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Book Reviews

Is There Progress in Economics? Knowledge, Truth and the History of Economic Thought
Stephan Boehm, Christian Gehrke, Heinz D. Kurz & Richard Sturn (Eds)
Cheltenham, Edward Elgar, 2003, pp. 432, £79.00 hardcover
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The question, 'Is there progress in economics?' provided the theme for the European Society for the History of Economic Thought annual conference, held in 2000 in Graz, Austria. The volume reviewed here collects papers from the conference. The uniformly high quality of the papers makes me wish I had been able to attend.

The book is divided into eight sections. This was done, I suspect, to permit each section to be about the same length as the rest. In truth, however, there are only three. There is an opening section containing essays by Donald Winch and Mark Blaug that sets the tone for the rest of the volume. There is a section that provides a roundtable session featuring Roger Backhouse, Uskali Mäki, Luigi Pasinetti and Erich Streissler in which each participant offers his own answer to the question put by the volume's title. Finally, there are 17 papers that provide illustrative case studies in a diversity of fields. In this last category, for example, there are treatments of the history of economic thought broadly considered, as well as its instantiation in the theories of such diverse thinkers as Adam Smith, François Quesney and Karl Knies. Developments in theories of money, trade, location, information and normative economics are documented and then examined to see if progress has occurred. There are also analyses of specific schools of thought within economics, including the classical long-period analysis and the Austrian approach. In what follows, I will review and respond to some of the contributions in the three broad areas identified above.

Winch and Blaug take very different (though not diametrically opposed) positions on the question of progress in their opening papers, and given the centrality of the question for what follows, it is worthwhile to look closely at their arguments. Winch notes that many types of history of thought—and these include both 'presentist' histories by mainstream economists like Samuelson and critical histories by dissenters like Marx, Arnold Toynbee or J.K. Ingram—take it for granted that progress exists: they just disagree about whether developments in mainstream economics exemplify it. Winch quickly adds, though, that there are many other ways to approach the historical enterprise, not all of which focus on progress. In the end he expresses a preference for a variety of 'thick history' in which one 'eavesdrops' on the conversations of the past. I agree with Winch that 'eavesdropping on past conversations' is an apt metaphor for doing history. Typically the conversations on which one eavesdrops have been going on for a long time, so one must figure out their context, who is

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being addressed and for what purpose. Participants in a conversation often alter their positions, or even hold many positions, and at times they change the minds of those who hear their words, whether the listener be a contemporary or someone who is eavesdropping at some later time. Conversations also demand that we be good listeners if we want to understand what is being said. Winch notes finally that one does not usually impose the notion of 'progress' on a conversation, and concludes that we may be able to write more interesting histories if we abandon the leitmotif of 'progress'.

Winch's piece contrasts nicely with Mark Blaug's offering, not merely in content but also in approach. One does not really know until the final few lines of his paper where Winch stands, and I suspect that the 'Postscript' that forms its conclusion was added at the request of the editors to make his final position clear. By way of contrast, Blaug states in his opening paragraph that progress is an important question in economics, principally because economics is a science. He takes as his task an elucidation of whether and to what extent progress has occurred. Within a couple of pages we know his answer: though theoretical progress and progress in terms of the growing refinement and sophistication of our statistical tools is evident, it is much less clear that there has been empirical progress, in the sense of a better understanding of how an economy works and an enhanced ability to predict the outcomes of economic action. Indeed, Blaug goes on to document with a whirlwind survey of past and recent developments (e.g. general equilibrium theory, game theory, developments in macroeconomics) that empirical regress has often been our fate, a fact that would be more evident had not the 'disease of formalism' obscured it. Blaug pulls no punches: the claim that the Arrow–Debreu model is an improvement over Adam Smith's description of the market process is dubbed a 'travesty' (p. 26), and in case his position is unclear, is followed a little later with this: 'If there is such a thing as "original sin" in economic methodology, it is worship of the idol of mathematical rigour, more or less invented by Arrow and Debreu and then canonized by Debreu in his *Theory of Value* five years later, probably the most arid and pointless book in the entire literature of economics' (p. 27). I would be shocked, except that I found myself agreeing with almost everything that he said. The opening pieces by Winch and Blaug are, in sum, the masterful (and very representative) sorts of pieces that one might have hoped for from these two masters of their craft.

Let us turn next to the roundtable discussion. The pieces here are much shorter, but they are substantive nonetheless. Roger Backhouse in good contrarian style argues, contra Winch, that even if one does 'thick' history one may be able to speak of an 'historically contingent' notion of progress, and alternatively, that even if one focuses on empirical progress of the sort that Blaug says is absent, one may find within the history of thought some counterexamples (his is the development of our understanding of financial systems since the days of Thornton). In the end, Backhouse finds Winch and Blaug's approaches complementary rather than competing. In a brilliant exercise Uskali Mäki demonstrates just how difficult it is to come up with definitive answers to questions of progress and its measurement. He offers seven apparently straightforward measures of progress, then applies them to the phenomenon of 'economic imperialism.' He then shows how each of the criteria could be used to argue for *either* progress *or* its absence! In his contribution Luigi Pasinetti notes a variety of ways in which the subject matter of economics differs from those of sciences like physics, and then points out that the Kuhnian revolution showed that even in the latter field progress was often a problematic notion. He concludes that a cautious pluralism is probably the most appropriate stance for a field such as ours. Finally, Erich Streissler argues, quite reasonably, that a 'yes' or 'no' answer to the question of whether there has been progress in economics is itself 'unreasonable,' that the devil is in the details. Providing some of the latter, he notes how sometimes progress occurred when a past contribution was rediscovered, and that sometimes contemporaries did not even realize the importance of the contribution, in which case the rediscovery with modern tools itself constituted progress!

Given that there are 17 papers that offer further examination of the details of progress or its absence in various fields, it is impossible to offer summaries of each of them, and it would be invidious to pick only a few for review. The sheer variety of approaches taken by the authors is itself compelling evidence of the many ways to interpret the word 'progress,' and, in this reader's mind anyway, provides support for Pasinetti's plea for a cautious pluralism when it comes to assessing progress in economics.

A quick review of some of the approaches taken should demonstrate what I mean. Some authors offer criteria for measuring progress in advance, and then judge developments in a specific field against them—this is the approach of Philippe Mongin, for example, in his assessment of normative economics. Other authors examine writings from earlier days from the viewpoint of present knowledge. This approach was taken by Hans-Michael Trautwein, who looked at Karl Knies' contributions to the theory of credit. Still others looked at recent developments to see how 'new' they really were; Andrea Maneschi does this for the new trade theory, and Stephen Meardon does it for the new economic geography. Each of these approaches highlights the question of progress in different ways. Most of the approaches taken were fairly straightforward, but some papers in the volume are truly unique. One that stands out is Sergio Cremaschi's examination of how the metaphors 'chosen' by Adam Smith (I use scare quotes because, as Cremaschi demonstrates, the metaphors were not simply chosen, but themselves depended on a host of factors) led to new combinations that helped to widen the scope of his theories. Another is Jack Birner's intriguing treatment of the importance of crucial tests in economics—itself a sort of history of our thinking about how to think about progress in economics.

I enjoyed this collection a lot, not least because it demonstrated the variety of ways that modern historians of thought are thinking about both their field and their discipline. There is much creativity and novelty in the approaches. Reading the volume made me feel more optimistic about the important contributions that historians of economic thought have to make to the profession. If only the profession allows us!

Bruce Caldwell
University of North Carolina at Greensboro

International Organizations and the Analysis of Economic Policy, 1919–1950
Anthony M. Endres & Grant A. Fleming
Cambridge, Cambridge University Press, 2002, pp. 304, £50.00
ISBN 0-521-79267-3

This book chronicles economic policy discussion during the period from 1919 to 1950 at four international organizations—the International Labour Organization (ILO), the Bank for International Settlements (BIS), the League of Nations (LON) and the United Nations (UN). The cast of characters who were associated with these organizations, and whose contributions are examined in this study, include Gustav Cassel, Gottfried Haberler, Michal Kalecki, James Meade, Oskar Morgenstern, Ragnar Nurkse, Bertil Ohlin, Jan Tinbergen and Jacob Viner.

