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# 1

## 50 Years a Keynesian\*

### Introduction

I expect that like most people I shall never cease to be surprised by the perceptions which other people have of who you are, what you have done and why. When I retired in September 1998 and that most courageous and supportive of editors, Tim Farniloe, offered to publish two volumes of my selected essays,<sup>1</sup> I re-read some essays about my life and work by John Hatch and Colin Rogers (1997), John Hatch and Ray Petrides (1997), Philip Arestis, Gabriel Palma and Malcolm Sawyer (1997a, 1997b) and Sheila Dow (1997). These authors all have differing time periods and views<sup>2</sup> from which to observe me. These are naturally reflected in their observations. Nevertheless, there is a dominant theme in all their accounts, that, as Hatch and Rogers put it (1997: 97), I have ‘always been a Keynesian economist in the very broadest sense [, that I have always] identified with the elegance of Keynes’s economics [and] also with its social purposes’. In this essay I try to explain the how and why of their evaluation.

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\* I thank but in no way implicate Stephanie Blackenburg, Giuseppe Fontana, Tim Harcourt, John Hatch, Prue Kerr, Bob Rowthorn, Trevor Stegman and Tom Wilson for their comments on a draft of this essay. Most of the policy papers referred to in the essay are gathered together in a companion volume, Harcourt (2001).

## First introductions to economics and to Keynes

I was not consciously aware of Keynes and his economics until 1950 when I started a four-year honours course in economics at the University of Melbourne. Yet I had already done four years of economics as a schoolboy. The two texts that dominated those schoolboy years were *Supply and Demand* (1922) by H. D. Henderson and *The Social Framework* (1942) by J. R. Hicks. With hindsight, they may be seen as excellent starting points from which to come to Keynes's writings. The first is a very clear exposition of the essentials of the supply and demand approach of Marshall's *Principles* (1890) without Marshall's own ifs and buts and smokescreens. (It is instead shot through with Henderson's sceptical view of life in general and economics in particular.) Hicks's book provided an illuminating way to learn the essentials of the Keynesian national accounting framework, Keynesian in the sense that the theoretical system of *The General Theory* (1936) was the principal impetus for the systematic development of the national accounting framework in the war and postwar years. When I wrote my first book (*Economic Activity* (1967), written jointly with Peter Karmel and Bob Wallace), I found that being forced to think through the logic of the national accounts (the topic of chapters 2 and 3 of *Economic Activity*) was extremely enlightening, not least because at the time I wrote the first draft (1963) I was also struggling to understand the logic of the system of production interdependence that is the core of Piero Sraffa's (1960) *Production of Commodities*, and of the interrelations between these two systems.<sup>3</sup>

My first explicit introduction to J. M. Keynes was not through *The General Theory* (or even *A Treatise on Money*, 1930) but through the *Tract* (1923) in the first-year lectures by Alf ('Sammy') Weller to those students electing to do the honours questions in their exams. Though it was 1950, 27 years on from its publication, I think in retrospect that it was still a stroke of good fortune to start from there. For in the *Tract*, while Keynes was still a resolute quantity theory of money person *à la* Marshall, he was already kicking against the constraints of his teacher's system, especially the long-period character of its propositions and their irrelevance as a guide to monetary policy. Moreover, though he still accepted the real/money dichotomy, he had already singled out deflation as a worse short-term

situation of economies than inflation (even hyperinflation) and, within this, the social evils of unemployment as opposed to the economic effects of falling price levels. So we took on board the appropriate passages and we were given the basis with which eventually to understand the full significance of the context within which is placed Keynes's best-known remark, '*In the long run we are all dead*' Keynes (1923: 65, emphasis in original).<sup>4</sup> Furthermore, our fledgling intellectual muscles were flexed by the detailed analysis of the forward exchanges in chapter 3. It is one of Keynes's most incisive contributions to economic theory; understanding it is a must for understanding his analysis in later years of the implications of an inescapable environment of uncertainty for decisions and decision-makers in an economy.

I did not read *The General Theory* until our second year when it was one of the set texts for the lectures given by Don Cochrane (of Cochrane and Orcutt fame) and Joe Isaac (my first mentor and the first economist to be made a 'judge' of the Australian Arbitration Commission, as it was called then). I tried to read *The General Theory* over the long vacation. Though it excited me tremendously, I cannot pretend that I got very far with it, as my tutors gently pointed out when I came to write my first faltering essays on its concepts. Cochrane did not help by sending me prematurely to Duesenberry's 1949 book when I told him I had read *The General Theory*. By the end of the lectures I think I was becoming clear on the outlines of the theory, helped – I thought at the time – by Paul Samuelson's Keynesian cross diagram.<sup>5</sup> We did not meet 'Mr. Keynes and the "Classics"', Hicks (1937), until our third year (the first year of the two-year honours programme known as Final Division). By then we were also reading *Value and Capital* (1939) and even the *Foundations* (1948). We were introduced to Michal Kalecki's writings on distribution and, in microeconomics, to Robert Triffin's (1942) *Monopolistic Competition and General Equilibrium Theory* following on from the 1933 classics by Joan Robinson and Edward Chamberlin and the reviews and articles they spawned.

The most significant of these articles for me was Kurt Rothschild's 1947 classic, 'Price Theory and Oligopoly'. The central theme of his article was that oligopolists were as interested in secure profits as in maximum profits and therefore that Clausewitz's (1943) *Principles of War*, rather than the theories of Joan Robinson or Chamberlin or

the then emerging game theory, was the appropriate framework within which to analyse their behaviour.

