G.C. HARCOURT (born 1931)

To understand how I came to my ‘dissenting’ views I need to be autobiographical. I like to say that I am an Australian patriot and a Cambridge economist. I was born in Melbourne in 1931 into a middle-class assimilationist, agnostic Jewish household with right-wing political views. Melbourne then was a stuffy, snobby place, marked by sectarian battles between Catholics and Protestants, who were nevertheless united in their unthinking anti-Semitism towards a large Jewish community. Political and especially religious problems were, from very early on, stark and frightening experiences for me. So, when I went to the University of Melbourne in 1950, itself a veritable paradise of enlightenment and tolerance after my schooldays, it was no accident that I became absorbed in the search for a political philosophy and a religious creed, as well as in economics (which I loved).

I found a political philosophy more quickly than a religious creed. I abandoned the right-wing views of my parents six months or so into my first year and became a democratic socialist, convinced by the lectures on economic geography that private enterprise competitive institutions were neither the most rational nor efficient means to develop society’s basic resources, especially when the needs of future generations had to be taken into account.

It was a much longer journey to religious belief, puzzled as I was by the divergence between the beliefs and professed moral values of Christians (in particular) on the one hand, and their actions on the other. By my fourth year though, helped by discussions with a number of theologians in my College (Queen’s) and by the Student Christian Movement, I had adopted a working hypothesis of belief in God. Basically I argued that personal morality could safely be left to a personal relationship with God, that it was a useless and misguided, indeed unhealthy, struggle to tackle on your own, and that Christian principles provided the basic blueprint for the just and equitable society that I wanted to see established, through institutions that would allow altruism, compassion, cooperation and justice to flourish. My religious and political beliefs merged at this juncture, fending off what I saw as the unhealthy absorption with self of the evangelicals, by diverting energies outwards towards community objectives and the care of other people. I differed from socialist humanists only in that I did not believe that these ends could be attained, unaided, by persons alone. I joined the Australian Labor Party (ALP) in 1953, the year that I was baptised, and when I took up my first lecturing post at Adelaide in 1958, I began to say that I was the only Jewish Methodist in Adelaide.

The Commerce Faculty at the University of Melbourne was Cambridge-orientated. We were brought up on Keynes’s works, reading the Tract in our first year and The General Theory in our second. Piero Sraffa, Dennis Robertson, Austin and Joan Robinson, Richard Kahn and Nicholas Kaldor soon became everyday names to us. Wilfred Prest’s dog-eared copy of
Marshall’s *Principles* was a familiar sight too. There was also a strong neoclassical influence so that Wicksteed’s *Common Sense*, along with the *Tract*, were the first two great works I read. Boulding’s *Economic Analysis* was our advanced theory book and I can still remember – now I wonder why – how excited I was when I realized that the formal structure of consumer theory was exactly the same as that of production theory. We were exposed to Kalecki’s writings too. Economic history and history of thought were also prominent; as an undergraduate I read virtually all the great books from *The Wealth of Nations* to *The General Theory*. I was defeated by Volume I of *Capital*, turning in desperation to Sweezy’s *Theory of Capitalist Development* in order to try to understand Marx’s concepts. I read a lot of Dobb’s work, *Political Economy and Capitalism* having a lasting impact on me. We read *Value and Capital* (including the mathematical appendices), the *Foundations* and Trygve Haavelmo’s *Econometrica* supplement, as well as many of the then classic papers of mathematical economics. I also read Hayek’s *Pure Theory of Capital*.

For my undergraduate dissertation (30,000 words) I tried to integrate the analysis of Kurt Rothschild’s classic 1947 *Economic Journal* paper on price theory and oligopoly with the macroeconomic system of *The General Theory* in order to study the reserve policy of Australian companies during the Great Depression. I used accounting data – explicitly applying what I had learned, in a first-year one-off course on profit and loss accounts, balance sheets and funds statements – in the empirical work. I did not get far with the synthesis, but it does illustrate that I have always been interested in micro foundations and that I did not, even then, accept the artificial distinction between micro and macro which does so much harm to the way we teach economic theory.

I graduated in 1953 and worked for the next 18 months on a Pilot Survey of Income and Saving in Melbourne. In August 1955, I left Melbourne, as it turned out permanently, for King’s College, Cambridge – where else? – to do a Ph.D., initially with Nicky Kaldor as my supervisor (a disaster for both of us) and then with Ronald Henderson. (Joan and I were married a fortnight before we left and we have, truly, lived happily ever after.) I intended to work on the implications for the theory of the firm and the trade cycle of the assumption that secure profits are as important as maximum ones; by the time I submitted in August 1958, it had become a study of the economic implications of using historical cost accounting procedures to measure income for dividend and tax purposes and to set prices in a period of inflation (with hindsight, not that unconnected).