The authors are owed a debt of gratitude for undertaking eight years of research, much of which involved the examination of archival sources in Geneva, and uncovering a mountain of interesting discourse from these relatively neglected international organizations. The product is a scholarly and meticulously documented survey of policy discussions. The authors have been very careful and considered in what they say and have tried to avoid broad generalizations; but numerous themes stand out. Moreover, the discussion provides a refreshing non-Anglo-American perspective by drawing on French, German and Scandinavian sources to illustrate the ideas of the diverse and cosmopolitan group of economists associated with these institutions.

The book covers numerous topics including macroeconomic management, public investment programs, trade and international finance, and employment and social policies. In this review, we develop a subset of the themes that appear in the book, particularly those that will resonate with the modern reader.

The first theme deals with the macroeconomic policy material covered in Chapters 2 and 3. Readers will be struck by the similarity between the policy approach of the 1920s and the framework that dominates our own period. In the 1920s, economic stabilization referred almost exclusively to price level stability, namely, avoiding inflation or deflation, an idea once again popular with inflation targeting. ‘Demand management,’ the authors write, ‘was synonymous with conducting a monetary policy aimed at bringing about price stability’ (p. 70). Indeed there was considerable support for an international policy of price stabilization. The control and stabilization of price level movements was believed to reduce the risk of unemployment. How was price level stability to be attained? Countries had to establish politically independent banks and give them a clearly defined price stabilization target, allow them to develop credibility over time in achieving the target, and that would lead to employment stabilization. No conflict was perceived between price level stability and high levels of employment; on the contrary, price stability was viewed as a necessary condition for high employment. Countercyclical monetary policy would be used to stabilize employment and output at high levels. However, this stabilization scheme was based on monetary rules, using various barometers of the business cycle, rather than active discretionary policy. All of this gives the book’s historical discussion a modern flavour, though the literature examined by Endres & Fleming may be unfamiliar to modern macroeconomists.

Other elements entered into the analysis. There was a key role for the quantity theory of money and a clear neglect of non-monetary factors in the period's business cycle research. Many commentators regarded cycles as essentially equilibrating phenomena. The response of the researchers at these international organizations to the Great Depression is particularly illuminating. 'The LON and BIS researchers reacted with studied nonchalance to the onset of the economic depression of the 1930s' (p. 73) note the authors. Some argued that greater price and wage flexibility was needed to ensure recovery, while at the LON an Austrian explanation of the 1930s depression highlighted dislocations in the structure of fixed capital. By 1932, as the severity and longevity of the Depression became increasingly apparent, 'a sentiment of powerlessness was widely held in international organizations—the depression seemed intractable' (p. 37). A LON report stated that 'knowledge of the causes of depression had not yet reached a stage at which measures can be designed to avert them' (p. 37). In other words, proactive policy measures were not recommended despite the increasing calamity. There was a general scepticism about the potency of fiscal policy and public works in particular. Questions about administrative and political constraints were always at the forefront. Gustav Cassel, for example, held an extreme crowding-out view and insisted that budgets be immediately balanced. The authors present interesting material on Richard Kahn's reaction to Haberler's reservations and procrastination over fiscal expansion during this period. Only towards the middle of the 1930s did a more tolerant attitude evolve towards fiscal imbalances due to public works.

Keynesianism became the new orthodoxy in international institutions in the 1940s, but they did not accept Keynesian ideas uncritically. Before 1943, government macroeconomic management 'was expressed with great timidity and accompanied by frequent qualifications in the work of international organizations' (pp. 46–47). There was a clear move away from price level stabilization—propelled by concerns about profit inflation if productivity grew rapidly—and a recognition of the limited effectiveness of monetary policy by the 1940s. Tinbergen at the LON, for example, analyzed cycles caused by non-monetary factors and noted the need 'to prevent the development of cumulative processes' (p. 39). Yet most of the international researchers were cautious about the ability of governments to fine-tune the economy, especially with the policy lags involved. There was also a strong concern about the credibility of deficit financing and an early appreciation of inflation and balance of payments constraints. The Keynesianism that emerged in these institutions was a rules-based Keynesianism that recognized the foreign exchange constraint. This contrasted with the more conventional closed-economy Keynesian model with its emphasis on active discretionary macroeconomic management. Endres & Fleming conclude that 'A more sceptical perspective on Keynesian and "contra-cyclical" macroeconomic management had infiltrated the economic work of international organizations from an early date' (p. 53).

The book's second theme is the work on trade policy and international finance, discussed in Chapters 5 and 7. The Geneva economists remained consistent advocates of freer trade under binding multilateral freer trade agreements. However, it is the work on international finance that is most intriguing, for again it is surprisingly modern. The authors note that the 1940s gave rise to 'an array of intellectual constructs different from the Keynes and White Plans' for the reconstruction of the post-war international monetary system (p. 167). They focus on the work of Nurkse for the LON in 1944. The LON provided the background for later IMF research. Of particular concern during this time were the abnormal capital movements unrelated to economic fundamentals. Malfunctioning international capital markets and financial crisis were highlighted and it was acknowledged that disequilibrating capital flows reduced the autonomy of domestic monetary authorities. Exchange rate volatility and over- and under-shooting of equilibrium levels was seen as primarily due to short-term capital movements driven by 'market psychology', which in some cases induced a 'cumulative process of capital flight' (p. 177). Moreover, 'flexible rates encouraged disequilibrating speculation more often than not in the interwar years' (p. 178). There was a clear recognition of policy rules needed to stabilize capital flows to preserve exchange rate and monetary policy autonomy. Nurkse proposed an international buffer stock scheme so that countries could weather temporary crises. International cooperation was seen as essential to restructure the international monetary system, although distrust of government led some commentators to eschew capital controls while others were more favourably disposed.

A final theme worth highlighting is the role of the ILO sketched particularly in Chapters 6 and 8. The ILO offered alternatives to the more orthodox and conventional perspectives of the LON and BIS. Per Jacobsson dominated the BIS research effort with his belief in self-adjustment mechanisms and his reservations about Keynesianism. Similarly, Haberler's Austrian approach was influential at the LON, where researchers tended to think in terms of structures rather than aggregates. The ILO had more American institutionalist and European historical school influences. The researchers at the ILO were quick to reject the 'Treasury view' opposition to deficit spending and linked under-consumptionist explanations of economic depression with the need for public works in the late 1920s. These public works were to be scheduled against an index of the intensity of unemployment and activated when aggregate unemployment reached a certain level. The ILO was more concerned about social justice, examining various income support, social security and unemployment compensation schemes. They found standard microeconomic theory irrelevant, so they introduced social or institutional aspects of human behaviour. Endres & Fleming write that 'The idea of "involuntary" unemployment constituted the context of all ILO research on unemployment insurance' in the 1920s (p. 139). The ILO later rejected the notion that unemployment was mainly caused by excessive wages and criticized marginal productivity principles. They devoted much attention to search costs and information deficiencies in the labour market. Social security payments and unemployment benefits, they argued, acted as automatic stabilizers that complemented fiscal activism.

Kalecki's work, initially for the ILO and later the UN, is particularly interesting for he linked macroeconomic policy concerns with income distribution and relative consumption and living standards between social classes. Kalecki distinguished three different types of inflation at full employment—two demand-side and one supply-side—and examined the different distributional implications of each. In 1949 he anticipated the possibility of stagflation. One wonders what turns the discipline would have taken in the 1970s if Kaleckian Keynesianism had survived. By the late 1940s ILO interest shifted to employment problems in developing countries.

Other themes taken up in this fascinating book, though not explored in this review for reasons of space, include the problems of transition from a control-based war system to a price-based system, and the similarities with the economies in transition literature today. What one finds refreshing is that the economists at these international organizations were not fixated on formal deductive modelling. They favoured a less formal, inductive policy-oriented analysis. They were concerned with concrete economic problems and attacked them in a pragmatic way, immersed in comparative data and intensely aware of institutional differences among countries. My only real criticism of the book concerns the chapter sequencing: Chapters 6 and 8 belong together, and a more logical order would have been to follow Chapter 5 on trade with Chapter 7 on international finance and then present the chapters on full employment and social economics. Certainly one conclusion that might be taken from this study is that the present does not depart radically from the interwar period, in terms of economic management. This in a positive sense means there may be similar scope to build a robust international financial structure on new foundations to minimize global financial instability (as was done at Bretton Woods). In a negative sense it may mean that if we ignore the lessons of the past we may again experience the turmoil of the 1930s.

