

40 Years Teaching Post Keynesian Themes in Adelaide and Cambridge

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I

Ray Petridis is a superb teacher and a notable Australian Post Keynesian economist. So I thought it appropriate, in a tribute to Ray, to write about how and what I have taught under the rubric of Post Keynesian economics from 1960 on, principally in Adelaide and Cambridge. Ray and I have been friends for about 30 years. I admire his integrity and high principle, his passion and enthusiasm, his thorough scholarship and clear, critical, analytical mind. That he played Australian Rules League Football in Western Australia for West Perth evokes awe and makes me feel even more honoured to be his friend.

II

When I came to Adelaide in 1958, I gave a few lectures to the fourth year Honours class on the themes in Joan Robinson's *Accumulation of Capital* (1956). Immediately prior to coming to Adelaide I had been in Cambridge as a research student and had attended Joan's lectures on what became *The Accumulation of Capital*. When the book was published in 1956, I locked myself up with it for a term, subsequently emerging to read a paper to the research students' seminar on what I thought it was about (see Harcourt 1995). That paper was the basis of my lectures to the Honours students. Afterwards I lost the paper in one of our moves so I have no idea now what was in it, nor what the class, which included Ron Hefford, Margaret Southwood (née Lawrence) and Deane Terrell made of it. But giving the lectures encouraged me to think about proposing a course on related issues.

My chance came in 1960. It was (and still is, I hope) the practice in Adelaide to have intending Honours students take in their third year an Interim Honours Course with the title of 'Economic Theory'. There was no set syllabus; the lecturers could do what they liked, as it were. For example, Eric Russell used to take the economic theory students through the arguments of Joan Robinson's *The Rate of Interest and Other Essays* (1952). When I was asked by Peter Karmel in 1960 to give some lectures for the course, I decided to use Nicky Kaldor's postwar writings on distribution and growth as a case study of how one distinguished economic theorist worked. Kaldor had been my PhD supervisor for a short time and, while we did not hit it off at all then, I found his writings extraordinarily stimulating and challenging. Since he was also very much an intuitive economist, who did not use high-powered techniques but who did have powerful and deep insights and ideas about important issues, his writings seemed to me an ideal case study for bright students in their third year.

The people in the class were indeed exceedingly bright – it included Dick Blandy, Gretel Dunstan, Jim Henderson and Neil Sarah. They were also ahead of their time – they staged a students' revolt against the lectures. Their ground was that they could not see the practical relevance of the papers we were reading, to my chagrin as I had taken great pains to try to absorb Kaldor's ideas and think up

snazzy ways of making them intelligible, mostly using diagrams to do so. Anyway I asked them to stick with it, which they very decently did. In class we thrashed out the ideas on the microeconomic foundations of his macroeconomic theories of distribution – (the results were ultimately published in my 1963 *Australian Economic Papers* article). After the event, the students said it had been a good learning experience. By having to go in depth into one person's ideas in an area of economic theory, they learnt a lot about how to theorise, how to identify questions and provide a framework in which to derive answers, as well as developing critical skills which, in this particular class, were already well on display! We concentrated mainly on 'Alternative theories of distribution' (Kaldor 1955-56), 'A model of economic growth' (Kaldor 1957) and the two papers on inflation and growth in *Economica* (Kaldor 1959a, 1959b), and emphasised the role of the distributive mechanism in the process of growth. As the Golden Age of Capitalism was then in full swing and fears of inflation emerging were never far away, a structure and framework (albeit imperfect and parts of which their author was subsequently to repudiate) with which to analyse such issues was in fact useful and relevant training.

