The general picture offered, then, is of economics post-1970 as a science distinct in terms of its tools, and able to make important contributions in subject areas shared with other social sciences. There are two reasons to be sceptical of this view, however—one from an export perspective and the other from an import perspective.

From the export perspective, it not clear that an economic conceptualization of non-market subject areas traditionally investigated by political science and sociology represents a contribution to shared topics of interest, or the creation of a parallel representation of those topics in methodological individualist and formal theory terms rejected by the majority of political scientists and sociologists. That is, the overlap between the fields might be seen in a kaleidoscopic way with competing, discordant visions of an increasingly contested terrain.

From the import perspective, although the editors—though not all the contributors to the volume—in their introduction and concluding comments minimize the possibility that the reorientation of economics consequent upon its investigation of non-market matters has influenced economists' standard toolkit, there is considerable recent evidence that this may not be the case. For example, behavioural economics, with its imprint of psychology on economics, largely rejects the standard rationality views of expected utility maximization; and Santa Fe complexity and computational approaches in economics, which have come from a variety of non-economic locations, largely reject the concept of equilibrium and calculus-based optimizing models. Thus, the editors may be right that there is a blurring of the borders between economics and other social sciences, but there is reason to doubt that economics remains as distinct a discipline as it was before 1970 and one without significant debate over its fundamental commitments.

Many of the papers in the volume, in fact, operate with this more complex type of picture. For example, Clive Granger compares the methodologies and the cross-fertilization of economic and statistics; Shlomo Maital discusses system dynamics and its emphasis on disequilibrium; Vernon Ruttan compares anthropology, sociology and political science with economics on the subject of development; Michael Gibbs and Alec Levenson explain how the labour field of personnel economics was influenced by organizational design researchers; Louis Lévy-Garboua and Serge Blondel look at how cognitive dissonance theory underlies a psychological approach to decision-making; and Dipak Gupta investigates the role of social psychology social identity concepts in connection with collective action.

In summary, however one sees the nature of change in economics, this book can be recommended to all who are interested in the evolution and development of the discipline.

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Keynes, Pigou and Cambridge Keynesians: Authenticity and Analytical Perspective in the Keynes–Classics Debate. By GERHARD MICHAEL AMBROSI. Palgrave Macmillan, Basingstoke. 2003. xii + 491 pp. £65.

The publication of Michael Ambrosi's remarkable volume brings to fruition research that, to my knowledge, has been going on for well over twenty years. The return on this investment of time is substantial. Ambrosi has established conclusively that Pigou's 1933 book, *The Theory of Unemployment*, was indeed the classical system that Keynes criticized in *The General Theory*, and that Keynes saw it centrally as just that. Ambrosi shows as well that Pigou's book was a precise and rigorous account of Marshall's analytical vision, in so far as it bore on the problems of employment and unemployment. Ambrosi's detailed analysis, his amalgam of rational and historical reconstruction—ingenious, accurate, careful, well supported by evidence drawn from a number of sources: in a word, convincing—makes more sense than any other account I know of, of the aims and approach of the difficult chapters 2 and 3 and the appendix to chapter 19 on Pigou's book of Keynes's book. Ambrosi's careful documentation also confirms one dimension of Keynes's revolutionary change in method, in that Ambrosi

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makes clear that Pigou starts from the real system and brings in monetary elements as complications and modifications only towards the end of his analysis, whereas Keynes was determined to integrate monetary dimensions—the money-wage and its setting and role, the money rate of interest and its determination and their central role—with the more traditional real dimensions of the analysis logically right from the start. This *is* a revolutionary break with the way in which Pigou and Keynes were brought up. One important byproduct of Ambrosi's careful analysis of the models underlying Pigou's book and Keynes's chapters 2 and 3 is to show that Pigou's and Keynes's results are identical at full employment, so justifying Keynes's claim that his was a general theory which took in the classical theory as a special case.

My only quibble with Ambrosi's exposition is to say that Keynes would never have claimed, as Ambrosi does on p. 119, that the marginal product of labour determined the real wage, but only that they are equal and measure one another when the firm, the industry and the economy are at equilibrium or, better, at the short-period rest state of Keynes's system. This is particularly so because both Pigou and Keynes are using a general equilibrium system exhibiting Marshall's stress on mutual determination. (I also cannot accept Ambrosi's claim on p. 17 that IS/LM is not Keynes: may I refer him to p. 173 of *The General Theory* where both the use and the limitations of IS/LM are clearly implied?) Ambrosi also points out that Pigou was, if not the first, then certainly one of the earliest pioneers of the two-sector neoclassical model, an important point noted but played down by Solow in his influential and courageous Presidential Address to the AEA in 1979. The author also puts to rest the canard that Keynes was unfair to Pigou; as he shows, if anything it was the other way around, though I doubt that Pigou was as sold on the authority of being the senior professor and Keynes a mere junior member of the faculty, as Ambrosi implies.

