rice) winter wheat and lower-return cotton-yarn-cloth. That was the first of the main points in my critique. It is really an obvious argument and one that should not be at all controversial. After all, we still teach today's students in first-year economics the basic law of "diminishing marginal returns": continued addition of one resource, in this case labor, while others are held constant, in this case land and capital, will sooner or later lead to diminished marginal returns to that one resource. Pomeranz, we find out now, actually agrees with me on this. He even takes the trouble to try to estimate the precise extent of it. I think this is as it should be; it would be very surprising indeed if involution did not occur, given the tremendously high degree of labor intensification in Yangzi-delta agriculture. Farm sizes, after all, were just one-hundredth that of England and per-capita cultivated acreage, just one-forty-fifth.

What is surprising and needs explaining is when involution does not operate. That brings us to the English agricultural revolution, the second of the main points of my critique. What is truly surprising about the English agricultural record of the eighteenth century is that, in the context of a preindustrial economy, labor productivity in agriculture doubled or went up by at least three-quarters. That makes it dramatically different from the Yangzi delta record, in which Pomeranz and I now agree returns to labor declined. Pomeranz does not dispute this central fact of a doubling of labor productivity in English agriculture during the century. Indeed, it would be difficult to do so, given the tremendous weight of the evidence. What he does instead is engage in a number of diversionary arguments and then somehow at the end almost deny that there was an English agricultural revolution (e.g., Pomeranz 2002a, 554: "despite Huang's insistence on an 'English agricultural revolution'"). We should bring the discussion back to the central point of the surprising doubling of labor productivity in English agriculture in the eighteenth century and ask Pomeranz to respond to it.

Related to this point is the basic structural difference between English and Yangzi delta agriculture, which I discuss at some length in my critique. English agriculture mixed cropping with animal husbandry, a mix that was well represented by the classic Norfolk system, while Yangzi delta agriculture was virtually a crops-only economy, to the exclusion of animal husbandry. This made for a great host of differences between the two economies, not least of which was the lower capital

[2]See Huang 1990, 342 for the few relatively "hard" numbers we have on population and cultivated area for 1393 and 1816.

intensity—in use of animal power and animal fertilizer—of delta agriculture. Pomeranz now concedes that "English farming was indeed more capital intensive" (2002a, 549), but despite that admission, somehow he still manages to maintain that there was really no great difference between English and Yangzi delta agriculture. His disregard of the basic structural difference in the two agricultures leads him, as we have seen, to untenable comparisons of grain output without considering animal products and also, as we will see, of food consumption.

The agricultural revolution is just the first of six developments in eighteenth-century England that I discuss in my critique, following the lead mainly of Jan de Vries. (As a memory device, we might refer to them in shorthand as "five revolutions and a mineral.") De Vries (1994) spoke of a host of changes demonstrated by the accumulated research of the last two decades in what he calls the "Revolt of the Early Modernists" against earlier scholarship on the industrial revolution that focused exclusively on the nineteenth century. The second of those big changes has to do with what de Vries calls the "new urbanization"—namely, the vigorous development of small towns and cities of sizes between 5,000 and 30,000, which exploded four-fold between 1750 and 1800, while older larger cities remained stationary. It was a process that began in England about 1670 and by 1800 had resulted in a population that was 27.5 percent urban, compared to perhaps one-third that rate in the Yangzi delta. Pomeranz now concedes this as well: "Jiangnan did indeed trail far behind eighteenth-century England by this indicator" (2002a, 553). Yet, somehow this makes no big difference to him, and he insists still on an equivalence between England and China.

The third and fourth big change, related to this new urbanization, centered on the process that has been called variously "proto-industrialization," "nascent capitalism," and, most recently, "industrious revolution." Although that process included handicraft production that was subsidiary to and lower paying than farming, it also saw changes in which handicraft production became a genuine alternative to farming, with higher returns than farming, and, as a consequence, led to some basic changes. It allowed handicraft production to become separated from farming and become urban, rather than rural, based, unlike in the Yangzi delta where it remained inseparable from farming. It also made for changes in demographic behavior by allowing young people to marry earlier, instead of waiting to inherit the family farm. Pomeranz does not seem to dispute these facts now, although he tries once more to find equivalence with China by emphasizing the involutionary dimensions of the English phenomenon. A more substantive response would require him to address the implications of the differences from Yangzi delta handicraft production that remained subsidiary to, and inseparable from, farming. To merely assert that involution occurred everywhere and that "under premodern conditions, average labor productivity in farming will almost always be significantly above that in home textile production" (Pomeranz 2002a, 549) does not help his case, especially not when his own figures (table 1, 551) show proto-industrial wages in mid-eighteenth-century England to be consistently well above those from farming. Here I want to ask Pomeranz: How would *he* account for the lack of separation of handicraft production from farming in the delta, compared to its becoming a basis for the "new urbanization" in England?

The fifth big change, and the one most recently highlighted in the literature, is what might be termed the "consumption revolution" of the eighteenth century. De Vries and others have demonstrated major new changes in rural consumption patterns. There were both more goods being produced in the countryside and more rural demand for urban goods, such as mirrors, paintings, books, clocks, pottery and deftware, curtains, and silver display objects ranging from spoons, decanters, and bible clasps to personal adornments for both men and women. Those changes, obviously, were related to enhanced incomes from the agricultural revolution, as well as from proto-industrial employment, and were also related closely to growth of the towns-cities in the "new urbanization." Those changes all helped prepare the ground for the industrial revolution to come. Pomeranz does not dispute this story of consumption change for England. Here I want to ask whether the two economies can be as much alike as he says if in one consumption changed dramatically and in the other it did not.

Pomeranz wants so much to maintain that China and England were roughly equivalent that he introduces in his response a new argument not in his book, and with that argument, new errors not in his book. In England, he claims that rural laborers, "still . . . the largest portion of the population," consumed mainly bread and potatoes (more than 90 percent), with very small proportions of meat or milk (2002a, 566). That, according to him, makes English food consumption comparable to Yangzi delta food consumption shown in Fang Xing's 1996 study (Pomeranz 2002a, 565).

The problem with this approach is that rural laborer consumption in England was hardly representative. As J. C. Drummond, Anne Wilbraham, and D. J. Oddy point out, although grain might have been the staple of the poor, those who could afford it generally ate more meat (Drummond and Wilbraham [1939] 1958, 299; Oddy 1990, 256). Pomeranz also makes the mistake of attempting to calculate proportions of food intake by pound weight alone, regardless of the differences between grain and animal products in cost and caloric content. Indeed, if typical English food consumption were predominantly of bread (grain) and not much of animal products, then its agricultural economy would have to have been a crops-only one like the Yangzi delta's. Yet, we know that English agriculture typically combined animal husbandry with crop production, in roughly equal proportions, as in the Norfolk system of rotating wheat–animal feed (turnip)–barley–animal feed (clover). Where did all that animal product go? Once again, it just will not do to disregard the mixed crops–animal husbandry structure of English agriculture and treat it as if it were a crops-only economy just like the delta's.

Pomeranz also errs in his misunderstanding of Fang's article on Yangzi delta consumption. According to Pomeranz, this article is based mainly on information about agricultural workers and hence is really just about "the very poor" (2002a, 565). Fang's cited sources, in fact, include information about both agricultural workers and peasant households in general, and Fang himself clearly intends his to be a study of general peasant consumption. Fang sees no problem with his sources from this point of view because, of the three agricultural treatises he mainly relies on, he knows that Zhang Lüxiang's *Bunongshu* (1658) has much to say about typical peasant households—part of the treatise is actually devoted to developing a plan for a household farming ten *mu*. He expects his readers to know also that Jiang's [1834] 1963 and Tao Xu's 1884 treatises come long after the mid-eighteenth-century formal legal recognition of the fact that most agricultural workers and their employers "normally sit and eat together, and address each other as equals" (quoted in Huang 1985, 95). This change in the eighteenth century is a well-known story, first studied in detail by Jing Junjian and examined further by me in both my North China and Yangzi delta books (Jing 1961; Huang 1985, 90–99; 1990, 63–69). Specifics about agricultural workers' food consumption, therefore, are fully applicable to the general peasant population. The agricultural worker's fare, in fact, was arguably better than his employer's family's because of the extra effort he had to put forth and because of the consideration that his employer needed to feed him well in order to elicit the best work from him.

What Pomeranz has done with his new endeavor on food consumption, in other words, is take the atypical (the poor, albeit a large proportion of the agricultural population who made up just over one-third of the total population) in England for the typical and the typical in the rural Yangzi delta for the atypical. He tries to find equivalence by comparing the poorest in England with the average in the delta. But the fact is that eighteenth-century England was sufficiently well off that the near subsistence wages of its village laborers were characteristic only of the poor, while eighteenth-century Yangzi delta's entire peasant economy was so close to subsistence that peasant employers consumed basically the same fare as their hired workers—at the same table. Agricultural workers, usually single males in their prime, were at the bottom of rural society not because they ate poorly relative to other peasants but because they were propertyless and generally could not afford to marry and maintain a family.