III

In August 1963 I left for a year's study leave in Cambridge. As it turned out, I stayed until the end of 1966 (see Harcourt 1999). One of the things I especially wanted to do when I returned to Adelaide was to lecture to the economic theory students. While in Cambridge I had, with Vince Massaro, made a thorough study of Piero Sraffa's *Production of Commodities* (Sraffa 1960; see Harcourt and Massaro 1964a, 1964b). I also sat in on the debates between Joan Robinson, Ken Arrow and Bob Solow which occurred when Arrow and Solow were in Cambridge for the academic year 1963-64 (see Arrow 1999). Reading and writing about Sraffa's book (and discussing it with its author) took me back to my undergraduate interest in the classical political economists and Marx, as well as to my understanding of the critique of neoclassical theory by Joan Robinson in the 1950s in her 1953-54 paper and 1956 book, for example. The economic theory course which I designed was intended to bring these arguments to the notice of the intending honours students.

Just to make sure that they knew what was being criticised (!), after 1971 I started the course by taking them through Harry Johnson's *Two-Sector Model of General Equilibrium* (1971), the first chapter of which is a brilliant introduction to the neoclassical theory of value, production and distribution in a general equilibrium setting. As was to be expected of Harry he made ingenious use of geometric arguments (my teaching device was to build up, step by step, and diagram by diagram, to his diagrams, which were pretty off-putting at first sight). He also brought out clearly the intuitively agreeable conceptual nature of neoclassical economics – that prices in both the commodity and the factor markets were true indexes of scarcity. (He was very careful to put in the neoclassical exceptions of large 'perverse' income effects.) These lectures would, I hoped, give students a conceptual understanding of mainstream economic principles and allow them to see exactly *what* Sraffa's prelude to critique of economic theory was directed at.

The next move was to take them through Sraffa's Introduction to Volume I of the Ricardo volumes (Sraffa with Dobb 1951-55, 1973). I emphasised the argument that Ricardo had an (embodied) labour theory of value and that the search

for an invariable standard of value which occupied the last months of his life was related to the need, as Sraffa saw it, to reproduce, in a heterogeneous commodities model, the clarity of the nature of the distributive process associated with the corn model and the essay on the profits of the farmers and how they ruled the roost, which preceded the writing and publication of the *Principles* (1817). In particular, Ricardo was seen as wishing to be able to say precise things about the size of the national product and the surplus when either distribution and/or methods of production and levels of overall activity changed.

The discussion of Ricardo was followed by lectures on at least Part I of Sraffa's 1960 book (production with one commodity per industry for a single period, first for subsistence, then with a surplus). I emphasised the meaning of the Standard commodity and the reduction to dated labour sections. I related them to Ricardo's preoccupations and Marx's transformation problem, the point that Ronald Meek highlighted in his insightful review article of *Production of Commodities* (Meek 1961, 1967) and which Vince Massaro and I noted in our review article (Harcourt and Massaro 1964b).

IV

In the first years of the course, all this was very much through a glass darkly, because I had not then written the *JEL* survey of capital theory (Harcourt 1969). This occurred in the second half of 1968 when, in August, Mark Perlman spent a day with me in Adelaide, at Wilfred Prest's suggestion, 'hard selling' me into writing it! In the long vacation of 1969-70, I was in Japan at Keio University where I wrote the first draft of the book of the survey (Harcourt 1972). From then on, in Adelaide, my economic theory lectures were also a prelude to lectures based on the book (initially, the manuscript of the book) for fourth-year Honours students. One of the Honours options I took was 'Capital and Growth'; another was 'Radical Economics'. There was often an overlap of students between the two classes.

'Radical Economics' was run as a seminar course. We discussed, for example, Maurice Dobb's *Political Economy and Capitalism* (1937) and Milton Friedman's *Capitalism and Freedom* (1962). I told the students that they could not decide where they stood on how best to approach economic issues and, indeed, run societies until they had absorbed the arguments of these two classics. We also looked at Harry Braverman's book (1974) and Steve Marglin's 'What do bosses do?' (1974, 1975), then cult essays for the Left; and much else besides. These three courses made up a large part of my teaching load at Adelaide in the 1970s and 1980s (I also taught courses at Flinders on some of these issues) until I left for Cambridge in September 1982.¹

