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'Horses for Courses': The Making of a Post-Keynesian Economist (1999)*

Introduction

Arnold Heertje's invitation to contribute to his series came at an opportune time, just a few days after I had been presented with the volumes of a *Festschrift* organised by Philip Arestis, Gabriel Palma and Malcolm Sawyer (1997a, 1997b) at my Cambridge college, Jesus.[†] I have been reading the essays in the volumes by friends and colleagues (an overlapping set) with gratitude and absorption; I have also been reflecting on their interpretations and explanations of what I have done. Sometimes these have been of considerable surprise to me. For example, John King sent me the first draft of his essay (King, 1997) which partly revolved around my first book, *Economic Activity* (1967). I wrote the book jointly with Peter Karmel and Bob Wallace and I had to point out to John that many of the perceptive and ahead-of-their-time insights of the Young Harcourt were in fact those of the Young Karmel and/or the Young Wallace. Bruce McFarlane (1997) perceived in his reading of some of my papers on socialist investment and planning, insights and foresight which, while most flattering, reflect more his own 'before the time was ripe' contributions than mine; and so on. Nevertheless, I welcome the opportunity, which Heertje's invitation provides, to try to put a structure on what I have attempted to do over a working life from which I am now approaching 'retirement'. I cease to be officially employed by the University of Cambridge and Jesus in September 1998 as I shall be 67 in June 1998.[1]

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Major influences

In order to make sense of what I have tried to do in economics, or, more accurately, what I have tried to do as a teacher, researcher, 'back room boy' policy maker and political activist, I need to stress (apart, of course, from the generous support of university friends and colleagues wherever I have been working) the major influences on me: religion, politics, sport, and last (but *not* least), marriage and family life generally. It is no accident that I wrote the opening passages of the essay in Rome while visiting our oldest child, Wendy, her husband, Claudio Sardoni, and our *numero uno* (and, to date, only) grandchild, Caterina. (Responding to the copy editor's queries in January 1999 allows me to report the birth of Emma-Claire Sardoni in Rome in November 1998.) Whatever I may have achieved in economics and other spheres would never have been possible without the extraordinarily generous love and support of Joan, of our four children, Wendy, Robert, Timothy and Rebecca (and their partners) and now, of course, Caterina and Emma-Claire and a host of relatives scattered around Melbourne and Sydney. The life of a scholar as well as those of a political activist and sports person inevitably have selfish aspects to them and the persons concerned are crucially dependent on the selfless support of their loved ones. I have always received this in abundance, for which I am more grateful than I can express in words.

I turn now to the influences of politics and religion. I was born in Melbourne, Australia, on 27 June 1931 in the main bedroom of the family home in which my twin brother John (who has just retired after a distinguished career as an academic dentist) still lives. My parents, Marjorie and Kenneth Harcourt, lived there for the 55 years of their married life. Sadness pervaded the house because an older sister, Robyn, was a blue baby and died in her fourth year when we were under two; this profoundly affected our parents though they rarely spoke of it or her.

Melbourne then was a stuffy, snobby, sectarian city, narrow-minded and intolerant. The Roman Catholics hated the Protestants (and vice versa) but they formed a united front when it came to the Jews and, indeed, foreigners generally. My parents were right-wing, agnostic (for most of their lives) and assimilationist Jews. They were born in Australia and, especially in my mother's case, were most anxious to be accepted as insiders, an impossible dream, I fear. I had fierce quarrels with my parents when I was young – who has not? They were mainly about religion and politics and 'correct behaviour' (very important to them). Moreover, I left home when I went to university, partly in order to establish my independence from my mother's possessive love. (My father understood that parental love consists just as much of letting go when the time is ripe as of care and support.) But I must stress that both of them were warm-hearted and fundamentally good, trying hard to do what they thought was right. Indeed, my father, who was much loved and respected, exhibited the old-fashioned virtues in his personal and business life (he was a leather merchant). His selfless devotion to and love of my mother following her severe stroke when she was 72 and he was already in his eighties, were of heroic proportions. I would have taken a different stance to them on our Jewish origins but it is easy to say this in another time and when not faced with the terrible uncertainties of the years when I was growing up. I have written elsewhere (see Sardoni, 1992) of how my early experiences of religious and political intolerance left indelible marks on me. My only excursion into journalism was to describe my experiences of anti-Semitism at the private secondary school to which my brother and I went in 1945 (see Harcourt, 1960). (There had already been unpleasant episodes related to our Jewish origins at our primary school which shook and disturbed me.) In Sardoni (1992), I described how starting economics in 1950 in the 'Heaven on Earth' atmosphere of Melbourne University quickly converted me to democratic socialism. Conversion to belief in God was a more prolonged, difficult process. I had no trouble in reconciling the precepts of Christianity with democratic socialist economic principles – indeed, I cannot see how any other conclusion logically may be reached. I was held up by problems of personal faith, and the observed inconsistencies between the practices of many Christians and their professed ideals and beliefs, for what I realise now were mainly priggish reasons.

Once I convinced myself that, despite my associations with Wesleyanism (my secondary school, Wesley College, and university college, Queen's, were Methodist institutions), I did not need to have a warm feeling somewhere near the pit of my stomach before I could believe, I coupled my democratic socialist beliefs with a decision to regard belief in the Christian God (all Three) as a working hypothesis: let us suppose it to be true and see how we go from there. I took that step in 1953. I still regard it as a correct decision – my working hypothesis has not let me down.

I became a democratic socialist because I thought that the existing ways in which our economic and social affairs were organised were unjust and irrational. A description, in the first year lectures on economic geography, of how the development of the Californian oil fields by private business had caused a large permanent loss of natural gas and oil first alerted me to the irrationality of unfettered competitive capitalism. I felt that any vital resources which were meant to serve the needs of both this and future generations should be developed by organisations with suitably long time horizons, so I argued for the nationalisation of all such resources. My basic reasoning, I believe, was correct, but I was naive to expect that politicians and managers of nationalised industries would necessarily have longer time horizons or more enlightened attitudes than the boards of private enterprises.

It seemed to me irrational that capitalist institutions should push the exploitation of human selfishness (often called freedom of choice to make it sound better) so far to the front; while simultaneously stunting the development of the Christian (and humanist) virtues of altruism, cooperation, tolerance, compassion, and striving for consensus, not to forget forgiveness. I believe that the only essential difference between socialist humanists and Christian socialists is that the former think that people can bring about desirable reforms by their own unaided efforts. By contrast, Christians believe that it is only by the workings of the Holy Spirit in and through individuals and those individuals in turn working in and through secular and religious groups, that people may be able to achieve what the humanists think people can do unaided. Maybe Philip Wicksteed, a hero from my undergraduate days, put it too strongly: 'If there is no descent of the Holy Spirit there can be no Kingdom of God, in spite of the most perfect and ideal social machinery' (quoted in Steedman, 1994, p. 87). But then, maybe not.

The Holy Spirit works in at least two ways. First, it removes the intolerable pressures associated with the impossible task of achieving and maintaining personal perfection (think of poor Wittgenstein). This was a preoccupation of Evangelicals during my youth in Melbourne, in my view a perverse and unhealthy obsession with gazing at their own navels. Secondly, the Holy Spirit helps believers to work vigorously and with some chance of success, or at least optimism, within the social and political institutions of their societies. I explicitly rejected the view that religion is related to individual but not to group or social behaviour. I was never under any illusion as to the chances of complete success. Just because a cause or course of action is right and just there is no guarantee that it will succeed. But I do believe that to strive for the Kingdom of God on Earth is both correct and feasible for committed democratic Christian socialists, even if I am also bound to accept that a just and equitable society is unlikely ever to be fully realised. At one stage on the way I was tempted to become a Roman Catholic, but a crucial sermon in which the preacher set out the characteristics of the Roman Catholic, Evangelical and Protestant versions of Christianity led me inexorably towards the last. So I was baptised in 1953 in Queen's College Chapel just before I joined the Australian Labor Party (ALP) (a lagged relationship, my political beliefs having been formed by mid-1950), and when I took up my first lecturing post in Adelaide in 1958, I began to call myself the only Jewish Methodist in that fair city.

School days and undergraduate days in the 1940s and 1950s

At secondary school I did mainly science subjects (very badly) because I wanted to be a vet. I took economics as an extra 'to make up the numbers'. The two economics books which stand out from those years are Hubert Henderson's *Supply and Demand* (1922) and J.R. Hicks's *The Social Framework* (1942). Henderson explained in clear prose the process of choice at the margin, especially the key role of the marginal purchaser who really cares and so helps the rest of us who don't – that much – when, for example, we purchase a tie! Hicks used the emerging national accounting framework to structure his discussions of the workings of a modern economy and of the historical development of different economic systems. This ideal introduction to the study of economics received Joan Robinson's blessing years later in her essay (1960) on how economics should be taught so that the young are not corrupted.

I spent three years in the Sixth Form taking the Matriculation (university entrance) examinations, struggling to get a Pass in Physics, a prerequisite for admission to the Vet School. I matriculated in 1949 (passing the dreaded Physics at the third attempt), but decided that I would rather be a school-teacher, teaching economics, not least because, although I was becoming enormously enthusiastic about the subject anyway, I could also get a scholarship to see me through university if I agreed to be a teacher on graduation (no other scholarships were then available).