While I was in Cambridge, Joan Robinson published *The Accumulation of Capital* (1956). I locked myself up with the book for a term, then emerged to read a paper on it to the research students’ seminar, with Robin Marris in the chair and the author herself attending the second session. This paper marks
the beginning of my friendship with Joan Robinson, though I had met her at previous research students' seminars (these were usually chaired by Piero Sraffa). Incidentally, 'the class of 1955–58' included Tom Asimakopulos, Charles Feinstein, Pierangelo Garegnani, Luigi Pasinetti, Amartya Sen and John Whitaker, as well as a host of bright Australians (such as Allan Barton, Keith Frearson, Hugh Hudson, Duncan Ironmonger and John McCarty) who were later to make their mark on Australian academic and public life.

The Accumulation of Capital had a profound effect on me. It presented a 'vision' of how capitalism works over time and, more tentatively, a conceptual framework with which to think about the processes involved and make sense of what I saw happening around me. It formed the core from which my own work and teaching were subsequently always to start. I realize now that, apart from her own very considerable contributions, not least the marvellous introduction (to which Lawrence Klein paid tribute in 1989), Joan Robinson had also synthesized strands of thought from other economists who had influenced me – Smith, Ricardo, Marx, Marshall, Keynes, Kahn, Kalecki and, increasingly in the years to follow, Sraffa.

I returned to Australia in March 1958 to take up a lecturing post in the Economics department of the University of Adelaide with which I was to be associated for 27 very happy years. There my mentors were, first and foremost, the late Eric Russell, who was ten years older than myself, and Bob Wallace, who was one year older and with whom I had overlapped at Melbourne. I wrote my first book, Economic Activity, with Bob Wallace and Peter Karmel, our dynamic youthful professor.

At Adelaide I developed my interest in the links between accounting practices and economic performances, at firm and economy levels. This culminated in 1962–63 in ‘The Accountant in a Golden Age’ (1965, 1982, 1992), a project first suggested to me by Harold Lydall. I was introduced to Wilfred Salter’s seminal work on vintage models when Peter Karmel asked me to write a review article of Salter’s 1960 classic, Productivity and Technical Change, for the Economic Record. I lectured to the interim honours class on Kaldor’s economics and wrote a critique of his (then) theories of distribution and growth (1963, 1982, 1992), concentrating on his strange assumption of full employment. Though naturally I welcomed the macrotheories of distribution as an alternative to marginal productivity, I was puzzled by the peculiar pricing behaviour inflicted on the consumption and investment goods sectors in Kaldor’s version, principally because of the full employment constraint. I was also politically active in a conventional sense, becoming president of our local branch of the ALP and secretary of the South Australian branch of the Howard League for Penal Reform.

In 1963 I returned to Cambridge, I thought for a year’s study leave. Richard Kahn and Joan Robinson made me extremely welcome. (I had been
corresponding with Joan Robinson about my 1963 paper criticizing Kaldor’s models. She also wanted me to be a witness to fair play in her debates with Solow and Arrow who were also in Cambridge on study leave.) I made a nostalgic return to the research students’ seminar to read a paper on the determination of the level of employment and the distribution of income in the short period in a two-sector model. The paper was inspired by hearing Bob Solow’s 1963 Marshall lectures on two mythical creatures, Joan and Nicky. Its themes jelled with my continuing interest in the process of price formation in oligopolistic industries, with the work I had done on Kaldor and Salter in Adelaide, and with the beginnings of the hardest intellectual task of my life – to try to master the argument of Sraffa’s 1960 classic, _Production of Commodities_. (I was then reading the book with Vincent Massaro; we agreed not to go on to the next sentence until we had understood the one before.)

I read the paper to an audience which included Arrow, Meade, Sraffa, Kahn and Joan Robinson. Evidently it went well for Joan Robinson and Meade both complimented me on it; Kahn asked me to dinner and to join the ‘secret seminar’. Then, to my amazement, I was offered in effect a lecturing post in the Faculty. As I was on leave from Adelaide, I felt I could only accept this for a limited period, so I obtained three years’ leave without pay to do so. Trinity Hall elected me as their first teaching fellow in economics. (The Vice Master clinched my election by saying that even if it were a disaster, it would only be a short-run one.)

