Beyond the East-West Binary: Resituating Development Paths in the Eighteenth-Century World

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DEBATE CAN ADVANCE SCHOLARLY discussion, and I am grateful to *JAS* for the chance to do so here. As much as possible, I would like to move forward by introducing additional arguments and evidence. However, some recapitulation of the book under discussion is inevitable, as is some review of debates related to Philip Huang's book on a related topic. Some return to previously plowed ground is further necessitated by the nature of his review. First, he has fundamentally misunderstood what my book claims, as well as the support for some of those claims. I will not correct all of these errors here, but I will need to go over some of the major examples. Second, a central contention of his review is that his 1990 book, *The Peasant Family and Rural Development in the Yangzi Delta, 1350–1988* remains the best framework for understanding the delta's economy over that entire period. Huang is, of course, entitled to that view: but in reasserting that book's thesis he ignores rather than responds to the critiques of that book (see Wong 1990; Myers 1991; Wong 1992). He also ignores plentiful new research on both Asia and Europe that suggests there were more paths to modernity than we once realized, most of them perfectly viable in spite of relying on more labor-intensive kinds of production than England's (especially in agriculture) during their early phases. And in reasserting his earlier positions—despite the fact that his book contains almost no data from the eighteenth century, which is the focus of my book—he implies that the same basic, single-variable story (with increasing man:land ratios as the unmoved mover) can be stretched across huge spans of time: periods of rapid population growth and no population growth, of political stability and instability, of ecological stability and crisis, of more and less technological change. I argue instead that we need to look at many factors to understand any region's changing fortunes and to develop at least rough methods for

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assessing which were the most important factors in a given period: there is no reason to assume, in China or elsewhere, that either a constraint or a capacity that was particularly important (or unimportant) at one historical conjuncture will always remain so, thereby locking societies into unchangeable long-run trajectories.

It also turns out that in making what is probably the most important calculation in his 1990 book—that of how much food one could earn with a day's weaving—Huang misplaced a decimal point in the price of rice and so is off by roughly a factor of ten (see below): a day's weaving earned over twenty days' worth of food, not two days' worth. In making this calculation, he also used a figure for how much grain it took to feed a person for a day that is 50 percent higher than the one he uses in his current paper. Were we to insist that he stick to this lower figure, a day's weaving would be worth at least fifteen times as many days' food as he says, and perhaps as much as twenty-five times. But since—unlike the decimal point error—the discrepancy between Huang's two figures for daily consumption could plausibly be resolved in either direction, we will discuss it only in passing. Fixing the unambiguous tenfold error in estimating returns to weaving is certainly enough for current purposes. Once we correct for that error, the economics of textile production look totally different from Huang's picture and much more like mine, even though cotton spinning was indeed poorly paid. I will work through the implications of this particular problem in more detail in the section entitled "Involution?" to show that no empirical or theoretical basis remains for Huang's use of that concept.

Huang also repeatedly measures labor intensity and other crucial variables in inappropriate or mistaken ways. For instance, he repeatedly confuses marginal and average productivity in agriculture, which also sometimes differ by as much as a factor of ten. Furthermore, Huang mischaracterizes his own sources, and relies on estimates by others (most notably Xu Xinwu) that were created by assuming exactly what Huang wants to prove. It is important not just to point out these and other errors but to show the extent to which correcting them would change Huang's results and affirm mine; generating those estimates requires some hard slogging. I hope the reader will bear with me through that process. The results will not only show Huang's criticisms to be groundless but also provide new measurements of important features of the Jiangnan economy, thus extending our understanding of the mid-Qing beyond what I did in *The Great Divergence*. But for the reader who does not wish to work through the numbers with me, I have taken the liberty of beginning those sections that involve significant amounts of calculations—but only those sections—with a series of bullet-points summarizing their main conclusions.

General Issues and Comparative Strategy

Huang writes as if I insisted on finding exact economic equality and extremely close institutional similarity between Jiangnan and England, which I do not do. The point, instead, is that because at a rather late date there were still many important similarities, as well as some differences that would seem to work in favor of Jiangnan, we need focused and specific explanations of the nineteenth-century divergence between these regions. Global characterizations of either region over long periods of time as uniquely entrepreneurial, or inward-looking, or involuted, seem less promising than more targeted discussions of particular problems with, say, energy supplies or external trade in some specific period. In fact, as we will see later, I actually offer an

analysis of problems caused at particular moments by the labor intensity of Chinese agriculture that shares some elements of Huang's: but while he sees this as the fundamental dynamic governing over six hundred years of Chinese history, I see it becoming a problem at particular moments in the face of particular technological choices and the conjunctural disappearance of certain once-effective palliatives.

Nor, of course, do I actually argue that the important differences between England and Jiangnan appeared overnight in 1800, as Huang asserts (especially in his discussion of how I treat coal [531–33; cf. Pomeranz 2000a, 59–61]). I use 1750 as a benchmark as often as I do 1800 and make repeated reference to gradually emerging circumstances in which certain long-standing differences favoring England became much more important and others favoring Jiangnan became much less so. I think it fair to say (as Paul Bairoch and others have argued from completely different evidence [1975, 7, 13, 14; Maddison 1995, 19; 1998, 40]) that the enormous divergence in standards of living that was apparent to mid-nineteenth-century observers was of rather recent vintage, but this is not to deny that what became apparent in this period had, like most major historical events, been taking shape slowly for quite a while.

For those who have not read my book, it is also important to note (as Huang does only in passing) that the Jiangnan/England comparison is accompanied by a comparison of the larger political/economic units—Europe and China—in which those advanced regions were embedded. Huang is, of course, free to focus on one axis of comparison, but readers need to know that both comparisons are included in *The Great Divergence*, as well as several other analytic strategies. In part this is because much of my argument about why these two areas ultimately diverged hinges on their different relationships to larger units; it is also because the available evidence sometimes lets us make much more confident comparisons for one pair than for the other. This will become particularly important in the discussion of textiles below.

That the Jiangnan/England comparison is accompanied by a China/Europe one also matters for another reason, one important when deciding whether any particular difference between two areas should be seen as a matter of degree or as the basis for assigning the two areas to fundamentally different categories. While I, like Huang and the Europeanists he relies on most heavily (Robert Brenner and E. A. Wrigley), have generally emphasized differences between England and continental Europe, we should both bear in mind that the main scholarly trend since the 1970s has been to narrow the long-range significance of those differences: the works of Nicholas Crafts (1977, 1985) and of Patrick O'Brien and Caglar Keyder (1978) comparing England and France are perhaps the best-known examples, but there are many others (Grantham 1989b; Hoffman 1996; de Vries and van der Woude 1997). And there is, after all, an undeniable historical fact that underlies this scholarly tendency: within a century of the British industrial breakthrough, several regions in western Europe had made equal or larger economic gains, canceling out their preindustrial disadvantages. So whatever preindustrial differences existed—and England was richer than any place on the Continent, even before the late eighteenth century—they were not sufficient to block development in western Europe for long. Nor is this simply a matter of various parts of the Continent having caught up to England along the same path. More careful analysis has shown that there was no single western European path to the twentieth century, in many ways anticipating arguments that are now being made on a broader scale in light of East Asian experiences. So when we find that China as a whole matches up well against Europe as a whole on various points, or that Jiangnan, while trailing England in certain dimensions, still was closer to it than either of these leading regions was to even relatively prosperous parts of continental Europe, this too

suggests that eighteenth-century Jiangnan does not belong in a radically different category from various regions that had much happier nineteenth and twentieth centuries. Given the state of the data, one can only argue for approximate equivalence. I make no secret of that—and the argument requires no more.

Specific Comparisons

Huang spends much of his essay criticizing arguments I never made or arguments that he distorts. About 20 percent of his review, for instance, is devoted to demonstrating that English agriculture was more capital intensive and less labor intensive than Yangzi delta agriculture. This is true by most measures, and I never said otherwise, despite Huang's unsupported claim that my "main empirical bases concern capital use in agriculture and population dynamics" (502).[1] While I do argue for comparable availability of fertilizer per acre, as he notes (507), the point that this makes is one about ecology and soil fertility, not about capital intensity. I do argue that standards of living, efficiency of factor markets, the dynamics of labor allocation, and certain kinds of ecological stress within core regions were roughly comparable between China and Europe (and between the Yangzi delta and England). These are all debatable propositions, but Huang barely touches on one (standard of living), and essentially ignores the other three, focusing his attention instead on claims that do not appear in *The Great Divergence*. As a result, he not only misapprehends many key points in our disagreements but also misses some areas of agreement.

So, contrary to Huang's assertion, I accept that Jiangnan agriculture was less capital intensive than Britain's and then ask if this was really the serious barrier to development that Huang and some others have claimed. Nevertheless, Huang's comparisons of labor intensity, both between Jiangnan and England and among different activities in Jiangnan, are distorted and need to be corrected in both areas. Three of his errors deserve particular attention:

1) Huang focuses on labor inputs per unit of land when what matters is the ratio of labor inputs to output;
2) He adds together adult male, adult female, and child labor indiscriminately, as if we would expect them all to be equally productive; and
3) Having omitted in most cases the step of comparing labor inputs per unit of output rather than per acre, he inevitably omits the further task of figuring out how much of a given output consisted of returns to a day's labor per se, rather than to land or capital.

These mistakes compound each other and make for huge cumulative errors: some of Huang's estimates of relative labor intensity are off by more than a factor of ten. Unfortunately, demonstrating this is laborious, particularly since I want readers to

[1]My only guess as to what this claim refers to is the argument in chapter 2 that Chinese institutions were probably as efficient as European ones (or perhaps more so) in allowing whatever land and capital was available to be rationally allocated. There is a vast difference between an argument about what kinds of barriers exist to combining factors of production optimally and a claim (which would be ridiculous) that the same ratios of combination were optimal or actual in two separate places. If there were perfectly efficient internal factor markets in present-day Canada and present-day Bangladesh one would certainly not expect that they would therefore use the same proportions of land, labor, and capital in their farming.

see how the work is done and to judge for themselves how much uncertainty remains for each given estimate. But working through the numbers does offer payoffs that go beyond resolving the disagreements here and let us see more precisely what was and was not different in economic development at the two ends of Eurasia.

First, Huang compares English and Jiangnan labor inputs per acre, ignoring (or in one case including but misstating) crucial differences in output per acre. If I work twice as long on my one-acre plot as somebody else but get twice as much from it, I do not suffer from lower returns to my labor: we eat and wear what is produced, not the land it is produced upon. Modern mines, factories, and office buildings, to take an extreme case, have very high ratios of labor inputs to land surface but even higher ratios of output to land surface, so nobody would call them examples of involution.

A rare case where Huang does go beyond simply stating land:labor ratios is his comparison of wheat production in Jiangnan and England. He begins by observing that English farms used about 4.27 adult male labor-days per *mu* of wheat, versus 7 in Jiangnan, "a ratio of about 1 to 1.6" (509). A few pages later (511), Huang posits 21.5 bushels as an average per-acre yield for English wheat; this works out to 7.6 Chinese *shi*. Since winter wheat in Jiangnan yielded about 1 *shi* per *mu* (511), or 6 *shi* per acre, this seems to increase the productivity difference further, to roughly 2:1.

But this is the wrong comparison. Winter wheat, as Huang himself notes, was a second crop in Jiangnan on land that also grew rice or occasionally cotton. Such second crops the world over almost always have lower yields than the primary crop, both per unit of labor and per acre. What makes them worthwhile nonetheless is that the land used for them is essentially free: in Jiangnan this was reflected in the widespread practice of sharecropping rents being based exclusively on the main crop (see, e.g., Li 1998, 127). If we compare the primary food crop on Jiangnan farms, rice, to English wheat, the story becomes quite different.

Using Huang's output estimate of 2.25 *shi* per *mu* (13.5 per acre) and 10 days of labor per *mu*, we get an output per labor-day of 0.225 *shi*: still inferior to the English ratio of 0.30 *shi* per day (7.6 *shi* per acre divided by 25.6 days per acre) but by a much smaller margin of 1:1.33. Yet even this is not the true comparison, because 1 *shi* of rice fed considerably more people than 1 *shi* of wheat: Chinese officials in the eighteenth century calculated the ratio at approximately one hundred people-days of food per *shi* of rice versus approximately seventy for wheat. The usual ratio of rice to wheat prices over the long haul from 1738 to 1789 was also almost exactly 10:7.[2] Once we make this adjustment, Jiangnan rice farming actually produced more nutrition per day of labor than English wheat farming, about 22.5 people-days of food per day of labor versus about 21 for the English case. If we do the same calculation for the entire Jiangnan rotation of rice plus winter wheat, productivity per work day falls to 17.4 people-days of food, so the comparison swings back in favor of England by 1.2:1, not enough to support Huang's claim of fundamental difference between developing and involuting agrarian regimes. Of course, wheat and rice were not the only crops grown in England and Jiangnan, but they were crucial (and Huang is, at any rate, at least partly wrong about the relative labor intensities of English arable farming and pasture).[3] Certainly, this kind of comparison between the labor:output

[2]See, e.g., Will and Wong (1991, 242) and Pan (1994, 92). The price ratio actually comes out to 10:7.1. Data from Wang Yeh-chien (1989, 431–32).

[3]Huang asserts (509) that pasturing animals necessarily involved less labor per acre than growing crops. This may seem obvious if our image is of North American cattle ranges; English livestock often were dairy cows, kept in relatively small spaces and requiring lots of care. Greg Clark (personal communication, 27 February 2002) has shown that in the mid-nineteenth century, Cheshire, which had 63 percent of its land in pasture and one cow per six acres, used 60 percent more laborers per acre than Norfolk, which had crops everywhere and almost no pasture.

ratios tells us more about the returns to labor and possibilities for development than the labor:land ratios that Huang frequently invokes without considering differences in output.[4] And lest anyone think that my choice of comparisons reflects a Sinocentric bias, consider that Robert Allen, perhaps the premier historian of English agriculture, has recently used a similar method and concluded that before the industrial revolution, there was little difference in the real earning power of Chinese, Japanese, and English farm workers (2001, 9).

Or look at the problem of agricultural labor intensity in a different way. We can ask, again using Huang's own figures, what percentage of the area's labor time had to be spent procuring food. As I will argue later, the significance of this measure can be overstated. If a society where this percentage is somewhat higher has an off-setting advantage in the amount of labor people are willing and able to do per year, the percentage of labor devoted to procuring food could be a bit higher than elsewhere without making much difference: as we will see, this is one implication of the work of Hayami (1986), Saitō (1985), Smith (1959), and others on how Tokugawa Japan's "industrious revolution" laid the basis for Meiji industrialization. But huge differences in this percentage would make the society that had to devote far more labor to procuring its food a less-promising candidate for sustained development. However, no such large differences existed between eighteenth-century England and Jiangnan.

To divide the problem into manageable parts, let me start with an unrealistic assumption, which we will correct shortly: that Jiangnan grew all its own food. For argument's sake, let us further stipulate that Jiangnan both grew and consumed food at the rates Huang postulates (511): 2.25 *shi* per *mu* of rice, plus 1 *shi* per *mu* of winter wheat (the equivalent in both nutritive and monetary value of about 0.7 *shi* of rice) for a total output of 2.95 *shi* of rice equivalent per *mu*, with consumption of 2.0 *shi* of rice equivalent per person per year. Thus one double-cropped *mu* would feed 1.5 people.[5] (Many Yangzi delta farmers who paid rent of course needed to work more and to produce more, but grain taken by landlords was also eventually eaten. What matters for our present purposes is how much labor was used to produce the food supply, not how individual families procured an adequate portion thereof.) Now notice—again using Huang's numbers—that the same double-cropped *mu* required seventeen labor-days per year (512). Thus it took 11.3 days of work (17 ÷ 1.5) to feed a person for a year.

What percentage of the total days worked in eighteenth-century Jiangnan would that have been? We cannot know for sure, but it is clear that it would not have been large. Assume, for argument's sake, that only half the population did any labor. This is bound to be an underestimate, since (if the age distribution of the population was similar to that in the early twentieth century) almost half the population was between ages twenty and forty-nine (Buck 1964, 377), and many older and younger people worked, too. Now, to depress further our estimate of total labor-days worked—and

[4]A contemporary analogy may be useful here, though it relies on price rather than physical output. A North Dakota wheat farmer puts in far less labor per acre than a New Jerseyan growing tomatoes for the New York City market; but an acre's tomatoes are worth vastly more than an acre of wheat, so the North Dakotan's output per unit of labor is probably not much higher. If it were, the New Jerseyan would either give up farming or move to North Dakota, given the lack of barriers to internal migration in the United States.

[5]This may be an overestimate, since most scholars estimate food consumption at more like 2.0–2.6 *shi* of rice per year (Marks 1991, 77–78; Will and Wong 1991, 465; Wang 1989, 428–29), and Guo Songyi's work (1994, 46–47) suggests a considerably higher figure. But let us use it for convenience and consistency with Huang's argument in the current essay.

thus to inflate the percentage of work devoted to food production—let us set the average work year at one hundred days per year: this is far below what anyone actually thinks was the case and would not even have supported minimal subsistence at the earnings per labor-day that Huang estimates.[6] In that case, there would be fifty days worked per year per Jiangnan resident, and still only 23 percent of them would have gone into grain production.