In 1950 I enrolled for a B.Com., a three-year Pass Course in which you could elect to take Honours in particular subjects by going to extra lectures and answering Honours questions in the examinations. I did this, going to lectures on *A Tract on Monetary Reform* (Keynes, 1923), my first introduction to the great one (inevitably pronounced 'Keens' until we were corrected) and lectures on Wicksteed's *Common Sense* (1910) and the sections on indifference curves in Hicks's *Value and Capital* (1939). I read now with pleasure Ian Steedman's wonderfully detached and detailed accounts of my hero Wicksteed (see for example Steedman, 1989, chs 7 and 10; 1994) and I can still remember the lectures on how allocation in the Victorian/Edwardian Wicksteed household illustrated the workings of a great nation[2] and how Wicksteed taught the notion of the margin and of an instantaneous rate of change without letting on what he was doing, while at the same time making any self-respecting person persevere by the strategically placed remark that failure to read through volume I of *The Common Sense* is a sign of intellectual shallowness.[3] I topped the first year of the course and, encouraged by my first mentor, Joe Isaac, who had tutored me at Queen's, left the Victorian Education Department and enrolled in the Honours course, hoping to become a university teacher rather than a secondary school teacher.

The Melbourne Department was very, even self-consciously, Cambridge-orientated (Joe, though, had done his PhD at LSE in the company of Frank Hahn). Marshall, Pigou, Keynes, Dennis Robertson, Maurice Dobb, Austin and Joan Robinson, Richard Kahn and, especially, Piero Sraffa became household names to us students. I say 'especially Sraffa' because one of my teachers, the distinguished economic historian, John La Nauze, greatly admired the Ricardo editions and his close colleague, G. S. L. Tucker, was Sraffa's student. Tucker had just gone to Cambridge to write the PhD dissertation which was the basis for one of the finest books in the history of economic theory, *Progress and Profits in British Economic Thought 1650–1850* (1960). Sraffa was spoken of with awe by La Nauze, Tucker and others at Melbourne, a feeling which I have never lost (not even when, fortified by a couple of whiskies, I went with Vincent Massaro to see Sraffa to discuss, that is, scream at one another, a draft of a note on sub-systems which we wrote in 1963; see Harcourt and Massaro, 1964a).

We were introduced to Michal Kalecki (by Joe Isaac) and Maurice Dobb (by Jim Cairns, who subsequently became the hero of the anti-(Vietnam) war movement in Australia in the 1960s and 1970s, as well as Deputy Prime Minister of Australia). Joe took us through the steps of the argument of Kalecki's theory of distribution, got us all excited, then translated the argument into terms of a trivial problem in order to show that in his view it was mere manipulation of identities, a real let down which I can still feel. Joe was, as always, gentle in his critique but I think he missed the point that there *were* refutable relationships in Kalecki's argument. Dobb on the development of capitalism from Feudalism (1946) and, especially, on *Political Economy and Capitalism* (1937) was to have a lasting effect on my thought, though I needed the instruction of three of my students, Alan Oakley in the 1970s, Prue Kerr and Claudio Sardoni in the 1970s and 1980s, to make me begin to understand the true subtlety and richness of Marx's theory, method and approach.[4]

In those days at Melbourne University you had to take in your third and fourth Honours years a number of compulsory papers, two specialisations, and write a 30 000 word dissertation. As my specialisations I took, first, History of Economic Thought (HET). We read the greats as each one of us had to prepare a seminar paper on Smith, Ricardo, Marx, et al. (I did J. S. Mill.) For my second specialisation, I took Mathematical Economics (an ideal complement to HET) – the value chapters and appendices of *Value and Capital* and the classic papers on duopoly, then all the rage. We were also taken through Dick Stone's 1945 *Journal of the Royal Statistical Society* article by Don Cochrane (of Cochrane and Orcutt fame) who was then teaching at Melbourne. I have described my traumas with mathematics elsewhere, as well as my view of its role in economics – a good servant but a bad master (see Harcourt, 1995a).

I want to spend a little space on the subject matter of my undergraduate dissertation. The single most influential article of my undergraduate years was Kurt Rothschild's 'Price theory and oligopoly' (1947). The combination of having read it and of my introduction to *The General Theory* in the second year spurred me on to see what differences oligopolistic behaviour at the level of the firm makes to the systemic results of the analysis of *The General Theory*. The immediate focus of my dissertation came from the sections in the consumption function chapters on 'financial prudence', 'writing off' the book values of durable assets well ahead of the need to replace them, so that total investment has to be all that greater in order to offset this compulsory saving before investment has an expansionary impact on the economy (Keynes, 1936, pp. 98–104). I wondered what differences it would make if instead of competitive price-taking behaviour we had Rothschild's strategic oligopolists indulging in a number of different sorts of price-making behaviour, desiring secure profits as much as maximum profits. I established a few rather incoherent inferences in the first chapter and then, in order to test them, made up funds statements from the profit and loss statements and balance sheets of various Australian companies over the depression years. (The Professor of Accountancy, Lou Golberg, had a splendid collection of published accounts of Australian companies.) It was an untidy and incompletely thought out piece of research but it was the beginning of a sustained train of thought, aspects of which I have worked on ever since.[5]

Thus my PhD dissertation at Cambridge was initially to be on the implications for the theory of the firm and of the trade cycle, of the assumption that firms desire secure profits as much as maximum profits. It ended up as a study of the implications of the use of historical-cost accounting procedures to set prices and dividends, and levy taxes in a period of inflation. I used the data set that, first, Brian Tew and Ronald Henderson and then, later, Ajit Singh, Geoff Whittington and Geoff Meeks were to use (see Tew and Henderson 1959, Singh and Whittington 1968, Meeks 1977). Perhaps the original intention was not *that* far departed from?

I graduated from Melbourne University at the beginning of 1954. I then wrote a Master's degree dissertation (on a pilot survey of income and saving in Melbourne) as the first research assistant to Dick Downing who had just taken up the prestigious Ritchie Chair of Economic Research at Melbourne University. I learnt how to do applied research and Downing introduced me to the Fowler brothers and Sir Ernest Gower, which taught me how to write.

Cambridge and King's in the 1950s

In 1954 I was awarded a travelling scholarship from Melbourne University to go to King's College, Cambridge in October 1955. Joan and I were in effect on our honeymoon – we were married on 30 July 1955 and left Australia two weeks later. It took us nearly five weeks by ship to get to the UK. Initially, Nicky Kaldor was my supervisor. He went on leave in 1956 and I transferred to Ronald Henderson. In my first Michaelmas term I wrote a paper on my topic. I read it as first cab off the rank to the research students' seminar run by Piero Sraffa assisted by Robin Marris, and regularly attended by Joan Robinson. The new research students had been gathered together at the beginning of the term by Marris and asked what they would like to talk on. When I mentioned my topic, Marris pounced: 'You're first!'

I wrote an outline of what I thought the implications were. It was pretty incoherent. I did not see the answers clearly until I turned a pithy paragraph by Joan Robinson (1965, p. 177) into maths and a diagram in chapter 5 of *Some Cambridge Controversies…* (Harcourt, 1972, p. 210): to wit, that when we have price-makers and retained profits are an important source of finance for investment, so that prices are set partly to finance investment, systemically something akin to the paradox of thrift *cum* the balanced budget theorem occurs. In some circumstances we get the paradoxical result that the Kahn/Keynes multiplier process may be negative!

The microeconomic aspects of this process were analysed in greater detail in an article on 'Pricing and the investment decision' which Peter Kenyon and I published in *Kyklos* in 1976. The article had a long gestation period. I wrote the first draft in 1966 and sent it to the Oxford *Bulletin*. It was refereed by G. B. Richardson, an unsung hero of British economics. He liked the idea but found a serious flaw in the argument. I put the paper aside and it was not until I ended a three-week spell in hospital in 1974 and went straight from hospital to a seminar by Kenyon that the solution came to me. I wrote out the skeleton of the argument when I got home that night and asked Peter to put the flesh of scholarship on it for the first draft. In Harcourt (1997a) I conjecture that the economy-wide implications of our analysis may be a serious obstacle to establishing sustained full employment without 'too much' inflation.

Henderson read my undergraduate dissertation in January 1956 and said, 'Right, I'm sending you to the NIESR to write two empirical reports.' One was on the woollen and worsted industry, the other was on the chemical industry. The object was to try out the data set which was then being transformed into the aggregate profit and loss accounts, balance sheets and funds statements for what was to become *Studies in Company Finance* (Tew and Henderson, 1959). So, for two terms, I spent 2–3 days a week in London at the National Institute of Economic and Social Research (NIESR) gathering the raw data. The Director at the time was Bryan Hopkin and Christopher Dow was prominent amongst the research workers. I remember that Dow, in a state of high euphoria, took me for a beer one evening while he outlined the imported-cost inflation model for which he is justly famous (Dow, 1956) – he had literally just finished the first draft. I did not understand him very well – the combination of his euphoria and English beer did not make for clear thought. But, some process of osmosis must have occurred for my own work was subsequently to be in this tradition, a tradition associated in Australia with Eric Russell, Trevor Swan, Russell Mathews and John Grant (Mathews and Grant, 1957, 1958).