V

In Cambridge I was asked to design a course of twenty-four lectures to Prelims (second year) under the rubric of Post Keynesian economics. I taught this until I retired in September 1998 though by then the course was reduced to sixteen and ultimately to eight lectures. I also gave a course of eight lectures (ultimately four) from the late 1980s on, on 'Capital and growth theory in the Cambridge tradition' to the advanced economic theory course in Part II (third year). For a number of years I gave four lectures on Post Keynesian Themes to Subject 3 (Macroeconomics) of the M.Phil. And for the post-graduate students generally, for many years I ran a fortnightly workshop in Post Keynesian economics in Full

Term. Locals and visitors would read papers which loosely came under the P.K. rubric. The numbers of lectures in my undergraduate courses were reduced over the period because it was thought that the students were getting too many lectures overall and voluntary redundancies were called for. As a good team person I responded, only to find that most of the rest of the 'team' did not!

I would start the lectures on Post Keynesian economics by saying that a group of economists, mainly in Cambridge, who had been Keynes's pupils and/or colleagues and admirers, had become increasingly dissatisfied with the mainstream theories of value, distribution and growth, and with mainstream approaches to economic theory in general, especially in the postwar period. They developed a critique of that theory. They also developed alternative approaches to economic theory. I taught the course in a back-foremost manner by, first, presenting the alternative approaches and the theories contained within them. Then, if there were sufficient time, I would sketch the reasons for discontent with mainstream theories. This involved in particular presenting a thumb-nail sketch of the capital theory controversies at the level of my 1975 *Thames Paper*, 'Much ado about something' (see Harcourt 1975, 1982). I concentrated on the significance of capital-reversing and reswitching results but I also took in the methodological critique associated especially with Joan Robinson, that differences were used, improperly, to analyse changes.

Because I am interested in both historical perspective and personalities, I started the lectures with brief biographies of Michal Kalecki, Nicky Kaldor, Joan Robinson and Piero Sraffa. (Had there been time I would have added Dick Goodwin, Richard Kahn and Luigi Pasinetti.) Then I plunged into macro theories of distribution, starting with Kaldor's famous 1955-56 paper. I did this because it is best known; but it is also idiosyncratic as it assumes full employment and is a long-period analysis. I would go through the algebra but, mainly, I used a simple diagram showing \bar{I}/Y_f as autonomous and S/Y_f as an increasing function of Π/Y_f , where \bar{I} = (autonomous) investment, Π = profits, S = saving and Y_f = long-period full employment income. (It was the late Hugh Hudson who suggested this diagram to me.) I listed Kaldor's two empirical observations:

- 1) $s_{\Pi} > s_w$, where s_{Π} is the marginal propensity to save out of profits, and s_w is the marginal propensity to save out of wages.
- 2) The greater flexibility of prices *vis-à-vis* money-wages in the long term.

I showed how a stable equilibrium value of Π/Y_f , which provided the saving to match the given value of \bar{I}/Y_f ,² was established by the response of the economy to initial situations of either excess demand or supply in the goods market.

Then I would ask: why full employment, why long period? This allowed me to discuss the prior contributions of Kalecki in which he established *simultaneously* a *short-period* theory of employment and the distribution of income, using many of the ingredients that Kaldor had, but not confining himself to either full employment or the long period. I used Joan Robinson's excellent diagram of Kalecki's theory (see Robinson 1977), because it enabled the degree-of-monopoly mark-up theory to be shown explicitly, introduced the concept of the reverse L-shaped cost curve and neatly illustrated Kalecki's aphorism (not to be found in this form in any of his writings) that wage-earners spend what they earn and profit-receivers receive what they spend.

Using the ingenious device of a rectangular hyperbola, Joan had shown how spending from wage-earners in the investment goods trades provided total profits and profits per unit (the size of which depended on the prices set) in the

consumption goods trades. I would first ‘do’ the competitive model of price-setting and then the degree-of-monopoly mark-up version. The former showed existing capacity in the consumption goods trades to be fully taken up but did not guarantee full employment of labour – that depended in addition on whether or not the autonomous level of investment expenditure was ‘high’ or ‘low’. With degree-of-monopoly pricing, unemployment of labour and the capital stock (in both sectors) emerged as a (highly likely) possibility.