Once again, this would compare rather well with England, even at a slightly later date. Eric Jones cites as one of the great achievements of England's agricultural revolution the fact that by 1801 the percentage of the labor force in farming had fallen to 36 percent (1981a, 71). And since we know that in eighteenth-century England farm workers worked more hours per year than other workers (see, e.g., Voth 2000, 128), the percentage of labor devoted to farming in England must have been more than 36 percent even in 1800. The comparison is not actually quite as favorable to Jiangnan as it may appear, since the English figure includes all food production and the Jiangnan only the grain supply. But given the extreme conservatism of our estimating procedure, there is little reason to worry that this adjustment would change the basic conclusion that growing food consumed less of Jiangnan's total labor effort than of England's. (For France, George Grantham estimates that just meeting the country's subsistence grain needs required anywhere from 26 to 47 percent of all labor time [1993, 486–87].) Moreover, since Jiangnan families used crop residues for much of their fuel supply, the labor inputs described here actually covered most of food and fuel; by 1800 in England, agro-forestry (much less agriculture per se) had long since ceased to supply most of the country's fuel and, as I emphasize in my book, also no longer supplied the bulk of its clothing fiber.

Huang would no doubt point out that Jones's figure shows that over 60 percent of the English labor force had exited agriculture, while the percentage of Jiangnan residents who did no farming at all was much smaller: he essentially argues along those lines (518–20), making much of the fact that English industrial production was more urban than that of Jiangnan. We will discuss later when and how it matters that a handicraft labor force be totally divorced from agriculture, but for now it should do to cite Jones himself, in the same passage discussed above. He points out that for a world in which people pursued mixed occupations, the productivity measure we need is the share of labor-days spent in farming, and only because he lacks such data for England has he used the cruder measure provided by the percentage of individuals who farmed. We are, after all, interested in how much labor was available for manufacturing, commerce, and other tasks besides producing food; ten weavers who take a few days a year off for harvesting are a much larger proto-industrial workforce than one town-dweller completely detached from the land.

But now we must correct for our original unrealistic assumption. Jiangnan was not really self-sufficient in food. It may have imported as much as 22 percent (and

[6]See for instance Li (1998, 149–51). Huang's own estimates suggest that an average-sized rice/wheat farm would require 126 labor-days, and he argues that since nobody could make a living on that, families were obliged to do much more labor-intensive kinds of work. By his estimate, growing and processing just three *mu* of cotton would take 540 labor-days, and eventually yield sixty-nine bolts of cloth (510), worth in Huang's calculation seventeen *taels* (1990, 84, 86). Even if the family paid no rent or taxes, this would not be enough to meet the bare minimum of expenses for a laborer's family compiled by Fang Xing (1996). We must therefore assume that Huang thinks most families of five put in a good deal more than 540 labor-days: over 108 per *person*, versus our estimate of 100 per *worker* with only half of all people working.

certainly no less than 15 percent) of what it ate.[7] In part, this reflects precisely what Huang emphasizes so strongly: relatively small farm sizes and high rents meant that people were forced to devote some of their land and labor to activities besides grain growing. In part it also reflects that at particular moments people saw these other activities as an opportunity rather than a grim necessity. Regardless, the question for the moment is how much the reality that Jiangnan obtained some of its food indirectly—by producing other goods and importing rice—made it an involuted economy in which huge amounts of labor were needed to acquire a modest food supply.

The easiest way to compensate for Jiangnan's food imports is simply to multiply the share of labor going into food production by five-fourths, reflecting that local grain-growing labor obtained only four-fifths of the needed food. This would not upset the favorable comparison, or at least parity, with England established above. Besides, by 1800 England was no longer self-sufficient in food, either: about 5 percent of grain was imported, rising to 15 percent by 1831 and much more rapidly after 1846 (Overton 1996, 75). But let us instead try a more demanding test: estimating how much of the product of the other, more labor-intensive activities in the Jiangnan economy had to be set aside for procuring the last 20 percent of the food supply and adding that to our original figure. This method will take us through Huang's estimates of how much more labor intensive these other activities—which he sees as the prime examples of involution (510–13, 516–20)—were than grain growing. We will see that while there are some differences in labor intensity, Huang has grossly exaggerated them.

Let us start with cotton growing. Huang says that a *mu* of cotton required twice as much labor as a *mu* of rice (510), which is plausible. But again, the relevant issue is the ratio between labor inputs and value added, not between labor and land. As Huang and I agree, a *mu* of cotton yielded roughly thirty *jin* of ginned cotton or ninety *jin* of unginned cotton (Huang 1990, 84; Pomeranz 2000a, 330); unginned cotton usually sold for twenty to forty cash per *jin* at mid-century but much higher in drought years, and never below twenty cash even amidst bumper crops (Kishimoto 1997, 139). An average price of thirty-five cash thus seems a reasonable guess. Thus cotton farming yielded 3,150 cash for the twenty-one days of labor per *mu* or 150 cash per day. By contrast, the rice/wheat field discussed earlier would have yielded the equivalent of 2.95 *shi* of rice at roughly 1,500 cash (1.67 *taels* at 900 cash per *tael*) per *shi* or 4,425 cash in seventeen days: this works out to 260 cash per day, or a 1.7:1 difference, already a bit below the 2:1 difference in labor intensity that Huang suggests. If 20 percent of the food supply had to be obtained by exchanging this raw cotton for imported rice, the labor needed to feed one person would rise to 12.9 labor-days ($0.8 \times 11.3 + .02 \times 3.8$) or 26 percent of our super-low estimate of total days worked: higher than before, but still below the English figure of 36 percent of labor going into agriculture, and at the low end of Grantham's range of French estimates.

But before we concede even that much labor intensification, two serious complications must be dealt with. First, as Huang himself emphasizes, the shift from grain to cotton caused "women and children [to be] drawn deeply into agricultural production" (1990, 53). One probably should not count each labor-day of a woman as equivalent to that of a man, though how much it should be discounted is unclear; one certainly should not count child labor equally with that of adults. This narrows the productivity gap between grain and cotton growing, though by an unknown amount.

[7]Wang Yeh-chien (1989, 429). Wang's figures are conservatively calculated, so the true percentages may be even higher.

Second, cotton and rice were not generally grown on the same land. Land that was ecologically suitable for paddy was unlikely to be used for cotton; not only did rice yield more per labor-day, but as we just saw, it yielded more per *mu*. Some conversion of rice land to cotton based on price considerations alone may have occurred in the late Ming and very early Qing, when prices fluctuated wildly, but price movements in the eighteenth century were much milder and unlikely to have been as important as the physical realities of soil, elevation, and hydrology.[8]

Different land, of course, will have different prices and different rents, which in a land market as active as Jiangnan's reflect in some rough measure the share of the output due to that land. Paddy land was much more productive and was priced accordingly. Li Bozhong provides both seventeenth- and nineteenth-century data showing that in eastern Songjiang, where rice yields were low and most land was planted in cotton by the late Ming (if not earlier), rents were roughly 60 percent of those in lower-lying western Songjiang, where paddy rice was more common (Li 1998, 120, 58 on Songjiang topography). If we take this difference into account by deducting rent from the returns per labor-day for both cotton and rice/wheat farming, the gap between them narrows a bit to roughly 1.66:1; if we were to adjust further for the larger share of cotton labor that was child labor, the ratio would probably fall to 1.4:1.[9] And if the cotton growers could squeeze in a rent-free second crop of wheat, the ratio would be 1.3:1. A decline in average returns to labor as more people move from grain to cotton farming is still evident, as one would expect it to be as more labor is applied to non-irrigated as opposed to irrigated land, but the decline is now modest enough to be treated as normal.

The same omissions—of the value of output, the contribution to production of land and capital, and the differences between adult and child labor—lead to much greater distortions when Huang compares the labor intensity of rice cultivation to the combination of growing, spinning, and weaving cotton. In fact, a more accurate accounting changes some of his differentials in labor intensity by more than a factor of ten.

For argument's sake, let us use Huang's late-seventeenth-century figures for labor time, yields, and prices, though mid-eighteenth-century data yield slightly more optimistic results. Huang estimates that growing one *mu* of cotton and turning it into cloth took 180 labor-days and produced 22.7 bolts; this cloth sold for about 0.25 *taels* per bolt, and the resulting 5.7 *taels* of income would have bought about six *shi* of rice in most late-seventeenth-century years (1990, 84, 86). Thus, it would have taken roughly sixty days of this kind of labor to earn the two *shi* of rice Huang says was needed to feed a person. This is 5.3 times as much labor as it took to feed

[8]Huang (1990) has a complicated position on this issue: on pages 45 and 83–84 he seems to agree that features of the land itself were most important, but on page 82 he argues that land did switch between cotton and rice based on price trends. For problems with his description of these price trends see note 25 below.

[9]To see how this works, divide the rice/wheat harvest into 2.25 *shi* of rice and 0.7 *shi* of rice equivalent from wheat and assume that rent was roughly 50 percent of the primary crop (rice) while there was no additional rent for the wheat harvest—as was indeed normally the case. Then set the rent on cotton land at 60 percent of the paddy-land rent. The result depresses net earnings from rice/wheat farming to 2,730 cash per *mu* or 161 cash per labor-day and reduces net earnings from cotton farming to 2,033 cash per mu or 97 cash per labor-day: a ratio of 1.66:1. Were we to assume that enough child labor was involved in cotton farming to reduce the labor per *mu* there from twenty-one days to eighteen adult-male day-equivalents (still making no adjustment for male/female differences), the ratio shrinks to 161:113, or 1.42:1.

that person through rice/wheat double-cropping: a big gap, though already much smaller than those that Huang suggests for cloth versus rice (18:1) or cloth versus wheat (27:1) when he mistakenly uses labor:land ratios rather than labor:output ratios.

But even this 5.3:1 ratio is a large overstatement. Of the 180 days of labor involved in creating cloth, 91 were spinning days. As Huang himself notes "spinning was done almost entirely by children and the elderly, seldom even by adult women" (1990, 85). Children would also have done at least some of the 46 days of ginning, fluffing, and sizing—let us say half of it—and probably a few days of the actual cultivation and cotton-picking. So even if we count adult female laborers as the full equivalent of adult males, about 120 of the 180 labor-days involved were child labor, which should be severely discounted, probably by roughly two-thirds.[10] Thus, the production of cotton cloth from beginning to end takes about 100 rather than 180 adult labor-day equivalents, and the difference in gross production per labor-day falls to 3:1. This is still nontrivial, but it makes the multiple of labor intensity for handicrafts versus grain farming a small fraction of what Huang had suggested.

And even then we are not finished with corrections. Comparing the labor intensity of handicraft production to that of farming (especially irrigated farming), as Huang does, once again comes out very differently if we look at the value added (net production) per unit of labor instead of gross product. Farmers on irrigated land were working with a very valuable capital asset; people working on dry land were still using a fairly valuable one; people working on a simple rural spinning wheel, or even loom, were working with cheap assets.[11] As a result, one would not expect gross output in most rural handicrafts to be nearly as high as in agriculture. This relationship changes only when one gets new industrial technologies embedded in more expensive capital equipment, allowing industrial workers to be far more productive. In the current preindustrial case, subtracting out land rents from both Huang's grain-farming scenario and his cotton-growing and cloth-making scenario reduces the difference in value added per labor-day to almost exactly 2:1. In other words, a day's labor in the cotton growing–spinning–weaving complex yielded about 50 percent as much value added as one in grain farming rather than the 4 percent suggested by Huang's 27:1 comparison. This result is fully consistent with the estimates in my book: those working an extended labor year in textile production might wind up with more income than those engaged exclusively in farming. It is also consistent with a conclusion we will reach later via a different route: that arithmetical and other errors in Huang's 1990 book led him to miscalculate various measures of earnings from

[10]I have decided not to discount women's labor, because it makes my calculations more conservative and because the usual method of discounting—by comparing wage rates for men and women—will not work here. First of all, we lack good wage data for eighteenth-century China. Secondly, assuming that differences in wage rates accurately reflect productivity differences would be to assume an efficient labor market, and whether anything even remotely like that existed is one of the points at issue here. In the twentieth century, children were paid anywhere from 12.5 percent to 37.5 percent of the wages of adult males (Huang 1990: 66); not much to go on, but enough to suggest that by counting their labor as one-third of an adult labor unit I am unlikely to be discounting it too much. Roughly the same discount seems common in studies of preindustrial Europe.

[11]See Pan (1994, 61) citing Xu (1992, 406). Even an improved handloom cost four to five *taels* in the late nineteenth century, and traditional looms much less; a century or so earlier the nominal prices would have been significantly lower still. And even in the late nineteenth century, a spinning wheel cost roughly 0.8 *taels*.

both cotton textile work and agriculture by multiples ranging from roughly five to fifteen.

In a premodern economy, there is nothing abnormal about some significant decline in labor productivity as handicrafts are added to agriculture. In fact, as we will see later, a standard model of mixed agricultural/handicraft economies derived from northwestern European experience (and adapted to Jiangnan in my book) takes it as axiomatic that the marginal productivity of labor in agriculture starts out well above that in rural handicrafts; marginal productivity in agriculture then gradually declines with a fixed stock of land while that in handicrafts is more or less constant. (The hundredth day weaving should be as productive as the first, while the hundredth day working on a modest-sized farm clearly would not be.) Furthermore, the received wisdom from this Europe-derived model is that when marginal productivity in agriculture finally declines to the level of that in handicrafts, people stop devoting further labor to agriculture.[12] Thus, under premodern conditions, average labor productivity in farming will almost always be significantly above that in home textile production. (This need not imply that wages were higher in farming: they usually were not, for several reasons.[13]) There is absolutely nothing peculiarly Chinese or involuted about a decline of the magnitude that remains once Huang's errors are corrected. Instead the evidence shows that rural Chinese, like their contemporaries around the world, lived with the reality of declining marginal returns to labor in a world without dramatic technological change and—again like their contemporaries elsewhere—were able to mitigate but not eliminate this problem.

Can we thus view engaging in handicrafts, whether in China or Europe, as a clear sign of desperation born of demographic pressure? Not necessarily, though the tradeoff of more total income per year in return for less average income per day must have been particularly attractive to large families of modest means. (Other possible reasons to accept this tradeoff will be discussed later, in the context of the industrious revolution.) More importantly, though, we no longer have any reason to see this dynamic in Jiangnan as different in kind from the similar pressures and choices that drove the increase in proto-industrial production in early modern Europe. Thus while English farming was indeed more capital intensive than Jiangnan's and most Jiangnan occupations other than grain growing did involve higher labor intensities, there is no foundation for Huang's claims that these differences were immense and place England and Jiangnan "virtually at opposite poles in a continuum from development to involution" (534). Instead, we are still in what I have called a "world of surprising resemblances" (2000a, part 1).

To nail down the point, let us compare the earning power per labor-day of a Jiangnan family growing cotton and turning it into cloth with those of mid-eighteenth-century English workers. Since we lack the data needed for a Chinese real wage index, I will continue to measure earnings in grain equivalents. In doing this,

[12]I took this model from Mokyr's work on Belgium and the Netherlands (1976, 132–64; cf. Pomeranz [2000a, 286–92] for my adaptation of the model). A graphic depiction of the model appears below (see Figure 1).

[13]Even if we assumed a perfectly functioning labor market, which existed nowhere, wages would reflect the marginal productivity of the last hour worked in agriculture, and landlords would presumably be able to drive farm wages (or returns left to tenants after paying) down to a level equal to the worker's next best option, in this case handicrafts. And given such realities as unequal power and information as well as the insecurity and sometimes stigma attached to landlessness, it is not surprising that farm wages were often lower than possible earnings in other rural occupations.

our procedure parallels that of Prasannan Parthasarathi, who showed that the real earnings of mid-eighteenth-century South Indian laborers were probably also roughly comparable to those of their English counterparts (1998, 82–89).

After we deduct the likely rent on a piece of cotton land so that we have a pure return to labor, we can calculate earnings from the cloth produced from one *mu* at 5.04 *taels* using Huang's late-seventeenth-century prices, enough for 5.3 *shi* of rice at the low grain prices of the time.[14] Again converting rice to wheat at the standard 10:7 rate, this is the equivalent of 7.6 *shi* of wheat, which is in turn (again using Huang's measurements) 21.6 bushels or 1,296 pounds. Since Huang's 180 days of labor (much of it by children) for this work was probably the equivalent of no more than 100 days of adult male labor, this would be 13 pounds of grain per day.

Parthasarathi, in his 1998 study, has converted the earnings of various groups of English workers to per-week earnings in grain. I have in turn then converted them into day wages in two different ways: one assuming a five-day workweek and one a (more likely) six-day workweek. In all cases, the English wages represent the payment to a family head and include the contribution of the rest of his family—they thus significantly understate the amount of labor done and overstate the returns per labor-day, biasing the comparison against Jiangnan (83 n. 18). (One might also expect weavers to do better than our combined weaver–spinner–farmer.) The results are summarized in Table 1.

The numbers are approximate, but the message is clear. At thirteen pounds of grain per adult male labor-day equivalent, the Jiangnan earnings in question compare very favorably to the wages of English agricultural workers and English rural and small-town weavers and probably exceed even those of London weavers. If these are earnings from an involuted activity, then the most important branch of English industry must have been similarly involuted.

What is Development?

Huang relies heavily on an assertion, also central to his 1990 book, that development consists of just one thing: "enhanced labor productivity through increased use of capital per unit of labor" (512). But it is not clear why we should focus exclusively on this kind of development, particularly before the industrial revolution. At a time when all the densely populated regions of the globe suffered from considerable underemployment and unemployment, finding new ways to employ labor was a very important form of development. It is worth remembering, as I point out in the book, that even in England, it counted against a patent application as late as 1720 if it was said to reduce the demand for labor; only 3.7 percent of English patentees in the eighteenth century overall cited labor saving as a goal, while many more referred to saving capital (Jacob 1988, 92–93; MacLeod 1988, 158–81). And where food shortages were still very common—as in continental Europe, if not England, even in the late eighteenth century—how close one came to maximizing total output mattered a great deal. As an alternative measure, one might try to look at total factor productivity, as Philip Hoffman has done for French agriculture over

[14]Xu (1992, 69) provides a range of prices; Huang's figure of 0.95 *taels* per *shi*, which I adopt here, is actually a bit on the high side for this period.

