What Do Economists Do, Who Are They, and Does It Matter?

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Michel Beaud & Gilles Dostaler, *Economic Thought since Keynes: A History and Dictionary of Major Economists*, Edward Elgar, Aldershot, 1995. Pp.488. ISBN 85278 667 1

This is a curious book. It is actually two books in one. The first book is a round-up in one hundred and fifty pages of economic thought (theory?) since Keynes. The second book is a dictionary of potted biographies of 150 economists (and their major publications) since Keynes.

Beaud and Dostaler have tried to summarise and compartmentalise developments in economics since the late 1930s. This is an ambitious project, either courageous or foolhardy. The result is a mixed outcome, not surprising given the length restriction. Some sections are quite lucid (Ch.4 on the trend to formalization); other sections are scrappy (Ch.6 on heterodoxies). One aim was to make "the material accessible to the general public" (p.1). The authors could not have thought through the implications of such an exercise, which demands an accessibility necessitating a painstaking translation of technical language. Some of the exposition is so truncated that one has to be reasonably acquainted with the material to understand the story. Worse, an effort to cram in a range of dissident or marginal elements leaves some of the exposition unintelligible. This cramming process is the means by which the dictionary part of the book, mildly tolerant of diversity, is reconciled with the history part of the book, predominantly concerned with 'the central corpus of contemporary economics'.

Given these limitations the book is a very useful vehicle to infer the discipline's *weltanschauung*. If one can discern a genetic imprint in the reproduction of the species, it is the priority accorded to theory. This has a number of adverse implications, which sum to give to economists' activities an idealist character, relatively impervious to external stimuli. In effect, ideas and the institutional world of their generation are taken as more real than the world they are supposed to interpret. Five inter-related implications are apparent and will be discussed in turn - the success of economic theory in explanation; the interplay between theory and 'reality'; the understanding by economists of the policy process and their impact upon it; economists' understanding of an activist role for the state; and the relationship between economic theories and politico-economic doctrines.

First, there is a manifest self-confidence that economists' theories have unlocked the essence of complex material processes. This prejudice is neatly reflected in the career of and entry for Roy Harrod. Harrod was an indefatigable contributor to economic debates at a more tangible level, especially concerning monetary issues, but his legacy to future generations appears to rest in his contributions to growth theory. Harrod's attempt to 'dynamise' the Keynesian model was admirable, but his growth theory remained an arm-chair product, useful only as a heuristic device. Harrod, by combining multiplier and accelerator links into a model, is said to have 'explained' the trade cycle (p.97). If only the trade cycle were that simple. A 'vast literature' was stimulated, but which was content to stay within the small-scale dimensions of the original theory. Solow, Swan, Tobin and Meade, by criticising Harrod's 'unrealistic hypotheses' are said to have allowed the model to permit stable growth (p.98). This mental legerdemain evinces remarkable power, demonstrating that one can wipe out material instability at the stroke of a pen! Beaud and Dostaler claim that Harrod's model "has important consequences for economic policy" (p.266). All this is embarrassingly naive. The notion that one could adequately theorise economic instability from a simple model in which the dynamics of the competitive accumulation process are omitted is absurd. Since Harrod, growth theory has become more sophisticated, but it continues to focus (for reasons of analytical tractability) on the properties of endogenously driven predetermined trajectories.

The most profound reflection of the priority accorded to theory is in the arena of general equilibrium:

> "The model of general equilibrium aims at answering a question which has haunted economic thought at least since Smith's parable of the invisible hand: how can an order be born from the interactions within a multitude of agents, taking independent decisions, each agent motivated by his own interests? The market system's efficiency, viability and optimality are at stake." (p.70)

If one wanted to capture in one phrase the other-worldly character of economic thought, this could be a good contender. For a start, this does not represent Smith's problem - Smith was not a general equilibrium theorist. Admittedly, one of Smith's sources of good order was divine intervention, but that peccadillo was a universal weakness of the time. In general, the source of order is to be found in the full matrix of economic, political and social processes which constrain behaviour. Some of these are structurally embedded; many are self-conscious interventions. The 'market' itself is woven into these processes, constructed by a matrix of supportive and regulatory institutions, variable across time and space. It follows automatically that the problem of socio-economic order is outside the economist's bailiwick. If economists are happy to reside in a dream-world, then the 'haunting' is all of their own making.

