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“Heinz” Harcourt’s collaborations: Over 57 varieties

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DRAFT

**“Heinz” Harcourt’s collaborations: Over 57 varieties**

**Introduction**

Until Michael and Lal asked me to contribute a chapter to their volume on collaboration I had not realised what a collaborator I was, nor that I was one of so many varieties. Their letter sent me to my CV which led me in turn to classify my joint efforts into categories: of people collaborated and of what forms the collaboration took. The latter includes books co-authored or co-edited; co-authored articles and review articles; notes; reviews; edited volumes of selected essays; and chapters in volumes. If I ignore items accepted but not yet published, I have collaborated 88 times with 103 collaborators. (To add perspective, I have published 29 books and over 360 articles, review articles, notes, chapters in edited volumes and reviews.) In the total are 17 books, two co-authored, 15 co-edited, including three editors of volumes of selected essays, with 22 collaborators; 14 articles involving 17 collaborators; four review articles with four collaborators; 12 notes with 15 collaborators; 33 chapters in books with 37 collaborators; and two reviews with two collaborators.

The person I have most collaborated with is Prue Kerr – thirteen times, consisting of three books and ten chapters in books. Next is Peter Kriesler – one

book, three articles and five chapters in books. Claudio Sardonì and Avi Cohen are equal third, both four times. With Claudio, I have one book and three chapters in books; with Avi, I have one article, one note, and two chapters in books<sup>1</sup>.

The rest of the chapter contains reflections on why and what I have done with others. I start by saying that my personal and working life has been greatly enriched by these joint happenings, whereby I have both made new friends and deepened already established friendships<sup>2</sup>.

My collaborators may be classified into two broad categories: first, present or past graduate students; secondly, internal or external colleagues. Three of my principal colleagues – Prue Kerr, Peter Kriesler and Claudio Sardonì – were my students: Prue as an undergraduate at The Flinders University of South Australia, a Master’s student at Adelaide University, and a Ph.D student at Cambridge University. I examined Peter’s Master’s Degree for Sydney University<sup>3</sup>, supervised his doctoral dissertation at Cambridge and we are now colleagues at the School of Economics of the University of New South Wales (UNSW Australia) in Sydney. Claudio did his Ph.D with me at Adelaide<sup>4</sup>.

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<sup>1</sup> Peter and Avi are coming up fast on the rails if accepted but not yet published items are included.

<sup>2</sup> I have not included in the text but I would like to mention that I prize being asked (or offering) to write Forewords to books and commenting on drafts of articles and books. I have no detailed idea of how often I have done this, but I am sure it runs into hundreds.

<sup>3</sup> It subsequently became the basis of his well-received book with Cambridge University Press, *Kalecki’s Microanalysis* (1987), to which I wrote a Foreword.

<sup>4</sup> It was the basis of his fine book, *Marx and Keynes on Economic Recession*, published in 1987. He published a second edition including Kalecki, in 2011. It is entitled *Unemployment, Recession and Effective Demand*. I wrote Forewords to both books.

Some years later in Cambridge he met and subsequently married our oldest child, Wendy.

### **Early collaborations**

My first ever published article was jointly authored with Duncan Ironmonger. The title was “Pilot survey of personal savings”. It was published in the *Economic Record* in May 1956, and was a summary of the contents of my Master’s Degree at Melbourne University. By then I was a doctoral student in Cambridge so I did not write directly one word of the article. Duncan and I had been undergraduates together at Melbourne and, when I worked on my Masters’s project, he was the expert advisor from the Australian Bureau of Census and Statistics on the stratified sampling method I used to gather data for the project, so he was the ideal co-author.

After being a research student at Cambridge (1955-58), I worked principally at Adelaide (1958-63; 1967-82), Cambridge (1963-66; 1972-73; 1980; 1982-2010) and the University of Toronto (1977; 1980). Since August 2010 I have been a Visiting Professorial Fellow at the University of New South Wales. Overwhelmingly all of my collaborations have been with people at one or other of these four centres.

## First Adelaide Years

In my first six years at Adelaide, 1958-63, before I went on study leave to Cambridge, my first collaboration was with Allan Barton, my great friend from Melbourne and Cambridge days, who was then at Adelaide. Allan was a whiz kid on many things, including detailed accounting procedures and taxation measures. I had a chapter on investment allowances in my Cambridge dissertation and we adapted the arguments there to propose for Australian primary producers, investment allowances in the place of accelerated depreciation allowances and/or cash grants, see Barton and Harcourt (1959). I suspect this policy proposal fell on deaf ears even though Australian primary producers were well represented in the Federal and State Parliaments through the then Country Party.

With Jim Bennett, who was a lecturer in the Commerce Department at Adelaide (members of the Economics and Commerce Departments worked very closely together<sup>5</sup>). Jim had spent some time at MIT (the other Cambridge) and we combined to write a short paper, “Taxation and business surplus”, published in the *Economic Record* in August 1960. I now regard it as the silliest paper I have ever published because it tries to combine two irreconcilable approaches to economic analysis: not Jim’s fault, I hasten to add, but mine. It did have the

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<sup>5</sup> Australian economics as well as commerce undergraduates have always had to take accounting courses, most sensibly so, since it may be argued that a necessary if not sufficient condition for the rise of capitalism was double-entry book keeping, a conjecture now conclusively and delightful established in Jane Gleeson White’s prize winning volume, *Double Entry* (2011). The script of an interview I did with Jane, which was a hit on the ABC’s “Big Ideas” program, is published as Gleeson White and Harcourt (2012).

amusing consequence that if only long-period normal profits were being received, companies using our measure of surplus for taxation purposes would pay no tax, as Peter Swan, an unreconstructed Chicago economist and friend of mine at UNSW, once had great glee in pointing out to me.

I first met Donald Whitehead in the 1950s when he was at Nuffield College, Oxford – he was the Oxford Secretary of the London Oxford, Cambridge research students seminar which met once a term in one of these three places. I was the Cambridge Secretary. Donald was a star, the life and soul of the graduate students and Faculty at Nuffield. In the 1950s I recommended to Peter Karmel, then Professor of Economics at Adelaide, that Donald be considered for a Lectureship in Economic Development at Adelaide, to which he was duly appointed. Oxford life for Donald had not been conducive to publication as opposed to teaching and in-depth discussions, so to get Donald started on what subsequently became an impressive list of publications before his far too early death at the age of 49 in 1980, we wrote together the chapter on “The wool textile industry” in Alex Hunter’s pioneering edited volume, *The Economics of Australian Industry*, Hunter (1963). Donald’s part of the chapter is far more interesting and innovative than mine.

The same is true of my collaboration with one of my Australian mentors, the late Russell Mathews, on the chapter on “Company Finance” for Ron Hirst and Bob Wallace’s now classic edited volume, *Studies in the Australian Capital*

*Market* (1964). Russell at this time was Professor of Commerce at Adelaide. His pioneering work with John Grant on inflation and company finance which culminated in their 1958 book had greatly influenced the approach I took in my Cambridge dissertation; in effect, I did a “Mathews and Grant” for UK companies. I adapted their analysis to include insights I had obtained from Joan Robinson’s *magnum opus*, *The Accumulation of Capital* (1956) which I had studied intensively while at Cambridge, see Harcourt (2001a), 7-8.

The most important joint project arising from those first years at Adelaide was the beginnings of what became my first book, *Economic Activity* (1967). It was written jointly with Peter Karmel and Bob Wallace. Peter had developed a superb, if demanding, first year course, “Outlay”, which was basically a rigorous introduction to the economics of Keynes. When he was appointed to be the first Vice-Chancellor of the newly established Flinders University of South Australia in the early 1960s, he asked me to take over the Outlay course and generously lent me his very full set of lecture notes.

As I lectured I began to realise that there was no suitable textbook which took our approach to the issues, so I suggested to Peter that we write one based on our lectures. As I was going on study leave in August 1963, Bob Wallace took the course over from me and also came on board as a third author. Bob had taught me in Melbourne and was a major reason why I wanted to come to

Adelaide<sup>6</sup>. He and Peter had written a way before its time article, “Credit-creation in a multi-bank system” (1962), published in the first ever issue of *Australian Economic Papers*, and his deep understanding of the integration of monetary and finance processes with real processes in systemic analysis served to more than make up for my deficiencies as a real man, not a money man.

### **Cambridge in the 1960s**

Soon after I returned to Cambridge, to my utmost astonishment, I was invited by Joan Robinson to apply for a Lectureship. I was interviewed and appointed in November 1963 (on the day after President Kennedy was assassinated). I had a moral duty to return to Adelaide, so I asked for and received three years leave without pay in order to take up the Lectureship and subsequently a Fellowship at Trinity Hall.