Following this I would lecture on Kalecki’s 1936 review (in Polish) of Keynes’s *General Theory*, referring my (non-Polish) listeners to the English translation of the review which was published in the December 1982 issue of *Australian Economic Papers* (see Targetti and Kinda-Hass 1982). Starting from a firm in either a competitive or imperfectly competitive setting, Kalecki built up to the level of the economy as a whole by setting out his theory of employment-creation and distribution – an extraordinary *tour de force*, which proved beyond doubt that he *had* independently discovered the principal propositions of *The General Theory*. Indeed he had, in some ways, done them better for he had a more explicit link between individual price-setting and macroeconomic outcomes, and he exploited Marx’s schemas of reproduction in a straightforward, easily understood manner, to bring out the principle of effective demand.

I also took advantage of Joan Robinson’s diagram to discuss how a democratic socialist economy, having decided on the level of accumulation, could set a turnover tax on consumption goods to make sure that the remaining labour force was fully employed in making consumption goods. The value of the volume of these consumption goods equalled the money income expenditure of the entire workforce (assuming $s_w = 0$). Or, if Kalecki’s humane view prevailed that socialist workers should have more jam today rather than have to wait for a tomorrow which never came, accumulation would be the residual after the desired level of production of consumption goods for the fully employed workforce had been decided. It was also possible to show the significance for any sort of society of the surplus of consumption goods per worker produced in the consumption goods trades over income per worker there for employment of workers in the investment goods trades – the non-available goods sector, as Keynes called it in the *Treatise on Money* (Keynes 1930).

VI

There were two natural steps to take after this: to discover what determines the size of the mark-up in Post Keynesian theory (and the real world) and what determines the rate of accumulation (starting from Keynes’s, in some details unsatisfactory, account in chapters 11 and 12 of *The General Theory*). I illustrated the mark-up theory with Adrian Wood’s model (1975). I pointed out that it was *explicitly* Golden Age analysis in logical time – expectations were realised – and microeconomic in scope. It showed how the determination of the rate of investment and its finance were simultaneously solved by setting a mark-up which provided enough finance (internal and external) for a rate of investment which maximised the rate of growth of the sales revenue of an oligopolistic price-leader firm in Kaldor’s archetypal oligopolistic industry.

I mentioned Jim Ball (1964) and Alfred Eichner (1976) but preferred to use, if I had the time, the model of Harcourt and Kenyon (1976) to redo the analysis in historical time. To explain why firms prefer to use retained profits to finance

investment, if they possibly could, I went through the arguments of Kalecki's principle of increasing risk (Kalecki 1971, chapter 9), which dates from 1937. In the lectures, I mentioned Joe Stiglitz's rediscovery of Kalecki's idea (initially, Joe did not realise it was a rediscovery) in his work on credit rationing in the 1980s.

I also had a digression on investment-decision rules and the choice of techniques, for it was implied in Wood's analysis that the choice of technique was independent of the investment-decision rule used. The digression gave me the chance to illustrate how starting from real world observations in order to build a model, instead of from a simple axiom, made a significant difference to the results obtained. I compared the outcomes of using three different investment-decision rules – two DCF procedures and the 'real world' pay-off period criterion. I showed that with the conditions postulated, which included orders of magnitude likely to be met in practice, the pay-off period criterion resulted in a more investment-intensive, less labour-intensive technique being chosen than either of the DCF procedures I considered. The analysis was done principally by using a simple figure which contained a 45° line and some concave-to-the-origin curves in expected receipts, investment per head space (as we say now) (see Harcourt 1972, p. 64).