I am personally very fond of the two-sector model paper, which was published in the _Economic Record_ (1965, 1982, 1992), after which it vanished, virtually without trace. So I was delighted when Robert Dixon wrote in 1988: ‘This much under-rated paper is one of the major building blocks of post-Keynesian economics’ (p. 247).

Then commenced what were probably the most productive years of my life. Drawing on my Adelaide experiences and working in one of the best faculties in the world, I wrote a number of papers which I can now see were in the post-Keynesian tradition. There was a unity to them, as reviewers of the volumes of my selected essays have pointed out. In addition to finishing ‘The Accountant in a Golden Age’ (can we use accountants’ methods to find out what economic profits are?) and the two-sector model, I wrote a satirical critique of the CES production function (1966, 1982) in which I incorporated the implications of Salterian vintages into the Robinsonian critique of the aggregate production function as it applied to econometric exercises. I also began to work on investment decision rules, investment incentive schemes and the choice of technique, including in this an analysis of the implications of the results of ‘The Accountant in a Golden Age’ for a bonus scheme for managers in the Soviet Union (1966, 1982). The papers under this rubric illustrated how models which were based on what business people or manag-
ers actually do result in significantly different predictions from those which may be obtained from models which incorporate standard profit-maximizing assumptions. In these papers, unwittingly at the time and as very small fry, I was allying myself with heretics such as Kaldor and P.W.S. Andrews. I also began the work on pricing and the investment decision which was to reach fruition ten years later in a Kyklos paper (1976, 1982) with Peter Kenyon, who was then a graduate student at Adelaide. This was an attempt to explain the sizes of the mark-ups set by firms in oligopolistic industries by relating them to the financial requirements of the firms’ investment programmes. Finally, with Vincent Massaro I wrote two papers on Sraffa’s 1960 book, one of which was a review article for the Economic Record. It had Sraffa’s blessing in the sense that we discussed virtually every word of it with him!

Joan, the children (then three) and I left Cambridge for Adelaide at the end of 1966, loathed to leave yet excited to be going home (and to be able to play Australian Rules Football again). As it turned out, I was about to embark on actions which fundamentally changed my life. In 1965 I began to get agitated about the Vietnam war, especially, of course, Australia’s involvement in it, with the accompanying conscription of 18-year-olds. I went back to Australia armed with well-prepared information from some of my Cambridge colleagues, who were already attacking Harold Wilson’s and Michael Stewart’s appalling acquiescence in the role of the US. When the Campaign for Peace in Vietnam (CPV) was set up in Adelaide in mid-1967, I became a Foundation Committee Member and later the Chairman. Thus began five-and-a-half years of intense direct political action, during which I averaged about two-and-a-half days a week on anti-war activities, as well as having a full teaching load, jointly editing Australian Economic Papers (which at that time was one of the few outlets for maverick opinion), and helping to bring up four young children (I reneged a lot on this count – the great support and love of my wife seems more extraordinary to me now than it did even at the time). I must confess, too, that I played cricket in the summer and Aussie Rules in the winter.

Because of my Jewish origins, I was doubly a target for the right. I received more death threats than most people have had hot dinners, as well as one actual attempt when someone tried to blow up our car. I have never regretted the involvement in direct-action protests, moratoria and so on. In the actual debates I tried not to forget the role for dispassionate argument, and that the people I disagreed with were nevertheless people. The works of two scholars had a great influence on me at the time – Hugh Stretton’s The Political Sciences (1969) and Noam Chomsky’s ‘The Responsibility of Intellectuals’ (1967). They made me realize that ideology and analysis are indissolubly mixed and that we must always tell our students and our contem-
poraries where we stand, both in the classroom and in society at large. Since then I have never given a course of lectures without an opening burst on values, ideology and analysis, and what my own political, religious and economic views are, so that the students may be on their guard from the beginning.

The other significant event was the request in August 1968, by the editor Mark Perlman (at the suggestion of my former professor at Melbourne, Wilfred Prest), that I write the survey article on capital theory for the second issue of the newly-formed Journal of Economic Literature. The rest, as they say, is history!

I shut myself away for four months behind a usually open door and, between protest marches and meetings, wrote ‘Some Cambridge Controversies in the Theory of Capital’ (1969, 1986). In order to make the task manageable, I split the topic into a number of self-contained working papers which I sent to about 30 people in Australia, the UK and the US. The then economics editor of CUP saw them and asked me to make a book of the survey. A Leverhulme Exchange Fellowship, which took us all to Keio University in Tokyo for three months over the long vacation period, December 1969 to March 1970, allowed me to escape from ceaseless political activities, live the selfish life of a scholar and write the book, the first draft in two months. I have never, before or since, worked so intensively. I wanted to get on paper what I saw in my mind as a unity.

