Chinese History In Economic Perspective

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Introduction: Chinese History in Economic Perspective

Thomas G. Rawski and Lillian M. Li

Economics and economists tend to bring out strong emotions both in the general public and among (noneconomist) scholars. How often does one encounter the sentiment, “If economists are so smart, how come they couldn’t predict such-and-such [the latest round of inflation, the October ’87 stock market crash, etc.]?” Economics has always been a controversial field of study, and economists often exhibit a strong professional affinity for contentiousness among themselves. Yet, while society might conceivably get along without economists, it would be difficult to imagine a world in which economics did not play a role, even the mythical world of Robinson Crusoe. Nor can historians avoid the economic aspects of history even when they would like to do so. Embedded in all their common notions of how history has developed are views, conscious or unconscious, of economic forces: the prosperity of the Italian city-states prompted the cultural efflorescence of the Renaissance, the Chinese had a rural revolution because the peasants were so poor, Europeans conducted oceanic explorations because they needed spices, and so forth. But fundamentally, historians need to know about the material side of history because they are concerned with human welfare, social development, and national histories. The classic definition of economics, after all, is that it studies the allocation of scarce resources among alternative uses. Therefore, subjects such as agriculture, money, industry, and trade compel historians’ interest for a variety of commendable reasons.

It is our contention, however, that the study of such subjects in economic history has not always employed a true economic approach or perspective, at least among historians of China. This book is dedicated to the idea that the history of China’s economy has been written many times in many ways but that the economic history of China has not yet been written. This, indeed, is not such a history either, but the essays in this volume are intended to illus-
trate how economic history is not the same as the history of an economy, and how an economic perspective involves more than an interest in some economic topic. Scholarship on China has excelled in studying the economy of China, but has barely begun to do so with a true economic perspective. The fundamental objective of this volume is to delineate and illustrate the potential contribution of systematically applying an economic approach to the study of China’s economic history.

STATE OF THE FIELD

Traditional Chinese scholarship did not neglect economic topics. Indeed, in the standard dynastic histories, sections on population, land taxes, and money, for example, assumed a prominent position. Local histories also treated these topics, as well as listing or describing local products, grain storage, and the like. A well-functioning economy was the hallmark of a successful dynastic regime, a visible sign of the harmony of heaven, earth, and man. Economics and morality were linked; a prosperous economy was a sign of the essential morality of the ruler. The model of the economy, like that of society, was based on the notions of harmony and stability, and not on the desirability of growth and change. The golden age of the past was one in which men plowed the fields and women wove cloth. Wars and famines signified the disruption of stability. The goal was to restore the status quo ante, the golden age, not to surpass it, because it could not be surpassed.

In recent decades, a different paradigm, that of Chinese Marxism, has dominated Chinese scholarship. The three broad areas that receive the most attention from historians in the People’s Republic of China are land tenure, foreign imperialism, and the “sprouts of capitalism.” In the post-Mao era, the “Asiatic mode of production” was added to this list. Studies of land tenure are closely linked to issues of servitude and subordination among China’s peasantry in each period of history. Studies of foreign imperialism stress the plundering of China’s economic resources by Western powers and Japan in the nineteenth and twentieth centuries, and the obstacles to the development of a modern economy posed by the unequal treaties. Studies of the “sprouts of capitalism” focus on the signs of development in China’s late imperial, or early modern, economy (roughly since the mid-sixteenth century), such as the expansion of handicraft production and the freeing of labor in the countryside, but the line of interpretation has shifted from time to time—sometimes emphasizing the sprouts themselves and, at other times, the smothering of the sprouts. The revival of interest in Marx’s idea of the Asiatic mode of production highlighted the dilemma of Chinese Marxist historians: how to fit Chinese history into the scheme of world history. Previously discredited by party historians because it tended to suggest that Chinese development did not fit into a unilinear world pattern, the Asiatic mode attracted renewed attention in the 1980s in part because it helped
legitimize China’s recent economic policies, which may seem to transgress the stages of history normally posited in the Marxist scheme of history.

How one evaluates the Marxist scholarship on China is to a certain extent a function of one’s ideological persuasion. Certainly the Marxist framework provides a compelling agenda for research. Critics think, and sometimes dare to say, that the agenda is limited and that the questions posed to some extent determine the outcome. But this criticism could be leveled at any paradigm or framework. What is striking to us, however, is the extent to which a materialist or economic interpretation of history has essentially transformed itself into social history. It is the struggle between social forces and the conflict of social classes that seem to determine the economic stage of history rather than the economic forces that determine the social. Marxist historiography has stood Marxism on its head.

Substituting modernization theory for Confucian or Marxist theory, the postwar generation of Western historians has also sought reasons for China’s economic backwardness in modern times. American scholarship in the 1950s and 1960s tended to focus on treaty-port developments and the introduction of Western trade and technology into China, implying that contact with the West, even on unfavorable terms, offered an opportunity for positive change that was missed. A second wave of scholarship has focused on the role of entrepreneurship and bureaucratic leadership (or the lack of it) in the nineteenth and twentieth centuries, finding in them a major reason for China’s “failure to modernize” along Western lines, even when exposed to Western influence. In a similar vein, scholarship in Taiwan has emphasized the institutional and bureaucratic aspects of China’s economic development in the last two centuries.

In an innovative and influential interpretive history, Mark Elvin tried to break away from the yoke of Western periodization schemes to show that China’s history followed a different “pattern,” in which a medieval economic revolution led to a “high-level equilibrium trap” that did not prevent further growth, but did impede significant change—“economic development without technological change.” Yet like other Western scholars, and indeed like the Chinese scholars, his underlying preoccupation is with explaining China’s poor economic performance in modern times.

Like Elvin, recent Western scholarship has tended to search back beyond

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the troubled modern period to find strengths and weaknesses in the Chinese economy before the nineteenth century that might help to explain its behavior after the Opium War. These studies have focused on the role of the traditional Chinese state in shaping the economy, particularly in the eighteenth century. Building on Ping-ti Ho’s work on China’s population,¹ these studies have, on the one hand, emphasized the positive role of the state in encouraging the settlement of undeveloped and frontier areas⁵ and in maintaining granary stocks to stabilize prices and prevent famines⁶ while, on the other hand, stressing the essential limitations of state power. Yeh-chien Wang’s work on Qing land tax, Madeleine Zelin’s work on tax surcharges, and Susan Mann’s work on the merchants’ role in collecting commercial taxes all tend to show how the Qing and Republican governments were unable, and sometimes unwilling, to capture a larger share of the country’s wealth for their own purposes.⁷

Some American scholarship, as well as some Japanese scholarship, has shared the Chinese interest in the primacy of social forces in governing economic history. For example, standing on different sides of an ideological divide, Ramon H. Myers and Philip C. C. Huang have disagreed sharply on the extent to which the land tenure system in North China produced social inequalities.⁸ The work of William T. Rowe and others on the growth of Chinese cities tends to emphasize the strength of commercial developments that took place largely outside the sphere of direct government influence.⁹ And G. William Skinner’s influential work on marketing and his macro-regions paradigm both stress the essential independence of economic activity from political trends as embodied in the dynastic cycle.¹⁰

Although there are notable exceptions not captured in this broad summary, it is striking how American scholarship on Chinese economic history,

somewhat like PRC scholarship, has really revolved around social and institutional history. In fact, the bulk of the work concerning the Chinese economy has been done, not by those trained in economics, but rather by social historians, anthropologists, and others. Most of these scholars—including some of the contributors to this volume—have not in the past made regular and systematic use of economic analysis to inform and structure their inquiries. In part this may be due to ideological or disciplinary predisposition, and in part it may reflect the types of sources available for the study of economic history. Traditional official records are strong on bureaucratic institutions and practices but weak in quantitative material. Even so, the tendency for researchers to neglect economic approaches in writing the history of China's economy may reflect their limited appreciation of how the economic perspective can sharpen an analysis of the historical record.

In the 1960s similar criticisms were raised by a group of "new economic historians" against the work of the earlier generation of economic historians in the West. Feeling that the traditional economic histories of Europe and the United States overemphasized the description of legal and other institutions, the new generation advocated the application of economic theory and quantitative methods to historical scholarship. With the advent of Robert Fogel and Stanley Engerman's study of slavery in the American South, and the ensuing controversies, the Cliometric revolution reached its heyday and, some have said, began to peak. Nonetheless, a more quantitative and analytic approach continues to prevail in the leading journals of economic history.