In regard to cloth consumption, Pomeranz does acknowledge that he had been mistaken in suggesting that a Yangzi-delta peasant might have consumed as much as ten bolts of cloth and two bolts of silk per year (enough for an unthinkable level of consumption of ten outfits of cotton and two of silk per year!). Pomeranz now says that I "rightly note that [his suggestion] . . . would yield an implausibly large amount of clothing for people in the region" and that therefore, "it is probably worth adjusting my [Pomeranz's] estimates for Jiangnan cloth production down a bit" (2002a, 567). Nevertheless, he somehow finds it possible to insist on rough equivalence between England and the Yangzi delta, not least by his new, faulty arguments about agricultural output and food consumption summarized above.

We come now to population history, in which Pomeranz simply refers to James Lee, Cameron Campbell, and Wang Feng's (2002) response to my critique. Now, the main point of my critique of their works was what they did with female infanticide. They estimated that 25 percent of girls born were killed, and they made that the cornerstone of their argument for deliberate birth control practices in China and hence for equivalence between China and Europe in deliberate fertility control (i.e., "preventive checks" in Malthus's terms), as opposed to deaths from poverty and starvation ("positive checks"). For their argument, they invented the special concept of "postnatal abortion" for female infanticide, making it neither a birth nor a death. I argued instead that female infanticide, whatever its yet-to-be-demonstrated extent, is evidence instead of the pressures of poverty. The poor accounted for most drownings and killings of female infants, as they did for most sales of girls and women. Female infanticide, like sales of girls and women, was symptomatic of what I call a mounting social crisis from the eighteenth century on.

I am actually a little surprised that Lee, Campbell, and Wang have elected not to dispute my argument but, rather, to concede readily that "[w]e have never disputed that female infanticide may have been more common among the poor or during crises" (2002, 598). This point is really all I wanted to argue. If female infanticide was the consequence mainly of poverty and subsistence pressures, then its prevalence, whatever the exact extent, serves to support my theme about a "mounting social crisis" in China, not its absence, and about the difference between China and England, not their equivalence.

What Lee, Campbell, and Wang concentrate on in their response to me is quite trivial from the point of view of my critique. They do a computer simulation of Chinese population at different fertility rates. They maintain that the 7.5 fertility rate that Arthur Wolf (1985) (with whose work I am in general agreement) argued for would result in an implausibly high rate of population increase. As Wolf pointed out in his oral response to Lee at the 3 June 2002 conference at the University of California, Los Angeles, on "*The Great Divergence*?" Lee, Campbell, and Wang have failed to distinguish between marital fertility and overall fertility (which would be lower), mistaking Wolf's marital fertility rate for overall fertility. I wish to point out in addition that Lee, Campbell, and

Wang do not discuss in their simulation exercise the reasoning behind their choice of mortality rates in the simulation, obviously the other key variable. If one introduced the female infanticide rate of 25 percent that Lee, Campbell, and Wang themselves had argued for, then that would surely raise the mortality rate—in a gender-specific way—above their present simulation, as well as lower the fertility rate below their present simulation. Or, are we in this simulation exercise still treating female infanticide as "postnatal abortion" to be removed from the count of both birth and death rates?

We come finally to coal. Pomeranz now agrees that China was indeed rich in coal deposits and that England's divergence is not to be attributed to its chance endowment of rich coal deposits alone, which he had argued in his book. He still insists, however, that transport problems precluded earlier development of coal for use in the Yangzi delta. Although this whole question seems to me to require more research, it should be pointed out here that the Pingxiang coal mines produced by 1905 nearly 200,000 tons of coal and 54,000 tons of coke a year, still relying mainly on transport via the Xiang River that connected to the Yangzi (Hornibrook 2001, 222–23). What needs explaining is the how and why of earlier development of coal in England. To answer that, I suspect, will require examination of both earlier industrial demand in England and earlier advances in British and European science and technology. Pomeranz does not seem now to disagree with this.

I would like to come now to a few more general points, not made in my critique of Pomeranz, that seem to me should be kept in mind in this discussion.

First, let us take apart that word "Malthusian." Lee, Campbell, and Wang, and perhaps also Pomeranz, would like to pin that label on me. By that word, Lee, Campbell, and Wang, I think, mean at least the following package of things: population determinism; the binary of "preventive" and "positive checks"; by implication, also the binary of the West vs. the non-West (complete with its possibly racist undertones); and, lastly, the idea of diminishing marginal returns with respect to labor. Let me say that I mean by "involution" just the last of this set of ideas and nothing more. I would never associate myself with either population determinism or Eurocentrism. The idea of reduced returns to labor under conditions of very high labor intensification is the only thing I take from Malthus. If that makes me a Malthusian, then I hope all of us would opt to be Malthusians.

Lee, Campbell, and Wang also point to differences between what they call social scientific history and social history. According to them, one involves rigorous testing of hypotheses with large-scale quantitative data, while the other employs merely anecdotal evidence. I would put it differently: too much of social scientific history ignores local contexts and knowledge. One intention of my critique of Pomeranz and of Lee, Campbell, and Wang is to show how disregard of local context, what I would call conditions of production and of life, can cause serious errors in number crunching.

Finally, I want to say something about "theory." I do not for one moment consider myself to be presenting a generalized "theory" of development or underdevelopment in the manner of modernization theorists. In fact, it was precisely the objection to unilineal modernizationism that originally drove my work and that of other social historians. In comparing England and China, what I suggest is that their historical records show two very different paths of development. In England, there was the conjuncture (in other words, coincidental intersection of semi-independent tendencies) of an agricultural revolution with a new urbanization, proto-industrialization, its concomitant demographic changes, and consumption changes, plus the early development of coal (and no doubt also other changes yet to be highlighted). That set the stage for the industrial revolution to come. In China, the path was very different. It required a social revolution and its reallocation of resources to jumpstart industrial development, then the help of modern urban industry to initiate a very distinctive rural industrialization, and then foreign investment and the stimulus of the world market, before the coming of vigorous modern development. Even then, which is to say today, a great deal more is needed in rural development before China can truly reach the level of labor productivity and income of the developed countries. In other words, in my view there is no single path or single causal factor to modern economic development or underdevelopment. What we are talking about here are two different historical records. The issue here is not anti-Malthusianism vs. Malthusianism, anti-modernism vs. modernism, or anti-Eurocentrism vs. Eurocentrism. To suggest otherwise is to try to set up phony issues rather than confront the genuine ones being posed.

Let me come back in the end to the central issue for Pomeranz and for the reader: if it is true that in the eighteenth century, Yangzi delta labor productivity declined and pressures of poverty were evidenced, among other things, in female infanticide and the widespread sale of girls and women—while in England there was a host of changes not found in the Yangzi delta, including a (near) doubling of labor productivity in agriculture, the increase of urban population to about three-fold the proportion in the delta, the rise of proto-industrial production in towns and cities, dramatic consumption changes, and the very early development of coal, none of which Pomeranz seriously disputes—then how plausible is it that the two economies remained roughly equivalent?

Appendix: Workdays Required for One *Mu* of Rice in the Yangzi Delta, as Reported by Mantetsu Investigators (Wuxi), Jiang Gao (Pu-Mao area), and Fei Hsiao-t'ung (Kaixiangong)

		Wuxi, 1941	Pu-Mao, 1834	Kaixiangong, 1936
preparing the seedlings		1.75	not counted? included below?	not given
soaking the seeds	0.25			
preparing the seedbed	1.0			
sowing the seeds	0.5			
preparing the field* (plowing, harrowing, applying base fertilizer)		3.0	3.0	4.0
transplanting the seedlings (including transport of seedlings)		1.5	1.0	2.0
weeding and intertilling three		3.0	3.0	"varies"
times, applying chase fertilizer		1.0	included in above	not given
irrigating		6.0 six times	"indeterminate"	one day each time
harvesting,		1.0	1.0	
and transporting		1.0	not given	
threshing		2.0	2.0 (including sorting and bundling of stalks)	not given
hulling		not counted	not counted ("at most one *shi* per day")	not given
total		20.25	10.0	not given

Sources: MT 1941, 69–74; Jiang [1834] 1963, 11a; Fei 1939, 159–65.

*In mulberry-rice growing Kaixiangong near the Taihu, the soil was turned with the "hoe" (*tieta*) by hand rather than with the plow and oxen (Fei 1939, 159–60).

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Facts are Stubborn Things: A Response to Philip Huang

KENNETH POMERANZ

Overview

In our last exchange, I showed in detail that Philip Huang's concept of "involution" was internally inconsistent; he had misrepresented my work and that of other scholars he invoked; he had repeatedly confused marginal and average returns; he had indiscriminately compared adult male, female, and child labor as if we would expect them to be equally productive; he had relied on a cloth consumption estimate which was derived by assuming his conclusion and which could not be reconciled with empirically based work that he had relied on elsewhere; and he had made a basic

My thanks to Sherman Cochran, Gail Hershatter, and R. Bin Wong for their careful and helpful readings of earlier drafts of this essay. They are not, of course, responsible for any remaining errors.

arithmetic mistake (a misplaced decimal) that completely invalidated his central point about the textile economy. Rather than answering these points, Huang has written a largely tangential response with eleven brief sections. Three are worth exploring, but doing so ultimately strengthens my position; the others are completely erroneous and need only to be corrected.