John Lodewijks
University of New South Wales

The Political Economy of Emerging Markets. Actors, Institutions and Financial Crises in Latin America
Javier Santiso
Houndmills, Palgrave Macmillan, 2003, pp. 268, $59.95 hardcover
ISBN 1-4039-6232-4

Booms and busts affect not only economies but also economics. The Great Depression, for example, led to a profound transformation in the discipline, ushering in Keynesianism and modern macroeconomics. In our own time, the roaring '90s gave way to crises and to more modest ambitions for the subject; now the lights, and the Nobel Prize, shine less on highly formal financial models and more on psychological, experimental or institutional economics—hopefully, for good. Be that as it may, after various disappointments, scandals and crashes, the rhetoric of economics now involves terms like overconfidence, cognitive dissonance or (my favourite) fear of regret. This readable book embraces such an approach and studies the financial history of Latin America during the last two decades, trying to understand 'how markets think' via the empirical investigation of its 'actors', hundreds of whom were interviewed—analysts, asset managers, economists and other people from rating agencies, financial information agencies and many other public and private, national and international institutions.

The author knows this world. Javier Santiso is the Chief Economist for Latin America in the Spanish bank Banco Bilbao Vizcaya Argentaria (not 'Argentina', as is wrongly stated in the flap of a book with some other, albeit less visible, errata).

Sensible and middle-of-the-road, Santiso argues that markets are neither a jungle nor a perfectly self-regulating paradise. His fellow economists are not the main culprits, and investors are far from being a homogeneous group. There are fine case studies of foreign investment in Latin America, and useful information about the role of economists in academia, the financial world and government, and about political leaders, international meetings, financial contagion, etc, in what the author aptly calls 'the confidence game'. Some of his findings are both interesting and disquieting, such as that 'negotiations and payments of bonuses take place around the end and beginning of the calendar year, exactly when financial crises take place in Latin America', or that 'dictatorships are less likely to reschedule their debts.' He points out that poor countries tend to have less political stability, a fact that can be measured, hence we find that in the last two decades 'Africa alone concentrated nearly half of all the politico-institutional changes that occurred over the period and Latin America 20 percent'.

Market failures are widespread—more widespread, it seems, than non-market ones. Santiso condemns the 'quest for short-term profitability to the detriment of long-term investment' and the 'unending race for profits.' There is something strange in his idea that this is due to the lowering of real interest rates, which had the effect of 'depreciating the future.' If this was so, long-term investment should have boomed in Latin America, as in fact it did in several countries.

Santiso's theory about states and markets contradicts his *in medium virtus* approach: 'Markets are essentially short term in focus while states are more long term in focus.' Even if in emerging markets 'herd behaviour, asymmetric information and market failures seem to be more salient than anywhere else,' there is more to the story: the prevalence of institutional dysfunction and the political abuse of property rights in those markets suggests that the 'long term focus' of governments can manifest itself in peculiar ways. This applies also to economic policies based on a real exchange rate appreciation that lowers inflation and boosts growth, but, unaccompanied by strong fiscal restraint, can inflate a bubble that eventually collapses. Although Santiso's comments on fiscal and monetary policies are few and far between, he recognizes some of these facts, but his diagnosis, as in the Mexican case, points to 'the irrationality of the markets.' He seems to have bought into the established view that if a crisis is triggered when 'economic fundamentals are sound,' then there must be something wrong with markets, and not something missing in the explanation from the political side. The argument is akin to the critique of the IMF, according to which if such a 'good pupil' as

Argentina failed so miserably, the markets should not be left alone. One can criticize the IMF, but hardly for being too liberal; it often errs in the opposite direction, as when it insists on the need for tax increases in the face of public deficits.

Finally, the author wishes to have it both ways when he acknowledges that 'the volatility and short-termism of financial markets is also possible within governments.' He seems to endorse the widely held fantasy that Argentina's troubles stemmed from excessive liberalism, a view that is contradicted by the economic policy of the 1990s, which increased taxes and public expenditure, and blew the public debt up to unprecedented levels—not to mention other illiberal measures ranging from microeconomic interventions to political, judicial and even constitutional meddling. But then he goes on to say that it would be a pity if Argentina were to return to old-fashioned illiberal models. He concludes poetically, calling for continuation of 'the unfinished dialogue between states and markets.'

What is really unfinished is our understanding. Perhaps one way of improving it would be to avoid going too far in putting states and markets on the same footing: they are essentially different. And perhaps there are no apprehensible collective entities such as 'markets that think', and no 'actors' after all. Odd actors they would be indeed, when the plays are not the same and nobody has the full scripts.

Carlos Rodríguez Braun
Universidad Complutense, Madrid, Spain

Guardians of the Nation? Economists, Generals and Economic Reforms in Latin America
Glen Biglaiser
Notre Dame, IN, University of Notre Dame Press, 2002, pp. 256, $23.50 paperback
ISBN 0-268-03875-9

Glen Biglaiser has written an interesting book comparing the performance of the countries of the Southern Cone of South America—Chile, Argentina and Uruguay—under military and civilian governments from the 1960s to the 1980s. Examples from Brazil, Peru, Colombia and Mexico are also used in the book to make some comparisons. Biglaiser, a political scientist at Bowling Green State University, challenges the 'conventional wisdom [that] suggests that military regimes provide the ideal circumstances for the attainment of high rates of economic growth in the medium and long run' (p. 2). On the contrary, Biglaiser finds 'that military regimes rarely achieve their economic goals. Indeed, the fragile democracies in Latin America and elsewhere, should take note of the lack of economic success under military or authoritarian rule' (p. ix). Biglaiser arrived at these encouraging findings through a process of exhaustive research; in addition to undertaking a thorough review of the relevant literature, he studied data from enterprises, analyzed biographical data and conducted 75 interviews (mostly with people who worked in the military governments, but also with some of their opponents).

The book is comprised of seven chapters. In the first, Biglaiser emphasizes the role of ideas in policy choice. The second chapter looks at the economic performance of Chile (1970–89), Argentina (1963–83) and Uruguay (1966–84). Chapters 3 and 4 relate some characteristics of the military and of economic policymakers to the choice of policies. Chapter 5 analyzes the process of privatization under military rule as a result of the interplay of the goals and visions of the militaries and the economists. Chapter 6 studies how economists were professionalized in the countries studied. Chapter 7 summarizes the argument and considers its implications for democratic governments.

Biglaiser's analysis, despite its many merits, has some problems. One of his central aims is to determine which economic policy better promotes economic growth. He assumes that 'a near consensus has emerged ... in favor of ... market-oriented reforms for stimulating economic growth' with minimal government intervention (p. 6), policies he labels 'neo-liberal'. But if these policies are so widely favored, and if military governments generally pledge support to them, why are they sometimes not adopted? Biglaiser's answer is that these policies can conflict with the short-term political survival of military regimes, because the policies only yield economic success in the medium-term. And although the military ruler does not face the same pressures faced by a democratic government, his decisions will be constrained by the 'fear of counter-coups, responsiveness to powerful interest groups and ... a broad range of societal and military interests' (pp. 14–15).

Hence 'economic policy making under military regimes is essentially an unintended by-product of the military ruler's strategy to retain power' (p. 57). It follows that 'technical' (by which Biglaiser means neo-liberal) solutions are more likely to be adopted by a military regime under one-man rule than when there is a high level of factionalism or when decisions are made by 'collegial rule': 'Contrary to conventional wisdom, pressure from interest groups is perhaps more intense in military regimes than in democratic governments' (p. 97). These groups are basically the domestic industrial elites fearing the challenges of a free market, and the state-owned enterprises fearing privatization.

Chile under Pinochet is the perfect example of Biglaiser's thesis. During Pinochet's dictatorship, orthodox policies could be applied *tout-court* because his power was unchallenged and because he backed a group of neo-liberal economists who were united in their views. Argentina and Uruguay, though, mainly implemented gradualist policies, and therefore didn't carry the reforms as far as Pinochet was able to. This brings us to the question of how the success of a policy is to be evaluated. If privatizations and the openness of markets are the criteria, then Pinochet's Chicago Boys were completely successful, but this argument is obviously circular. Moreover, comparing the case of Chile with the abysmal economic performance of military governments in Argentina and Uruguay only allows us to make the modest claim that Pinochet's policies were better; but it does not permit more general conclusions, to wit, that neo-liberal policies are always preferable.