Although many saw the reswitching episode as the centrepiece of both the survey article and the book, I thought the methodological critique by Joan Robinson concerning changes versus differences at least equally as important. Certainly Joan Robinson had already decided on the primacy of this aspect of the critique. When I sent her the first draft of the book in May 1970, her major criticism (apart from her reaction to some of the jokes) was that I had not emphasized it enough.


To be known for these papers and the book became something of a bind, for I spent the next ten years or so writing commissioned articles on these themes. The only paper that had to meet the refereeing test, as it were, was the one written with Kenyon; it fell at the Economic Journal hurdle (courtesy of my old chums, David Champernowne and Brian Reddaway) but cleared the Kyklos one in style (1976, 1982, 1992). Personally, I think it a good example of post-Keynesian analysis of a problem set in historical time and starting from ‘real world’ observations. I also chaired, at John Hicks’s suggestion, the 1975 IEA Conference at S’Agaro on ‘The Microeconomic
Foundations of Macroeconomics’ and edited the volume of the conference (1977). Writing the ‘Introduction’ was one of the hardest tasks I have ever undertaken, so I was chuffed when Hicks said how much he liked it, adding that he thought it could not be done. The issues of the conference were, of course, those I had been grappling with, ever since my undergraduate dissertation.

In the 1970s, partly from the impetus of giving the Edward Shann Memorial Lecture on the social significance of the Cambridge controversies (1975, 1982), but mainly because of the emergence of the great inflation and the attempts to tackle it by monetarist policies, I became increasingly involved in policy debates and policy-making in Australia. With Eric Russell and other colleagues, I attempted to apply post-Keynesian analysis – containing strong Kaleckian–Robinsonian inputs and using the Meade–Russell model of the Australian economy (1957) – to the problem of inflation within the context of the Australian institutions of centralized wage-setting through the Arbitration Commission. As the economist on the ALP’s 1978 National Committee of Enquiry, I drew together the party’s progressive strands of thought on economic policy in a discussion paper which emerged from the enquiry. My own ideas may be found in a paper written with Prue Kerr (1980) and in the 1982 John Curtin Memorial Lecture, ‘Making Socialism in Your Own Country’ (1982, 1992). When Hans Jensen (1988–89) wrote about my ‘civilized economics’, he discerned a structure running through the two volumes of my selected essays (1982, 1986) from which emerged a coherent set of policy proposals. This involved a package deal of redistribution through the public sector as the quid pro quo to wage-earning groups for accepting incomes policies directed at the rate of increase of money incomes, using the traditional Australian institutions of indexation and the Arbitration Commission. Fiscal and monetary measures were to be directed towards the level of activity and the rate of growth. I put nationalization of certain key industries, including financial intermediaries, back on the agenda for discussion but sat on the fence concerning the tariff (leave it as it is and concentrate on export promotion). I opted for a fixed exchange rate, with the proviso that in an economy like Australia’s, a change may have to be contemplated from time to time. I like to think that Bob Hawke toyed with the idea of implementing such a package deal for a good half hour after the election of the ALP government in 1983.

The bottom dropped out of my personal and intellectual world in February 1977 when Eric Russell died, completely unexpectedly, after playing squash. This happened while I was on six months’ unpaid leave from Adelaide, teaching at the Scarborough Campus of the University of Toronto with the wonderful group that Lorie Tarshis had gathered there. To help to cope with the (shared) grief of Eric’s death, I wrote the first of what was to become a regular series
of intellectual biographies. My memoir of Eric was published in the *Economic Record* (1977, 1993); I then wrote a fuller study for the Newcastle Lecture in Political Economy (1977, 1992). One of my more encouraging colleagues at Cambridge calls my essays on Tarshis, Shackle, Boulding and Goodwin, for example, ‘mere chit chat’. I think they serve a more serious purpose of attempting to show how people’s background and personalities influence their approaches to economics, and how ideas arise in the form that they do. I brought together the intellectual essays I had written up to 1993 in Harcourt (1993a).

I was in Canada again in 1980, working especially with Jon Cohen. I wrote an exploratory and speculative paper on ‘Marshall, Sraffa and Keynes: Incompatible Bedfellows?’ (1981, 1982, 1992), in which I tried to think through the issues associated with the concept of centres of gravitation both in their work and in the make-up of modern economies. I remain ambivalent about its value but several of my research students have taken the paper as the starting point for their own work, and a number of papers have also appeared in the literature on convergence to natural prices in classical models. Path dependence, of course, is now all the rage.