When my appointment was announced in the Cambridge University *Reporter*, the Cambridge University Press wrote to me to ask me whether I had any books on the go. I did not realise that such a distinguished Press liked textbooks but they jumped at the chance to publish what became *Economic Activity* (1967), as did I and its other two authors. I wrote the first drafts during my years at Cambridge and I spent a wonderful summer at Stanford in 1965 with Bob Wallace and his family (he was on leave there) writing second drafts. I

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<sup>6</sup> I had gone as an honours student to Bob’s first ever set of lectures (on international trade). Bob came to Adelaide after two years at Oxford, and he, and then I, encouraged many others to come to Adelaide.



had brought a bottle of fine brandy with me and Californian public radio played classical music continuously. It should be possible to find more mellow and poetic passages in the book as the combination of brandy and music made their impact on our composition as the Californian evenings drew in.

When Robin Matthews left Cambridge to take up the Drummond Chair at Oxford in 1965 (succeeding John Hicks), he asked me to take over his Part I Lectures on the economics of Keynes. I told the undergraduates that 30 years ago Keynes was lecturing to a select group of undergraduates from the proof sheets of *The General Theory*. I added that I was not Keynes and nor were they as select a group of undergraduates, but I was going to give a course of lectures on the economics of Keynes from the manuscript of the emerging book. My most distinguished pupil who attended the lectures at that time is Mervyn King, a former Governor of the Bank of England and now Lord King. Three times in semi-public he has praised the lectures as the ideal introduction to systemic analysis of the economy. I really must get this from him in writing.

*Economic Activity* was published in 1967; for a while it was used widely in Australia and overseas and Paolo Sylos-Labini at La Sapienza in Rome arranged for an Italian translation which was published in 1969.

As well as *Economic Activity*, another volume was published by Cambridge as a result of my time there, this time jointly edited. R.H. (Bob) Parker, the distinguished historian of accounting, and I had been colleagues at

Adelaide before he went, first, to the University of Western Australia and subsequently to the University of Exeter. He asked me to join him in editing a selection of readings in the concept and measurement of income. The selection was published in 1969, a selection that John Hicks subsequently was to describe as a classic. Bob was undeniably the senior editor – hence Parker and Harcourt and his important section of the Introduction – but I did contribute a section based on my earlier work on historical and replacement cost accounting. We also reprinted in the selection what has become my second best-known publication, “The Accountant in a Golden Age”.

The paper was first published in *Oxford Economic Papers* in 1965. The research project on which it was based started when I was in Adelaide. Harold Lydall (who succeeded Peter Karmel as the George Gollin Professor) was puzzled by some findings he had made when comparing accounting rates of profit with economic ones. He asked me to see if I could find the cause(s) of his puzzles. I was helped most in this pursuit in Adelaide by Deane Terrell, my first ever Honours student there<sup>7</sup>, who had recently returned from Oxford and MIT, and was now a Lecturer at Adelaide, and subsequently by Dr Lucy Slater, the whiz kid programmer at the Department of Applied Economics (DAE) in Cambridge. They ran the simulations from which my findings arose because my meagre grasp of algebra would not allow me to work out the general case. This

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<sup>7</sup> Much more importantly, Deane was Captain of the University of Adelaide Australian Rules Football team for which I played.

was done later in the 1980s by Franklin Fisher, see Fisher and McGowan 1983, Fisher, 1984, when he appeared as a witness for IBM in US v IBM to argue that IBM may not be making monopoly profits, or if it were, this could not be inferred from the use of accounting data. In effect, we had asked the same question: if we know what the “true” economic rate of profit is (as we would in a Golden Age), would an accountant let loose in a Golden Age with his/her conventional tools give us the right, i.e., same, answers? My simulations of various possible scenarios and Fisher’s elegant algebra both showed conclusively that the answer was “no”, often by large orders of magnitude. So in a sense Fisher and I had collaborated by osmosis even though he was not aware of my article when he wrote his 1984 one, see Fisher 1985, where he replies to his critics.

Soon after I arrived in Cambridge in September 1963 I met Vince Massaro. Vince was a graduate of Notre Dame, he wrote his Ph.D dissertation on the immiserisation of wage-earners thesis in Marx’s *Capital*, he was the son of Sicilian migrants, a devout Roman Catholic much influenced by the pacifist Roman Catholic Dorothy Day, so naturally he was awarded a NATO Fellowship to come to Cambridge to study the writings of Joan Robinson and Piero Sraffa.

I had had a look at Sraffa’s 1960 classic, *Production of Commodities by Means of Commodities* while in Adelaide. I was completely bamboozled by it but I was determined to study it in depth during my leave. I suggested to Vince

that we work together on this project which we did over the academical year 1963-64. Piero had looked after the Cambridge research students in the 1950s and I had come to know him then. He also loved to meet Italians and Italian speakers so Vince often went to see him in his rooms in Trinity.

The result of our collaboration were two papers – “A note on Mr Sraffa’s sub-systems” in the *Economic Journal* (1964a) and a review article of *Production of Commodities...* in the *Economic Record* (1964b). We had the great advantage of clearing what we wrote with Piero himself so that our note and review article may be claimed to be definitive because Piero finally gave us the OK to go ahead and submit them, though not before some terrific dramas, see Harcourt (1999). Vince and I became and remain firm friends – I was best man at his wedding in Cambridge to Denise; she worked at the *DAE* and Vince courted her when he was in Cambridge.

Another collaborator in Cambridge at this time with whom I formed a life time friendship is Geoffrey Whittington. He was a research officer in the *DAE* working on a collection of UK accounting data, the collection of which had started at the National Institute in London (it was the data on which the empirical parts of my 1950s Ph.D dissertation were based). Geoff worked in collaboration with the pioneering work of Ajit Singh and Gay and Geoff Meeks on the behaviour of UK companies, especially the implications of their take over activities. Geoff was an outstanding applied economist who had advanced

accounting qualifications in his considerable armoury. He and I wrote a paper, Harcourt and Whittington (1965), that commented on the irrelevancy of the British differential profits tax in an article that had been written by A. Rubner and published in the *Economic Journal* in 1964, a journal that then would have been classified as a “brownie point” outlet had there been RAEs operating – which, thank goodness, there was not. It was Geoff’s first publication.

Since then Geoff and I have collaborated twice. He was the third editor of the second edition of Parker and Harcourt, published by Philip Allan in 1986. Part of his contribution was to remove the section of essays on depreciation that had appeared in the first edition and included “The Accountant in a Golden Age”, thanks, pal. We also wrote a joint chapter on the concepts of income and capital for John Creedy’s edited volume, *Foundations of Economic Thought* (1990).

We linked the accountants’ concept of a going concern to the ‘vision’ of capitalism that sees the capitalist classes (all three) as the driving forces of the capitalist mode of production, as opposed to the mainstream ‘vision’ which has the consumer queen in the driving seat. Which ‘vision’ dominates has important implications for the concept of replacement costs to be used in replacement cost accounting reforms to which Geoff contributed hugely over his working life and which were also part of my Ph.D dissertation and subsequent projects.

Though I didn't write any more joint articles or books in Cambridge during the 1960s, I was enormously helped with my single author publications by many colleagues there. I would especially like to single out Maurice Dobb, the kindest and most supportive of men, who responded more than fully and in beautiful handwriting<sup>8</sup> to requests for help; Esra Bennathan, whose enthusiastic encouragement and friendship I value deeply; Bob Rowthorn, to my mind, the most fertile and sharpest mind of the younger people then at Cambridge; Ken Arrow and Bob Solow who were on leave in Cambridge when I was and who brought their great skills to bear on some technical puzzles that had had me foxed; and, last but not least, Joan Robinson who took a great interest in whatever I was doing and usually approved of it – which was just as well as disagreeing with her was not an easy or forgettable past time.

### **Return to Adelaide 1967-72**

I left Cambridge for Adelaide at the end of 1966 to begin another exciting phase of my life. I immediately became deeply involved in the anti-Vietnam War movement in South Australia (Australia and New Zealand were the USA's only "respectable" allies in that most immoral of wars) and I averaged two and a half days a week on anti-war activities for the next eight years.

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<sup>8</sup> Maurice always sent hand-written versions of his manuscripts to publishers because he did not trust typists to get his punctuation right.

As far as my academic life was concerned, a career-changing event occurred in 1968. Mark Perlman was visiting Melbourne University. He had recently been appointed the founding editor of the newly established *Journal of Economic Literature* (*JEL*). While in Melbourne the author he had commissioned to write the survey article (on capital theory) for the second issue let him know he could not do it for fear of offending his patrons. Wilfred Prest, who was Professor of Economics at Melbourne then and when I was an undergraduate, suggested to Mark that I might be an appropriate replacement – he thought from what he knew of me as an undergraduate that I could be good at explaining what other people had written. Perlman visited me in Adelaide in August and after a hard day's sell I agreed to write the survey – I had no patrons to offend! – and to deliver a first draft by the end of the year.