The object of the lectures was to construct an approach to distribution and accumulation in modern capitalism. So far, we had covered distribution and the connection between oligopolistic pricing, the finance of investment and the investment decision itself at a microeconomic level. The next step was to analyse economy-wide accumulation. I started with a revision of Keynes's theory and the critique of it by, first, Abba Lerner (1944) and then by Kalecki (1936, 1982), Joan Robinson (1965) and Tom Asimakopulos (1971). Lerner repatriated Keynes's theory within its own framework by distinguishing between the marginal efficiency of investment (*mei*) and the marginal efficiency of capital (*mec*). In effect, he restated Keynes's theory as two propositions:

- 1) In full stock-flow equilibrium, $mec = mei = r$, where r = the rate of interest.
- 2) In short-period flow equilibrium, $mec = r$, both $< mec$.

Lerner's conclusions depended on there being a downward sloping relationship between the *mec* and the stock of capital goods, and the *mei* and the current production of capital goods, for the reasons which Keynes gave. These were rising marginal costs of production of capital goods in the short period and lower expected cash flows in the future, the higher the rate of investment now. Kalecki, Joan Robinson and Asimakopulos all showed that these reasons will not do unless, for the first, rational expectations in effect prevail so that individual competitors when planning investment use in their calculations, *not* the known current market price of capital goods but, rather, the ultimate equilibrium price (at which $mec = r$). For the second, it was argued that Keynes had ignored his own insight that, in an uncertain environment, expectations of the future depend excessively on the present. For he allowed short-period supply curves in the future to move to the right as more investment was contemplated now, but future demand curves for the products to be produced by the supply curves were left unaffected by higher levels of investment now. If it were not so, it was not inevitable that the values of the *mei* would be lower, the greater were the levels of production of investment goods now.

Kalecki, Joan Robinson and Asimakopulos overcame these limitations by postulating the two-sided relationship between profitability and accumulation, resulting, in Joan Robinson's case, in her famous banana diagram (see Joan Robinson 1962a, p. 48). On the one hand, the current (rate of) accumulation determines the current (rate of) profits through the macroeconomic distributive

mechanism; on the other hand, higher expected (rates of) profits in given situations, especially with regard to finance, induce higher planned (rates of) accumulation. At the (top) point of intersection of the two curves, what happens and what was expected to happen coincide – this is Joan Robinson’s version of Harrod’s warranted rate of growth, g_w . Under stringent conditions, an iteration procedure could be shown to take the economy to this point.

But it is not an optimum point for anyone except accumulators. Depending on the strength of the ‘animal spirits’ which determine the shape and position of the second curve, the point of intersection may be associated with unemployed labour or inflationary situations in the economy as a whole. In any event, the stability of the two curves is most unlikely to occur as we proceed from one short period to another. So the theory predicts that accumulation and income tend to fluctuate from period to period, exactly what has been witnessed over the period of time for which capitalism has been the dominant mode of production: a natural corollary of Trevor Swan’s uniquely appropriate remark that investment depends on those ‘animal spirits’ which cannot be bottled.

VIII

Now the apparatus was set up, I used it for an analysis of historical episodes and to suggest package deals of policies for specific situations. To do the first task required an introduction to theories of conflict inflation, which then allowed me to suggest reasons for the Golden Age of capitalism (or the Long Boom, as the Marxists called it) in the post-war period, the stagflation era, and the present era of low(er) inflation, often slow growth and high and sustained unemployment in much of the advanced industrialised world. Though Bob Rowthorn (1977, 1980) was the innovator, I used Steve Marglin’s parallel analysis in the 1980s (Marglin 1984), because his basic model fitted neatly with the tools I had developed in preceding lectures. As in Don Harris’s writings (1978), he made Marx’s analysis of the sphere of production explicit in order to determine the size of the potential surplus available for profits and accumulation at any moment of time. Whether the potential was realised or not depended upon the strength of the Kalecki/Keynes elements in the sphere of distribution and exchange.