In my first year as a research student I went to Joan Robinson's lectures on what became *The Accumulation of Capital* (1956), often with Tom Asimakopulos and Keith Frearson. I did not make that much sense of the lectures, so when in my second year the book was published I locked myself up for a term with it, determined to try to understand. I emerged to read a paper on the main propositions to the research students seminar, taking two sessions to do so. Joan was invited to a third session to answer questions – Marris was in the chair for all three. The author ticked us off for only asking technical questions – as research students we were rather hung up on economics for economists, Price and Real Wicksell effects and all that – rather than discussing broad conceptual issues and the sweep of the analysis. I have more sympathy with her response now than I did at the time, no doubt reflecting the fact that I am *now* 14 years older than she was *then*.

We also had seminars on recent topics in the literature. Malcolm Fisher introduced us to the literature on the permanent income and life cycle hypotheses. Joan Robinson took us through the optimum saving literature. Amartya Sen[6] impressed us all by having the paper he read to the class published in the *Economic Journal* (1957). It certainly provided inspiration. The last paper I gave to the Thursday seminar before returning to Australia was read from the proofs of an article I published in *Accounting Research* (Harcourt, 1958) on the implications of changing the company tax base to a measurement of income based on replacement costs. Sraffa threw me completely at the beginning of the seminar. I gave the orders of magnitude of the differences between depreciation at historical cost and at replacement cost; he asked why anyone should wish to compare them!

First years at Adelaide

When we left Cambridge for Adelaide in early 1958 I had written a (very rough) first draft of my dissertation and I had started to absorb the framework of *The Accumulation of Capital*. With hindsight, I see that I had returned to my undergraduate interest in Kalecki, in particular, to the Marxian schemes of reproduction coupled with the Keynes/Kalecki theory of effective demand – their solution of the realisation problem – together with beginning to understand the macro theory of distribution and how its ingredients were the accumulation plans of the capitalists, the different saving behaviour of groups in the economy and the pricing policies of price-making firms. That is to say, I was absorbed in mainly classical problems done in the modern post-Marxian and post-Keynesian way. I was mostly interested then in the different systemic effects (as I would say now) according to whether historical costs or replacement costs were the base for the mark-up. I was not yet analysing the choice of technique nor theories of technical progress nor capital theory as such. (As an undergraduate I had been fascinated by problems in capital theory and had read Schumpeter, Boulding and Hayek (*The Pure Theory of Capital*, 1941), the last while waiting to go on in my 'bit' part in the college play.)

While in Cambridge I read Joan Robinson's 1953/54 *Review of Economic Studies* article but understood little of it. I was pretty much at sea when I met with Pasinetti in Caius to discuss the analysis of the choice of technique and Wicksell effects in her 1956 book, *The Accumulation of*

Capital. Indeed, I was much more interested in tax reforms – changing the company tax base to replacement costs – and accounting reforms so that pricing policies would be more 'logical', much influenced by the writings of Russell Mathews and John Grant which I started to read in 1957. This changed the direction of my research and I proceeded to do for UK data what they had done for Australian data – construct estimates of stock appreciation and capital consumption at replacement cost in order to see what 'true' profits were and relate the underlying changes to the trends of financial ratios in balance sheets. The original 'bits' I added were to put their micro models into the basic macro model of *The Accumulation of Capital* and also to make a preliminary investigation of the effects of different investment incentive schemes within this framework.

In 1958 I started my 27 years of happy association with Adelaide gown and town. Its economics department was then headed by a young Professor, Peter Karmel (who was only in his twenties when he was appointed). The social and political environment then was benign in many dimensions: universities were starting to be well funded, governments were calling more and more on economists for advice, the Australian economy was to share in the prosperity and optimism of the Golden Age of capitalism. As well as a close friend from Melbourne days, Bob Wallace, the department had the person who was to become my greatest mentor and friend in Australia, Eric Russell. A contemporary of Peter Karmel at Melbourne, Eric had gone to King's in the 1940s to do a second undergraduate degree; he was in the cohort which included Robin Marris, Harry Johnson, Aubrey Silberston, I. G. Patel and Frank Davidson. Though he had published little when I came to Adelaide he was already noted for his paper with James Meade on how the Australian economy worked (Meade and Russell, 1957), and for his part in helping the trade unions in their annual basic wage cases presented by their youthful charismatic tearaway advocate, Bob Hawke (who subsequently became Labor Prime Minister of Australia in the 1980s and early 1990s). I learnt a tremendous amount from Eric about how to 'do' theory, applied work and policy, especially about relating policy to the way the economy worked while simultaneously respecting the lessons of fairness and decency.[7] In Harcourt (1997a), I returned to Meade and Russell and the work Eric did on wages policy in Australia in the 1960s (Russell, 1965). I allied it with the analysis of Wilfred Salter's 1960 classic and a fundamental Kaleckian insight (1943). Kalecki highlighted the difference between the political economy of *getting* to full employment after a slump, on the one hand, and the political economy of *sustaining* it, on the other. I suggested 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 advocate to the Industrial Relations Commission in the recent (1996–97) Living Wage case (partly through the good offices of our son, Tim, who is a research officer at the ACTU). Alas, they did not carry the day with the Commission.

The first six years at Adelaide were amongst the most happy, exciting and productive of my life, though much of the final output only appeared in the next phase of my life. In 1963 I returned to Cambridge on study leave. To my amazement, I was appointed in November 1963 to a University Lectureship and elected soon after to a Fellowship at Trinity Hall. Cambridge was then one of the most outstanding departments in the world (hence my amazement) and an extraordinary environment in which for someone in their early thirties to teach and research. I only held these posts for two years and one term on unpaid leave from Adelaide following my year of study leave, 1963–64, because I felt I had a moral obligation to return to Adelaide.

A young don at Cambridge in the 1960s

By the time I returned to Cambridge I had written, in addition to the offshoots from my PhD dissertation, a review article of Salter's 1960 book (Harcourt, 1962), a paper criticising Kaldor's (then) theories of distribution and growth (1963; 1982) and the first draft of what, next to my 1969 Survey article on capital theory in the *Journal of Economic Literature*, is my best-known paper, 'The accountant in a Golden Age' (1965a; 1982). The paper on Kaldor was critical of his assumption of the need for full employment for his distribution mechanism to work. I asked: what pricing policies would the decision makers in the investment goods and consumption goods sectors need to follow in order that the mechanism worked *and* planned investment became actual investment? By showing that they would be very peculiar indeed in a number of different scenarios I hoped to show that it would be a good idea, not to scrap the distributive mechanism, but to drop the assumption of full employment which was so unKeynesian – and unnecessary. While the paper was critical it also laid the foundations for more positive developments which came in Cambridge. There, following hearing Bob Solow's 1963 Marshall Lectures on two mythical creatures, 'Joan' and 'Nicky', working with Vincent Massaro on Sraffa's *Production of Commodities* (1960) and following up the work I had done in Adelaide on Salter's book, I wrote the first draft of what is my favourite theoretical paper, 'A two-sector model of the level of employment and the distribution of income in the short run' (1965b; 1982). It was originally presented on a nostalgic return visit to Sraffa's research students' seminar, to an audience which included such well-known research students as Joan Robinson, Richard Kahn, John Cornwall, Vincent Massaro, Robin Matthews, Ken Arrow, James Meade, Bob Solow, Luigi Pasinetti and Piero Sraffa himself.

I also finished 'The accountant in a Golden Age' during 1963–64. The project was originally suggested to me by Harold Lydall (he had succeeded Karmel at Adelaide). He was puzzled by some arithmetical examples he had worked out in which the accounting rate of return was not equal to the internal rate of return. I set the issue in the context of a Golden Age where answers are always known because expectations are always realised. I asked the question: if an accountant were to be let loose with his [sic] box of tools in a Golden Age would he come up with the correct answer for the rate of profits (which we already knew)? The answer was, in general, no, often by large margins. The result has stood the test of time, despite the efforts of John Kay to destroy it (see Kay, 1976, 1978, Wright, 1978), and its independent rediscovery by Franklin Fisher in the 1980s (see Fisher and McGowan, 1983; Fisher, 1984).