In the section on agricultural labor productivity, Huang usefully corrects his own previous estimate of labor per *mu* of rice (which I had accepted for use in our last exchange). Unfortunately Huang does not follow through by using this figure to estimate labor productivity and misunderstands my estimates, which were biased enough in favor of England that changing this one number leaves their conclusions intact. In fact, estimates using even higher labor inputs for rice still give us equal agricultural labor productivity in Jiangnan and England circa 1800. Moreover, reducing labor productivity estimates for rice, as Huang now wishes to do, undermines his "involutionary" model in another way, erasing the supposed decline in productivity as people grew more wheat and cotton.

In the section on consumption, Huang questions the significance of laborers' budgets for comparing consumption in Jiangnan with that of England more generally, arguing that impoverished landless laborers in Jiangnan were typical of their society, while England's equally poor rural proletarians were very unusual. We need more research about consumption and income distribution, and Chinese data are scarce. But again, Huang never follows up on his opening gambit with any analysis or further evidence. As I will show, what evidence we do have is largely against him; it suggests that probably the bottom 75 percent of the Jiangnan population lived at least as well as their English counterparts.

In the section on handicrafts, Huang concedes that he underestimated earnings from weaving by a factor of ten and claims that this does not matter because prices are irrelevant. But since Huang's principal argument is that the growth of textile production yielded such low returns that it was the antithesis of true development, this error alone would destroy his case. Moreover, Huang's discussion of handicrafts actually shows why price data are indispensable, not irrelevant. It also repeats Huang's earlier errors of treating male, female, and child labor indiscriminately.

Huang's other sections are insubstantial, but correcting them does illuminate his methodological problems and some other issues. He relies on old, often vague, truisms, such as the claim that because English farms were larger, English agriculture must have been more productive per person (see the sections on urbanization and diet). He introduces no new data and ignores well-known facts (e.g., the much lower yields per acre on English farms, the much higher expenses on English farms for feeding work animals and for other capital costs, and the fact that large English farms supported wage laborers as well as their owners) that show these truisms to be myths. Most of these assertions were already refuted in our last exchange; he ignores that here and repeats them as if they remained unchallenged common sense. Many also rest on simple misreadings. Space precludes correcting all of Huang's errors; I highlight a few in my next two sections, before moving on to meatier issues.

Refutations Unrebutted

Huang's section on handicraft productivity asserts that the enormous error I pointed out in our last debate—that by misplacing a decimal point in the rice price, he underestimated net labor productivity in cotton weaving by more than ten times—

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is not a "substantive issue." He claims that upon reading about this error, he did not know where it appeared (although the page was cited) and "eventually" found it in a "passing reference" in his 1990 book. But, this point was actually the foundation of Huang's work. His entire argument rests on textile labor yielding very low returns; this is his only calculation of those returns (1990, 84–86), and it is wrong by a huge margin of ten to fourteen times for weaving and over five times for weaving and spinning combined (Pomeranz 2002, 555–64). I will consider other handicraft issues later.

The section on demography is not much better. Here, too, Huang tries to wave aside a mathematical disproof of his claims. As James Lee, Cameron Campbell, and Wang Feng have shown, applying the fertility rates that Huang (2002, 526; 2003) takes from Arthur Wolf (1985) to the years 1700–1900 yields a Chinese population of almost 10 billion by 1900. Even using the very low life expectancy that Huang concocts (2002, 525; 2003), this birthrate yields a 1900 population of about 1.5 billion—still more than triple the actual figure (Lee, Campbell, and Wang 2002, 603). (Huang [2003] complains that Lee, Campbell, and Wang do not discuss "the reasoning behind their choice of mortality rates," but why should they? They can prove Huang wrong using either their preferred rate [which is itself probably higher than reality—see Lee, Campbell, and Wang 2002, 603] or the exceptionally high death rate that Huang prefers.) Huang asserts that it is "trivial" (2003) that his birthrate yields such implausible population totals but gives no reason for why we should think so; in fact it shows irrefutably that Huang's demographic argument is untenable.

Other Old and Misstated Issues

Huang also insists that Lee, Campbell, and Wang argue for a female infanticide rate of 25 percent (2002, 525–26; 2003). Lee and Campbell estimated such a rate for one sample from Daoyi village, Liaoning (1997, 69); the rate in another sample was "probably" under 10 percent (1997, 70 n. 24; Lee and Wang 1999, 51). Huang himself notes that "[t]he actual rate no doubt varied substantially over time and space, and might well have been substantially lower than what Lee suggests for Daoyi" (2002, 525). (Lee and Campbell [forthcoming] subsequently found that Daoyi had the lowest life expectancy of their eleven Liaoning samples and noted earlier that this community was "under far greater demographic pressure than most late imperial Chinese communities" [1997, 81].) Huang next takes the comment that "[w]e have never disputed that female infanticide may have been more common among the poor or during crises" (Lee, Campbell, and Wang 2002, 598) to be a concession that "female infanticide was the consequence mainly of poverty and subsistence pressures. . ." (2003); these are very different claims, and Lee and Wang emphasize high dowry costs, not subsistence pressures, as the causal factors (1999, 178 n. 23). Huang then adds that if female infanticide was largely due to poverty, "then its prevalence, whatever the exact extent, serves to support my theme about a 'mounting social crisis' in China" (2003). But, Huang has not shown that infanticide is the consequence mainly of poverty and claims that infanticide indicates a mounting social crisis without any evidence that it was increasing—there is actually strong evidence that it declined after about 1780 (see the very sharp decline for one sample in Lee and Wang 1999, 50, and more general comments on 52). And, why is the "exact prevalence" of the practice unimportant?

The sections on farm sizes, capital intensity, and the separation of farming and industry repeat previously discredited claims without any new arguments or data. Huang insists that England's agricultural labor productivity far exceeded Jiangnan's, but the only data he provides concern farm sizes. As I pointed out previously, these say nothing about productivity, which is a ratio of labor to output (2002, 542–44). Huang now claims that these figures were only intended as background for a discussion of labor productivity. But, he never actually provides such a discussion, and as we will see again below, all actual measurements contradict his claims.

The section on cloth consumption rests entirely on a misquotation. Huang writes, "Pomeranz now says that I [Huang] 'rightly note that [Pomeranz's suggestion] . . . would yield an implausibly large amount of clothing for people in the region' and that therefore, 'it is probably worth adjusting my [Pomeranz's] estimates for Jiangnan cloth production down a bit.'" The passage actually reads: "He [Huang] does, however, rightly note that *were all of this cotton turned into locally consumed cloth*, it would yield an implausibly large amount of clothing for people in the region. *To repeat, I made clear that this cloth was not all consumed locally. Nonetheless it seems to me that* it is probably worth adjusting my estimates for Jiangnan cloth production down a bit" (Pomeranz 2002, 567; text omitted by Huang italicized). Citations preceding this passage show that I never held the view that Huang attributed to me. The following two pages (567–69) show that my adjusted estimates are still more than double those that Huang takes from Xu Xinwu. The three pages (569–71) after that produce extensive argument and evidence (none of which Huang answers) showing that Xu's estimates have no basis in Qing-era sources (they are deduced by assuming that in "semi-feudal" China, consumption *had to be* at bare subsistence levels) and that my numbers are more consistent with other work that both Huang and I have cited approvingly. Thus, what Huang claims is an acceptance of his position is actually a refutation of it.

The section on coal also tries to create the false impression that Huang has exposed errors by others. Huang says, "Pomeranz now agrees that China was indeed rich in coal deposits," when I have always said that; my point is that the vast majority of that coal was far too many land-locked miles from Jiangnan to be economical at a time when transport costs largely determined fuel prices everywhere (2000, 63–64, esp. n. 143). Huang notes that I emphasize transport problems but continues, "it should be pointed out here that the Pingxiang coal mines produced by 1905 nearly 200,000 tons of coal and 54,000 tons of coke a year, still relying mainly on transport via the Xiang River." Huang's use of "mainly" elides what is clearly stated on the very page he cites (Hornibrook 2001, 222): that this coal came out of the mine on electric trains, after which a steam railroad took it to the riverbank. Since coal prices rose rapidly with overland carriage, especially in the mountains, these omissions are critical. Moreover, even if the modern technologies that transformed Pingxiang had magically been available in 1800, the mines' 1905 output would have supplied under 2 percent of a minimal estimate of Jiangnan's *subsistence* fuel needs and slightly more than 1 percent of England's 1815 coal production (when its mines still depended on water transport).[1] Such a small amount of coal could not have mattered.

