A Farewell to Alms: A Brief Economic History of the World. By *Gregory Clark.* The Princeton Economic History of the Western World. Edited by *Joel Mokyr.*
Princeton, NJ: Princeton University Press, 2007. Pp. xii+420. $29.95.

For most economic historians, the Industrial Revolution is the defining moment of human history. Beforehand, economic performance in all parts of the globe had remained in a tight range between subsistence and a level not that much higher; since then, income (however maldistributed) has been multiplied more than tenfold, while population and life expectancy have also shot up. It is a very frustrating moment as well. Despite being well defined chronologically (1750–1850) and spatially (England), explanations for why the Industrial Revolution happened where and when it did remain frustratingly elusive. Gregory Clark's *Farewell to Alms* purports to resolve this puzzle. In tackling this broad question Clark has a larger purpose, namely, to debunk the now commonly held view that institutions are critical to economic performance.

The book is written in a lively style and with considerable attention to economic intuition. To support its arguments it marshals what is now more than twenty years of research undertaken by Clark on the long-run evolution of prices in England. The book is broadly organized in three parts. The first is devoted to the Malthusian economy; the second discusses the industrial revolution; and the third deals with the diffusion of the new economy.

The section on the Malthusian economy contains a wealth of poorly documented information, which more often than not is related to the question but not decisive. The early chapters in fact largely follow the logic laid out by Wrigley and Schofield nearly three decades ago.[1] Additional evidence on heights, nutrition, and wages lead Clark to make two contradictory points: standards of living did not change much in England for the millennium prior to 1700, yet the standards of living were much higher in eighteenth-century England than elsewhere. Since there is little evidence that in the times of William the Conqueror England was a significantly richer economy than its European counterparts, this seems rather odd. Clark might have paused to consider whether there was more long-term change in the English economy than his data suggest. But this is a book in a hurry.

Chapters 4–6 are devoted to demography, which, according to Clark, plays a key role in driving economic change. Here Clark wants to emphasize the deep relationship between income and fertility: as is well known, prior to the demographic transition better-off households had more children for a variety of reasons, including earlier marriage, lower mortality of women, and shorter birth intervals. One is often left puzzled as to whether the important differences are between Europe and the rest of the world (say China) or within Europe (i.e., between England and the Continent). While the former might explain why Europe was more likely to experience the Industrial Revolution, it would not locate it in Britain, and it would leave demography unable to account for the considerable variation in real wages across Europe. Conversely, to the extent the focus is on Britain versus Europe, the evidence about the Ache or the Yanomamo (e.g., table 4.4) is a distraction.

The problem of connecting argument and evidence deepens when we reach the section on mortality and the higher likelihood that the children of the rich would

[1] Edward A. Wrigley and Roger S. Schofield, *The Population History of England, 1541–1871* (Cambridge, 1989); Edward A. Wrigley, R. S. Davies, J. E. Oeppen, and R. S. Schofield, *English Population History from Family Reconstitution, 1580–1837* (Cambridge, 2005).

survive (chap. 6). While one might quibble with the problem of selection in using wills to infer the number of surviving children, one wonders about the extent to which this pattern is either peculiarly European or English. The evidence in chapter 6 is exclusively English, and the reader is left to infer what might be happening elsewhere. There are two possibilities: perhaps this positive selection in favor of the rich was intense only in England, or perhaps in fact it was significant everywhere. The evidence where it exists would be consistent with the latter opinion.[2] Still, we must dash forward.

Chapters 7 and 8 provide a transition to modern growth by emphasizing first the slowness of technical change and then the irrelevance of institutions. To be sure, technical change was slow prior to 1750 relative to what it has become since then, but then again we have heard the same contrast made between the years before the Great Depression (slow) and after World War II (fast) or even before and after the Internet. The discussion of institution stands out for its economic naïveté. Take the case of taxes. Clark wants to use the fact that government revenues have grown massively with the economy to argue that the disincentives arising from taxation are weak. But economists know well that there is a difference between marginal and average taxation as measured by the ratio of revenue to economic output. If the marginal rate is high enough, revenue will be very low, and the observed rate of taxation in the economy can also be low as a consequence.