Writing the review article of Salter's book opened a rich vein of research interests for me for the next ten years. In 1964 I reviewed for the *Economic Journal* Minhas's book (1963) on the CES production function, factor reversals and empirical tests for them. I used Salter's work on vintages to point out that the data from which the CES function was estimated were inappropriate total or average data and that there was no clear relationship between the desired marginal values of the parameters of the 'best-practice' techniques and the total values of the data used. Moreover, Minhas used accounting rates of profit data which were 'hopeless' indicators of the 'true' economic rates of profit needed. The review was the embryo of a paper which I published in the *Review of Economic Studies* in July 1966. The article was an ironic piece criticising conceptual foundations. One referee and a number of readers interpreted it as inadequate and amateur econometrics. Bob Solow (1997) called it an 'atypical paper' (by me) but I like to think it belongs to a class of papers I have written over the years. Certainly, the idea for the papers I wrote with Peter Praetz and Al Watson on CET production frontiers in the 1970s (see Watson, Harcourt and Praetz, 1970a, 1970b), came from this paper. In it I asked the question: suppose we grant the neoclassicals everything except their abstraction in early aggregate production function exercises from the existence of vintages, will the methods used in the initial work on CES production functions allow unbiased estimates to be made of the value of a key parameter, the elasticity of substitution of capital for labour of the Salter 'best-practice' isoquant? I tried to show by using a series of hopefully plausible growth scenarios with vintages, that large and unpredictable biases emerged. In the latter papers, we made up a number of 'worlds' in which we knew the values of the CET parameters. We then argued that the econometric method used by Allan Powell and Fred Gruen (1968, 1970) to estimate them did not allow the authors to come up with unbiased estimates of the 'true' values. As their work was directed at policy proposals associated with estimates of supply responses in Australian agriculture, we thought it worthwhile to check the reliability of their method (but see Ray Byron, 1970, on the reliability of *our* method).

A natural next step came when Audrey Silberston asked me to Nice in 1966, where 20 or so Eastern European and Russian economists and a corresponding number of Western economists gathered together for an International Economic Association Conference on 'Plan and Market'. I wrote a paper on choice of techniques rules in Western and planned economies. I started at the level of the individual firm faced with Salter isoquants for its investment decision and asked: what investment–output and investment–labour ratios would be chosen, according to the rules used, and could anything in general be said about the ordering of the ratios which resulted? Western governments then, especially in the UK, were experimenting with all sorts of investment–incentive schemes, so I also did a taxonomic analysis of the effects on the choice of technique adopted of different schemes – accelerated depreciation, investment allowances, cash grants, and so on – when they in turn were combined with the different investment–decision rules. I came up with the, perhaps surprising, result that with the orders of magnitude usually met in the real world, the pay-off period criterion resulted in the most investment–intensive techniques being chosen (see Harcourt 1968; 1982).[8] (By the time I wrote *Some Cambridge Controversies*… I had reduced the analysis to a simple diagram which consisted of a 45 degree line and three concave to the origin curves. It allowed the essential economics of the findings to come out starkly and, I like to think, crystal-clear! (See Harcourt, 1972, p. 64.)

While at Cambridge I wrote the first and second drafts of *Economic Activity* (Harcourt, Karmel and Wallace, 1967). The book was based on the lecture course on Keynesian economics which first Peter Karmel, then I and then Bob gave at Adelaide, and which I gave at Cambridge after Robin Matthews went to Oxford as Drummond Professor in 1965. I used to tell the Cambridge students: '30 years ago, Maynard Keynes lectured from the proof sheets of *The General Theory* to a select group of undergraduates and others. *I* am not Keynes and you *may* not be as select, but I am lecturing from the proof sheets of a book on Keynesian economics.' (Mervyn King, now Governor of the Bank of England, was at the lectures.) I still think the book is a clear account of the 'state of the art' at the time. Especially is it noted for making all assumptions explicit and for stressing the limitations as well as the illuminations of the analysis. We also made the correct choice of not using IS/LM as the core model.

The wider political background of the time was, in my case, a growing realisation of the immorality of the war in Vietnam and of Australia's disgraceful role in it as one of the USA's few 'respectable' allies, a role adopted in order to achieve political ambitions at home and some ill-defined, long-term insurance policies abroad. The details of the background to the war itself were provided for me by Martin Bernal and Ajit Singh in particular. The Faculty at Cambridge and its visitors were, like most other communities then, torn in two between hawks and doves, especially in the early 1960s. For example, in 1963–64, Ken Arrow was a dove, Bob Solow was still a hawk and many of the Faculty were with him. But by the end of 1966 when I left for Australia there had been a marked increase in the number of doves, including Solow who courageously spoke out for his changed position.

Anti-(Vietnam) war activities and controversies in capital theory

I had been a member of the ALP since 1954, President of my local sub branch in the late 1950s, early 1960s and also active in penal reform as Secretary of the Howard League for Penal Reform (S.A. Branch). But I was very much a political animal within the normal channels. This changed when I came home, scandalised by the war and Australia's role in it, including the immoral conscription by birthday ballots of 18-year-old males. I was soon radicalised by the emerging student and anti-war movements of which I immediately became a part in 1967 – a founding member of the Executive Committee of the Campaign for Peace in Vietnam in South Australia and subsequently for two spells, its chairman (as it was called in those less 'politically correct' days).

The two most intellectually important influences on me articulating my new position on direct action and involvement were Noam Chomsky's essay 'The responsibility of intellectuals' (1967) and Hugh Stretton's *The Political Sciences* (1969); the practical influences were the day-to-day experiences of helping to organise a protest movement. My economic analysis started to change as well; not only was my personality intruding more into my writing, but also, as I no longer accepted that ideology and analysis could be separated, I made the former explicit in my writings and my teaching, especially by the end of the 1960s.

I have described elsewhere (see Sardoni, 1992, pp. 5–6), my experiences of those years and those of Joan and the children, now four (Rebecca, our fourth child, was born – when else? – on 10 May 1968, the day the French riots started), with the attendant death threats, potential and, on one occasion, an actual attempt, and so on. Here I want briefly to describe the events which were to change my academic life.

In August 1968 I heard a mysterious rumour that Mark Perlman wanted to me to do a 'Hahn and Matthews' on capital theory for the newly formed *Journal of Economic Literature*. Sure enough, Perlman arrived in Adelaide to ask me to do just this – evidently, the person he initially commissioned had pulled out and Perlman was left in Australia with a new journal to edit and no author for the survey article for the second issue. Wilfred Prest, the Professor at Melbourne when I was an undergraduate, suggested to Perlman that he get me to do it.

Following a day's 'hard sell', I said 'yes'. I partially separated from the anti-war movement (with the blessing of my comrades there), closed a door which was usually open and wrote the first draft in four months (see Sardoni, 1992, pp. 6–7). Having to do so wonderfully concentrated the mind, for while I was aware of the issues in a vague, general way I did not fully understand them, especially the details of the reswitching and capital-reversing debates. Reading the literature more closely, in particular a draft of the wonderful paper by Amit Bhaduri (1969) in which he connected the analytical details to older debates in political economy associated with Marx and the original neoclassicals, I wrote 'A child's guide to the double-switching debate' as the first of five working papers. I wrote these, each on different aspects and issues, because I found it was the only way to avoid being overwhelmed by the massive volume of literature to be surveyed. I sent the first paper to about 30 friends ranging from Joan Robinson to Bob Solow and Jim Mirrlees. The favourable feedback emboldened me to keep on and the first draft of the survey was duly sent to Perlman in time for him to have it refereed.

The then economics editor of Cambridge University Press saw the working papers and asked me to write the book of the survey. The book (Harcourt, 1972) would never have been written had I not been invited by Masao Fukuoka to spend three months at Keio University in Tokyo on a Leverhulme Exchange Fellowship. I describe in the Preface (p. ix) how I lived the selfish life of a scholar and wrote the first draft in two months. There were two main themes: the critique of the conceptual basis of price as an index of scarcity in the theory of value and distribution; and the methodological critique emanating from Joan Robinson of the error of using differences to analyse changes, which became her dominant critique of neoclassical economics. I think she went too far in playing down the first theme ('The unimportance of reswitching', 1975) – the conceptual basis of an approach *is* a legitimate target for criticism (see below and Harcourt, 1996) – but was spot on with her insistence on the second critique, one with which Hicks increasingly was to agree and which Kaldor always stressed. His last book was called *Economics without Equilibrium* (1985), his 1984 Raffaele Mattioli Lectures (Kaldor, 1996) are replete with examples of analysis which avoids the error, and one of his first papers (1934) already contained the seeds of doubts. Moreover the best mainstreamers – for example, Ken Arrow, Franklin Fisher, Frank Hahn – were simultaneously developing path-dependent models.

Emergence of inflation, policy proposals, Spain, Canada and *AEP*

In 1972–73 I had another year's study leave. It was spent in Cambridge as a Visiting Fellow of Clare Hall. My 1972 book had been published in May. I gave upwards of 50 seminars on it and my afterthoughts, subsequently published in *Oxford Economic Papers* in 1976.

I wrote a number of commissioned papers on these issues in those years, significantly one relating to the policy implications (see Harcourt, 1975; 1982). I also met up again with Tom Asimakopulos, together with his family, all at Clare Hall. Though we never did get round to writing the book on growth we planned to do, we formed a close friendship which led to a steady exchange of papers for comment from then on until Tom's death in May 1990 (see Harcourt, 1991; 1993a).