Huang's concluding section, on theory and methodology, answers charges never

[1]On minimum per capita domestic fuel needs see Pomeranz 2000, 308–9; Smil 1932, 150; Asian Development Bank 1982, 114 (taking into consideration the figure I used for subtropical Lingnan, rather than the higher figure I used for North China). On British coal output, see Mitchell 1988, 247.

made, while charging me (and James Lee) with alleged faults of social science history generally; unsurprisingly, it has no citations at all. Huang says that he takes from Malthus only "the idea of reduced returns to labor under conditions of very high labor intensification," but he also insists (2002, 517–18; 1990, 87–88) that it was subsistence/population pressure that made people work more and that China lacked "preventive" population checks (2002, 527–29; 1990, 329). He continues to confuse "marginal" with "average" returns, as I have noted before (2002, 555–58, 561–62 n. 31). Moreover, diminishing returns—and Huang's claim that eighteenth-century England had transcended them (2002, 517, 534)—need to be demonstrated, not just asserted. Finally, Huang insists that "too much of social scientific history ignores local contexts and knowledge"—an accusation without content or relevance. Any method is sometimes poorly executed, but what are Huang's examples? Why are they relevant to Lee, Campbell, and Wang's work or to my work, which is methodologically quite different?

Huang's other sections are more substantive. But, they are unconvincing, too.

Handicrafts

Huang's new comments about handicrafts are puzzling. Since it is now established that his error with prices led to a ten-fold underestimate of earnings from a day's weaving—which could actually buy an adult's rice for twenty-plus days—he says that prices are irrelevant. But, he also says that "the heart of my case for involution . . . [is that] . . . cotton handicraft production represented a reduction from farming in returns to labor" and that spinning "paid just one-third to one-half of what farming paid." Both claims require relative values for cloth, yarn, and rice. Indeed, any productivity comparisons among people producing different goods require converting one good to another at the rates that prevailed at the time. And when we redo Huang's only such conversion (1990, 84–86) with the correct rice price, the earnings for textile work overall (combining poorly paying spinning and lucrative weaving with appropriate weights[2]) jump by over 500 percent (Pomeranz 2002, 560–61) and completely contradict his argument. These earnings could support a woman and a couple of dependents, as both my calculations and contemporary testimony show (Pomeranz 2000, 319; 2002, 558–59; see also Walker 1999, 55 for Tongzhou, which is right across the river from Jiangnan). They probably exceeded what women had previously earned growing grain[3] and compared well with long-term male laborers' wages (Pomeranz 2002, 547–50; 2000, 101–2, 319–20; Li 1998, 149)—although

[2]Note that Huang now only talks about the returns to spinning, while in his last essay he was vociferous about the need to remember that most households combined spinning and weaving (2002, 521; see also Pomeranz 2002, 559 n. 27).

[3]Estimates of female earnings per day in textiles (which, unlike those of a wage laborer, should reflect their *average* productivity) depend partly on how much of the less-productive tasks we assume they could have off-loaded to children or the elderly. In the most pessimistic scenario, in which women in the prime of their lives had to do everything, their average productivity would remain below what it was in grain farming (assuming it to be half of male productivity in the fields at any given date). If we assume they could off-load most spinning work, then the average productivity of these women and their helpers would rise above what it probably was in late–Ming era farming, and that of the women alone would be considerably higher, at least using 1750 prices. By 1800, or certainly by 1850, the rise in the relative price of rice would have pushed women's average productivity and earnings down considerably.

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Huang still compares female textile earnings to what *male peasants* earned growing rice, without mentioning that women (whose productivity in farming was well below men's [Li 1998, 148–50]) had never earned that much.

Thus, while average *unadjusted* earnings per labor day may have declined slightly as the ratio of female to male days worked rose between, say, the years 1600 and 1750, the situation was more complex than this one statement suggests. Earnings per male labor day were either stable or up slightly over that period.[4] Female earnings per labor day may have also risen as women (and children) left grain growing (since smaller farms no longer needed them) for textile work (see Li 1998, 133–55 on the increase in labor productivity as women moved into textiles—although my figures indicate smaller gains than his. Huang has made no specific response to either Li's argument or mine). Overall, then, earnings per labor day *adjusted for the composition of the labor force* may have risen or, at worst, declined very slightly. In other words, Jiangnan held off diminishing returns fairly well, despite supplying much more labor. As I showed before, this compares well with other early modern economies, including England's—where at least from 1750 to1800, output per labor hour probably declined (2002, 557, 562–64). We have also seen that adult textile earnings in Jiangnan could buy more food than the roughly contemporaneous wages of English weavers, urban or rural (Pomeranz 2002, 549–51).[5] So, why should the expanding textile economy indicate "involutionary" desperation in Jiangnan but a "revolutionary" escape from Malthusian pressures in England?

Agricultural Labor Productivity

Huang's discussion of farming does use some data, but to no avail. He begins with a superficially valid point: in showing that a rent-paying family growing 7.5 *mu* of grain still had enough grain to eat, I failed to note that grain was only about 60 percent of basic consumption expenses. But although I accepted Huang's average farm size of 7.5 *mu* for purposes of our initial discussion, this size was true (if at all) only in the delta's most crowded prefectures, where people mostly grew cotton or mulberries. The larger delta I discuss had 59,000,000 registered cultivated *mu* circa 1770, or 10.5 *mu* per five-member farm family.[6] This confirms Li Bozhong's (1998) estimate that mid–Qing era Jiangnan farms averaged 10 *mu* in size and largely closes the gap between food needs and total needs about which Huang worries.

[4]Rice yields rose without increased labor inputs, but there was also an increase in the double cropping of wheat, in which productivity per labor day was probably lower, and the two effects seem to have roughly canceled each other out. Allen's simulator model and Pomeranz forthcoming provide various scenarios ranging from slight declines to significant increases between 1600 and 1800. See also n. 9 below, which shows that if we use John Buck's Jiangnan wheat data rather than that for the rice/wheat region more generally, double cropping with wheat increased labor productivity, giving us a larger overall increase over time.

[5]The estimate could be raised or lowered by changing the mix of adult female and child labor in textile production and thus the extent to which adult women could concentrate on the more productive tasks, but the estimate made in Pomeranz 2002 is a fairly conservative one.

[6]This assumes, conservatively, that 10 percent of Jiangnan's population was off-farm. For population and acreage figures, see Perkins 1969, 230; Liang 1981, 401–13; see also Pomeranz 2000, 328–29. For urban population, G. William Skinner estimates 9.5 percent was off-farm in the Lower Yangzi, including peripheral regions with much less urbanization (1987, 75).

Huang also ignores the other income that most families had. Adult males in mid–Qing era Jiangnan could, and typically did, raise ten *mu* of rice by themselves. Women and some children earned income other ways, such as making cloth with purchased cotton, and through silk and other commodities. (A family that grew mostly cotton would probably not have enough labor to spin and weave it all; thus, grain-growing families often bought raw cotton and supplemented their income with textile work.) To ignore this additional income, Huang would have to deny that his most basic observation—the increased involvement of women and children in production—applied to the large majority[7] of delta families!

Huang's one useful point is that his figure of 10 labor days per *mu* of rice, which I accepted for argument's sake, is too low. He now prefers 15.25 days, which I am happy to adopt, although it seems slightly high.[8] But, the resulting reduction in Jiangnan labor productivity is comfortably offset by the fact that my earlier estimates were of *gross* labor productivity: in my calculations I did not deduct the capital costs from output for either England or Jiangnan. Since, as Huang keeps repeating, English farms used far more capital than Jiangnan's, this focus on gross productivity biased those comparisons heavily in England's favor.

Thanks to Robert Allen, we now have a simulation of net labor productivity on farms in England and Jiangnan circa 1800 (see description in Allen 2002, 3–10). Using a Norfolk four-course rotation, the English farm yielded 34,623 calories per labor day. For Jiangnan rice (including its threshing), Allen generously estimates 16.44 total days per *mu*. Using this estimate of Jiangnan labor inputs and assuming

[7]Growing enough cotton for Huang's estimate of delta cloth production would have required only 4,500,000 of the 59,000,000 *mu* in the "big delta"; as late as 1816, his more narrowly defined delta had a population (including urbanites, rice growers, etc.) of less than 40 percent of my 1770 figure for the larger delta (1990, 342).

[8]Francesca Bray assembles data on labor inputs in rice: except for tropical Java (which can support wet rice year-round), nowhere do these inputs exceed 17 male labor days per *mu* (1986, 149). Her figure for east China, 1921–25, is less than 10 per *mu*. Buck's data for various places in the lower Yangzi for the 1920s vary quite a bit with reference to place and type of rice, ranging from considerably less than 10 days per *mu* to a single figure of 15.3 (in Jiangning, which is in the lower Yangzi but not in the delta) (1930, 228). Jiangning was unusual because it used absolutely no animal labor for irrigation and so expended almost 40 percent of human labor time in irrigation (Buck 1930, 310). Wujin, in the delta, cut this proportion by more than half by using most of its animal labor for irrigation, yet total animal use was still only 0.4 days per *mu* for glutinous rice and 1.5 days for other rice (Buck 1930, 232, 310). Oxen were commonly rented by the day for these purposes in eighteenth-century Jiangnan (Li 1998, 77–79). Buck's data for the 1930s for four locations that can loosely be included in the delta range from 5.53 to 8.97 days (1937, 315). Shen Jingxian, an early-nineteenth-century Jiangnan resident, estimated total labor involved in irrigation at 2.5 days per *mu*, yielding a total labor per *mu* of 12.5 days (quoted in Li 1998, 202 n. 2). Huang's truly high figure, 20 days per *mu*, comes from wartime Rongxiang, Wuxi, which was unusual because there were absolutely no labor animals (MT 1941, 78), and which also suffered various war-related inefficiencies. Two-thirds of the population ages sixteen to fifty-five in the surveyed villages was female (MT 1941, 88–89), but the labor-input figures that Huang cites do not adjust for this. The surveyed villages had also reverted to more labor-intensive manuring when supplies of beancake fertilizer were interrupted (MT 1941, 71), and they were very poorly located in respect to water sources (MT 1941, 68, 71), requiring extra pumping time. Finally, the farms in these villages were tiny, even in terms of Jiangnan; they averaged 2.54 *mu,* which was so small that it "stunned" the Japanese researchers "even in comparison with the farms they had observed in fieldwork in other delta locations" (Bell 1999, 112). It would not be shocking if such true microplots were worked more intensively. Had that level of labor intensity been usual in the eighteenth century, it would have been impossible for one man to cultivate 10 *mu,* given the length of the growing season for rice (see Fei [1939] 1962,152–53; Li 1998, 71).