Our goal is not to champion the introduction of Cliometrics into Chinese economic history but rather to advocate adopting a more self-conscious economic perspective that may or may not involve quantitative analysis. Our belief is that the use of economic theory can illuminate issues that might otherwise prove inaccessible. In addition, the contributors to this volume have reached the surprising conclusion that applying economic analysis to historical topics often enlarges the interpretive significance of phenomena that historians, and not economists, are best qualified to comprehend.

ECONOMIC THEORY

What do we mean by an economic perspective? We mean the application of economic theory and methods to the study of historical topics.

Classical economic theory, as developed in the West, rests on a number of key concepts, which some call principles and others may call assumptions. The most fundamental of these is the concept of choice. Donald N. McCloskey

defines economics as "the study of human choice under constraints."¹² Income and wealth, the conventional measures of economic well-being, define the extent of choice available to consumers. In most economies, choice is exercised primarily in markets, which offer opportunities to sell commodities and human skills in return for income, which can be translated, again through the marketplace, into consumption goods. Prices signal the rates at which any individual's resources of money, time, and skill can be converted into desired commodities or services. For the economist, prices demand attention because they offer precise measures of both choice and constraint that (important for the historian) are often recorded in great detail. Markets and prices thus emerge from the centrality of choice as natural focal points for historical inquiry.

Rationality is a closely related concept. Rationality means that people are motivated by self-interest, primarily pecuniary. Economic rationality means that individuals, families, and organizations have well-defined ideas about how various opportunities affect their well-being and that choice rests upon comparison of the cost of available alternatives. Economic rationality suggests that people know how to calculate costs and benefits and that they are free to act according to their choices.

The centrality of choice in economics leads to the concept of opportunity cost, which defines the cost of a specific action in terms of the value of alternative options rather than actual monetary outlay. Or, in McCloskey's words, "choosing one thing means giving up another, because things are scarce, constrained."¹³ In the economists' view, the cost of education, for example, includes the value of income-earning opportunities forsaken by the student as well as the actual tuition she or he pays. The opportunity cost of moving to a new location must comprehend the value of wages lost while on the road as well as transportation costs. Opportunity cost is quite literally the value of "the road not taken."

Much of economic analysis revolves around the concept of equilibrium, which portrays economic circumstance as the outcome of a balance of conflicting forces. Market price is determined through bidding, a process of organized struggle between buyers, who seek to force the price to the lowest possible level, and sellers, whose interest is served by attaining the highest possible price. Market forces ceaselessly push price and quantity in the direction of equilibrium. If demand exceeds supply at the current price, anxious buyers will bid up the price, simultaneously curbing demand and attracting additional supplies. If price is so high that supply exceeds demand, sellers' prices will be bid down, leading toward the balance between desired purchases and sales that characterizes an equilibrium position.

¹³ Ibid., 1.
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Together with the idea of *entry and exit*, which simply maintains that productive resources, including human labor, will abandon occupations offering low rewards and gravitate toward the areas of greatest opportunity, the economists' equilibrium notion offers a valuable tool for historical researchers. Even though the interaction of supply and demand in particular markets may not leave clear tracks in the historical record, the qualitative consequences of changes in equilibrium positions often generate shifts in the direction of resource flows that will not escape the historian's notice. As D. K. Lieu has observed, businessmen in China (and elsewhere) "are ready to clear out at any time" if they see better prospects in another trade. The appearance of new businesses and the abandonment of old trades thus become a sensitive barometer of relative profitability in different lines of endeavor. * Similarly, if large numbers of workers migrate from North China to Manchuria, or from the rust belt to California, no statistical analysis is required to verify the existence of regional differences in economic opportunity.

OBJECTIONS TO ECONOMIC THEORY

When thus presented as a series of abstract concepts, economic theory often provokes the deepest skepticism, if not outright hostility, among noneconomists.

Some have charged that these ideas of neoclassical Western economics are not universal principles or absolute truths but are, instead, a series of assumptions that are largely a matter of perspective or even faith, not susceptible to proof or argument. Moreover, these ideas are culturally and historically specific, a product of a particular phase of Western history, and are not universally applicable. Some, like Karl Polanyi, have argued that these ideas themselves have shaped people's behavior and the development of economic institutions, especially markets, that they have been, in short, not descriptive but prescriptive. *

Others object to economic theory because they believe it to rest on a view of human nature that is self-fulfilling, possibly erroneous, and certainly repugnant. "Rational economic man as a reflection of human nature is a fiction. . . . But it is a powerful fiction, and it becomes less and less a fiction as more and more of our institutions get pervaded by its assumptions and other paths are closed," writes one recent critic. Adam Smith's notion that individuals pursuing their own self-interest are "led by an invisible hand" toward improving the society and economy in which they live is difficult to reconcile with more flattering views of human nature and human good.

There are those who believe that the classical economists’ view of human nature is not only incorrect but that it can be replaced by a superior form of morality. Amitai Etzioni, for example, argues for the replacement of utilitarianism with ethical principles that stress intention, not result, for the replacement of individual calculation with collective rationality, and for the replacement of economic rationality with values and emotions.\textsuperscript{17}

Most others who object to economic theory do so on the grounds that it is empirically invalid. They say that simple observation will reveal that not everyone is motivated by monetary self-interest above all other considerations and that the notion of economic rationality must therefore be false. The economists reply that economic rationality need not imply ceaseless calculation of cost and benefit by households and businesses, nor must economic decisions rest exclusively on financial considerations. Although economists often construct theories on the assumption that individuals and business firms pursue maximum financial rewards, the notion of rationality encompasses the possibility that a desire for prestige or perhaps stability, as well as monetary gain, may motivate economic behavior. The recent debate about the “moral economy of the peasant” highlights this controversy, with James C. Scott arguing that in peasant societies the dominant motive is survival and security, so that risk minimization, not profit maximization, is the principal goal.\textsuperscript{18} Economists respond that peasant rationality is essentially no different from anyone else’s rationality and that avoidance of risk is not inconsistent with rational calculation.

Critics also protest that rational choice implies perfect information and intelligence. But what if someone does not have all the information needed, or what if he or she is stupid or, worse still, lazy? I could increase my financial resources if I thought about my investments all the time, but I do not choose to use my time that way. The opportunity cost, measured in work or recreational time lost, is simply too high. But economists reply that decisions based on limited information and crude calculations may in fact reflect rational behavior. After all, the time and expense required to collect further information or to conduct detailed studies of opportunity costs may outweigh the anticipated benefits of prolonged search and analysis.

Finally, skeptics reject the idea that people actually have a choice in economic matters and are free to enter into or exit from economic activities as some kind of economists’ wonderland, full of Mad Hatters. Surely, in real life people are not always free to change jobs, change residences, or change investments according to the dictates of rational calculation.\textsuperscript{19} Custom, law,

\begin{itemize}
\item Amitai Etzioni, \textit{The Moral Dimension: Toward a New Economics} (New York, 1988).
\item The Nobel Prize–winning economist George J. Stigler tells the story of an economist who carefully decided how far from the city to locate his country home by efficiently balancing the
\end{itemize}
social practice and prejudice, inertia, and any number of restrictions on behavior exist today and were even more decisive in premodern times.

Economists, however, recognize that market activity and price formation do not occur in a social or cultural vacuum. They see the institutional arrangements that circumscribe and encapsulate economic activity—the household, legal structures, customary market procedures, forms of contract arrangement, business organization, even ideology and morality—as constraining economic activity along with limitations on the stock of physical and financial resources. But economists regard institutions as flexible rather than immutable. If costs exceed benefits, economists anticipate change (perhaps gradual) in the relations between individuals and social institutions, as well as between buyers and sellers. The post–World War II increase in female employment in the United States represents such an event, with the unorganized response of millions of women to altered labor market conditions leading to changes in marriage practices, family size, child rearing, educational patterns, eating habits, and many other aspects of life long regarded as determined by custom and tradition rather than the marketplace.

The clash between economists and noneconomists is perhaps best embodied in the economists’ favorite term, *ceteris paribus* (literally, all other things being equal). While economists will acknowledge the importance of noneconomic factors, those bothersome factors are generally left in the background of their theories and models. Let others study politics, law, social class, injustice, and the like. Models can be pure and “elegant,” a favorite expression of economists, because all those other factors can be held constant or set aside. And since such factors are not easily quantifiable, how much more convenient to leave them out. Quantification of the nonquantitative is best left to the “soft” social scientists—the sociologists, the political scientists, and the historians.

