What *were* 'the children' doing? Joan Robinson, Kahn and Kaldor and the 'Keynesian Revolution' through the eyes of Ambrosi

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Preliminary: not to be quoted

It is taken for granted that Joan Robinson, Richard Kahn and Nicholas Kaldor were the main standard-bearers of the revolution in economic thinking that Keynes thought he would bring about by means of *The General Theory*. I call them 'the children', after their own name for themselves in recognition of their junior status in comparison to Keynes.[1] Two recent books, Harcourt (2006b) and Pasinetti (2007), review their work entirely from the point of view of their foundational role in the development of Keynes's theory. Neither policy nor methodology have a substantial role in their accounts. By contrast, Tily (2007, Ch. 4), concentrates on what happened to theory and policy in the hands of Keynes's successors. He accuses the usual suspects of subverting Keynes's theory – Hawtrey, Hicks, Robertson, the Oxford Studies group – and also views the role of at least one of 'the children' as suspect. In his account, Kahn supported Keynes's monetary policy, but Joan Robinson, while supporting Keynes's monetary theory ('The rate of interest', 1951), did so a bit late. Thereafter she did not pursue that course 'with any vigour and was content to align herself with Kalecki's basically "Keynesian" policy' (Tily 2007: 286). The general impression is that, for some undeclared reason, 'the children' went along with Robbins, Beveridge *et al.*, playing down Keynes's emphasis on monetary policy and playing up fiscal policy.

Policy *ought* to follow from theory. As we know, many economists contemporary with Keynes proposed public works as a remedy for the unemployment which was such a disgrace of the 1920s and 30s.[2] But as Keynes remarked, the remedy did not – could not – follow from the 'classical' theory most of them espoused. An important motivation for writing the *General Theory* was to provide the theoretical foundation for this policy, to make theory and policy coherent and to provide the justification for policies to alleviate unemployment. This applies both to public works and Keynes's monetary policy prescription: cheap money. The latter suffered a complete eclipse in 'Keynesianism', as Tily documents.

But it is Michael Ambrosi's *Keynes, Pigou and the Cambridge Keynesians* (2003), a book published five years ago but not mentioned to me by anyone but Geoff Harcourt,[3] that really challenges the position of these three supposed followers of Keynes. He casts an entirely new, and remarkably unflattering,[4] light on the theory

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[1] They referred to the journal they started (*The Review of Economic Studies*) as 'the children's magazine' (*CW* XIII: 326).

[2] see, e.g., the letters to *The Times* 17 October 1932 (*CW* XXI: 138-9 and June 1932 (XXI: 125-6).

[3] See his review in *Economica* (Harcourt 2006a).

[4] Harcourt's response to this is to 'wonder if he had an unpleasant time' during his year in Cambridge in the 1970s (*ibid.*, p. 360)..

and, uniquely, the methodology of ‘the children’. His conclusions on this matter, though they are not the central concern of the book, are too important not to be given an airing, which is the purpose of this paper. My motivation is not iconoclasm, but to encourage yet again a careful reconsideration of the methodological foundations on which the *General Theory* was based and how difficult it seems to have been for this foundation to be accepted even by those we think of as his closest disciples. Ambrosi’s assessment is damning:

> The fact is, however, that from the ‘inner circle’ of Keynes’ putative disciples at Cambridge virtually nothing was published of which it could be said that it contributed towards optimising the expression of Keynes’ original ideas (GMA: 247).

Out main purpose here is to follow Ambrosi in bringing out the methodological foundations of Keynes’s theory and its subversion by his supposed followers.[5] Although ‘the children’ were not the only actors in this little drama, their role is the more important for their position in Post-Keynesian thinking as torch-bearers of the Keynesian Revolution. The story unfolds through Keynes’s controversy with Pigou, the central point being revealed in connection with Pigou’s EJ paper of 1937.