Chapter 9 offers the core of the argument, namely, that the British economy evolved out of the Malthusian equilibrium because of a set of changes in individual preferences that made people more willing to invest: modern man is patient. Evidence for this change comes from the dramatic decline of return on debts after the Black Death. The rise of patience and thus of investment in human, physical, and financial capital was, Clark argues, the result of the long-run effect of the greater survival of the heirs of the rich. These children were endowed either by nature or by nurture with the preferences of accumulators, and over the generations they replaced the less patient members of society. Yet while Clark's figure 9.1 provides evidence of this change in England, it was in fact a general European phenomenon.

The next set of chapters (10–13) focuses more extensively on Britain and on the Industrial Revolution. While entertaining, the first two of these chapters seem shallow. The next two reprise some of Clark's best work on innovation and the diffusion of technology. Unfortunately, the relationship between the evidence presented and the argument at hand is often opaque. Among points readers should keep in mind are the differences between industrial productivity growth and aggregate growth. While the book industry had tremendous productivity growth following the innovation of moveable type, its impact was smaller than that of cotton textiles because demand for books was relatively small. This is Clark at his best. Among points generous-minded readers will want to forget is the discussion of why land values in England did not rise. Here Clark's failure to consider geography does him a serious disservice—it was not the price of land in the Midlands that benefited from the Industrial Revolution but the price of land in Ohio, Ontario, and Australia.

The last set of chapters (15–17) builds heavily on information from Clark's 1987 *Journal of Economic History* article entitled "Why Is the Whole World Not Developed?" These chapters develop two key ideas: the first is that capital scarcity is not a major problem in economic growth; the second is that labor efficiency is the key to

[2] See, e.g., H. Hadeishi, "Economic Well-Being and Fertility in France: Nuits, 1744–1792," *Journal of Economic History* 63, no. 2 (June 2003): 489–505; D. R. Weir, "Family Income, Mortality, and Fertility on the Eve of the Demographic Transition: A Case Study of Rosny-Sous-Bois," *Journal of Economic History* 55, no. 1 (March 1995): 1–26.

prosperity. The argument runs as follows: although there is a connection between output per worker and capital per worker, it is a superficial one. Indeed, capital per worker is abundant where workers are efficient. Efficiency here is measured by techniques that net out the impact on productivity of differences in capital, energy, and raw materials. The case is developed using the textile industry around 1910, when managers of textile firms had access to the same British-made machines all over the world, but the number of looms they made available to workers varied systematically. In rich countries a worker tended multiple looms, while in the poorest countries multiple workers tended only a single loom. Workers in the poorest countries, one must conclude, were thus less efficient than workers in rich countries. There are myriads of possible explanations, including labor relations, health status, and the culture of work, and one should not be surprised that the answer remains elusive.

What is more puzzling is Clark's rash abandonment of capital. Indeed, his premise of why England exited the Malthusian economy relies first on the tendency of the rich to have more offspring. That tendency in the long run produced a society of industrious individuals who accumulated the wealth that was found in their probates. Yet the evidence about industriousness (high labor efficiency) all occurs at a very late date. The early data show instead that individuals who accumulated wealth (by whatever process) had more children. If we accept the extremely dubious contention that such a demographic pattern had consequences, it stands to reason that it produced a population of individuals who accumulated wealth and that by 1750, say, England had become capital abundant. (That is certainly the conclusion to which Clark's colleague Peter Lindert came.)[3] Yet this could have been accomplished simply through a relatively modest change in the savings rate without a massive change in individuals' drive to work. Now if we consider the rest of the world, where there were fewer individuals driven to die wealthy, it had to be poorer than England; thus, the return on capital would have to be higher. That in the last half of the nineteenth century England exported half its savings is consistent with the notion of differential levels of capital per person across the world. What then of the industrious British? Well, one might simply say that faced with abundant capital they found work more appealing because their wages were higher. Thus, it was high wages that made the workers industrious, not their genes or their culture.