So I took temporary 'leave' from my anti-war activities (with the blessings of my comrades in the movement) and retired behind a door which prior to that I always left open for colleagues and students to drop in for chats. I put a notice on the door "Man at work", which someone thoughtfully altered to "Maniac ...".

As the project in its totality threatened to overwhelm me I decided to write working paper drafts of segments of the survey. I sent these to about 30 friends around the world, some of whom were sympathetic to the approach of Cambridge, England, to the then ongoing controversies in capital theory

between the two Cambridges (Mark had asked that in the survey I concentrate on the issues involved), others who were not sympathetic, but as friends sent me invaluable comments and criticisms. I sent off the draft on time, then revised it in the light of Mark's feedback and the final version was published in the June 1969 issue of the *JEL*. The list of people thanked in the opening footnote is in a sense a mini *Who's Who* of the profession at the time. I singled out Joan Robinson for her comments and great encouragement which kept me going. I especially thanked Pippa Simpson for her expert mathematical advice, without which I would have been even more lost than I obviously had been in the jungle of squiggles that characterises most modern economic theory.

One of the people whose comments were most helpful to me was Mario Nuti who was then teaching at Cambridge. A by product of sending out the working papers was that Frances Welch, Mario's partner, was at the time the Economics Editor of Cambridge University Press. She came to know of them and as a result commissioned me to write for the Press a book of the survey. It was published in 1972, a pleasing by product of what I like to think of as bedside reading. Though I did not implicate anyone in the views I took in the survey, this whole experience of willing collaboration is a leading highlight of my working life.

Mark Perlman and I became firm friends. He wrote a Foreword to a selection of my essays, Harcourt (1995a), published by Edward Elgar in the



Economists of the Twentieth Century series that Mark edited with Marc Blaug. I was delighted to be a joint editor with Hank Lim and Ungsuh Park, two of his former doctoral students, of his *Festschrift* volume, most appropriately titled *Editing Economics* (2002). When he retired as editor of the *JEL* in 1980, I organised a round robin letter signed by 36 AEA members located all around the world, which was sent to Moses Ambrovitz who succeeded him. It thanked Mark for his outstanding, fearless and liberal editing, see Lim *et al* (2002), 3-4.

I spent the academical year 1972-73 as a Visiting Fellow at Clare Hall, Cambridge, where the Harcourt family overlapped with the Asimakopulos family – Tom was also a Visiting Fellow. Tom and I had been Ph.D students at King's in the 1950s. We were both close friends of Keith Frearson who had taught me at Melbourne and who was then a graduate student at Cambridge. In 1955-56 we went to Joan Robinson's lectures on what was to become *The Accumulation of Capital* (1956). Keith was enthralled, Tom was irritated by her criticisms of MIT economists, and I was mystified, not least because when she came to a crucial point in the argument, she dropped her voice so much that she went unheard, at least by me. In the 1960s Tom went on leave to MIT. Listening to Bob Solow's lectures, the scales fell from his eyes, he twigged what Joan had been on about and from then on became one of her most devoted (but always critical when justified) disciples. I had published in *Australian Economic Papers* one of the first fruits of Tom's conversion, see Asimakopulos (1969a, 1969b).

At Clare Hall Tom and I decided to write a book on economic growth reflecting the Cambridge approach. The economics editor of Allen and Unwin had been urging me for some time to write such a book. I said I would if he gave me lunch at his London Club (The Reform). In the event, he had to fork out for two lunches and we never did get to write that book. We did, however, collaborate on a note, “Proportionality and the neoclassical parables”, which was published in the *Southern Economic Journal*, Asimakopulos and Harcourt (1974). It was a comment on Charles Ferguson’s Presidential Address to the Southern Economic Association, Ferguson (1972). Our note established clearly why only the “corn” model produced results that were consistent with the central neoclassical view that all prices are indexes of scarcity; in  $n$  commodity models the “agreeable” parables reflecting this intuition were not generally applicable. The technical skills and extreme clarity of the exposition are overwhelmingly due to Tom, whose teaching and written work had these traits in abundance. Until Tom’s untimely death in 1990, we regularly exchanged and commented on our ongoing research papers. My evaluation of Tom the person and the economist may be found in, for example, Harcourt (1991) (2008).

After Tom died a week-long conference in his honour was held at the Levy Institute of Bard College in Up State New York. The papers given at the conference formed the basis of the volume, *Income and Employment in Theory and Practice* (1994) that Alessandro Roncaglia, Robin Rowley and I edited for

Macmillan. Unbeknownst to me at the time of the conference, I was actually dieing from the onset of type 1 diabetes. Most fortunately, Esther and Hy Minsky – Hy had diabetes – were at the conference and, realising that something was wrong, took my blood sugar level on Hy’s machine. It went off the Richter scale and I ended up in Emergency at the hospital in Up State New York. I often remark that, except for the Minskys’ timely intervention – my Minsky moment – the participants could have stayed on for another day for a conference in my honour and so spread the overheads.

### **Collaboration in Adelaide in the 1970s**

In Adelaide in the 1970s I supervised some outstanding Master’s and doctoral students with some of whom I collaborated. Peter Kenyon came to Adelaide in 1974, after finishing his undergraduate course at Monash University in Melbourne, to do a Master’s degree under my supervision.

In my last year at Cambridge I had written a paper, “Pricing and the investment decision”, in which I tried to analyse the determination of the size of the mark-up by a price leader in an oligopolistic market structure, whereby discretion in setting prices was directed towards raising internal funds with which to finance planned investment expenditure. The paper was rejected by the *Oxford Bulletin of Economics and Statistics*. I was able to deduce from one of the referees’ reports that a referee had been G.B. Richardson, an unsung hero of

the British and, indeed, the world economics professions. (Moral: never let your initials be put on your referee report.) He liked the project but detected a logical flaw which vitiated my arguments<sup>9</sup>.

I put the paper on the back burner but I suggested to Peter that he work on these and other issues in post-Keynesian price theory for his Master's thesis. In 1974 I had a serious operation which put me in hospital for three weeks. On the day I was discharged Peter was giving a progress report on his research. I went from hospital to his seminar on the way home. Listening to his report, the solution to the logical problem suddenly came to me so when I went home, in a state of euphoria, I sketched the theoretical arguments and gave them to Peter to put the scholarship around them. We submitted the resulting paper to the *Economic Journal* (then edited by David Champernowne and Brian Reddaway) because I thought that Brian would like the “down-to-earth” nature of our analysis. Alas, neither he nor the referees did like the paper and he asked Champ to write the rejection letter. He told Champ he was embarrassed to do so because he and I were such good friends. Champ wrote ruefully in the letter, “where did that leave him?!”<sup>10</sup>

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<sup>9</sup> During one of our many moves I lost the file containing the “go to whoa” correspondence and drafts associated with the paper so I cannot now check exactly what the flaw was – I think it had something to do with inconsistent time periods associated with price setting and investment planning.

<sup>10</sup> See my essay, Harcourt (1995b; 2012b) in Shepherd (ed), *Rejected* (1995) for a full account of the making of the paper and Harcourt (2004) and Harcourt (2012) respectively for my tributes to Brian and Champ, alas, both now dead.

Bruno Frey, who I had met at Cambridge, was then editor of *Kyklos*. He had often asked me to submit a paper to the journal so I suggested to Peter that we send our paper to him. It was quickly accepted “as is”, it was published in 1976 and is now regarded as a classic in the post-Keynesian literature on pricing, see, for example, Coutts and Norman (2013).

As I mentioned above, Prue Kerr, having been an undergraduate student of mine at Flinders, came to Adelaide in the 1970s to write a Master’s thesis under my supervision on the characteristics of the Cambridge School of Economics, especially in relation to Marx. Prue’s outstanding thesis (both examiners praised its maturity and deepness of thought and analysis) and our discussions while it was being written mark the beginning of our long lasting, still ongoing friendship, collaboration and my education in what Marx was on about. (As I have often written, Marx’s *Capital* was the only “great work” I could not make head nor tail of when as an undergraduate I did History of Economic Thought in 1952.) As far as Marx and Prue are concerned our collaboration culminated in one of my favourite essays – our joint chapter on Marx, Harcourt and Kerr (2001), written for Malcom Warner’s *International Encyclopedia of Business and Management* (2001), all his readers needed and wanted to know about the great man.