It is *ceteris paribus* that allows economists to be optimists. Although economics is called the dismal science, in fact economists tend to maintain a rosy view of the world controlled by an invisible hand. If only the government and others would stay out of it, the rational response to opportunity could produce growth and a better life for everyone. In the field of Chinese studies, the number of fresh eggs he could get against the number of friends who would still be willing to visit him. In his review of Stigler’s memoirs, Robert Krulwich dryly comments, “Here, I say, is why more and more people ignore economists.” *New York Times Book Review*, Oct. 23, 1988.


21. In all fairness, it must be said that economists tend to recognize their professional weaknesses and know how to laugh at them. Evidence for this can be found in the rich store of economist jokes that end with the punch line, “Assume...”
optimism of the economists stands in marked contrast to the gloomy prognostications of the political scientists. The Chinese economic reforms of the 1980s inspired great hope among most economists, who tended to see the possibility for continued growth and change, while political scientists warned of bureaucratic competition, political backlash, social discontent, and other dangers, which they said might thwart the reforms.

THEORETICAL REASONING

The tendency of many economists to sweep noneconomic factors into the dustbin of ceteris paribus is indeed regrettable. Recently, however, a few economic theorists themselves have begun to question the basic assumptions of the approaches that have dominated their field. The study of macroeconomics has been described as "a religious battlefield," where the most fundamental beliefs are being challenged. George A. Akerlof, who has contributed to this battle, has said:

The unwritten rules that only economic phenomena be considered in economic models, with agents as individualistic, selfish maximizers, restrict the range of economic theory and in some cases even cause the economics profession to appear peculiarly absurd—because, without relaxation of these rules, certain almost indisputable economic facts, such as the existence of involuntary unemployment, become inconsistent with economic theory. . . . Individualistic maximizing behavior constitutes an assumption that sharply restricts the domain of possible economic models. It is an assumption that turns out to be surprisingly restrictive.

While recognizing the importance of noneconomic factors in governing economic behavior, a theorist such as Akerlof is nevertheless concerned primarily with perfecting an economic model, albeit one that he considers reasonably consistent with reality. For some economic theorists, it might be said, the model is the reality. Many economists tend to value work that contributes to the building of economic theory and to dismiss the study of real data as mere "empirical work." Economic historians, however, have argued for the importance of economic history to the development of theory. It is our contention that just as economists need to test their theories against historical reality, historians can and should enrich their work through the use of economic theory, as well as economic methods.

Economic theory can serve several purposes for historians. At a practical

22. An insight attributed to Mark Kuperberg of the Economics Department, Swarthmore College, whom we also thank for the reference to Akerlof’s work (see n. 23).


24. The contributions that historical studies can make to economic theory are outlined in essays in William M. Parker, ed., Economic History and the Modern Economist (Oxford and New York, 1986).
level, some knowledge of economic theory can provide essential context for interpreting evidence that would otherwise be misunderstood. Upon learning of the small share of imported grain (and more generally, of foreign trade) in the economic life of late Qing China, the historian (and even the economist) naturally assumes that foreign trade must have played a small role in China's economy, especially in the interior. But this assumption overlooks the economists' "marginal principle," which teaches that market prices are determined by the behavior of "marginal" buyers and sellers, who are on the brink of indifference between patronizing the local market or doing business elsewhere. If the demand for and supply of a particular commodity is "inelastic," meaning the amount people will purchase or sell is relatively inflexible in the face of changes in market price (as in the case of heating oil, milk, or insulin), then small changes in quantity may lead to relatively large changes in the price. Alternatively, if the demand for a commodity is elastic, small changes in price may lead to relatively large changes in the quantities people desire to buy or sell. Thus shifts in the behavior of marginal buyers or sellers can generate large changes in the prices or quantities available to all buyers and sellers.

Loren Brandt's study of Yangzi rice markets nicely illustrates these ideas. Despite the small volume of overseas rice trade, Brandt finds that by the end of the nineteenth century, rice prices in interior markets, like Chongqing and Changsha, were quickly affected by fluctuations in Asian grain markets. This means that the daily lives of rice farmers, rice consumers, would-be rice farmers, grain merchants and shippers, the families and suppliers of these agents, their customers and suppliers, and others in interior regions, like Sichuan and Hunan, were significantly affected by what seem at first glance to be minor economic phenomena. Brandt's study shows how actions in apparently insignificant components of an economy can produce significant reactions, even in distant places, through the medium of market forces. Many people can verify this "principle" from their personal memories of the oil crisis of the early 1970s, when rising energy costs affected travel habits, auto designs, building codes, and so forth in the United States, Japan, and even oil exporters, like Canada.

The economists' campaign to win the minds, if not the hearts, of historians can probably not succeed merely by reciting economic principles as abstractions or immutable laws. More persuasive, perhaps, is the reasoning that is derived from economic theory. Economic theory can serve as a lever for increasing the power of a given set of data and a tool for squeezing as much meaning and implication from it as possible. For economists, economic

theory will suggest a story, or sequence of implications, about sets of initial economic circumstances or facts. The predictions obtained from theoretical reasoning can range from simple propositions about the impact on relative prices of meat and fish of the Pope’s decision to end the Catholic tradition of meatless Fridays to Karl Marx’s grand vision of capitalist decline. The stories told by economic historians fall between these two extremes, typically using short chains of reasoning based on economic concepts to obtain predictions that can be tested with historical evidence. Their method involves selecting a model, or analytic framework, based on assumptions that appear to fit the historical circumstances under investigation, studying the logical implications of the model in search of testable conclusions, and comparing these predictions, as well as the model’s assumptions, with concrete evidence from historical sources.

Several examples can illustrate the value of theory-based analysis as a source of hypotheses for the historian to investigate. Consider the case of railway development, which, by reducing transport costs and transit time, creates new opportunities for trade among cities and between town and countryside. Construction of a new railway line should raise the price that farmers receive for fruit crops, which now gain unprecedented access to urban markets, and lower the cost to farmers of urban factory goods. Terms of trade (price of interregional “exports” divided by price of imports) should improve for both townspeople and farmers. But China’s new railways became the focus of military strife among competing political groups, bringing death and destruction to hapless farmers caught between rival armies.

Lacking detailed information concerning changes in local production or the damage inflicted by military operations, how can the historian begin to determine the economic consequences of railway construction in rural China? Here is where recourse to economic theory, with its capacity to reveal causal links that may provide unexpected opportunities to examine the consequences of historical events, begins to display its potential. The concept of entry and exit immediately directs the researcher’s attention to changes in population density and migration patterns as indicators of altered patterns of economic opportunity in regions affected by railway development. The economic theory of rent implies that trends in land rents and land prices can reveal whether, from the viewpoint of local farmers, the opportunities created by railway development outweighed the damage caused by periodic military incursions and, if so, by how much.

on the consequences of railway expansion comes from Thomas R. Gottschang’s finding that the coming of the railway apparently slowed the pace of out-migration from North China, despite reducing the cost of travel to and from Manchuria. Apparently the increased opportunity arising from proximity to rail transport outweighed the reduced cost of migration in the eyes of farm families in Hebei and Shandong.29

Further examples of how historians can benefit from thinking in terms of economic theory arise from applying the concept of market integration, also known as the law of one price, which postulates that the universal desire to buy cheap and sell dear attracts buyers to low-price markets and sellers to high-price outlets, thus squeezing interregional price differences toward the minimum necessitated by the costs of shipping goods between separate markets. Market integration is made possible by good and cheap transportation, adequate information about costs, and efficient commercial institutions. Consumers, as well as economists, like market integration because it gives them access to a wide range of products at low prices. Producers value market integration because it expands the actual and potential market for their goods. Historians should also be keenly interested in market integration not simply for what markets show about links among various segments of the economy but also because, as the work of Skinner copiously demonstrates, analysis of marketing relationships may affect a host of political and social factors ranging from taxation to marriage and even language.30

Here again, a dose of theory can help the historian to leap over documentary lacunae, as well as overcome skepticism about the heuristic value of economic principles or assumptions. Did agricultural wages, productivity, and incomes rise in China during the decades prior to World War II? To answer this question, one would hope to find reliable information on trends in agricultural production and farmers’ incomes. Unfortunately, the information available to the researcher is both thin and of questionable validity. Wage data for nonfarm occupations, however, are relatively abundant. Can theory offer a useful link between agricultural circumstances and nonfarm wages?