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that 70 percent of rice land also grew wheat means that net output per labor day comes out to 31,249 to 34,194 calories: 90.3 to 98.8 percent of the English level (depending on how one estimates fertilizer costs). Various small adjustments in the model to make Jiangnan productivity higher or lower are possible, but basic comparability remains between Jiangnan's productivity and England's. Using Huang's figure of 15.25 labor days per *mu*, or Li's 2.5 *shi* per *mu* rice yield instead of Huang's 2.25 *shi* per *mu* means that Jiangnan labor productivity would exceed England's. And although Huang says (again without data) that "[o]utput per unit of labor in animal products in England, I expect, will be substantially higher than in grain" (2003), I have already noted (2002, 543 n. 3) that this is questionable. (In Allen's simulation, pasture yielded fewer calories per labor day than crops but more money. Must we use prices here while rejecting them elsewhere?) Moreover, raising labor inputs for rice in this way, while correcting mistakes in Huang's labor intensities for cotton and double-cropped wheat, makes labor productivity for these three crops almost identical,[9] eliminating one of Huang's two main examples of supposed "involution." The difference between farming and textiles (his other example) also narrows, although the differences among male, female, and child labor discussed above are far more important in that case.

Finally, Huang fails to understand that my argument does not require exactly equal labor productivity in Jiangnan and England. What I need to show, and have shown, are six things:

- There were not *gross* disparities in either average labor productivity or earning power between mid-eighteenth-century Jiangnan and English farmers (Pomeranz 2002, 543–44). (Huang again confuses average productivity with wages, based on marginal productivity, claiming that I contradict myself by saying different things about these two variables; a footnote on the very page he cites [Pomeranz 2002, 549 n. 13] points out the difference.)
- These levels of output and income were enough above subsistence that they provide no evidence of an inexorable crisis growing in eighteenth-century Jiangnan, unless "crisis" is defined broadly enough also to encompass England at that time. Indeed, comparable farm earnings should mean higher rural incomes in Jiangnan, since considerably more Jiangnan farm families than those of England also earned money from handicrafts.[10]
- These earnings allowed peasants (especially owner-farmers, who earned more than the tenants in our examples) to buy some goods beyond those needed for biological survival.

[9]For both wheat and cotton, Huang has used Buck's figures for the vast and heterogeneous "Yangzi Rice/Wheat Region" (1937, 314, 316), which are considerably higher than those for the delta. For wheat, Huang uses 7 days per *mu*, while Buck's figures for his three sites within Jiangnan are 2.45, 3.21, and 10.44 (the last is for Wuxi, which was an outlier in many other respects, too—see n. 8); the average for Buck's seven sites in Jiangsu, including Wuxi, was 5.2 days per *mu* (1937, 315). As Li notes (1998, 217 n. 14), Jiang's ([1834] 1963, 10a) estimates for labor costs circa 1830 suggest 2.5 to 3 labor days per *mu* for wheat.

For cotton, Huang uses 21 days (1990, 84), while Buck's figure for Kunshan—his only one for cotton in the delta—is 16.33 days per *mu* (1937, 316–17). If we use the Kunshan cotton figure with the 15.25 days per *mu* that Huang now suggests for rice, then we wipe out the entire gross labor productivity difference between cotton farming and rice/wheat farming. See Pomeranz 2002, 546 for calculations using the figures Huang preferred at that time.

[10]On the very high rate of unemployment in post-enclosure rural England, see Allen 1992, 240–51, 261–62.

- There is thus no reason to assume that increasing labor inputs in eighteenth-century Jiangnan reflect an average family's increasingly desperate struggle to survive. Instead, increasing labor inputs probably sustained a slow but significant increase in consumption (and the purchased portion thereof) similar to that of early modern Europe.
- Given Jiangnan's huge edge in net land productivity (probably more than 5:1, in terms of net output per acre in Jiangnan over net output per acre in England) and similar labor productivity, Jiangnan agriculture compares quite well overall to England's.
- Since areas of Western Europe where agricultural labor productivity circa 1800 was 50 percent or less of England's (Allen 2000, 20) industrialized successfully in the next century, Jiangnan's much higher agricultural labor productivity could not have barred industrialization. These claims would be sustained even if Jiangnan's agricultural labor productivity in 1800 were 75 percent of England's rather than, as it appears, over 90 percent.

Consumption, Class, and Laborers' Budgets

We thus reach Huang's section on consumption. Huang does not dispute that the food consumption of Jiangnan's rural laborers was comparable to England's (Pomeranz 2002, 545–46), nor does he dispute that the percentage of their income devoted to basic calories was similar. (He does claim [2003] that I calculated proportions of food intake "by pound weight alone," rather than by calories, but this is another misreading: the very next sentence has calorie figures. For other contradictions between Huang's claim and the passages upon which he claims they are based, see Pomeranz 2002, 559 n. 27.) He also could not seriously dispute these points, as Jiangnan agricultural wages bought slightly more grain than those of England (see Pomeranz 2002, 565 n. 36).[11] Instead, Huang claims that the consumption patterns of agricultural laborers were typical for Jiangnan but highly abnormal for England.

Huang's argument about Jiangnan consumption relies on a curious non sequitir: since mid–Qing era agricultural laborers had attained formal legal equality and sometimes ate with their employers, "[s]pecifics about agricultural workers' food consumption, therefore, are fully applicable to the general peasant population" (2003). He says this without evidence: the treatises that Fang Xing cites did also discuss other social groups, but the sections that Fang used are unambiguously about provisions for hired laborers (1996, 92–94; see also Qian [1640] 1967 19a–20b; Jiang [1834] 1963, 11b). Huang's assertion also misses the point: I said that other population groups could probably spend more on things *other than grain* than did farm laborers, Jiangnan's poorest nonbeggars (2000, 137 n. 110; 2002, 565). Lacking data on wealthier groups' diets, I did not discuss them. Moreover, if Huang were right that almost everyone in Jiangnan ate the same diet, then it would undermine his position

[11]For seventeenth-century agricultural day wages, see Allen 2001, 9. Jiang implies a daily wage of 0.05 *shi* of rice (13,923 calories) in the early nineteenth century ([1834] 1963, 11b), which would be almost 40 percent higher than these earlier figures, but the text has some ambiguities based on approximations in calculations. Li (1998, 149 [seventeenth and nineteenth centuries]) and Pomeranz (2000, 101–2, 319–20 [eighteenth century]) deal with annual wages, which are always less than 365 times day wages.

further. Since humans generally want only so much starch, people who have met basic caloric needs generally devote their additional income to other things; but until that point, they will generally put grain first. Peasants, even tenants, certainly earned more than agricultural proletarians: the net income per labor day for our simulated Jiangnan tenant, for instance, was roughly triple the agricultural day wage (see n. 11), and this higher net income per day went with better assurances of steady work. Moreover, tenants, unlike laborers, usually could afford to marry—as Huang himself notes—and we have seen that wives could earn more than their own subsistence, widening the disposable income gap further between tenants and laborers. Landowning peasants, paying perhaps 15 percent in taxes instead of 40–50 percent in rent (Bernhardt 1992, 43–46), had higher incomes still, and a small group owning more land than they tilled earned even more. If these much better-off groups ate the same diet as agricultural laborers, then this would suggest that laborers' diets were so satisfying that those who could afford to eat better saw no need to do so. That would be a far more radically "optimistic" position than the modest point I made about Fang's article: that the percentage of the whole society's income spent on grain must have been lower than that for landless laborers.