When I came to choose a topic for the 30 000-word honours dissertation we were required to do, my two loves came together in an ambitious (one of the examiners thought it over-ambitious!) attempt to work out the implications for systemic behaviour in a Keynesian framework of microeconomic foundations containing Rothschild's Clausewitzian oligopolists. The particular issue I homed in on was Keynes's remarks in the chapters on the consumption function (see Keynes, 1936: 98–104) on the implications of 'financial prudence' – writing off the book values of fixed assets well ahead of their actual wearing out and replacement – for the impact of current investment expenditure on activity. I used Lou Goldberg's wonderful collection of company accounts (Lou was then Professor of Accounting at the University of Melbourne); I tried to test the implications of my attempted marriage against the reserve policy of Australian companies during the Great Depression. I used a case-studies approach, taking the accounts of individual companies and constructing flow-of-funds statements. The results of my labours could most favourably (charitably) be summed up by the Scottish verdict 'not proven'. I suspect that the essence of what I was getting at is to be found in a pithy paragraph in Joan Robinson's writings (see *Collected Economic Papers*, Vol III, 1965: 177) and the analysis of this on pp. 210–14 of Harcourt (1972). Oligopolists have some discretionary power over the setting of prices *and* also wish, as far as is possible, to finance their investment expenditures internally from retained profits. If we now consider a world of oligopolists, it follows that the expansionary effects of a rise in overall planned investment may be offset, always to some extent and sometimes more than offset, by the contractionary effects of the accompanying rise in planned savings. The latter results from the redistribution of income implied by the rise in profit margins and prices designed to bring about the required rise in internal finance.<sup>6</sup>

Partly simultaneously with and partly preceding these developments in my thoughts about economic theory were radical changes, first in my political beliefs, and subsequently in my religious ones. The direct stimulus for the changes was not J.M.K. but the first-year lectures on economic geography (eat your heart out, Paul Krugman)

which constituted, in essence, a course on comparative economic systems. It was a shock to my system for while they revealed great variations in the nature of the organisation of different social systems, they also revealed two common characteristics in all – that injustice and poverty thrived and that the rationality of the different forms of organisation was conspicuous by its absence. The result was my conversion from being a doctrinaire free marketeer – my parents' views absorbed uncritically (for details see Harcourt, 1998: 3–7) – to, I suppose, an equally doctrinaire socialist (soon and later, respectively, to be coupled with the adjectives, 'democratic' and 'Christian'). These philosophical changes were given practical content when I joined the Australian Labor Party (ALP) in early 1954.

### Other influences

I have stressed the influence of Keynes and the writers on price formation in different market structures. But I want also to mention the discussion of Joseph Schumpeter's work on business cycles in my second year in a course on economic history, and of the great economists in my third year when I took HET (*History of Economic Thought*) as the first of my honours options. (In my fourth year I took mathematical economics.) The small group taking HET read Smith, Ricardo, Malthus, J. S. Mill, Marx, Jevons, Marshall and others in the originals. I confess that Marx's *Capital* defeated me and I had to depend on Maurice Dobb and, later, Paul Sweezy to give me what few clues I acquired. That had not stopped me from calling myself a Marxist in my second year, nor from writing an economic history essay on an industrial revolution in the sixteenth century within a Marxist framework. In the essay I foolishly used terms such as exploitation, surplus labour and value, proletariat and capitalist – much to the annoyance of one of my teachers, a Latvian who was opposed to anything Marxist. Unfortunately he also marked the essay.

I graduated at the beginning of 1954 and spent the next 19 months or so working for a Master's degree by dissertation as the research assistant to R. I. Downing, who had just taken up the prestigious Ritchie Chair at Melbourne University, one of the very few pure research chairs in Australia. Downing, an Australian and a

graduate of Melbourne University, was also a Cambridge buff, an enthusiastic Keynesian (he had been at King's just before the Second World War) who also knew and admired Kalecki. The project I worked on was financed by the Reserve Bank of Australia (the Central Bank). I had to design and carry out a pilot survey of income and saving in Melbourne, to see whether an annual Australia-wide survey along the lines of Harold Lydall's work in Oxford and George Katona's at Ann Arbor was a practical possibility. This was Keynesian economics in a down-to-earth mode, examining the feasibility of acquiring information at the level of the household on three fundamental variables in the Keynesian system – income, saving, wealth – in order to estimate their aggregate values from the bottom up, as it were.

The project was successfully completed in 1955 (though not without many trials and extreme tribulations, of which I shall say nothing, in print anyway). I subsequently received the degree in 1956. My first article, 'Pilot Survey of Personal Savings', written jointly with Duncan Ironmonger (who had been the excellent expert advisor from the Australian Bureau of Statistics (ABS) on the stratified sampling procedures used to gather the information) was published in the *Economic Record* in 1956. By then I was married to Joan (30 July 1955) and we were in Cambridge. I was doing a PhD at King's, supervised first by Nicky Kaldor and then, when he went on leave for the academic year 1956–57, by Ronald Henderson.<sup>7</sup>

## **At Cambridge in the 1950s**

My PhD topic was initially on the implications for the theory of the firm and the trade cycle of the assumption that secure profits are as important as maximum profits in oligopolistic market structures. When I told Robin Marris<sup>8</sup> this at the first meeting of the new/old research students in Michaelmas Term 1955 to set up the seminar series, he pounced – 'You're first!'. So two weeks or so into Full Term, I gave a preliminary paper on my topic to the seminar. Joan Robinson was there, chain-smoking and startling us all in the subsequent discussion by saying (of business people's behaviour) 'I think the buggers do...'. Despite surviving this rather traumatic beginning, I quickly lost my way in the first term, partly because Nicky and I did not hit it off at all.<sup>9</sup> So being assigned to Henderson at the

start of 1956 literally saved my academic life. Henderson had a look at my honours dissertation and sent me to the NIESR (National Institute for Economic and Social Research) to work on a then emerging data-set, the preparation of aggregate profit and loss, balance sheet and funds statements for all the quoted public companies in the UK for 1949–53. Bryan Hopkin, the then Director, wanted some reports written on a sample of the data – the chemical industry and the woollen and worsted industry – and its uses. That was my task for the first eight months of 1956. It was hard work – the nearest thing to mechanisation was a Marchand calculator – but invaluable. In addition I came to know the research officers at the NIESR, especially Max Corden, a fellow Australian, Sig Prais and Christopher Dow. Dow took me for a beer on the evening he finished the first draft of his celebrated imported-cost inflation model (Dow, 1956), an approach I subsequently was to take in my own research though I cannot pretend that I had then other than the merest inkling of what he had done, due in equal measure to his euphoric account and my response to English pints.