Table 1. English wages of the mid-eighteenth century converted to grain equivalents

Worker category	Based on six-day week	Based on five-day week
Agricultural laborers	5.0–5.8 pounds	6.0–7.0 pounds
Rural/small town weavers	6.7–8.3 pounds	8.0–10.0 pounds
London weavers	10.0 pounds	12.0 pounds
Spitalfields silk weavers*	20–23.3 pounds	24.0–28.0 pounds

Source: Parthasarathi (1998, 83, 84, 87).
*Including loom rental, payments to non-family assistants, and other working expenses.

the long haul,[15] though data problems probably make this impossible for China. One can also try to think through which particular features of an advanced agrarian/handicraft economy, if any, were either necessary or sufficient conditions for entering the radically different universe of sustained growth in energy use and per capita income that privileged parts of the world have experienced in the last two hundred years. This is what I have tried to do and what numerous scholars making comparisons within Europe have also attempted. This approach forces us to confront important questions about the ways in which the industrial revolution does and does not represent a break with earlier history. Both the question of what kinds of growth constituted success for eighteenth-century economies in their own terms and the question of whether Huang's more narrowly defined kind of development must necessarily precede other important parts of the transition to a modern economy are crucial to my argument, but Huang seems not to have even noticed that they are issues.

Huang's insistence on his particular definition would make most sense if we adopt a highly teleological approach, looking in the eighteenth century only for hints of the kind of development that dominates discussion in a world of extraordinary energy use, very rapid technological change, and other modern peculiarities. But even if we take that tack, the picture is more complex. While reaching the living standards of rich nations today requires that at some point the capital intensity of production increases and output per unit of labor rises sharply, there is in fact no consensus on when this must happen or whether (as was once thought) it must occur first in agriculture. Much work on Tokugawa Japan, for instance, argues that the labor-intensive and market-oriented nature of Japanese farming and industry in that period, combined with modest increases in the availability of desired goods for purchase, was crucial in creating the relatively skilled and disciplined work force (as well as the capital accumulation) that proved essential for industrialization (Hayami 1986; Saitō 1985; Sugihara 1996, 2000). Much of the work on proto-industrialization in Europe makes very similar points, as we will see.

There were enough features of eighteenth-century Jiangnan that resembled these and other core regions scattered around the globe—as others besides me have argued

[15]The growth of total factor productivity (TFP) is measured by the rate of output growth minus the rate of growth of all inputs (land, labor, and capital). When output increases faster than the total of these inputs, TFP rises, which is generally taken to reflect either improvement in organization, technology, or both (Hoffman 1996, 81–142). What matters here is that this measures how effectively an economy uses all its resources, not just labor, and is thus a better gauge than labor productivity alone of economic development across various situations, including those in which labor may not be the scarce factor of production.

(e.g., Wong 1997; Li 2000)—to ask whether its economy of highly commercialized labor-intensive handicrafts and agriculture also helped in some ways to prepare the ground for modern growth. As one part of this, let us consider how early modern labor intensification, which Huang opposes to development, in fact may have been an important part of it. The more we are willing to concede that Chayanov, on whom Huang relies, is right about at least some peasant households in some periods—that they aim to secure a target income needed for their biological and social reproduction rather than operating like a profit-maximizing firm—the more one would expect such people to prefer producing for home use and to respond to rising returns per labor hour by limiting their work for the market, accepting a more-or-less constant cash income while enjoying the additional security, autonomy, and/or leisure to be gained by focusing more of their time and effort within their households. It would then become all the more important to investigate how a given society arrived at the very different situation in which most people focus most of their labor on the market, work extremely intensely, and tend to provide more labor to the market in response to increased cash incentives in the hope of then purchasing more goods.

In both his 1985 and 1990 books, Huang aligns himself with a perspective—based largely on one particular interpretation of the English experience strongly associated with his colleague Robert Brenner—which insists that this change to a modern economic culture requires that people be forcibly deprived of any alternative through proletarianization; thus the importance of enclosures in England and Huang's central stress on the near absence of "managerial farming" using large amounts of wage labor in China.[16] But much work on British agriculture—including that of Mark Overton, whom Huang cites repeatedly—has questioned the centrality of this path even for Britain, arguing that exposure to market opportunities and the availability of new consumables that could only be obtained for cash might have been more important in encouraging increased efficiency.[17] Other research on a variety of other economies makes a strong case that the pressure of rising population, in combination with rent, taxes, or other cash obligations, did the trick even without proletarianization. This is the burden, for instance, of George Grantham's work on European agriculture—in which population growth (as both a creator of needs and a source of labor) plus market access spurred enough specialization and labor intensification to enable production to keep up with or to outpace slightly the number of mouths—and of quite a bit of other work questioning the empirical basis of Brenner's claim that French development was seriously retarded relative to Britain's by the persistence of small peasant farms (Parker and Croot 1985; Cooper 1985; Grantham 1989a and b; Hoffman 1996, 189–92). Similar arguments are central to much of the massive literature on European proto-industrialization (Kriedte, Medick, and Schlumbohm 1981; Levine 1977, 1985, 1987; Ogilvie and Cerman 1996). And for other parts of Europe and the world, a wealth of studies suggests that some combination of the pressure of growing population and the lure of new purchasable

[16]1990, 60–64; and in the current essay (514). Brenner, it should be noted, is even more concerned with showing that only under these circumstances (in which landlords have first lost the ability to squeeze surplus from their tenants though extra-market coercion but have gained the right to throw tenants off the land if they wish) will landlords invest in making the land more productive and shedding excess workers; but Huang emphasizes the decision-making of tenants rather than that of landlords, and that is, at any rate, the part of the argument most relevant for present purposes.

[17]Overton 1996, 133–92, 197–207. See especially his concluding statement of doubts about the Brenner thesis (204–5).

goods did this social and cultural preparation for modern development. These changes, sometimes referred to as an "industrious revolution," did not require proletarianization and were often well underway long before incomes per hour of labor began to rise significantly.[18] Thus, labor intensity—even increasing labor intensity—should not be seen as the antithesis of early modern development.

Of course, if some society had a form of agriculture so labor intensive that it needed all the labor it could mobilize just to grow a bare minimum of crops, this would block industrialization; under such circumstances, Huang's emphasis on raising productivity per labor hour as the sole measure of development would be justified. But nobody claims that this was true in Jiangnan; Huang himself has highlighted the growing amount of labor that the region devoted to handicrafts for sale (1990, 77–88). Huang sidesteps this point now by focusing on the percentage of the population living in towns as an indication of the capacity of agriculture to support industrialization (519–20): Jiangnan did indeed trail far behind eighteenth-century England by this indicator. But what matters most, as many Europeanists have noted and we have seen above, is not where people lived or whether they were fully separated from farming but the share of labor that they devoted to nonagricultural pursuits.[19] And by that measure, as we saw earlier, Jiangnan stacks up well even against England and certainly against other parts of Western Europe. Examples from Japan and Taiwan (and parts of China) to France and the Low Countries show that the complete separation of huge numbers of workers from agriculture need not precede significant industrialization as long as the society has other ways to elicit labor for nonagricultural tasks and to supply those people with food, fiber, and other necessities. This was no problem in eighteenth-century Jiangnan.

We will return to issues related to the possibility of paths to a modern industrial economy consistent with eighteenth-century Jiangnan's type of labor-intensive agriculture—questions which are certainly important in my book, and on which Huang focuses his critique. But my book was equally concerned with looking at China to learn the factors most important in helping Europe avoid what Huang would call an involutionary path, given that some tendencies in both directions were present throughout the early modern period. For thinking about this question, one also needs to look further at the extent to which different economies were able to increase total output through mobilizing more labor (what Huang calls growth without development), the reasons why this remained an important capacity in the eighteenth century, and the particular circumstances under which it became less important in the very late eighteenth and nineteenth centuries.

We must also avoid assigning to preindustrial capital the same primacy that capital is generally accorded in analyzing modern economies. In the same passage defining development that I quoted at the beginning of this section, Huang says that expansions of production through increasing labor intensity "had obvious limits and need to be clearly distinguished from development" as he defines it (512). His implication seems to be that development through increasing capital intensity has no

[18]See deVries (who most certainly includes England and Holland in his account of this development) 1993, 1994; Grantham 1989b, 1993; Voth 2000, 184–88 on population pressure and 192–210 on consumption; Saitō 1985; Hayami 1986; Sugihara 1996.

[19]Jones (1981a, 71). De Vries (2001, 11–13) also notes that "the urban percentage is the wrong indicator." Thus, while Huang criticizes me for not using de Vries' measurements of urbanization as the crucial measure of development (519–20), de Vries himself sees the increase in time spent on nonagricultural tasks as the more important issue, whether or not the people doing these other things are fully detached from the land.

such limits: that one can indefinitely substitute capital for land (or labor) if it happens to be in short supply. This appears to be true, or close to true, for the contemporary world with its astonishing energy supplies and the ability to turn petrochemicals into increased per-acre yields (through chemical fertilizers and pesticides). But it is highly anachronistic to make that assumption for the preindustrial world in which food, clothing fiber, fuel, and building materials all came primarily from vegetative growth and in which the production of yield-improving fertilizers, derived from either animals or plants, also required the use of land. Adam Smith certainly recognized that there were diminishing returns to capital as well as labor, and Karl Marx made that expectation a linchpin of his analysis. Only once one assumes near-constant technological innovation is it sensible to relax that assumption.

It was with this in mind that I asked whether the English pattern of saving labor to maximize profits (as capitalist farms tend to do), even at the cost of per-acre yields much lower than some in Asia, might have also soon reached important limits had not coal (each ton of which provides as much energy as the sustainable yield from at least one acre of temperate-zone forest and much more on average) and New World imports (especially of cotton) significantly reduced the need for England to meet its fuel and fiber needs from its own land or from purely European imports. This in turn forms part of a larger question central to my book: whether, without certain contingencies, the pressures toward maximizing output (or from the point of view of the struggling poor, maximizing income) by increasing labor intensity that one finds in eighteenth-century England could have continued outweighing the forces that pushed in the other direction, forces that eventually prevailed in the nineteenth century. If that had happened, Britain might well have had a nineteenth century more like that of Flanders, the Kinai, or Jiangnan.

Asking whether England could have been Jiangnan is not just a matter of symmetry with the more common question "could Jiangnan have been England?" It makes empirical sense for a number of reasons. First, Britain's population soared during its first century of industrialization, and while food yields kept up for a while, they did not keep up in the long run. Second, despite Huang's insistence on an "eighteenth-century English agricultural revolution" (502–3), there is considerable evidence that gains in total output were quite modest over the second half of the eighteenth century. The great achievement of that period was in maintaining or perhaps slightly increasing per-acre yields while shedding workers: an achievement that does not address my question.

Gains in land productivity, which are more germane to this question, had tapered off considerably by the mid-eighteenth century. There is a range of scholarly opinion on the timing and completeness of this slow-down, but all of it is more consistent with my views than with Huang's. At one end of the spectrum Greg Clark has argued that "the finding of little productivity growth in agriculture from 1700 to 1850 is consistent with all of the reliable information we have." Robert Allen takes a less radical view, but his work also contradicts any notion of uninterrupted progress in providing more food for Britain's growing population: he argues that the big changes in the middle and later eighteenth century were in the distribution of income from farming, not in output (with most of the gains in per-acre yields being achieved between 1650 and 1725).[20] F. M. L. Thompson also argued that the eighteenth century saw no agricultural revolution; the crucial breakthroughs came later, with

[20]Both Clark and Allen are cited in Overton 1996, 6–7. See also Clark 1991, 454–55 and Allen 1989, 80.

modern farming based on off-farm inputs (Thompson 1968, 63–73). Mark Overton gives us a much more positive reading of eighteenth-century developments: if per-acre productivity was 92 in 1600 and 100 in 1700, he calculates that it reached 135 by 1750 and climbed another 16 percent (to 158) by 1800. (It was then almost flat until 1836.) But even this assessment should give us pause: on the same scale, Overton tells us, per-acre productivity had already been 115 in 1300, so that it had risen by less than 40 percent in five hundred years (probably roughly comparable to increases in Jiangnan rice yields over the same period).[21] Third, when we remember that England's population increased from 5.77 million in 1751 to 16.74 million by 1851 (much faster than in China, not to mention Jiangnan) it becomes clear that even a far more impressive performance than this in maximizing per-acre output would not have sufficed by itself—and indeed during the late eighteenth century and especially the nineteenth century, England went from being a net exporter of food to a significant net importer (Overton 1996, 75–77). Early modern English farming succeeded in bringing per-acre yields on lagging farms up to the level that the best farms had reached long ago and perhaps slightly raising that best level, but as the very slow growth of the late eighteenth century suggests, there was not that much more room to continue this growth without using modern inputs, and amidst a tripling of population slow growth was clearly insufficient.

Fourth, the increases in yields that England did achieve seem to have been accompanied by serious ecological strain in some areas. This would have made them very difficult to sustain, much less to surpass, without the arrival of imported guano, some locally mined phosphates, and, much later, chemical fertilizer (Ambrosoli 1997, 412; Winter 1999, 40–61; Hobsbawm 1975, 106; F. M. L. Thompson 1968, 62–77; Overton 1996, 105). Fifth, even if one takes the most generous view of the capacity of English agriculture to keep up with English food needs during early industrialization, food was only part of what had come from agro-forestry: it is indisputable that clothing fiber increasingly came from abroad, and fuel and building materials from underground. One can argue about the extent to which technological progress in industry alone might have created enough English manufactured goods to trade for these needed inputs, even without the overseas expansion, post-Napoleonic agrarian reform on much of the European mainland, and fortunate location of English coal that I highlight; in fact, the weakest part of my argument (and the strongest case for real European exceptionalism) probably lies in the area of science and technology, which Huang does not address. But one simply cannot argue that the release of labor from agriculture amidst either stationary or modestly growing per-acre yields was enough to power the process without primary product imports and vastly increased use of fossil fuels.

Involution?

Huang insists that the best way to look at the Yangzi delta is still through the prism of involution, as he did in 1990. His use of that concept has been subject to a number of critiques, and it would be tedious to rehearse all the problems that they raise. But a few old and new points are worth making here:

[21]Ellis and Wang (1997, 185) estimate a 40 percent increase in rice yields from the 1200s to 1800s for Wujiang, a Jiangnan county for which Chao Kang has assembled some admittedly incomplete data. This figure does not include further gains from increased multi-cropping. Li Bozhong (1998, 125–27) suggests a 47 percent increase from the late Ming to 1840.

- The concept is inadequately defined and treats as peculiar to China economic phenomena that are common elsewhere, even in advanced industrial economies.
- In attempting to make eighteenth-century Jiangnan as unlike England as possible, Huang ignores or misstates the conclusions of a mass of literature on eighteenth-century England: that literature actually shows that gains in per-capita income were rather modest and the increase in labor required to create them substantial. For most of the century, real wages were probably falling.
- Huang's criticism of my discussion of textile earnings misconstrues my argument. It also rests on a peculiar refusal to use price data, which is essential to any analysis of the economic returns to different activities in a commercial economy. The price and earnings data we have for the eighteenth century, though sketchy, uniformly contradict his claims and support mine.
- One crucial calculation that Huang did make—of the amount of grain that a day of weaving would earn—is invalidated by a simple arithmetic mistake: he misplaced a decimal point. Once this is corrected, we see that weaving was ten to fifteen times as lucrative as he has said, no matter what prices we use. Overall textile earnings, combining spinning and weaving, naturally rise less but still increase several-fold. And were we also to correct for an inconsistency between two different estimates Huang uses of how much grain an adult consumed per day by insisting on the lower figure, the number of days' rations earned by a day's textile labor would rise by another 50 percent.

First, though Huang says that involution consists of "diminishing marginal returns to labor" (2002, 506), this definition does not work. Involution cannot simply mean falling returns to labor as it is added at a particular moment: diminishing marginal returns to all factors of production are the norm. If I take unpaid leave for the next year and do nothing for pay other than to accept my most lucrative speaking engagements, I would earn much more per day than by working at University of California, Irvine; but I would not work many days, and my total income would fall. That does not mean that by instead working year-round, I am engaged in involution. Similarly, if a farmer farms not only his best *mu* of land but also his next best *mu,* or spaces his seeds carefully rather than tossing them on the ground, that may also lower his per-hour yields, but (unless his second *mu* is truly awful land or his planting of seedlings incredibly slow and clumsy), this is not involutionary either.

Nor, for similar reasons, can involution simply mean a situation in which people substitute labor for other factors of production: in even the highest-wage economy, there are some labor-saving innovations that are not worth it, and in even the poorest, there are some that are.[22] Whether a labor-saving device is worth it depends on the particular technologies, prices, and other circumstances confronting people with a particular task in front of them. Labor-saving and labor-using are not general features of societies regardless of those particulars.

Thus, for the term involution to be useful, it cannot just indicate declining marginal returns; it must refer to one or both of two things. One possibility is that it refers to falling average returns to labor in an economy as a whole over time (which is what Huang usually seems to mean [512–13]), so that workers in, say, 1800, produced less per hour of work than in 1700. The other is that it refers (as it did in

[22]For instance a $10 million robot that mowed lawns would not be worth it, even in Sweden or Japan. But nowhere is labor so cheap that it would save money to have dozens of people stand all day blocking a roadside ditch rather than using plastic cones.

