

the legislation. No mention of costs and benefits and their distribution, evaluation of alternatives, externalities, the “polluter pays” principle, cost minimisation, cost recovery, optimisation. Local authorities are obliged to produce a development plan, but are not required to cost it, never mind estimate benefits or evaluate options.

Also, economists have to a large extent ignored the planning system in both teaching and research. This book is a reminder and a reproach about how deficient both the system itself and the economics profession is in contributing to a systematic analysis of design and performance in this crucially important domain.

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KEVIN O'ROURKE and JEFFREY G. WILLIAMSON, 1999, *Globalization and History: The Evolution of a Nineteenth Century Atlantic Economy*, Cambridge, MA: MIT Press, 343pp. + xii, IRE 42.99, 54.59.

Scholars create a lot of confusion when they invent a new term for something and then conflate the novelty of the term with newness in the thing it represents. Globalization is such a term. Scholars have been scrambling around analysing it as if it were a new social phenomenon. But is it just a new word? This highly acclaimed book has the admirable intent of placing globalization in a historical context. The authors are rightly sceptical of recent ahistorical accounts that treat it as a “new” phenomenon that has engendered “more” and “freer” international trade and capital flows than ever before. O'Rourke and Williamson contend that something akin to “globalization” happened before, in broad outline if not in its specifics. They support this contention by examining the Atlantic economy between 1830 and 1945. At the risk of oversimplifying, they try to show that trade is the dominant force that determines the relative wealth of nations and that the freer the trade, the more equal the distribution of wealth among countries.

The book examines two historical periods in some detail. The authors use primarily general equilibrium models to estimate the effects of “globalization” on convergence of national income levels and, especially, factor prices: urban unskilled wages and land rents. Globalization for these purposes is defined as the free movement of commodities, finance, and people. The econometric models and many of the data are explained in detail in earlier articles by the authors, so readers must take quite a lot on faith unless they are willing to do some digging.

The first period to be examined is the “globalization” of 1830-1914 (particularly 1870-1914), when commodity markets became relatively free mainly because of improvements in transport technologies. The discussion of the role of transport on commodity price convergence is quite interesting. The book convincingly argues that the “first” globalization differed from the current one because the rising import penetration of markets was transport- instead of policy-driven. Cheap transport enabled cheaper commodities to penetrate new markets and policy makers did not respond by increasing tariffs to offset the effect. Commodity prices converged.

People also moved. The book argues that they moved because of relative wages. Migrants left Europe for North America seeking a better life. Even when wages then rose at home, they continued moving because of demographic effects (the share of

population of prime emigration age) and industrialisation effects (falling proportion of the labour force in agriculture) and because of the pull of previous emigrants. As a result, urban wage rates converged between the relatively high-wage US economy and the lower-wage Western European countries. Wage rates rose in both areas, but they grew faster in labour-rich and relatively low-waged Europe. Moreover, rents rose in North America and fell in Britain, again bringing convergence.

During 1914-1940, however, increasing protection of world markets was associated with a divergence of commodity prices as well as wages. The movement of labour was impeded and regional wage levels again diverged. Protectionist policies segmented world commodity and factor markets between the two world wars. As a result, the authors argue, the world economy “lost all of its globalization achievements in three decades, between 1914 and 1945”. Yet, with respect to recent globalization, “In the half-century since then, it has won them back in every market but one; world migration is no longer ‘mass’” (p. 2).

I will just note the rather odd implication that a reified world economy “failed” to “win back” mass migration after the Second World War (surely it was the rich countries that control the world economy who did everything in their power to ensure that mass migration did not reoccur). More significant questions arise from the authors’ causal link between commodity prices and factor prices in the late nineteenth century and later in their attempts to generalise the late nineteenth-century case to a more universal link between economic liberalism and economic convergence.

The authors conclude unequivocally that “Heckscher and Ohlin were right. Commodity market integration led to factor price convergence in the late nineteenth century” (p. 74). But they only show that the two processes occurred at the same time. First they show that commodity prices converged due to rapidly falling transport costs. Then they show that factor prices converged due to rapid migration from Europe to North America. And then they say that commodity price convergence caused factor price convergence. There appears to be some sleight of hand here. It happens in their casual shift between analysing commodity-trade/prices and migration/wages and then calling both processes “globalization”, thus equating them. By this linguistic mechanism, two parallel processes—falling transport costs and migration—are defined as part of a bigger process (globalization) and are thus treated as a single causal mechanism. But do we really have causation here or just historically-specific correlation?