A limitation of Marglin’s model was the assumption of Say’s Law in the latter sphere so that what was potentially there was in fact realised – or at least, if not, not because of a lack of effective demand. Marglin’s justification was akin to Kaldor’s, that he was interested in medium- to long-term growth and distribution and in explaining trends, not fluctuations. This, of course, was not acceptable to many Post Keynesians, not least Kalecki, Joan Robinson, Goodwin and Asimakopulos and also the later Kaldor, who came to see the trend and cycle as indissolubly mixed. Marglin’s methodology of regarding short-period situations as stations on the way to the long-period cross was not acceptable to them either, differences I made explicit in the lectures.

Be that as it may, the essence of the story was a conflict between the desired accumulation of the capitalists and the desired standard of living of the wage-earners. If, taken together, they made impossible demands on what was available, *constant* rates of inflation served to bring about an uneasy truce between the two groups. They implied for both classes unrealised expectations, which, however, did not worsen. By making plausible assumptions about the curve-shifting factors at work in the three historical episodes considered, Marglin’s

apparatus produced outcomes which matched the broad characteristics of the episodes. It certainly gave me a buzz to show this; I cannot, of course, speak for my listeners.

IX

Now on to real political economy! First, I lectured on Kalecki's classic, 'Political aspects of full employment' (1943; 1971, chapter 12). In the paper he sets out the essential ingredients of the political business cycle and the reasons for which Tommy Balogh (1982) was afterwards to call Monetarism 'the incomes policy of Karl Marx'. By this was meant, of course, the need to recreate the reserve army of labour in order to cow wage-earners, make the sack an effective weapon, and increase the size of the potential surplus for profits and accumulation. Kalecki emphasised the crucial distinction between the political economy of *getting* to full employment after a deep prolonged slump, on the one hand, and the political economy of *sustaining* full employment, on the other – a distinction with which we have become only too familiar in recent decades. The unsavoury role of some of the profession as hired prize-fighters is also uncompromisingly mentioned by Kalecki – 'a powerful block is likely to be formed between big business and *rentier* interests and they would probably find more than one economist to declare that the situation is manifestly unsound' (1942; 1971, p. 144). Such straight talking was, I hoped, a welcome change from the manipulation of squiggles which increasingly dominates the teaching of modern economics.

Finally, as I mentioned above, I pointed out how many Post Keynesians, especially Kalecki, Kaldor and Joan Robinson, had increasingly moved away from mainstream equilibrium question-begging – always seeing economic issues in terms of existence and stability – to using instead cumulative causation models. I quoted Kalecki's famous dictum: 'The long-run trend is but a slowly changing component of a chain of short-period situations; it has no independent entity' (1968; 1971, p. 165); also Joan Robinson's: 'The short period is here and now, with concrete stocks of the means of production in existence. Incompatibilities in the situation ... will determine what happens next. Long-period equilibrium is not at some date in the future; it is an imaginary state of affairs in which there are no incompatibilities in the existing situation, here and now' (Robinson 1962b, p. 690). I illuminated the difference between the two approaches with a wolf pack analogy (see Harcourt 1992, p. 4). Of course, I also pointed out the convergence of alternative approaches in recent years with the introduction of hysteresis and the use of path-dependent and lock-in processes generally in mainstream economics. But as I also wished to give credit where credit was due, I mentioned as pioneers Allyn Young (1928), Kaldor (1934!), Joan Robinson in the 1950s, Goodwin in the 1940s (and Adam Smith in 1776). In my last years of lecturing I gathered all these threads together in themes I initially developed in my 1992 Horne Address, 'Markets, Madness and a Middle Way', 'The Harcourt plan to 'save' the world' (1993a) and the policy proposals that subsequently grew out of this (see Harcourt 1993b, 1994, 1995, 1997a). This allowed me to bring some of my great loves and influences together: Kalecki, Wilfred Salter, Eric Russell, Kaldor, Joan Robinson and, of course, Keynes. I tried to show how these packages depended for their effectiveness on which view of the world dominated the vision of the policy makers concerned. I hope I went out with a bang, but that is not for me to say.

