

New Perspectives on the Chinese Rural Economy, 1885-1935

Daniel Little
Association for Asian Studies
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China's rural economy in the last decades of the nineteenth century and the first 30 years of the twentieth century was extremely poor; it was stagnant or even declining in per capita terms; and it embodied substantial inequalities of land, tenancy and security--or so conventional wisdom would have it. R.H. Tawney's bleak observations in the 1930s set the stage for much work on the economic history of this period in the 1960s and 1970s. Tawney emphasizes extortionate taxation and credit relations, warlordism, minute landholdings, poor soils and population pressure as the chief causes of increasing rural misery in China. Tawney wrote, "There is even some reason to believe that, with the increased pressure on the land caused by the growth of population, the condition of the rural population, in some parts of China, may be actually worse than it was two centuries ago It is difficult to resist the conclusion that a large proportion of Chinese peasants are constantly on the brink of actual destitution" (Tawney, 1966, pp. 71-72). American scholarship in the 1960s and 1970s emphasized the poverty and stagnancy of the Chinese rural economy, thus confirming the broad outlines of Tawney's analysis. However, in the 1980s several important recent works have challenged this conventional wisdom. Thomas Rawski and Loren Brandt argue for a substantial degree of growth in agricultural output, rural incomes and living standards, while Philip Huang argues that the rice economy of the Yangzi Delta was locked in a pattern of "involutionary growth" with little or no improvement in per capita output and living standards.

These disagreements raise a number of issues important for China scholars more broadly: the direction and nature of change in rural welfare during the period, the nature and rate of agricultural development (output, productivity and application of new technologies) and the character and pace of social change during this period (rural to urban migration, land tenure change, concentration of landholdings). If the generally upbeat assessment offered by Rawski and Brandt is sustained, then a rather deep reassessment of the status of welfare and social

change in China's countryside in the early twentieth century will be needed. If Huang's view is validated, then customary assumptions about the logic of economic development in an agrarian economy will need rethinking. The papers in this symposium offer a critical consideration and assessment of these recent contributions.

The papers in this symposium identify important dimensions of economic change in the late Qing and early Republican period (1885-1935) and try to sort out the most credible conclusions that can be drawn. The substantive issues may be summarized along the following lines. First is the question of the status of structural transformation of the Chinese economy in the Republican period. To what extent was the proportion of agriculture to industrial output changing during this period? How much growth of manufacturing and industrial employment was occurring? How extensive was the growth of commercialization of agriculture? How rapidly was modern industry eroding traditional manufacturing? Second, there are issues directly concerned with processes of development within the agricultural sector. Was there significant productivity growth in agriculture during the period? Were there significant processes of technological change under way? Was commercialization stimulating greater efficiency and investment? To what extent did new communication and transportation technologies stimulate change in the rural sector? Third, each of the books under review is forced to arrive at assessments of China's population trends during the period. What is the best estimate of the rate of increase of the rural population? How much urban migration or inter-rural migration was occurring? Finally, there are a host of issues relating to the net effects of these various processes on the welfare of the rural population. What was happening to rents and wages? Was population pressure on resources placing increasing strain on the rural economy? Were rural incomes subject to greater instability? Are there available data that would indicate the direction of change of nutritional adequacy in the rural population? To put it crudely: was the rural population in a state of immiseration during the period? Was it holding its own? Or was there significant, if slow, improvement in rural welfare?

THE RECEIVED VIEW

Many observers have regarded the late Qing and Republican period as one of

agricultural stagnation, stagnant or falling real rural incomes, worsening tenancy relations and increasing rural inequalities. These unfavorable economic developments are often taken as setting the stage for the successful peasant revolution in China: increasing rural misery gave peasants a strong motive to support a party that promised land reform and a program aimed at improving the lot of the rural poor. Dwight Perkins holds that China's rural economy in the early twentieth century was almost stagnant, with little or no per capita growth in gross domestic product. There was growth of output, but it occurred at essentially the rate of population increase--resulting in stagnant per capita incomes (Perkins, 1975a, pp. 121-22). Perkins acknowledges that there was sustained growth in certain modern sectors (e.g. cotton textiles, transport, banking), but reminds us that agriculture and traditional manufacturing dwarfed the modern sector; and he argues that these sectors showed little or no growth (Perkins, 1975a, pp. 120-25). The benefits of modern-sector growth would only be realized in living standard improvement in later decades Perkins' rhetorical foe is the position that held that living standards were falling during this period (represented by Tawney); against this position, Perkins maintains that the balance of evidence suggests that this was not the case: "the view that the incomes of all or of the vast majority of the people were declining during the first half of the twentieth century is not supported by currently available evidence" (Perkins, 1975a, p. 124). Perkins also makes an effort to assess the direction of change in land concentration, tenancy and income distribution during the period. He holds that tenancy rates remained approximately the same during the period, and he denies that there was an abrupt increase in tenancy or landlessness during the early twentieth century (Perkins, 1969, p. 100).