Even if one finds the authors’ causal link between commodity price convergence and factor price convergence convincing for late nineteenth century Western Europe and North America, it is a further jump to their more universal conclusion that Heckscher and Ohlin were right in general. Their analysis is least convincing when they move from arguing that their econometric analysis “suggests” causation between commodity prices and factor prices (for that is all such econometric models can do) to bolder generalisations that globalization causes convergence, based on something they call “the econometric facts” (p. 133)!

Such generalisations exceed the evidence presented in the book for several reasons. There are methodological questions about using urban unskilled wages to measure convergence among economies that were varyingly dominated by agriculture, and which were varyingly upgrading to skilled labour. There is a limited range of countries in the study, particularly lacking peripheral countries. “Convergence” is measured among the mainly core countries for which there are data, leaving out huge

less developed Atlantic regions in the Caribbean, Latin America, western Africa, and even the European periphery. The authors even admit that convergence disappears when Eastern Europe and, especially, the global South are included (p. 9). Yet they still conclude that globalization causes economic convergence.

As the geographical definition of the "Atlantic economy" is restricted, so is the time frame. A longer view of history might place liberal trade and migration regimes in the context of broader historical phases or cycles, rather than as an independent policy variable that, if imposed, would cause convergence.

The authors sometimes admit that the impacts of globalization and the strength of Heckscher-Ohlin type mechanisms varied from country to country. Yet they would have been more convincing had they given a more detailed analysis about how policy, institutions, and other regional characteristics intermediated to alter the effects of changing trade relations on specific national economies. One of the most interesting parts of the book is the analysis of the great migratory waves of the late nineteenth century (Chapters seven and particularly eight), because this is precisely where a great deal of variation existed. The econometric analysis indicates that migration accounted for 125 per cent of the real wage convergence observed in the Atlantic economy between 1870 and 1910.

From a more radical viewpoint, this result indicates that convergence of wages or incomes is not equivalent to convergence of developmental levels. Even while migration was causing wage convergence, other developmental factors like capital flows, technological upgrading, changing labour productivity, and capital accumulation were actually causing a net 25 per cent divergence of wage rates across the Atlantic economy. To put it more precisely, even though income inequality was falling because of an unprecedented and possibly unique wave of migration, uneven development was rising between core regions and peripheral regions. Yet O'Rourke and Williamson prefer to ignore these contradictory influences by stating that, "In theory, the forces of late-nineteenth-century convergence could have included commodity price convergence and trade expansion, technological catch-up and human capital accumulation, but in fact migration was the central force" (p. 165).

Instead of comparing the contradictory forces of convergence and divergence, O'Rourke and Williamson focus on the inconsistency of recent liberalism that encourages commodities and capital to move freely but discourages labour migration. Their solution to diverging wages after 1914 is to reintroduce free movement of labour to globalisation. They write: "The convergence power of free migration, when it is tolerated, can be substantial given the late-nineteenth-century evidence. Convergence explanations based on technological or accumulation catch-up in closed economy models miss this point. The millions on the move in the late nineteenth century did not" (p. 166). This finding is ironic since it is based on the Heckscher-Ohlin-Samuelson (H-O-S) model, which some economists have explicitly used to justify anti-immigration policies on the basis that economic convergence can result from the free movement of commodities and capital alone, without movement of labour (for an excellent critical discussion of H-O-S see Edwards, 1985).

But this discussion still confuses convergence and development. Orthodox theories, whether in economics or sociology, are primarily concerned about questions of income inequality. To the extent that they are against inequality, whether class inequality within a country or regional inequality among countries, they hope to reduce it without changing the basic economic relations of society. They hope to reduce

class inequality within capitalism through reforms that enable more fluid patterns of social mobility and fairer systems of allocating education, occupations and rewards. Similarly, they want to solve global inequality within a world capitalist system through open trade and free movement of productive factors.