After many ups and downs my topic became the implications of using historical cost-accounting procedures for setting prices and measuring incomes for dividend and taxation purposes in an inflationary period. I put the historical-cost pricing models of Trevor Swan, Eric Russell and Russell Mathews and John Grant into the Marxian–Kaleckian framework of Joan Robinson's (1956) *The Accumulation of Capital* to derive my inferences, which were tested against the NIESR data referred to above. My first policy proposals were that the measurement of profits for taxation and dividend purposes should be on a replacement-cost rather than an historical-cost basis, as should the setting of prices by firms. I also started a long-to-continue investigation of the impact of investment-incentive schemes on investment decisions and outcomes.

In 1955 I had gone, usually with Tom Asimakopulos and Keith Frearson, to Joan Robinson's lectures on what was to become *The Accumulation of Capital* (1956). I found them as stimulating as they were frustrating, not least because Joan dropped her voice whenever she came to a critical proposition. So when the book was published in 1956 I took off a term from 'research' (much to Ronald Henderson's displeasure, he had no love for either Joan or her influence), locked myself away with the book and then emerged to

read a paper on what I thought it was all about to the research students seminar over two successive meetings. Marris chaired the sessions and Joan came to a third session to answer questions. She was not impressed by us, she thought we could not see the wood for the trees (even those she planted) because we asked nit-picking questions about, for example, price and real Wicksell effects rather than about broad conceptual issues. Probably she was right, but she was not without blame as she was to admit when in 1962 she published her own tell-it-to-the-children version (just as she had done in 1937 for *The General Theory*).

This detour was an invaluable experience because it gave me a framework and focus for my subsequent work, a framework which obviously I think I understand much better now than when I was first developing it. That is to say, I was starting to work on mainly classical problems done in the modern post-Keynesian, Kaleckian and Marxian manner. The first fruits were, as I said, to be found in my PhD dissertation. It allowed me to analyse the relationships between pricing practices (in this case, whether historical or replacement costs were marked-up), the level of activity and the aggregate distribution of income in periods of inflation. It also allowed me to link these outcomes back to the temporal structure of the liabilities side of balance sheets through the impact on the need for firms to borrow short-term and long-term because of what was happening to their cash flows.

In retrospect, I see that I put too much emphasis on the allocation-of-resources aspect of what was going on. I argued that by using replacement costs the price mechanism would be able the better to do its grand (neo-classical) job, signal where resources should move to and from because of the 'true' costs of production involved – clearly a naive inference in a world of oligopolists. Moreover, it distracts attention from the need explicitly to consider what determines the sizes of the mark-ups used (regardless of the cost-base to which they are applied) and so to look at the relationships between accumulation plans, financial needs and price-setting. Because of Keynes's realisation in 1937 that he had neglected the finance motive in his discussion of the determination of the rate of interest, proceeding from *The General Theory* alone meant that readers sometimes missed the significance of the vital distinction between finance on the one hand, and saving on the other, and the

role that finance, *not* saving, played as the ultimate constraint on the achievement or not of planned accumulation.<sup>10</sup> This emphasis was to be found in the later (and earlier) Keynes and in the writings of Kalecki and Hy Minsky. It is part of the modern agendas of both the post-Keynesian and mainstream literature; see, for example, the many papers which Asimakopulos (1983) spawned and the huge literature created by Feldstein and Horioka (1980), to which Paul Dalziel and I made a modest critical contribution in the *Cambridge Journal* in 1997. We defended Keynes's fundamental insight that, as James Meade put it, nationally and internationally, the investment dog wags the saving tail and not the other way around.<sup>11</sup>

### Cambridge themes in first Adelaide years

In March 1958 I started a lectureship at the University of Adelaide. In 1960 I gave some lectures on Kaldor's postwar writings which resulted in me asking why such an eminent Keynesian as Kaldor *insisted* that full employment was the natural long-period position of a growing capitalist economy (see Harcourt, 1963a). There was a convenient analytical dichotomy which provided a rationale for his view. For if it were true that prices and money-wages were sticky in the short period but flexible in the long period, with prices being the 'more' flexible of the two, the Kahn–Keynes multiplier would serve to determine output and employment in the short period and (providing  $s_\pi > s_w$ ) the distribution of income in the long period. A crucial proviso was that full employment was 'given' by the requirement that accumulation proceeded at a pace which allowed Harrod's  $g_n$  to be realised; for then Kaldor's long-period mechanism ensured the equality of  $g_w$  with  $g_n$ . This was a neat logical solution but why should anyone believe that it described the world, especially when we had the prior contributions of Kalecki in which both employment and the distribution of income were determined simultaneously and in the short period, without there being any constraint to be at full employment.

Because of my earlier work, it was natural for me to ask: what pricing policies must be followed in the consumption and investment goods sectors in order that the Kaldor mechanism works in the short period (by 1957, Kaldor was arguing that his mechanism

applied to the short period as well as the long period)? Note that I was old-fashioned enough to believe it necessary to distinguish explicitly between the roles of the two sectors and also to have investment leading and saving following. Moreover, it was also made explicit that the chief decision-makers were business people, that it was their accumulation, profit-making and employment and output decisions which drove the economy along (sometimes well, sometimes poorly), not those of lifetime utility maximisers whose consumption/saving patterns led and all other institutions were but neo-classical agents devoted to helping them fulfil their plans, as if a Ramsey optimiser ruled the world. I stress these points because if we examine the helpful, if misguided, article by Chari (1998), a Lucas admirer, on Robert Lucas's influence on macroeconomics in the last quarter of a century, we see that the whole Marx–Kalecki–Keynes view has been completely suppressed in most macroeconomic analysis, in Chari's view, a good thing too.<sup>12</sup> If I have not persuaded you, do have a read of an insightful article by John Lodewijks (1999) in which he sets out the core characteristics of the macroeconomic sections of three leading modern texts, Mankiw (1998), Parkin (1996) and Taylor (1995). His account supports my generalisation. God alone knows what harm it has done to the now too many cohorts of undergraduates brought up on this fare. I was relieved to see that Bob Solow takes a broadly similar view on these general matters: see the many wise remarks in his recently published *Frederico Caffé Lectures, Monopolistic Competition and Macroeconomic Theory* (Solow, 1998).