In one of the follow-up papers I began to draw out some policy implications of the capital theory controversies and the post-Keynesian approach more generally. When I returned home in 1973 inflation was soon to take off in a major way and the Golden Age of capitalism came to its end. Australia was about to go through an (unintelligent) Thatcherite experiment several years before Thatcher came to power in the UK as the Fraser government from 1975 on cumulatively introduced monetarist policies, combined with an ugly confrontationist approach between government, capital and labour. Eric Russell, Barry Hughes, Philip Bentley and I, in support of Ralph Willis who subsequently became Treasurer in the Keating ALP government but who was then a lone voice crying in the ALP wilderness, started to develop a package deal for ridding the system (slowly) of inflation while trying to maintain high levels of employment and external balance. With Australia's history and tradition of centralised wage fixing through the Arbitration Commission (sadly now abandoned), this involved arguing for, amongst other things, indexation combined with restraint on the rate of increase of money-wages, then an unpopular and indeed dangerous thing to do. I remember one burly communist trade union official berating me for doing so (even though I argued it was intended to protect real wages and employment). One of my dearest friends, Donald Whitehead, changed from calling my proposals the silliest he had ever heard to broadly agreeing with them, sadly just before he died. The ingredients of the package deal we had in mind I described initially in 'The social consequences of inflation' (1974; 1982) and, more fully, with Prue Kerr, in 'The mixed economy' (1979), and in the 1982 John Curtin Memorial Lecture, 'Making socialism in your own country' (see Sardoni, 1992).

In essence it involved 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, the rate of growth and the post-tax distribution of income. Nationalisation of certain key industries including financial intermediaries was put back on the agenda for discussion and I sat on the fence concerning the tariff, that is, leave it as it is and concentrate on export promotion. (The act in my professional life I most regret in retrospect is that I publicly supported, as did most other Australian economists at the time, the cut in tariffs by the Whitlam ALP government in the early 1970s.) I opted for a fixed exchange rate, with the proviso that in an economy like Australia's a change might have to be contemplated from time to time.

I was the economist on the ALP's National Committee of Enquiry in 1978. I wrote the first draft of the discussion paper which was meant to be the broad background to the economic policies of the ALP government when it was returned to power in 1983 – only very indirectly, it is true, but I like to think that the Accord between wage-earners (through the Australian Council of Trade Unions) and the government in its early years owed something to our debates and suggestion in the 1970s.

Two other significant events of the 1970s and 1980s were, first, an invitation from Fritz Machlup to chair a small IEA conference on the microeconomic foundations of macroeconomics (it was held at S'Agaro in April 1975) and, secondly, the start in 1977 of what was soon intended to be a visit every two years to the economics department of the University of Toronto's Scarborough College. The first event was in a sense the culmination of my interests of the past 23 years or more. John Hicks was the initiator and chief intellectual influence of the conference. (As he had 'retired' he asked that a younger person from the Commonwealth with access to secretarial facilities chair the conference.) The resulting volume (Harcourt, 1977b) received mixed, not always fair, reviews and Hicks himself was depressed by the overall outcome of our discussions. Nevertheless, I think that we aired in honest and helpful ways some fundamental issues and that, amongst the papers and, especially, amongst the discussions so ably recorded by Sue Howson and Don Moggridge, there were many helpful clarifications and leads as to what to do next. I found writing the Introduction one of the hardest tasks of my working life and so I was pleased when both Hicks and Paul Madden, my then colleague at Adelaide, said they liked it.

I had met Lorie Tarshis at Stanford in the summer of 1965 and I had come to know Sue Howson and Don Moggridge at Cambridge in the 1960s and 1970s. So I gladly accepted their invitation which they initiated through Lorie (who was then the Chairman of the Division of Social Sciences at Scarborough) to spend a semester there in 1977. Though I disliked intensely commuting 20 miles from downtown Toronto, I loved working with the splendid, mostly young, team which made up the department, and, especially, getting to know Lorie well and also Jon Cohen. Jon became a daily running companion and a close soul mate as well (the two are not unconnected). I had another spell at 'Scarberia' in

1980 but when I returned to Cambridge in 1982 I decided, regretfully, that it would be too strenuous to try to keep the visits going.

I wrote two papers during these visits which were not published at the time. One concerned an account of my growing horror at the political and social implications of the theoretical work then being done under the rubric of monetarism and new classical macroeconomics. I outlined this in an angry and passionate manner and called for all branches of Keynesian economics, no matter how they differed on theoretical issues, to unite on policy and in opposition to the awful policies which were being rationalised by these other theoretical developments. The paper was thought to be in poor taste, unprofessional and unfair. Looking back now I think it was 'spot on'. When Paul Davidson read of its existence in Harcourt (1995b), he asked to see a copy and then most generously offered to publish it with an addendum on my current assessment of the issues in the *Journal of Post Keynesian Economics* (see Harcourt, 1996–97). In the addendum I said I stood by the general thrust of the paper and I commended Hahn and Solow in Hahn–Solow (1995) for their incisive internal critique of new classical macroeconomics.

The other paper was a joint one with Jon Cohen, predicting what we thought would happen to private and public education in the USA and other capitalist countries as a result of trends then emerging – essentially, growing public deprivation, demoralisation and squalor, while the private sectors would flourish, attracting the best teachers and increasing proportions of students, mostly from income strata where parents, on the whole, could afford to pay. I think we were right here too but at the time (1980) and now, we could not get the paper published. Jon and I also circulated a 'round robin' Keynesian manifesto on policy which a few brave souls on the Canadian economics scene were prepared to sign with us.

Finally, I want to mention another aspect of my years at Adelaide, my joint-editorship with a number of colleagues, the late Hugh Hudson (who was the founding editor), Keith Hancock, John Hatch, Merv Lewis and Bob Wallace, of *Australian Economic Papers (AEP)* from the 1960s to the 1980s. I tried to make *AEP* in those years an outlet for *all* approaches to economics, provided that the work itself was sound within its own approach. Certainly a number of economists outside the mainstream had important papers published in *AEP*. I single out especially Tom Asimakopulos's first paper in his Robinsonian mode (1969), Victoria Chick's reassessment of the nature of the Keynesian revolution (1978) and Ferdinando Targetti's and Bogulslawa Kinda-Hass's translation into English of, and comment on Kalecki's review article in Polish of *The General Theory* in 1936 (1982). I regard Kalecki's article as the best and most incisive article we ever published. It shows conclusively that Kalecki had independently thought out the principal propositions of *The General Theory* and that in addition to producing a theory of employment, he also produced a macroeconomic theory of distribution. The latter takes in pure competition as a special case. He showed that market structures made no essential difference to the new theory of effective demand.

Piero Sraffa

The editor of the series read the first draft of the essay and asked that I 'add a little more about Sraffa and perhaps his reaction to [my survey article and book on the Cambridge controversies in capital theory]' (25 September 1997). I have already mentioned how significant Sraffa's writings were for me as a result of my undergraduate years at Melbourne University. As well as the Ricardo volumes, we read his famous 1926 *Economic Journal* article, 'The laws of returns under competitive conditions'. It was a major article on the honours reading list for the lectures on imperfect and monopolistic competition (indeed, it was the starting point) and it was in that context that I first tried to understand its arguments. We now know that Sraffa himself had by then (the early 1930s) repudiated this context, or, at least, considerably played it down (see, for example, Harcourt, 1990; Sardoni, 1992, pp. 301–5).

The Piero Sraffa I first met in October 1955 and who looked after the research students at Cambridge, was a different person from the Sraffa I came to know in the 1960s, 1970s and 1980s. In the 1950s, though a kindly person, he was rather remote and withdrawn (he was still recovering from a serious accident in which he fractured his skull). In the 1960s he was much more approachable and easy to get on with, for by then he was both a relieved and fulfilled person: the Ricardo volumes were finished (except for the Index), *Production of Commodities*... had been published and astute people such as Krishna Bharadwaj and Maurice Dobb understood what he was on about. He still had his fiery temper and acute critical mind, as I illustrate below.

Early in my sabbatical year of 1963–64 I met Vincent Massaro who was spending a year in Cambridge on a NATO Fellowship to study the writings of Joan Robinson and Sraffa. Vince was a first-generation American of Sicilian origin, a devout Catholic, a devoted admirer of Dorothy Day and the *Catholic Worker* – and also (something of) an anarchist. He had done a PhD dissertation at Notre Dame on the doctrine of increasing misery in Marx (so naturally he was awarded a NATO Fellowship to come to Cambridge!). I had had a quick look at *Production of Commodities...* in Adelaide when it was first published, because I had been told that Sraffa had been working on a book on capital theory for many years and that this was it. A colleague at Adelaide, John Dillon, the distinguished agricultural economist, also had a quick look and dismissed it as 'old hat input-output'. I found the book incomprehensible – a completely unfamiliar terrain – so I put it away for study leave. Vince and I agreed to read it together from cover to cover and not go onto the next sentence until we felt we had understood the last one. This procedure, combined with Vince (mainly) talking to Piero (in Italian) and a detour to write our *Economic Journal* note on sub-systems (Harcourt and Massaro, 1964a), allowed us to write a review article of the book for the *Economic Record* (Harcourt and Massaro, 1964b; Harcourt, 1972, appendix to ch. 4), getting each sentence approved by Sraffa in the process.