Prue left Adelaide in the late 1970s to do the M.Phil in Economics at Cambridge where, some years later, I became her Ph.D supervisor. Prior to this

in 1980-81 she edited my first selection of essays, *The Social Science Imperialists*, published by Routledge in 1982.

Education in Marx and collaboration were also added to in the 1970s by my supervision of Allan Oakley's fine Ph.D dissertation on the formation of Marx's views up to the writing of *Capital*. It subsequently became the basis of three outstanding volumes, Oakley (1983), (1984), (1985). Claudio Sardoni came to Adelaide on an Italian scholarship and wrote his dissertation on Marx and Keynes on recession, showing that when they examined the same issues they mostly came up with same answers, adjectives aside. I quote below from the Foreword I wrote to the book based on the dissertation, Sardoni (1987). After noting that "I learnt more from Claudio than he ever did from me", I wrote that his book was "a fine example of analytical history which gives readers the feel both for what their great predecessors achieved and for what is the appropriate framework within which to continue their work ... the book is an absorbing story of theories which were not only relevant in their authors' days, but in ours too" (xi).

Claudio and I were subsequently to write three joint chapters for edited books. The first was "Keynes's vision: method, analysis and tactics" in John Davis's volume *The State of Interpretation of Keynes* (1995). The second was our chapter, "*The General Theory of Employment, Interest and Money: three views*", in the *Festschrift* volume for Paul Davidson edited by Philip Arestis

(1997). The third was “George Shackle and Post-Keynesianism”, a chapter in the memorial volume for George edited by Peter Earl and Stephen Frowen (2000).

In the chapter for Davidson’s *Festschrift* we compared and contrasted the interpretations of *The General Theory* to be found in two great biographies of Keynes – Moggridge (1992) and Skidelsky (1983, 1992, 2000) – with those of Paul Davidson over many years. Paul and Skidelsky both had a post-Keynesian interpretation, not a view Don Patinkin ever accepted. Claudio and I thought that Skidelsky’s chapters on *The General Theory* contained deeply incisive and correct understanding of the significance of the contributions of Keynes’s *magnum opus*. Moggridge documents superbly the making and the aftermath of the book and of its contents. We praised Paul’s painstaking, evidence-based, accounts in many places of Keynes’s essential insights. Paul built substantially on these in his own contributions to our understanding of modern monetary production economies. Claudio’s understanding of the messages in primary sources together with his analytical skills greatly enriched the narratives of the chapters. I was also delighted to find that Shackle’s biographers, Peter Earl and Bruce Littleboy, stated that Claudio and I got it right in our discussion of George Shackle and post-Keynesianism, see Earl and Littleboy (2014), 39.

In 1992 Claudio edited for Routledge a selection of my essays from the previous 30 years, entitled *On Political Economists and Modern Political*

*Economy*. Routledge had previously published the first volume of my selected essays in 1982, *The Social Science Imperialists*, which, as I noted above, Prue edited. In 1986 Omar Hamouda edited a further selection, *Controversies in Political Economy*, which was published by Wheatsheaf Books. So I have been three times more fortunate with editors than was Ricardo who had Piero Sraffa; or rather, one and a half times as Piero's extraordinary edition of Ricardo's works and correspondence was latterly done with the collaboration of Maurice Dobb.

### **Collaboration in Canada**

Another major collaboration arose from two visits I made to the University of Toronto in the winters of 1977 and 1980. Jon Cohen and I edited a *Festschrift* volume for Lorie Tarshis, *International Monetary Problems and Supply-side Economics* (1986), published by Macmillan. The title takes in issues that were very much on Lorie's mind at the time. We presented the copy to him as a (pleasant, we hope) surprise at a conference in his honour held in Toronto.

I had come to know Sue Howson and Don Moggridge in Cambridge in the 1970s. Don was then a Lecturer in the Faculty and a Fellow of Clare; he had taken on the gigantic task of editing Keynes's papers alongside Austin Robinson and Judith Allen. Don was a Canadian and decided to return to Canada in the late 1970s to the University of Toronto (U of T). Lorie, a Canadian and a



graduate of the U of T, had subsequently attended Keynes's lectures in the 1930s when Keynes was making *The General Theory*. After the Second World War Lorie taught at Tufts and then at Stanford where I first met him in 1965. In the 1970s Lorie decided to return to Canada, sickened by the growing tide of illiberalism in the USA, and by the war in Vietnam. A department of economics principally staffed by a group of very bright young scholars including Sue and Don had been set up as an outreach Campus of the U of T 20 minutes out from downtown Toronto, Scarborough (known to us all as Scarberia). Lorie was the wise guiding chair. Through Sue and Don I was invited there in 1977 and subsequently I was asked to come for a semester every two years.

In 1977 while I was in Toronto my greatest Australian friend and mentor, Eric Russell, tragically died in Adelaide after a game of squash. Jon Cohen was also at Scarborough. He had similar traits to Eric's and became my greatest friend there. We both were tremendous admirers of Lorie, for why see, e.g. Harcourt (1995c), so we set about preparing the *Festschrift* for Lorie. This was one of the most enjoyable collaborations of my life, working with one friend in order to honour another by commissioning contributions from other mutual friends and admirers of Lorie. They provided fine chapters with which we believe (hope) Lorie was well pleased.

It was through Jon that I came to meet his namesake Avi Cohen who lived near Jon and Lorie and taught at York University. Avi had been a graduate

student of Don Harris at Stanford (Don was a close friend of Joan Robinson and mine). Avi and I had a mutual interest in controversies in capital theory. I persuaded him to spend a year's leave (1980) at Cambridge as a Visiting Fellow of Clare Hall, one of my four Cambridge colleges. From this our sustained collaboration and friendship grew.

When Timothy Taylor, the managing editor of the *Journal of Economic Perspectives*, one of the few journals left that all economists can both read and gain sustenance from, asked me in the early noughties to write about the Cambridge-Cambridge capital theory controversies for the journal's Retrospectives section, I asked that it be a joint paper with Avi. Otherwise I felt I would be trespassing on the understanding of the issues concerned that I had learned from him. The upshot was the paper "Whatever happened to the Cambridge capital theory controversies?" (2003). In it we argued that the 1950s to 1970s debates were but the latest in a series of such debates about similar issues dating back at least to Böhm-Bawerk, J.B. Clark, Irving Fisher and Thorstein Veblen at the turn of the last century. We set out the arguments and results involved and the unsolved issues between the two camps about the significance of the results.

At about this time Edward Elgar approached me to edit volumes of readings in capital theory. I asked him whether Avi could be a co-editor and as well could we follow the model of the book on readings in capital and growth,

which was published by Penguin in 1971 and which I had co-edited with Neil Laing, a colleague of mine at Adelaide. Neil took an independent and original though basically neoclassical approach to the issues. The idea was that by me writing an introduction to the readings with a Cambridge, England, stance and Neil, another, neoclassical one, readers, principally students, would not only have the differences set out but would also have primary evidence in the readings to enable them to make up their minds on where they stood on the issues.

I had come to know Christopher Bliss at Cambridge in the 1960s. Though we did not agree on the capital theory debates he had been a great help to me with his comments on the working papers for the *JEL* survey. Moreover, his 1975 monograph on capital theory and the distribution of income, Bliss 1975, is one of the finest works of scholarship in modern economics. So I asked Edward could Chris also be a joint editor and contribute a Laing-like introduction to the volumes while Avi and I wrote the other, “correct”, introduction.

The three volumes were published in 2005, Bliss *et al* (2005). In retrospect I realise I made a serious tactical error, one which I avoided when collaborating with Neil, whose surname started with a first letter lower in the alphabet than mine – hence Harcourt and Laing with my introduction coming first. In the 2005 volumes I am relegated in citations to Bliss, *et al* (eds) and Chris has first bite of the cherry at persuading readers who is right, as his

introduction also comes first. Nevertheless, despite some dramas and hissy-fits on the way, the final product is one we could be proud of and the editors especially appreciated David Laidler's endorsement of the volumes. David is one of the finest scholars in our trade so it was reassuring that he wrote that our "collection addresses the topic's intrinsic difficulties head on. [Moreover] because it is beautifully balanced and thoughtfully organised it makes the many complexities of capital theory accessible to anyone willing to make the effort to work through it. And that ought to be all of us".

In 2010, the late Marc Blaug and Peter Lloyd edited a volume, *Famous Figures and Diagrams in Economics*. Avi and I wrote/drew the chapter on capital-reversing and reswitching. As has become our practice, I sketched the first draft (it is published in Harcourt (2012b)) and Avi provided the scholarship and analytical polish. We used the relevant diagrams from my 1972 book to give them the chance to go down in history – but, as far as I know, none of the reviews of the Blaug Lloyd volume have mentioned our chapter.