Not surprisingly, the answers to my question of what pricing policies must be followed in order that planned investment becomes actual investment and full employment is maintained produced some very peculiar behaviour both within and between the consumption goods sector and the investment goods sector. For example, in one scenario we find that the entrepreneurs in the investment goods sector are active, bidding for or sacking labour and raising or lowering production in response to changes in planned demands. In contrast, their counterparts in the consumption goods sector are passive, accepting the loss or return of labour and the consequent changes in production. Their only active role is to set the prices of consumption goods appropriate

to each situation. There is a glaring weakness in my analysis. I implicitly treated each sector as one giant firm (the modern representative agent heresy) and so derived sector results. I needed to delve one layer deeper in order to find out the behaviour of individual firms which would give those sector results. Solow (1998) takes on such considerations head on (but it is over 30 years later). Robin Marris also worked on similar themes for many decades; his mature views are to be found in Marris (1991, 1997).

At much the same time I reviewed Wilfred Salter's 1960 classic (see Harcourt, 1962; 1982). I found it one of the most illuminating books I have ever encountered in economics. Salter built on the insight as to why old machines could operate side by side with new and better ones (old machines only have to cover their *variable* costs, new machines have to expect to cover their *total* costs), to make serious and profound policy recommendations concerning the rate of change of nominal incomes in a fully employed economy. In order to have high levels and rates of growth of overall productivity in a fully employed economy, the ground rule for adjusting money incomes should be that they increase at the rate of *overall* productivity plus prices.

At the time I read Salter's book there was a heated debate occurring in Adelaide (indeed, in Australia) on an appropriate wages policy for Australia. Eric Russell, my mentor at Adelaide, was virtually the odd (but ultimately correct) man out in the debate. With James Meade he had written the definitive account of how the Australian economy works (see Meade and Russell, 1957). (I always preferred Russell's Kaleckian means of establishing the paper's main results; see Harcourt, 1977a; Sardoni, 1992.) With Salter, alas, to die at the ridiculously early age of 34 in 1963, he had given crucial evidence combining his theoretical analysis with Salter's empirical work to the Australian Arbitration Commission on behalf of the wage-earners in the 1959 Basic Wage Case. Eric's arguments were opposed by those economists who gave tremendous weight to the 'evils' of inflation and who were not prepared to allow changes in prices to influence the setting of the rate of increase of money incomes. It seemed to me at the time, albeit through the proverbial glass darkly, that Eric was correct. I am comforted by the fact that my other Australian mentor, Joe Isaac, also agreed with Eric in the debates.

I recently returned to these themes, together with the implications of Kalecki's remarkable 1943 article on the political aspects of full employment, especially his vital distinction between the political economy of *getting* to full employment, on the one hand, and *sustaining* it, on the other (see Harcourt, 1997). I argued that if economies followed the Russell/Salter rule of adjusting *money* incomes for effective productivity *plus* prices, they would greatly improve their chances of entering virtuous regimes of Salter processes in which *overall* productivity would grow at agreeable rates because low productivity industries would be knocked out and investment in high productivity industries would be encouraged. This would enhance the chances of restraining increases in nominal incomes, so allowing full employment to be maintained because *real* incomes would be growing at relatively satisfactory rates. These ideas were put by the Australian Council of Trade Unions (ACTU) advocate to the Industrial Relations Commission in the 1996–97 Living Wage case (partly through the good offices of our son, Tim, who was then a research officer at the ACTU). Alas, they did not carry the day with the Commission that year but received a more sympathetic hearing the next year. The current emphasis on the need to create flexible labour markets and to introduce enterprise bargaining threatens to produce a pattern of wage levels and changes in wages which will throttle the benefits of Salter processes by allowing old machines longer lives in declining or low productivity industries and frustrating the rate of introduction of new machines in high productivity or expanding industries, so perpetuating sluggish growth in overall productivity.

While I supported Eric in the discussions at Adelaide and in letters to him when he was on leave in Oxford in 1960, I did not write directly about the issues, except to list Salter's policy conclusions in my 1962 review article. My own writings at the time were on much more basically microeconomic policy proposals – investment allowances for primary producers (Barton and Harcourt, 1959), a rather silly article with Jim Bennett (Bennett and Harcourt, 1960) on reforming the company taxation system (silly because it mixed up a neoclassical with a post-Keynesian approach) – and on theoretical issues – the critique of Kaldor's theories, a note on Joan Robinson's 1956 volume and Harry Johnson's critique of it, Harcourt (1963b) and the first draft of 'The Accountant in a Golden Age' (Harcourt,

1965a; Sardonì, 1992), my second most cited paper after the survey of capital theory in the *Journal of Economic Literature* (Harcourt, 1969).

### Cambridge in the 1960s: an insider's view

In the autumn of 1963 I returned to Cambridge on study leave and almost immediately (the day after President Kennedy was killed) was offered a University Lectureship in the Faculty, soon to be followed by a Fellowship at Trinity Hall. I took leave without pay from Adelaide and stayed until near the end of December 1966. This was possibly the most productive, certainly the most exciting period of my working life. Themes and issues which I had been looking at separately now came together. A major catalyst was hearing Bob Solow's 1963 Marshall Lectures on the Cambridge theories of distribution and growth associated especially with Kaldor and Joan Robinson. These spurred me on to write my own favourite theoretical paper on employment and distribution in a two-sector model in the short period – Harcourt (1965b); Sardonì (1992). It combined a macroeconomic theory of distribution, a Kaleckian/Keynesian theory of employment, a Salterian discussion of the choice of technique, a Kaleckian/Robinsonian approach to price-setting and Sraffa's emphasis on production interdependence.<sup>13</sup> The macroeconomic theory of distribution and the Kaleckian/Keynesian theory of employment led to an expression for the multiplier which reflected the distribution process through its effect on the size of the leakage into saving. The approach to price-setting emphasised retained profits as a source of finance for investment expenditure and as a determinant, together with the level of aggregate demand, of the sizes of the mark-ups in the two sectors.

The paper made explicit the structure of my approach to understanding the processes at work in modern economies from then on. A major item missing was an explicit role for Keynes's monetary insights and the determination of the rate of interest, always something of a mystery to me. That is one reason why in 1974 when my old teacher, Jim Cairns, who was then Deputy Prime Minister and Federal Treasurer, asked me whether I would consider being Governor of the Reserve Bank of Australia, I said 'no', adding 'You know me, Jim, I'm a real man, not a money man'.