In the sections on joint production and fixed assets I found a slip in Sraffa's example on pp. 68–69 in which he showed how to calculate the direct and indirect labour associated with the production of a tractor. I modified the example in order to overcome the slip (see Harcourt, 1972, 190–92), and discussed it all with Sraffa. We reached agreement (I thought) that there *was* a slip and that I had avoided it in my modification of his example; but when we had our last meeting to discuss the completed manuscript before we sent it off, Sraffa took exception to my argument. I rashly said: 'But, Piero, last time we discussed this, you agreed I was right.' I can still see him arching his splendid eyebrows and hear his strident tones as he shouted: 'I am not the Pope, I am not infallible.' I had only witnessed this intensity of temper once before, when Vince and I were summoned to his room in Trinity to discuss the draft of our sub-systems note. In it we had attributed to him the view that the means of production currently being used came from the immediately preceding year and that those parts of the current gross product which are *equal* to the means of production will be used as means of production in the following year. *That* time I was fortified by a considerable amount of (Piero's) whiskey, this time I was not!

Piero took me several times as his guest to Feasts in Trinity and I took him on his last ever visit to Rome, in 1973. By then his short-term memory was playing him tricks so that he had lost his tickets and passport on his previous visit. I was the only one of his friends free at the time to go with him (I was then on leave from Adelaide at Clare Hall). We went for a week; we stayed at the Hotel Hassler at the top of the Spanish steps where he was always an honoured guest. (If I had not been his guest I would have ended up at the bottom of the steps had I tried to enter alone.) I had an excellent week seeing the sights of Rome during the day and dining with Piero at various fine restaurants in the evening when all doors opened to the famous, much revered 'Signor Sraffa' (and sidekick). His mission in Rome was to get a protégé elected to the Italian Lincei Academy; he would return each evening, lie on the floor clutching his head and saying 'I am a failure, I am in despair.' He would then get up and suggest we go out for dinner, despair soon departing. We drank champagne on the return flight to the UK – at 10 o'clock in the morning.

I was very fond of Piero and much in awe of his intellect and critical abilities. But apart from the discussions I mentioned above and the story of how reswitching came to Cambridge,[9] I did not have many serious discussions with him as, over the years, I tried to get his agenda and contributions into focus. In this endeavour Krishna Bharadwaj's writings (and our many discussions) were of enormous importance for me, especially her superb 1976 Dutt Lectures (Bharadwaj, 1978), which put Sraffa's critique into an appropriate historical perspective. Sraffa was a great admirer of Marx as well as of 'his' David. He saw his own role as making clear and precise some obscure, or incoherent, or unfinished parts of Marx's grand schema while at the same time setting out what he saw as the incoherence and logical flaws in the conceptual foundations and arguments of the neoclassical theory of value and distribution. I have discussed these views in detail in a number of papers (see Harcourt, 1978, 1981, 1982, 1983, 1984, 1986, 1990).

As to my articles and book on the capital theory controversies and Sraffa's reactions to them, apart from missing my moment of glory because neither Piero nor I could 'do' matrix algebra, I have only one other episode to relate. On New Year's Day 1973, I received from Joe Stiglitz the first draft of his critical review article of my 1972 book which he wrote for the *Journal of Political Economy* (see Stiglitz, 1974). I wrote Joe many pages of comments. This led him to clean up his act and remove part of a sentence which I prized: 'a few of the jokes are probably almost as funny as Harcourt seems to think they are.' (But I must also say that Joe very decently removed sections I found personally offensive.) I showed Piero the article and my reply to Joe. He made two sets of comments. Joe had written: 'There is a well-known propensity of individuals to dislike what they don't or can't understand'. Piero commented: ' "Don't or can't understand"? don't is wilful, can't is stupid?'! Secondly, Joe had drawn on his referee's report on my 1969 survey article for his article and at the end of the first draft he must have forgotten he was reviewing my 1972 book. For he wrote: 'It is a shame…that this major review article…on a subject that has been the source of so much discussion in recent years should have served to protract the debate rather than to clarify the issues, to shed more heat than light.' He blamed not me but the editor (Mark Perlman). Piero commented: 'p. 14…"blames the editor". Does he want a censor?'

Let me leave it at that!

Return to Cambridge in 1982

Mark Perlman wrote a Preface to a selection of my essays (Harcourt, 1995a), in which he classified them into four groups:

1. works analysing contemporary economic theoretical problems;
2. works synthesising states of debates in economic theory;
3. works having a distinctly biographical flavour and pertaining to various contemporary economists; and
4. works pertaining to economic and allied social policies.

The beginning of my interest in writing intellectual biographies arose from three sources: first, Angus Wilson's request that I send him details on Joan Robinson's contributions for the oration he was preparing for the award of an Honorary Degree to her by the University of East Anglia in the mid 1970s; secondly, the tragic death of Eric Russell in February 1977 while I was in Canada (I have described elsewhere – see Sardoni, 1992, p. 8 – how I wrote two papers on Eric as part of a shared attempt to cope with our grief at this terrible loss); and, thirdly, Paul Davidson's suggestion that I oversee a number of occasional portraits of the founding mothers and fathers of post-Keynesianism for the *Journal of Post Keynesian Economics*. Since then I have produced a steady stream of intellectual biographies, some of which arose from experiments in oral history, for example, Tarshis, Shackle, Boulding, Goodwin (see Harcourt, 1993a), Stone (see Harcourt, 1995a), others from memories of personal contacts, together with reading or rereading their writings, for example, Joan Robinson, Kaldor, Tom Asimakopulos (see Harcourt, 1993a), Krishna Bharadwaj, Austin Robinson and Josef Steindl (see Harcourt 1995a). I have enjoyed doing this for I have always been interested in what makes people tick and also perhaps too much prone to search for heroines and heroes.

When I returned to Cambridge in 1982 to a teaching post in the Faculty and a Fellowship at Jesus, my major task was to write the intellectual history of Joan Robinson and her circle – Austin Robinson, Richard Kahn, Piero Sraffa, Nicky Kaldor, Michal Kalecki, Richard Goodwin, Luigi Pasinetti. I wanted to see whether there was a coherence to the tradition this group of economists both inherited from the classical economics, Marx and Keynes and passed on. Sadly, virtually all the major figures have now died, starting with Joan and Piero in 1983, ending – to date – with Austin in June 1993 and James Meade in December 1995. Since I nearly joined them several times myself between 1992 and 1994 I am rather lagging in getting it all together in *book form*. But there are now well over 30 separate essays which provide the essential background to the project and so I do not entirely despair of getting it finished before I call it a day. When I retire I shall be able to spend more time in King's Modern Archives where most of the papers of these economists are housed. While I shall draw mostly on what is in the public domain – I am not a fan of the modern 'tell it all' biography – I do think it necessary to provide evidence on what was going on behind the scenes when their articles and books were being written.[10] [With Prue Kerr, I published an intellectual biography of Joan Robinson in 2009 (See Harcourt and Kerr (2009). The number of background essays by Prue and I was by then over 100.).]

I must confess to having been sidetracked on occasions and in different directions. In the last six years I have returned to policy on a large canvas, following the invitation to give the second Donald Horne Address in Melbourne in February 1992, on 'Markets, madness and a middle way' (1992; 1995a). It spawned further papers, one on macroeconomic policy for Australia in the 1990s (1993c; 1995a), another on 'The Harcourt Plan to "save" the world' (1993b). This subsequently become 'A "modest proposal" for taming the speculators and putting the world on course to prosperity' (1994b; 1995a). In 1997 I published the paper on economic policy, accumulation and productivity to which I referred above and gave the Seventh Colin Clark Memorial Lecture on 'Economic theory and economic policy: two views' (Harcourt, 1997d). The latter drew on a review (Harcourt, 1997b) I wrote of Joe Stiglitz's 1990 Wicksell Lectures, *Whither Socialism?* (Stiglitz, 1994, 1996), and a review article (Harcourt, 1997c) of Kaldor's 1984 Mattioli Lectures (Kaldor, 1996). Stiglitz's lectures contain one of the best *internal* critiques I have ever read of mainstream economics (taken by him and Kaldor to be represented by the Arrow–Debreu model of general economic equilibrium (AD)). It is based on Stiglitz's work on the economics of information and missing markets; it highlights the inability of AD to take in the essential fact of life, one which Smith and Marx recognised, that capitalism is the greatest ever generator of change through endogenous technical advances.

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 (as Kaldor and Stiglitz do, too) 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 general equilibrium framework. 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 in his well-known article on the case for flexible exchange rates (1953).

The essays on macroeconomic policy relate principally to the problems of small open economies. It allowed me to ride some hobby horses, for example, 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* effective productivity is not only equitable, it is also efficient. It encourages investment in profitable, productivity-enhancing industries and hastens the decline of industries whose time has not only come but gone.

In the paper on a 'modest proposal' 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. 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. Next step the universe, of course.

If it were to be asked had I ever considered being an official policy advisor, the answer is 'yes' – and I decided not to be. In 1974 I was approached by Jim Cairns, who taught me at Melbourne University and who was then Deputy Prime Minister of Australia and Federal Treasurer in the Whitlam government, to see whether I would consider being either Governor of the Reserve Bank of Australia or permanent head of one section of an intended revamp of the Commonwealth Treasury. To the first request, I immediately said 'no', adding 'You know me, Jim, I'm a real man, not a money man.' I promised to think about the second request but on reflection and, especially, after talking to Eric Russell and Peter Karmel, I said 'no' to it, too. They pointed out (and I agreed) that I had neither the appropriate temperament nor the necessary guile to withstand the inevitable machinations of very bright civil servants who would undoubtedly be resentful at having a rank outsider brought in over their heads.