On the English side, one can agree with Huang's proposition that "those who could afford it" ate more meat than rural laborers, since, as I showed (2002, 566), those laborers averaged less than an ounce of meat per day. But, how many English people ate better than rural laborers, and by how much? Huang provides no specifics, and the pages he cites are not about this subject (see also Pomeranz 2002, 566). Huang also conflates two issues—undoubted class differences and dubious urban/rural differences. Although Huang claims that only "village laborers" ate a protein-poor diet, Oddy says specifically that there is no evidence for a difference in eighteenth-century urban and rural diets (1990, 270); Gregory Clark, Michael Huberman, and Peter H. Lindert show that in 1837–41 (when clearly comparable data begin) and in 1863, "rural workers consumed more calories and protein from grains, milk and cheese, and apparently more vitamins from green vegetables than London workers" (1995, 228). Thus, urban workers ate no better than farm laborers and probably worse. Since "laborers in husbandry" made up 15.5 percent of the population in 1801–3 and common laborers made up most of the "industry and building" group that included 24.7 percent of households, we are already nearing 40 percent of the population. If we add the miserable 20.1 percent who earned two-thirds *less* than farm laborers (Lindert and Williamson 1982, 400–401), close to 60 percent of the population probably ate no better than Oddy's sample. Another 11.1 percent were soldiers or sailors; the 1811 navy diet is better than Oddy's samples but not by much (Drummond and Wilbraham 1958, 465, 467).

All-England averages for protein consumption are consequently just slightly higher than the figures Huang dismisses as atypical. For milk, Holderness's widely accepted estimate equals 0.9 pints per person per *week* for all of England in 1800, which is fairly close to Oddy's figure of 0.5 for the rural poor (Clark, Huberman, and Lindert 1995, 231). For cheese, Holderness's standard figure is just over half an ounce per day (1989, 165–70). Only for meat, at 4.3 ounces per day in 1800 (Holderness 1989, 155–60), is the all-England figure much better than that for laborers; it is still easily accounted for by a fairly small group that ate large amounts of meat (e.g., if 20 percent of the population averaged one pound each and the 11 percent of soldiers and sailors averaged seven ounces, then this would raise the English average to its observed level, even with the remaining 70 percent of the population still eating as little meat as did landless laborers) (see also Clark, Huberman, and Lindert 1995, 230). This hardly justifies Huang's claims of a greatly superior English diet, especially since Jiangnan also had a wealthy minority that ate very well, and additional protein sources (e.g., dofu/tofu) not available to the English.[12] Finally, in terms of overall calories, China's 2,651 calories from grain alone per adult male equivalent (Pomeranz 2000, 39—with Jiangnan probably higher) compares favorably with England's 2,700 calories from all foods circa 1790 (Fogel 1992, 268–69).

Having disposed of Huang's claim that the diet of England's rural laborers was extremely atypical, we can address my actual point: that the percentage of income that rural proletarians spent on grain was similar in England and Jiangnan and probably more typical in England than in Jiangnan. Our best indicator here is how much of the population had incomes close to those of agricultural laborers. Peter Lindert and Jeffrey Williamson estimate an average annual income for English households headed by "laboring people in husbandry" at £31; categories with incomes of £40 and under total 48 percent of all households, including almost 20 percent who were paupers and vagrants, averaging a miserable £10 per year (1982, 400–401). Thus, roughly 50 percent of households earned 1.3 times the income of agricultural laborers' households or less: income for this bottom 50 percent averaged £22, or 30 percent *less* than that of laborers in husbandry. Almost 75 percent of households earned 1.8 times the level of farm laborers (£55) or less; this huge group averaged £30 per year, just *below* the average for agricultural laborers.

The large majority of Jiangnan residents were tenants and/or owner-farmers, who probably earned more than 1.8 times the income of landless laborers. As we have already seen, even tenants' income per day worked was roughly triple that of farm laborers. With assumptions strongly biased toward the laborer, we can reduce the difference between tenant and laborer household income to 2.2:1, but that still means that Jiangnan tenants (and, a fortiori, owners), who together made up the bulk of Jiangnan's population, appear to have had incomes that exceeded the incomes of landless laborers by a wider margin than that by which the incomes of any but the top 25 percent of English households exceeded the incomes of their landless laborers.

[12]Buck's modern data suggest rural consumption of less than one ounce per person per day of pork, but this figure might have been a good deal higher in the mid-Qing period. For one thing, pigs were only profitable if one used their manure, and demand for that may have decreased. There was no apparent change in the amount of manure applied per acre in Jiangnan after the Ming period (probably because beancake use grew), despite a large increase in population per acre (see Perkins 1969, 73). Thus, the number of pigs per person probably declined. It is also likely that the growth of Shanghai and hard times in the 1930s meant that more pork was sold to the cities than had been 150 years before. At the other end of the range, Li (2002) has made an ingenious but speculative argument (based on the amount of pigpen manure used as fertilizer and on data from Chen 1983 and Qian [ca. 1640] 1967), which suggests that the average farm family of five kept one sow and raised a litter of six shoats for six months before slaughtering them. By my calculations, such a model farm would have produced 5.5 ounces of pork per person per day. This seems high (since pigs were generally not very profitable and Jiang [{1834} 1963, 7a–8b] estimated pig manure needs without figuring in human wastes)—and it does not tell us how much of this pork the farmer sold. Reconciling Buck's figure of 0.82 hogs per farm in the Yangzi rice/wheat region ([1937] 1964, 246) with his low estimate figure for animal products consumed requires the assumption that sows were bred very infrequently and were allowed to live relatively long lives; if they were instead bred annually, then his estimates of pork production would approach Li's, and we are unlikely to find much data on how the frequency of breeding changed over time. Thus, the case for significantly higher pork consumption in the eighteenth century than in the twentieth must remain speculative, but it is plausible enough that, even without looking at dofu/tofu, fish, eggs, and poultry, we cannot assume a significant protein gap in England's favor.

In other words, the incomes and thus consumption of landless laborers in England were probably more typical of that society than were the incomes and consumption of landless laborers in Jiangnan. This is the exact opposite of what Huang has asserted in his 2003 response.[13]

Thus, investigating Huang's question empirically reinforces my position. Laborers' diets and the percentage of income they spent on grain were similar in Jiangnan and England. Most people (tenants and smallholders) in Jiangnan probably ate somewhat better than proletarians, and could certainly buy more things beyond grain. England's distribution of income was much more unequal (the bottom 48 percent of households earned 11.5 percent of total nominal national income in 1801–3, and the bottom 73 percent totaled 24 percent [Lindert and Williamson 1982, 401–2]), and urban diets were no better than rural diets. Urban workers probably did spend more of their income on nonfood, but this often represented high urban prices (e.g., housing) without better quality.

If perhaps 15 percent of Jiangnan residents were beggars and vagrants and another 15 percent landless laborers—both probably high figures[14]—we might have a bottom 30 percent that was equally miserable in both societies. But, England had a further 20 percent of the population whose incomes averaged only 1.3 times those of farm laborers and still another 25 percent of the population whose incomes averaged less than 1.8 times that of farm laborers. In Jiangnan, however, one would find right above the laborers two huge groups—tenants and owner-farmers—whose incomes, as we already saw, were considerably higher than this (2.2 times that of laborers even for the tenants using a very conservative estimate). Thus, the next 45 percent of society probably lived better in Jiangnan. Above that level, English incomes rise rapidly, so that most of the top quarter there probably lived better than in Jiangnan: perhaps even by enough to make total English income and consumption per capita exceed Jiangnan's. Recent research, indicating that England's income distribution was even more unequal than the nominal income figures used above, suggests this (Hoffman et al. 2002, 341–43), but the issue is not settled, certainly not lopsidedly. Against this extensive if imperfect data, Huang offers only bald assertion.

[13]This calculation requires, first, measuring income by household unit, not individual, so as to produce figures comparable to Lindert and Williamson's figures for England. I then assume that the laborer found year-round work at a day wage that would have bought 10,000 calories of food per day: a generous assumption, given what we know of both day and year wages (see n. 11). For the tenant, I assume that he worked only 190 days per year on his ten-*mu* farm (growing rice and wheat), while he, his wife, and perhaps a child or parent together spent another 310 labor days turning purchased cotton into cloth. (Note that this gives the adults in the tenant family much more leisure time than the laborer.) This yields an average net income of 21,900 calories per day, 365 days a year. One could object that since the tenant family has more mouths to feed, it will actually have less disposable income than the farm laborer, but to the extent that very poor people had fewer dependents, the same problem would affect Lindert and Williamson's data. Moreover, Fang, the typicality of whose figures on laborers' consumption patterns is the point at issue here, posited a laborer with a family of five; if we instead assumed such people were single, the percentage of their income spent on basic grains would, of course, fall sharply, leaving them much better off than their English peers.

[14]Huang has emphasized the relative rarity of wage labor in Qing-era Jiangnan (1990, 63–66). And if, as is generally assumed, very few landless laborers or people below them could marry, then we would have a pool of 30 percent of men too poor to have families—considerably above the admittedly rough guesses of 10–20 percent that one usually hears.

Conclusion

No sensible person should expect to have the last word about the causes of the great divergence. There is, however, a gathering consensus, *pace* Huang, about the timing of that divergence, which invalidates various old explanations of it, including his. Many issues remain wide open and point to numerous areas for Sinological research: the history of science and technology, consumption, and sectors beyond farming and textiles, among others. This also suggests that with our understandings of both the eighteenth and twentieth centuries much revised, we should revisit the nineteenth century: undoubtedly a century of crisis, but not for the simple economic/demographic reasons Huang imagines. A better picture will involve both dynamism and decay; politics and culture, as well as economics; very different processes and outcomes in different regions; and probably some bad luck and conjunctural effects. Progress requires measuring where that is appropriate, rather than invoking old stereotypes, and it requires reading, citing, and responding to other scholars accurately.