Likewise, in this book the authors eschew the more interesting question of uneven development and instead concentrate on whether different trading regimes are associated with convergence or divergence of regional incomes. They attempt to demonstrate that more open economic relations cause economic convergence, where "poor countries start to catch up on rich countries" while protectionism causes inequality. But I would argue that this misses the real barriers to developmental convergence. Unequal development is not defined by uneven wages or unequal incomes *per se* but by an uneven international division of labour where the more technologically advanced and innovative activities are concentrated in core regions and more routine ones are aggregated in peripheral ones. From this perspective, wages and incomes may converge and diverge cyclically, but ultimate convergence is ruled out so long as the global division of labour is unequal. Nothing in the present book convinces me that free trade will enable technical upgrading in peripheral regions, since free trade cannot touch the protectionism that is enforced by corporations themselves through their controls of technologies.

Different core and peripheral trajectories come out of O'Rourke and Williamson's own figures concerning wage convergence during 1870-1910 (pp. 126, 155). If we subtract their estimated rise in real wages that was due to migration from the actual wage rise, we find a profound division between the core and peripheral European countries in the study. In each core country, the real wage rise during 1870-1910 was predominantly from factors other than migration, presumably from capital deepening, technical upgrading, and rising labour productivity. The proportion of rising real wages that is explained by factors other than migration varies from 78 per cent in Norway to 119 per cent in Belgium. The core countries with the greatest rise in real wages, Sweden and Denmark, underwent tremendous spurts of technology-led development during precisely this period. In each peripheral country, however, migration was the overwhelming cause of rising real wages, ranging from 81 per cent in Ireland to 211 per cent in Spain. This suggests that real wages rose in core countries due to long-term developmental factors that would be sustained over the long run, while they rose in peripheral countries because of a temporally limited outward movement of labour. In these cases, the economic fundamentals that would underlie a more sustainable long-term developmental path were almost entirely missing.

I emphasise this difference between the core and peripheral regions of Europe because the period 1870-1910 is precisely when some small European states attained a significant level of industrialisation and development. If Senghaas (1985) and others are correct, the convergence of these countries had little to do with free trade and a lot to do with policies that encouraged the modernisation and diversification of their agrarian economies; the early development of agricultural processing industries; higher wages to develop indigenous markets for basic manufactures; and the deepening of agrarian-based industry into the production of specialised capital equipment for export. The turn toward history in economic studies of globalization is undoubtedly a good thing and O'Rourke and Williamson are to be commended for their efforts to raise this issue (of course, history and globalization have been the meat and potatoes of other social sciences, and of radical political economy, for some time). Yet

this volume is ultimately unsatisfying because it concentrates on how endowed things and people are moved around the globe once they appear in a given place. But it never gets around to the real brass tacks of who produces or extracts what, how, where and why (and why not).

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BRIAN NOLAN, PHILIP J. O'CONNELL and CHRISTOPHER T. WHELAN (eds.), 2000, *Bust to Boom? The Irish Experience of Growth and Inequality*, Dublin: IPA. IRE20.00, 25.39.

and

KIERAN ALLEN, 2000, *The Celtic Tiger: The Myth of Social Partnership in Ireland*, Manchester: Manchester University Press. IRE13.00, 17.76.

Amid public debate about the social impact of Ireland's current economic boom, these two books are a timely contribution to the social science literature on the Celtic Tiger and adopt a central, though not exclusive, focus on the distributional and social class impacts of the boom. The Nolan et al., volume contains contributions from economists, sociologists, political scientists and a political economist, most of them present or former employees of The Economic and Social Research Institute (ESRI) and it marks the Institute's 40th anniversary. Kieran Allen's sociological work is an explicitly Marxist account of the Celtic Tiger and as such adds to the literature on the phenomenon from a theoretical perspective which up to now has been largely absent.

The ESRI volume adopts a more mainstream theoretical approach with different contributors drawing on new growth theory, welfare regime theory, modernisation theory, and critical theoretical currents within the new political economy to elucidate aspects of the subject. Much of its contents either repeat work published elsewhere (John Bradley and John Fitz Gerald on the changes in the Irish economy) or update data and evidence on the Irish labour market (Philip J. O'Connell); on changing class structure (Richard Layte and Christopher T. Whelan); on inequality in education (Emer Smyth and Damian F. Hannan); on earnings inequality, returns to education and low pay (Alan Barrett, John Fitz Gerald and Brian Nolan); on income inequality (Brian Nolan and Bertrand Maître); on poverty trends (Richard Layte, Brian Nolan and Christopher T. Whelan); on taxation and social welfare (Tim Callan and Brian

Nolan); on cumulative disadvantage (Brian Nolan, Richard Layte and Christopher T.