Clifford Geertz's [1963] more clearly specified version of involution) to the continued addition of labor at a given moment in time up to what seems a perverse extent: a point at which the marginal productivity of the labor being added is so low that nobody would pay a living wage for this work. For instance, weeding the same plot of land for the twentieth time might lead to a tiny increase in output but not enough for anybody to want to pay for the service. One would do this only on one's own plot and only if one needed more income so desperately and was so bereft of other opportunities (e.g., for hiring out) that even this minimal gain was more valuable than any alternative, including leisure. Huang in fact uses involution to mean both these things—though more often the former—which matters to both his argument and mine.

Insofar as involution refers to a long-term decline in real wages per day, this does appear to have occurred in China, as I note in my book (2000a, 95). But it also occurred in early modern Europe—the bread-buying power of a day's work took a nose dive between roughly 1430 and 1550 and did not surpass 1430 levels until 1840 at the earliest (and in some places, much later).[23] Even in England, where wages fell the least and recovered the fastest, there is no clear evidence that working-class living standards improved significantly until after 1840 (Mokyr 1988, 69–92). Meanwhile, recent research suggests that in addition to the increase in labor-days per year for almost all occupations, the average length of the workday may have increased. Hans-Joachim Voth finds evidence that the intensification of labor in London was sufficient to reduce the average amount of sleep per day by about an hour between 1760 and 1800 (1998, 35). Though Voth's evidence about sleep is only suggestive, he has very solid evidence for the more general proposition that hours worked per year increased substantially, as had long been suspected. If we ignore changes in the composition of the labor force (i.e., compare workers in like occupations across time while ignoring the gradual shift of workers out of agriculture), the increase is a stunning 35–45 percent between 1760 and 1800, culminating in an average work year of 3,300–3,600 hours; even if we do adjust for changes in sectoral composition, the increase is 20–27 percent over those forty years and peaks somewhere between 3,000 and 3,600 hours (1998, 37–40). In a subsequent study, Voth expands his range to England in general and forward to 1830. Calculating several different ways, he consistently finds an increase in the average work week of 20–23 percent for 1760–1830. London hours finally began to fall sometime after 1800 but hours in northern England continued to rise through 1830 (2000, 118–29, especially 126).

Thus, China's probable long-term decline in real wages per day alone can hardly show that China was on a radically different and less promising path. Part of the usefulness of deVries' concept of an "industrious revolution" is to show that stagnant or declining real wages can be reconciled with increased purchases of a variety of goods if households reallocated their time so that they provided more labor hours to the market (while decreasing the amount spent on production for personal use, though not necessarily by the same amount). In the especially well-documented case of late-eighteenth-century England, real wages fell by at least 8 percent from 1750 to 1800, but consumption per capita still climbed a modest 10 percent because of the increase in hours worked per laborer (Voth 1998, 51).

[23]Abel (1980, 136, 161, 191); Braudel (1981, 132–35); Clark (1991, 446); Allen (2000, 40; 2001). Allen's data suggest that real wages in London (though not the rest of England) held up better than elsewhere in Europe, and had recovered to fifteenth-century levels by about 1750: but even in London, they did not clearly surpass those levels until after 1850.

I argue that something similar—an intensification of labor, slight increases in consumption (at least before 1750), and stagnant or falling real wages—happened in at least the most advanced regions of China. I develop a number of hypotheses, which Huang ignores, about where the similarities and differences may lie. How apt my comparison turns out to be will depend in large part on future research into Chinese consumption (of which more later). Huang gives us no reason to reject such a comparison out of hand or insist that China's decline in real wages and increase in labor hours represent an involutionary process categorically different from the same phenomena in Europe.

On other occasions, Huang suggests that what made diminishing returns to labor involutionary in China was that they declined to an exceptionally low, perhaps even sub-subsistence level, as in the Chayanovian model of a peasant economy; labor could be mobilized for such minimal returns only because it had no opportunity cost.[24] I have already noted in my book that Huang's estimates of earnings in textile work rely on prices from a very unusual time—the second-lowest cloth prices in a four-hundred-year period, according to Zhang Zhongmin, in a year when raw cotton prices were quite high (Zhang 1988, 207–8; see also Pomeranz 2000a, 101). (Huang uses no eighteenth-century prices and draws most of his late imperial price data from the deep depression years of the late seventeenth century.) Use more typical prices, and the earnings look better. In his current essay, Huang asserts his preference for not using the available price data (517), an approach that seems unpromising for tracking changes over time in the rewards to different activities but which is consistent with the puzzling way he treats the price data in his 1990 book.[25] He says he prefers to rely on Xu Xinwu's work (which has problems we will discuss later). But in the section Huang cites, Xu gives us one off-hand early Qing comment on prices and notes that it may be unusual and that relative prices fluctuated considerably; he then has a few other scattered prices for the very unstable years of the seventeenth century, which are, predictably, very different from one another (1992, 88, 90–92). Xu makes no estimates and provides no data for the period under discussion here, but Huang turns his attempted snapshot of one earlier moment into an unchanging "basic condition of production" (517).

Huang makes much (517) of the fact that I use prices from three different sources that report on heterogeneous kinds of cloth and other conditions, but I do not mix and match them arbitrarily as he implies. Instead I show that in whichever set, the resulting earnings are much higher than Huang's figures; Lu Hanchao also reached a more optimistic conclusion than Huang about the earnings of rural textile producers, even using essentially the same prices as Huang (1992, 482–83). While Huang may be right when he cites Xu to the effect that we know of no cotton textile worker who amassed lots of capital this way (1990, 86), this could easily be an artifact of the limited records that survive. At any rate, it is much more significant that we know of many people who supported a family by spinning and weaving alone: the names

[24]1990, 65, 309. Note that the example on page 65 is actually from Henan, not Jiangnan. Also see my discussion below of Huang's textile earnings estimates, which also suggest sub-subsistence returns to textile labor.

[25]Consider, for instance, page 82, where Huang claims that rice prices varied by season, but "did not vary much" from year to year, and follows this with a brief table of seventeenth-century prices. The three recorded summer prices on this list are 1.0, 2.0 and 4.0 *taels* per *shi*; the two sixth-month prices are 1.3 and 4.9 *taels* per *shi*; the two second-month prices are 1.0 and 3.0, and so on.

of some of them fill the gazetteers' lists of "chaste widows." James Shih cites three such examples; two are of special interest become they come from mid-century and cite per-day earnings figures. One of these widows earned 50 to 60 cash per day by spinning and weaving, the other 50 cash; this matches perfectly with my estimate that 210 days a year of such work would earn about 10,800 cash.[26] (Huang calls this estimate "implausible" and says it is based on some unspecified "manipulat[ing]" of the data, but he makes no argument [517].) By contrast, Huang's insistence, following Xu Xinwu, that 210 days of spinning and weaving would have yielded only enough income to buy grain for 1.5 people (517–18) makes it hard to see how any of these widows could have raised children, as many of them did. If we accept Fang Xing's estimate that grain was about 55 percent of the basic set of necessities the poor needed, then anyone earning money at the rate Xu and Huang suggest would have needed to work 255 days a year just to earn her own subsistence; probably more in fact, since a smaller family cannot always get by on proportionately less housing, cooking fuel, and so on.

Huang's point that spinning alone earned very meager returns is correct, though even those returns were probably greater than he suggests. But since, as he emphasizes, most households combined spinning and weaving (though not always in perfect proportion to each other), what matters is surely the return from the whole enterprise, not from one intermediate step. In general, Huang seems to have misread this portion of my book, since he makes three claims about it that are flatly contradicted by the text (as well as other errors).[27] And because we in fact agree that combining spinning and weaving was the norm, the one genuine mistake that he finds in my work—an error in appendix E's allocation of labor time between the tasks of preparing, spinning, and weaving—is insignificant for my larger argument.[28] (It is worth noting, however,

[26]Shih (1992, 128); Pomeranz (2000a, 319). My thanks to Yeh Wen-hsin for alerting me to Shih's book.

[27]First, Huang complains that I average the productivity of spinners using two different technologies rather than ignoring the better technology, which did not spread beyond Songjiang (516–17): this ignores the fact that, in my very next sentence, I drop that procedure and assume for argument's sake that the typical spinner used the less efficient technology (2000a, 321). (Huang also ignores Li Bozhong's evidence, albeit limited, that the improved spinning wheel was sometimes used outside Songjiang [2000, 48–50].) Second, he says that I imagine a market for yarn where none existed (521). I do briefly develop scenarios for women who only spun and for women who only wove, which implies a market in yarn. Both Mark Elvin (1999, 151) and Li Bozhong (2000, 75, 77, 83) discuss such a market; so does Fang Xing (1987, 88), though that is for North China. Far more importantly, while I mention these other scenarios, my general argument rests squarely on what Huang and I agree is the much more common case of the family that both wove and spun. See Pomeranz 2000a, 102, 290, 316–26; a scenario involving a woman who only wove takes up one page of this eleven-page appendix and reads, "Probably few rural weavers purchased all their yarn. . . . *But for argument's sake* [emphasis added], let us imagine a woman who only wove. . . . " Finally, Huang says that I ignore the "mountain of evidence" that most households combined farming and proto-industrial activities (518), when I repeatedly do the opposite: my entire description of the delta takes this for granted, and I include a rather lengthy analysis of some consequences of the normative *nan geng nu zhi* ("men farm, women weave") household division of labor (2000a, 84–85, 248–50).

[28]In analyzing earnings from cotton spinning and weaving (appendix E), I somehow omitted the time spent removing the seeds and otherwise preparing the cotton. Since I began with the amount of time needed to make raw cotton into cloth and subtracted spinning time to get weaving time, this error means that I overestimated the amount of time spent on weaving, the most lucrative part of the production process. But, as Huang and I agree, most cloth was produced in households that did every stage of the production process themselves, and I have

that though we agree that combining spinning and weaving was the norm, Huang cannot possibly be right in claiming that the existence of a market in yarn is simply my "imagination" [521]: see for instance the Jiaxing gazetteer that says: "People of humble means take what they have spun or woven, whether yarn or cloth, to the market at the break of day to exchange it. . . ." [*Jiaxing fuzhi* 33:7a, cited in Elvin (1999, 151)] as well as the discussion by Li Bozhong [2000a, 75, 77, 83], and consider the simple fact that not every family can possibly have had the right distribution of younger and mature labor to insure that it had neither a surplus nor a deficit of yarn compared to its weaving capacity.)

More importantly, though weaving was only about one-seventh of the total labor involved in turning raw cotton into cloth, it was far more lucrative than Huang thinks: enough so to invalidate his picture of earnings for textile production overall. Huang's picture rests on a simple error: he misplaced a decimal point. That neither he nor, so far as I know, anyone else has noticed this until now is perhaps more interesting than his having made the error in the first place: it suggests that we have been so in thrall to the false image of a desperately poor, involuted, and Malthusian China that even a figure that was in fact wildly inaccurate did not stand out as unlikely.

In his current essay, Huang says that grain farming and cotton weaving earned about the same per labor day (512) and refers us to his 1990 book, which says that in either of these activities a day's labor earned about enough for three catties of rice or two day's rations for an adult male (84–86). Let us first clarify a relatively small problem in Huang's work that unfortunately will make our calculations messier than they need to be: the rations of 1.5 catties per day that Huang assumes here (1990, 85) would equal an annual intake of slightly over 3 *shi* per year, as opposed to the 2 *shi* figure that he uses in this paper (509). Consequently, to be consistent with what he has done in the rest of this paper, we should inflate all the estimates that follow of how many days' rations a given task earned by 50 percent; on the other hand, in order to show with maximum clarity where Huang went wrong in the original 1990 calculations that underlie his notion of "involution" it is best to use the same estimate of daily rations as he used in his 1990 book. Thus, I will assume here that it took three catties of rice to feed an adult male for a day, while placing in parentheses the results we would get if we assumed a ration of two catties a day, as Huang does here. Either way, his errors in this matter are enough to completely invalidate his conclusions.

On pages 84–86 of his 1990 book, Huang says that a bolt of cloth took a day to weave and was worth roughly 0.2 to 0.3 *taels*. He then tells us that "with contemporary rice prices running around 0.06 *taels* a catty in the late seventeenth century, this translated into gross earnings of 3.3 to 5.0 catties of rice a day," while an adult in the 1980s consumed roughly 45 catties of rice per month, or 1.5 per day. But rice sold for approximately 0.9 to 1.0 *taels* per *shi* in the 1690s, and a *shi* was roughly 160 catties (1990, vii). Thus the price of rice was 0.006 *taels* per catty, not 0.06, and a

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the right figure for the total amount of labor needed to turn raw cotton into cloth. Thus this mistake has no effect at all on my estimates of the income of those households, or of a statistically average woman who participated proportionately in each stage of the process. It does mean that households that only wove must have been a very small percentage of the labor force, but my argument was always primarily based on those who both wove and spun. And it does mean that to the extent that weaving was a separate occupation, it paid even better than I realized (since it took less time, and the difference between the price of yarn and the price of cloth comes from separate data); that relatively small group would have earned far more per day than farm laborers if we make this adjustment, not "roughly the same" as Huang claims (512, see also 1990, 84–85).

day's weaving was worth thirty-three to fifty catties of rice per labor day: twenty-two to thirty-three days' worth rather than two. (Or thirty-three to fifty days' worth rather than three if we use 2.0 *shi* as the annual consumption of an adult male.)[29] Even once we subtract the cost of the yarn in this bolt (0.02 *taels*), the *net* earnings for a day's spinning would be roughly thirty to forty-six catties of rice or twenty to thirty days' worth (or thirty to forty-five days' worth at 2.0 *shi*).

Since weaving was only a part of overall textile production, the correction to earnings per day in that process is less than the multiple of ten to fifteen that is needed to correct Huang's error for weaving; but it is still a multiple of more than five due to this error alone.[30] Add corrections for a few smaller errors discussed above, and one gets the same conclusion I reached previously by a different route: Huang has miscalculated the degree of labor intensification involved in switching from grain growing to cotton textile production by roughly a factor of ten. Ironically, Huang is probably roughly correct that weaving and grain farming were very roughly equal in gross value of output per labor-day (though, as we saw, a much larger portion of the farmer's output should really be considered a return to capital and land, rather than labor, and would have been due as rent if the farmer was a tenant). This, however, is because in making this claim he uses the wrong measure of productivity in grain farming, and so is off by roughly a factor of ten in that calculation, too. If we not only correct these errors in measuring productivity, but also hold Huang to using the same estimate of daily consumption for these purposes that he used earlier in this paper, then his estimate of how many days worth of food could be paid for with an average day of either farming or weaving would be off even more—a factor of fifteen.[31]

[29]Huang seems to be using the correct rice price elsewhere, since he says that unginned cotton was worth twice as much as the same weight of rice (1990, 84) and that unginned cotton cost about 0.013 *taels* (86)—though he mistakenly treats this as the price of three catties of unginned cotton (which equaled one of ginned cotton) rather than one.

[30]Accepting Huang's estimate that six out of seven days' worth of textile production earned about twelve ounces per day of grain—barely half (three quarters) of an adult male ration—while the seventh day, weaving, produced two (three) days' worth of grain, then the seven-day total would produce roughly 5 (7.5) days' worth of adult male rations, or 0.71 (1.07) days of adult rations per labor-day (which might well be a sub-subsistence wage for a woman, too, at least if we adopt Huang's 1990 assumption that 1.5 catties per day were required). If we leave the estimate for the other 6 days unchanged but say that the seventh day actually produced about 25 (37.5) days of rations, then the 7 days of work would now yield 28 (42) days of rations: 5.6 times as much. If one then corrects, as we have done elsewhere, for the fact that much of the non-weaving labor was done by children, so that these 28 (42) days of rations were actually earned with an input of perhaps 4.3 adult labor-day equivalents, one would have earnings of 6.5 (9.8) days of adult rations per adult labor-day equivalent: a bit over nine times Huang's figure. Also note that this revised figure—28 (42) days of rations earned in 7 days of textile labor (now assuming that an adult woman, such as a widow with small children who could expect no help, did all the work herself)—works out to 4 (6) days' adult male rations earned per day. This would be about .04 *shi*, or 60 cash at mid-century prices, matching both the widow's earnings cited by Shih and my estimate that a 210-day work year would yield a bit over 12,000 cash. See above (559–61).

[31]Huang does not explain his claim (512) that farming paid "roughly the same" per workday as weaving—i.e. enough for two days' worth of food—but he is presumably referring to the twentieth-century data he has assembled (1990, 65) from J. L. Buck's surveys showing that an agricultural day laborer's cash wage was generally about the same in value as the day's food that he also received for working. (The citation in Huang's book is incorrect—it refers to Buck 1937, but there is no wage data on the page indicated—so it is hard to be sure.) If that is indeed the source for Huang's two-day estimate, then it is noteworthy for current purposes that Buck also tells us that a year laborer's wages in the "Yangtze Rice-Wheat" region

Nor was average labor productivity in textiles clearly stagnant, as Huang insists, and as I assumed so as to keep my income estimates conservative. His favorite authority, Xu Xinwu, cites sources suggesting that turning raw cotton into a bolt of cloth took seven days in the Kangxi reign and six later in the Qing; Wu Chengming and Xu Dixin cite estimates of seven days for the early Qing and five days for the late Qing and conclude that labor efficiency increased (Xu 1992, 51, 53; Xu and Wu 1985, 390). This is not much to go on, but it at least suggests that the unchanged handicraft productivity that Huang assumes needs to be demonstrated.

Farming households spent money on labor-saving as well as labor-intensifying innovations (though the balance was certainly toward increasing labor). One good example is the spread of beancake fertilizer. (Huang's attempt to fit fertilizer into his involutionary model contains a number of errors, which are discussed in the Appendix.) This required anywhere from 1/35 to 1/50 as much labor as an equally nutritious application of manure, but enough of it to provide just the supplementary fertilizer on 7.5 *mu* of paddy would have cost about 4.5 *taels:* enough to feed an adult male for an entire year circa 1750 (Pomeranz 2000a, 98 n. 140). Clearly a farm family would not have made such purchases had they been desperately short of cash but had plentiful, essentially free family labor at their disposal (Pomeranz 2000a, 98–99).