The second implication concerns the dialectical interplay between 'theory' and perceived problems, in particular that between orthodox theory, errant theory (e.g. Keynesianism), empirical/institutional work, and new problem areas. Keynes broke away from 'classical' macroeconomic orthodoxy. Hicks gutted the most original aspects of Keynes' model, which was then taken as authoritative not merely for expositional purposes but for 'scholarly' development. Later, Franco Modigliani, who considers himself a Keynesian, had as his objective that of "integrating the main building blocks of the *General theory* with the more traditional and established methodology of economics that rests on the basic postulate of rational maximising behavior on the part of economic agents" (1980, quoted in Beaud & Dostaler, pp.82,349). Why? 'Scientific concern', notes Modigliani. All the 'new' versions of macroeconomics carry through the momentum, to give macroeconomics 'rigorous microeconomic foundations' (p.137).

On the other hand, Arrow and Hahn (in 1971), and Debreu (in 1974) have acknowledged the restrictiveness of general equilibrium theory, excluding (at the least) money and uncertainty. Hahn and others are said to have worked at 'enriching the theory of general equilibrium' by trying to make it more realistic (p.72). Stigler and others have attempted "to give greater realism to the mainstream vision of orthodoxy" by confronting imperfect information and the costs of information retrieval (p.144). Thus, "The economics of information and the economics of uncertainty were thus opened as new fields of specialization" (p.144).

Now this is a strange phenomenon. Whereas orthodox adherents have been busy roping in errant conceptual frameworks and new arenas (for example, environmental policy), others of their brethren have been acknowledging the profound weaknesses in the orthodox vision, necessitating changes to make the conceptual framework 'more realistic'.[1] On the one hand, the theorisation of elements of uncertainty (as in Keynes) is being emasculated; on the other hand, the theorisation of elements of uncertainty (extending neoclassical theory) is being introduced. Evidently, the versions of uncertainty differ, a point highlighted by the authors (p.137). Given this anomaly, the argument that one is aiming for increased realism by introducing 'uncertainty' into a model, of a particular type that does not threaten deterministic outcomes, needs to be taken with a grain of salt.

Complementary to this devious process is a certain indifference to those who toil on the margin of the discipline with institutional and empirical detail, attempting to build non-orthodox conceptual frameworks. Post-Keynesians insist on crediting Kalecki with the discovery of monopoly pricing, even though American Institutionalists had been discussing it for some time, and with greater sophistication. Industry economists, economic historians and sociologists have left some formidable literature on the business enterprise. Yet Ronald Coase and Oliver Williamson, for example, have established considerable reputations for what are important but rudimentary and over-stylised ideas, with hardly a concession to previous toilers in the field.

The third implication concerns the understanding by economists of the policy process and their impact upon it. Economists claim for themselves too substantial an influence on economic policies and on economic outcomes. The most dramatic reflection of this delusion exists within the Keynesian fraternity - that the acceptance and application of Keynesian principles of demand management were responsible for the fortunate years of the Post-World War II boom. The presumption is a myth (c/f Jones, 1988; 1989). The boom was produced by historically-specific circumstances - at the global level centred on *Pax Americana*; at the national level a multiplicity of structural interventions by governments. Demand management was complementary to the success story but not decisive. The macroeconomic policy apparatus itself was an arena subject to multiple influences.

Beaud and Dostaler present an extraordinarily ambiguous argument about the impact of Keynesian theory on government policy - is the attribution of the label 'Keynesianism' to the era substantive or symbolic? The authors ultimately bring down a positive conclusion but with much attendant confusion. The ambiguity is immediately transparent over interpretation of the early policies directed to combat the Depression in the U.S.A. and in Sweden. Such policies had nothing to do with Keynes or Keynesian policies, yet the authors give them an honorary Keynesian label (pp.16;40). It's a good thing the authors didn't try to pin the 'Keynesian' label on recovery mechanisms being pursued successfully in Hitler's Germany! The ambiguity continues in interpretation of post-War developments, although the authors do acknowledge more complex origins of policy motivation than is typical for economists (p.45;54). Fortuitously, the authors ignore developments in the international arena, in which Keynes was soundly trounced by the Americans over the terms on which the IMF and the World Bank would be constructed.

As hard evidence for the substantive impact of Keynesianism and of the triumph of 'economic science', the authors cite Walter Heller on his experience as Kennedy's Head of the Council of Economic Advisers in the early 1960s (p.56). One might deem it a reasonably safe bet that getting the story from the horse's mouth would provide the definitive interpretation. However, Heller's story claims too much influence for his own vision and role. The macroeconomic policy contribution during this period came not from increased spending (which advisers such as Galbraith hoped for) but from tax cuts. There might now be a Keynesian conceptual interpretation for the broader beneficial impact of tax cuts, but the decisive influence came from the business community which had in mind its own specific beneficial impact. Tax cuts favouring business were also made during the Eisenhower years, and that was an era of explicit repudiation of Keynesian principles. Increased government spending did come a little later, but it was within the 'Great Society Program' and was dictated not by pump priming considerations but by the federal Government pragmatically buying its way out of a racial civil war. Heller gets his labels wrong - he claims that the 1960s in the USA represented the "completion of the Keynesian Revolution" (quoted p.56); and he also gets his history wrong - he infers that the seed for Keynesianism was sown with the Employment Act of 1946. On the contrary, the Employment Act was conscientiously gutted of any Keynesian influence (c/f Apple, 1980).