Another important statement of the received view of the 1960s is Albert Feuerwerker's *The Chinese Economy, Ca. 1870-1911* (1969). Feuerwerker's précis too emphasizes economic stagnation: "Fundamental economic change and modern economic growth, however, in so far as they have been accomplished in twentieth-century China, did not come of their own momentum out of the late-Ch'ing economic system. They were preeminently the by-products of a new and possibly still tenuous political integration which itself was achieved only after decades of political strife, foreign invasion and civil war" (Feuerwerker, 1969, p. 1). Feuerwerker maintains that agricultural techniques remained roughly unchanged throughout the period (1880-1930s), with output increasing in pace

with population growth through small increase in cultivated acreage (Feuerwerker, 1969, p. 3). He takes it as certain that rural living standards did not improve throughout the period, but doubts that evidence exists to demonstrate a significant decline in living standards (p. 5). Feuerwerker believes that tenancy rates probably did not increase in the early decades of the twentieth century, and he doubts that effective rent levels increased during the period (p. 14). He thus adopts roughly the same view as Perkins: that output approximately kept pace with population increase, with the result that average rural welfare remained about constant.

Scholarship in the 1970s focused more attention on distributive issues in the rural economy: the status of tenancy, landlessness, wage labor, peasant welfare and rural inequalities. Such authors as Mark Selden, Victor Lippit, Carl Riskin and Joseph Esherick argued that inequalities increased during the period. Mark Selden emphasizes the deterioration of living conditions in Shensi. He details the destructive effects of warlordism and famine in Shensi, and he argues that tenancy in Shensi increased substantially in the 1930s, accompanied by increasing landlessness (Selden, 1971, pp. 7-8). These worsening conditions are a central causal factor in Selden's analysis of the successes of Communist mobilization in Shensi. Likewise, Carl Riskin emphasizes the significance of income and land inequalities in the Chinese rural economy (Riskin, 1987, pp. 24-26). And Victor Lippit focuses attention on the disposition of the rural surplus: through rent, taxation and usurious interest rates the peasant was separated from the surplus available within the rural economy (Lippit, 1974). He argues that incomes based on these sources represented a significant portion of China's national income in the 1930s: rent (10.7 percent), farm business profits (3.4 percent) and rural interest payments (2.8 percent), for a total of 16.9 percent. Moreover, Lippit argues that, for reasons internal to China's rural elites, these incomes were not devoted to productive investment but elite consumption.

In short, the received view represents the early twentiethcentury Chinese rural economy as largely stagnant. Technological change in agriculture was sparse. Living standards for peasants were stagnant or falling. The main fissure of disagreement within the field concerned the causes of the stagnation. One school of thought (the *technological* school) held that the chief obstacles to development were technological and demographic; population pressure on

resources led to an economy in which there was very little economic surplus available for productive investment. The other theory was the *distributional* school, which held that the traditional Chinese economy generated substantial surpluses that could have funded economic development, but that the elite classes used those surpluses in unproductive ways.

REVISION

Brandt, Rawski and Huang disagree about a great deal; but they agree in rejecting many features of the received view. Consider first some of Thomas Rawski's central findings. Rawski argues that economic growth was significant and sustained in pre-war China. It was driven by modernization of transport, factory industry and commercial banking (Rawski, 1989, p. xx). Much of Rawski's book focuses on industrial growth, but he maintains that agriculture expanded in per capita terms as well. He estimates that agricultural growth averaged 1.5 percent--about .5 percent ahead of population growth. This process of growth led to sustained increase in output and income per capita (Rawski, 1989, p. 268), and this increase led to rising living standards. Rawski provides a new analysis of Buck's data on rural living standards, to support the conclusion that rural welfare was rising during the pre-war period (Rawski, 1989, p. 287 ff.). He argues that there is good evidence of rising consumption of cotton cloth, which he takes to support the conclusion that living standards were rising (Rawski, 1989, p. 289). Rawski summarizes his findings relevant to the rural economy in these terms: "This study has produced a variety of direct and indirect evidence of increasing per capita output, income and living standards in large areas of rural China prior to the outbreak of the Pacific War in 1937" (Rawski, 1989, p. 320).