Having claimed incorrectly that I base my argument on a separation of cotton growing, spinning, and weaving, Huang next claims (517–19) that because of this, I fail to understand the difference between Jiangnan handicrafts and English proto-industrialization, particularly as described in David Levine's work on rural textile workers (Levine, 1977, 1985, 1987). Levine does emphasize, as both Huang and I note, that a group of rural proletarians emerged in early modern England who supported themselves on textile work without access to land. Thus these people could marry and start families without waiting until they inherited access to land. This independence of their elders and earlier age at marriage led to a sharp increase in the

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would have bought 1,187 kilograms of rice per year (Buck 1937, 306), which equals roughly 13.5 *shi.* If this were indeed the equivalent of only two times a worker's grain needs, he would be eating an absurd 6.75 *shi* per year. Since the wages for day laborers in Huang's table range from 43 percent to 90 percent of year-round laborers' wages (1990, 65) we should probably reduce Buck's figure to the equivalent of about 9.5 *shi* per year, but even that is from 3.2 to 4.8 times basic grain intake (rather than 2 times), depending on whether Huang is assuming a 2.0 or 3.0 *shi* diet here. But let us put this problem aside, since Huang has also made a conceptual error that creates considerably larger problems. To the extent that wages measure productivity (which involves assuming a fairly efficient labor market, which Huang generally does not believe existed) they would measure the marginal productivity of the last unit of labor hired, not the much higher average productivity of all the labor involved in grain production. To compare this to the productivity of the weaver is thus again to confuse marginal and average. To measure average productivity in grain farming—the appropriate comparison—refer again to my estimate using Huang's data that seventeen days of work required on one double-cropped *mu* generated a year's worth of food for 1.5 adults, or 32.25 people-days of food (at 2.0 *shi* per person per year) per labor-day. (This would be one adult, or 21.5 people-days of food per labor-day if we use the figure of 1.5 catties per person per day that Huang uses in his 1990 calculations of weaver's earnings; he does not make clear whether he was assuming 1.0 or 1.5 catties per day when he made the estimate that agricultural laborers earned the equivalent of two days' food per day of work.) We just saw that one day of weaving was actually worth well over twenty (thirty, or even forty) days of rice, rather than the two (three) that Huang mistakenly calculates due to his arithmetic error (1990, 86). This restores the rough equality of (gross) productivity between grain farming and weaving that Huang posits, but only because he has miscalculated the productivity of both activities by roughly a factor of ten; and net returns (after subtracting the shares of land and capital) would be much higher for weaving, as discussed above.

population. So far, so good—and certainly different from the Yangzi delta. But Levine also emphasizes that they needed at least two adult incomes to survive and usually child labor as well. He says that "the additional labour inputs provided by wives and children were crucial determinants of proletarian incomes during the period of industrialization," refers to "the family economy of the early industrialists in which men's labour might contribute as little as one-quarter of the total income," and argues that the fact that children could begin to earn money from a very early age in the proto-industrial economy was crucial to the survival of their families (1985, 175, 176, 189). Levine has also argued that the desperate need of these families, who owned no productive assets, to offer as much labor as possible to the market depressed wage rates further.[32] In sum this was an economy in which, in Levine's words, "many mouths were being supplied by the income generated from the labour of almost as many hands" (1985, 176). It would thus be a mistake to read Levine's work as indicating that these rural proletarians were necessarily more prosperous than textile workers in Jiangnan. He shows that a different institutional structure allowed young handicraft workers to marry sooner than had been possible earlier, when there was less wage labor available—which was not necessarily a good thing for the larger society—but the fact that these proto-industrial families needed multiple incomes to survive is at least as important as the fact that none of these incomes came from farming.

Thus the resemblance I point out between Levine's picture of England and Huang's picture of Jiangnan is quite real. I had no desire or need to turn Levine's story into a "purely involutionary" one as Huang charges (521): though in fact some Europeanists do see proto-industrialization as largely a dead end in terms of development.[33] On the contrary, it is precisely my point that dynamism and crisis can be found together in both early modern Europe and China and that we therefore need to avoid one-sidedly seeing only dynamism in Europe and only involution and a mounting crisis in China simply because we know which tendency won out in each place during the nineteenth century. Insofar as what Levine sees in proto-industrialization that leads to modern industry is (a) the growth of merchant profit; (b) the growth of merchant control; and (c) the growth of a workforce available for non-farm work many days per year, there is significant, though not complete, commonality.[34] Certainly (a) was present in Jiangnan, and (c) as well; (b) is more debatable, though there is a trend visible in the anecdotal literature toward greater merchant control of local textile marketing (and a decline in direct marketing of goods by producers), while the growing importance of long-distance markets, informal

[32]Levine 1977, 58–87. See the discussion in Pomeranz 2000a, 93. See also Kriedte, Medick, and Schlumbohm 1981, 57, 77–86.

[33]See for instance Kriedte, Medick, and Schlumbohm 1981, 41, 86, 146, 155, 185. Note also that in arguing for the importance of proto-industry to subsequent development in some parts of Europe, this work says that early modern Europe more closely resembled "development with surplus labor" than any contemporary third world country (1981, 28–29) and was marked by precisely the kind of Chayanovian dynamics (41) that Huang sees making late imperial China unlike early modern Europe.

[34]While it is true that Levine emphasizes the development of a work force that has cut all its ties to agriculture and so is available all year round, this appears to be an English peculiarity which is in no way essential to early industrialization: see for instance Postel-Vinay 1994 on France (into the very late nineteenth century). This is also where Saitō (1985), Hayami (1986), and others working on Japan come in, showing that a disciplined workforce that responded to market incentives could be created even without complete proletarianization, an argument which in many ways anticipates de Vries' concept of industrious revolution (1994) (a term he adapted from Hayami).

trademarks, and quality standards certainly made merchants more important.[35] It is Huang who overlooks the common elements in Chinese and European proto-industrialization, seeing only progress in one part of the world and only stagnation in the other.

Moreover, Huang assumes that whatever labor increase one does find in agriculture or proto-industry was not offset by any reductions elsewhere: note that in his examples of dramatic labor intensification with the shift to silk and cotton in this article, he implicitly assumes (as he seems to have done in his book) that the women and children who did this work were not previously doing any work that that they now abandoned. (Somebody, for instance, must have made the hemp or ramie cloth that cotton replaced.) His book makes clear one further crucial implication of the involutionary scenario: because, as he sees it, peasant households were engaged in a desperate battle to turn their super-abundant labor into even the most marginal increase in income, they would buy almost nothing that they could make for themselves, meaning that there was very little market for consumer products (1990, 91). DeVries' concept of industrious revolution, on the other hand, while acknowledging the weak performance of real wages, suggests that as families provided more labor to the market, they cut back on making things for themselves: in a sense they responded to the opportunity cost of their time and made time-saving purchases that at least partially offset the increase in time that they devoted to paying work. This brings us, then, to the issue of consumption.

Consumption

Here, too, some rather lengthy calculations are needed, but the basic points are simple:

- Huang ignores the methods used in Fang Xing's article on consumption and thus misstates its implications.
- Huang's empirical claims about food consumption in England are inaccurate; when they are corrected, we see that Jiangnan compares quite well to England in what was still the most important area of popular consumption.
- Huang simply ignores my arguments about kinds of consumption other than food and textiles.
- Huang fundamentally misconstrues what I say about cloth production, exports, and consumption in Jiangnan, suggesting that I attempt to hide the fact that Jiangnan exported much of the cloth it made when I, in fact, highlight it.
- Even if we make a more generous allowance for both raw cotton exports and cloth exports than I made in my book, the cloth remaining to be consumed in Jiangnan would still be roughly comparable to British levels.
- Huang's reliance on Xu Xinwu's estimate of cloth consumption to show that people in the high Qing lived at a bare subsistence level is circular, since Xu's figure was constructed by *assuming* precisely that. In fact, Xu has virtually no evidence about levels of cloth consumption in the Qing.

[35]On informal trademark see Lai and Hamilton 1986; for an argument about the relationship of market niches, product identification and differentiation, and merchant power, see Hamilton 1998.

- While Huang dismisses my estimates of cloth consumption for China as a whole circa 1750, these estimates are much more consistent than Xu's with other more firmly based work (which Huang himself has praised and relied upon) and with other reasonably well-established facts.

Huang and I agree that too little work has been done on the important subject of consumption and that Fang Xing's 1996 article is a useful step forward. Huang, however, ignores the limitations of this study (523–24), about which Fang himself is quite straightforward. First of all, Fang is looking at the very poor. His seventeenth- and eighteenth-century data come largely from the discussions of the consumption of landless laborers in *Shenshi nongshu* and *Bu nongshu*, who were among the poorest members of society (91–92). He has no independent estimates of income and basically limits himself to placing a cash value on the items that these guidebooks say are the bare minimum that a landowner's laborers will need either to receive from the landlord or to be able to buy with their wages. Moreover, though Fang assumes that his typical worker headed a family of five, he makes no allowance for any income provided by the laborer's wife except for some cloth she makes for the family. (In fact, most often a single landless laborer could not have afforded to support a family of five.) As a result, Fang's work provides a floor for thinking about consumption among employed people, not an average—nor does he claim it represents an average. The point I made using his work was that this floor was quite similar, in terms of the percentage of income spent on food, to the patterns of consumption one would find among a comparable class of Europeans (including Englishmen). Nothing Huang says affects that claim, and new research by leading Europeanists tends to reinforce it.[36] It is of course possible that the very poor were a larger percentage of Jiangnan's population than were the very poor in England, but that would be a different matter, and it is not obvious. Future research might help here—and a major purpose of a book like mine is to suggest where we most need new research—but Huang's blanket assertions, based on the assumption that all peasants were more or less equally poor, get us nowhere.

For the most part, Huang simply does not engage my arguments about consumption, except in respect to textiles. In particular, he ignores the evidence I present about food grains, which were the most important item of consumption at both ends of eighteenth-century Eurasia and of which Jiangnan's average consumption appears to have exceeded England's.[37] In a forthcoming article, I also take a stab at comparing

[36]The classic literature on Europe is discussed in Pomeranz 2000a, 92; see especially Braudel 1981, 131–33. More recently, Allen (2001, 9) confirms that the grain-buying power of rural laborers in eighteenth-century China, Japan, and England was quite similar (he cannot do urban comparisons due to lack of data); Hoffman et al. (forthcoming) shows that using improved class-specific cost-of-living indices widens the gap between rich and poor in early modern Europe, making trends in real income for most of the 1500–1800 period even better for the former and worse for the latter than was apparent using the old indices. The net result may be that we have somewhat understated European (or at least English and Dutch) wealth in the late eighteenth century, but that we have, if anything, also understated how close to the edge the majority still was on the eve of mechanized industrialization.

[37]At the time of my book I was unaware of Guo Songyi's article estimating grain production per capita in 1753. If we accept his estimate, we would arrive at a per capita grain consumption figure much higher still than mine (1994, 46–47). Guo estimates grain production for that year as 275,737,216,000 *jin* of unhusked rice and its equivalent that (converting at an average of 55 percent to edible grain) becomes 151,655,468,000 *jin* or roughly 1,000,000,000 *shi* of edible rice equivalent. Subtracting perhaps 15 percent for next year's

protein intake—admittedly with very thin data, especially on the Chinese side—and find that for the vast majority of the population, there was probably no great difference here, either. Huang's claim to the contrary—that a typical English meal "consisted of nearly equal proportions of grain (bread) and cheese/butter/milk/meat" (506)—would greatly surprise D. J. Oddy, author of the chapter on food in the *Cambridge Social History of Britain*. His data for laborers from 1787–93 suggest an intake of 9 pounds of bread and 0.5 pounds of potatoes per person per week, as compared to 0.3 pounds of meat, 0.1 pounds of fats (including vegetable oils), and 0.5 pints of milk. In caloric terms, this makes meat 5 percent of the diet, milk 1.5 percent, and all fats 3.1 percent, with bread and potatoes making up over 90 percent.[38] Even the data Oddy assembles for 1863 gives us 0.9 pounds of meat, 0.3 pounds of fats, and 1.4 pints of milk, versus 14.8 pounds of bread and potatoes. (The 1787–93 data works out to forty-nine grams of actual protein per person per day, which is actually below the probable intake of Jiangnan laborers [Oddy 1990, 269, 274].[39]) No doubt some other Englishmen and women ate better, but certainly not all of them; and rural laborers were still, as we have seen, the largest portion of the population.

In fact, Huang's claim that the eighteenth-century English diet was equal parts meat, bread, and dairy would also surprise J. C. Drummond and Anne Wilbraham, authors of the book which he cites as the putative authority for his claim: they in fact say nothing of the kind. On precisely the pages that Huang cites (1939, 245–50) they say that in the early eighteenth century when times were fairly good, most village laborers in the South ate meat once or twice a week; by the late eighteenth century they "were lucky if they had meat once a week" (1939, 245), and very rarely had milk (1939, 246). Northerners were better off with respect to milk (1939, 247), but meat was even more rare for them. The sample budget that Drummond and Wilbraham provide from a northern laboring family includes purchasing twelve pounds of meat per year—less than half an ounce per day—for the entire family; the southern one includes twenty-six pounds (all of it bacon, and therefore probably more fat than protein) (1939, 247). Thus this work, like Oddy's, confirms my view that the diet of ordinary folk in Jiangnan stacked up quite well against their English counterparts; why Huang cites it for the opposite contention is a mystery.

Moving beyond food—and contrary to what Huang says (521–22)—I spend a number of pages on the spread of various household goods through rural western Europe, noting both the existence and the limits of increased popular consumption, and suggesting some indications (at this point still anecdotal) of a parallel growth in rural consumption in parts of China. But since Huang wishes to focus the argument on textiles—certainly an important and relatively well-documented commodity—let us do the same.

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seed, would give us 850,000,000 unhusked *shi* for people to eat. If the population was roughly 220,000,000—Guo uses the official figure of 184,000,000 but that is generally considered too low—we would have about 3.77 shi per person per year, about 70 percent above the 2.2 shi per person per year average suggested by Marks and adopted in my book. And, as discussed above (560–61), Huang himself used an estimate of daily consumption (from 1980s data) that would equal over 3 *shi* per adult per year in mistakenly "proving" that a day's weaving would pay for only two or three days of food.

[38]I assume, for argument's sake that the meat was high-calorie (not especially lean) beef and the fats butter. For caloric conversions, see Guthrie 1995, appendix E.

[39]See also Pomeranz forthcoming, which adds figures (even lower than Oddy's for England) from France and the Netherlands and shows that Jiangnan protein intake was probably as high or higher.

Huang finds my discussion of textile consumption "particularly misleading" (522), in large part because I introduce production figures for the United Kingdom and Jiangnan. He says that this is to "ignore once again basic knowledge" (522): that Jiangnan exported much of the cloth it produced. Of course I know that, and say so, noting that the United Kingdom exported about a third of its cloth production in 1800 and that the share of Jiangnan cloth production that was exported was quite likely more than that (2000a, 331). This puts me roughly in line with Li Bozhong's estimate that about 40 percent of Jiangnan production was exported, though Huang prefers a higher number that seems to have been derived tautologically.[40] The problem is that we are all guessing about this proportion until we come up with better data. Thus it seems best to compare production figures, while noting that the share of this which should be deducted for exports is unknown for Jiangnan but seems likely to be in the same ballpark as for the United Kingdom, rather than to pretend we have good, independently grounded consumption figures.

Huang does not actually challenge my production estimate, which is based on land-use and cotton-yield estimates very similar to his. He 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. As the book makes clear (2000a, 332–33), this case is one where I had trouble insuring that my estimate was conservative because Jiangnan both imported and exported raw cotton in unknown and changing amounts. We do know, however, that the amount of raw cotton coming in (mostly from north China) had been gradually waning since some time in the seventeenth century as that region's own textile production grew and that Jiangnan's exports of raw cotton gradually rose in the eighteenth century. I am now inclined to think (mostly for reasons having to do with the limited supply of labor for spinning) that net exports of raw cotton became larger at an earlier date than I had previously thought. But even some rather drastic assumptions about how much raw cotton Jiangnan exported would still leave an impressive amount of cotton cloth production within Jiangnan.