In any event, it is clear in retrospect that Jim was already on the skids; he was soon to be sacked, first, from the post of Treasurer and then from the Deputy Prime Ministership. Another chap and I spent one morning in Canberra outlining to him a Left-Keynesian budget for him to take to Cabinet. He said he had to leave us at lunch time because Whitlam wanted to see him. When he returned after lunch, we asked him what Whitlam had wanted; he replied: 'The bastard's sacked me!' – so much for the chances of our Left-Keynesian proposals having any influence!

The other major 'sidetrack' was collaborating with Peter Riach on *A 'Second Edition' of The General Theory* (Harcourt and Riach, 1997). Peter had a bright idea based on the well-known fact that composers have often died before they could finish certain works which others then took up and completed. Keynes had thought of writing at least 'footnotes' to *The General Theory* soon after it was published, but his severe heart attack in 1937 and then the Second World War and his death in early 1946 prevented him from doing so. Peter's project, which he asked me to join, was to ask a number of Keynes scholars, ranging from Golden Oldies to the up-and-coming, to each write an essay of around 6000 words on, first, what they thought, based on whatever evidence there was in the *Collected Writings* and elsewhere, Keynes might have written and, secondly, what they have done and why on particular aspects of *The General Theory* in the post-war years. It is a most opportune time to have done this for Keynes is surely rising again from the dead. The book was published in two volumes by Routledge in January 1997, just over 60 years on from the publication of *The General Theory* itself. The first volume tracks (more or less) the chapters of the original *General Theory*. (Some of the contributors write, in the first sections of their essays, 'as J. M. Keynes'.) The second volume contains an overview (by James Tobin), essays on extensions and new developments since *The General Theory* was first published, and essays on predecessors and successors (see Harcourt, 1997e). Riach and I wrote a long introduction setting the chapters in context, and between us and the contributors we accumulated a 50-page bibliography.[11]

Doing Economics

Steve Medema and Warren Samuels asked me to contribute to their volume, *How do Economists do Economics?* (1996). So how do I and how do I think we ought to do economics? With Eric Russell, Nicky Kaldor, Joan Robinson, Piero Sraffa, Richard Kahn and Michal Kalecki and, of course, Keynes as mentors, I am a 'horses for courses' person – how and what you do depends upon what the purpose is. If doctrinal debate is the issue – the robustness of a fundamental intuition or insight in a particular approach, say price as an index of scarcity in neoclassical economics, surplus labour and value as the origin of profits in the capitalist mode of production in Marxian economics – it is proper to operate at a high level of abstraction, to use simple, *very* unrealistic, models (which are appropriately closed) with which to capture the essence of the problem and which exclude all other 'matters of the real world' as irrelevant for the purpose in hand.

My favourite example is the debate which Ian Steedman (1975, 1976, 1977) initiated on whether the existence of joint production destroyed the general validity of the Fundamental Marxist Theorem (FMT) (Morishima, 1973) – the necessary and sufficient condition for positive profits in the sphere of distribution and exchange is positive surplus labour (and value) in the sphere of production. For *that* purpose it was legitimate to use a Golden Rule, Golden Age model, and ignore real life puzzles such as the realisation problem. The argument then turned on the correct formal specification of what, initially, were ideas expressed in literary form. Steedman inappropriately used equalities in order to estimate necessary and surplus labour (in his examples the latter turned out to be negative) whereas inequalities (linear programming) were the appropriate formal tools *in this context* to capture the basic idea that the monopoly of capitalists *as a class* of the means of production and of access to finance enabled them to make the wage-earners *as a class* work longer than was necessary to produce their wage goods *alone* with the existing techniques of production and accumulated capital goods. Joint production techniques could be handled as an unessential complication (though an inescapable feature of the real world) which should not destroy this essential insight. If it *appeared* that they did, it was the specification and not the insight which must give way, as indeed happened (see Morishima, 1976).

Or, to take another issue with which I have some familiarity: is the demand curve for 'capital' as a whole well-behaved? Stationary-state comparisons of long-period competitive equilibrium positions are the simplest and most appropriate method of analysis to test for the robustness in heterogeneous capital good models of the results of simple one all-purpose commodity models in which the marginal productivity theory of distribution and the related neoclassical 'parables' rigorously 'go through' (see, for example, Harcourt, 1976).

To descend from these dizzy theoretical heights to a more practical plane – the testing of inferences of theory against real world data. One approach is to follow the frankness and honesty of Bob Solow in his most famous empirical work on the aggregate production function and technical progress. He wrote in reply to Anwar Shaikh's critique (1974) of his procedure and findings:

> It merely shows how one goes about interpreting given time series if one starts by assuming that they were generated from a production function and that the competitive marginal-product relations apply. (Solow, 1974, p. 121)

The limitations are set out, the meaning of the findings is coherent, the usefulness of them then turns on whether or not it is believed that the underlying simple theoretical model captures the essence of the processes which spawned the statistical observations of the times series used in the first place. Solow and (especially) his surrogates, then and now, presumably believe that they do. Their critics do not – and some of them have provided either alternative approaches or different theoretical structures to explain what the data show.

As another example let me take the empirical work I did in the 1950s and 1960s on the effects of using historical-cost accounting procedures in periods of inflation. The inferences to be looked for in the data were rather broad: that if inflation continued, and if the taxation authorities and management insisted on basing taxes and dividends respectively on accounting profits rather than on 'economic' profits – those adjusted for stock appreciation and capital consumption at replacement cost – and if management insisted on setting prices by 'marking up' historical costs while not widening their margins, we ought to see over time a decline in 'economic' rates of return coupled with a deterioration on the liabilities side of balance sheets. The latter would result from financing not expansions, but firms as 'going concerns', trying to continue to do tomorrow what they had done today. Whereas, if replacement cost procedures were used, such deteriorations should not be observed – for these reasons. Plain competition, or bad management, or changes of fashion could also lead to similar happenings. But, anyway, if the first set of tendencies were the most powerful at work in the specific concrete situations examined, we knew what we had to look for and, hopefully, find. (In my case inflation proved disobliging and, relatively, went away for 20 years.)

In this example I did not use any sophisticated econometric techniques – the essential aim was to understand the sources and construction of the data set, what processes it was hypothesised lay behind it and spawned the observations, adjust it from its raw form to get it in the appropriate form to allow estimates of the relevant theoretical variables to be made, and then analyse what the results showed. Such a procedure belongs to a well-established Australian tradition (not exclusively, of course, in Cambridge, for example, it is known as the Reddaway method and I am sure that many other economists follow it too). It involves thorough understanding of the sources and limitations of the data, especially of how 'far off' the actual observations are from their theoretical counterparts, the use of simple tables, ratios, graphs, to get a 'feel' for what the orders of magnitude are, never going further than the data themselves warrant while being ready to take advice from the experts on what traps to look for and what techniques may be used either to avoid them or to tease out answers which otherwise would remain hidden.

I want now to consider cases intermediate between high theory and the rough and tumble of applied work, those concerning theory and specific issues. There are at least two basic approaches. One is axiomatic, for example, as Frank Hahn often says, let us see how far the assumption that the world is inhabited by 'greedy people' will take us. The other starts by observing behaviour, institutions, 'stylized facts' and then constructs simple models incorporating the essence of the observations in order to try to explain the original observations, and so on. Arrow, Debreu and Hahn are outstanding proponents of the first approach, Kaldor, Kalecki, Joan Robinson – also Keynes, Marx and Adam Smith – of the second. A vital question is whether there are large differences between the answers to the same questions according to which approach is taken.

The work I did in the 1960s on the choice of techniques illustrates this. I compared the investment–labour ratio which would be predicted by the axiomatic approach, using some versions of the set of DCF procedures, with those which would be predicted by starting from 'real world' investment-decision rules – the pay-off period criterion, the accounting rate of profit rule (and, in some instances, the various recoupment period procedures of socialist managers in the 1960s). As I mentioned above, I was able to show that for the orders of magnitude likely to be met in the real world, the pay-off period criterion always resulted in a more investment-intensive, less labour-intensive technique being chosen than did any of the other investment-decision rules. I also showed that under the conditions assumed the ratios associated with the net present value rule (NPV) were intermediate between those associated with the pay-off period criterion and the internal rate of return rule (IR) respectively (except in long-period competitive equilibrium when the NPV rule and IR rule not surprisingly resulted in the same technique being chosen). I then allied this set of findings with an analysis of the impact of a variety of investment-incentive schemes which were combined with the various investment-decision rules, in order to see how robust the main results were and also whether what was predicted to happen was in accord with what the policy makers had in mind when they introduced the schemes.

Finally, may I say a little about my other two major research preoccupations – surveys and intellectual biographies? A useful survey should put a structure on an area in the literature. It should draw out the main thrust of the arguments, identify the major articles or books, and show how the rest cluster around them. It should suggest the areas and issues of disagreement, try to reconcile what is reconcilable, point out what is not and why, and suggest where, if anywhere, we should go from here. Doing all this gives perspective and senses of relevance and balance. If in the process it also enlightens and even amuses, why, these are surely acceptable pluses.