Pigou 1933 and the *General Theory*

There is no doubt that A. C. Pigou plays a central role in the evolution of Keynes’ thinking. (Was it Mark Blaug who said ‘No Pigou, no Keynes’?) Keynes could hardly ignore him: he was, after all, the Professor of Political Economy in Cambridge and a prolific writer of economic theory. As everyone knows, Keynes took him as the representative of ‘classical economics’, and a substantial section of the *General Theory* is directed against his work. It is now conventional to perceive Keynes’ treatment of Pigou in the *General Theory* as an unprovoked attack, motivated by some spite or animus on Keynes’s part. Lawlor’s evaluation, though using stronger language than usual, is not untypical: ‘…Keynes singled “the Professor” out for crucifixion in his appendix to Chapter 19… [H]is …*Theory of Unemployment* (1933)… so irked Keynes that he felt compelled to devote a whole appendix to its excoriation…(Lawlor 2007: 68). At the very least Keynes is seen as setting up Pigou as a straw man and treating him unjustly. The story looks very different through the eyes of Ambrosi, who devotes his Ch. 5 to the issue of Keynes’s fairness or otherwise toward Pigou.

Keynes took Pigou’s work extremely seriously; it was the Cambridge tradition to take colleagues’ work seriously, and he had discussions about the new book with Beveridge, Hawtrey, Robertson and Shove (XIII: 310-17, XXIX: 27-34) –

[5] There are other interpretations of the Pigou (1937) episode, notably Aslanbeigui and Oakes (AO) (2007) and Moggridge (2006). They concentrate on Keynes’s use of his position as editor of the *EJ* to protect his intellectual capital. AO comment (p. 17, continuation of n. 1): ‘Unaccountably, Ambrosi ignores the treasures of the [Modern] Archives [, King’s College Cambridge], which are indispensable to the account he claims to offer. Instead, his book employs selected published sources. Because it neglects archival evidence of authorial objectives, strategies and tactics…the book is marred by interpretative guess work, unfounded speculations. and factual errors.’ No examples of these failings are given in their paper, which itself does not seem to rely heavily on unpublished material.

significantly, not with Pigou.[6] Keynes did not review it.[7] This correspondence showed no particular sign of the irritation which so many commentators have found in the *General Theory*. Although Keynes did think it 'simply nonsense from beginning to end' (XIII: 310), this is an intellectual assessment, not an emotional reaction. Later he was to write to Kahn that 'The stuff he writes…has a dreadful fascination for me, and I cannot leave it alone' (letter to Kahn 26 March 1935, XIII: 525). Pigou (1931) reviewed the *Treatise on Money* critically but their correspondence remained amicable.

While Keynes was also a 'classic', there was no obvious source of friction between them, and they had always got on well (though there is evidence of an earlier difference on methodology, which I shall bring up in a footnote later). But by 1933, Keynes had made significant progress toward the *General Theory*. His first two lectures of the Michaelmas Term, 1933, were in effect a critique which nearly had the shape of GT Ch 2, and put the case against what Schumpeter called Real Analysis in a discussion of the distinction between a neutral money economy and a monetary economy (Rymes, 1998: 47-58). The *Theory of Unemployment* was an example of just such Real Analysis.

Ambrosi credits Peter Clarke (1988) with giving him 'a glimpse at an academic scene where there was considerable "teasing" and sublime provocation going on between Keynes and Pigou' (GMA: 29). For example, Pigou's view, expressed in his Preface,

> that economists who had concentrated on monetary explanations of depression had tended 'to overstress somewhat the role that money plays in more normal times' (Clarke: 1988: 273)

would be understood as a dig at Keynes, who had concentrated on money from the very beginning, and whose current lectures were entitled 'The monetary theory of production'. Pigou by his approach (from the 'real end') and in his foreword 'challenged all those who entered this topic from the "monetary end" (GMA: 29). When Pigou brings out *The Theory of Unemployment,* Keynes upstages him (Clarke's words, *ibid.*) by changing his title to *The General Theory of Employment.*

'But the preface of Pigou's *Theory of Unemployment* went further than just teasing…' (GMA: 29). Pigou wrote:

> While it is natural and right in the present deplorable state of the world's affairs that many economists should seek to play a part in guiding conduct, that is not their primary business. They are physiologists not clinical practitioners; engineers, not engine-drivers. (Pigou 1933: v)