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Dingxin Zhao responds to Elizabeth J. Perry's review of *The Power of Tiananmen: State-Society Relations and the 1989 Beijing Student Movement* (*JAS* 61.1:237–38):

Elizabeth Perry's review of my book contains many criticisms with which I cannot agree, and I feel that obliged to make some clarifications in the following.

Perry states in her review that the book "does not provide much new empirical evidence" (p. 237). It is interesting that, when I was revising the book, two readers expressed concerns that I had brought in too much new empirical evidence contradicting existing knowledge and wanted to know why my version of the stories was closer to the truth. Thus challenged, I added an example in the introduction (pp. 33–35) to explain the detective style of my evidence collection. The example shows how, after a painstaking reconstruction, I came to the conclusion that the so-called Xinhua Gate Bloody Incident, a crucial event in the early stage of student mobilization, was almost certainly an unfounded rumor. The new evidence greatly enhanced my understanding of the 1989 student movement, since rumor-driven and government-brutality-driven mobilization involved different mechanisms. If I indeed achieved what Perry has called "a fresh and insightful interpretation" (p. 237) of the 1989 movement, then I owe much of it to such new materials.

In the campus ecology chapter, I provide a detailed analysis of how the Beijing campuses at each of the five levels—dormitory (shared by six to eight students), dormitory building (housing several hundred students), student-living quarters (accommodating up to ten thousand students), the whole campus, and the university district—facilitated student mobilization during the 1989 movement. Perry claims that the analysis is tautological because "spatial patterns are explained by spatial patterns" (p. 237). Perry's reasoning contains several flaws, but most straightforwardly, my analysis is not circular because, although the campus layout shaped student mobilization during the 1989 movement, the student mobilization did not in any way change the campus layout in Beijing. In the same paragraph, Perry also raises the criticism that the ecological argument cannot explain the student protests in cities such as Shanghai where campuses are dispersed. To this, my answer is that the fact that universities in Shanghai are more dispersed at most only removes the ecological impact at the university-district level. Shanghai universities, and indeed almost all Chinese universities, share similarities at the other four aforementioned levels. These factors facilitated Shanghai student mobilization when other conditions became ripe.

Perry contests my criticism of the "elite factionalism" explanation of the development of the 1989 movement (chap. 7). She disagrees with an argument that such a factional explanation implies state autonomy. She also complains that I have offered "little information on the internal operations of Chinese leadership" (p. 237) to support my argument. To answer, an argument that the development of the 1989 movement was determined by the conflict among top state elites must mean that the state elites had great autonomy in decision making and in shaping the direction of the movement. To the second criticism, I would reply that, since my argument does not rely so much on knowledge of the internal operations of the Chinese leadership, it is the factionalism school's burden, not mine, to provide such information. Let me stress that the book never denies the existence of power struggle within the Chinese government. I have pointed out some crucial developments of the movement that cannot be explained by the factionalism model and have also developed a more powerful explanation. To such a competitive argument, as I have suggested on p. 32, a valid criticism should start by showing that my argument actually explains less empirical evidence than the factionalism model.

Chapter 9 of the book tries to address one question: Why did traditional rhetoric and activities dominate the 1989 movement? Many scholars explain the traditionalism by the impact of traditional Chinese culture. My approach is to compare the 1989 movement with the May Fourth Movement in 1919 and the December Ninth Movement in 1935–36. Considering the scale of social changes that occurred in twentieth-century China, it is safe to hypothesize that, if tradition had been a major determinant, the two earlier movements should have appeared more traditional. The finding, however, shows the reverse as true. This rather unexpected discovery compelled me to explore state repression capacity and the strength of intermediate organizations uncontrolled by the state for explanations. Perry, however, argues that my comparison failed to differentiate types of authoritarian states and varieties of international influences and thus missed some theoretical opportunities (but she does not spell out what the opportunities are). I have two points to make. First, the elements that Perry raises are all present in my analysis. They are not treated at great length because doing so would turn the chapter into a book without changing the substance of my argument. Second, a valid criticism should point out how my alleged omissions lead to a flawed argument. Perry, however, never attacks the chapter's argument, and I have to assume she actually accepts it. I stress that the chapter never denies the importance of culture in shaping human activities. Nevertheless, cultural repertoire is always larger than the activity pattern of any single movement. We need to explain why a movement manifests some specific elements of a culture.

In the book, I argue that countries with stronger intermediate organizations are much less likely to experience large-scale social movements and revolutions. Perry counters by asking how this squares with the fact that large-scale movements such as the civil rights movement and the antiwar movement happened in the United States (a country with strong intermediate organizations). Anticipating criticisms of this kind, I have provided a guideline of falsification to this kind of comparative argument on p. 32, but a simple answer is that the scales of these "large-scale movements" in the United States pale in comparison with the Chinese cases. One cannot deny that, during the twentieth century, the United States never experienced revolutions and rebellions of nearly the scales of the Boxer Rebellion, the Republican revolution, the Communist movement, the Cultural Revolution, and, most recently, the 1989 movement.

Perry has also made other complaints on the matters of definition and measurement, which seem to me to be on the unreasonable side. For example, few readers of the book will mistake the concept of intermediate organizations, namely the intermediate organizations uncontrolled by the state, for China's local governing and production unit called *danwei.* Also, after reading my analysis between chapters 2 and 5 (including various interview results), few would think that I have provided scant empirical evidence for my argument that the basis of state legitimacy in China has shifted in the post-Mao era from ideology to economic and moral performance. In fact, when it comes to p. 218 (Perry starts her criticism by quoting a sentence on that page) I have to assume that the readers have read the earlier chapters. Still I provide a brief discussion on p. 218 (including n. 22–23) to justify my position once again. Careful readers may find that the two examples that I have selected from numerous available materials have offered strong ethnographic evidence to show the importance of Chinese officials' moral performance in the people's minds.

This is enough for the readers to read the book and make up their own minds.

Elizabeth J. Perry responds to Dingxin Zhao:

Having concluded my review by saying that *The Power of Tiananmen* "is an extremely thoughtful and provocative book that deserves to be widely read and debated" (p. 238), I suppose it is only fitting that the debate should thus proceed. Professor Zhao Dingxin and I may well disagree about what qualifies as "much new empirical evidence," but it should be pointed out that my complaint about the paucity of fresh data in his book referred specifically to "new empirical evidence *about the events of 1989*" (p. 237, emphasis added). I particularly complimented his rich description of the social conditions and campus culture of the 1980s that helped set the stage for the 1989 uprising.

We do, however, part company when it comes to analytical issues. First is the matter of his "ecological explanation." It is obviously true, as Zhao notes in his response, that "campus layout shaped student mobilization during the 1989 movement." As he elaborates in his book: "[A]t each university big-character posters and announcements were concentrated, and mobilization was initiated, only in specific places" (p. 252); "[i]n my study, I found that marching along the avenues between dormitories before demonstrations on the street was a standard way for Beijing students to achieve a high level of mobilization; this is another example of an ecology-dependent strategy" (p. 253). Zhao's "ecological analysis" is circular in making the obvious point that spatial configurations (in the form of campus layout) shaped spatial configurations (of poster placement and marching routes). Naturally, the situation of buildings and streets around the universities dictated the location of student protest actions. This truism, however, is clearly not an *explanation* of the events of 1989 or even of Chinese student mobilization more generally, inasmuch as campus layout remained basically constant throughout the history of the PRC (and in some places well before), whereas student mobilization was an infrequent and protean phenomenon—with 1989 being unique in both scale and endurance.

Second is his critique of the "elite factionalism" approach. Zhao claims in his response that "an argument that the development of the 1989 movement was determined by conflict among top state elites has to mean that the state elites had a great autonomy in decision making and in shaping the direction of the movement." I can think of no one who has suggested that the "development" of the movement was "determined" by elite conflict. In any case, elite conflict (such as that between Li Peng and Zhao Ziyang) may signal a political opportunity to the masses, who quickly steer the movement in directions unanticipated and undesired by either elite faction. Moreover, elite factions usually have supporters within society, as Zhao Ziyang's tearful visit to the protesters at Tiananmen Square so poignantly indicated. Although I do not disagree with Zhao's position that state-society relations provide a more comprehensive framework for understanding the events of 1989 than a simple focus on elite machinations, in my view his criticisms do an injustice to the scholarship on Chinese factional politics.