As I write (March 2015) Avi and I are following the same procedure in order to produce 1800 words on the Cambridge debates for Cyrus Binha and Chuck Davis's edition of *Global Economics: Encyclopedia of Crisis and Transnational Change*.

### **Further Collaboration in Adelaide**

When in Cambridge in the 1960s I published a paper, Harcourt (1966), in *R.E. Studs* (my only ever excursion into what Dennis Robertson called “The Green Horror”) on biases in empirical estimates of values of the elasticity of substitution of CES production functions. It was a satirical paper (though one referee thought in serious econometrics that failed); it included making up imaginary scenarios to see whether the econometric methods used in the CES literature in fact established ‘true’ values or were subject to arbitrary biases because the theoretical models behind the specifications, especially the variables they contained, did not match those of the actual processes that threw up the data used.

Subsequently, I collaborated in Adelaide with Al Watson and the late Peter Praetz on a similar project. We argued that one of the joys of living in the age of computers was that it allowed economists to play God – we could make up worlds with known parameter values and then see whether econometric methods actually provided unbiased estimates of these values. Fred Gruen and Allan Powell has published an important article in the *International Economic Review*, Powell and Gruen (1970), the last in a series of papers on econometric estimates of constant elasticity of transformation (CET) functions, in order to get a handle on supply responses in Australian agriculture. The “Trinity from Adelaide” wrote a comment on their article, using a Monte Carlo experiment to

argue that their method was not successful. My only contribution was to pose the question and suggest the approach. Al and Peter as excellent econometricians then look over.

Our comment, Watson, Praetz and Harcourt (1970), led to a cross reply from Allan and Fred, together with comments by Ray Byron (who had been a referee) that visited a plague on all our houses, see Powell and Gruen (1970), Byron (1970a, 1970b)<sup>11</sup>.

### **28 Years in Heaven: return to Cambridge in 1982**

Joan and I returned to Cambridge in 1982 for 28 wonderful years in the Faculty (I retired in 1998 but visited the Austin Robinson building a couple of days a week after that) and in Jesus, literally Heaven on Earth. As for collaboration, I first mention the cooperative editorial board of the *Cambridge Journal of Economics (CJE)*. I had been associated with The *CJE* since it started in 1977. The editorial board met every Friday for a working lunch in which the editors accepted, rejected or gave another chance to would be authors. Despite our often heated arguments – I hold the record for the greatest hizzy-fit when I smashed a tea cup on the table, exasperated by a highly predictable response by

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<sup>11</sup> It was Ray who christened us “The Trinity from Adelaide”, Byron (1970b, 576), a wry comment which cost him a chair of econometrics at the University of Western Australia. The Head of the UWA Economics Department who interviewed Ray for the post was an evangelical Christian. He was scandalised that anyone would blaspheme in the pages of the *Record*, a charge to which Ray, a fiery red-haired person of Irish descent, did not take to kindly, to say the least. When he blamed me for him missing out, I told him that he was much better off in his post of Reader at the ANU.

Tony Lawson to my assessment of a paper (we now used paper cups) – I looked forward to each Friday and I count the members of the editorial cooperative amongst my best and closest friends.<sup>12</sup>

I also contributed to Memorial Issues for the Cambridge greats who had died. Gabriel Palma, one of my closest friends, and I wrote the Introduction to the *Memorial issue* for Richard Kahn, Harcourt and Palma (1994).

Before I discuss my many years of collaboration with Prue Kerr then and now, I discuss other collaborations from this period. The first is my only excursion into *Economic Letters*, then the counterpart in economics of *Nature* in the natural sciences. I met Mohammed Dore in Canada and later in Cambridge. We discussed his search for the best form of taxation of exhaustible resources when the market structure in which the output was sold was oligopolistic. I suggested that a sales tax would be preferable to a profits tax. He adapted a model by Partha Dasgupta and others to establish a neat expression for the tax which contained the price elasticity of demand as the key parameter, see Dore and Harcourt (1986). He did all the squiggles and exposition but insisted, most generously, that I be a joint author.

After the note was published, I received a letter from an economist based in Sweden. It said in effect that until he read the note he had always admired my

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<sup>12</sup> In June 2011 the journal hosted a conference in Cambridge in honour of my 80<sup>th</sup> birthday on “The future of capitalism”. The Special Issue arising from the conference was published in November 2014. It includes a “blush making” intro by Stephanie Blankenburg and among the excellent papers, Costis Repapis’s take on developments in economics over the last 50 and more years as seen through my over 100 reviews covering the period.

economic intuition and sensible use of maths, both characteristics, he argued, were now conspicuous by their absence in the *Economic Letters* note. Mohammed took him on; there was an acrimonious but inconclusive exchange of letters. I showed the exchanges to Bob Rowthorn who said “You’re all wrong!”

Next, in the early 1980s at the annual Trieste international summer school for mavericks I met Omar Hamouda, a doctoral student at McGill of my friend Athanasios (Tom) Asimakopulos. During the after lunch siesta I read and commented on drafts of Omar’s dissertation on John Hicks’s writings. Thus began a collaboration when Omar regularly visited Cambridge. He edited the second volume of my selected essays, Harcourt (1986), and we wrote a joint survey article on post-Keynesianism for the *Bulletin of Economic Research*, subtitled “From criticism to coherence?” It has become a much cited article. In it we argued for a ‘horses for courses’ approach to economic theorising and applications – hence the question mark following coherence. We included Piero Sraffa’s classic contributions under the rubric of post-Keynesianism. This is still a controversial proposition, see Harcourt and Kriesler (2013), vol 1, Introduction and Chs 2-4.

Luigi Pasinetti and I became friends when we were Ph.D students at Cambridge. Subsequently we were colleagues at Cambridge in the 1960s. We read and commented on each other’s papers. Luigi often asked my advice about



the tone of his replies to criticisms of his work by, for example, Frank Hahn and James Meade. I believe I helped him get the logic across more clearly by toning down polemical asides! Mauro Baranzini who was a friend and admirer of Luigi suggested to me that we prepare a *Festschrift* volume, Baranzini and Harcourt (1993), for Luigi's 60<sup>th</sup> birthday in 1990 (Luigi had returned to Italy by then). We started five years beforehand and presented it to him in 1993, three years after his birthday (Economics is not an exact science.) Mauro and I wrote a long chapter analysing Luigi's many original contributions, providing the evidence for our claim that he is probably the last of the great system builders in our increasingly Balkanised Trade. The wide range of topics in the chapters of our distinguished cast of contributors back up this claim. We celebrated with the recipient at a dinner party at the Graduate Centre in Cambridge ("the Grad Pad"). The Vice Chancellor was having dinner at the next table and when I told him why we were there, he presented us with a bottle of the best champagne – we do things in style in the Ancient Universities.

The National Bureau of Economic Research celebrated its first 50 years of existence with a volume edited by Berndt and Triplett in 1990. Lars Osberg, the review editor of the *Review of Income and Wealth*, who I had come to know in Canada and Cambridge, asked me to write a review article of the volume. I asked Mike Kitson, a colleague of mine at Cambridge who had worked with Nicky Kaldor in the *DAE*, to be a co-author, an inspired request, Mike

contributed some of the most important sections of our review article, including a masterly account of the difference in the approaches of the NBER and those of Cambridge. “The neoclassical approach is a way of doing economics, it is not *the* way. The Cambridge approach is one alternative; an approach, however imperfect, which tries always to place great emphasis on the complexities of the real world. Reliable measurement is dependent on relevant theoretical hypotheses. The neoclassical approach, displayed in this volume, provides clarity and internal consistency. An alternative Cambridge approach, sceptical of the ability of markets to clear, would more readily accept that individual and collective actions are affected by institutions and political and social forces. The resulting picture of the world that emerges may be less-defined but also perhaps less distorted”. Harcourt and Kitson (1993), reprinted in Harcourt (2001), 233-34: This is pure Kitson.

One of my favourite papers was written with my Brazilian Ph.D student, Jorge Araujo<sup>13</sup>. A friend of mine, Mike Lawlor, one of the finest Keynes scholars I have ever met, came across in Keynes’s papers in the King’s Archives, a three way correspondence between Maurice Dobb, Joan Robinson and Gerald Shove on whether an economy could grow if the firms in it were only receiving normal profits. Jorge went meticulously through the correspondence (like Prue he is a born researcher in archives) and then set out

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<sup>13</sup> I was lucky enough to have six wonderful Brazilian doctoral students at Cambridge, and to visit Brazil twice.

beautifully the formal analysis of what became our article, Araujo and Harcourt, (1993); Harcourt (1995), rigorously establishing our combined intuitions concerning the issues involved. We ended up using diagrams and analysis developed by Paul Davidson (1972) and Don Harris (1975, 1978). These provided solutions to the issues raised and helped to illustrate the transition in and development of Joan Robinson's approach from its Marshallian-Keynesian starting point to her mature stance in which the classicals, Marx, Keynes, Sraffa and Kalecki came to dominate her approach and views. It also illustrated what fine and subtle minds the three correspondents had.