Suppose, for argument's sake, that raw cotton imports to Jiangnan had completely ceased by the mid-eighteenth century, so that exports equaled net exports. Most of these exports went to Guangdong in return for sugar. (Jiangnan also exported some raw cotton to eastern Shandong, but the quantities do not seem to have been very large [Xu Tan 1998, 92].) Let us assume for the moment that all of Jiangnan's sugar was paid for with raw cotton exports and that sugar consumption was ten pounds per

[40]Li (1998, 109). Huang rather misleadingly invokes instead Xu's guess about the share of Songjiang production that was marketed; this was bound to be considerably higher, since Songjiang was unusually specialized in cotton textiles even within Jiangnan and since the share of output marketed would include that sold elsewhere within the delta—no small matter given the Delta's exceptional wealth, demographic weight (at 31,500,000 people, perhaps as much as one-sixth of China's population circa 1750), its presumed near-monopoly on its own market, and the fact that in all other regions it faced increasingly intense competition from local output, often complicated by high transport costs. Huang provides no citation for Xu's estimate, and I have been unable to find it. However Xu and Wu (1985, 392) also refer to this estimate, attribute it to Xu Xinwu, and describe how it was made: by first estimating how much cloth a typical textile-producing family could make, and then subtracting an estimate of the family's own consumption which, as we will see below (570), assumes that these families lived at bare subsistence. Thus for Huang to use this estimate of cloth sold to try to show that families consumed very little of the cloth they produced is completely circular.

head: double the upper bound of my estimates for China as a whole, absurdly high from Huang's point of view and leading to what Huang would have to consider excessively high estimates of cotton consumption in Lingnan.[41] A quick calculation shows that even that level of sugar consumption could have been paid for by exporting 103,000,000 pounds of ginned cotton or 3.3 pounds per capita circa 1750. This would still leave Jiangnan's cotton cloth production at about 11.2 pounds per capita—now about 13 percent below the United Kingdom's production of all kinds of cloth combined in 1800 rather than 12 percent above but still certainly close enough for our purposes.[42]

If we make this rather generous downward adjustment, subtract what was probably used for stuffing of jackets, quilts, and so on, and then assume that 40 percent of the cloth made with the remaining cotton was exported, we would still have local consumption of 6.6 pounds of cotton per capita in 1750 (plus some silk), versus 8.7 pounds for all textiles put together in the United Kingdom fifty years later. In short, the numbers for consumption have considerable uncertainty built in—as I made clear from the outset—but they still support basic comparability. And neither Huang nor anyone else has yet raised questions about the method by which the Jiangnan cotton production estimates that underlie these numbers were derived. No doubt Huang would think that even this much lower estimate of 6.6 pounds per capita makes Yangzi delta peasants look too well-clothed: but we need to remember that not everyone in the delta was a poor peasant, that cloth was not only used for clothing, and that Huang's figures, taken from Xu, depend on assertions about how long people kept using their clothes that actually come from the twentieth century. (This would vary with local norms, living standards, and cloth quality, and is very hard to observe, anyway.) I see nothing here, then, to justify Huang's abrupt dismissal of these estimates or of the general conclusion to which they point; his counter-proposal that we simply adopt Xu Xinwu's estimate of consumption is made without any supporting argument.

Moreover, in the passage Huang quotes on page 522, I am comparing cloth consumption estimates for China and Europe, not Jiangnan and England (and certainly not China and England, as Huang attests). For the comparison of China and Europe, it makes no difference how much of Jiangnan's cotton production was used locally and how much elsewhere in China. Huang dismisses my suggestion that China-wide cotton production circa 1750 was close to that in 1900 with one word: "implausible" (522). And this hypothesis is, in fact, hard to prove, or disprove. However, I made a number of arguments in the book about why this seemed fairly likely, and Huang does not explain what he thinks is wrong with any of them. Among other things,

[41]Since Lingnan in 1753 had about 17,500,000 people (Marks 1997, 280), these imports alone would give it a consumption of six pounds of cotton per capita, even before counting some local production, Indian imports, and north China imports. Such figures would be reconcilable with my China-wide estimates for this date (though a bit on the high side) but certainly not with Huang's. They should therefore represent a generous allowance for raw cotton exports in estimating what was left to be spun and woven within Jiangnan.

[42]Mazumdar (1998, 55) says that white sugar sold for as a bit less than three *taels* per picul (one hundred *jin* or 133 pounds) in mid-century. Kishimoto (1997, 139) suggests twenty to forty cash per *jin* (normally twenty to thirty) for unginned cotton in average year. So, one hundred *jin* of cotton would sell for about three thousand cash or 3.3 *taels,* and a pound of unginned cotton was worth roughly one pound of sugar (in fact probably a bit more). If Jiangnan did consume ten pounds of sugar per capita per year, it would have needed to export 310,000,000 pounds of unginned cotton or 103,000,000 pounds of ginned cotton to pay for the sugar. Such exports would decrease the amount of cotton to be spun and woven in Jiangnan itself by 3.3 pounds per capita.

per-acre cotton yields in the early twentieth century are about the same as those in the eighteenth, and Huang himself has argued that cultivated acreage in both of the two main cotton-producing areas (Jiangnan and the north China plain) was not much different in the Republican period than in the mid-Qing (1985, 322, 325; 1990, 342). (And at least in north China, far more people needed to be fed by the latter date.) Readers can go back to my book for the details. But for now, let me add one additional argument.

Huang and I have both approvingly cited the work of Richard Kraus—Huang has called it a "convincing picture of the broad trends in the decades between the 1870s and the 1930s" (1985, 125), though now he says that in relying on Kraus I have no "firm figures" for 1870 or 1900 (523). Kraus estimates that total cotton output in 1900 was 1,500,000,000 pounds, down from 1,850,000,000 pounds in 1870. But 1870 itself was just two years after the end of the Nian Rebellion in north China and just six years after the end of the Taiping Rebellion that devastated Jiangnan. Opinions differ about how fast Jiangnan recovered from the Taiping once peace returned, but I doubt that anyone would say that recovery was complete by 1870; moreover, a number of former cotton producers in the western part of the delta had by this time switched to silk in response to a French and Italian silkworm blight that opened new markets. As for north China, nobody thinks that recovery from the Nian was particularly rapid or complete (for one thing there was no coordinated state/gentry effort even remotely comparable to the Tongzhi Restoration in the Yangzi valley). Besides, 1870 saw a serious drought in the heart of the southern Zhili/western Shandong cotton country.[43] Under the circumstances, it seems very likely that 1870 cotton output was still significantly below the pre-Taiping peak. If it was 15 percent below, then the peak would have been 2,127,500,000 pounds; and in that case, cotton output could have been 1,500,000,000 pounds in 1750, as I suggest, could have risen by over 40 percent over the next several decades (not bad since there was probably little increase in acreage), and still could have come back down to the levels cited by Kraus for the later nineteenth century.

If such an estimate is even in the ballpark, then there is no conflict between my estimates for the eighteenth century and the lower ones for the twentieth century that both Huang and I cite. If, on the other hand, we accept Xu Dixin and Wu Chengming's estimate for 1840 as valid even for that date, we are led into some statistical improbabilities.[44] Moreover, contrary to Huang's claims, those estimates and that of Xu Xinwu are based on extremely thin evidence.

[43]*Zhongyang qixiangju qixiang kexue yanjiuyuan* (1981, 206). There was, in fact, not a single good weather year in this region between 1866 (when the Nian Rebellion was still in progress) and 1880 (when the horrific 1876–79 north China droughts had finally ended, and the floods that followed when the rains finally came were also over) (204–11).

[44]Xu and Wu (1985, 322) come up with a China-wide output figure of 970,000,000 *dan* or 1,280,000,000 pounds. (Since they work upward from a per-capita estimate to the aggregate one, their figure would be about 1,130,000,000 pounds if we corrected their population estimate in light of Skinner 1987.) To get from there to Kraus's 1870 estimate would thus require an increase of 45–64 percent between 1840 and 1870; this seems completely implausible since these thirty years were among the grimmest in all of Chinese history, not least in the country's main cotton-growing areas.

Regional data for Lingnan also cast doubt on Xu and Wu's estimate. That region's imports from India alone averaged over 57,000,000 pounds of ginned cotton from 1825–32, a bit over two pounds per capita (Morse 1966, iv, 105, 123, 145, 162, 186, 223, 253, 325; population from Marks 1997, 280). Thus, these imports alone (for which the numbers seem very solid) would give Lingnan a bit more than two-thirds of the consumption level that Xu and Wu

Xu Xinwu cites only one piece of Qing evidence bearing on total cotton production: a 1902 guess by an unidentified Westerner that China had about 400,000,000 people, each of whom used three pounds of cotton per year (1992, 181). His estimates for other years, Huang says, are "anchored by firm and precise *1936 data*" (523, my emphasis added), but in fact Xu does not tell us why the 1936 data are particularly reliable, much less why we should accept his backwards interpolations from them to other years. Xu's 1840 figure was created by constructing an absolute minimum level of "necessary" cotton consumption, assuming that this minimum represented actual per-capita consumption, and then multiplying by population; a slow increase is then assumed for each interval after 1840 in order to eventually get to the higher 1936 figure (1992, 192–93, 218). Xu does cite one estimate by a Qing literatus (Hong Liangji, writing in 1793) of the level of cotton consumption needed for basic subsistence (which Hong says had been easily attainable earlier, but is now becoming less so). Xu then dismisses Hong's estimate for no other reason than that it is two-thirds bigger than his estimate and "under the brutal exploitation of feudalism, the peasants of our country could not have enough food and clothing, and certainly could not have had that high a consumption level" (1992, 193–94). (Of course, Hong's guess might well have been wrong, but Xu's treatment of it certainly gives us no reason to follow Huang in treating his estimates as definitive.)

In short, Xu has made no effort actually to estimate Qing consumption levels but instead has assumed, based on a deduction from the class nature of Chinese society, that popular consumption was at the bare minimum needed for survival; this makes Huang's reliance on Xu to prove this point completely circular. Certainly, Huang has given us no reason whatsoever to insist that Xu's numbers are also valid for 1750. Moreover, in China in general (if not in Jiangnan), ramie, which I omitted in the book, would be a nontrivial further addition to the per-capita textile supply: though it was gradually replaced by cotton for most clothing purposes from the fourteenth century on, China still produced roughly two billion pounds of it per year, or almost five pounds per capita, as late as 1914–18 (Xu and Wu 2000, 124). I thus see nothing in Huang's argument that should lead to abandonment of my claims about China-

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suggest for the country as a whole at that time, and they appear to have been almost exclusively consumed within Lingnan (Morse 1966, iv, 186). While Guangdong was a richer than average province, Guangxi was not. More importantly, Lingnan still produced a lot of ramie (a lighter fabric than cotton) and was the warmest, most humid region of China; it should have used less than the average amount of cotton for padded jackets, quilts, and probably even everyday clothing. The region also grew some cotton of its own. Thus, if Wu and Xu's figure for 1840 were right, we would expect Lingnan's imports of non-Indian cotton to be tiny, but this was not the case. Certainly the British, who brought Indian cotton to Canton, knew they had only a small share of the regional market, and as Greenberg (1951, 80) summarizes it "the price of cotton at Canton turned on the nature of the China crop rather than the amount being imported from India," which would obviously not have been the case if Indian cotton had been most of the supply. (The sugar for cotton trade with Jiangnan, mentioned earlier, was known to be very large, and there were imports from north China as well.) If Indian imports were, say, one-third of the market (any more and their volume could hardly have had so little effect on price), then total Lingnan consumption would have been double Wu and Xu's national figure; if they were less than one-third of total shipments, the Lingnan figure gets higher still. Thus this regional example also suggests that Xu and Wu's estimates are too low even for 1840, but the more important point for present purposes is that Huang has given us no reason to doubt my argument that per-capita consumption should have been a good deal higher in 1750 than in 1840. Indeed, as I point in the book, even if per-capita consumption in each region of China remained exactly the same between these two dates, China-wide averages would have fallen significantly, since rich regions like Jiangnan, which did not come close to keeping up with the empire-wide population increase, would have had a declining weight in these averages.

wide textile consumption and how it compared to European levels. Certainly much more work needs to be done before we can prove or disprove my suggestions about consumption more generally (though it is worth noting, *pace* Huang, that I do in fact suggest that the most prosperous parts of western Europe were moving ahead in certain areas in the eighteenth century and offer some possible explanations [2000a, 153–57]), and I hope my book will contribute to stimulating such research. However, Huang's insistence that we already know the answers before making any detailed investigation gets us nowhere, and on the crucial issue of textiles, I think I am still on fairly firm ground.

Demography

James Lee, Cameron Campbell, and Wang Feng seem to me to have ably explained the many errors in Huang's critique of their prize-winning work in their article in this issue, "Positive Check or Chinese Checks?" Their calculation that, if we took Huang's preferred figure for the birth rate—which is central to his claim that population control came through Malthusian pressures rather than fertility control—and combined it with what we know of death rates, Chinese population would have gone from two hundred million in 1700 to over ten billion by 1900 is particularly compelling as a refutation of Huang's general picture of social and demographic dynamics.

However, it is worth adding here one or two smaller points here that relate very specifically to my own work. Huang's preference for a Malthusian, mortality-driven history of Chinese population leaves us in a quandary with respect to Jiangnan in particular. We know that this region experienced little population growth from 1750 to 1850, while China's population as a whole probably roughly doubled;[45] and even Huang would probably agree that Jiangnan had the highest standard of living in China. If fertility control was not at work, are we to conclude that this particularly prosperous area also had vastly higher death rates than the rest of the country? This is, of course, not impossible—indeed Mark Elvin has recently argued that people in some frontier areas were longer-lived than those in Jiangnan, despite being poorer, because there were still unclaimed resources they could survive on in times of extreme need (1999, 142–73). But even if this turns out to be true for some frontier districts, does Huang really want to argue that it was also true for the densely populated parts of the north China plain or the middle Yangzi, which also experienced much more rapid population growth than Jiangnan during these years (and suffered far more disasters in the late eighteenth and early nineteenth centuries)? Here, too, there are certainly puzzles still to be solved, and new data are desperately needed, but Huang's *a priori* insistence that involution must drive the story leads to implausible conclusions. Finally, to the extent that we do wish to make population a prime mover in the story, does it not makes sense to focus, as I do, on the very rapid population growth in many of China's less developed regions after 1750 rather than the fairly slow (circa 1393–1750) and then very slow (circa 1750–1850) population growth in Jiangnan itself?

[45]See for instance Li (1994). If we use Huang's preferred source, Cao Shuji (2000, 5:691–92), Yangzi delta population grew by 38 percent between 1776 and 1850: still far enough below the empire-wide rate to sustain my argument. But the mid-nineteenth-century figures that Cao uses are precisely the ones that G. William Skinner has convincingly criticized, arguing that they probably overstate the population of several provinces (including Zhejiang and Jiangsu) by 25 percent or more (1987, 1–79, see especially 74–75). If the 1776 figures are much less inaccurate than the 1850 ones, as Skinner's argument suggests, this would reduce the population growth rate for the delta over this period to almost zero.

Back to the Big Picture: The Nineteenth Century and Beyond

Reinterpreting the emergence of modern economies necessarily involves more speculation than reconstructing various specifics of eighteenth-century economies, but our new understanding of these specifics makes the task necessary. In a provocative series of essays, Kaoru Sugihara has divided the last five hundred years of global economic history into three periods that help place the relationship between population, labor intensity, and development in a new perspective. Had the world ended in 1820, he tells us, an economic history of the previous three hundred years would have been primarily the story of the "East Asian miracle" of sustaining record numbers of people at a slightly improving standard of living through a combination of labor-intensive land-saving and resource-saving techniques. That a relatively few people in Britain had perhaps done slightly better in per-capita terms by a different path might merit a short chapter at the end. (And, I would add, in light of recent evidence about rising labor inputs, the British path might not even have seemed that different until a later date.) By 1945 the big story would very look different, resembling the story most of us grew up with: it would be dominated by the success of western Europe and the neo-Europes in reaching unprecedented levels of per-capita consumption, based on unusually rapid technological innovation plus unprecedented inputs of capital, land, and natural resources per capita. But by 2000 the story looks different again, with East Asia (rather than, for instance, resource-rich lands in Africa and Latin America where Europeans had had considerable power to create the institutions they wanted) once again providing the most total growth in output and with some of its areas catching up to Europe even in per-capita terms. Moreover, this new surge of East Asian growth has occurred along a growth path that, Sugihara argues, represents a fusion of East Asian inheritances (including an industrious revolution) and European features, rather than a simple imitation of what had worked in the West (1996, 2000).

This is not the place to go into Sugihara's arguments—especially those about the post-1945 world—in detail, but his way of framing the issues does remind us that periodization matters enormously, and claims about how a particular trajectory over one period enabled or (especially) foreclosed possibilities at a later date must be made with great care. It also reminds us that while economic modernity must eventually include an increase in output per unit of labor based on the accumulation of capital embodying new technologies—which Huang treats as the whole of development—there are in fact other transformations involved as well and many possible patterns and sequences through which they may be interrelated. The case of densely populated Japan is worth considering here.

Huang concedes that Tokugawa Japan provides "instructive" evidence that a highly labor-intensive agriculture was not inconsistent with labor productivity development; he then tries to minimize the relevance of this fact by saying that there was no population growth in eighteenth-century Japan, while China doubled (515). True enough, but population growth in Jiangnan was very small after about 1750, as it was in Japan after 1730; moreover, even after a century of zero population growth the Kinai, Japan's most developed area, had roughly three times as many people per square mile as Jiangnan.[46] And while in some ways the advanced regions of Japan

[46]The Yangzi delta minus Tongzhou, Yangzhou, and Haimen (which are north of the river and not included in Jiangnan) had an area of 61,100 square kilometers (approximately

appear to have been more prosperous than Jiangnan, in other ways they were probably worse off: food supply per capita, for instance, appears to have been lower than most estimates for either Jiangnan or China in general.[47] Jiangnan need not therefore have had precisely the same developmental possibilities as the Kinai, but their many shared features are suggestive—high per-acre yields, extensive commercialization (despite Japanese institutions that, on paper, look even less like those of a textbook market economy than those of either China or most of western Europe), labor-intensive handicrafts taking place mostly in rural households, and so on—and the Kinai may have faced even more intense local resource pressures.