Turn now to Loren Brandt's analysis. Brandt maintains that commercialization progresses rapidly during this period, bringing greater integration between domestic and international markets in rice, cotton and other important commodities; and that commercialization in turn induced growth in agricultural output, improvement in the agricultural terms of trade, rising real incomes for farmers and laborers alike and a probable overall reduction in the range of income inequalities in the countryside of central and eastern China. Brandt argues that real farm wages were rising during the period; that farm

wages were closely linked to other forms of employment; and that it is reasonable to conclude on the basis of these points that rural welfare was rising during the period. The data that Brandt employs here takes the form of scattered cross-sectional studies of wages for seasonal and long-term agricultural laborers. Brandt uses these conclusions about real wages to make inferences about productivity in agriculture. He reasons along neoclassical lines: the wage is determined by the marginal product of labor; if wages are rising, we can infer that the marginal product is rising, from which Brandt infers in turn that the average product (a measure of productivity) was rising as well. And in a competitive labor market with few barriers between types of employment, the level of the farm wage ought to be closely correlated with the returns to other forms of labor--with the result that we can conclude that other forms of rural income were rising as well. On the basis of this line of reasoning, Brandt estimates that labor productivity increased between 40 and 60 percent during the time period (Brandt, 1989, p. 132)--suggesting that the rural economy was improving rather respectably during the period.

Brandt's interpretation of the distributive performance of the commercializing Chinese economy likewise challenges the received view. He argues that commercialization of the rural economy had the effect of significantly narrowing income inequalities in rural China (Brandt, 1989, p. 138), by increasing the demand and opportunities for labor. And he denies the common view that land concentration was increasing during this period. He maintains that the relative share of income flowing to the bottom of the income distribution (tenant farmers, small owner-farmers, landless workers, peddlers, handicraft workers) improved during this period relative to landlords (Brandt, 1989, pp. 169-170). He doubts the common belief that land holdings became more stratified during this period, and he believes that the terms of tenancy had improved for the tenant by the 1930s, reducing the effective rent from about 50 percent of output to about 40 percent (Brandt, 1989, table 6.20, p. 171)--thus improving tenant incomes at the expense of landlords. And he holds that the increasing opportunities for sideline activities (textiles, refining oils, sericulture, etc.) primarily benefited the poorest strata.

Philip Huang offers a strikingly different assessment of the development of China's rural economy. Huang's book covers a very long time horizon; he treats the Yangzi rural economy over a 600-year period, leading through the post-Mao

reforms. He maintains that the Chinese Yangzi delta economy was characterized by a system of subsistence-level farming based on peasant family production; “only in the 1980s did transformative development begin to come to the delta countryside, to result in substantial margins above subsistence in peasant incomes” (Huang, 1990, 1). Huang holds that this rural economy was heavily involuted, organized around self-exploiting family production. The stimulus of population increase led to intensive rather than productivity-enhancing growth, and the results were stagnant levels of welfare for the rural population. The farm family system drove out hired labor managerial farming because of low opportunity cost of family labor (14). Thus in Huang’s view the farm economy was characterized by “growth without development” (11). It was highly involuted due to population pressure and did not show significant growth in productivity through this whole period. Agricultural output expanded just enough to keep pace with population increase, largely through intensification of production. “There was little or no expansion [after the southern Song and early Ming] until the introduction of modern inputs after 1950” (Huang, 1990, 14). Finally, Huang rejects Rawski’s and Brandt’s arguments that living standards were rising appreciably around the turn of the twentieth century (137-143).

METHODOLOGICAL PROBLEMS

There is a very wide range of disagreement across these several schools of thought on the rural economy of Republican China. Moreover, these disagreements matter a great deal to our understanding of China’s history in the early twentieth century. To what extent is it possible to resolve these issues? What problems stand in the way of our reaching relatively definitive conclusions on these central economic issues? How much knowledge is it possible to arrive at concerning the main economic characteristics of the Republican economy?

In a schematic way it is clear that an economic history of rural China should address the following issues:

Demography. What was the absolute population size and distribution at various time points during the period? What were the trends of population growth during the period? How much urbanization occurred during the period?

Inputs and technology. How much land was under cultivation? What crops and products were in production? How much irrigation was available, and what was the trend of extension of land and irrigation? What fertilizer technologies were in use?