Several other aspects of Biglaiser's book deserve some criticism, though lack of space prevents full development of these points. Biglaiser uses the terms 'liberal', 'orthodox', 'monetarist' and 'neo-liberal' interchangeably; while these perspectives are related, their differences ought to be kept in mind. He also neglects the fact that not all of the interest groups in Latin America supported industrialization; exporters of primary goods were strongly opposed to industrial policies, tariffs and the like, and they always lobbied in favor of neo-liberal policies. Finally, neo-liberal economists were not the only advocates for privatization; interest groups seeking to benefit from the purchase of privatized enterprises also supported it. These interest groups were no less prone to corruption than the state officials who opposed privatization because it might infringe on their own corrupt dealings. Privatization, which could generate enormous proceeds from the sale of state enterprises, opened new opportunities for corruption to state officials who favored it.

Despite these criticisms, this book is the product of serious research, and well worth reading, especially for those interested in the economic performance of Latin America.

Ramón García Fernández
Fundação Getúlio Vargas, São Paulo, Brazil

Gender, Development, and Globalization – Economics as if All People Mattered
Lourdes Benería
New York & London: Routledge, 2003, pp. 212, $ 75.00 hardcover
ISBN 0-415-92706-4

Globalization of the production process, in which parts of a good are produced in different countries, assembled elsewhere and sold across borders, has been paralleled by changes in labor contracting towards part-time and temporary workers. The globalized form of production has also been concurrent with financial and trade liberalization, structural adjustment and increased international borrowing to finance development. Lourdes Benería's *Gender, Development, and Globalization* uses gender as a central organizing theme to discuss topics in international development and the global economy. Benería's emphasis on 'all people' is consistent with her starting point that 'women's issues cannot be isolated and separated from the socioeconomic and cultural contexts in which they are immersed' (p. i).

Through an engaging review of contributions to feminist economics, Benería highlights the ways in which a gender perspective can enrich our understanding of current trends in international development such as the formation of global markets, production restructuring, employment informalization, integration of women in the labor force, and the changing patterns of gender constructions. The main objectives of the book are twofold: first, to examine how gender has been integrated into economics as a central category of analysis; and, second, to examine how development and the global economy *interact* with the social construction of gender, inequality and human welfare.

In Chapter 1, Benería invokes Heilbroner & Milberg's (1995) book on *The Crisis of Vision in Modern Economic Thought*, which emphasizes that mainstream economic analysis is embedded in a specific social order, capitalism, and has an ideological function. Benería agrees, and points out that a feminist perspective enriches Heilbroner & Milberg's critique by addressing not only capitalist institutions but also patriarchal institutions that may not necessarily change with changes in economic systems. Benería's observation that improvements in traditional economic indicators don't necessarily translate into improvements in women's lives ties into her argument that feminist thought represents an elaboration of Heilbroner & Milberg's critique of economics.

A historical overview of the gradual integration of women's issues and gender analysis into economic thought is provided in Chapter 2. This chapter, which shows the various ways in which gender was discussed in economics before a feminist influence was felt, provides an excellent starting point for research or teaching in the area of gender and economics.

Benería also addresses the relation between macroeconomic policies and gender. The structural adjustment packages of the 1980s and 1990s are revealed as gender biased. The household becomes a refuge from the negative effects of macroeconomic policies that tolerate unemployment and enforce budget cuts. Benería reviews emerging literature on the gender dimensions of international trade and finance; and cites research indicating that these effects have not been gender neutral and have had adverse effects on women inside and outside the household. Benería makes the case that macroeconomic models ought to recognize the links between productive and reproductive activities. Although Benería briefly reviews some of the literature on gender, trade and finance, her study would have been enhanced by a more thorough discussion of the international financial architecture.

The major contributions of this book are (i) the parallel Benería draws in Chapter 3 between the modern process of globalization and Karl Polanyi's analysis in *The Great Transformation* (1967); and (ii) her attempt to 'engender' Polanyi's work by arguing that the construction and growth of markets has gender dimensions. In Chapter 4, this task is given a more concrete presentation in a review of modern trends in economic restructuring, labor market informalization and the generation of precarious jobs.

One of the gender dimensions of globalization examined by Benería is the changing character of the employment contract: temporary and part-time employment have become the fastest growing components of the labor force in many countries due to an increasing reliance of firms on contingent work and decentralized production systems that increasingly involve women's labor. Benería treats these trends as elements of the micro-foundations of globalization, and she pays particular attention to their gender characteristics.

The gender dimensions of these processes are emphasized together with their connection to persistence of poverty and economic insecurity across nations. Benería argues that the links to the market have been historically different for men and women, and that 'Polanyi's analysis needs to be expanded to incorporate gender dimensions' (p. 75). She notes that, 'parallel to the deepening of market relations, a large proportion of the population engages in unpaid production' (p. 74), and that women are disproportionately concentrated in this type of work.

The importance of unpaid work is addressed in Chapter 5, where Benería discusses various aspects of the paid–unpaid labor debate and the attempts at measuring unpaid work. She points out that monetary measurement of non-paid work is crucial for showing that budgets are not neutral tools of resource allocation, but do have significant gender effects.

In Chapter 6, Benería proposes 'an agenda for human development'. She notes the need to 'design systems of social protection to deal with the negative effects of economic restructuring and globalization' (p. 168). At the same time, she also advocates 'a search for solutions from the bottom up' (p. 165), which may leave some readers wondering how comprehensive such solutions could be.

Benería's theoretical contributions deserve to be more strongly emphasized in her conclusions. Her comparison of Polanyi's 'double-movement' analysis of the evolution of labor as a commodity with contemporary changes in employment contracts under conditions of globalized production is a significant contribution in itself. But it becomes even more useful and intriguing when Benería puts it in an empirically grounded gender context by connecting it to the increased instability of women's employment outside the household, and to the gender-biased assumption behind public budgeting that production for export will be supported by unpaid, predominantly female, domestic labor.

Zdravka Todorova
University of Missouri – Kansas City

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The Future of the American Labor Movement
Hoyt N. Wheeler
New York, Cambridge University Press, 2002, pp. 257, $70.00 hardcover
ISBN 0-521-81533-9

The American labor movement is in pretty weak shape. Can it regain its strength? Many observers doubt that it can or will anytime soon. Hoyt Wheeler, who teaches management at the University of South Carolina, hopes that it will recover. He knows the task won't be easy, but pulls together a lifetime of observations to analyze organized labor's strengths and weaknesses and to offer many thoughtful suggestions for reversing the fortunes of American labor unions.

Wheeler begins by identifying 'enabling conditions' needed for a strong labor movement—workers' perceptions that the work role is important to their lives; solidarity among workers; workers' perceptions that there are distinctive worker interests; the benefits of collective actions at least equaling the cost; employer opposition being held within 'tolerable bounds'; and government support, or at least tolerance. Next he explores 'ideal types' of unions—pure and simple unionism (a.k.a. business unionism, the dominant type of unionism in the US); militant radical unionism; cooperationist unionism; social democratic unionism; and reformist unionism (which aims at the betterment of society as a whole).

This framework is then used to assess recent changes in industrial relations and union strategies. These two chapters offer an impressively informative overview of recent developments, but Wheeler concludes that the challenges facing organized labor are so great that it must go beyond its standard, pragmatic bag of tricks. To this end, he argues that reformist unionism should be given a closer look. Chapter 4, therefore, examines the Knights of Labor of the late 1800s, while Chapter 5 appraises the Carolina Alliance for Fair Employment (CAFE). CAFE originated in the work of Southerners for Economic Justice, which grew out of attempts to unionize workers at J.P. Stevens, a large textile manufacturer, in the 1970s. CAFE is representative of a range of modern organizations that look like 'the social working-class movement long dreamed of by left-leaning intellectuals and reformers' (p. 103). Its 'menu of programs' excludes direct collective bargaining but includes assistance to union organizing campaigns, protests and picketing, education of workers as to their legal rights, community action campaigns with respect to the environment and schools, lobbying and litigation. These two chapters hardly inspire confidence. It is hard to see how lessons from the Knights' *failures* can be used to strengthen unions today, and CAFE comes across as a feeble organization waning in power, with few successes to its credit.