Why then, should we conclude that Chinese population growth (much less Jiangnan's very slow population growth after 1750) was driving an involutionary process that had to lead to disaster? Huang emphasizes the obvious point that there was "a terrible toll of lives exacted in the mid-nineteenth century" by wars, drought, and other calamities (528). I do not deny this, nor do I have any stake in doing so. On the contrary, a grim nineteenth century is rather helpful to my claims that we can reconcile a relatively good eighteenth century with the widespread poverty that undoubtedly existed in the early twentieth century. (It is worth noting, though, that various measures of things getting better or worse—e.g., mortality on the one hand and consumption of non-grain goods on the other—need not move in tandem.) But the existence of bloody conflict and other disasters does not by itself establish where we should locate their roots, any more than, say, the massive killing and territorial conflicts in central and eastern Europe between 1914 and 1945 establish in and of themselves that pre-1914 overpopulation (which many contemporaries perceived) was responsible for that round of blood-letting. We come back, then, to some very basic and as yet unresolved questions of modern Chinese history: Should we focus on failures of the state in the nineteenth century? If so, what parts of the state? (One could make a case for focusing on the monarchy, the army, Manchu-Han relations, local administration, or the higher reaches of the civil service during and after the Heshen scandal—these would have very different degrees of probable linkage to the sorts of grass-roots socioeconomic issues on which both Huang and I have worked.) Should we focus on imperialism and opium; on various kinds of social and economic dynamism that, however positive they might have been in one sense, placed unbearable strain on a Qing fiscal system and administrative structure that did not

25,000 square miles) (Wang 1989, 427) and a population of 31,500,000 in 1778. This gives us a density of 1,200 per square mile (slightly higher than Wang Yeh-chien's figure, since his delta includes counties to the north). Hanley and Yamamura (1977, 91) define the Kinai so that it would be no more than 580 square miles and put its population at 2.2 million in 1721, dropping to 2 million over the next sixty-five years and then staying there until the 1870s (1977, 121). Thus its population density was about 3,790 per square mile—in other words, more than three times that of Jiangnan. Trewartha (1965, 498, 501, 511) gives areas for the constituent parts of the Kinai that add up to 530 square miles, which would raise its population density to over four thousand per square mile.

[47]See Nishikawa (1986, 436–38) on caloric intake, which seems to have been under 1,700 kcal per capita. By contrast, the estimate of 2.2 *shi* of rice equivalent per day would be 1,837 kcal from grain alone, and most other scholars use higher figures. Huang, as we have seen above, uses 2 *shi* in the current essay, but over 3 *shi* in at least one crucial point in his 1990 book. Guo Songyi's grain output estimates (1994, 46–47) would lead to a figure of 3.77 *shi* per person circa 1753, though declining thereafter as population increased, much as I have suggested for cotton production.

change enough to keep up with them; or on a socioeconomic-ecological crisis rooted largely in population pressure?

Huang sees clear evidence of an inexorably mounting social crisis in the fact that there were large numbers of very poor people in late-eighteenth-century China. There certainly were, but so there always had been, and so there were everywhere at the time. He cites Hong Liangji and Chen Hongmou saying that they thought things were getting worse (529–30), though it is not clear how much either one had Jiangnan in mind; other contemporaries, such as Huang An, thought popular material welfare was improving in Jiangnan, or (like Chen Hongmou on another occasion) even complained that things were getting morally worse because ordinary people were overindulging (Huang An 1816, quoted in Shih 1992, 160; Chen [1820] 1962, 68:5a–6a). Simply citing individual anecdotes will not get us very far, as Lee, Campbell, and Wang emphasize in their essay (2002, 602). We have at this point no evidence that the number of very poor people was growing faster than the overall population, that they were increasingly desperate, or that, in what was certainly an imperfectly integrated labor market, an increasing number of unemployed vagrants was necessarily linked to downward pressure on the earning power of those who did have a piece of land, a secure tenacy, or other guaranteed access to work.

In lieu of direct evidence, Huang attempts to build his case for a general social crisis by pointing to the existence of wife-selling, a subject now being studied in detail by Matthew Sommer. Whatever may eventually emerge from Sommer's work, Huang's use of wife-selling cases for purposes of this essay is very shaky. He takes a sample of 628 legal cases from a 150-year period in three counties—none of them anywhere near Jiangnan—which seem unlikely to be representative of caseloads even in the counties from which they come.[48] He finds about 10 percent of them involve the buying and selling of women and asserts that this ratio holds for legal cases in China as a whole in the late eighteenth century; he then arbitrarily suggests that we multiply the number of such cases by twenty or one hundred to get the actual incidence of wife selling (528–29 n. 31). Even so, using his most expansive estimate, such cases would involve less than 1 percent of the population in any given year. Moreover, many of these cases appear to involve husbands who were chronically ill or disabled; in a passage in his book which looks forward to his current work in progress, Sommer refers to "the plethora of cases in legal archives in which women who had been abandoned or whose *invalid* husbands (emphasis added) could not support them resort to prostitution, adultery or remarriage" (2000, 318). The survival strategies of such people have their own intrinsic interest, but are of limited relevance to understanding either trends or levels of income among the able-bodied poor.

But for argument's sake, let us grant that China's mid-nineteenth-century catastrophes must be rooted in a socioeconomic-ecological crisis that had been slowly gathering force for some time as population grew. Indeed, my own argument about declining consumption of non-grain items between 1750 and 1900 suggests some sort of serious downturn, though not one that had been steadily building for centuries. This still would not establish that this crisis was equally severe or took the same form everywhere. Indeed, one implication of my argument—in which the diminishing

[48]Note how different his three county samples, all from imperfectly preserved archives, are from each other. Land disputes, for instance, make up 32 percent of Baxian cases, 20 percent of Baodi cases, and 62 percent of Dan-Xin cases. Decade-to-decade fluctuations are even more suspect: marriage cases, for instance, make up only seven of eighty-two Baxian cases in one decade and all forty of the cases in the next decade. See Huang 1996, 240 for the data.

ability of Jiangnan to exchange manufactures for primary products from other regions that were rapidly filling up and developing their own textile industries plays a central role—is to suggest that the roots of socioeconomic crisis may have lain largely outside the delta, but nonetheless affected the delta powerfully and in a way that is usefully contrasted with Europe's growing ability to export both manufactures and people while importing primary products. Population growth does play a role in my tentative explanation of China's nineteenth-century problems—but if demography is to be the motor, it seems best to focus on the regions where population was growing rapidly. This would not include Jiangnan.

One irony of Huang's polemic, then, is that he misses the fact that for certain periods, we actually share some common ground. Population growth in China, if not in the delta, does seem to me an important part of the mounting pressures on nineteenth-century China—though, as I suggest in the book and elsewhere, these pressures quite likely disrupted the state's ability to maintain order and infrastructure well before they posed a Malthusian problem of a more direct sort (if, in fact, they ever did). And those pressures elsewhere did ultimately make themselves felt in Jiangnan: in a decreasing ability to trade manufactures for rice and timber (which is one way the region had long turned its plentiful skilled labor and relatively plentiful capital into a substitute for scarce land), in an increasing burden of transport fees added to the grain tribute, and eventually, in the form of an armed invasion of "bare sticks" from Guangxi.

And at some point, once buffering its population pressure through trade became more difficult, Jiangnan did indeed find that additional labor just could not push up per-acre yields much more in the absence of modern inputs, which were not available until much later. Oddly enough, while Huang argues against me that "land yields are not infinitely inflatable in response to increased labor input" (511), it is he, rather than I, who seems to assume just that. He argues that the same basic process of labor intensification and roughly constant per-capita output had begun by the fourteenth century and kept right on going through the nineteenth-century crisis down to the 1980s. The same assumption seems to have guided him, as we saw, in the case of cotton, where he dismissed my hypothesis of declining China-wide per-capita production after 1750, even though population boomed while there is no evidence of increasing per-acre yields and little of expanding acreage. In fact, when I look briefly at the long run of post-1800 China near the end of the book (290–93), I make a series of arguments about the difficulties of shifting part-time agriculturalists in a very labor-intensive system completely out of farming that are not that different from what Huang observes for rural China in the 1980s.[49]

But the importance of completely separating workers from the land hinges on the existence of large-scale mechanized production facilities that need to be centralized and/or to have fixed equipment too expensive to allow it to be idle even briefly—and a host of examples, some of them already cited here, show that this was not a general requirement of early industrialization. In France, for instance, a large percentage of the industrial labor force still did some peak-season farm work even on the eve of

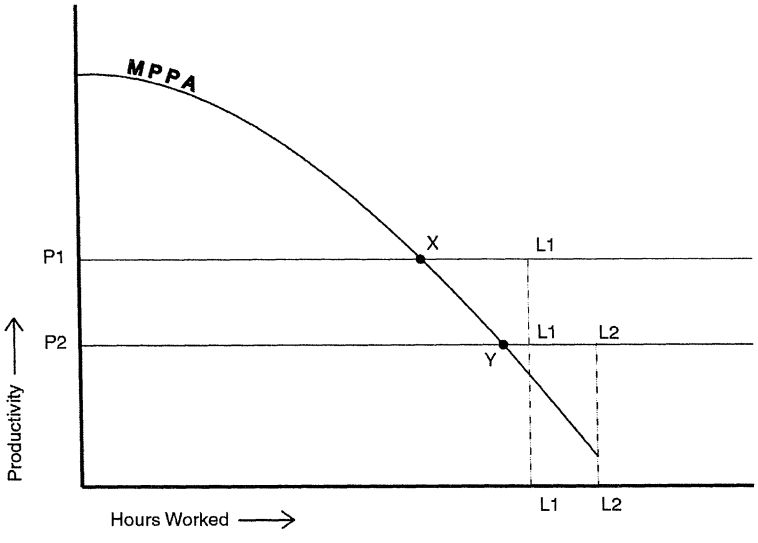
[49]As this may suggest, the Maoist period that immediately preceded the 1980s seems to me to be one for which Huang's basic involutionary argument may be quite useful for thinking about Jiangnan: population soared, migration was more or less forbidden, and extra labor was funneled in huge quantities into both labor-intensive farming and rural industries that, for the most part, were supposed to provide inputs to agriculture rather than generate goods to be sold elsewhere in exchange for farm products. But those conditions were temporary and, to a great extent, state-enforced.

World War I—by which time capital intensity far exceeded that of the early industrial revolution—yet labor productivity in French industry actually exceeded that in Britain on the eve of World War I (Postel-Vinay 1994, 65–66, 72–74, 78–79; O'Brien and Keydar 1978, 91). The Japanese and Taiwanese cases are even clearer on this matter, even though yet more capital-intensive machinery was available by the times that they began industrializing (the very late nineteenth century and mid-twentieth century, respectively).[50] And the stunning growth in parts of rural China over the last twenty years has been carried out with the explicit aim of encouraging peasants to "leave the farm but not the village" (Ho 1994, 1, 4, 6, 280), so that a very large percentage of people in the rapidly growing rural industries belongs to families who also do some farming, and may well help out with it at peak times (see for instance Ho 1994, 1995). As an essay in the last issue of *JAS* makes clear, one reason development economists are now so interested in these East Asian experiences as a model is precisely because they have not involved a sharp division of labor between farming and proto-industrial households (or regions) but rather a diversification of income sources within the rural household that fosters flexibility, encourages risk-taking while buffering the shocks of the market, and seems otherwise beneficial (Francks 2002, 33–55, especially 33–36). Meanwhile the history of industrialization in Europe has been revised in light of Sabel and Zeitlin's work, which makes it clear that even there, flexible production was far more important, and rigid patterns of mass production based on large amounts of fixed capital less dominant than in the classical models (1985, 1997).

Moreover, a fairly simple model of an economy with mixed occupations (such as Jiangnan's) shows the crucial importance of the relative prices for different goods that Huang prefers to ignore. A graphic representation similar to the one in my book (2000a, 291), and already discussed briefly above, appears in Figure 1.

The curved line MPPA represents the diminishing marginal physical returns to additional inputs of agricultural labor (let us say for rice growing) given a certain supply of land, technology, and so on. The flat line labeled P1 represents the marginal returns to additional inputs of labor in handicraft production (let us say for textiles), translated into rice at the prices for rice and cloth prevailing at a given date. As the MPPA line falls to the level of P1 and below, it no longer makes much sense for a household to put more labor into agriculture: instead it shifts into handicrafts. The total amount of labor as represented by the line P1–L1 can be divided into two segments: the one to the left of the intersection with MPPA gives us the amount of labor going into agriculture and the one to the right the amount of labor going into handicrafts. If the price of cloth relative to grain falls significantly (as I maintain it did from 1750–1840) so that we are now on line P2, it will be longer before the MPPA curve crosses that line; people will thus put more labor into agriculture (as one would expect when agriculture begins to pay better relative to other tasks). If the people involved have a fixed number of total hours they can or will work, then the handicraft labor supplied will shrink a bit (Y–L1 is shorter than X–L1): if they absolutely must make a certain target income that can not be met at L1, they will do more textile labor as well as more farm labor, and the total labor supplied will shift out to L2. Notice that in the absence of major technological changes, relative prices—

[50]Sugihara (2000, 11–13, 18–20, 29, 30, 1997); Oshima (1986, 792, 805); see also Ho (1986, 97), showing that Taiwan's number of farm households in 1980 was well above that in 1965 or 1960, but only 10.2 percent still had farming as a sole occupation, and less than half as a primary occupation.



MPPA = Marginal Physical Productivity of Agricultural Labor

Figure 1: Simplified model of labor allocation by rural households in a commercialized economy.

largely a matter of trade conditions—drive the system. We need not resort to intensifying population pressure in Jiangnan itself to explain labor intensification: a good thing since, as we have seen, there was little increase in that pressure between 1750 and 1850, and there was certainly a sharp decrease in that pressure after 1850 that was not fully undone until after 1950.[51]

The model should also make clear the crucial importance of labor productivity in industry relative to the cost of agricultural products. It is when that variable rises (from P2 to P1) that an area will see exit from agriculture in absolute, not just percentage, terms and the abandonment of agricultural activities with particularly low marginal returns. If the relative productivity of handicrafts falls, however, continued labor intensification in both agriculture and industry are likely. That in turn suggests the crucial importance of two variables: industrial technology and trade.

Returning from the model to historical reality, the importance of industrial technology for the industrial revolution is self-evident. I paid less attention to it in my book than one would in a general history of the industrial revolution, since I had relatively little to add to a massive specialist literature on the subject and because I saw no indication that others had overlooked the importance of this factor. The second issue, however, is trade: in particular, opportunities to trade with areas that would provide massive quantities of land-intensive primary products in return for the labor- and/or capital-intensive manufactures that core regions could produce in abundance. After all, if an area producing more and more industrial goods (whether through greater labor productivity or through greater labor inputs) and requiring more primary products (due to population growth, rising consumption, ecological decay at home,

[51]On post-Taiping population in Jiangnan and its gradual recovery, see Ho 1959, 236–47.

abandonment of agricultural tasks with small but non-zero productivity, or any combination thereof) faces static markets for its manufactures and/or a static supply of importable primary products, the price of those primary products will rise relative to those of handicrafts. The result will be to depress industrial productivity relative to farm productivity (since each unit of industrial production is worth less in terms of farm products), to choke off growth, and to push the economy toward greater labor intensity in both agriculture and handicrafts.

This, as I argued in the book, is what happened to Jiangnan from 1750–1840: its trading partners in north China, the middle and upper Yangzi, and elsewhere saw rapid population growth and greatly increased their own cloth production, thus diminishing both their exportable surpluses of food and timber and their need for Jiangnan textiles. It is also likely, though not yet proven, that long-distance trade was hobbled by the increasing disrepair of some of the transportation infrastructure (Pomeranz 2000a, 247; 2000b, 50–53). The resulting fall in the value of textile labor relative to rice production (which I estimate at between 25 and 50 percent over this period [2000a, 323–26]) was a serious blow to Jiangnan. (Mechanized competition later made the price squeeze even worse.) This downward pressure on textile earnings was partly offset by producing better quality cloth (Li 1998, 108; Fang 1987, 92), which one may see as a modest form of technological progress; but this probably required more labor and still did not fully offset the deterioration of Jiangnan's position. As noted above, Wu and Xu provide some faint evidence for increased labor productivity in textiles during the nineteenth century, but the evidence is sketchy.[52] Jiangnan weavers held on to most of their market, but probably at a high price in increased drudgery: it is noteworthy that Xu Xinwu suggests that by the early twentieth century, the average number of labor-days per year for those still in cotton textiles had soared to 305 (1992, 469), even though home spinning, the most time-consuming part of textile work, had been greatly reduced.

So in looking at the late eighteenth to early twentieth centuries, Huang and I actually share some ground, though I see this period as much more unusual than he does, and, of course, we differ as to the mechanisms that explain these occurrences. (We also differ in that he sees the movement of many former cotton textile workers in the western part of the delta into silk production after 1860 as an example of still further involution: I doubt that, both because the move began at a period of minimum population density—toward the end of the Taiping Rebellion—and because, thanks to strong foreign demand, the prices paid for Chinese silk seem to have been high enough to have raised incomes per labor-day.[53])

The English contrast to Jiangnan's deteriorating trade environment could not be sharper. There was an enormous boom in New World exports—historically conditioned, as I argued, by the area's remarkable ecological bounty and by an unusual set of institutional arrangements, including both slavery and the massive British investment in naval power and shipping. Institutional reforms on significant parts of the European continent during and after the Napoleonic Wars also loosened supply-side constraints on imports from there, though these changes took a long time to reach some of the most land-rich parts of the continent (e.g., Russia). Soaring

[52]See pp. 562 sup.

[53]For Huang's view, 1990, 121–22, 126–27. For a revisionist view of earnings per day in silk during the entire 1860–1940 period, which places them well above those in rice or wheat farming, see Zhang 2001, 35–55, 99–100, 114–20; for an interpretation closer to (though not identical with) Huang's, see Bell 1999.

quantities of land-intensive imports allowed England to experience a huge population boom, raise per capita consumption, and specialize more in manufacturing than before without facing sharply rising prices for primary products. British agriculture was relieved through this trade of the need to meet the country's soaring demand for fiber: gradually (and after 1846 not so gradually) it was also relived of the need to meet much of its demand for food. Without these "ghost acres," as Eric Jones calls them (1981b, 82–84), and the trade boom more generally, the British form of agricultural capitalism, which maximized profit (and increased output per workday) by shedding labor but did not maximize total agricultural output, simply could not have provided all that was needed. Others have seen this before me (e.g., Thomas 1985, 729–53), but the point seemed worth reemphasizing, as it is often glossed over in accounts that treat England (or Europe) in isolation from the wider world.