One of my most enjoyable and, I believe, important collaborations was with a gifted New Zealand economist, Paul Dalziel. I sponsored his visits to Cambridge in the 1980s and 1990s. Paul is not only a gifted technical scholar, he is also a courageous moral person with fine ideals based on his Christian faith. He and his wife, Jane Higgins, were virtually the only voices crying in the wilderness protesting against the extreme monetarist neo-liberal policies implemented in New Zealand in the latter part of the 20<sup>th</sup> century. Paul complemented his passionate religious ideals with an increasing interest in, and mastery of, post-Keynesian analysis – hence his visits to Cambridge.

In 1993 James Meade published in the *Economic Journal* an account of his role in the “Cambridge circus” in the development of the analysis of Richard Kahn's classic 1931 *Economic Journal* article on the multiplier. Meade analysed

the multiplier process through the leakage into saving through the marginal propensity to save ( $s$ ) rather than the build-up in expenditure through the marginal propensity to consume ( $c$ ). Meade's multiplier formula,  $1/s$ , became known as "Mr Meade's relation". Both Kahn and Meade emphasised the central Keynesian insight that, logically, investment leads and saving follows. Meade put it vividly in Meade (1975), 82, when he pointed out that the essence of the Keynesian revolution was that Keynes changed our view of the world from one of a saving dog wagging an investment tail to the other way around.

In 1980 Feldstein and Horioka published in the *Economic Journal* an influential article in which they argued that, for the world as a whole, it was saving not investment which led, thus returning to a pre-Keynesian view. I suggested to Paul that we should take them on, adapting the analysis of Meade's 1993 article – process/period analysis in a closed economy model – to an open economy model of the world as a whole in which domestic saving and international capital movements were taken into account in our confirmation of the Kahn, Keynes, Meade insight. When I write 'our', it was Paul who provided the elegant formal analysis.

We sent our draft to James and he wrote agreeing with what we had done and providing in one succinct paragraph (which he kindly allowed us to include in our note) what it eventually took us 15 printed pages to establish! After an unsatisfactory round of exchanges with the *Economic Journal*, our note was

finally published in the *Cambridge Journal of Economics* in 1997 (it is reprinted in Harcourt (2001)).

Another important collaboration was with my long-time Australian friend, Peter Riach, who left Australia for the UK at much the same time as I did in the 1980s. Peter became Professor and Head of the Department of Economics at De Montfort University. He hit upon a wonderful project. Noting that famous composers often died leaving unfinished scores which others then finished, he referred to Keynes's promise to Ralph Hawtrey in 1936, never fulfilled because of illness, World War II and his early death, to write "some footnotes" to *The General Theory*, see Harcourt and Riach (1997), xiv. Peter suggested we commission a cast of scholars of Keynes of all varieties to write chapters on what they thought Keynes would have written in the late 1930s and why they themselves had subsequently worked on the aspects of Keynes's insights that they had.

The project resulted in *A 'Second Edition' of The General Theory*, two volumes, published by Routledge in 1997 (and subsequently translated into Japanese with an introduction by the late Hirofumi Uzawa whom I greatly liked and admired, see Harcourt (2014)). The chapters in volume 1 mirror the original chapters in *The General Theory*, those in Volume 2 are overwhelmingly post *General Theory* and Keynes's death. Volume 2 also contains what is probably Jim Tobin's last considered views on the significance of *The General Theory*,

Tobin (1997). Some of our authors chose to write at least the first sections of their chapters as J.M. Keynes, which they did very well. As well as writing the Introduction with Peter, I collaborated with Wylie Bradford, one of my best-ever doctoral students at Cambridge, to write on “Units and definitions”, Ch 7. Again alphabetical order is the correct index of the depth of contribution.

E. Roy Weintraub wrote a rather embittered and unfair review of the volumes in the *Economic Record*, Weintraub (1998). Amongst other things he wrongly classified all our contributors as post-Keynesians (about whom he has a thing) and he denied our volumes any place at all in the canon of respectable, proper HET. In contrast, Tony Thirlwall, a self-declared unreconstructed Keynesian, published a delightful and cleverly amusing review article in the *JPKE*, writing as JMK resurrected, Thirlwall (1999). The late Bernard Corry, a great HET scholar, also published a pleasingly fair minded favourable review in the *Manchester School*, Corry (2000), not long before he died.

Bertram Schefold took over as general editor of a prestigious German HET series. He asked me to contribute a chapter on the representative firm and increasing returns debates of the 1920s in the *Economic Journal*, starting with John Clapham’s empty economic boxes (1922) and ending with the 1930 symposium edited by Keynes, containing articles by Dennis Robertson, Gerald Shove and Piero Sraffa. I asked Stephanie Blankenburg, whose fine M.Phil dissertation on Gramsci and Kalecki I had supervised (this was the beginning of

our now long-standing friendship), to collaborate with me. The result, Blankenburg and Harcourt (2001), was that I wrote on the actual debates and Stephanie drew out their implications for a critique of modern endogenous growth theory, ideas she had developed in one of the essays of her Ph.D dissertation<sup>14</sup>. The English version, “The representative firm and increasing returns: then and now”, was later published in 2007 in a volume edited by Philip Arestis, Michelle Baddeley and John McCombie (and published by Edward Elgar). Again alphabetical order has nothing to do with authorship order for I would have insisted that Stephanie be first, as her deep insights, criticisms and analysis are startling.

### **Collaboration on Visits to OZ**

While we were in Cambridge from 1982 on Joan and I always returned each year to Australia for a month or more, as befits “A Cambridge economist *but* an Australian patriot”, see Hatch and Petridis (1997). From 1997 on our main port of call was the School of Economics at UNSW, mainly because Peter Kriesler was there. One of my UNSW colleagues was Mehdi Monadjemi who had done his doctorate with the late John Cornwall, an outstanding post-Keynesian scholar. John and I had been friends since we met in Cambridge in 1963. When Mark Setterfield, who had been a pupil of Peter and mine at

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<sup>14</sup> I regard her dissertation as the equal best dissertation I have ever examined and/or read.

Cambridge and then had been John's doctoral student at Dalhousie, asked me to contribute a chapter to a *Festschrift* for John, Setterfield (1999), I asked Mehdi to join me. Our chapter was entitled "The vital contributions of John Cornwall to economic theory and policy: a tribute from two admiring friends on the occasion of his 70<sup>th</sup> birthday", Harcourt and Monadjemi (1999). I surveyed John's contributions and insights and Mehdi supplied a case study on the role of housing and its finance over the trade cycle, a study arising from his dissertation and an ideal complement to my overview.

### **In Cambridge from 1982 on**

My principal intellectual reason for returning to Cambridge in 1982 was to attempt to document the contributions of those who had worked with Keynes – Richard Kahn, Austin and Joan Robinson, James Meade, Piero Sraffa – or who had been greatly influenced by him – Nicky Kaldor, David Champernowne, Brian Reddaway, Dick Goodwin, Dick Stone and in my generation, especially Luigi Pasinetti. By 1982 the first group had become elder statespersons and were to die in the 1980s and 1990s – indeed, only Luigi and I are now alive! I had been writing oral histories and essays in intellectual biography since the 1970s. Drawing on this background, in 1990s I collaborated with my colleagues and friends, Allan Hughes and Ajit Singh, to publish short obituary tributes to Austin Robinson, Harcourt, Hughes and Singh (1993), and to the great Indian



economist, Shukhanoy Chakravarty, who had spent time in Cambridge and who had died tragically young in his mid-fifties, see Harcourt and Singh (1991).

I came to know Dick Stone in the 1960s when he and Allan Brown were running the Cambridge growth project team. To my delight he told me how much he liked *Economic Activity*, the proof sheets of which he had read in the DAE Library. One of my oral histories was based on conversations I had with Dick in the 1980s (it was subsequently published in Harcourt (1995)). I was the obituary editor of the *Economic Journal* for eight years. When Dick died in 1991, I asked Hashem Pesaran to write Dick's obituary for the journal. Subsequently we made it a joint obituary, a complementary combination of Hashem's great technical strengths and his appreciation of Dick's outstanding technical contributions with my evaluation of Dick the person, see Pesaran and Harcourt (2000). Our obituary article has been well received by the profession, especially by those who knew and admired its subject, and it is often cited and downloaded.