Wheeler then turns to 'a new version of an old reformist strategy,' with a fact-filled investigation of employee ownership (especially ESOPs). He concludes that these widespread plans have a mixed record of supporting and encouraging unions and union goals, but that 'employee ownership is a strategy that has enormous potential for the American labor movement' and that 'an organizing campaign in which it is argued that the company is a good company, in fact so good that employees should own a share of it, could be very powerful' (pp. 142–143). The ensuing chapter on social democratic unionism in Europe argues convincingly that the recent strategy of making the union a full-service provider could be winning strategy. Unions could attract more interest by offering their members services such as inexpensive (or free) legal aid and tax services, especially if such activities receive tax breaks under the disguise of union dues. A chapter on Europe's Trade Union Regional Networks (TURN) is less compelling, however.

Ultimately, Wheeler argues that politics and public opinion explain why organized labor is weak in the US but much stronger in Europe. The American state isn't very friendly toward unions, while in Europe unions are generally strongly supported by the government. This makes policy prescriptions very difficult. Wheeler suggests legal changes designed to boost unionization to European levels and to hobble employer attempts to fend off unions. These include: requiring employers to provide organizers with the names, addresses and phone numbers of all employees and reasonable access to the employer's premises, bulletin boards and publications; treating employer predictions of dire consequences from unionization as threats against workers; considering use of antiunion labor-management consultants to be proof of antiunion animus; and binding arbitration when bargaining reaches an impasse. But Wheeler's proposals are dead on arrival since they have no chance of passage. His 'vision for a renewed American labor movement' within one big alliance of the left, rebuilding strong, lasting relations with the liberal wing of the Democratic Party, and the AFL-CIO in the center seems almost as probable as the Knights of Labor's failed 19th-century attempts to turn back the clock. If labor unions wish to become strong again, they need to become popular again. My reading of history is that they became popular the first (and only) time largely by accident—due to the onset of the Great Depression, which discredited business and made organized labor a very sympathetic movement.

It is ironic that organized labor isn't popular now, since it has ready access to two of the nation's most important opinion-shaping bully pulpits—the nation's classrooms (where members of the National Education Association and the American Federation of Teachers have captive audiences) and Hollywood. Yet both of these institutions have used their sway to push other causes. Indeed, Hollywood and the media may have done as much as anyone to undermine unions by dramatizing union violence (from *On the Waterfront* to *Hoffa*)—an Achilles' heel of unions that Wheeler generally ignores, just as he generally ignores that fact that public sector unions are now collectively almost as large as private sector unions, with which they do not really have much of a common interest. If Wheeler can offer viable ways for private sector unions to turn the tide of public opinion, then the rest of his suggestions might have promise. However, there seems to be no easy path for unions to shake their image in the pubic mind as anachronistic, selfish parasites.

Robert Whaples
Wake Forest University

A Critique of Welfare Economics: A Retrospective Reissue
I.M.D. Little
Oxford, Oxford University Press, 2002, pp. 302, £25.00 hardcover
ISBN 0-19-828119-6

Ethics, Economics, and Politics: Principles of Public Policy
I.M.D. Little
Oxford, Oxford University Press, 2002, pp. 162, £18.99 hardcover
ISBN 0-19-925704-3

I.M.D. Little, Emeritus Fellow of Nuffield College, Oxford, has capped his career with a reissue of his most significant work, *A Critique of Welfare Economics*, first published in 1957, paired with *Ethics, Economics, and Politics*, a review of the interfaces between philosophy, economics and politics. There is much in common between the two books, but they serve very different purposes and are aimed at different audiences.

In the *Critique*, Little considers issues that are well understood by economists, but are frequently considered to be outside of the discipline's scope. By the 1950s, due to work by Bergson, Samuelson, Kaldor and Hicks, welfare analysis was presented as positive analysis. Arrow and Debreu had established the conditions under which competition leads to Pareto optimality; and Kaldor and Hicks had separately proposed that an objective test of welfare improvement would be the possibility of welfare gainers compensating welfare losers and still coming out ahead. This is the context in which Little's *Critique* was initially published, with welfare economics seen as a positive branch of economic analysis, unburdened by subjective ethical judgment.

What was missing from this analysis was a recognition of the ethical presumptions underlying it. To adopt aggregative utility as the criterion for welfare assessment is itself an ethical judgment that is ordinarily made only implicitly. In the *Critique*, Little argued that welfare economics is an inherently ethical subject, involving valuation and comparison of possible outcomes, and not just a topic for purely positive analysis. The same arguments apply to contemporary economic analysis, which may be faulted for precisely the same false positivism. All too frequently, economists use the language of optimality, social welfare, real income, wealth, standard of living and so on, without reflecting upon either the meaning of the language or the ethical presumptions that lie behind it. As Little writes in his concluding chapter, 'This book might, in part, be described as a study of the usage of the influential and persuasive language in economics ... the ethical issues involved should be brought into the open' (p. 274).

Little's most important substantive contribution to welfare economics was his reconsideration of the criteria for evaluating changes in welfare, known as the Little Criterion. He argues that a change increases welfare if (a) it would produce a not-unfavorable redistribution of income, and (b) losers couldn't bribe gainers to vote against it without losing more than the change induces. This criterion goes beyond the conventional Kaldor–Hicks compensation criterion by incorporating judgment of distributional impacts, and allows for ranking of equally efficient Pareto-optimal states. It also explicitly brings ethical analysis into the picture by leaving the definition of a 'not-unfavorable redistribution of income' unspecified and up to the decision-maker. In this way the ethical nature of welfare economics is brought into the open.

Welfare economics is by its nature a branch of moral philosophy (as Keynes considered all of economics); while welfare analysis based on the Kaldor–Hicks criterion is often presented as positive analysis in showing the optimality of competitive markets or the welfare consequences of trade policy, for example, it is rooted in the ethical presumption that simple aggregative utility is the appropriate standard of judgment. The adoption of more nuanced versions of utilitarianism, or other ethical frameworks, based on justice, virtue, rights or care, gives rise to the possibility of a different assessment. Welfare economics is inevitably about value judgments; the Little Criterion simply accentuates this characteristic while bringing distributional considerations into play.

While there is little new in this reissue, the issues Little raised in the *Critique* are still relevant and well worth reconsideration, especially by anyone troubled by the glib use of the language of positive economics in welfare analysis. The *Critique* would be particularly useful for graduate students as a reminder of the ethical presumptions that underlie much of orthodox microeconomics. As Little concludes on p. 279, 'Economic welfare is a subject in which rigour and refinement are probably worse than useless. Rough theory, or good common sense, is, in practice, what we require.'

Many of the same themes reappear in Little's latest work, *Ethics, Economics, and Politics*, which exhibits a deeply-rooted skepticism about positivist welfare pronouncements. However, the work was written for a very different audience and with a very different purpose in mind. Little's primary intent is to survey the intersections between the three fields, but the book may also serve as a broad-minded guide for decision-making by political bodies in economic matters.

The first section, on 'Economics and Philosophy,' revisits many of the issues addressed in the *Critique*. Again, Little observes that satisfying the Arrow–Debreu optimality conditions is necessary but not sufficient for improving social welfare in economic terms. Once the distributional differences between various Pareto optima are considered, it is possible to order them using the Little Criterion.

The second section, 'Politics and Philosophy,' is in many ways the heart of the book. After considering the contractarian bases for the existence of the modern state, Little evaluates the two most commonly invoked criteria for state action, utilitarianism and justice. His treatment of utilitarianism is more broadly based than the traditional treatments of Bentham, Mill and Sidgwick, which underlie the conventional treatment of utility by neoclassical economists. Little sees room in utilitarian analysis for the incorporation of moral rules, recognition and satisfaction of claims rights, and preferential weighting of individual utilities by individuals in their decision-making, all considerations that make utility-based ethical evaluation both more subjective and more practical than economists generally suppose it to be.

While Little's discussion of rule utilitarianism is thoughtful and nuanced, his assessment of justice-based evaluation of policy is more problematic. Little is deeply—and rather unapologetically—skeptical of the language of 'justice' in all its forms, writing in the book's conclusion that 'my general position is anti-metaphysical' (p. 139). Part of the problem lies in the language itself, as justice is a concept that is used to refer to a broad range of issues, including the defense of rights (moral, natural, human, civil and otherwise); the distribution of wealth, income and access; and the procedures of political, economic and social institutions. Justice may therefore be seen as libertarian, distributive or procedural, among other things. Little views it as primarily procedural, writing that 'Justice is about protecting persons and property, enforcing contracts, and supporting important social conventions' (p. 133). With this narrow conception of justice in mind, there is little room for consideration of distributive justice or entitlement rights. It is no surprise that Little does not find John Rawls's Difference Principle or Ronald Dworkin's emphasis on 'equality of resources' to be useful in policy-making. Because he views these justice-based criteria for policy to be impractical, unconvincing and, in some cases, contrary to his sense of procedural justice, particularly with regard to property rights, Little views rule utilitarianism as the most appropriate guide for public policy by default.

