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Kevin H. O'Rourke and Jeffrey G. Williamson, *Globalization and History: The Evolution of a Nineteenth-Century Atlantic Economy* (Cambridge, MA, and London: The MIT Press, 1999) pp. xii, 343. ISBN 0 262 15049 2.

Most of the fourteen chapters present data and results from two or three of the authors' previously published papers along with summaries of the relevant literature. The resulting flood of information is controlled and purposeful, resulting in an impressive demonstration that careful research in economic history has much to offer.

They argue that the defining characteristic of the New World was great natural wealth and a relative lack of labor. As transportation costs fell it became feasible to shift land-intensive production processes to the New World, reducing the pressure on the land in the old. Expansion of production in the New World attracted capital flows to develop railroads and other public infrastructure, much of it closely related to trade. The high wages and growth attracted young workers from throughout Europe while the Irish famine provided a focal point.

Trade and the great migrations of labor would both tended to reduce land values in Europe while increasing them in America. Predictably, landowners in Europe opposed trade. On the continent the opposition succeeded to such a degree that the relative prices of agricultural goods rose while they fell rather dramatically in free trading England. The authors argue the success of free traders in England was due to strong industrial interests that would benefit from the shift away from agriculture.

As unskilled wages fell relative to average income in the Americas countries that were open to foreigners began to change policies. The slowdown in immigration was associated with declining capital flows that were severely interrupted by two world wars and the depression. The authors argue that the interruption of globalization was at least partly an endogenous response to rising income inequality and not an exogenous shock associated with wars.

This brief survey allows one short paragraph per hundred pages and does not begin to do justice to the painstaking presentation of evidence. Each step in the chain is subjected to extensive scrutiny while the idiosyncrasies of individual countries and periods are given full attention.

At its best, the long historical perspective over a variety of countries allows convincing evidence to be presented with simple charts and tables. For example, figure 9.2 shows a scatter diagram of countries for the period 1870 to 1913 with an index of the change in equality on the vertical axis and the real wage on the horizontal. There is a clear negative relationship indicating that the resource-rich, high-wage countries experienced reduced equality as immigration depressed wages and resource intensive exports increased land rents. Figure 9.4 repeats the exercise for 1921 to 1938 and shows the relationship has completely reversed itself. Isolationism in the interwar years provided a great leveling experience in the USA, while resource poor countries like Germany and France experienced reduced equality "consistent with the rise of fascism" (p. 182).

Sorting out the relative contribution of trade and immigration to the converging real wages in the Atlantic economy requires more formal methods. High wages in the resource rich Americas were depressed by immigrants and by exports that increased land rents. Computable general equilibrium models provide the foundation for the conclusion that immigration was the dominant influence. These are summarized in the book and explained more fully in the articles where the results were originally published. The summaries of the CGE models focus on conclusions, and readers interested in the reasoning will have to ferret out the original works.

Not every link in the chain finds support in the data. For example, the opening narrative assumes that trade and immigration are complementary: fresh immigrants provide the cheap labor that augments exports of resource intensive commodities that attract new immigrants. However, the link between trade and immigration is so weak that it is generally not possible to determine whether they are substitutes or complements.

There are, of course, errors and artless passages. Their index of equality is the ratio of unskilled wages to per capita income. Higher values show greater equality as the returns to skills and property are less important. The authors incorrectly label this an index of inequality leading to the annoying pronouncement that a –1.45% change in their index of inequality for the U.S. indicates "inequality rose dramatically" (p. 176). In figure 9.4 the scatter diagram of countries has two quite distinct points labeled USA. Once the reader recognizes that the index of inequality is mislabeled, then the statement in the text indicating that the U.S. experienced moderate gains in equality in the interwar period identifies the correct point. Such errors are regrettable but probably inevitable given the volume of information presented.

The authors are careful to ground their analysis in theory and rely heavily on the specific factors model that allows three inputs; land, labor, and capital. The model is more suited to the task than the traditional Heckscher-Ohlin model with only land and labor because it allows an abundance of land in the New World to attract both labor and capital. However, the choice leads to the curious spectacle of tests of the Heckscher-Ohlin theory grounded in a model that does not bear their name. O'Rourke and Williamson nimbly explain that the key Heckscher-Ohlin insight is that relative prices of traded goods influence relative rewards of factors and it is this insight, not the details of the trade model later ascribed to them, that requires testing. In chapter four they find quite strong evidence that higher relative agricultural prices to manufacturing prices increase land rents relative to wages. The cost of abandoning the Heckscher-Ohlin model is that statements about the absolute level of wages are impossible.