What of intellectual biography? As I said, I believe in heroines and heroes and I like to know what makes them tick. I became an economist because I hated injustice, unemployment and poverty. Most of my heroines and heroes became economists for the same reasons and they devoted their lives to trying to do something about understanding how these ills arose and how to get rid of them. Some – not all – understood the *Realpolitik* of policy advice, as I have tried to myself. Intellectual biography allows us to begin to see the links between the historical settings of the persons concerned, their class, their racial, educational, religious and philosophical backgrounds, and the issues of the day on which they have worked. By analysing the intertwinings of all these aspects, we get a better understanding of the writings and contributions of these economists, of limitations as well as achievements, of the particular forms which their analyses take, and hopefully, we are inspired to follow on from where they left off. If not, it is at least to be hoped that we have had a good read on the way.

I have written elsewhere (see Harcourt, 1995a), about the vexed question of the use of mathematics in economics and why I think mathematics is a good servant but a bad master. My own most ambitious theoretical paper was the two-sector model of distribution and employment in the short period (1965b). I wanted to extend the analysis into a process in historical time, seeing how the model economy moved from period to period but I did not have the necessary mathematics to allow me to do this. It is partly because of this paper that I admire Kalecki's contributions so much, for he had the technical expertise and the genius to do superbly what I wanted to do but could not. (My paper, of course, owes much to his contributions.) Dick Goodwin's writings spring to mind also at this juncture. So the most use I ever made of mathematics was in my papers on the choice of technique. Even then I eventually reduced the argument to a simple diagram which exploited the properties of the two most useful (and used) constructions of our trade – the 45° line and the graph of a concave-to-the origin quadratic (see Harcourt, 1972, p. 64).

Similarly, I regard econometrics as a set of valuable tools and techniques which enable us to squeeze information from data in technical forms which people generally understand. But *always* the guiding principle must be the economics of the problem and its importance and relevance, not what economic problem can we find to which to fit any fancy technique that we have come across. It is most important to get the conceptual aspects of a problem clear, together, sometimes, with conjectures as to outcomes, *before* starting any formal or statistical analysis. This I have always tried to do.

What of the future?

Arnold Heertje also told me, after he read the first draft, that he 'would like to have [my] view on the future development of economics' (25.9.97) – a tall order indeed for someone who has always stressed the role of uncertainty in economic theory and life. I hope it is clear from what I wrote in the previous section *how* I would like to see economics develop; but I cannot pretend that it will necessarily do so. Undergraduates and graduates the world over are no longer flocking to economics courses; rather they are leaving in droves for courses in business studies, embryonic hired prize fighters all! Those that do come are increasingly mathematically literate but tend to have narrow interests in general, so that history, politics, sociology, philosophy, psychology and knowledge of institutions are at a heavy discount. No doubt the trade will continue for a while yet along its present very technical lines and highly competent if uncritical normal scientists in a Kuhnian sense will continue to be turned out and subsequently fill the 'brownie point' journals with their writings.

I must not paint too gloomy or jaundiced a picture. Some of the leaders in the discipline and some of the elders of the tribe, those who inspire (or have inspired) by example, are both imaginative within our discipline and are happy to embrace the findings and even the approaches of other disciplines. But, as ever, mavericks tend to get short shrift; just as John Hicks, Kaldor and Joan Robinson were marginalised in their generation as soon as they stepped out of line, so, too, are their modern equivalents. This means that most people tend to be cautious, are prepared to work only within defined guidelines, while making sure that undergraduates and graduate courses are similarly designed and constrained.

I think this is a great pity, indeed shameful. Of course, as a liberal educator I accept that students have a right to know and to be taught what is the best and latest in mainstream theory and practice. But as an economist I also care about comparative advantage and the potential value of sensible product differentiation. And, again as a liberal, I believe in creating an environment in which open minds and quirky thoughts may flourish. Alas, little of this is possible in the current climate in economics the world over.

Au revoir, *not* adieu

Are my Christian democratic socialist flames still burning as brightly now as when first lit in the early 1950s? The answer is 'yes'. While the going was tough in the 1970s and late 1980s, recent events, for example, the publication of *Reclaiming the Ground* (Bryant, 1993), the election of first John Smith and then Tony Blair as leader of the British Labour Party, the accompanying return to basics (in a non-Majorite manner) cheered me up no end, as have the elections (and re-elections) in Australia of ALP governments, for all their faults and limitations. (Most annoyingly, just as the UK has returned a Labour government (1 May 1997), Australia is in its second year of an extremely unpleasant, narrow, ruthless, little-minded conservative government, laughingly called a coalition of the Liberal and National Parties. Alas, near the end of their first year in office (January 1998), Blair's 'New Labour' government seems intent on making many of the mistakes that the Whitlam, Hawke and Keating Labor governments made in Australia. Do we never learn?) Of course, the agendas have changed; the proposals are now more modest, more basic – jobs, homes, schools, hospitals, trains, the environment (!) – than in those heady days immediately after the Second World War. But they still make up a worthwhile agenda for the creation of just and equitable societies, for middle ways which are tailor made for each country's history, sociological structure and inherited institutions. And they are likely to be supported as more and more citizens recoil from the excesses and horrors of both command and market, free-for-all, economies. In the despair of her last years Joan Robinson pleaded with her favourite pupil at Williams: 'Whatever you do, promise me you won't go into economics' (see Turner, 1989, p. 206). I would rather say to my pupils,[12] 'Come, join me, there is a job to do and a world to win.'

Notes

† I am indebted to Nick Brown and to Arnold Heertje for their comments on a draft of the chapter.

1. In writing this chapter I have drawn on a number of related essays, see Harcourt (1994a, 1995b, 1996) and Sardoni (1992).
2. I also remember standing under the shower, screaming at my poor father that since Wicksteed had shown that there was no such thing as a supply curve, that only demand determines prices, the P. W. S. Andrews-type cost-plus pricing which he said was followed in his trade just could not be!
3. Another carrot-stick gem I tell students is that misdirected effort, no matter how great, earns no marks.
4. Prue Kerr and I recently wrote the entry on Marx (Harcourt and Kerr, 1996) – all managers ever want and need to know – for Malcolm Warner's *Encyclopedia* for managers (1996).
5. For an independent assessment of the dissertation, see Hatch and Petridis (1997, p. 3).
6. The class of 1955–58 was, in retrospect, a good vintage – as well as Sen, it included Tom Asimakopulos, Charles Feinstein, Pierangelo Garegnani, Luigi Pasinetti, John Whitaker and a whole host of bright Australians – Allan Barton, Keith Frearson, John Harper, Hugh Hudson, Duncan Ironmonger, John McCarty – who were to make their mark in academic and government life in Australia.
7. I also learnt from him how to teach undergraduates and to be a proper university colleague and citizen generally – he was an outstanding role model. See Sardoni (1992, ch. 18) and Harcourt (1977a, 1993a) for accounts of Eric's life and contributions.
8. Initially I thought that the orderings were a completely general result but I subsequently found that I had a wrong sign in one of my differentiations. Showing *why* the result was not general led to some interesting *economic* analysis in itself.
9. In 1965 Levhari (who was Samuelson's pupil) published a paper in the *Quarterly Journal of Economics* in which he claimed to have shown that while reswitching and capital-reversing could occur in an industry, they could not occur in the economy as a whole. I was the first person in Cambridge to read Levhari's article. I told Piero about it, that some chap in the *Quarterly Journal of Economics* said his (Piero's) result was wrong for the economy. PS: 'He's wrong – and you show it.' G. C. H.: 'I can't do matrix algebra.' PS: 'Neither can I' – so he asked Luigi Pasinetti who promptly did so in a paper presented at the First World Congress of the Econometric Society in Rome in 1965 and subsequently published in the 1966 symposium in the *Quarterly Journal of Economics*.
10. To work regularly as one of the editors of the *Cambridge Journal of Economics* has been extraordinarily helpful to me for my task (a glance at the ideals and rubric of the journal will show why), as well as a most pleasant and satisfying task in itself. I look forward immensely to our regular Friday working lunches where, fortified by the excellent bread and cheese, fruit and Frank Wilkinson's no-nonsense pot of tea, we get through a great amount of cooperative work in a stimulating, friendly, often argumentative but basically supportive environment.
11. With Paul Dalziel, I have tried to mount a modest counterattack to the increasingly dominant view that in the world economy as a whole, saving

determines investment, as if Keynes and James Meade and his 'relation' never existed (see Dalziel and Harcourt, 1997).

12. As I have mentioned my students only in passing, I want to stress that I regard teaching undergraduates and supervising research students (since I returned to Cambridge in 1982 I have always had 8–11 PhD students working on a wide range of topics) as the most responsible and rewarding tasks university teachers are privileged to have. Nor have I elaborated on my sporting passions; so let me say for the record, that I played Australian Rules Football until I was 47, that I still play cricket (and make runs and take wickets) and that I go for a long bike ride most days because the combination of my recent brushes with death and acute Achilles tendon problems have, alas, ended my nearly 50 years' habit of a daily run (*not* jog).

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