Justice is often used to provide a rationale for redistributive policies, either via Rawls's Difference Principle to ensure the provisioning of 'basic goods,' or via Sen's capabilities framework. While Little argues against the incorporation of these or other justice-based criteria, he does not believe that redistributive policies are inappropriate. As noted above, the Little Criterion allows policy-makers to take account of the distribution of income and wealth in their decisions. According to Little, redistribution is justified if it leads to an increase in social welfare, not because it is just. Little's broader sense of how utilitarianism ought to apply in policy judgment is more pragmatic than the usual treatment of utility in economic analysis. But he is dismissive of justice-based approaches to social ethics, without providing a full and detailed critique of such approaches. Readers interested in a more complete evaluation of economic justice should consult Hausman & McPherson (1996), an excellent monograph on ethics and economics, especially Chapter 11 on 'Justice and Contractualism.'

The final section of the book, on politics and economics, surveys some of the issues first raised by James Buchanan and Gordon Tullock, concerning game theoretic analyses of the functioning of the government as an economic actor, the shortcomings of treating voting as an indicator of social preferences, and the development of moral, social and political conventions. Material in this section will be familiar to most economists and political scientists.

Ethics, Economics, and Politics is sometimes argumentative, especially on claims of economic justice, but it is not intended to develop and defend a single position. Its principal theme is that the best available criterion for public policy-making is rule utilitarianism leavened with a recognition that even the utilitarian criterion is subjective and subject to debate in the political process. Little concludes that there is no general way to resolve issues of freedom versus welfare, and that the assessment needs to be made on a case-by-case basis. His refreshing and sensible view that there are few unambiguous answers in economic policy debates merits careful attention from the economists who design and the politicians who implement policy.

Jeff Konz
University of North Carolina at Asheville

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The Wealth of Nations Rediscovered: Integration and Expansion in American Financial Markets, 1780–1850
Robert E. Wright
Cambridge, Cambridge University Press, 2002, pp. 240, $55.00 hardcover
ISBN 0-521-81237-2

Robert Wright is a historian of US finance and politics in the late colonial-early national period, and a prolific one at that. In 2001 and 2002, he published two other books in addition to this one, one on commercial banking, and the other a financial interpretation of key events in the creation of the 'American Republic.' The book on commercial banking, based on the author's PhD dissertation,is history as historians believe it should be written: rich with archival materials and deeply interdisciplinary. In *The Wealth of Nations Rediscovered*, Wright moves beyond history per se and applies economic concepts, especially the idea of asymmetric information problems, to early US financial history.

This book has three, somewhat competing, theses. One is that 19th century US economic growth was 'finance-led.' A second thesis, which explains the title, is that Adam Smith 'got' all of the important insights associated with the application of asymmetric information concepts to the financial system, and so did the financial and political leadership of the early national period. A third thesis is that the early 19th century US economy was a capitalist economy, not a 'moral economy,' as some have argued.

The first of these theses is dominant. The book is a contribution to a resurgent line of research in the economic history, which argues that the acceleration of US economic growth after about 1820 was finance-led. This is an argument that has been made before by economic historians such as Richard Sylla (1998) and Howard Bodenhorn (2000), and in itself is not an original contribution. Rather the contribution is to bring a historian's tools and materials to the subject, and to pack a good deal of narrative meat on the more or less bare bones of economists' empirical and theoretical work.

The central argument of the book is that there were three financial innovations in the 1790s which together made up a 'financial revolution' which was essential to subsequent successful growth: commercial banking; an effective central bank; and well-functioning securities markets. These three innovations (described in Chapter 2) together produced wealth by reducing asymmetric information, increasing the volume of savings in financial form, and so the level of real investment.

This savings-led reasoning is characteristic of this literature. The basic idea is that financial intermediaries transformed existing wealth (savings) from real (land, precious metals) into financial (bank deposits, equity) form. However, it was equally if not more important for the financial system to expand its ability to create credit, both in total and in average loan size. Only then could wealth expand at a rate faster than that permitted by the existing stock of wealth, be it real or financial. Wright's historical analysis recognizes the significance of the dramatic improvement in the 'credit scene', but credit does not make its way into the macro- theoretic story that he tells.

Regardless of the line of causality between saving and investment, Wright realizes that there is a chicken and egg problem with his information-based argument. At about the same time that financial modernization was taking place, there were also improvements being made to transportation and communication that would make financial intermediaries' access to information cheaper and faster. Perhaps the transportation revolution deserves the credit for the faster economic growth. But, Wright argues, following Sylla (1998), the financial revolution must have been the primary mover because the infrastructure improvements had to be financed. Wright stresses the point that financial intermediaries found ways to reduce asymmetric information despite the high communications and information costs of the period prior to the 'transportation revolution.' In a later chapter he argues, using similar logic, that because all three sectors of the real economy (agriculture, trade and manufacturing) depended on the services provided by the financial system, the root cause of US economic growth was the development of the financial sector.

In Chapter 3, Wright discusses the precise mechanisms which commercial banks used to address adverse selection, moral hazard and principal–agent problems in the early national period, allowing them to transcend the local, intra-familial lending of the colonial period. These include specialization, economies of scale, credit rationing and monitoring. Achieving greater scale was key to cost-effective financial intermediation, which requires the pooling and diversification of risk and the spreading of fixed information costs across a number of financial transactions. Drawing on archival evidence from correspondence between important players in politics and finance, minutes from meetings of bank boards of directors, and contemporary money and banking texts, Wright is at his best here, as he provides an abundance of historical examples that make the economic history come alive and give the reader a real sense of how financial markets worked in the early national period.

The corporate form figures prominently, but confusingly, in this discussion. Wright asserts that the corporation was both a way for firm size to grow (allowing financial intermediation) and a solution to the principal–agent problem. Although there were unincorporated private banks during this period (which do not figure in Wright's account), they could achieve equity growth only through the retention and accumulation of profit over time. As a result, the introduction of the joint-stock ownership form into banking in the early 1780s was important because incorporated banks could achieve significant scale more quickly than private banks, and, importantly, prior to the actual realization of profit (especially since equity purchases were typically financed in part through bank loans). Wright asserts that 'Joint-stock corporations ... arose partly to overcome the principal–agent problem,' and then proceeds to explain by listing institutional devices, such as requiring directors to own corporate stock and stockholder monitoring of corporate officials through general stockholder meetings (pp. 39–42). In fact, these are devices designed to address the principal–agent problem, which was *created by* the corporate form. In this sense, the corporate form was a mixed blessing.

Chapter 4 explores the important and underappreciated institutional interconnections between commercial banks and securities markets that characterized the US financial system from the very beginning. Debt and equity securities were ideal instruments for banks' secondary reserves, and worked well as collateral for loans—far superior to land, in several ways detailed by Wright. Financial assets had smaller price swings, better secondary markets, lower carrying costs and greater negotiability. US government securities were particularly useful on several of these fronts, since they had the deepest and most liquid secondary market.

In this chapter, Alexander Hamilton is the hero of the hour. In an important insight, Wright argues that Hamilton's system of funding the Revolutionary War debt simplified the stock of public debt, previously a motley array of different forms of state government debt, and so brought the information needs of investors 'within existing market and technological constraints' (p. 62). Hamilton also receives kudos for the creation of the Bank of the United States, which Wright, following David Cowen (2000), argued implemented the central banking policy of the US Treasury. The First Bank of the US stabilized the macroeconomic environment, and thereby moderated swings in security prices and stabilized the value of collateral behind the stock of bank loans.

Chapters 5 and 6 focus on the securities markets. Chapter 5 argues that these early securities markets performed well on market integration and efficiency measures. Wright presents an assortment of quantitative evidence on price differences between markets and on bid–ask spreads, but conducts no hypothesis testing. Still, any reasonable reader will be convinced that the markets were not wildly unintegrated or inefficient. Wright also presents original data on the ownership of Maine corporations showing wide ownership geographically. What is interesting here is the fact that bank stock was held much more widely, geographically speaking, than the stock of nonbank corporations. This cries out for further discussion: why were information problems, which presumably kept nonbank stock ownership more local, lower for banks? And how did this promote larger capitalization of the banking industry and greater realization of the economic benefits from financial intermediation?