Unskilled workers in such nonfarm industries as cotton mills and coal mines often came directly from rural villages. China’s cotton and coal magnates were profit-seeking entrepreneurs operating in fiercely competitive markets that offered little chance to “pass along” rising costs in the form of higher prices. They had every incentive to keep wages as low as possible. Unless forced to raise wages by government fiat or union pressure, employers sought to avoid raising wages except when it was necessary to assure an


adequate work force. As long as rural labor incomes remain stable, mines and mills can attract workers without raising wages. If rural incomes begin to increase, mines and mills will find their labor supply drying up unless they offer higher wages to village recruits. Under these circumstances, a pattern of rising real wages for unskilled workers in China’s cotton and coal industries can be taken as evidence of rising real incomes in the rural regions that supplied miners and mill hands and also in more remote areas linked through labor markets to the immediate supplying regions. Because inter-regional wage differentials induced large numbers of Chinese workers to cross provincial and even international boundaries in pursuit of economic opportunity, evidence of rising real wages for unskilled workers in the widely dispersed cotton and coal industries furnishes strong support for the view that the rising trend of labor income was national in scope.  

Underlying this reasoning is the economists’ conception, or model, of how markets, in this case labor markets, function. Textile mills or coal mines located in city A customarily obtain unskilled workers (perhaps indirectly through the agency of labor recruiters) from rural areas B and C. The mills or mines pay wages that are higher than typical farm incomes. This premium compensates workers for the cost of journeying to an unfamiliar locale, separation from their families, and the risk of industrial accidents. If farm incomes in B or C begin to rise, mill or mine wages will look less attractive to potential recruits, who will become less willing to leave their villages. The mill or mine owners (or labor recruiters) can look elsewhere for job candidates or raise wages to encourage more volunteers from the customary locations. If young villagers elsewhere are willing to move in response to economic opportunity, nonfarm employers may prefer the cheap option of seeking recruits from alternate rural locations D and E by offering the standard wage. If the rise in farm incomes is a local phenomenon confined to B and C, this approach will prove successful in damping upward pressure on nonfarm wages for unskilled labor. If, on the other hand, farm incomes are increasing across a wide range of localities from which mills and mines might seek to recruit new workers, nonfarm employers will find themselves unable to maintain an adequate work force without raising the wages offered to unskilled recruits. If farm incomes—which provide the financial alternative against which potential miners and textile workers measure the benefit of leaving their home villages—continue to increase, wages paid by mines and mills will rise too.

Thus, once it is assumed that labor markets function in the manner specified, with employers seeking cheap labor supplies and villagers willing to migrate in response to premium wages, the theory of market integration, here

applied to the market for unskilled labor, encourages the historian to perceive the trend of unskilled workers' earnings in coal mines and cotton mills as a barometer of farm incomes, not only in the workers' home villages but also in other villages where mines and mills could easily have sought fresh recruits. The link between farm and nonfarm wages is not automatic. Application of this reasoning requires the historian to determine that the wage data pertain to occupations open to village recruits and to verify the historical relevance of the behavior patterns postulated in the framework, or model, outlined above. If these tasks can be accomplished, economic theory permits the historian to construct a powerful and revealing analysis of phenomena that are simply not amenable to study through conventional methods.

The theory of market integration can also help to estimate interest rates in historical situations. Interest rates are of historical significance because they are part of broader economic cycles, because they tell us something about trends in the economy, and because they influence individual choices between current and future consumption. Yet interest rates are difficult for historians to discern. Consequently Donald N. McCloskey and John Nash's suggestion that interest rates are inherent in the seasonal fluctuation of grain prices is useful for Chinese historians, since the Chinese historical record contains a great deal of detailed information about grain prices. Whoever holds grain harvested in autumn for resale or consumption in the spring sacrifices the use of the money that could be obtained by immediate sale of the autumn harvest. Whoever loans money during the winter months makes an identical sacrifice. In other words, the opportunity cost of holding grain is the cash that could be obtained from autumn sales, plus whatever interest could be earned by that cash over the winter. The law of one price, here applied to the market for money, insists that, over a suitably long number of years, the earnings from assigning funds to holding grain must match the returns from assigning funds to holding debtors' promissory notes. Thus, McCloskey and Nash explain, interest rates, and the variation of interest rates across time and space, can be calculated from the seasonal rise in grain prices that begins with the annual post-harvest trough and ends at the seasonal preharvest peak.32

To recognize the importance of market integration is one thing; to define and measure it is another. As some of the essays in this volume show, even with good price data, it may be difficult to discern whether and when true market integration existed in history. Even in today's world of data collection and widespread information networks, economists still have difficulty establishing what actually constitutes market integration.33 In antitrust cases, the


appropriate definition of a market includes both the “product market” (i.e., whether the product has reasonable substitutes) and the geographic market. When Mobil Corporation tried to acquire the Marathon Oil Company in 1981, Marathon brought an antitrust suit against Mobil. Mobil attempted to demonstrate that the relevant market for oil was nationwide and that hence the merger would have only a slight impact on prices. For Marathon, on the other hand, the task was to demonstrate that the markets for oil were regional and that hence the merger was likely to have a great impact on prices. Marathon won the case because, in the words of the court, “the persistence of price differentials in various areas of the nation demonstrates that motor gasoline does not move from area to area in response to price changes easily or as readily as Mobil asserts. Rather, they indicate that the relevant geographic market for motor gasoline is something less than nationwide.”

Here the debate among lawyers and economists centered, not on the theoretical importance of market integration, but on exactly how to define and measure it.

ECONOMIC METHODS AND THE DATA PROBLEM

The second aspect of an economic perspective or approach involves method. Methodology in economics can mean different things. Broadly defined, it can mean a way of thinking or a general approach to hypothesis testing or problem solving. More narrowly conceived, it can refer to particular statistical techniques: the Gini coefficient, the Chow test, and so forth. Although economics often involves the use of numbers and quantification of some sort, its approach is not absolutely dependent on quantification. At least two of the articles in this volume (by Susan Mann and Emily Honig) involve little quantitative data, and yet they fully reflect an economist’s way of thinking.

Historians of China may be discouraged from pursuing economic topics because of the apparent lack of data. And yet there are, as we shall describe later, many more data than meet the eye. Moreover, generations of historians have contributed fruitfully to the analysis of economic trends in Europe and North America without the benefit of careful compilation or systematic analysis of quantitative data. A generation of new economic historians, focusing its attention on the economies of North America and Great Britain, has demonstrated that better, fuller results and sounder interpretations are often available when research using conventional documentary sources is combined with diligent mining of quantitative materials, which are always deficient in a variety of dimensions. Before succumbing to the defeatist view

that certain data are uniquely defective, Chinese historians should consider
the implication of Nicholas Crafts's new study claiming that the average
annual growth of British per capita income between 1801 and 1831 should be
reduced from the long-accepted Deane-Cole result of 1.6 percent to a much
lower figure of 0.5 percent, implying that per capita incomes rose by 16 per-
cent rather than 61 percent during 1801-31. If British historians cannot yet
determine whether industry and commerce grew slower (Deane and Cole)
or faster (Crafts), or whether agriculture grew much faster (Deane and Cole)
or slower (Crafts) during 1760-80 than during 1700-60, perhaps their
data, which have supported hundreds of studies in what McCloskey calls
"econometric history" are no better than the Chinese historians'.

Historians are particularly concerned with detecting trends and cycles.
Contrary to the political scientists' old adage "In China if something hap­
pens twice, it's a trend," the identification of trends in economic history is
a bit more complicated. Was the economy growing or stagnating? Were in­
comes rising or falling? Was land distribution becoming more equal or less
equal? Was the standard of living rising or falling? Not only is this the stuff
of which the truly important historical debates are made, but it should be
apparent that this is also the material of present-day debates among politi­
cal candidates. These are questions of measurement that are at the heart
of economic methodology.