Third is his critique of the "cultural approach." I am sorry to disabuse Zhao of his assumption that I accept the argument of his chapter on this subject simply because I did not explicitly attack it in my review. Zhao claims that he has hit upon "an unexpected discovery" in finding that "the rhetoric and activities of the 1989 Movement were actually truer to traditional Chinese culture than those of earlier student movements" (p. 268). The chapter fails to explain what the author means by "traditional Chinese culture," but if we assume that he is referring to Confucian values, then many of the "culturalists" whom he criticizes have in fact already highlighted the greater "traditionalism" of the 1989 movement as compared with earlier movements such as those of May Fourth, May Thirtieth, or December Ninth. Zhao treats Chinese culture as if it had somehow evolved unilinearly from "traditional" to "modern" during the past century. Hence, his peculiar assertion that "if we believe that the patterns of a social movement are determined by culture, the 1989 Movement should appear less, not more, traditional than those two earlier movements [May Fourth and December Ninth], since Chinese culture has only become more modern over the course of the twentieth century" (p. 268). Why should we expect that May Fourth (1919) or December Ninth (1935–36) would appear more "traditional" than 1989 simply because they occurred earlier in time? This assumption flies in the face of recent approaches to Chinese culture (influenced by general trends in cultural studies) which have eschewed teleological assumptions in favor of investigating what, of a multitude of available possibilities, is being drawn upon at any particular time. As a rich body of scholarly literature on the Republican era (1912–49) has argued, the explosive combination of state fragmentation and social fermentation in this period gave rise to a political culture replete with new implications for popular protest. Under Mao, by contrast, many of these developments were reversed. Zhao's empirical findings on protest movements in the Republican period (based almost entirely on secondary scholarship) are not original, nor do they convincingly refute a culturalist perspective.

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In dealing with both the "factionalism" and "culturalist" schools, Zhao levels blanket criticisms that ignore major distinctions within these approaches. The differences in substance and style among, for example, Lowell Dittmer, Roderick MacFarquhar, and Andrew Nathan—all of whom have made significant contributions to the literature on elite factions—are at least as great as the similarities. By the same token, scholars who have highlighted the importance of Chinese culture—ranging from Lucian Pye to Joseph Esherick and Jeffrey Wasserstrom—have done so in very different (and sometimes diametrically opposed) ways.

Finally, although I am certainly sympathetic toward an approach that spotlights the role of social organizations in generating protest movements, I remain unpersuaded by Zhao's claims about the relationship between the "strength" (never defined) of intermediate organizations and the likelihood of "large-scale" (never defined) social movements. Although he states in his response that he has "provided a guideline of falsification to this kind of comparative argument on p. 32," I searched in vain on that page—and others—for such a guide. Zhao may find my calls for greater clarity of definitions and measures "on the unreasonable side," but surely he must admit that it is impossible to assess his propositions without some indicators of the concepts he employs. For a book that is bold in its criticism of alternative analytical approaches and parsimonious in its acknowledgment of the contributions of previous scholarship, one might be forgiven for expecting a little more methodological rigor.

Brian Keith Axel responds to N. Gerald Barrier's review of *The Nation's Tortured Body: Violence, Representation, and the Foundation of a Sikh "Diaspora"* (*JAS* 61.2:741–43):

Professor N. Gerald Barrier's thoughtful review of my book, *The Nation's Tortured Body,* raises several important questions pertaining both to studies of Sikhs and to the analysis of diasporas. The issues involved, however, are more generally significant because they concern methods of knowledge production on the one hand and understandings of gender and subject formation on the other hand. I welcome the opportunity to respond to Barrier's concerns with the intent of opening up a productive dialogue that may help make our studies of diasporas richer and more compelling.

Barrier's critique revolves around a single point of a radical disjuncture between my object of study (i.e., the reality of Sikh diasporic life) and my text's depiction of that object. This point is elaborated through discussions of several important issues. Due to space restrictions, however, I will limit my response to addressing only one: the significance of the figure of the *amritdhari,* a Sikh who has undergone initiation into the religious order of the Khalsa, within histories of Sikh subjectification. The *amritdhari* also presents an image with a "visible identity" (W. H. McLeod, *Textual Sources For the Study of Sikhism* [Manchester: Manchester University Press, 1984, p. 73]), the body of which is defined and accentuated by certain symbols (e.g., the "5 Ks," or *panj kakke,* and the turban). According to Barrier, my analyses of this and other issues—which may seem somewhat arcane for nonspecialists—fail because they do not stem from a "thorough review of facts" (p. 742). Additionally, my neglect for "fact-based studies" are said to lead to portrayals that are "overdrawn and inaccurate" (p. 742).

At this level of critique, Barrier's commentary relates to a more general debate within the history of the social sciences and humanities concerning the discernment of reliable or trustworthy data and the development of accurate techniques of knowledge production. In other words, what is at stake is the status of an epistemology that posits a definitive movement from the reality of experience (or its archival container) to a hoped-for transparency of representation. The real, in this view, is understood to exist prior to and outside of representation. Barrier's critique of my work may be seen as demanding an adherence to this theory of knowledge. Taken in these terms, *The Nation's Tortured Body* cannot help but appear guilty of disrespecting the real of present or past Sikh life. My text does not aim, however, tó respect the real in this normative sense but, rather, to draw the very allure of actuality and the realism of experience *into question.* Thus, I do not ask how fact constitutes the ground for representation. Rather, I ask: How does representation produce reality?

It is here, however, that Barrier's review exhibits a fundamental misunderstanding of the book's stated intention. The text allocates a significant amount of space to analyses of *visual images* of violently dismembered and gendered bodies of Sikh corpses in relation to *representations* of the *amritdhari.* Barrier's critique views this emphasis as misplaced and offers a correction: "What Axel is really talking about [i.e., instead of the *amritdhari*] is the more prominent *keshdhari* Sikhs (approximately 80 percent of Sikhs, but who, then and now, maintain most of the five Ks or symbols most of the time, but are not initiated" (pp. 741–42). Barrier's enumeration notwithstanding, the text indeed does state quite clearly that I mean to talk about the figure of the *amritdhari.* This is not, however, because I believe the majority of Sikhs really are members of the Khalsa. Rather, historical analysis demonstrates that the *amritdhari,* as a specifically gendered representation, has had a definitive role—changing through time—in producing the realities of diverse Sikh subjects.

This is but one of several interrelated issues with which the book is concerned. It is significant for, until recently, the history of this gendering has been overlooked. Let me briefly specify gender's broader constellation of significance. Since the nineteenth century, the discourses of Sikhs, colonial regimes, and nation-states have valorized several different kinds of Sikh subjects in relation to specific corporeal formations. The masculinized figure of the *amritdhari* has become a normative or iterable model that tends to organize these disparate productions of subject and body. My basic argument is that many practices of subject formation have repeatedly cited this model and, in so doing, have established it as an authoritative reference. This citational process is often most effective, precisely, as a practice of negation (e.g., "not all Sikhs are *amritdhari,*" "*keshdharis* are not *amritdhari*"). Just as importantly, however, during the 1980s and 1990s, Indian police and military institutionalized this citationality in a variety of ways. For example, official military periodicals warned: "Amritdharis are dangerous people and pledged to commit murder." In addition, within the torture scenario, Indian police deployed the symbolics of the *amritdhari* against Sikh victims. More generally, visual images and speech acts concerned with these symbolics have situated points of mediation between various populations of Sikhs and non-Sikhs around the world. This mediation occurs on many levels: through debates about "who is a Sikh," through the disciplining of bodily techniques, and by means of the commodification of images circulating in print media and on internet websites. These processes are crucial for understanding the formation of other types of diasporas as well. They provoke us to rethink our received analytical models. For instance, rather than define diaspora as a group of individuals displaced from a homeland, we might begin to understand diaspora as a globally mobile category of identification.

Within contexts of diasporic subject formation, these diverse processes often operate beyond the control, or despite the intention, of individuals, and they may be defined by the sometimes bewildering conjunction of processes that appear unconnected. I provide an example in a chapter called "The Homeland," which investigates Sikh studies as a globalized form of knowledge production that generates a particular kind of Sikh subject. Barrier questions the soundness of one of this chapter's main points of critique. This concerns the cover of a book called *The Transmission of Sikh Heritage in the Diaspora* (1996), which displays, and thus reiterates the normative model of, the gendered image of the *amritdhari*. Barrier fails to disclose that he himself is one of the editors of this book (and, for that matter, that I am a contributor). What is perhaps more important is that his own rebuttal demonstrates precisely the effects of the processes that I attempt to delineate: "Axel makes much of the choice [of the book's cover] in terms of symbolism. . . . Had he bothered to check details, Axel would have found that the jacket did not reflect a significant intellectual choice, but rather what often happens with Indian publishers" (p. 743). The cover was chosen "to increase sales and Sikh interest in the contents." In other words, the subjectifications of Sikh studies—like other sites of subjectification—cannot be reduced to individual intention. Rather, the formation of subjects is mediated in this case by the global circulation and commodification of gendered representations, and it is oftentimes compelled by the desires of capital pitted against the contrasting desires of the intellectual.

Barrier concludes his review by recommending that "few Sikh specialists or Indian historians will read the book thoroughly" (p. 743). This prognostication is most unsettling, but not because I presume any readership. Rather, I would hope that, whether within this text or another, a site of mediation might emerge between the differing projects of critical theory and Sikh studies. Our present dispute, then, would seem to be a starting point for that discourse: inaugurating an interchange that would bear upon the vagaries of disparate approaches and help us understand how to better portray subjectification or criticize repressive orders.