The authors' interpretation of the period deserves to be quoted at length:

> " ... the 1950s and 1960s constitute the golden age of economic policies. With or without reference to Keynes, more or less interventionist, based on the structural transformations or more centred on the subtle management of economic fluctuations, it is indisputable that these policies had Keynesian components. The strategic variables were the aggregate quantities; the stress was on demand and its components (investment, consumption, government spending), with intervention on public investment and the determinants of private investment, income distribution (in particular through budgeting and redistribution) and public finance." (p.57)

It would be hard to find a country for which this is an accurate characterisation. In the case of the U.S.A., the subject of discussion immediately preceding this paragraph, the 'strategic variables' were not aggregate quantities; they were associated with the development of a military-industrial complex. There is abundant literature on this subject written in English by non-economists; it is available in the libraries to which economists have access; and it is a pity that it has not been accessed. The expression 'Military Keynesianism' was coined pragmatically to transcend this interpretive conundrum, no doubt for people who insist on having the label 'Keynesian' applied to policy processes for which it is only vaguely related.

In general, the treatment of the relation between Keynesianism and the post-War boom (Chs.2 & 3) is an incoherent mixture of what economists thought, what governments did, what economists thought that governments did, what economists thought that governments should have been doing, and what economists thought of their own role in what governments did.

The ambiguity comes home to roost in the reproduction of glib conventions regarding the decay of the boom and of the status of Keynesian theory. "The coexistence of inflation and a rising unemployment rate challenged the certainties associated with the Phillips curve and symbolized the failure of Keynesian policies." (p.113)[2] The key word is 'symbolized'. Stagflation was merely an excuse for the attack on Keynesianism. The Phillips Curve is still with us as a key Central Banking weapon. Keynesianism didn't produce the long boom, so the demise of the boom can hardly be responsible for the demise of Keynesianism.

The fourth implication concerns economists' understanding of the role of the state. The authors' claim for the post-War period as a 'triumph of interventionism' (Ch.3) is seriously misleading. The historical literature on the evolution of capitalism naturally differs on the role of the state, but the evidence is overwhelming that the state did not 'twiddle its thumbs' before 1945. On the macroeconomic policy front in England, for example, the key institutions with a modern brief (Treasury, Bank of England) were set in place by the late seventeenth century. By the late nineteenth century, the Bank and the Treasury were running an active agenda. Indeed, the Bank was run off its figurative feet as both national central bank and global central bank - in its latter role furiously propping up the Gold Standard, whose operation was far from automatic. This institutional cabal, against the odds, returned the pound in 1925 to its pre-War level, with dramatic implications for sections of the British economy. All this activity can hardly be called inaction.

The authors' use of the label 'interventionism' is evidently intended for a specifically Keynesian meaning - the appropriation of macroeconomic instruments to be used assertively for more 'progressive' ends. Yet it is not a matter of interventionism or not - the *laissez-faire* state is a contradiction in terms. Rather, it is a matter of what kind of intervention and for what purposes. It is not inconsequential that the macroeconomic instruments after 1945 were not steeped in the Keynesian ethos but became a site of 'contestation'. Even in Britain and in Canada, Australia and New Zealand, where there were bureaucrats sympathetic to Keynes' vision, macroeconomic policy had still to be effected through the institutions which had been in the possession of the pre-Keynesian macroeconomic establishment. Inflation readily became a concern of the macroeconomic policy-makers in the 1950s, before the Phillips Curve became a staple in the academic literature. Much ink has been spilt over what priorities might reasonably be labelled 'Keynesian' and what Keynes' personal priorities would be were he to be still around. These debates are idealist and pointless. What matters is that shifting macroeconomic priorities reflect shifting values and interests, and not the evolution of considered expert opinion according to some abstract notion of the public interest.