How economists can use incomplete and imperfect data in studying his­
torical problems can perhaps be illustrated with an analysis of the fate of the
traditional Chinese junk trade in the Republican period. As railways and
steamships were introduced to the Chinese economy in the late nineteenth
and early twentieth centuries, it has often been assumed that they displaced
the traditional sailing vessels, or junks. But Thomas G. Rawski's hypothesis
is that the junk trade not only survived the introduction of modern transport
but actually increased its volume at the same time that modern transport
grew. If he can prove his case, it would be extremely significant for evaluat­
ing China's prewar economy because it would show that the rapidly growing
freight carriage by railways and steamships represented trade creation—an
important sign of commercialization and economic expansion—and not trade
diversion—a mere substitution of new technology for old with no change in
cargo volume.

Economic theory links changes in production (in this case, of transport
services) to the level of capital formation (construction of new junks).
Wooden sailing vessels have long service lives. If we assume that participants

35. N. F. R. Crafts, British Economic Growth during the Industrial Revolution (Oxford, 1985),
as reviewed in the Journal of Economic Literature 24, no. 2 (1986): 683—84; and Phyllis Deane and
in the shipping and boat-building trades display the income-seeking behavior that economists expect in any market economy (shippers do not abandon useful vessels in the absence of significant technological change; shipyards do not continue to operate if sales volume and price plummet), then we must expect any decline in the volume of junk traffic to quickly erode the demand for new ships. This is exactly what occurred on the Liao River in Manchuria, where diversion of riverine traffic to the railways prompted observers to note that “there are no new ships built for the river and [the] majority of the ships now being used are those constructed more than ten years ago.” Information about shipbuilding in the Yangzi Delta (including Shanghai), however, shows that the industry continued to thrive despite unrestricted competition from new carriers. A 1941 survey at Suzhou found that 14 of 36 ships were less than ten years old. Another study lists over 20 places near Shanghai and along both banks of the Yangzi where shipyards continued to operate even after 1940.

If evidence from shipbuilding data indicates that junk traffic did not decline prior to 1937, how can we investigate the stronger proposition that junk shipping actually increased despite growing competition from steamships, motor launches, railways, and trucks? Fortunately, we have some data showing that the junk trade expanded in several important ports and fared well in competition with rail, steamship, and cart traffic in delivering cotton to the major textile center of Tianjin. This information may be supplemented by a series of calculations that estimate the volume of wheat arriving in Shanghai by junk. Wheat was one of the most important commodities shipped into Shanghai. If junk-borne shipments of wheat increased along with the expansion of railway and steamship carriage, the overall argument about the survival and growth of the junk trade is greatly strengthened.

Our estimate rests on an equation. The volume of wheat arriving by junk was roughly equal to (1) the wheat required by Shanghai flour mills, minus (2) the net import of wheat into Shanghai from abroad, minus (3) the net inflow of domestic wheat carried by steamship, minus (4) the inflow of domestic wheat into Shanghai by rail. Gathering together various pieces of admittedly imperfect data, we reach the conclusion that junk-borne shipments of wheat into Shanghai may have risen from 139,000 tons in 1914 to an average of 244,000 tons in the 1930s. But how can we defend our estimate in the face of the known imperfections in the data we have used? The key is to look very carefully at the assumptions employed in constructing the data from which these results are derived. For example, there are several different figures for overseas wheat imports (2) during 1931–33, the end point of our

time series. Our calculations employ the largest of these figures, a tactic that lowers the estimated junk inflow for 1931–33 and thus tends to undercut the working hypothesis. Second, lacking data on railway shipments (4) in 1914, the starting point of our time series, we assume the lowest possible figure—none at all. This raises the estimated inflow of junk-borne wheat in 1914, again in opposition to the proposed conclusion. Despite these two challenges, the calculations are still able to show that junk-borne shipments of wheat into Shanghai were substantially larger in 1931–33 than in 1914.

Historians unfamiliar with quantitative research may complain that this type of scholarship is no more than a tissue of assumptions, with results predetermined before pencil meets paper. Nothing could be further from the truth. Assumptions are all laid out for the readers' scrutiny and critical evaluation, and precisely to show that they do not control the conclusions. In a world of imperfect sources, the researcher must convince critical readers that empirical results are strong enough to overcome possible defects in the underlying data. The tactic of demonstrating an assertion to be valid even under assumptions that stack the deck against the proposed conclusion is commonly used in economics for precisely this purpose. Findings that can survive the impact even of contrary assumptions are called robust. Robustness is a characteristic eagerly sought by applied economists and carefully weighed by readers who find themselves suspicious of published results. If evidence favoring the proposed conclusion is so striking that it emerges even from data that are skewed in ways that suppress the very trend the researcher seeks to establish, even a skeptical audience should acquiesce.

In this way—as illustrated by the examples of using wage data to study farm incomes, deriving interest rates from grain prices, and seeking information about junk traffic by investigating the fortunes of shipbuilders—historians can use economic reasoning to assist them in separating historical fact from fiction. Although it was generally said that steam and rail transport displaced junk transport, economic theory led to the suspicion that this might not actually have been the case, and application of economic methods showed that it almost certainly was not the case.

Identifying trends can help economists and historians to distinguish how people behave from what people say. If professors complain of low incomes, we conclude that they desire higher salaries. It is only when significant numbers of teachers leave academe that we can identify professional wages as being too low in the equilibrium sense. Unlike intellectual historians, who interest themselves in conscious ideas, or cultural or social historians, who investigate attitudes and perceptions (mentalités) that are unconscious, economists are skeptical about words and self-perception. Because acquiring information is costly in terms of both time and money, economists believe that people tend to be well informed only about matters of direct importance to their livelihood and may not see the larger picture. Further-
more, what people say and write about their economic circumstances is often intended to change those circumstances and may not be a reliable guide to the circumstances themselves.

But in focusing on broad trends, economists may overlook cyclical events or regional variations that greatly affect the lives of those who experience them and therefore offer important material for historical studies. Thus the conclusion that junk traffic increased in China during the decades prior to World War II submerges the reality of a regional decline in junk activity along the Liao River. In a monumental and influential work on Chinese agriculture from 1368 to 1968, the economist Dwight H. Perkins estimates the expansion of agricultural output in this period on the basis of population size. The key to his estimate was the assumption that everyone must have eaten a minimum diet (or they would not have been alive).

The story of a rapidly expanding population sustained by six centuries of increased agricultural productivity certainly paints a rosy picture of the Chinese economy and implies that everyone had at least a subsistence diet, contrary to a gloomy Malthusian picture that might otherwise be imagined. But in fact Perkins’s calculations say only that on the whole people must have had enough to eat. His equation does not take into account those who may have died from undernutrition, nor does it consider patterns of regional development and decline or the possibility of extreme inequality of income and welfare. Economists seeking to define long-term trends in average income or consumption may not notice that a minority may have eaten, and lived, exceedingly well, while a larger group may have suffered from inadequate diets (since on the whole, or on average, people had enough to eat). As a result, the measurement of macroeconomic trends, while offering valuable information to historians, leaves much important work to be done in terms of investigating the distribution of gains and losses among regions, groups, and individuals.

ECONOMISTS AND HISTORIANS NEED EACH OTHER

Our message, then, is not that the economic approach to historical research overshadows other types of inquiry or that economics is a panacea for scholarly problems. Indeed, the economic approach tends to have its own limitations, such as taking the whole to be the same as the sum of the parts or underrating the importance of noneconomic causation in history. Economists need to take historical realities into account. But historians need to adopt an

41. This perception is shared by economists who report that “rapid growth in underdeveloped countries has been of little or no benefit to perhaps a third of the population.” See Hollis Chenery, “Introduction,” in Chenery et al., *Redistribution with Growth* (London, 1974), p. xiii.
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economic perspective, particularly when writing about the economy. We contend that history written without the insights that emerge from systematically applying economic theory and method will be incomplete and impoverished. If historians are willing to suspend a certain disbelief about the economists' principles and assumptions, systematic application of theory and methods may produce insights and results that will weaken the initial disbelief. The knowledge required for historians to make use of the economic approach is neither remote nor inaccessible. The successful completion of this volume demonstrates that a brief period of intensive preparation will enable historical researchers without extensive economic training to fruitfully apply the insights of economic analysis with results that will appeal to economists as well as historians. This volume stands as the proof of this assertion, and we now turn to a survey of its contents.