While Keynes had spent many years in public affairs, Pigou considered these activities 'undignified for a "real" economist like himself';[8] this is a 'demeaning

[6] Judging by the *CW* and by the fact that no correspondence on the book in mentioned in Bridel and Ingrao.
[7] I found only two reviews, by Harrod and by Sweezy.
[8] Ambrosi points out that Pigou did not always practise what he preached: he was a appointed a member of the Economic Advisory Council in 1930 at the suggestion of Keynes and served under his chairmanship (GMA: 31)

comment' (GMA: 30) on Keynes, 'returned like a prodigal son from the flesh-pots of Whitehall' (Clarke: 27). Ambrosi dug deeper. Eugene O'Neill had published a play called 'The Hairy Ape' in 1922; it won the Pulitzer Prize for Drama in that year. it was performed in the Cambridge Festival Theatre on 26 November 1928, to critical acclaim in the *Cambridge Review.* If anyone had forgotten the sensation it caused, their memories would have been jogged by a special report by Alistair Cooke on the Theatre's first ten seasons in *Theatre Arts Monthly*, November 1931.[9] Its coal-shovelling engine driver Yank had ambitions:

> *Paddy* [a co-worker]: Is it a flesh and blood wheel of the engine you'd be?
> *Yank*: Sure ting! Dat's me!...I'm de end! I'm de start! I start somep'n and de woild moves! It – dat's me! – de new dat's molderin' de old! I'm de ting in coal dat makes it boin; I'm steam and oil for de engines.

In addition, Ambrosi tells us, O'Neill directs Yank to take the pose of Rodin's 'Thinker' repeatedly. Pigou's reference to engine drivers is a barb directed at Keynes and others 'who pose as "thinkers" …preposterously believing [themselves] to be moving "de woild"' (GMA: 31).
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Thus Ambrosi reveals the exceptional degree of aggression hidden in Pigou's words – hidden, that is, except from the very person meant to be insulted and his immediate associates.[10]

So the attention given Pigou in the *General Theory* can now be seen partly as a response to an aggression rather than an aggression initiated by Keynes. One could argue that Keynes should have answered Pigou in articles and not allowed his dispute with him to distort the shape of the *General Theory* (and attract the opprobrium of later generations not in the know), but an answer was imperative. One must also take into account Keynes's frustration – especially with Pigou and Robertson – that, to his mind, 'classical' theory was not fully spelled out, so that he was in the position of not always dealing with something very concrete [refs to CW].

Pigou's review of the *General Theory*

Pigou's review (1936) of the *General Theory* is astonishingly rude. Pigou was obviously furious at Keynes's lumping together of 'classical economists' including himself and tarring them all with the same brush. He felt that many of the accusations against himself were quite unfounded. And he is at the head of a long queue of commentators who found the exposition obscure:

> How is it that an author, whose powers of exposition enabled him to write on the philosophy of Probability in a way that amateurs could follow – not to say one whose vividness of phrase has made him a valued contributor to the *Daily*

[9] These details go beyond those provided by Ambrosi. They come from a later source (Cornwell 2005). It is odd that the play was only put on for one night.

[10] How on earth did Ambrosi connect 'engine-driver' with O'Neill's play! Is it a legitimate connection? Is there any correspondence about it in Keynes circle? I would love to hear the story behind this piece of research.

> *Mail*, when he comes to the subject to which he has devoted most attention, is barely intelligible to many – for I am not alone in this – of his professional colleagues? (Pigou 1936: 119)

Ambrosi comments that, subsequently, Pigou made rather a habit of claiming he could not understand Keynes (GMA: 33); the clear inference was that the fault was Keynes's, as an author.[11] The *Daily Mail* jibe is by now also familiar territory. In a footnote he refers to it as a poisoned dart (but he confesses that he himself had contributed to the *Sunday Express*).