My interest in inflationary situations and Salter's contributions came together when I examined the choice of technique in inflationary conditions, comparing and contrasting the outcomes of different investment-decision rules and, in a subsequent paper, Harcourt (1968), the impact of different investment-incentive schemes on the choice of technique. I also used the analysis of 'The Accountant in a Golden Age' to analyse what effects the Russian system of bonus payments for managers, then all the rage, had on the choice of technique in planned economies (see Harcourt, 1966). I queried whether the results had an economic rationale or were even what the proponents of the bonus scheme intended. As I have said elsewhere, I see with hindsight that I was using the post-Keynesian method of starting from real world observations, what people actually do, what economic societies they operate in, rather than an axiomatic one of, say, an assumption of profit-maximisation. The object was to work out the results of businesspeople's behaviour and policy-makers' declared aims and to compare the results with those arising from the application of standard economic theory.<sup>14</sup> For example, I showed that with the orders of magnitude likely to be met in the real world, the pay-off period criterion resulted in a more investment-intensive, less labour-intensive technique being chosen than would have occurred if a DCF (Discounted Cash Flow) procedure had been used.

In 1966 I wrote the first draft of 'Pricing and the Investment Decision' (Harcourt and Kenyon, 1976; Sardoni, 1992). In it I tried to develop an endogenous theory of the size of the mark-up, relating it to investment plans and the desire for internal finance and trying to make sure that the model was set in historical time as Joan Robinson called it (to contrast it with logical time), and which we now include under the rubric of path-dependence.

The paper had a long gestation period (see Harcourt, 1995a: 233), and by the time it was published Al Eichner's and Adrian Wood's writings on similar themes had captured centre-stage. Both, of course, made splendid contributions but Eichner's model has a weakness associated with the use of Keynes's MEC (Marginal Efficiency of Capital) schedule and Wood's analysis is explicitly in the logical time of a Golden Age model. (Jim Ball (1964) preceded us all.) An implication of our analysis was that in the oligopolistic industries with which we were concerned, margins would be

greater, the greater was the investment planned, *ceteris paribus*. But investment would be less, the higher were the margins and therefore prices set. In microeconomic terms at least, this is a drawback on accumulation, productivity growth and attaining and sustaining full employment. Furthermore, the higher price levels may make the control of inflation more difficult. These arguments may not go though at the level of the system as a whole but, at the least, they need to be explored. Salter processes are at their most effective when competitive market structures are present. (Oligopolists *may* behave like competitors but they cannot be forced to do so.) The increase in international competitiveness of the last two decades may have brought the world economy closer to the competitive model than when the writings referred to above were first developed. If so, our minds may be put more at ease on this particular score.

### Capital theory controversies and policy proposals

Soon after I returned home I was asked by Mark Perlman to write a survey article on capital theory for the newly formed *Journal of Economic Literature* (see Harcourt, 1999a, for the full story). This made me read intensively the literature, much of which had been created around me in the 1950s and 1960s by people mainly from the two Cambridges. Especially did it force me to try to make sense of the heated discussions I had witnessed in Cambridge between Joan Robinson, Kaldor and Pasinetti on the one hand, and Solow, Hahn, Meade, Christopher Bliss and others on the other. Ken Arrow also participated and was respected by both sides, both, for example, claiming learning by doing (Arrow, 1962), for themselves. Sraffa mostly stood aside from the day-to-day skirmishes, though he did ask me to show why Levhari's claim (1965) that reswitching and capital-reversing could not occur in the economy as a whole was wrong when I drew his attention to the article (see Harcourt, 1999a). Writing the survey while simultaneously taking an active, time-consuming role in the anti-war movement radically changed my approach to economics, teaching and politics, changes which were to affect the policy debates and proposals with which I was associated in the 1970s.

Once my 1972 book on capital theory was published, I turned to its policy implications and to policy issues in general. The 'long boom' or 'Golden Age of capitalism'<sup>15</sup> was coming to its end, Australia as with the rest of the advanced world was experiencing rising inflation and, soon, rising unemployment. Two papers from the first half of the 1970s set out the theoretical background and the social and political philosophies involved. Fittingly, the first paper, Harcourt (1974), arose from a lecture to the South Australian accounting profession, for it has always been my belief that the Australian training of economists, whereby some knowledge of the rationale and procedures of double-entry book-keeping is required, is a *sine qua non* for understanding how capitalism originated, developed and works.

In the lecture, I outlined the ingredients of the package deal of policies that came to be called the Adelaide Plan: so-called, because it was the outcome of discussions between Eric Russell, Barry Hughes, Philip Bentley and myself in Adelaide. In Harcourt (1999b) I summarised its essentials. Sufficient, therefore, to say here that it was an attempt to provide a sensible, effective and more just and humane alternative to what we saw to be the misguided attempts to tackle accelerating inflation by short sharp shock procedures; their professed aim was to quickly push unemployment above the then fashionable concept of its natural rate. This was designed to force decision-makers and wage-earners to revise their inflationary expectations and then move slowly back to the natural rate with a lower rate of inflation in mind.

In Harcourt (1977b; 1982) (the paper arose from a seminar I gave in Melbourne in 1975), I compared and contrasted the three rival theories then on offer (and the policies that flowed from them) from policy-makers and academics alike in Australia – Monetarist, Bastard Keynesian and Post-Keynesian. (Mervyn Lewis identified a fourth group, the structuralists, who came at macroeconomic policy from an essentially orthodox microeconomic base, forerunners of Gordon Brown's supply-siders.) When discussing policies I tried explicitly to identify the political constraints on the possibility of acceptance and implementation. I did this also later in the decade in a paper given at a symposium on unemployment at the Winter School of the Economic Society of Australia and New Zealand (Harcourt, 1978). There, I wrote:

When putting forward these suggestions, I shall try to operate within the constraints imposed by the present Federal government

and *its* advisors, to try to devise a package that is acceptable to *their* philosophy, and to *their* political and economic outlook. I do this, not because I agree with them – they are in the main repugnant to me – but because I find the present level of unemployment so *unnecessarily* wickedly high as to make unthinkable *either* an emasculated Pontius Pilate act *or* the attitude of let them – the polties, their advisors *and* the unemployed – stew in their own juice, it can only hurry on the time when the whole system may be overturned (p. 61).