So much for the macroeconomic sphere. If economists have trouble interpreting the macroeconomic sphere, their difficulties are compounded at the structural level. We don't even have an appropriate label for the matrix of components and linkages at the sub-aggregate level. 'Microeconomics' serves as a surrogate label, but it is thoroughly unsatisfactory, because its conceptual baggage is overly abstract and its ideological baggage is strait-laced. This baggage has dominated the discipline's mental space, such that the role of the state at the structural level has the epistemological status of the contents of a black hole - it simply cannot be grasped.

Inside the economists' black hole, the state has been a constant midwife in the development and sustenance of national capitalisms at the structural level. Yet here we have two economists (one of whom has written a book on *A History of Capitalism* (Beaud, 1983)!) claiming that the triumph of interventionism occurred after 1945. Again, the authors appear to be generalising from the British experience to which Keynes' work and strictures were addressed (an inverse Chauvinism?). Yet the British experience was unique, and Keynes himself was a selective observer. The peculiar role of the British state in economic and industrial policy cannot be inferred from the works of economic theorists.[3]

The fifth implication concerns the relationship between economic theories and politico-economic doctrines. At the simplest level, one has the competing doctrines, crudely and incorrectly labelled, of 'free markets' and 'interventionism' (p.2). Of concern here is the presumption that general lessons for the role of government follow logically from the results of economic analysis. Keynesians have fought it out with von Hayekians or with Neoclassical revisionists to demonstrate that the superiority of their analysis implies a scientific foundation for a particular doctrinal position. Keynesian and Monetarist econometric model builders fought it out over claims of greater scientific legitimacy for particular macroeconomic policy structures, in turn linked to particular doctrinal positions.

The dark side of these debates is the presumption that one's own analysis is value-free - that the causal nexus runs from theory to doctrine. The reality, of course, is that theory construction is informed by the individual's ideological predilections; politico-economic doctrines pervade what we are happy to call 'theory'. The authors, in a perceptive passage, acknowledge this mutual determination (p.43). That the internal debates take place along these lines indicate much self-deception as well as self-indulgence. A little more honesty in the discipline might allow its members to spend less time shadow-boxing in the form of theoretical disputes and more time confronting head-on the nature of their doctrinal disputes. In the process, the character of such doctrines and the influence on the development of theory itself might be themselves examined and better understood. The outcome of such a shift of emphasis would be an inevitable down-grading of the status as 'science' which theory-building now enjoys.

In general, what does the profession appear to be up to? The 150-page summary is a useful window. There is the core deference to neoclassical and general equilibrium theory; but we knew that already. Of greater interest is the interplay between the core and the 'real world'. This is difficult to pin down, but what emerges is an arena of incoherence - something of a mush. The enterprise involves the joint attempt at maintaining conceptual purity while simultaneously flirting with realism. This is a contradictory exercise, so that 'realism' is of a contrived sort; hence the mush. All professional activity takes place with one foot, as it were, tied to the ground, and that tie is the core of respectable theory. This is a world in permanent intellectual tension yet at the same time a world of substantial intellectual security which ensures its continuity.

So much for 'central corpus of contemporary theory'. Beaud and Dostaler's second 'book', the dictionary, exposes us to the 'key' personnel, and allows one to complement the picture painted above. The dictionary adds to a considerable and expanding literature on the thought of living economists. Until recently, economists were generally given to anonymity, apart from significant exceptions like Keynes and Galbraith. Now it is open season on economists. This veritable orgy of biographical cameos is something of a mystery as most economists appear to be not very interesting people.

As the authors admit, the cast is mostly English-speaking, and mostly 'American'. Ironically, "more than a third of those American economists are of foreign origin, coming in particular from Eastern Europe". So American economics might be hegemonic, but it is a polyglot product. This foreign-born group comprises a diverse set of individuals - from pillars of intellectual respectability (Gottfried Haberler, Fritz Machlup), through people who worked on the respectable margins (William Fellner, Richard Musgrave, Jacob Mincer, George Katona), the econometricians (Tjalling Koopmans, Jacob Marschak), the system builders (Wassily Leontief, Simon Kuznets), development economists (Bela Balassa, Albert Hirschman, Ragnar Nurske), to intellectual dissidents (Nicholas Georgescu-Roegen, Paul Baran; later Hirschman).