The coal story had similar effects, though different sources. Just as British agroforestry ceased to provide most of the country's fiber, it also ceased to provide most of its fuel. And, as Wrigley made clear, had England had to provide the fuel for both home use and industrial development through wood and charcoal, it simply could not have: there just was not enough land in Britain (1988, 54–55, 80–81). This much, I think, is beyond dispute. So, once again, is the contrast to Jiangnan, where annual vegetative growth remained the main source of fuel, as well as food, fiber, and building materials. Indeed Jiangnan's own land probably became more dominant as a source of all these necessities as imports ebbed; and given finite supplies of land, these intensifying demands could only be met in very labor-intensive and sometimes environmentally destructive ways.[54] Moreover the coal revolution in England had critical effects which went well beyond even the huge ones associated with easing land/resource constraints: these effects stem from its intimate relationship to the development of steam power, the transport revolution, iron and steel production, and military power.

What is controversial here is less my description of what occurred than my interpretation of it. Huang, however, misstates central parts of that interpretation. I do not, of course, claim that the English coal boom was "something that occurs only after 1800" (532–33—no citation provided). On the contrary, I refer to Britain's long experience with coal, related both to its relatively early deforestation and to the many visible surface outcroppings of its coal deposits, while noting the very rapid and accelerating increase in production during the eighteenth and nineteenth centuries: 70 percent from 1700 to 1750, 500 percent from 1750 to 1830, and 1,400 percent from 1815 to 1900 (2000a, 61, 283). Nor do I, as he suggests, claim that it was "chance access to coal and colonies, and that alone, that distingished England from the Yangzi delta" (532, again without citation; further, Huang does not even say I make the assertion, but rather says that this is "the impression [I] leave the reader with"). On the contrary, by emphasizing that the British coal boom was impossible without steam engines to pump the mines and that the gradual refinement of the steam engine was intimately related to skills developed through British leadership in the production of precision instruments, I make it clear that geographic luck was not the whole story (2000a, 61–62).

[54]Li Bozhong has provided a number of interesting details about how fuel shortages and lumber shortages hobbled Jiangnan industry in his recent book (2000, 282–97, 314–42), which generally fit the arguments I made (2000a, 63–65, 228–41, 289–91). On some environmental effects of decreasing long-distance trade within China, see Pomeranz 2000b, 50–53.

What I do claim is that luck was not inconsequential. Coal had to be present in large quantities, with enough outcroppings that people knew how to find it and got used to using it in a period in which prospecting was not yet scientific. It helped greatly that the coal lay relatively close to a great and fuel-hungry metropolis (and artisanal center) that had greatly depleted its wood supply at an early date and in which people had come to accept the use of coal for routine heating despite its environmental costs (something much of the rest of Europe would not yet tolerate). It also helped that the basic problem of the English mines was how to pump out the water (for which steam-powered pumps were perfect), rather than how to prevent the spontaneous combustion of gas, which was at least an equally pressing problem in mines located in more arid areas (including most of China's mines) (Yu Mingxia 1991, 23, 27). And Britain's various bits of both skill and good fortune worked together synergistically. Early steam engines were so wasteful of fuel, hard to move, and dangerous that they were not worth installing anywhere except at the pithead, where fuel was basically free (since they could use small coals that were not worth transporting). Finding some use for the early engines was probably quite important in getting people to keep refining them—and once refined, they came to solve a number of crucial problems besides pumping water out of mines. Coal per se made possible land- and labor-saving approaches to old problems; coal plus steam provided solutions to problems that had previously had no solutions and opened up a new world. This is not a story of pure luck, but luck mattered.

On the Chinese side, the situation was quite different. China does indeed have substantial coal deposits, as Huang notes, but the vast majority of the coal deposits, in the northwest, was too far away to be of any use to Jiangnan before railways. Tim Wright, whom Huang cites, notes that even in the early twentieth century, the price of coal in the northwest quintupled between the mine and a riverbank fifty kilometers away (Wright 1984, 9). China's nine southern provinces have just 1.8 percent of its coal, and its eleven eastern provinces (an overlapping group) 8 percent (Sun Jingzhi 1988, 93). Remoteness from the Jiangnan market not only raised transport costs to prohibitive levels but also meant distance from concentrations of skilled artisans, who might otherwise have helped solve technical problems. Meanwhile, Jiangnan had found other ways to alleviate its immediate shortages of fuel for everyday use: trade, the use of crop residues, cooking with woks, recycling iron, and so on—and nobody could have known how many new doors a coal/steam combination would open. Ironically, it may have been part of England's good fortune in the long run that London could not even meet its routine domestic fuel needs without coal.

This still does not completely settle the issue, nor do I claim it does. As I noted in the book, there were a few mines in northern Jiangsu which shipped some coal to Jiangnan, though certainly nowhere near enough to make a big impact (2000a, 65), and there were some mines supplying coal to Beijing, which was also an important though much smaller market.[55] In these cases, government ambivalence about large-scale mining (which was chiefly anxiety about large concentrations of young men) were a further retardant, not due to luck, and I note that as well (2000a, 64). It is peculiar, however, that Huang points to the Pingxiang mines in the Jiangxi-Hunan highlands, which after 1896 supplied the Hanyang complex, as an example of a mine that could "as easily have supplied the Yangzi delta" (533), since the very article he cites points to serious transportation problems that had to be overcome with the help

[55]Naquin (2000, 433). While I do note the existence of mines near Beijing (2000a, 64), I probably do not say enough about them.

of technologies (including electricity and railroads) that were not even imagined during the eighteenth century (Hornibrook 2001, 213, 215, 222–23, 226). Overall, it seems hard to doubt that a series of geographic accidents combined with anthropogenic factors (some intended, some not) to make a breakthrough in the extraction of fossil fuels much less likely in Jiangnan than in England, without having to posit, as Huang does, a lack of Chinese demand for fuel due to general under-development (533).

To sum up, I did not claim that lucky access to coal and colonies was the whole story of the great divergence. I am rather proud of William Rowe having said the argument was "at times dizzyingly multi-factored" (2001, 408); even coal and colonies, as I make clear, were not simply matters of luck. I do think, though, that China/Europe and Jiangnan/England comparisons help show that those two factors have been underestimated. And I do argue that this divergence, though not appearing suddenly after 1800, appeared much later than many scholars have thought. We do violence to the historical record if we let our retrospective knowledge (that, by the mid-nineteenth century, economic divergences had greatly overshadowed similarities) blot out our knowledge of those similarities, if we fail to explore their emergence and decline, or if we fail to acknowledge that the obstacles to development faced by eighteenth century societies at both ends of Eurasia formed overlapping, though not identical, sets.

What I have tried to do, then, is to suggest some ways that we can begin rank-ordering the importance of the many differences. To do that, we must first recognize that there were differences that cut in various directions, that some were long established and some fairly new, and that the relative significance of each of these differences shifted over time. We must also understand that the areas we compare were not isolated and so cannot be fully understood without seeing what was happening in areas with which they had important relationships: it mattered to Jiangnan, for instance, when its trading partners started producing more cloth and exporting less grain. And we will do much better if we recognize that there have been multiple paths to modern economies, and that even those elements that appear eventually in all of them need not appear in a uniform sequence. If we continue to recognize development only when we see facts conforming to a stylized version of English history—one that many economic historians have now modified substantially even for England, and which certainly does not describe many other nations (both in Europe and East Asia) that have become wealthy—we are simply putting on blinders.

My own guess is that the late eighteenth century was a crucial moment in part because Jiangnan/China was running into more serious problems on an empire-wide basis,[56] while England/Europe found itself in a world in which fossil fuels, ghost acres, and new technologies relaxed previously important land constraints and meant that areas in which it was comparatively backward (such as per-acre agricultural output) now mattered much less than they would have without those sources of relief. I fully expect that these guesses and many of the smaller hypotheses and estimates supporting them will be superseded by new research. But the key questions seem to me to be empirical and matters of refining, confirming, or overturning relatively specific, focused comparisons, while realizing that no one of them alone determined the fate of these societies. And I suspect that the general idea that at least the two ends of

[56]See Elvin 1998, 753 for an argument that given the relative openness of internal markets in China the only ecological equilibrium that could be reached in the long run was an empire-wide one; the vision suggests the same sorts of problems from import substitution and declining long-distance trade that I have highlighted, though not necessarily with the same timing.

Eurasia shared many economic resemblances until late in the eighteenth century will prove durable. Nothing will be resolved by placing China and Europe back in fundamentally separate categories of "developing" and "involutionary," insisting that only the contrasts between them are relevant, or insisting that these categories retained their relevance across six hundred years in which almost everything else changed.

Appendix: Beancake Fertilizer, Per-Acre Yields, and Labor Intensity

In my book and again here, I make the point that the adoption of beancake fertilizer was, among other things, a labor-saving innovation: a point that others have made as well, both for China and for Japan (see below). Huang's argument against this claim (507–8 and note 8) contains a number of nonsequiturs and other errors.

First, he argues that manure "*became*" (507, emphasis added) the dominant fertilizer, overtaking less labor-intensive green manures, as if this were a recent development; in fact, manure's dominance had begun much earlier, under very different land-labor ratios. Having pointed out himself that manure use is very labor intensive (507) Huang then needs to somehow avoid the conclusion that the massive adoption of beancake fertilizer in the late seventeenth and eighteenth centuries—a switch *from* manure to green fertilizer during our period—represented a significant savings of labor. (Manure use in the Yangzi delta in fact seems to have reached 1930s levels by the late Ming [see Perkins 1969, 73], when large-scale beancake use was just barely getting started and when human population—a main source of manure—was probably not much more than half of what it would be by the 1930s. And, as Perkins famously argued [1969, 71], the number of hogs, the other main potential source of fertilizer, probably kept pace with population growth or slightly outpaced it.)

To avoid this conclusion, Huang first argues, based on old work of Li Bozhong's, that this additional fertilizer did not raise yields: a view that, as Huang himself notes, Li has moved away from. While Huang portrays this as a suspicious reversal of position without good reason ("nowhere does Li confront the evidence he himself presented earlier" [507 n. 8]), Li does in fact explain this. He notes that his earlier work had relied on the fact that adding "top dressing" fertilizer had relatively little effect in increasing the yield of early rice but had failed to note that this dressing does substantially raise yields of intermediate and late rice—and increased fertilizer use was indeed often linked to the spread of these other rice varieties (1998, 47–48).

Huang then points (again relying on Li) to what appears to be a decrease in the average effectiveness of each pound of fertilizer on rice yields over a period of centuries. This may well have occurred—as it did in many other times and places—though the evidence for Jiangnan is unclear. (Ellis and Wang [1997, 185], for instance, drawing on earlier work by both Dwight Perkins and Chao Kang, make the case for a slow but steady increase in average rice yields in the Lake Tai region over the long haul from the Song to mid-Qing. Meanwhile, Ellis and Wang, themselves soil scientists, find no evidence of the declining fertility that Huang speculates may have come into play [1997, 180].)

The particular argument that Huang makes for this claim is, however, quite dubious. He says that "rice yields in the delta improved little or not at all throughout the Ming and Qing, continuing to hover in the one to three *shi* range" (508). This is of course a huge range and tells us only that some farms in the delta achieved very

high yields at an early date, while it took a long time for other farms to catch up to this level. But this is exactly what was happening in late medieval and early modern Europe as well: most of the gain in average yields per acre came from the majority of farms gradually catching up to the yields that a few farms had already reached at the beginning of the period. (Plots of land have many subtle differences, and before modern chemistry, adapting best practices from one plot to another could be extremely slow, even if we were to assume institutions that maximized the incentives to do so.) In England for instance, Robert Allen informs us that "wheat yields of 20 bushels per acre were common" in northeastern Norfolk during medieval times, though elsewhere in the country yields of ten bushels were more common. Five hundred years later, he continues, "Young's tours in the 1760s, the Board of Agriculture Reports of 1790–1815, and the 1801 crop returns all point to a wheat yield of 20–22 bushels" (Allen 1991, 239). Overton gives a range of results for different English counties in 1801, of which the highest is twenty-four bushels per acre; his national average is also twenty-two bushels per acre for 1801 (1996, 77). The picture, then is much like what Huang describes for Jiangnan, and we could just as accurately say that English yields from 1300 to 1800 hovered around ten to twenty-four bushels per acre, but this would not be very helpful.

Moreover, in this passage Huang has again confused marginal productivity with average productivity. If the average effectiveness of fertilizer did indeed decline over time, that would be an interesting fact, but the question that matters to a peasant making production decisions at a given moment in time is marginal productivity: what will adding more fertilizer do or not do? And here it is quite clear that peasants rightly understood that adding fertilizer increased their yields: otherwise we would be at a loss to understand why they kept adding fertilizer, especially the expensive beancake. In fact, if there were no increases in yields to be expected from the application of beancake, which was fairly expensive, this would only strengthen my case that using this new kind of fertilizer must have had some other benefit, such as labor saving. (Li also notes [1998, 48–49], citing a contemporary observer, that the switch to beancake allowed for an important savings of labor and points out that Thomas Smith has made the same point about beancake in even more densely populated core regions of Tokugawa Japan.) And, of course, nothing Huang says gainsays the point that it was enormously labor saving to use a new fertilizer that weighed so much less than a comparably effective amount of manure. Instead, Huang follows with a comment about the high cost of credit used to purchase fertilizer in the 1930s and 1940s, which is of little relevance to the issue of labor-saving changes in the eighteenth century. His point here may be that labor-saving innovations introduced at any one time tended to eventually be overtaken by long-term changes that increased labor intensity. But as we have seen, that is perfectly normal in the absence of rapid technological change and would at any rate be irrelevant to my basic point that households deciding to purchase beancake were not acting as if they had a near-infinite supply of extremely cheap household labor to spend, as involution would predict.

Huang then turns to criticizing Li Bozhong's estimate of the magnitude of the fertilizer revolution. This is somewhat beside the point, anyway, since we are concerned with whether the fertilizer raised yields and saved labor where it was applied, not with how many acres benefited from those increased yields. Moreover, Huang seems not to notice that even if Li's most aggressive claims were not true, the impact of beancake fertilizer would be quite substantial. For argument's sake, let us assume that Huang is right that the 10,000,000 *shi* figure that he, Li, and I have all taken

from Bao Shichen does indeed refer to common *shi*, rather than the much larger Guandong *shi* to which Li thinks the sources refer, though this is not very likely. (Li's interpretation appears to be quite consistent with Adachi Keiji's estimates of the number of boats involved in this trade, their capacity, the number of trips they made each year [1978], and with other sources [Li 1998, 113–14, 209 n. 35] while the source Huang cites to support his point in fact contradicts it.[57]) Let us further grant that not all the shipments were beans and not all beans were made into beancake: for argument's sake, I will stipulate that only 2,000,000 *shi* of beancake fertilizer resulted from this trade. This would still be 352,000,000 pounds of fertilizer, which would be the nutritional equivalent of roughly 11.5–17.5 billion pounds of properly diluted manure (Pan 1994, 36–37). Buck (1937, 258) estimates that an adult male unit produced 992 pounds of night soil per year. Jiangnan's roughly 31,000,000 people would be equivalent to roughly 24,000,000 adult equivalents (Perkins 1969, 301) and anywhere from 18,000,000 to 21,000,000 adult male units, depending on one's estimate of the difference in food intake between men and women (Pomeranz 2000a, 39 n. 47 for some of the difficulties here); they would have provided about nineteen billion pounds of night soil prior to dilution or perhaps as much as fifty-seven billion pounds after dilution. Thus, even with this rather cautious estimation, beancake from Manchuria (not counting additional imports from Huguang) would have provided about 20–30 percent as much fertilizer as all the people in the region put together. If, as seems more likely, Li is correct that the imports were measured in Guandong *shi*, they would have been equal in nutritional value to 50 to 75 percent of the human nightsoil available in Jiangnan, and probably made some of the latter redundant, accounting for the fact (noted above) that late Ming levels of manure application were already at twentieth-century levels.

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[57]Huang's claim in note 8 that Wu Chengming believes these were common *shi* is puzzling, since on the page cited (Xu and Wu 1985, 273) Wu says nothing at all about this issue. (Huang may be relying on Wu's guess as to total long-distance grain shipments on page 277, where it does seem that he failed to multiply the Manchuria-Jiangnan beancake shipments by 2.5 before creating his overall estimate: but since that is meant as a very rough and conservative approximation and involves combining changing and uncertain figures for different parts of the trade over a hundred years apart from each other, it would be unwise to rely on that omission as a conscious endorsement of any particular interpretation of any particular figure.) Moreover, later in his book, (1985, 669) Wu says that the freight rates for this traffic refer to Guandong *shi.* And on page 362 of the English version (Xu and Wu 2000, 362), Wu makes calculations that are accurate if these were Guandong shi, but would make no sense otherwise, as follows: First Wu says that the largest ships in this trade had a capacity of 3,000 *shi,* without specifying whether these are common or Guandong *shi*. Then he tells us that it was roughly 2.5 Guandong *shi* to the common *shi*, and 20 common *shi* to the ton, "in which case the burden of the sea-going vessels would have been up to 375 tons" (2000, 362). Two sentences later, he suggests an average capacity of 185 tons for these ships, which would clearly be absurd if he was assuming these were common *shi* but arithmetically reasonable if these were Guandong *shi*. (If the largest ships had a capacity of 3,000 common *shi*, even they would have carried only 150 tons.)

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