CONTENTS OF THIS VOLUME

The essays in this volume fall into two groups. The first group relies primarily on grain price data from the Qing dynasty to establish long-term trends in the Chinese economy, analyze the nature of market integration, and delineate the role of the Qing state. They could be described as studies of price behavior. The second group of papers focuses on the study of land, labor, and capital in more localized situations in the twentieth century. Narrower in focus and more recent in time, these papers center on the issue of market response. In different ways, these papers illustrate some of the general ideas about economic perspective and approach discussed above and point to further opportunities for work in Chinese economic history.

The essays on Qing price history make use of grain price data from the Qing period compiled from the holdings of the First Historical Archives in Beijing and the National Palace Museum in Taibei. These data form what is perhaps the richest, longest, and most detailed price series for the history of any national economy. Starting formally from the beginning of the Qianlong period in 1736, each governor was required to submit a monthly report of grain prices in his province. This included the high and low price of each major grain grown in each prefecture. This monthly provincial report was compiled from ten-day reports submitted by each prefecture (fu), which in turn had collected ten-day reports from each county (xian). Thus the high and low prices noted in the prefectural reports represent the highest and lowest prices reported by any county within a given prefecture during that month. The number of grains for which prices were collected varied with each province. In the South, as many as five or more different grades of rice were included. In North China, five to seven grains were reported, including wheat, millet, and sorghum.

Although the analysis of these grain price data has just begun, even the
preliminary results have great importance for Chinese historical studies. The records of grain price behavior permit us to establish basic long-term trends in China’s historical economy, as well as to identify some shorter-term cycles. Grain was the single most important commodity in the agrarian economy of imperial China and the best indicator of economic trends. Yeh-chien Wang’s article, “Secular Trends of Rice Prices in the Yangzi Delta, 1638–1935,” delineates two long-term cycles in that key region of China: a steep downtrend from the 1640s to the early 1680s followed by over a century of generally rising prices; and a second cycle of price declines followed by steep inflation from the 1880s until the world depression of the 1930s.

Lillian M. Li’s paper, “Grain Prices in Zhili Province, 1736–1911: A Preliminary Study,” shows broadly similar patterns for wheat, millet, and sorghum prices in North China, with a peak in the 1820s and another steady climb starting in the 1890s. Li’s data suggest the presence of distinct short-term cycles of perhaps four- or five-year intervals for coarse grains in the eighteenth century, and possibly longer cycles for wheat prices. She also finds considerable price fluctuation in the late nineteenth century, before the steady upward climb of the early twentieth century.

Much more work remains to be done to determine whether Wang was correct in his earlier, pioneering work when he concluded that North and South China grain prices in the Qing moved essentially in a synchronic manner, thus contradicting Skinner’s hypothesis of asynchronic regional cycles and also implying considerable interregional market integration as early as the eighteenth century. Also on the agenda is work that will connect the analysis of Qing grain prices with the work of Brandt, who dates China’s integration with the international rice market from the late nineteenth century.42

Grain prices can provide insight into the functioning of markets. In particular, price data can illuminate the extent of market integration within, as well as among, regions. Peter C. Perdue’s essay, “The Qing State and the Gansu Grain Market, 1739–1864,” reveals that even the remote Northwest of China had achieved a considerable degree of market integration, primarily through strong state intervention. Because of the strategic importance of the Northwest, the Qing court maintained a heavy military presence there and, to support it, a granary system that kept relatively high per capita levels of grain reserves. Perdue believes that, in conjunction with private storage and commerce, public grain storage worked to support the integration of key markets of Gansu with each other and with neighboring Ningxia Province.

Although Perdue's conclusions must be described as tentative because his data series are far from complete, his Gansu data present a strong case for market integration even in relatively remote areas of China.

In "Grain Markets and Food Supplies in Eighteenth-Century Hunan," R. Bin Wong and Peter C. Perdue further pursue the issue of market integration, this time within Hunan Province, a major rice-exporting region in central China. Since the general outline and functioning of the Hunan grain markets is relatively well documented, Wong and Perdue utilize the grain price data to test whether high levels of integration actually existed among those prefectures known to be heavily engaged in the rice export trade. They find that separate analyses of high prices and low prices each tend to confirm integration among the exporting prefectures and lack of integration between them and the nonexporting prefectures. They also see in the separate reporting of high and low prices an opportunity to test for integration within each prefecture, since the high and low prices reported for any month from each prefecture presumably represent price quotations from two different counties within the prefecture. They find that, with one exception, exporting prefectures had high levels of internal integration. Intraprefectural integration was also high in relatively isolated prefectures. In short, Wong and Perdue's findings are reassuring because the "price data generally confirm the outlines of the export trade based on qualitative information."

Because there is a considerable amount of qualitative information about Hunan's rice trade, one may conclude with some degree of confidence that high correlations of prices or price differences (the difference between the price in the current period and the price in the previous period) do represent market integration. Without confirmation of trading patterns, the occurrence of high price correlations might arise from common climatic patterns or changes in the stock of money rather than from market integration. In the case of Gansu, for example, it might be argued that the strong military presence in the province produced a type of integration of prices based, not on true markets, but rather on a large measure of government intervention. Perhaps this could be seen as a kind of false or pseudo market integration, which is not to deny its historical significance. The same hypothesis could be advanced with respect to grain markets in Zhili, where the presence of the Imperial court, bannermen, and the military was so pervasive.

We can also use grain prices to examine the short-term fluctuations in periods of crises. Li, in her article on Zhili, uses grain prices to test the impact of crises in several different ways. Like Perdue, she uses regression analysis to try to measure the relative impact on prices of the passage of time, seasonality, and natural catastrophes. Overall, she finds that flood or drought did affect prices, as one would expect. But the differences between the price levels in normal years and those observed under crisis conditions were rather slight in comparison to the differences recorded during crises in
seventeenth- and eighteenth-century Europe. Most likely, the operation of the government granary system helped to stabilize prices and avert catastrophes, as well as to provide relief during the crises themselves.

The topic of government grain storage raises a number of theoretical as well as historical problems. Recent scholarship on the Qing granary system has excited considerable interest. In fact, some of our contributors have completed a book describing the state granaries and offering important data on their management and holdings. For Chinese historians, the state's major role in grain storage comes as no surprise, although the extent and efficiency of the Qing granary system is quite remarkable. For non-China economists and many historians, however, the notion of public storage requires considerable explanation. Economists, working from the principle of opportunity cost, will immediately question whether government effort had any significant effect on market circumstances. Private citizens store grain because they hope to profit from the regular differential between low autumn and high springtime grain prices and also from high prices that occur in the wake of disasters, such as flood, drought, and war. Government storage efforts intended to limit seasonal price fluctuations and to curtail irregular price peaks will reduce the profitability of private grain storage and lower the risk to private citizens of not holding grain stocks. Thus, economists will reason, public storage encourages a reduction of private storage, creating the possibility that energetic official intervention may have no significant effect on the total quantity of grain stored, seasonal price fluctuations, or the price consequences of periodic natural or manmade disasters.

Of course the Chinese historians have a response to the economists' skepticism. The economists' assumption that private and public grain storage are substitutes for each other is based on the premise that the private sector has the capacity to store grain as conveniently as the government. In fact, the forthcoming granary volume will show that in per capita terms, the granary stocks were highest in China's most remote and least commercialized provinces. The government, in short, appears to have intervened precisely where the private sector was least able to ensure market stability. Put another way, the government was subsidizing the storage of grain. In highly commercialized regions, such as the Lower Yangzi area, with its dense network of markets and transport arteries and extensive private commerce, the government could and did leave the job to private efforts and to the market. Larger public storage programs might merely have replaced private efforts rather than compressing the amplitude of fluctuations. The topic of grain storage thus illustrates the economic sophistication of Qing officials. It also provides a

fruitful example of how the economists’ approach to a problem may help to structure historical inquiry and how, conversely, understanding the historical context helps to modify the predictions of economic theories. Although much work has already been done on granaries, the availability of price data now creates an opportunity to combine economic analysis and documentary research to test and measure the impact of Qing grain storage efforts on the economy.

Another theoretical problem raised by the grain price data, also illustrated in Li’s article, is that of how to define and identify markets. Do wheat, millet, and sorghum in North China belong to the same market, or do they constitute separate markets? The principle of substitution teaches that a rise in the relative price of wheat or any other commodity will increase the demand for, and hence the price of, items that provide close substitutes for the initial product as buyers seek to maintain their living standards or contain costs in the face of adverse price change. The question here is to what extent people were willing to substitute one grain for another or, elsewhere, one type of cloth for another. Such questions bear closely on the question of market integration and call for further inquiry.