The French (and French-Canadian) background of the authors brings to the list a cosmopolitan flavouring. Naturally, there is a French contingent - Maurice Allais, Alain Barrere, Marcel Boiteux, Claude Gruson and Francois Perroux (more are discussed in the text). There is also a handful of perhaps unexpected inclusions from elsewhere - the Indian Krishna Bharadwaj, the Russians Vasili Nemchinov and Viktor Novozhilov, the Japanese Shigeto Tsuru, the Australians Colin Clark[4] and Geoffrey Harcourt, and the Brazilian Celso Furtado. There is also included a large contingent of radical and Marxist-inspired individuals - Samir Amin, Paul Baran, Charles Bettelheim, Suzanne de Brunhoff, Maurice Dobb, Andre Gunder Frank, Ernest Mandel and Paul Sweezy. The French have a greater tolerance of radical economic theory; in Anglo-America, most of this list would not get a guernsey as economists, leave alone make it into a list of the top 150. The authors appear to have gone beyond the demands of tokenism with the inclusion of this substantial coterie. Women constitute a grand total of five. The development economists Barbara Ward and Ester Boserup, spanning the generations, might have been included without any concessions to tokenism. Samuel (aka Sam) Bowles is included in a transparent tokenism, representing the entire (and disparate) grouping of 'radical political economists' that was spawned in the USA in the early 1970s.

There are some occasional curiosities and light-hearted moments which leaven an otherwise ponderous book. Abba Lerner liked building mobiles, and left them behind when he moved on. William Baumol was a wood sculptor and lectured on this art at Princeton. Fritz Machlup directed a cardboard-manufacturing company in Austria for a decade before he had to flee. It is rather ironic, with this tangible experience behind him, that Machlup became a staunch defender of marginalism against 'realist' attacks, reinforcing the neoclassical firm at the core of the discipline. Machlup's directorship must have been at some distance removed from the shop floor, more's the pity.

It would be nice to have discovered more about the 'whole' individual - any worldly experience that might have influenced economists' intellectual (and implicit political) orientation. We are told, for example, that Albert Hirschman "fought with the French army (1939-1940)", but not that he was a member of the Resistance, risking his life ferrying people through France to the Spanish border to escape to freedom. Did this exposure to the depths of the human condition influence his future? We are also surprised to read that Robert Heilbroner "worked in management and the business world" but are not treated to any insight on its significance.

We are getting warmer when we are informed that Walter Heller developed his macroeconomic policy activism after "[h]aving been deeply affected, along with his entire generation, by the Great Depression and the New Deal". This is an interesting generational phenomenon. Galbraith, of course, was profoundly influenced by his apprenticeship research into the Depression's adverse structural impact on the agricultural sector. The book notes Galbraith as having been affected by the 1929 crash, by the New Deal, and having worked in the Office of Price Administration (in which he gained an intimate feeling for the mechanics of corporate pricing), but curiously omits to note the relation between Galbraith's beginnings in agricultural economics and his later frame of mind.

The authors, however, overstate the case regarding the impact of the Depression - many economists remained staunchly libertarian; many economists simply ignored it. The Depression affected Walter Heller, but the next entry in the book is for John Hicks, who was from roughly the same generation (ten years older), and who appears to have lived a life entirely of the mind. Hicks is credited with being "one of the most influential economists of the twentieth century". One fears that this is an accurate evaluation. Hicks would appear to be a role model for professional development and advance (even including his latterday reservations about the worth of his past). Certainly, the generation which invented new classical macroeconomics has picked up the baton (though one imagines that their hardline stance in the prime of life presages no future recanting).

The institutional cloistering of intellectual activity since 1950 offers substantial structural reasons for a proliferation of the Hicksian prototype. Academic economists, of course, do venture out regularly into the 'real world' but, in line with 'the primacy of theory' which is their battle-cry, the interaction is predominantly one-way. The academics give the policy-makers the benefit of their theoretically-derived expertise, denounce the unsavoury imperfection of the real world, collect their consultancy fee, and repair quickly back to the cloisters convinced of the indubitable correctness of their opinions. Such has been the case with the Anglo-American (but mostly American) condescension towards the 'third world' and, latterly, the decayed eastern bloc, and the arrogance with which it prescribes the appropriate medicine for its speedy development.

A previous generation had the misfortune to live through World War. Some compensation existed in the opportunity to acquire some tangible knowledge and skills. The contribution of economists to War (and immediate post-War) administration and the impact of the War on economists and economics has never been adequately examined. Some of those listed here were in war-time administration and post-war clean-up - Walter Heller, Richard Kindleberger, Simon Kuznets, James Meade, Richard Stone, and Robert Triffin; and in other countries, Celso Furtado, Claude Gruson and Gunnar Myrdal. The work of Triffin, centred on international finance, was clearly influenced in the long term by his bureaucratic experience, as was that of Kindleberger, albeit briefly. The work of Kuznets and Stone, centred on information systems, were clearly influenced by the demands of war and applied to the demands of a prosperous peace.[5]

In this regard, Meade was an important figure in that he held a senior administrative position during this significant period. Again, this experience is noted merely in passing. Not noted is Meade's participation in the difficult negotiations leading to the establishment of the General Agreement on Tariffs and Trade. The construction of GATT was a masterpiece of fusion between principles and *realpolitik*. The dialectic of and balance between these two forces ought to be the subject of serious examination by historians of economics. This book hints at the relation, as noted above, with respect to the broad issue of Keynesianism and its material influence; but too much attention is paid to principles in the form of high theory and not enough to the material context which has its own imperatives and in which the principles might or might not be relevant.