Ever since the publication in the *JEL* in 1969 of my article on the Cambridge-Cambridge capital theory controversies, I have often been asked/commissioned to write survey articles. One of those – it is on post-Keynesianism – was commissioned for Shri Bhagwan Dahuya's series, *The Current State of Economic Science*. I had come to know Luke Spajic, a graduate student at Cambridge, and we had had many discussions on issues in banking

and finance, issues on which he was knowledgeable and I, a tyro but a keen learner. So I asked Luke to combine with me whereby I would write on the real aspects and he on those relating to banking and finance, especially the theory of endogenous money. Of course, we stressed that one of Keynes's greatest insights was that the real and money must be integrated from the start in the analysis of a monetary production economy. This did not preclude another core insight, this from our founder Adam Smith, of the advantages of specialisation and the division of labour. The survey was published in 1999.

Robert Skidelsky's superb three volume biography of Keynes (1983, 1992, 2000) was being written and published while I was in Cambridge. Skidelsky was a close friend of the Kaldors and stayed with them for much of the time while he was writing volumes 2 and 3. I came to know him and we had many discussions on matters Keynes. So when volume 3 was published I decided to write a review article of the three volumes. Before this I had examined an outstanding Ph.D dissertation Sean Turnell had written at Macquarie University in Sydney. Sean and I became close friends when I sponsored his first visit to Cambridge as a Visiting Fellow at Wolfson College.

I suggested to him that we make the review article a joint effort. I would write the first draft of our evaluation of volumes 1 and 2, he, that of volume 3. This division of labour reflected our comparative advantages: my knowledge of Cambridge social groupings and the intricate goings on of the Bloomsbury circle

of whom Keynes was a key member<sup>15</sup> and my many years of teaching and writing about Keynes's books and articles. Sean was a whiz kid on international trade, capital movements and institutions and so Keynes's roles at Bretton Woods and in post war reconstruction were very much his cup of tea.

In the event our review article took over four years to write and came to 16,000 words (after all Skidelsky had written three large volumes). We had intended it for the *Cambridge Journal of Economics* but the referees and editors felt otherwise<sup>16</sup>. So we sent it to Jayati Ghosh, a former doctoral student of mine at Cambridge in the 1980s<sup>17</sup>, who is closely associated with the *Economic and Political Weekly* (EPW), the influential and widely read Indian journal. It was quickly accepted, see Harcourt and Turnell (2005), but we had to prune it, so losing some relevant, dispensable for this purpose, footnotes. The unexpurgated version is the title essay of a selection of my essays, Harcourt (2012a), published by Palgrave Macmillan.

Our evaluation of Skidelsky's volumes was highly favourable, especially on Skidelsky's take on what happened at Bretton Woods and the disastrous consequences of this for the survival of the Bretton Woods institutions in the postwar period. We also especially liked Skidelsky's deep understanding of the

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<sup>15</sup> Noel Annan once quipped that the Bloomsbury circle lived in squares and loved in triangles.

<sup>16</sup> A necessary but not sufficient condition to be an editor is to have one or more articles rejected by the journal. I have been necessary more than once.

<sup>17</sup> Jayati and Terry O'Shaughnessy, another of my Cambridge Ph.D students, were the rapporteurs at the Keynes Centenary Conference in King's in 1983. Our mutual efforts – I was very much the junior partner – recorded the discussions at that never-to-be-forgotten gathering, see Worswick and Trevithick (1983)

meaning and relevance of *The General Theory*. (Don Patinkin had criticised him for adopting a post-Keynesian interpretation, see Skidelsky (1992), xi, a criticism up with which we would not put). We did part company with the author over his evaluation of the criticism by Étienne Mantoux (1945; 1952) of the theoretical and applied analysis in *The Economic Consequences of the Peace*, Keynes (1919; C.W., vol II, 1971). Mantoux wrote his book after *The General Theory* had been published and criticised Keynes's use of the pre-*General Theory* quantity theory of money framework with its implicit assumption of full employment which made the problem of reparations, economically anyway, seem more serious than it in fact was. He wrote as if he were *Lord Keynes* after *The General Theory* criticising *Mr Keynes* before *The General Theory*.

In footnote 17 I mentioned Terry O'Shaughnessy. Terry is an Australian from Adelaide who I had met in the anti-Vietnam war protests in the 1960s and 1970s. He was then an engineering undergraduate and a member of Christians for Peace. He subsequently became a Communist – Australia then had three communist parties, Russian, Chinese and intellectuals, all meeting in their own separate telephone booths. Terry belonged to the last and was a journalist on their newspaper. He did a Master's Degree in Political Thought at Macquarie and subsequently came to Cambridge to do the M.Phil in Economics and then a Ph.D which I supervised.

At this time there was a bitter dispute about whether *The General Theory* was set in the short period or the long period, with Richard Kahn and Joan Robinson arguing for the first, Pierangelo Garegnani, John Eatwell and Murray Milgate, for the second view. Terry and I wrote a paper on this theme, Harcourt and O'Shaughnessy (1985), for the Keynes Centenary Conference at the University of Kent, that resulted in a volume, *Keynes and his Contemporaries*, which I edited, Harcourt (1985). We came down on the side of the short-period interpretation, a correct but dangerous stance.

### **Prue Kerr and I**

As well as Prue and I collaborating on Marx, we wrote a chapter, "The Mixed Economy", for the 1980 volume, *Labor*, edited by Jane North and Pat Weller. The arguments of our paper were fed by me into Discussion Paper No. 6 of the series of discussion papers put out by the ALP National Committee of Enquiry which was set up in the late 1970s to find out why the Labor Party had fared so badly in Federal elections after the dismissal of the Whitlam government in 1975 by the Governor General, a shameful act which greatly divided the Australian community. We contributed a package deal of economic policies, which, I like to think, Bob Hawke, the incoming ALP Prime Minister in 1983, implemented for a good half hour after coming into office. The government did put into place the Accord, an incomes policy designed to fit in

with the then characteristics and institutions of the Australian labour market which Eric Russell, Wilfred Salter and others had developed since the 1950s, see Harcourt (2001b).

Prue and I reckon that between us we have written over 100 essays on the theme of Joan Robinson and her circle. In 2009 our intellectual biography of Joan was published in Tony Thirlwall's series with Palgrave Macmillan, "Great thinkers in economics". Though very much a joint work, the first drafts of the 12 chapters were divided equally between us. I wrote the chapters on most of Joan's major books – *The Economics of Imperfect Competition* (1983), *Introduction to the Theory of Employment* (1937a), *Essays in the Theory of Employment* (1937b), *The Accumulation of Capital* (1956), *Economic Heresies* (1971) and *Introduction to Modern Economics* (1973), her introductory textbook co-authored with John Eatwell. Prue wrote on Joan Robinson's contributions to Marxian economics, concentrating especially on her exchanges with Maurice Dobb while she was writing her 1942 *Essay on Marxian Economics*. Prue also wrote on Joan's pre-war and wartime essays and addresses on the BBC concerning left-Keynesian theory and policies for the war and post-war years. One of Prue's chapter was on the three books Joan wrote for the general reader, *Economic Philosophy* (1962), *Economics: An Awkward Corner* (1966), and *Freedom and Necessity: An Introduction to the Study of Society* (1970). Prue wrote the chapter that centres around Joan's 1978 book on development

economics, a book we both admired. I wrote the chapters on Joan's role in the run up to the publication of *The General Theory* and after, and on *The Accumulation of Capital* and the Cambridge-Cambridge capital theory debates. The Introduction and Conclusion were combined efforts.

Our book was the culmination of decades of work, in which we published Introductions to some major volumes as well as articles and chapters in books. These include the entry on Joan in *The International Encyclopedia of the Social and Behavioural Sciences*. Smelser and Baltes (2001), our "General Introduction" to the five volumes of essays on Joan in Routledge's series, *Critical Assessments of Leading Economists* (2002), in which we included an essay, "On Joan Robinson and China", co-authored with Pervez Tahir. Pervez had been my Ph.D student at Cambridge; he wrote on Joan's contributions to development economics, Tahir (1990a). Subsequently he was the 1990 Joan Robinson Memorial Lecturer at Cambridge where he wrote a comprehensive manuscript on what Joan had written on China. Our paper presents the gist of his findings, Tahir (1990b). Also in 2002 we co-authored the Introduction to the Palgrave Archive Edition of Joan's books. In 2003 we published "Keynes and the Cambridge School" in *A Companion to the History of Economic Thought*, edited by Warren Samuels, Jeff Biddle and John Davis. In 2010 we had a chapter, "*The Accumulation of Capital* over 50 years on" in Stefano Zambelli's *Festschrift* for our great pal, Vela Velupillai. We drew on this for our

“Introduction” to the republication of her 1956 *magnum opus* in the Palgrave Classic Economics Series, published in 2013. In 1956, as a graduate student, I had locked myself up with her book for a term and then read a paper on it to the research students’ seminar, spread over two meetings. Joan came to the third to answer our questions. Alas, during our many moves since then, I lost the paper. I would dearly liked to have compared its findings with our evaluation over 50 years on.