While the articles of Li, Perdue, and Wong and Perdue deal only with grain prices, the articles of Yeh-chien Wang and James Lee, Cameron Campbell, and Guofu Tan show how price data can be used in conjunction with other long-term data. In his article, Wang arrays his rice price data from the Lower Yangzi region together with population data, information about silver stocks, and weather trends, to consider what factors may have influenced long-term cycles of inflation and deflation in the Yangzi Delta. Wang’s preliminary, and pathbreaking, estimates of China’s monetary silver stocks indicate a roughly parallel growth of rice prices and the stock of monetary silver throughout the Qing period. He finds that in China, as in England, long-term trends in food prices display a substantial correlation with changes in population and population growth. Wang describes two long swings in rice prices during the three centuries prior to World War II. In both cases, periods of rising prices coincide with relatively rapid growth of both population and monetary silver, while interludes of deflation are associated with stagnant or declining population. Wang’s discussion highlights opportunities for further study of major factors underlying the long-term path of China’s economy. Can we sharpen the causal interrelations among population, money stock, commercialization, climatic change, food production, and the material well-being of the Chinese peasantry? Do available studies understate the long-term significance of international money flows for China’s agrarian economy? How closely were the rice markets of the Yangzi Delta linked to the farm economies of other regions within China’s vast land mass?

In “Infanticide and Family Planning in Late Imperial China: The Price
and Population History of Rural Liaoning, 1774–1873," James Lee, Cameron Campbell, and Guofu Tan employ price data to help analyze a unique and rich set of Qing dynasty population registers from Daoyi, a rural suburb of present-day Shenyang in Liaoning (formerly Fengtian) Province in southern Manchuria. Their major demographic findings are, first, that significantly higher levels of infant mortality were found among females than among males, although there were greater fluctuations in mortality among males; second, that most couples appear to have practiced a considerable degree of family planning; and, third, that infanticide, particularly female infanticide, was a principal means of family planning. In this article, the authors pose the question of how food prices might have influenced birth and death rates. In other words, were fertility or mortality affected by times of scarcity, as indicated by high food prices? Their answer is that there seems, on the whole, to have been little relationship between food prices and mortality, but there was a strong relationship between high food prices and infanticides, particularly, but not exclusively, female infanticides. While these conclusions are likely to be hotly debated, in part because it is unclear whether Liaoning or Manchurian family patterns are generalizable to the rest of China, Lee, Campbell, and Tan have pointed toward a direction in research that has not previously been pursued in Chinese history.

The second group of essays in this volume focuses on issues more familiar to modern Chinese history—urban and rural poverty, the economic consequences of political unrest, and economic growth or the lack of it. In dealing with the factors of land, labor, and capital in local or regional settings, these essays pursue large issues in a more focused, and perhaps more manageable, way than the first set of articles. In each case, we see the interplay between economic analysis and historical inquiry. Economic models open new avenues of inquiry for historians, while the historical context illuminates the social and institutional conditions that shape the impact of economic forces in particular times and places.

In the first of these essays, "Land Concentration and Income Distribution in Republican China," Loren Brandt and Barbara Sands—the only economists among our contributors—address the issue of land concentration and income distribution in twentieth-century China. They challenge the commonly held view that there was increasing concentration of land ownership in the late nineteenth and early twentieth centuries and that concentration of landed wealth necessarily produced wide inequalities in the distribution of income. Although the data from the 1920s and 1930s show a highly unequal distribution of land, Brandt and Sands show that shifting the statistical base from landholding per household into per capita terms narrows the gap between poor and wealthy households. They argue that without comparable data for earlier periods, there is no basis for claiming that the degree of concentration of land ownership was rising over time.
Their key point, however, is that in the complex economy of North China, the distribution of per capita income depended on earnings from the disposition of labor and goods as well as land, so that the concentration of landholdings need not have coincided with the concentration of incomes. If, as hypothesized by Peter H. Lindert, “the common folk,” who specialized in producing and selling goods that embodied large components of unskilled labor, “were among the greatest gainers” from the expansion of China’s domestic and international trade, the spread of commercial agriculture following the growth of trade and transport may emerge as a significant source of reduced income inequality in the North China countryside.44

Although Brandt and Sands analyze three villages selected for their distinct economic characteristics, skeptics will note the small size of their sample and the possible biases inherent in their principal source, the South Manchurian Railway Company village surveys, which form the basis for a number of controversial studies of peasant welfare in North China.45 For our purposes, however, of greater interest than the ultimate correctness of their interpretation is the economic approach or perspective that they have employed. By posing a theoretical issue, then isolating a number of key variables and finding an appropriate set of data, the authors create a framework for systematic analysis of the issue at hand. Finally, by placing the Chinese issue in comparative, international terms, the authors provide a baseline or context within which to judge the issue of large or small. At what point should income inequality be considered large? Too large? Such judgments require not only quantification but also appropriate context.

Lynda S. Bell approaches the issue of rural income from another perspective: she looks at the silk industry in Wuxi, an area in the Lower Yangzi region that developed into a major sericultural and silk-reeling center in the nineteenth century after the Taiping Rebellion. In “Farming, Sericulture, and Peasant Rationality in Wuxi County in the Early Twentieth Century,” Bell explores an apparent paradox: why, in one of the more prosperous regions of China, did peasants in the 1920s and 1930s experience low incomes from sericultural activity? And why should peasants continue to pursue sericulture even though, as she effectively demonstrates, the returns per unit of labor were lower for mulberry cultivation and silkworm raising than for rice or wheat farming? Does this mean that farm households were not acting rationally or that they were engaged in a kind of “self-exploitation” in the manner described by A. V. Chayanov for the Russian peasantry? The key to this paradox, Bell finds, is that women supplied most of the labor in seri-


45. Myers, Chinese Peasant Economy, and Philip C. C. Huang, Peasant Economy, also rely on these surveys.
culture. Compared to other domestic industries they could engage in, such as cotton weaving, sericulture brought superior returns. Only factory work could have brought higher wages to women, but the opportunity cost—in terms of domestic labor lost to the family if the woman left for a factory—outweighed the extra income that might have been earned. Moreover, Bell's calculations reveal that even at the depressed silk prices of the 1930s, income from sericulture allowed Wuxi farm households to buy more rice than they could have grown on the land occupied by mulberry plants. So Bell finds peasant choices ultimately to be rational, but cautions that rationality need not imply that they were earning large profits; rather, rationality was what kept them going in an economy in which subsistence, rather than profit, was still the major preoccupation. Participation in an international market presented new opportunities, but Wuxi peasants found that it also presented new risks.

While the economic value of female labor implicitly figures in Bell's article, it is the main topic of Susan Mann's essay, "Women's Work in the Ningbo Area, 1900–1936." Using rich qualitative materials from a relatively commercialized region of China, Mann delineates the factors that affected both the demand for, and the supply of, female labor. On the demand side, she shows that there were many opportunities for female workers both within and outside the household in the Ningbo area and that the hierarchy of jobs, from the women's perspective, was less related to wage levels than to the perceptions of social respectability and the degree of personal convenience associated with each type of work. On the supply side, the availability of female labor from each household was dependent on three major factors: its size, its other resource endowments (these two were, of course, closely related), and its stage in the family cycle. Families with adult women who had no child-care responsibilities (young women before marriage or "able-bodied widows") were most likely to have labor to spare and therefore to benefit from new opportunities for female employment within the household. Factory employment, which violated social conventions that restricted respectable women to working within the household, was acceptable only to women from "poor households strategizing to keep their menfolk afloat."

In "Native-Place Hierarchy and Labor Market Segmentation: The Case of Subei People in Shanghai," Emily Honig addresses an apparent puzzle: why were people from Subei, the area of Jiangsu Province north of the Yangzi River and south of the Huai River, routinely barred from certain types of employment in Shanghai, even when they would have worked for lower wages than employers paid to natives of south Jiangsu? Regarded as inferior human beings, the Subei people in Shanghai were condemned to the least attractive and least remunerative forms of employment—rickshaw pulling, night soil and garbage collecting (literally, as she says, "shit work"), barbering, and so forth—within a clear hierarchy of jobs. Honig employs the
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Economists' concept of segmented labor markets to show that in Shanghai, it was not race, religion, or ethnicity that formed a barrier to free entry and exit, but native-place hierarchy.