Ultimately, the 'worldly' economists themselves may have contributed to this neglect of their worldly interaction. Of course not all life experiences and their impact are appropriated and understood self-consciously. More significant, the individuals concerned may have been happy to escape from public duties, cut the linkages and return to a purer arena of intellectual activity. This appears to have been the case with Meade, whose later work shows little trace of his worldly experience. It was certainly the case with Austin Robinson (not included in this volume) who, in spite of long periods in active public life, was inclined to take most pride in his contribution to the development of the theory of the firm (Robinson, 1992). Due to what appears to be rather perverse priorities, future generations are not privy to the lessons of his bureaucratic life. One faces the possibility that a large part of the isolation of economists' professional life is a product of personal preference. Such preferences act in mutual determination with disciplinary socialisation processes to entrench and reproduce an essentially other-worldly intellectual tradition which appropriates rather than accommodates external stimuli.

Why then should we be interested in economists and in their writings? The answer appears to be that we study select individuals because they have acquired status in the profession. Each particular selection will exhibit eccentricities, as does this one, but there is an implicit preference for the 'theorists'. Behind the selection is not merely a self-validation of the profession's priorities (we have made them famous so they deserve to be famous) but a presumption of external validity - that is, scientific advance. The creation of a pseudo-Nobel prize in 1969, and the annual publicity associated with its awarding, have added to this presumption of scientificity.

As there are no generally accepted principles as to what constitutes scientific advance in the social arena, one is left with an uneasiness as to the precise origins of the status accorded to select individuals, and even to the entire project. Does the economics profession deserve this attention? Some uncertainty about the worth of all this endeavour comes out of the mouths of the *glitterati* themselves. Frank Hahn, with forty years of prestigious theoreticist publications to his credit (in the words of the authors):

> "holds that there remains an immense gap between the theoretical fields, in which scientific rigour is applicable, and practical economics, in which the economist still has little to say as a scholar. ... [Hahn] questions the status of macroeconomics which, although necessary, notably for economic policy, currently remains without theoretical grounding and requires reconstruction. This reconstruction can only be based on a dynamic theory of general equilibrium that would integrate time, money and growth, a goal that seems remote at this point." (p.259)

One might reasonably inquire as to the use of concepts of 'scientific rigour' and 'scholarship', the pursuit of which forbids its adherents from having much to say of a practical nature. This gap between 'scholarship' and practical utility goes to the heart of the matter. Is respectable economic theory relevant, and if not, why not? In spite of Hahn's acknowledgment of the separation, and his admiration of Keynes for the practicality of Keynes' theorising, Hahn ultimately plumps for the continued dependence on an unrealisable ideal. In effect, we can have general equilibrium or we can have nothing.

Similarly, Ragnar Frisch is claimed to be "highly conscious of the scholar's social responsibility. In his view, econometrics must, in order not to engage in futile games, stay in touch with practical realities", and is said to have been "constantly keeping in mind the practical questions of economic development and of development planning" (p.240). Trygve Haavelmo, as early as the 1950s, expressed concern with econometrics, drawing attention to "the dangers of developing a technical expertise devoid of both coherent theoretical foundations and fruitful links with reality" (p.254). Haavelmo is said to have become interested in 'more concrete questions', such as economic development and its attendant income disparities, as well in demography, migration and education. Kenneth Arrow "has frequently insisted that mathematics is a tool that cannot take the place of economic reasoning, himself underlining the abstract and limited character that many have critically attributed to the general equilibrium theory. He has also drawn attention to the importance of history, and of the variability of economic and institutional conditions between eras and countries" (p.171).

All this criticism by the masters of central tendencies within the discipline is admirable but it is of little effect. The contributions of these people to a broader understanding which transcends the theoretical contributions for which they have become famous (and represented in this book) have been modest or non-existent.[6] What the discipline's gurus say is of far less significance than what they do. Ironically, some of the profession's big names have been energetic in movements lobbying for the demilitarisation of American politico-economic priorities. Yet these efforts (and no doubt others) have stayed within the personal realm and have not fed back into the academic persona of the economists concerned.