Property relations and control of labor. What forms of tenancy and land ownership were in place? How were these arrangements changing during the time period? What forms of wage labor were in use? Was there a tendency of change in wage labor?

Productivity. What was the absolute size of the production of central commodities--rice, wheat, cotton? What were the factor productivities? What trends existed?

Human welfare. What were the income levels and food security of various groups: landless workers, smallholding peasants, tenants and other groups? How extensive were income inequalities within the economy? Where were economic surpluses going? What was the trend of real welfare and inequalities?

It is evident that an economic history demands temporal differentiation. Ideally we would want to have time series estimates for each of these groups of variables. But we also need to have spatial differentiation of these variables, in order to capture the crucially important regional variations that are present in economic development in a large economy like China's. As G. William Skinner's work has so convincingly shown, we need to have these data broken down in spatial terms: regions differ across China, and data that is averaged over large regions give misleading impressions of the processes of development that were underway (Skinner, 1964-65, 1977a, 1985). In principle, then, we can picture the empirical core of an economic history as a three-dimensional data table representing a set of variables as they change over time and place.

Most of these variables demand quantitative treatment. Therefore it is necessary to canvas available data sources to arrive at estimates of such variables as population size, tenancy rates or volume of commerce. But the

economic historian is also interested in causal relations among various sets of factors. How did population pressure affect technological change? How did tenancy relations affect investment (and hence productivity)? How did tenancy relations affect average rural welfare? How did rural welfare levels affect population trends?

What problems stand in the way of providing an objective and authoritative economic history of rural China along these lines? It is evident, to start, that estimates of some of these variables depend on estimates of others. For example, if we have good data on population size and trends, income distribution and absolute levels of output, then we can construct credible estimates of real welfare. If we have good estimates of real wages then (making neo-classical economic assumptions) we can estimate labor productivity. If we have good estimates of the absolute amount of grain consumption and if we make assumptions about the direction and rate of change of consumption then we can make an estimate of demographic variables. And so on for various sets of these variables.

But it is glaringly obvious that there are very considerable problems of data in this debate. This is a problem that confronts all research in economic history--witness the "standard of living" debate in English economic history of the 1960s (usefully analyzed in retrospect in Crafts 1985). But the problems are particularly severe in the case of China. Ideally we want estimates of a variety of common economic variables: population size over time, urbanization rate, amount of land under cultivation, average crop yields, prevalence of various agricultural techniques and inputs, level of output of various export goods (cotton, silk), amount of commercialization, amount of transport of commercial goods, prevalence of landlessness, tenancy rates and rent levels, wages for various kinds of employment, productivity in various sectors of the economy. But these data are hard to come by; in practice there are only a few data sources which scholars have relied on to tease out into estimates of economic trends.

Consider some of the main data sources on which Chinese economic history depends. Population and production data are crucial. In some cases the imperial state itself collected relevant data (generally for tax purposes), and these sources are used extensively by all commentators. But (as Joseph Esherick shows convincingly; Esherick 1981) these data can be severely skewed as a result of strategic under- or over-reporting of data. Moreover, as Skinner

shows in his analysis of Sichuan's population data, there are often internal inconsistencies that cast very profound doubt on the validity of official population and production data (Skinner, 1987). (For a comprehensive review of the state of Chinese demography see Lavelly et al, 1990.)

A second major source of data, employed by almost all the authors surveyed here, are the agricultural surveys undertaken by John Lossing Buck and his research teams in the 1930s. The Buck surveys collected a wide range of information: land usage, wage rates, tenancy rates, cropping patterns and so on. Here again, however, there are substantial problems of bias and completeness of coverage; Buck's surveys of land concentration, for example, excluded absentee landlords, thus underestimating land concentration (Esherick 1981).

A third data source that has been extensively employed by several authors under consideration here (including especially Philip Huang and Ramon Myers) are the Mantetsu surveys performed in North China by the Japanese South Manchurian Railway Company in the 1930s. These surveys offer village and household-level surveys of economic activity in North China. They provide very extensive detail on the organization of the local economy in a variety of villages. Their limitations, however, are severe as well: only a relative handful of villages are studied (raising questions about the representativeness of the data); and the surveys are done at the behest of a conquering army--raising a different set of questions about bias of investigation and response. (Joshua Fogel [1987] provides a useful description and assessment of the Mantetsu surveys.)