Like much of the rest of the industrialised world, Australia was caught in what the late Arthur Okun (1978) called ‘The great stagflation swamp’. In those years, prices and money-wages were relatively sticky and so any contractionary fiscal and monetary policies designed to drive inflation out of the system had a seemingly irreducible floor, in the USA of about 6 per cent per annum ‘despite massive excess supplies of idle people, machines and plant’ (Okun, 1978: 7), a situation which was prolonged for over three years in the USA.

The Federal government and its advisors seemed not to have learnt from the American experience. Nevertheless, I tried to persuade them to attempt a cautious increase in government expenditure, concentrating on expenditure with a low import content and directed to social purposes. I mentioned the balance of payments constraint but said that if the value of the multiplier was less than two we could probably take it in our stride. If there were to be an accelerator effect, that would be a problem – but with the then level of excess capacity and finance puzzles any lift to investment would be a while coming – by which time I hoped the other measures I outlined would have given Australia a more competitive cost structure.

At that time virtually all governments in Australia were against a rise in government spending and so the second-best policy was a cut in taxes (accompanied by a rise in interest rates). I actually advocated a cut in sales taxes, not only because of the effect on spending but also because of its favourable impact on the rate of inflation. (By the early 1990s I had changed my mind to argue that government spending should be designed according to the longer term needs of the community – subject to the political

philosophy of the party in power – and counter-cyclical policy should be through changes in taxes and interest rates, see Harcourt, 1993b; 1995b.)

I coupled the macroeconomic proposals with a plea for the return of indexation through the Arbitration Commission. I argued that the relationship between real wages and unemployment in Australia (as a dyed-in-the-wool Keynesian, I should have written unemployment and real wages) was complicated, certainly not obviously negative, as was then argued in official circles, and therefore *real* wage cuts were not needed, if they ever were. I also coupled this with an earlier suggestion of a relativities wages fund, whereby the groups who thought they had fallen behind in past years in the wage–wage spiral could argue before the Arbitration Commission for a share in a predetermined amount granted overall, over and above the indexation procedures, in order to restore relativities. The full restoration might well be spread over a number of periods, but the justice of the claim would have been established from the beginning. Such procedures would require further coordination and cooperation – agreement as to what *was* a fair structure of relativities, the target to be aimed at and eventually reached.<sup>16</sup> I concluded by saying that my suggestions for Australia were in accord with the suggestions made overseas by the best Keynesians, Bastards and otherwise – I cited Jim Tobin and Sid Weintraub. I closed by saying that:

If we were not operating within the political constraints imposed by the present government, I would repeat again the need for what Joan Robinson calls ‘a real social contract which would satisfy the reasonable demands of the workers for more control over their own work, more security against redundancy, better social services and so forth; but that [I feared], must await our return to another [better] world’. (p. 69)<sup>17</sup>

In the late 1970s the ALP set up a National Committee of Inquiry into why the party had done so badly in the 1975 and 1977 elections and I was appointed as the economist on the committee. This allowed me to feed these and other ideas into the discussion paper

on economic policy and the future of Australia which we produced, Discussion Paper no. 6 (1979). I drew on a paper I wrote with Prue Kerr (Harcourt and Kerr, 1979), and a review of Hy Minsky's 1975 book on Keynes (Harcourt, 1977c).

I would like to think that the combination of explicit theory and policies within a judgement of what was politically feasible in a given situation and specific period of time would have had Keynes's blessing for it was his own practice that, at least implicitly, I had in mind. Certainly, the most important ingredient of the package deal of policies of the Hawke–Keating governments in the 1980s and early 1990s – the Accord – may find its rationale in these arguments. Of course, I do not claim any originality for them – Ralph Willis, a lone voice crying in the ALP wilderness for many years, was making the same arguments and I was merely making explicit what I had learnt from Russell, Salter, my Cambridge mentors and Kalecki, together with my maturing understanding of Marx's analysis of capitalism.

At the same time as Tommy Balogh and Nicky Kaldor in the United Kingdom were identifying Monetarism as the incomes policy of Karl Marx, I was scandalising the increasingly technocratic and value-free economics profession in Australia by talking explicitly about these attempts to recreate the reserve army of labour. The objectives were to make the sack effective again, to produce a cowed and acquiescent workforce for international and domestic capital to exploit, and to reverse the swing of economic, social and political power to labour which had occurred over the Golden Age back to capital, disguised as a laudable attempt to control inflation. I publicly attacked as hired prize-fighters those economists who were supporting the monetarist/Bastard Keynesian measures designed to bring all this about. In retrospect, I bitterly regret not 'spilling the beans' on a secret meeting I attended at the University of Melbourne sometime in the 1970s at which Heinz Arndt argued that the economics profession had a duty to wean the public off the acceptance of full employment, as it was then understood anyway, as an indispensable object of policy. Of the ten or so Australian Professors of Economics there, only one other was as scandalised by the argument as I was (see Harcourt, 1995b: 31).

## **Return to Cambridge in the 1980s**

In 1975 I chaired a small IEA (International Economic Association) conference at S'Agaro on the microeconomic foundations of macroeconomics. Its theme was, of course, the principal theoretical theme with which I had been grappling since the early 1950s. The volume of the conference, Harcourt (1977d), was not well-received either by its reviewers or the true initiator of the conference, John Hicks, who was distressed by our inability to come up with definitive or at least suggestive solutions to the issues we raised. Nevertheless, I think we acknowledged the difficulties I documented in the introduction to the volume and made some progress towards understanding their source.