I returned to Cambridge in 1982 to try to carry out one last research project – to document the intellectual history of those we may loosely think of as Keynes’s pupils. I call it ‘Joan Robinson and her Circle’ for I want to use her contributions as the focal point around which to put the writings of Kahn, Kalecki, Kaldor, Sraffa, Pasinetti and the other gifted people, most of whom worked at Cambridge in the Cambridge tradition before and after Keynes’s death. I believe it to be a worthwhile object to explore what is coherent and lasting in the contributions of these scholars. Most of the papers that I have written in the 1980s and 1990s may therefore be seen as prefatory to this task. I am also an editor of the *Cambridge Journal of Economics* and am associated with other journals which furnish outlets for non-mainstream publications.

I must confess to having been sidetracked from my main task on many occasions (not least by nearly kicking the bucket four times between September 1992 and September 1994). Since 1992, as a result of being asked to give the Second Annual Donald Horne Address in Australia, I have written a series of interrelated policy papers. I started with the Horne Address, ‘Markets, Madness and a Middle Way’ (1992, 1995), continued through macroeconomic policy for Australia in the 1990s (1995), ‘The Harcourt Plan to “Save” the World’ (1993b), a ‘Modest Proposal’ to tackle the harmful systemic effects of speculation in the foreign exchange, stock and property markets (1994, 1995) to a paper (1997a) which combines the policy insights of Eric Russell’s writings and Salter’s 1960 classic with the difficult issues raised by the vital distinction that Kalecki made (in 1943!) between getting to full
employment, on the one hand, and sustaining full employment, on the other. The other major sidetrack was to edit with Peter Riach, *A Second Edition* of *The General Theory* (2 Vols, 1997b). We asked over 40 Keynes scholars ranging from the venerable to the up-and-coming to write chapters on what they thought Keynes would have written in, say, 1938 or 1939 if he had not had his heart attack, and why they have done what they have done on those aspects of *The General Theory* in the postwar period. In volume I, the chapters trace (more or less) the chapters of *The General Theory*; in volume II, we have an overview, extensions, new developments, predecessors and successors.

In January 1997, through the kindness of the editors of this volume and Gabriel Palma, a conference was held in Jesus to celebrate my 65th birthday (27 June 1996). At it, I was presented with two splendid *Festschrift* volumes, edited by Philip, Gabriel and Malcolm and published by Routledge, Arestis, Palma and Sawyer. A third volume, containing chapters by my former Ph.D. students and edited by Claudio Sardoni and Peter Kriesler is now at the publishers, Routledge. I 'retired' in September 1998 but I hope to continue on as an elder of the post-Keynesian tribe (see Harcourt 1998, 1999).

Omar Hamouda (who edited the second volume of my essays) asked me to record what I thought the purpose of economics is. Evidently, I said that the purpose is:

to make the world a better place for ordinary men and women, to produce a more just and equitable society. In order to do that, you have to understand how particular societies work and where the pockets of power are, and how you can either alter those or work within them and produce desirable results for ordinary people, not just for the people who have the power. I see economics as very much a moral as well as a social science and very much a handmaiden to progressive thought. It is really the study of the processes whereby surpluses are created in economies, how they are extracted, who gets them and what they do with them. All economies have created surpluses in one way or another. Capitalism does it in a particular way and that is the process in which I am most interested because I live in capitalist economies. At the same time, I would like to help to create a society where the surplus is extracted and used in a way quite different from that of a capitalist society.

Sheila Dow kindly suggested that this could stand as a succinct statement of the post-Keynesian credo.

**Harcourt’s Major Writings**


Robert L. HEILBRONER (born 1919)

I was born on 24 March 1919 and began my intellectual life as an economist at a propitious moment, in the autumn of 1936, when I matriculated at Harvard, quite caught up in the full flush of Rooseveltian political ideals and ignorant of the name, much less the pronunciation, of John Maynard Keynes. It is difficult today to convey the sense of discovery that permeated economics in the late 1930s, when neither growth nor general equilibrium, rational or any other kind of expectations, choice theoretics or, for that matter, micro or macro had yet entered the economic vocabulary. One of my most vivid academic recollections was a debate mounted by the economics faculty in a crowded hall, where distinguished professors argued with trembling voices and empurpled faces as to whether savings did or did not equal investment. It speaks to the innocence of the age that six years later I was able to publish a small article in the American Economic Review (1942) on the perplexing question of why a changing level of income was required to bring about