Both Mann's and Honig's articles show that the economists' notion of choice has to be tempered by the social historians' understanding of gender, class, and native-place ties. Despite the heuristic value of the economists' notion of ceteris paribus, historians find that all other things are rarely equal and in fact it is the "other things" that may hold the key to understanding the flow of events. Still, the approach of these two papers is entirely consistent with an economic perspective. To start with the assumption that there should be a unified labor market with no barriers to exit or entry and with essentially one wage scale is not wrong; what would be wrong is to stop there. Looked at from the employers' perspective, labor market segmentation rests on their need to assess the qualifications and character of would-be employees or associates. With no access to data banks or credit histories, they must seek a quick and inexpensive screening device. Discrimination on the basis of ethnicity, place of origin, linguistic background, or education, can be partly understood simply as a cost- and risk-reducing business decision.

Susan Mann's Ningbo women benefited from their reputation for diligence, skill, and gentility. Employers preferred workers from Ningbo and other south Jiangsu communities over migrants from the north not only because of their superior technical and social skills but also because kinship ties and networks of regional association were available for disciplining and controlling south Jiangsu workers, making it more profitable to hire them, even when they might require higher pay than northerners. By the same token, Subei natives were discriminated against. Businessmen preferred to deal with those whose background seemed to increase the likelihood of the successful fulfillment of agreements. When disputes arise, the existence of voluntary organizations, such as native-place associations (huiguan), increases the probability of speedy resolution of conflict by informal procedures acceptable to all parties. Drawing on the economic theory of clubs, Janet T. Landa has proposed just such an explanation for the tendency of Chinese businessmen in Southeast Asia to deal preferentially with Chinese whose ancestors migrated from the same district or province, secondarily with other Chinese, and only if other contacts are not available, with local non-Chinese or with foreign business partners.46

Finally, Kenneth Pomeranz's article, "Local Interest Story: Political Power and Regional Differences in the Shandong Capital Market, 1900--

1937,” illustrates how political structures can decisively influence the outcome of economic change. During the early twentieth century, Shandong Province experienced an expansion of markets and commerce similar to that in the Wuxi region of Jiangsu Province in an earlier period. As in Jiangsu, Shandong villagers were quick to avail themselves of new economic opportunities, specializing in peanuts and other cash crops in some regions and, as Pomeranz documents, exporting large quantities of underpriced copper coins whenever it became possible to do so.

Shandong’s political elite found themselves torn between the gains available from encouraging economic integration and the benefits for themselves and their mercantile allies of using military force to obstruct integration and then exploit the resulting regional price gaps for pecuniary gain. With leaders in different regions responding differently to market circumstances, Shandong’s economy displayed lines of demarcation that reflected the impact of political decisions more than economic, social, or geographic forces. Despite a national trend toward economic integration, the needs of state making during this turbulent period of Shandong’s history prompted local authorities to restrict the movement of specie across administrative boundaries, leading to marked regional variations in both the silver-copper ratio and local interest rates that illustrate a real political constraint on the spread of purely market forces.

CONCLUSION

The essays in this volume do not fall into any single neat line of interpretation about the economic history of China over the last two or three centuries. Pomeranz’s detailed work on Shandong cautions us against any broad generalizations about the extent to which the treaty ports in nineteenth- and twentieth-century China affected the hinterland economy. Pomeranz shows us that the more advanced, coastal area did interact with the hinterland but that political intervention prevented a higher degree of market integration.

The works of Bell, Mann, and Honig also contain a cautionary message. Even in the Lower Yangzi macroregion, the most agriculturally prosperous and commercially advanced area of China, the opportunities for economic gain for individual peasants or workers, although often greater than ever before, could be undercut by international economic instability, gender differences in the returns to labor, and unequal access to the urban labor market. The story that Brandt and Sands tell, however, contains the reverse message. In the much more adverse conditions of North China, all may not have been so bad as it appeared. New employment opportunities provided more channels for a family’s economic gain than just landholding. Entry to and exit from these lines of work appear unimpeded in the North China world they describe.
The lessons of the articles in Part 1 are somewhat different. In some cases, the findings of these grain price studies confirm previously known trends or previously advanced hypotheses. For example, Wong and Perdue’s study of Hunan’s grain price series confirms commercial patterns already discerned through qualitative sources. Li’s case studies of crises parallel the results of Pierre-Etienne Will’s documentary study. Perdue’s delineation of marketing patterns in Gansu coincides with G. William Skinner’s predictions about the spatial patterns of Gansu’s commodity trade. In other cases, such as Wang’s study of money supply or Lee, Campbell, and Tan’s study of Liaoning, new materials have generated new hypotheses about long-term trends.

These essays also contain the potential for even bolder messages, perhaps revisions of current received wisdom, about China’s economic history over the last two or three centuries. Some readers may derive from the essays in Part 1 a picture of the eighteenth-century economy as more advanced in commercial development and market integration than previously thought. Certainly, the quality of the Qing bureaucracy’s price data seems higher than that of its population records, the systematic fabrication of which Skinner has recently exposed. Wang’s compilation of information on stocks of monetary silver creates an opportunity for using the equation of exchange to investigate the implications of Dwight Perkins’s long-standing assertion that, on the average, Chinese living standards, as measured by the availability of grain, experienced no long-term upward or downward trend during the Ming and Qing dynasties. The essays in Part 2 all illustrate, in varying ways, the extent to which commercialization, including the development of foreign as well as domestic trade, penetrated the local economies of many areas. The story of expanding commercial networks finds a basis in these papers, but there are other stories that have been, and will be, told about the modern economy.

Despite the many insights and contributions contained in the essays that follow, we believe, however, that the real lessons of this volume are not the substantive ones. In each case, economic theories and methods have been employed to clarify the facts of history and to advance its understanding.


48. If we assume parallel growth between silver stocks and money supply, between grain prices and the general price level, and between foodgrain production and total output, the equation of exchange can be used to derive the time path for income velocity of monetary circulation implied by Wang’s data on silver, grain prices, and population together with Perkins’s hypothesis of stable per capita output. The plausibility of the resulting velocity estimates and of changes that might arise from adjustments reflecting known biases in the underlying data (we know, for example, that money supply grew faster than silver stocks in the late nineteenth and early twentieth centuries) should make it possible to evaluate the degree to which Perkins’s results, the grain price data, and Wang’s new monetary estimates provide a mutually consistent picture of overall economic trends.
Without a fundamental understanding of the laws of supply and demand and the significance of market integration, none of the essays in Part 1 could have been written. Without an appreciation of how factor markets operate, the essays in Part 2 would have been greatly weakened. Wang’s article provides an excellent example of how economic theory, in this case the quantity theory of money, can inform both the construction and interpretation of economic data to help formulate new questions and hypotheses.

In many of the essays, however, a simple economic approach in itself would lead to an impasse or a seeming contradiction. These apparent puzzles, such as Shandong’s lack of monetary integration or Wuxi’s apparent poverty in one of China’s most prosperous regions, can only be explained with reference to the institutional and social context that historians are uniquely qualified to understand and explain. Without knowledge of the social prejudices attached to Subei people, their lowly position in Shanghai’s labor force would defy understanding. Without knowing the history of the Chinese bureaucracy and the fundamentals of Confucian political theory, Western-trained economists find it difficult to comprehend why the Chinese state should have maintained a vast civilian granary system in the Qing period. Often the results of economic analysis raise questions that compel us to further noneconomic inquiry. The surprising demographic behavior of the Han Banner population of Liaoning causes us to want to know more about their ethnic background, their family structure, and their food allocation habits and in particular to understand whether they were very different from Han Chinese who lived within the Great Wall. In short, economic analysis cannot stand alone and, in almost every case, offers rich opportunities for work with other disciplines—sociology, anthropology, politics, and history.

Chinese economic history is barely coming into its own as a field of study. What this volume is intended to show, to its authors as well as to our colleagues and students, is that further study of China’s economic history that systematically utilizes the theories and methods of economics can generate new hypotheses and fresh perspectives that will enrich the study of all aspects of China’s history as well as deepen our understanding of the structure and evolution of the Chinese economy itself.