In February 1977 Eric Russell died after playing squash. While I had felt torn up by the roots when I left Cambridge at the end of 1966, I nevertheless wanted to stay in Adelaide while Eric was there. Moreover, in 1972 my mother had had a severe stroke and we did not feel justified in leaving Australia while she was alive. But when she died in 1981 and openings occurred in the Faculty and at Jesus, I decided with the selfless support of my family to take them up. The major task I set myself was to write the intellectual history of Joan Robinson and her circle – Austin Robinson, Richard Kahn, Piero Sraffa, Nicky Kaldor, Michal Kalecki, Dick Goodwin, Luigi Pasinetti. I wanted to see whether there was a coherence to the tradition this group of economists both inherited from the classical political economists, Marx and Keynes and passed on, adding, of course, their own very considerable contributions. Virtually all of the group are now dead and since I nearly joined them four times between September 1992 and September 1994 I am rather lagging in getting it all down in book form. But there are now well over 30 separate essays which provide an essential background to the project and now I am retired I hope to get into King's Archives more often to examine the papers of these economists, most of which are housed there. Prue Kerr and I have just started collecting the critical articles on Joan Robinson's contributions for a series of readings which Routledge publishes. Reading these and writing the introduction to the volumes should be a great help in focusing my mind on the other project. I believe it to be a vital project, necessary in order to help to salvage the wisdom contained in the writings of Keynes,

his immediate colleagues and their followers, and Kalecki and his, together with the other strands that fall under the rubric of post-Keynesianism.

I have supplemented this principal project in two ways: first, with Peter Riach who suggested to me that we organise the writing of *A 'Second Edition' of The General Theory* by Keynes scholars from all over the world (Harcourt and Riach, 1997, 2 vols). Of course, the title may be jokey and pretentious, but the intent was serious. We wanted, first, to document as well as could be done what Keynes himself might have written in, say, 1938 or 1939 on certain aspects of *The General Theory*, had he not been so ill from 1937 on. Secondly, we asked the scholars concerned to explain what and why they had done on certain aspects of *The General Theory* (and extensions under the Keynesian umbrella) in the postwar period. Together, this would serve to provide up-to-date reports within an historical perspective at, I believe, a peculiarly significant and appropriate juncture in the development of economics itself and, more importantly, the particular economic problems of much of the world. These were (are) sensible aims, explicitly stated, but, I have to say, wilfully overlooked in two savage reviews of the volumes. If Freudian ideas were still in vogue I would hazard an explanation of the rationale for the reviews. But as they are not, I won't. Moreover, other reviews have been positive and favourable, indeed generous. For example, Tony Thirlwall (1999) has written a veritable *tour de force*, writing most convincingly as John Maynard Keynes reflecting on and reacting to the various themes in the volumes.<sup>18</sup>

Secondly, following the request to give the Second Donald Horne Address in February 1992 I have written a series of policy papers (Harcourt, 1992, 1993a, 1993b, 1994, 1997) which have pulled together the strands of my previous incursions into policy and added new ones in a way which, I hope, is up-to-date, properly relevant and infused with the spirit of the economics of Keynes – and his contemporaries and followers.

The idea of the Horne Addresses is to ask an Australian living abroad to come home to give a wide-ranging address on issues of vital importance for Australian citizens. The conjunction of events to which mine was addressed was the launching of the Republican movement in Australia and the U-turn on economic policy of the Federal ALP government that was then occurring. The background

was the emerging reaction against the 'let the market rip' policies of the 1980s which characterised part of economic policy in Australia, and the crowing over, and then second thoughts about, the implications of the collapse of Communism. I never held any brief for the awful regimes of the USSR and the Eastern European economies but I did point out that the achievements of those Western industrialised capitalist economies that had gone overboard on Hayekian/Friedmanite policies from the 1970s on were not that much to write home about either. There was therefore a case to be made for middle ways – the Kaleckian approach to democratic socialism, for example, for Eastern Europe, the Keynes/Kaleckian (with modern additions) post-Keynesian blueprints for Australia and other similar countries.

I preceded my outlines of middle ways with an account of what modern (and not so modern) theory had to say about the conditions which need to be satisfied for markets to be safely left to do their thing, pointing out that these conditions are spectacularly *not* satisfied in the markets for labour, foreign exchange, financial assets and housing. I recognised that it was a *non sequitur* to jump to the proposition that some form of intervention and regulation would necessarily do better – the case for this had always to be made.

The common theme connecting these papers was the argument that many markets and indeed economic systems themselves are characterised by cumulative causation processes. This viewpoint implies that very different policy proposals and institutions are needed than those associated with the more orthodox view that there are strong equilibrating forces present. Radically different attitudes would be taken towards, for example, speculators and speculation because their systemic effects would not be the benign ones identified by, for example, Milton Friedman (1953) in his well-known article on the case for flexible exchange rates.

The essays on macroeconomic policy in the collection relate principally to the problems of small open economies. It allowed me to ride some hobby-horses, for example, as mentioned above (see pp. 17–18), that government expenditure should not principally be used for pump-priming but rather should fit in with the longer-term needs of economies, taking into account the social and political philosophy of the government in power. I also drew

attention to the danger of forgetting those old-fashioned but profound lessons from the writings of Russell and Salter concerning the macroeconomic effects of incomes policies on rates of accumulation, and from Kalecki concerning the vital differences between getting to, and then sustaining, full employment. Because at the economy-level capital and labour are complements, changing money incomes according to changes in the cost of living *and* overall productivity is not only equitable, it is also efficient. It allows income-receivers to benefit from the growing overall real output of the economy and it encourages investment in profitable, productivity-enhancing industries and hastens the decline of industries whose time has not only come but gone.

In Harcourt (1994), which was entitled 'A "Modest Proposal" for Taming the Speculators and Putting the World on Course to Prosperity,' I tried to set out the problems of the various broad regions of the world, show how they are interrelated and what particular combination of policies and institutions might serve to tackle their problems effectively and simultaneously. I included various 'carrot and stick' measures to induce speculators to behave in ways which were less systemically harmful. My proposal for inducing less speculation on foreign exchange markets was in a sense a generalisation of the Tobin tax (without, I must confess, me having read Tobin's proposal). I suggested that the taxation authorities identify what proportions of foreign exchange transactions of both buyers and sellers could be regarded as speculation and that these proportions carry with them penal rates of taxation on the incomes of the transactors concerned. I resurrected Keynes's suggestion (1936: 160) that there be an inverse relationship between the levels of tax rates and the lengths of time for which shares were held. I argued that unless housing purchases and sales could be shown to be for legitimate social purposes, penal rates of taxation should go with them. There is a Utopian tinge to such an exercise (though I did try to take into account the constraints imposed by present political and ideological climates). Nevertheless, unless such interrelationships and schemes are explicitly set out, it is difficult to get people of good will to think about the causes and cures of the world's ills.