The first part of Ambrosi's large volume is concerned with this fundamental core. The remainder of the book is concerned with the developments of Keynes's approach and theory by his younger colleagues, Richard Kahn, Joan Robinson, Piero Sraffa, and also by Nicholas Kaldor, who was not yet in Cambridge in the 1930s but who was already an influential critic on Keynes's side in the debates that followed. Ambrosi devotes considerable space to a discussion of Joan Robinson's first attempt to extend the approach of *The General Theory* to the context of the Marshallian/Pigovian long period in 1936 and 1937. In later years she repudiated many of the ingredients of the analysis, as it depended on the long-period marginal productivity theory of distribution, including the elasticity of substitution. But it did take in Keynes's view that aggregate demand is the ultimate concept, and that different values of the mps affect the distribution of national income between wages and profits and so the average propensity to save. Ambrosi argues that Kahn and Keynes parted company on what determines the rate of interest in the short period and the long period, Kahn wanting the liquidity preference theory to dominate, Keynes willing to concede a role for time preference in the frozen never-never land of the long-period stationary state constructed by Pigou. Ambrosi shows, again conclusively, that *The General Theory* is concerned principally with discussing the reasons for sustained unemployment in a short-period setting in its own right, another important break from the Marshallian tradition of making the long period the dominating concept. And Ambrosi is surely right to argue that analytical progress in the present will have as one of its ingredients a development of Keynesian liquidity preference theory in a short-period context. (Other candidates I am not so sure about: Pigou's marginal analysis of supply conditions under imperfect competition—has Ambrosi forgotten the superior contributions of Kalecki, Tarshis and Marris in this respect?—and Joan Robinson's contribution on depreciation, the significance of which I find mystifying.)

I have reservations concerning Ambrosi's conjectures about people's motivations, based in part on a year spent in Cambridge in the early 1970s—I wonder if he had an unpleasant time? He is right, of course, that Joan Robinson was Marshallian in Pigovian form in 1936 and 1937 and neoclassical in her distribution theory. She probably overdid her later accounts of Sraffa's lack of comprehension (as opposed to lack of sympathy) with *The General Theory*. Someone as steeped in the classical political economists and Marx as Sraffa, and as critical of Marshall, would not have been overly

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impressed by Keynes's solution of the realization problem. He distrusted any use of utility—hence his critical dislike of liquidity preference and his reservations, I conjecture, about how Keynes used the concept of own rates of interest (used earlier by Sraffa in his internal critique of Hayek's 1931 *Prices and Production*) in chapter 17 of *The General Theory*.

Joan Robinson increasingly moved away from her 1930s position, so that *The Accumulation of Capital* (1956) was very much a Marx/Kalecki story, especially on distribution; and she became even more distanced in later writings as she sloughed off her neoclassical past both in theory and in method (as she saw the latter). So, while I agree that Sidney Weintraub's contributions are consistent with Ambrosi's findings, they may not have been as serious an influence on Joan Robinson as Ambrosi argues.

Before I close, I would like to point out that Frank Hahn *should* have, but as yet has not, received the Nobel prize, as Ambrosi implies in n. 4, p. 324. To sum up, however, Ambrosi's book is one of the most important sustained works of relevant scholarship I have ever read. The author deserves thundering applause for his perseverance and his achievement.

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Applied Economics and the Critical Realist Critique. Edited by PAUL DOWNWARD. Routledge, London. 2003. xii + 308 pp. £75.

The essays in this collection engage, in a variety of ways, in a dialogue with the project of critical realism in economics. The latter, which is associated primarily with the work of Tony Lawson and other economists at Cambridge University, seeks to reorient the discipline of economics towards a greater concern with ontological issues, that is towards a concern with tailoring the tools used by economists to the nature of socioeconomic reality (see Lawson's *Economics and Reality*, London, 1997, and *Reorienting Economics*, London, 2003). To that end, philosophical arguments are deployed in order to develop a rather abstract account of the nature of the socioeconomic world, which is then used to characterize the methods most appropriate for studying socioeconomic affairs. More specifically, it is argued that the socio-economic world is both *open* (i.e. displaying few stable deterministic or stochastic regularities between observable events) and *structured* (i.e. consisting not only of observable events but also of unobservable causal mechanisms, such as social structures, rules and institutions). The openness of the socioeconomic is said to call into question the usefulness of mathematical and econometric modelling, both of which, it is argued, presuppose that the socioeconomic world is a 'closed system' in which event regularities are ubiquitous (cf. N. Cartwright's chapter in U. Mäki, *The Economic World View*, Cambridge, 2001). Emphasis should instead be placed on the use of qualitative methods—case studies, surveys and the like—to aid in the discovery and illumination of the causal mechanisms that underlie and govern the flux of observable events.

The essays in the present collection represent attempts to forge closer links between the abstract, philosophical and methodological arguments deployed by critical realists and the concrete social scientific research required to discover the causal mechanisms that account for observable economic phenomena of interest. Topics addressed concern the capacity of mathematical models to do justice to the multi-dimensional, qualitative nature of many economic phenomena; the epistemological problems involved in discovering and testing hypotheses of unobservable causal mechanisms; the role of regression and non-parametric statistical analysis in applied research; and the merits of survey analysis and grounded theory. A recurrent theme is the importance of avoiding excessively prescriptive philosophical and methodological positions, and of ensuring that economic methodology and philosophy is attentive and responsive to practice. The most interesting contributions, for this reader at least, were those chapters that engaged in a systematic and thorough way with a concrete issue in applied economics, in particular Steve Fleetwood's analysis of atypical (part-time, insecure, intensive)

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