In Chapter 6, Wright makes a valuable contribution to our understanding of how large the securities markets were in this period. He documents the growth in the number of brokers and dealers, and estimates the volume of trading in the Philadelphia secondary securities market using data from the account books of early securities traders. But brokers and dealers were not involved in primary security market transactions. According to Wright, 'all early IPOs ... were what analysts call today DPOs, or direct public offerings' (p. 130). Early corporations would simply advertise that they were issuing stock, and invite the public to subscribe to stock at a specified place and time. Even without investment banking services per se, Wright concludes from his evidence on subscriptions to IPOs of commercial banks and internal improvement companies that early corporations 'could raise equity capital without the help of intermediaries' (p. 134). While Wright emphasizes the large number of issues of small stock holdings to individuals, in some cases there were very large subscriptions to IPOs made by financial institutions (commercial banks, savings banks, insurance companies) and governments. In this sense, financial intermediaries were a big help to the capitalization of other financial firms and of non-financial corporations.

Chapter 7 is probably the weakest in the book. In it Wright argues that the US financial system was able to integrate and expand so well because it was unregulated or lightly regulated. (This was truer for the securities markets, which, he says, were self-regulated and transparent, than for commercial banks, which were subject to state regulation.) He provides no real argument or evidence to support this conclusion. Much of the chapter is devoted to a discussion of the Safety Fund banking system adopted in New York State in 1829. Wright's political history of the debate over bank regulation is quite good, but economic historians will find little backing for his strong conclusion that the regime was a 'heavy-handed government failure.' At the very least, the Safety Fund banking system should be acknowledged for having introduced liability insurance and supervision to bank regulation, features that would later be copied in modified form by more successful state regulatory regimes.

Wright's book is useful as a bridge between the economic history literature and the political history of the early national period. Historians will benefit from passages explaining and applying basic information-theoretic concepts to financial practices and arrangements; economic historians will benefit from the book's many detailed historical examples of these financial arrangements, drawn from primary documentary sources, which economists tend not to use. The book is riddled with original financial data and important insights into the contribution of financial markets to capitalist growth in the early 19th century US. I recommend it particularly to economists interested in finance and development, whether from a historical or theoretical perspective.

Jane Knodell
University of Vermont

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Austrian Economics and the Political Economy of Freedom
Richard Ebeling
Cheltenham, Edward Elgar, 2003, pp. 285, $95.00 hardcover
ISBN 1-84064-940-2

Richard Ebeling's *Austrian Economics and the Political Economy of Freedom* is the latest volume in Edward Elgar's New Thinking in Political Economy series. While Ebeling is an engaging writer (and speaker), there is, alas, very little 'new thinking' in this volume. The insights Ebeling provides are largely drawn from Mises, Hayek and contemporary Austrians like Israel Kirzner, although

Buchanan, Tullock and Stigler make occasional welcome appearances. The volume's ten chapters (all of which have been previously published) address topics ranging from the gold standard to classical liberal political economy and the global economy. Ebeling is the Mises Professor at Hillsdale College, and seven of the chapters were originally delivered as lectures in Hillsdale College's Mises Lecture Series; perhaps this explains the preachy tone of the work. Despite the highly disagreeable (at least to this reviewer) 'preaching to the converted' tenor apparent throughout the volume, Ebeling, to his credit, makes no pretense that the work is anything other than an attempt to draw attention to the unduly neglected insights provided by Austrian economics and classical liberal political economy. Moreover, Ebeling's erudition and his mastery of the Austrian literature are evident throughout the work. For example, how many readers will know that F.A. Hayek's 1976 proposal to eliminate legal tender laws, allowing for far greater consumer choice in currency use and thus helping to mitigate inflationary monetary policy, was originally proposed by Richard Strigl in 1932. Similarly, Ebeling shows that earlier Austrians (e.g., Oskar Morgenstern in 1934) were well aware of what we now view as public-choice type insights regarding the economic logic of politics. Ebeling's footnotes abound with many such interesting details.

One highly influential modern Austrian master is missing from Ebeling's account—the late Ludwig Lachmann. This is a serious omission. For one thing, Lachmann's insights point to potentially serious weaknesses in the modern Austrian paradigm (particularly so in the case of the more neoclassical renditions of Austrian theory). For another, unless contemporary Austrians make a truly serious effort to grapple with the thorny issues raised in the still unresolved Lachmann–Kirzner debate of the 1980s over whether entrepreneurship is equilibrating or disequilibrating, the contemporary Austrian research program appears stuck at an impasse. Kirzner argues that market processes, and the workings of entrepreneurship in particular, lead to ever-greater plan convergence. By contrast, Lachmann, who assigns central importance to radical subjectivism, uncertainty, and the passage of time, emphasizes that entrepreneurs may err and thus prove 'discoordinating' rather than coordinating. Though Ebeling reiterates the standard litany of Austrian themes—man as purposeful actor and creator, and the role of competitive processes in coordinating the plans of economic actors—he never addresses Lachmann's concerns regarding the leap that many Austrians make from the equilibrium analysis of simple single-actor choice models to the claim that real-world markets routinely coordinate plans and tend towards equilibrium. Ebeling takes potshots at mainstream economics throughout the book, contrasting the pure logic of choice, which depicts agents as purely 'passive responders' to given constraints, with the Austrian 'logic of action,' which views man as 'doer and creator of market opportunities' (p. 41). Ebeling is utterly scathing towards the pure logic of choice, and he challenges mainstream economists to explain how individual optimization based on actual, i.e. disequilibrium, prices can ever lead to overall plan convergence: If prices 'are not equilibrium prices, what is the process by which they change, and who changes them?' (p. 39). Surely, however, we can raise similar queries regarding Austrian claims that entrepreneurship is always equilibrating. Why does entrepreneurship not generate pervasive plan discoordination? The Austrian theory of the entrepreneur-driven market process seems no less contrived than the conventional theory of choice. Is Kirznerian entrepreneurship a more convincing account of the path to equilibrium—any less of an analytical fiction—than the Walrasian auctioneer's *tatonnement* process so disdained by contemporary Austrians?

In some of its variants Austrian political economy, like Walrasian theory, trivializes the heterodox problem situation, in which time, uncertainty, ignorance and disequilibrium are pervasive. Ebeling simply asserts his own faith that market processes generate a 'process of mutual feedback as all traders learn about the actions of others through the medium of prices and mutually adjust their activities to be consistent with the decisions of others' (p. 47). But no convincing theoretical explanation or empirical evidence is presented to buttress this claim.

In Chapter 3, Ebeling reiterates the hoary Austrian claim that government intervention leads to full-blown socialism, 'if the interventions are taken to their logical end.' (p. 89). Ebeling's 'if' is question-begging. It assumes that government officials would, for example, routinely set prices below market-clearing levels as ever-increasing deadweight losses rear their ugly head. Elsewhere, he repeats, without providing a supporting rationale, Mises' assertion that an inflationary credit expansion must end either in hyperinflation or 'a conscious decision on the part of the monetary authorities to halt the credit expansion' (p. 140).

The Austrian 'interventionist dynamic' seems more an article of ideological faith than a reasoned position grounded in fact or logic. Indeed, the Austrian 'economic' critique of planning (the socialist calculation argument) and the Austrian 'political' critique of planning (Hayekian 'Road to Serfdom' arguments) appear largely incompatible with one another. The calculation critique (see Ebeling's Chapter 4) contends that planning leads to welfare losses and economic chaos. The Austrian political critique of planning suggests that the logic of planning inexorably results in the creation of a totalitarian polity. But the logic of the political critique is faulty. We may grant that Soviet style 'command' planning necessitates a totalitarian polity, yet question whether a democratic socialist would try to impose planning upon a reluctant populace in the face of evidence that such a centralized system is unworkable.

The Austrians want to have their logic both ways. Austrian political economy provides no convincing explanation of why a democratic polity would succumb to the lure of planning of the kind that so rightly terrified Mises and Hayek. And if planners (of whatever stripe—'command' or democratic) are all would-be dictators anyhow, then their inability to perform accurate planning calculations is irrelevant, since dictatorial planners don't operate on behalf of the well-being of the populace anyway. But if planners are not would-be dictators, and their goal *is* to increase human well-being, and if planning is doomed to failure (as Mises and Hayek argue it is) then they'll abandon planning. Either economic calculation is largely irrelevant or the journey along the road to serfdom is contingent on a set of subsidiary hypotheses (plausible or otherwise) that the Austrians have yet to present. Ebeling notes that public choice reasoning militates against economic liberalization. But it also militates against the Misesian logic of interventionism: if economic planning reduces the wealth of existing rent recipients they have a powerful incentive to lobby against any such moves.