X

My Post Keynesian lectures were complemented by lectures on growth theory to Prelims. I started with Adam Smith and Ricardo, using Luigi Pasinetti's model of the Ricardian system (Pasinetti 1960). I went through Marx – what I had to say may be found in Harcourt and Kerr (1996) – and on to Harrod. I turned Sen's algebra in Sen (1970) into a diagram which exploited the familiar Keynesian cross. I then did Solow/Swan (1956), concentrating on Swan for patriotic reasons and because I think his diagrams are quite brilliant for illustrating the issues and their solutions, especially the neoclassical solution to the Harrod problem of g_w not equalling g_n . I contrasted this with the Post Keynesian solution whereby it is s and not v (or c) which is induced to change by economic incentives. I went through Solow (1957) because of its seminal importance in the literature and also to pave the way to the closing lectures on endogenous growth theory. In these I relied on the superb exposition in that answer to a teacher's prayer, Jones (1998), to give the essence of the 'new' developments. I put 'new' in quotes because I finished up with Heinz Kurz's brilliant critique (1997) concerning what Adam Smith and David Ricardo could have learnt from the 'new' growth theory.

XI

When Robin Matthews appraised me in the early 1990s and I told him what I lectured on, he said there was a curiously old-fashioned stamp to it all. He was, of course, correct but, nevertheless, I made (and make) no apology for this. I have been trying to pass on to coming generations what I had found of great value in my own development and understanding of economic processes and the design of policy. I tried to share what I thought was of lasting value rather than peripheral or faddish. If I were to start again today, I would include more on the later Kaldor, drawing particularly on his collected papers and his Mattioli lectures (Kaldor 1996; see also Harcourt 1997b).

An anonymous referee of a draft of the article (I reckon I know who the referee is, you have been warned) took me to task on a number of related fronts. First, the referee was 'surprised' that the material in the lectures was 'not that much different from' what Peter Groenewegen taught at the University of Sydney and 'what a ... student would have learned' from the Post Keynesian course at UNSW. Why "surprised"? Has the referee thought about how causation may have run?! I made explicit in the article that I wanted to concentrate on fundamentals, not fads.³ Remember, in the 1980s I was lecturing to the second ('Prelims') year at Cambridge. The object of this year is to help students to create structures of thought about economic issues to use in their third (and final) year to tackle applied and policy questions. I was introducing them to what I think is a peculiarly Cambridge tradition in which I had been brought up and in which I passionately believed. There were many opportunities for them to go to lectures on mainstream approaches, and though I commented in passing when there were overlaps, to 'compare and contrast' was not the principal object of my set of lectures – hence, no doubt, 'the impression that [I gave the] lectures in splendid isolation from broader developments in the profession'. (No doubt also that the practitioners of the latter always meticulously told their hearers what the PKs had to say on these matters.) After all, there is only so much that can be covered in 24-16-8 lectures. So my first reaction to the comment was to wonder how quickly the referee spoke in

lectures and what account was taken of the absorption effect in classes – perhaps unfair but not completely beside the point.

The referee also asked about contact amongst Australian Post Keynesians when I was at Adelaide. It was spasmodic at best. Melanie Beresford, Bob Dixon, Keith Frearson, Peter Groenewegen, Peter Kenyon, Prue Kerr, John King, Peter Kriesler, Bruce McFarlane, Allen Oakley, Ray Petridis, Ron Peters, Trevor Stegman and Mike White, for example, were all close friends of mine and I learned much from them. But there was no systematic exchange of contents of courses, as opposed to reading one another's papers.

What, it is asked, 'was it like to teach Post Keynesianism in a context of that body of thought's marginalisation?' Well, it could be, no it was, disheartening. At Adelaide, especially after Eric Russell died in 1977, I was rather isolated intellectually, except for a few splendid Honours and research students over the years. (At a personal level, I believe I was completely integrated in a happy team of close friends.) At Cambridge the intellectual atmosphere in the Faculty became increasingly hostile, but there was the great compensation of having the substantial group of people around the *Cambridge Journal of Economics* who in their own various ways carried on and expanded the Cambridge traditions. As ever, there were wonderful undergraduates and research students (both MPhils and PhDs – I always had 8-11 PhD students 'on the go' at any moment of time). Nevertheless, both in teaching and appointments in the Faculty this strand became increasingly marginalised as it was, of course, in the profession generally.