The main point of the research program is to shed light on current issues and controversies. We are once again experiencing a period of greater economic integration. Tariffs have been slashed. Capital flows measured by current account

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imbalances relative to GNP are beginning to approach the levels common in the late 1800s. Inequality is once again rising and immigration is becoming a contentious issue. As before, capital is not flowing to the poorest countries, which tends to accentuate differences. These strong parallels suggest that the current emphasis on postwar data (Barro 1991) is unnecessarily limiting.

Barro's work highlights the influence of education. Those countries with strong educational systems are converging—growth is faster in the poorer countries—while those with poor educational systems are lagging further and further behind. In addition, the quality of human capital is believed to powerfully affect returns to capital explaining why some poor countries do not attract capital (Lucas 1990). O'Rourke and Williamson are unable to find evidence of the role education plays, in part because the UK and USA educational systems are so similar. However, the low level of education in Spain and Portugal clearly explains much of their relatively poor performance.

It is surprising how effective the historical perspective is in the hands of skilled researchers. It now seems obvious that capital and labor does not flow to the poorest countries because they lack some important third resource—land, oil, coal, political institutions, or education. If capital flows to resource rich countries with well-educated workers then wage differences are accentuated. Labor flows attracted to the same rich resource countries naturally depress wages leading to greater international equality. The current preferences for free flowing capital and restricted labor seems unlikely to stimulate global equality. Given that inequality in the prior period of globalization triggered backlashes in immigration policy in the U.S. and trade policy in Continental Europe, recent protests by farmers in Europe and student-worker coalitions in the USA may be just the beginning of a predictable backlash for the current period of integration.

O'Rourke and Williamson have clearly tailored their research to respond to recent ideas and controversies. I'd like to point out some relatively new ideas that they may wish to incorporate in future work. Dani Rodrik argues that the quality of institutions helps explain relative economic performance and the key characteristic is that good institutions prevent destructive infighting by special interests. Countries with weak conflict-resolution institutions try to accommodate the insatiable demands of interest groups often leading to poor macroeconomic policies in the form of inflation or overvalued domestic currencies. I believe such a thesis has ready historical application. For example, O'Rourke and Williamson take it for granted that the protection in Continental Europe springs from the greater power of landowners. Is it possible that the strength of democratic institutions in the UK is the real source of free trade in the UK? Might some of the relatively strong performance of the UK and Scandinavia be due to these institutions?

Similarly O'Rourke and Williamson consider it obvious that immigration will never play as large a role as it did historically. Yet in the U.S., the percentage of population growth due to immigration reached a peak value of 53% (1901 to 1910), declined to a low of 4.3% (1941 to 1950) and has since climbed to 33% (1981 to 1990) (Borjas 1994, p. 1668). Under the circumstances it would be useful to explain why they believe immigration will never again play such a dominant role.

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All in all, the book is a testament to the value of a broad historical perspective and is highly recommended.

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Jan Peil, *Adam Smith and Economic Science: a Methodological Reinterpretation* (Cheltenham and Northampton: Edward Elgar, 1999) pp. xii, 206, $80.00. ISBN 1 85898 919 1.

Donald Winch's short study of *Adam Smith's Politics* (Cambridge University Press, 1978) seems to have touched off a minor avalanche of *revisionist* interpretations of *The Wealth of Nations*. They are all variations on a common theme—that the father of modern economics was not a rabid, free-market ideologue. The point has been made so often of late that it grows tiresome. So it is incumbent upon new entrants to the field to convince readers they have something fundamentally new to say. This task Peil implicitly declines. In the opening chapter he claims novelty for his work only relative to "traditional" studies written before the bicentenary of *The Wealth of Nations*. According to Peil, they concentrated on recovering Smith's intended meaning, often in order to elicit support for neoclassical economic theory. More recent studies, having reached a "second stage" of illumination, worry less about Smith's intentions and seek instead to engage him in a "dialogue" on the idea of a free market economy and the theories appropriate to understanding it. Peil's professed goal is merely to continue the dialogue; he promises this "will help us rethink some of the fundamental principles of economics and consequently lead us to a better understanding of today's economic problems" (p. 13).

Chapter Two examines the context in which *The Wealth of Nations* was written. Smith belonged to an early stage of the Enlightenment, in which scientists stressed values and norms over analytical causality. He was pushed by the development of commercial society to approach traditional "ethico-political" problems from a new economic point of view. But his advocacy of free trade was still rooted in moral concerns; he believed it "would contribute to the realization of *the natural order of liberty and equity* in society" (p. 48). Smith's