He finds a raft of inconsistencies of expression and definition, but most interesting to us (the reason will soon emerge) is this:

> [T[hroughout the main part of his book he supposes that some new investment is being undertaken every year. It is evident that, if this is happening, capital equipment cannot be [as he assumes] unchanged. He is assuming a stationary state and at the same time a moving one.[12] …. Nobody could make use of mutually inconsistent hypotheses in this way if he had achieved complete coherence among his ideas. The lack of clarity in Mr. Keynes' explanation is *mainly* due, I suggest, to a lack of clarity in his thought… (*ibid.*: 122)[13]

Keynes did not reply to this. He did not reply either, Ambrosi notes (p. 170), when Robertson repeats the point (*CW* XIV: 99, n. 2).[14]

Pigou (1937)

Pigou wrote 'Real and Money Wage Rates in Relation to Unemployment' in lieu of a Presidential Address to the Royal Economic Society, and its purpose appears to be to counteract the analysis given in Keynes's Appendix to (*GT*) Chapter 19, though Kahn thinks it is directed at Joan Robinson (Kahn to Keynes 19 December, XIV:266) Pigou adopts Keynes's assumption of a short period but, wishing to avoid the inconsistency' which had disturbed him so much in the *General Theory*, constructed a short period in which there was no investment. *Inter alia*, 'land and fixed capital (i) last for ever, so that no element of depreciation enters into prime cost; and (ii) consist of things of which it is impossible to make any more…' (Pigou 1937: 406). These assumptions support, more strongly than before, the same framework that Pigou has used in 1933.

Keynes saw the paper only after it had been paged for the *Economic Journal*; he was recovering from a heart attack at Ruthin Castle, and Dennis Robertson had recommended accepting the paper. As a sort of Presidential Address it would not be

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[11] This ploy was taken up by many others. See Chick (2007) for some examples.

[12] This criticism was later to be repeated by many others. The conventional settlement was that the changes were small in comparison with the stocks, though for Keynes the important distinction was that investment represents demand long before it comes on stream and affects supply. The first element belongs to the short period, the second to the long period.

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[14] Ambrosi finds this lapse astonishing. And there is more to come. But Keynes was most remarkable in the unevenness with which he responded to criticism.

refereed. But when Keynes saw it he thought it the work of a sick man (Pigou also had heart trouble) and that the paper, if published, would be an embarrassment to its author. He prepared a short note and sent it on 7 August to Kahn and Austin Robinson for comment. He asked the latter to hold up the printing of the *Journal,* which he did (Aslanbeigui and Oakes 2007: 23) but after hearing back from Austin Robinson (11 August) that '[Pigou was] so clear that *Dennis and he* had made sure that it represented what *they* wanted to say…' (my emphasis), on 14 August Keynes cabled to Austin Robinson to print the article. Austin, in his reply, and Joan Robinson separately, each said they agreed with Keynes about the quality of the article (11 and 10 August, respectively; XIV 239-40). Kahn later (18 October, XIV 258-9) reported that Sraffa and Shove shared the opinion.

The body of Keynes's note took up points of theory. The last paragraph of the note read:

> I do not know if it is worth adding that … Professor Pigou has so tied himself up in his 'simplified model' that he has provided from the very outset that his own conclusion should be impossible. For if capital is fixed and if the 'period of production' in every industry is unalterable, it is impossible that there should be an increase in the quantity of employment. There also seems to be a confusion between a 'short period' during which finished capital equipment is assumed to be constant, and a 'short period' during which no new capital goods are allowed to be in course of production. The former 'short period' merges into the long period and the changes of the real world; but the latter relates to a frozen land remote in its characteristics from all experience. (*CW* XIV: 238).[15]

The first part of the paragraph deals with a matter of theory – that in Pigou's model income could not change. This has a bit-part to play later. The second part of the paragraph concerns methodology. It is, of course, Keynes's answer to Pigou's charge of mutually inconsistent assumptions in the *General Theory*. This is the key passage around which the main argument of Ambrosi's book revolves.[16]

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[15] Keynes wrote in connection with Pigou's 'Analysis of supply' (1928) that Pigou 'examined the effects of an increase in demand on the rate of growth (or decay) of individual firms in a totally static world. Pigou never made clear the precise characteristics of the abstract world in which [he is] moving. …[This] abstract model [was] so completely static as to be quite remote from anything in experience' (letter of 10 Jan 1928, Bridel and Ingrao: 157).