The methodological problem, then, is this: for virtually none of these groups of variables are the data sources fully adequate and fully credible. Instead, economic historians are unavoidably forced to combine sketchy and problematic data, assumptions about trends they cannot directly observe and assumptions about the economic system that derive from economic theory (e.g. the assumption that the real wage of unskilled labor is equal to the marginal product of unskilled labor), in order to arrive at estimates of other economic variables. The point to be emphasized here is not that the resulting estimates cannot be taken seriously. It is rather that the reader needs to follow the economic historian's reasoning very carefully, in order to arrive at an assessment of the overall credibility of a given claim (e.g. the claim that real

welfare for the rural population was probably rising on average during the early decades of the twentieth century).

There are other epistemological problems that confront economic historians besides paucity and unreliability of data. An issue that divides much of the work in this field is the question of the relative importance of qualitative and quantitative data. Rawski and Brandt make an appeal for investigation solely on the basis of available quantitative data--grain prices, maritime records detailing quantities of grain transported, official population data. Philip Huang, on the other hand, as an historian pays substantial attention to a variety of non-quantitative sources: criminal case archives, the Mantetsu studies, the observations of rural welfare by interested observers. Does one data source trump the other, as Rawski and Brandt maintain; or is it mandatory for the historian to cross-check the inferential conclusions his or her quantitative analysis produces on the basis of qualitative data?

A related issue concerns the need for sociological detail in economic history. As Kathleen Hartford notes in her article below, there is disagreement among contributors in this debate about the relative autonomy of economic processes and economic rationality. Rawski and Brandt essentially assume that political and social institutions matter little to economic change. Huang believes, and Hartford emphasizes, that the specifics of institutions and political culture have a great deal of effect on the results: whereas commercialization in the institutional context of 18th century English agriculture led to managerial agriculture, within the context of the cultural situation of the lower Yangzi it led to involution (according to Huang)~ And so we are faced with another important methodological question for the economic historian: how much institutional detail and sociological or even anthropological detail is needed in order to account for processes of economic development in China?

Finally, we must question the validity of the theoretical assumptions made by various authors. Quantitative analysis requires economic theory, on a more or less grand scale; in order to arrive at an estimate of labor productivity it is necessary to employ a set of assumptions that permit us to make inferences from a given set of data about population size, land acreage and output to conclusions about labor productivity. But what assumptions should we use? Rawski and Brandt are neoclassical economists. Their analysis depends on the equilibrium conditions of competitive markets. How well or poorly, however, do such

assumptions fit the circumstances of the Chinese rural economy? Huang embraces no single economic theory; but he comes closest to a neo-Marxist theory, emphasizing surplus extraction and power rather than freely competitive markets. But we can ask of Huang as well: how well do these modeling assumptions fit the terrain of late-Qing China?

IMPORT FOR CHINA STUDIES

Why is this debate important for China scholars outside of the precincts of economic history? There are several important reasons. First, it has seemed important to many China historians to arrive at judgement about China's potential for autonomous economic development independent of western intervention. Were there economic institutions and processes at work within China's domestic economy in the late Qing that might, in other circumstances, have led to a process of modernization and change? Or was China caught hopelessly in a high level equilibrium trap, from which it could be liberated only through some exogenous shock (Elvin, 1973)? Much of the import of Rawski's book is the conclusion that there were powerful processes of modernization and growth already at work in China in the 1880s. This conclusion supports a counterfactual historical judgement: if China's domestic and international circumstances had been somewhat different; if the Qing had survived in a reformist mode, or if the Republican revolution had installed an effective national government; if China had not been invaded by Japan; if China had not been drawn into civil war and the warlord era--then China might well have developed into a modernizing market economy. This conclusion is sympathetic to those who offer a "China-centered" approach to the study of China (e.g. Cohen, 1984).

A second reason this debate should be of interest to China historians more generally has to do with the causes of the Communist revolution. Our construal of the Chinese Communist Party's successes in rural mobilization and ultimate seizure of power depends a great deal on our assumptions about the material welfare of the rural population. If things were bad and getting worse, then mobilization is easy to understand. If the economy was generally improving and if the results of improvement were being experienced as a generally rising standard of living, then we cannot cite immiseration as a cause of the revolution.

And if (as Rawski and Brandt believe) the processes of commercialization and the extension of ever-more-efficient markets were undercutting the forms of pre-capitalist exploitation that existed in rural China (extortionate rents, bonded labor relations), then we cannot explain the success of mobilization as the consequence of the Chinese peasantry's willingness to challenge an exploitative and worsening social order. If, on the other hand, this benign view of the neoclassical school is unpersuasive, then the immiseration and worsening inequalities interpretation remains salient.

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