Australians in Cambridge in the 1950s: a Comment on William Coleman's Conversation with Murray Kemp

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I enjoyed William Coleman talking with Murray Kemp (and vice versa) in *HER* 41, Winter, 2005; but I was rather taken aback by Murray's too bleak a picture of Cambridge Ph.D.s (Oz-types) in the 1950s. May I try to put the record straight?

It is true that I had two supervisors. I had Nicky Kaldor for a term, Michaelmas Term 1955 (a disaster). Then he went on leave for a year and I had Ron Henderson from then on. Ron was most supportive and encouraging. He immediately sent me to London to work at the National Institute for Economic and Social Research (NIESR) on the soon-to-be-published combined accounts of all quoted public companies in the United Kingdom for the period 1949-53. I wrote two reports for the NIESR in 1956. The data I examined became the raw inputs into my dissertation, Harcourt (1959). It was, in effect, Mathews and Grant on inflation and company finance: Mathews and Grant (1956, 1957, 1958) for the United Kingdom with Joan Robinson's *Accumulation of Capital* (1956) thrown in. I was four years from go to whoa in the writing and then submission of my dissertation. For the last 20 months of the period, I was a full-time lecturer in Adelaide; certainly I 'took….years', as Murray says (Coleman, 2005, p. 7), but not, given the circumstances, an excessive period of time.

I also had two periods of bad research blues. One was due to getting off on a false start with Nicky.[1] The other was when my empirical calculations seemed to have 'blown up'; but Spring, Ron Henderson's common-sense support, together with Joan Harcourt's even greater support, saw me through.

As well as writing a dissertation, I took a term off in 1956 to work on Joan Robinson's *Accumulation of Capital*, which had just been published. I had been to her lectures on it in 1955 but had not made much of them, not least because she dropped her voice when she came to a critical point or argument. Keith Frearson and Tom Asimakopulos also went to the lectures. Keith was enraptured, Tom irritated and puzzled. It was not until he went to Bob Solow's lectures at MIT several years later that the pennies dropped. He saw what (and who) she was getting at. From then on, Tom became her devoted but not uncritical disciple. Reading her book helped me enormously and also started our friendship – she approved of the paper I wrote on the book and presented at the research students' seminar.[2]

The research students' seminar met on Thursdays in full term. It was presided over by Piero Sraffa, assisted by Robin Marris. Sraffa was a little aloof as he was still recovering from falling off a mountain and cracking his skull. Robin was very approachable. We all took turns to read papers on our on-going research. As well as Sraffa and Marris, Joan Robinson regularly attended, other teaching officers less so, usually to hear their own students. As well as this seminar, Malcolm Fisher ran a seminar on, for example, new theories of the consumption function, at which we took turns to read papers to start discussion. Joan Robinson also ran a seminar on various topics at which we also read papers. When my turn came she forgot to come; she rang me at the Marshall Library (then in Downing

Street) from her home in Grange Road to apologise and asked me to come to see her. My 'reward' and her 'penance' was then to walk with her from Grange Road back to Downing Street, a well-meaning but excruciating experience for me because Joan had no small talk and I soon ran out of things to say – you better believe this! Amartya Sen read a paper at Joan's seminar on Tinbergen's 1956 article in the *Economic Journal*, 'The Optimum Rate of Saving'. This subsequently became the basis for a note he published in the *Economic Journal* in 1957. This inspired me to attempt a similar thing – my first single-authored paper, 'The Quantitative Effect of Basing Company Taxation on Replacement Costs' (Harcourt 1958), was presented first at Piero Sraffa's seminar. Incidentally, Piero nearly scuppered me right at the start. I commenced by comparing depreciation at historical cost with capital consumption at replacement cost for UK companies for a number of years. Piero asked why anyone should want to compare them!

As well as seminars there were also a series of lectures for research students. Richard Kahn lectured on liquidity preference and the stock market, lectures based upon his 1954 *Manchester School* article (Kahn 1954; 1972). While the lectures were comprehensible, as we could all sit in the front row, his inability to write on a blackboard was astounding. Dick Goodwin gave a different set of lectures each year. I went to his absorbing lectures on planning in India (he had just returned from a visit to India).

I don't think it is true to say (Coleman 2005, p. 7) that the Australian contingent all regretted going to Cambridge; and it is certainly not true that none of us (except me!) got our Ph.D.s. It is true that supervision was very variable, with Brian Reddaway regarded as the greatest catch, and there was considerable wastage. Nevertheless, even those who ended up without a degree were glad they went – Keith Frearson always returned to Cambridge for leave and his teaching was enriched by what he learnt and read at Cambridge. Hugh Hudson won the Stevenson Prize (for the best essay by a graduate student with less than six terms residence), edited Nicky's first two volumes of *Collected Papers* (Kaldor 1960a, 1960b), and was regarded by Richard Kahn and Joan Robinson as the brightest of us all (he had stiff competition from Amartya Sen, Luigi Pasinetti, Pierangelo Garegnani, John Whitaker, Charles Feinstein and Tom Asimakopulos, for example). Ron Peters returned several times. John McCarty, Duncan Ironmonger and Allan Barton all got their Ph.D.s; Michael Schneider received an M.Sc. (but that is what he entered for). There were two sad cases, no names, no pack drill, with one not getting a degree, the other an M.Sc. Most of us returned to Cambridge on leave, often on several occasions. Wilfred Salter received his Ph.D. just after I arrived in September 1955. It became *Productivity and Technical Change* (Salter 1960) after he went to the USA and then to the ANU to work with Trevor Swan.

As for facilities, we mainly worked in a first-floor room of the Marshall Library then located, as I wrote, in Downing Street, conveniently close to a café/ restaurant at which we took tea, ate toasted raisin bread and talked. There were Marchand calculating machines for those of us who were empirically inclined. Provided you were on the right side of Donald Missen and James Clayton, the two librarians who worked with Piero Sraffa, the Marshall Librarian, you could get a key to enter the Marshall in the vacations. Donald and James did not approve – they thought we should be enriching our (lack of) culture by trips to Europe and that research should be reading substantial leather-bound tomes in full terms only. Just as Sir Dennis (Robertson) did.

So it was not all gloom and doom, and while things are much better-organised these days I am not sure that this constitutes that much of an improvement. We were most of the time a happy united band and, I submit, not too bad a vintage. So Murray should either stick to theory or get his facts right.

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Notes

1 It took about 10 years to get our relationship back on an even keel and we ended up friends. Just before he died in 1986 I received an appreciative note from him for my review, Harcourt (1986), of his Okun Lectures (Kaldor 1985), in the *Economic Journal* and I wrote his obituary for *Economica* (Harcourt 1988; 1993).

2 It was a very long paper and took two consecutive sessions to get through. Joan came to a third session to answer our questions – she thought we research students concentrated too much on technical nitty-gritty and missed the big picture. Alas, during one of my moves I lost the manuscript.

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