[16] How I wish I had remembered this passage when Maurizio Caserta and I were writing 'Provisional Equilibrium' (1997). What we were talking about there was the ability of the theorist to 'stop' the progression of the short period into the long period in order to get results, while knowing all along that the temporary closure represented by the short period must eventually dissolve. (See also Chick and Dow 2001.) Currie and Steedman (1990) and Toye (1998) point out that Marshall's conception of 'periods' originated with actual time, and what could change in such periods followed from the length of time under consideration. By contrast, periods today are defined analytically, as theoretical closures which have a relation to worldly reality. In Chick (1983) I pointed out that investment was so low in the UK in the 1920s and 30s that the whole interwar period could be seen as a real-world approximation to the short period.

No further correspondence between Keynes and Kahn is published in *CW* XIV except the two letters of 18 Oct., referred to earlier, and a brief note of 20 October, until a crucial letter from Kahn on 22 October, though there are 25 surviving letters from Keynes to Kahn and 20 from Kahn to Keynes in the interval between them (Marcuzzo 2005: Table 1.1). (They need not all be about this matter, of course. One of them, published in XXI, clearly was not.) Marcuzzo says nothing in her article on the correspondence between Keynes and Kahn (2005) on Pigou 1937. This will be investigated; from Kahn's letter of 22 October there is clearly something interesting that must be unearthed.

Kaldor writes to Keynes on 27 September with the article that eventually becomes Kaldor (1937). There is a substantial discussion of points of theory. Robertson also sent Keynes a note on the debate (XIV: 252). This was not published.

On 7 October, Keynes sends a revised version of his note to Kahn and mentions the Kaldor piece (but apparently does not send it).

All this has been going on behind Pigou's back. Keynes finally writes to Pigou 12 Oct., enclosing his note (the revised version, presumably)[17] and Kaldor's article. Pigou writes Keynes 18 Oct, saying his paragraph 8, the subject of Keynes's point in the first part of his last paragraph, is badly expressed, but

> 'I don't assume or make any assumption which implies that money income is fixed. The argument was that, *if* a cut in wages leaves employment unchanged, money income has no ground for change; that, therefore, we cannot conclude that a wage cut leaves employment unchanged without getting into a contradiction about money income. Kaldor's article, on the other hand, … interprets me, I think, correctly… (*CW* XIV: 256).

He advises that 'it would be best for Kaldor's article to be published, but not yours.' Keynes replies, 20 October: 'No, I am quite clear that my article ought to stand… . I am concerned to dispute precisely what you re-affirm in your letter under reply. That is to say, I maintain that, if there is a cut in wages, unemployment being unchanged, there *is* a ground for a change in money income' (XIV: 257). Pigou agrees to reply in the March issue.

Kahn's letter of 22 October

On 22 October, Kahn wrote to Keynes, having seen Pigou's reply (*CW* XIV: 260). It is sufficiently important to report almost in full.

> (1) It is clear that D. H. R., Kaldor, and Pigou still all fail to see the fundamental fallacy – which is the determination of the rate of interest by the rate of discount of the future (Piero agrees about this). I am not sure whether your own reply brings out the grossness of the error sufficiently forcibly for the ordinary reader to take in that it has been perpetrated.

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[17] Aslanbeigui and Oakes (p. 23) say that Pigou was never shown Keynes's *original* note (my emphasis). This is true, but in their context it looks as if he was not sent *any* version. They also allege that Keynes 'did not give Pigou an opportunity to withdraw or revise his article'. But see Austin Robinson's letter of 11 August, cited above.