Prue is an expert scholar in Archives, ably gathering relevant material to provide evidence to back up her extremely subtle analysis of issues and people. I have a huge file of our correspondence over the decades. I never cease to be amazed by the subtlety of her thought and her writing. She is able to penetrate to the core of difficulties and explain them with highly intelligent clarity. In many respects, she is the Virginia Woolf of economic analysis – intuitive, deep, innovative. Furthermore, she has a sure feel for context combined with balanced, if often unexpected, evaluations. To have collaborated with such a multi-talented person is one of the most pleasant and rewarding experiences of my life as an economist. Furthermore, her close friendship with Joan (Harcourt) and myself has enriched our lives for nearly 50 years.

Overlapping my last period in Cambridge and now time at UNSW was a most exciting collaboration with Peter Nolan, my long-time friend and colleague at Jesus. Both of us are friends of Amiya Bagchi, the distinguished Indian



economist and historian, who had been a Fellow of Jesus in the 1960s. When I was asked to contribute a chapter to a *Festschrift* for him, I asked the editors that Peter and I co-author it. As I have often mentioned, the article that most influenced me as an undergraduate and ever after is Kurt Rothschild's 1947 classic in the *Economic Journal*, "Price theory and oligopoly". As a radical from the 1960s on I was also familiar with the writings of Stephen Hymer who had been Charles Kindleberger's doctoral student at MIT and who was a guru of the left until his tragic death in a car accident when he was only 40 years old.

Both Rothschild and Hymer had predicted what would be the likely outcomes in the market structures of capitalism over the long haul – Rothschild, a world of giant multi-national oligopolies, Hymer, one of monopolies. Peter has a huge set of case studies of large multi-national companies. We used these as the empirical evidence to test who of our heroes was closest to the truth in our chapter, Harcourt and Nolan (2009), which we subtitled "Kurt Rothschild and Stephen Hymer revisited". Rothschild won.

### **At the School of Economics UNSW, 2010 –**

I come now to post-Cambridge years at the School of Economics at UNSW where Peter Kriesler, John Nevile and I have collaborated on many papers. They are mostly concerned with post-Keynesian theory and policy but there are also historical essays on Kalecki and Joan Robinson, Harcourt and

Kriesler (2011), Kalecki and Rosa Luxemburg, Harcourt and Kriesler (2013a) and Harrod and Fel'dman, Kriesler and Harcourt (2015).

In the second half of 2007, Michael Szenberg and Lall Ramrattan asked me to edit *The Oxford Handbook of Post-Keynesian Economics* for Oxford University Press, USA. I started on this mammoth task during my last years at Cambridge. In the first half of 2010 I asked Peter to be a joint editor – Peter is noted for his editorial skills and expertise with computers, the internet and so on. (I am hopeless with anything mechanical, having only recently graduated from a quill pen to a biro. In Cambridge I was completely dependent on a succession of long-suffering Fellows Secretaries to “do” my emails.) The result has been six years hard (but loving) labour culminating in our two volume handbook, Harcourt and Kriesler (2013b). Many of my former collaborators are, of course, to be found in the volumes. The volumes themselves, to our great relief, so far have been favourably reviewed, notably by Steven Pressman in the *JEL* (2014) and by Renée Prendergast in *ELRR* (2015).

In 2013, Palgrave Macmillan published *Financial Crises and the Nature of Capitalist Money. Critical Developments from the work of Geoffrey Ingham* which was co-edited by Jocelyn Pixley and myself. Geoff Ingham and I were colleagues and friends for many years at Cambridge, first within the Faculty of Economics which used to be proud of its team of sociologists until the squiggle merchants took over and booted them out, and then in the Faculty of Social and

Political Sciences (SPS). I had been a sort of mid-wife to the making of Geoff's ground-breaking book, *The Nature of Money* (2004). I came to know Jocelyn at UNSW and I wrote a most favourable review of her splendid book on emotions in finance, Pixley (2004), see Harcourt (2005). We both are foundation members of the Geoffrey Ingham fan club.

Jocelyn had the innovative idea of getting economists and sociologists inside one set of covers to discuss and develop Ingham's ideas, especially in the light of the recent and ongoing financial crisis. As the word 'no' is missing from her vocabulary, she insisted that I be a co-editor rather than just helping her as she put the volume together. The climax was a great two day conference in Geoff Ingham's honour at his college, Christ's, in August 2013, at which the volume was launched and the contributors talked to their chapters. As Geoff had long been wine steward at Christ's we naturally celebrated our scholarship in the only way Oxbridge understands, that is to say, scholarship has always been born and celebrated there in food and drink.

### **Post-Keynesians down under**

I had promised Joan (Harcourt) that the *Oxford Handbook* would be my last major project. However, I had in mind a smaller project, preparing a volume of selected essays to take me into double figures on this score. Peter Kriesler pointed out to me that he and I had collaborated on many papers since I had

come to UNSW and that he had also collaborated for many years with John Nevile at UNSW and Joseph Halevi of Sydney University. He suggested that we put together four volumes of selected essays by post-Keynesians down under under the rubric of theory and policy from an historical perspective.

Palgrave Macmillan had responded favourably to my initial proposal for a tenth volume and they then readily agreed to the larger project which is now (March 2015) nearing completion (I hope). These collaborations have always been rewarding. In general we agree but, if we do not, we argue matters through either to reach an agreed position or, occasionally, to put in a dissenting footnote. The four volumes contain joint essays by Peter, John and Joseph written over many years and mine since 2010. One pleasing offshoot is to be able to bring together in one place Joseph's remarkable contributions over an extraordinary range to the political economy of our time, contributions that have never been properly appreciated, possibly not even known about, by his Philistine colleagues at Sydney.

At UNSW I share a room with John Nevile who has been at the University for 50 years, having joined as Professor of Economics in the 1960s. There was a splendid conference to celebrate John's 80<sup>th</sup> birthday and his many outstanding contributions to our trade and to University and Australian life generally. Peter Kriesler, John Langmore, a long-time friend of John and mine, and I gave a paper which was subsequently published in the Special Issue in

honour of John Nevile in the *Economic and Labour Relations Review* (John has been associated with the journal since its inception). The title of our article, “Faith, works and talents entwined: driving forces behind John Nevile’s contributions” (2013), says it all.

As I noted, Allan Barton was one of the first persons with whom I wrote a joint article. Allan died in 2012. He was one of the most selfless persons I have ever met. A negative by product of this was that he never found the time to publish his Cambridge dissertation on the multi-product firm, Barton (1961). I repeatedly nagged him to do so because it anticipated by many years ideas subsequently made well known by Oliver Williamson and others. When Allan died, Selwyn Cornish, Richard Holden and I prepared an obituary tribute to him for the *Economic Record*. Richard, a fine scholar with a detailed and deep knowledge and understanding of the issues of Allan’s dissertation, confirmed in our obituary how far ahead of his time Allan had been.

The *Economic Record* has a rule of thumb: 1000 words for obituaries of the greats, 500 for the also rans. The editorial board decided in their (lack of) collective wisdom that Allan, who held Chairs of Accounting at Macquarie and the ANU who had also been the most successful ever Treasurer of the ANU and who greatly influenced the structure of government accounts and methods used, fell in to the latter category, see Cornish, Harcourt and Holden (2013). The

unexpurgated version of our tribute will be published in one of the four volumes of selected essays<sup>18</sup>.

### **Conclusion**

So that is it, an account of a life time of much collaboration and deepening friendship combined. The great satisfactions of an academic life may never be able to be fitted comfortably, if at all, into the mainstream model of individual behaviour but as a source of life enrichment in this “veil of tears”, they are hard to beat. Many readers no doubt will detect a Polyanna gloss on what I have written. If so, too bad, for that is how I see it.

G.C. Harcourt

UNSW

March 2015

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<sup>18</sup> For completeness I should mention that I have written joint reviews, been interviewed for articles in journals, and I collaborated with Peter Kriesler and Craig Friedman on a chapter for Ed Nell's *Festschrift*, Forstater and Mongiovi (2014), and with Jan Toporowski on an article, “The lender of last resort and capital market stability” (2003). Craig, Peter and Jan were definitely the senior partners.

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