If one can readily generate disquiet with the priorities of those who have acquired the status of 'major economists', it seems reasonable to inquire of the ground rules which allow people into the fold in order to qualify for selection. Who is allowed to be called an 'economist'? One needs to get beyond trivial and circular criteria such as the necessity for possessing higher degrees in economics. In any case, several *eminence grises* on this list fail this test - Friedrich von Hayek, Gerard Debreu, Tjalling Koopmans, John von Neumann, and Jan Tinbergen. Apart from von Hayek, who obtained his formal training in related social disciplines (law and political science), the others came from mathematics and/or physics, backgrounds which seem to have been accorded special favour when the economics discipline considers refugee applications.

If one chooses to do work on concrete issues, one will probably be admitted into the fold if there is a touchstone of respectable theory, but one's chances of acquiring 'major' status are slim. In this volume, the important field of industrial structure and business regulation is represented by Joe Bain, George Stigler (partly) and Oliver Williamson. In the arena of labour, this volume includes one individual, Henry Phelps Brown. Jacob Mincer and Gary Becker make it into the list because of their having fathered human capital theory (with Theodore Schultz). For all the attendant empirical work, however, human capital theory is an *a priorist* imposition of neoclassical axioms on an enormously complex arena in which such axioms are singularly inadequate. The complex contributions of Institutionalist economics to the study of wage labour, a field they once dominated, has been pushed to the far margins of the discipline. Other arenas of significant public importance are sparsely represented, though one can hardly blame this volume as it merely reflects the discipline's prejudices. Taxation warrants one entry, Richard Musgrave, now retired. Social welfare (other than as a byproduct of development economics) warrants no entries at all.

If one chooses to work on economic policy issues from a broad institutional/historical perspective, or if one chooses to be employed within the policy arena, the chances of being accorded recognition as an 'economist' are slim indeed. These broader criteria would open the list to many other candidates. This author would nominate three individuals as representative of a broader tradition - William Beveridge, John Blair and Andrew Shonfield. Beveridge (as leader of a cooperative endeavour) produced the landmark *Report on Social Insurance and Allied Services* (1942) and the omnibus *Full Employment in a Free Society* (1944). If one wanted a symbolic label to characterise the post-War era (at least in Britain), it might very well have taken Beveridge's name rather than that of Keynes. Blair authored the monumental *Economic Concentration* (1972) and the equally monumental *The Control of Oil* (1977), and was a long-time toiler in the U.S. Federal Trade Commission and the Senate Subcommittee on Antitrust and Monopoly. As a self-conscious Institutionalist, he made no concessions to orthodox theory and effectively disenfranchised himself from inclusion in any list determined by contributions to 'theory'.[7] Shonfield's *Modern Capitalism* (1965) is a masterly comparative work on post-World War II economic structures (if anything flawed by the author's concessions to the economist's imagination). *Modern Capitalism* is worth half a dozen works by half a dozen of some of the economists included in this volume. If the authors had read Shonfield their own understanding of the post-War period would have been greatly enhanced.

Is the nomenclature of 'economist' to be defined in terms of a particular conceptual toolkit(s) or in terms of the subject matter? The former is the implicit defining aspect, but it is not unreasonable to include the latter criterion. As with applied economists and policy-makers, there are myriad people usually labelled sociologists, international political economists, historians, etc., who work on 'economic' subject matter. One might even hazard the generalisation that such people have contributed substantially, perhaps in a superior fashion, to an understanding of economic processes - the authors themselves flirt with this notion (p.153). Why limit the nomenclature to a constrained range of conceptual approaches (essentially to those working within the profession)? To transcend this limit, however, is to open a pandora's box. What are the questions? What is the terrain? What are the ethical questions? What are the economic and social 'problems'? Naturally, there are fundamental disagreements on these fundamental issues. Ultimately, it would be more honest to confront these issues explicitly than to hide them by artificial delineations. The authors, admirably, confront the dilemma of 'adjacent specialists' but hastily put it aside - "We chose not to open this door, for fear that we might not be able to close it" (p.4).

The authors have given the reader an opportunity to peruse conveniently the mass of publications of 'the greats'. One can't help but be impressed by the scale of the collective enterprise and somewhat numbed by the aggregate of the individual energies which has produced this edifice. At the same time, however, all this compressed evidence generates the nagging question of whether the game has been worth the candle (the authors evidently had the same reaction). Have these contributions improved our understanding of how the system works? Have they improved material living standards or equity in any part of the globe?