As the discussion above suggests, I found *A Farewell to Alms* less than satisfactory. Like its title, it is often glib and too ill disciplined to make its case. I would have strongly preferred to see the author take the scientific approach of considering the evidence both in favor of and against his thesis and clearly delineating necessary and sufficient conditions for accepting parts of it. Instead, he took the lawyerly tack of piling on evidence that is related to the question but often of limited consequence. That approach does have the merit of allowing Clark to produce a book that sells, something not to be neglected by a scion of the British Isles conditioned by culture or genes to the industry of wealth creation.

Those of us who retain a deeper interest in the Industrial Revolution may well want to stay with some older works like Wrigley's *Continuity, Chance and Change* and some more recent papers by Robert C. Allen.[4] And for a more measured examination of the international textile industry, the papers of Steven Broadberry and his coauthors

[3] Peter H. Lindert, "Unequal English Wealth since 1670," *Journal of Political Economy* 94, no. 6 (December 1986): 1127–62.

[4] Edward A. Wrigley, *Continuity, Chance and Change: The Character of the Industrial Revolution in England* (Cambridge, 1988). See Allen's papers at http://www.nuff.ox.ac.uk/General/Members/homepage.aspx?nuffid=ALLERC00.

will prove useful.[5] While perhaps less scintillating than Clark's volume, these works will prove more instructive.

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Konfessionalisierung und Staatsinteressen: Internationale Beziehungen 1559–1660. By *Heinz Schilling.* Handbuch der Geschichte der internationalen Beziehungen, volume 2. Edited by *Heinz Duchhardt* and *Franz Knipping.* Paderborn: Ferdinand Schöningh, 2007. Pp. xvi+673. €128.00.

This book is a massive achievement: 600 pages of text, more than fifty pages of secondary literature, sixteen maps, and many illustrations provide not only the most comprehensive survey of its subject but also a challenging synthesis of the relation of religion, foreign policy, and the genesis of the European power system from the middle of the sixteenth century to the 1660s. Anyone interested in the nature of this most complex and excruciating crucible of state building, foreign policy and religion, whether in history, sociology, or political science, needs to take account of this work. Schilling accomplished this most rare of achievements, a comprehensive synthesis of the specialized historical literature on the subject and an argument sustained by this breadth of material making sense of the inchoate plurality of forces, persons, developments, and interests characterizing this period. The specialized literature he makes available to the reader includes English, French, Italian, Dutch, Polish, Spanish, and Romanian titles, allowing one to check specific details about virtually every aspect of foreign policy in the period broadly understood, from the military revolution to dynastic relations—a "handbook" in the fullest and best sense. For this achievement alone, an English translation, making this resource available not only to students of early modern history, would fill a much lamented void. History, sociology, and political science libraries can hardly afford to miss this reference. But there is more. Whoever ventures to read these 600 pages as a book finds a carefully crafted argument, tracing the gradual establishment of a European power system by 1660 from the disintegration of European Christendom. The impact of the establishment of confessional churches and the enormous influence of religious conflict is traced in virtually all areas affecting foreign policy, including informal networks and culture. It was not least from the experience of the devastating consequences of the religious wars that "fundamental confessionalism," shaping European relations from the 1580s to the 1630s, gradually lost its grip and "state interest"—not merely princely interest—gradually emerged as a prime category in European power relations.

Space permits me to describe in more detail Schilling's sophisticated argument. It is organized in three main parts. Part A traces a number of common European developments of the period, from issues of state building to the military revolution and from networks of orders and churches to issues of the development of international law, diplomacy, means of communication, and practices of court ritual.

Part B describes the regional zones from which European relations emerged from 1550, in particular in south and western Europe (Spain, France, England, Ireland, Scotland, the Netherlands), the Nordic Baltic region (with Sweden, Denmark, Poland, Russia), and the middle and southeastern region (with the Empire, the Balkans,

[5] See http://www2.warwick.ac.uk/fac/soc/economics/staff/faculty/broadberry/wp.