During my time at Cambridge in the 1980s and 1990s I had a number of public debates with Frank Hahn, usually before the undergraduate Marshall Society, over the issues associated with different approaches to economics. The first was in the early 1980s and the room, a large one, was packed out. I was perceived to have had the better of the exchanges (there was a large Italian contingent present!). By the last exchange, though, the numbers had fallen considerably. Moreover, our arguments had not changed *that* much, but there was now much more sympathy among the students for Hahn's views. I often clashed with Hahn in the Faculty coffee room. He has the makings of a splendid intellectual bully and he was always surrounded by acolytes, to which admiring crowd he could play shamelessly. Once he portrayed me falsely as a neo-Ricardian (I was actually attacking the way Hahn was caricaturing Pierangelo Garegnani's views, not defending the views as such) and he was delighted when in the end I lost my cool and became heated in my replies (when I could get a word in edgewise). He said something to the effect: 'Look at his red face and hear his incoherent utterings, he is mad like all neo-Ricardians'. As at much the same time Terry O'Shaughnessy and I were having vigorous debates with John Eatwell and Murray Milgate concerning the neo-Ricardian long-period interpretation of Keynes, this was a bit rich.

Attendance at my Cambridge lectures went down over the years, partly because of the changing characteristics of the population of students, partly because questions on the examination papers which related to my lectures became fewer and fewer. Students were also more and more advised by their Directors of Studies not to worry about going to my lectures, or so I was told. I would like to think that I was as clear and as enthusiastic a lecturer at the end of my time as I was when I started, so that it was changes in the values of other variables which were responsible. But perhaps I kid myself. When I retired in September 1998, virtually all of my courses were not continued. As to how other academics and students

reacted to what I had to say, I believe, immodestly, that the many seminars and invited lectures I gave over the years at other universities in the UK, USA, Canada, Australia and elsewhere continued to be well, even enthusiastically, received.

Finally, it is suggested that I ignored the deep divisions in the Post Keynesian camp and did not discuss how compatible the various strands were or were not. First, I must protest that even before I left Australia I had written a paper on the sources of divisions within Post Keynesianism when trying to set out what Post Keynesianism was, historically and analytically (see Harcourt 1982b, a paper which was originally written for a seminar at the Reserve Bank of Australia).⁴ I also made asides on these matters in the lectures, but they were not central to the principal aim of the courses as such. I have written extensively on these and other related issues and the students were referred in the reading lists to the relevant books and papers (see, for typical examples, Hamouda and Harcourt 1988; Harcourt 1982, 1996, 1996-97; Harcourt and O'Shaughnessy 1985). But I did not have time in the lectures to go into great detail on the issues. In any event, as I wrote above, to have done so could have frustrated my aim of presenting an analytical structure within which to think about how to tackle important issues of political economy.

So, to conclude: I returned to Cambridge in 1982 because I wanted to document what I thought was lasting in this particular Cambridge tradition and to teach within it. In that task, which I still regard as vital, Ray has joined me with his approach and influence in Australia. I feel honoured to be able to pay homage to such a splendid co-worker and good friend.

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Notes

- 1 I taught the large first-, second- and third-year macroeconomics courses for much of the time as well.
- 2 I would mention that the value of \bar{I}/Y_f was determined by the accumulation requirements of g_n , Harrod's natural rate of growth, and that this would be covered in a companion set of lectures on growth and cycles.
- 3 I have become increasingly disturbed at how the contributions of the founders and the first generation of Post Keynesians are being written out of the script by many of its current adherents.
- 4 I also gave the paper to a seminar at Monash. I asked someone afterwards how it went, only to be told that I must realise that half of the people present thought I was a sociologist.

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