We can envisage interest groups like trade unions acting to short-circuit the Hayek–Mises interventionist logic.

Ebeling's sincere and passionate commitment to the normative vision embodied in classical liberal political economy is apparent throughout the book. But he never seriously engages those who might find such a normative worldview less than compelling. He never enquires why the classical liberal rules of the game might prove self-sustaining once they are put in place. Ebeling takes welfare statists and other advocates of government intervention to task for supposing that public officials are 'selfless agents of the public interest' (p. 191) rather than the self-interested agents supposed by public choice theory. But he quite happily indulges in a similar romance by supposing that people might play by classical liberal rules of the game of their own accord. Ebeling chides John Stuart Mill for envisaging the transformation of agent-type, but neglects to notice that the long-run viability of the classical liberal order is similarly predicated upon a dramatic change in agent-type.

These reservations notwithstanding, I encourage readers eager to learn what Austrian political economy is all about to work their way through Richard Ebeling's book. I certainly do not wish to suggest that Ebeling has nothing to teach the reader, though I had hoped the lesson would be more nuanced.

Andrew Farrant
Dickinson College

Prematurity in Scientific Discovery: On Resistance and Neglect
Ernest B. Hook (Ed.)
Berkeley, University of California Press, 2002, pp. 378, $80.00 hardcover
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Discoveries are labeled 'premature' when there is a delay in their acceptance by the scientific community. The extreme case of prematurity is when even the discoverer fails to appreciate the step that has been taken, a phenomenon described as 'snowblindness' by Arthur Koestler in *The Act of Creation*. The theory of prematurity that Gunther Stent propounded in 1972 could itself be called premature because it took more than two decades to arouse interest.

This book contains the papers delivered at a 1997 symposium and the 25 very dense chapters cover a wide range of highly technical material. The contents fall into three sections. The first consists of the editor's overview and a paper by Stent presenting the key arguments of his two seminal papers on this topic. The middle section is subdivided into several parts with personal reports and case studies from a number of fields. The final section consists of philosophical perspectives and closing considerations from various commentators including Stent and the editor.

Stent's examples of prematurity were Mendel's laws; the implications of Avery's 1944 report on DNA as the mediator of bacterial transformation;

Michael Polanyi's 1914–16 theory of gaseous adsorption of solids; claims for extra-sensory perception (ESP); and the 1960s claim of the transfer of memory between animals by nucleic acid extracts. His hypothesis to explain the neglect of these ideas is that the premature hypothesis cannot be connected to canonical knowledge by a simple series of logical steps. In other words there is a disjunction or mismatch between the new claim and the picture of the world that is held by other scientists. He turned to structuralism *a la* Piaget for further detail:

> In the parlance of structuralism, canonical knowledge is simply the set of pre-existing 'strong' structures with which primary scientific data are made isomorphous in the mental abstraction process. Hence, data which cannot be transformed into a structure isomorphic with canonical knowledge are a dead end; in the last analysis they remain meaningless ... until a way has been shown to transform them into a structure that is isomorphic with the canon.

Critical commentators have pointed out that this neurophysiological account of intellectual history does not do justice to the social or public nature of scientific knowledge, or to the very different reasons for the delay in recognition of the individual cases. Some of the items on Stent's list were picked up immediately by some people in the field and others, such as ESP, are of course still beyond the pale of respectability for scientists.

Some of the case studies provided by other contributors are engrossing even for an outsider. Nathaniel C. Comfort's chapter on the plant breeder Barbara McClintock demonstrates the vagary of popular accounts of science. Evelyn Fox Keller's widely read biography of McClintock put about the most widely believed version of her story, that her discoveries on the 'transposing' or movements of genetic elements in corn were met with 'stony silence', nobody understood, she was marginalized to the periphery of her field, a loner working 'virtually in isolation for over thirty years ... persisting with minimal funding. ...'

Close examination of the published records, interviews with coworkers and with McClintock herself, and scrutiny of her research notes show that the Fox Keller story is a gross distortion. McClintock was a highly regarded figure in the field both before and after her discovery of gene movements. She was never marginalized or worked in isolation or denied funding. The field accepted the discovery but did not accept some of the speculations that she pursued when she used the phenomenon as a launching pad for a vague theory regarding the control system in cells during early embryonic development. It was this aspect of her work that was not accepted, not the mainstream work that she pursued with normal funding, recognition and eventually an unshared Nobel Prize. Unfortunately she never let go of her development theory and the lack of recognition of her favorite brainchild remained a sore point for her, as recorded in the 'standard account' by Fox Keller.

We like to think that science never goes backwards, except under the kind of political control exerted by Stalin. A non-technical paper by Oliver Sacks, 'Scotoma: Forgetting and Neglect in Science', describes how several medical conditions have been discovered and forgotten over the last two centuries. One of these cases, involving migraine, which Sacks came across in a search of rare pre-1900 books, had been clearly described in the 1850s by the younger

Hershel of the father-and-son team of astronomers. Similarly Sacks found virtually nothing on Tourette's syndrome between 1903 and 1970, although the 1903 publication was the culmination of years of French research on tic behavior. In 1974 Sacks had a serious climbing accident in a remote part of Norway and for two weeks one of his legs was denervated and immobilized. He experienced the feeling that the leg did not belong to him. His surgeon had never encountered an experience of this kind (the reverse of the phantom limb phenomenon when a missing limb is experienced as present). Again he searched the literature and found nothing until he reached back 100 years and found extensive writing on both the phantom limb and the 'negative phantom' experiences, based on cases from the Civil War.

This book can only have limited interest for economists and political economists because the case histories are drawn overwhelmingly from the hard sciences and biology. The solitary paper on political science is only six pages in length; in it George Von der Muhll addresses the difficulty of establishing a fruitful shared perspective in the field after the Second World War when positivism was rampant in the so-called 'Behavioral Revolution'. Von der Muhll sketches three models of inquiry, of which only one, rational choice theory, has thrived in the study of politics. The other two are general systems theory and cybernetic theory. General systems theory achieved acceptance and prestige in economics, a situation that calls for an explanation because even its more enthusiastic adherents do not claim that it illuminates events in the real world. The answer may lie with the powerful influence of a particular conception of the nature and function of scientific theories that was borrowed from physics, as noted below.

Several of the contributors compare and contrast Stent's theory with Kuhn's paradigm theory to account for delays and difficulties that some theories experience in achieving recognition. However there is no mention of Karl Popper's theory of metaphysical research programs, which is a potentially powerful aid to explore the influence of unstated and largely unconscious presuppositions of a metaphysical or philosophical nature. This is a serious deficiency in the book and it suggests that Popper's theory may be another case of prematurity. It was published in 1982 in *Quantum Theory and the Schism in Physics* and it is relevant to the rise of general equilibrium theory because this field became dominated by ideas from mathematical physics under the influence of John von Neumann (see Ingrao & Israel, 1990). In addition to propounding the theory of metaphysical research programs, Popper criticized various key components of quantum theory, especially the concern with mathematical formalism at the expense of realism, which is a substantial part of von Neumann's legacy in economics.

Popper's ideas about evaluating research programs have more 'bite' than the traditional positivist (anti-metaphysical) philosophy of science in coming to grips with metaphysical ideas in a useful (critical) manner. The effort by positivists and pragmatists to eliminate talk about metaphysics did not get rid of metaphysical assumptions; it merely rendered their influence unconscious. Possibly the most helpful exponent of Popper's ideas for economists is Larry Boland (2003) who does not explicitly address metaphysical issues, but has however proposed a Popper/Hayek model to increase the realism of the methodological core of neoclassical economics.

This is a fascinating collection of papers that would require a polymath of the natural scientists to provide an adequate critical commentary. Neither the theory developed by Stent to describe the regulating principles of scientific innovation nor Kuhn's theory of paradigms provides any help to economists in search of a sustainable framework for analysis. Karl Popper and more especially Larry Boland may be more helpful in that respect.

Rafe Champion
Centre for Drug and Alcohol, New South Wales Department of Health

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