## Conclusion

So: here it is, nearly 50 years a Keynesian, still pottering on and thinking about the themes which excited me so much when I started economics at the University of Melbourne in 1950. I am glad that I have been able to write accounts of many of the leading actors in the drama I have witnessed and documented in places of lasting public record (see for example Harcourt, 1993c, 1995b; Baranzini and Harcourt, 1993; Pesaran and Harcourt, 2000). But, ultimately, it has been the policy aspects of what I have tried to do that I care most about. I am not sure that I really want to be an economic dentist – one actual dentist in the family is enough, I would have thought – but I would like to do my damndest to make sure that those least able to protect themselves from the malfunctioning, natural and also, now, more often man-made, of modern societies are in fact protected. That, for me, as I believe it was for Keynes and his followers, is the proper rationale for our ‘miserable subject’.

October 1999

## Notes

1. The present volume of recent essays and the volume of my essays on policy from the 1950s on.
2. John Hatch and I go back to the 1960s when he was a research student and I was a young don at Cambridge. Subsequently we were to be colleagues for many years in Adelaide. I first met Colin Rogers in the 1980s. When he decided to leave his native South Africa and settle in Australia, he came to Adelaide to the post that became vacant when, having left for Cambridge in 1982, I resigned in 1985. I had known Ray Petrides from the 1970s, both in Australia and on his frequent visits to Cambridge. Philip Arestis, Gabriel Palma and Malcolm Sawyer came to know me well personally when I returned to Cambridge in 1982. I first met Sheila Dow in 1980 but we had corresponded before that.
3. A précis of the insights so gained is in paras 12 and 13 of Harcourt and Massaro (1964a; 1982: 178–9).
4. ‘But this *long run* is a misleading guide to current affairs ... Economists set themselves too easy, too useless a task if in tempestuous seasons they can only tell us that when the storm is long past the ocean is flat again’ (Keynes, 1923: 65, emphasis in original).
5. We were also referred to Lorie Tarshis’s (1947) marvellous textbook, but I was too dumb to see that it was the better basis for a proper

understanding of the economics of Keynes. I also read Harrod's (1951) *Life* which is often more his than Keynes's.

6. Tom Wilson (20 August 1999) writes that in the 1950s he 'rejected the generally accepted claim that oligopolists didn't try to maximise profits. The empirical evidence did not support the assumption [it was an error to suppose] that profit margins and ... prices were ... arbitrary ... price competition was present in price setting.'
7. Henderson subsequently went to Australia to set up an Australian equivalent of the DAE/NIESR at Melbourne University. I first met him in 1954 when he was a 'small l' liberal aligned with the isolated minority around Dennis Robertson in the Cambridge Faculty (and the much respected Treasurer and Fellow of Corpus). He was on leave from Cambridge at Melbourne and we shared a room. Downing suggested that I try out my questionnaire for the pilot survey on Henderson as he had a most complicated income and wealth situation. He agreed, the questionnaire 'worked' – but he told me that had I or one of the other interviewers knocked on his door because he had been chosen in our sample, he would have refused to answer on grounds of invasion of privacy. I tell this anecdote because Ronald was to become famous for his pioneering enquiry into poverty in Australia which required much more intrusive questions than those of my questionnaire. He also became an early proponent of an incomes policy, including the indexation of money-wages, to complement monetary and fiscal policies in Australia – a very changed (for the better) set of principles.
8. Marris helped Piero Sraffa run the research students' seminar and generally offer pastoral and other advice.
9. This was to be a temporary, not a permanent state; see Harcourt (1988) for my appreciative evaluation of Nicky.
10. Trevor Stegman (2 June 1999) writes that I should add 'in an economy operating below full employment'. There are vital differences between the two situations, in that more saving – not spending – would be required in the full employment situation, but planned additional investment would still need to be financed before it could become actual investment. I hope this goes somehow towards answering Tom Wilson's criticism (20 August 1999) of the same passages.
11. Of course, inflation proved disobliging by 'going away' for several years after I submitted my dissertation. In the 1970s I appeared before Russell Mathew's committee on taxation reforms in Australia and replacement cost accounting (Russell and John Grant were the Australian pioneers on these issues). In response to questions from Russell, I said: 'The trouble with you and me being so far ahead of our time is that when our time comes, I at any rate have forgotten what I said.'
12. In my view Lucas should get credit for independently rediscovering the essence of Keynes's critique of Tinbergen's 1930s empirical work on the investment function, renaming it the Lucas critique – otherwise, I think his influence has been disastrous.

13. At this time Vincent Massaro and I were reading Sraffa's 1960 book and discussing his sub-systems with him prior to writing our two papers (1964a, 1964b) on it.
14. Many of these ideas went into my lectures at Cambridge as well as the chapters of *Economic Activity* which was then nearing completion. I also used the latter for the course on macroeconomics I gave to British civil servants at the Civil Service training centre near London Zoo. Roger Opie gave the microeconomics lectures, so there are several cohorts of British civil servants who were first introduced to economics by two dinky-di Australian Keynesian economists. I understand there was a file in the UK Treasury on my investment-incentive writings; I hope it was more benign than the one which was soon to be started by the Australian spooks when, having returned home in early 1967, I became one of the leaders of the anti-Vietnam war movement in South Australia.
15. Tom Wilson (20 August 1999) suggests that we are now in the Second Golden Age in the USA and the UK – low unemployment, low inflation, decent growth with a lousy (my word) distribution of income and wealth. I would prefer to call it, following Joan Robinson, a Bastard Golden Age because I do not think that by the standards of the first Golden Age, unemployment may be regarded as 'low'.
16. Tim Harcourt e-mails (30 June 1999) that something similar to this was actually brought in by Bill Kelty (the General Secretary of the ACTU) during the Accord years, showing that 'the Australian system provided the institutional flexibility to deliver such measures.'
17. I had started the paper by saying I was going to be 'very old-fashioned' in it. I look now as if I had just come out of the Ark.
18. Tony also gave me an earlier opportunity to help keep the Keynesian tradition alive by asking me to chair the sixth and centennial Keynes seminar at the University of Kent in 1983, on Keynes and his contemporaries (see Harcourt, 1985).

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