One would have to conclude that an air of unreality pervades the edifice. There is a detachment from that 'real world', in all its manifest complexity, from whose interpretation the discipline is supposed to draw its legitimacy. This detachment is a natural consequence of the convention that 'one foot shall be tied to the ground'. Evident lifelines are eschewed - there is an indifference to the broadening lessons of institutional detail and of historical processes; an indifference to the strugglings of other social disciplines to deal with the same totality. Those who have ventured into alien territory and returned with lessons are, at best, tolerated at the margins. What influence have the polymaths (even those listed in this volume - Boulding, Georgescu-Roegen, Myrdal, etc) had on the central tendency of the discipline? Not a jot. There is more than a whiff of a discipline overly self-indulgent, permeated with the 'boys in short pants' brigade.

Academia exists as one of the luxuries of a civilised society. Academic freedom necessitates the provision of a space whereby boffins may reside and do their work unimpaired. Yet it is quite another matter to let the boffins take over a discipline; more, to let the boffins run amok with policy advice in a world of which their knowledge is, at best, sketchy.

This isolation has been defended as the cost of building a rigorous science, as the Frank Hahns would have it. Yet the organic link in the natural sciences between high theory and its subject does not exist in economics, for predictable reasons. From the beginnings of systems-building in economics, the theorists have been happy to steal the analytical forms from elsewhere and live with the messy consequences. This methodological monism has been variously and shiftily defended by a set of ambiguous conventions about the relation between 'theory' and 'reality': theory captures the essence of reality, and particularistic conditions merely need to be added to ensure its direct relevance; theory isn't realistic of necessity; theory isn't realistic, which is unfortunate, but we are working on ways to make it more realistic; theory is realistic *per se*; and so on. The nexus between theory and 'reality' remains an inexcusable arena of fudging. It is not accidental that considerations of appropriate methodological principles are treated so cavalierly within the discipline.

After having perused the intellectual lives of the greats, there is less a sense of wonder and excitement than a sense of bafflement. One might ask the hypothetical - if none of this intellectual edifice existed, would it have to be re-invented? Consider the long tradition of general equilibrium analysis and, within that, the core of work on the existence, uniqueness and stability of equilibrium. No doubt the psychological need for elegance would seek to generate a substitute; but in another historical epoch (the present) in which the natural sciences are less conducive to borrowings of this particular character, the pursuit of elegance might be thwarted. General equilibrium analysis, if it had to be born anew, might not be born at all! What an optimistic thought and from, a moral viewpoint, how appropriate.

Ironically, the general tenor of this book is that the body of work which now constitutes 'economics' has failed us. It ends limply, despairing both at the 'Babel' of schools which has produced an anarchistic and incoherent dimension to the discipline as a whole, and at the core a theoretical structure which so many individuals criticise but which carries through the generations undaunted - a juggernaut out of control.

The authors have added their names to those, mostly in their dotage, who have cast doubt on the discipline's priorities. If the concerns expressed merely serve to let off steam, enhancing the illusion of disciplinary 'openness' and social-mindedness, nothing will change. The expressions of concern coming from people late in their careers are reflections of moral cowardice rather than moral courage. To the authors' negative ending, one might suggest - why not pull the plug? The essential responsibility for those of such mind is to contribute to some assertive input into changing the educational and reward structures by which the next generation is lured into the existing core vision; only then will the seemingly uncontrollable juggernaut be inhibited in its tracks.

The biographical urge appears to be consuming the discipline as an exercise in self-congratulation. Ironically, the more detailed exposure of our genealogy may serve as a vehicle for critical disciplinary self-examination into the social value of its dominant priorities.

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Notes

1 Toohey (1994, Ch.4) contains an accessible account of the self-criticisms of respectable economists.

2 There was, of course, nothing 'certain' about the Phillips (or Samuelson-Solow) curve - it was merely an empirical generalisation relating crude aggregate variables, accompanied by crude macroeconomic theorising and no theorising of the structural underpinnings whatsoever.

3 A timely treatment of twentieth century British industry policy and the political culture which channelled its character can be found in Tomlinson (1994).

4 The editors missed from their list of Clark's publications the 1958 book *Australian Hopes and Fears* (London: Hollis & Carter).

5 Of course, the priorities of Kuznets had been set long before, influenced by the American Institutionalist tradition in the person of Wesley Mitchell; it was the experience of an earlier generation of administration during the First World War that produced the National Bureau of Economic Research which in turn set Kuznets on his future path.

6 At least Haavelmo has to his credit a theoretical contribution of rare practical significance – the demonstration that a balanced budget is not neutral but has positive multiplier effects.

7 Blair died young, effectively having worked himself to death for the national interest.

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