> (2) As I expected, Pigou has seized on your last paragraph (which I am still opposed to your printing) and declared triumphantly that you have misunderstood him. Why not force him (and your readers) to concentrate on what is important by agreeing to omit the final paragraph?
> (3)I have not seen Kaldor's article but I am sure that publication of it will darken counsel. After all we could all of us write replies to Pigou if you wanted them and I do not see why Kaldor should be thus favoured.
> …
> (5) …It is of the *highest* importance to make it abundantly clear, so that the casual reader will recognise, that as far as Pigou is concerned the issue is not one of schools of thought but of the most *crashing* and *stupid* errors of statement and of reasoning, such as nobody would deny once his eyes were opened. Your reply ought to be that such argument is ruled out of court.
> …

Kahn's (5) is, I imagine, a restatement of (1) without the matter of substance. What is this gross error?: the time-preference theory of the rate of interest. This is one of the many places in which Ambrosi's rigorous examination of Pigou's theory, which I cannot even give you a taste of in this context, really pays off: he shows that in the stationary state as constructed by Pigou, the rate of interest can *only* be determined by time preference. (Note that Kahn was the only one of 'the children' to concentrate on destroying the role of time preference; Robinson and Kaldor accepted it within the confines of the stationary state.) Keynes knew this. Keynes in reply says: 'I am not so clear about this' and goes on to state the point with perfect clarity. It is Pigou's 'assumption that the demand for money at a given rate of interest depends on the rate of discount of the future' which is wrong (25 Oct., XIV 261). The only role for the rate of interest in the stationary state is to keep the rate of saving equal to zero. There is no investment, and because of stationary equilibrium, no need for liquidity (or money). Not only were Keynes and Kahn choosing different battlegrounds, but Kahn's stance was wrong *and* undermined Keynes's claim to include the classical system as a special case..

(3) is simply outrageous but gives a flavour of Kahn the shock-trooper of the Keynesian Revolution. Kaldor was at that time an outsider (LSE, not Cambridge), and there is nothing Cambridge does better than to favour its own. But how can a respectable academic condemn a paper he has not seen! Keynes, ignoring these reprehensible aspects, replies, 'I am quite clear that I must print Kaldor's article… . The most useful opportunity for the rest of you will be after the Professor's reply in March. My present intention is not to say any more myself, but to leave to you any further stages.' (25 Oct, XIV 262)

Now let us look closely at (2). The words 'still opposed' indicate clearly that there is correspondence missing from the *CW* which would be relevant. No further word from Pigou is printed, no letter listed in Table 5.1 after Bridel and Ingrao's essay, yet something or someone convinced Keynes to write to him on 25 October as follows: 'In view of your assurance that you are not meaning to assume constancy of money income, I am deleting the last paragraph of my note…' (XIV 258). He also informs Kahn. It is a very strange *volte face*. The only possible explanation I can see is Kahn's letter (and whatever was in the earlier one(s)). But why did that letter have the power to change Keynes's mind after such a vigorous defence earlier? And – this is the real puzzle – why was the *whole* paragraph deleted when only the first part of it was in contention? Was it just a case of carelessness – throwing the baby out with the bathwater? But it was such an important baby. Was there something else behind it?

I cannot believe it was absent-mindedness on Keynes's part. Aslanbeigui and Oakes (2007) and Moggridge (2006) maintain that Keynes accepted Kahn as the strategist in the 'Keynesian Revolution', and the former agree with Ambrosi that Kahn's strategy was to concentrate on internal inconsistency in Pigou's theory (see especially A and O: 23-4). This explanation fits with Keynes's action after Kahn's 22 October letter: a difference in methodology can be perceived as the presentation of an alternative paradigm, whereas Kahn's strategy was to defeat Pigou on the level of theory. Whatever the answer to these puzzles, the paragraph was not published until it appeared in *CW* XIV (1973), and the methodological dispute over the nature of the short period and its relationship of the long period was never joined. It was, apparently, due in the first instance to Kahn that this very important disagreement was never debated and was, in fact, lost to the history of the Keynesian revolution and Post-Keynesian economics.

At issue here is a feature of methodology which has concerned me before: 'necessary compromise'.[18] In this case, compromise is necessary in order to preserve a link with reality: recall that Keynes's complaint against Pigou is that the 'frozen land' removes Pigou's theory from any contact with reality. Reality and logical purity, while both desirable, are often at odds and impossible to achieve simultaneously. Keynes chose compromise and a temporary closure (to use the language of Chick and Dow 2001) – a Marshallian short period - that retains contact with reality; as the 'frozen land' passage shows, he knew exactly what he was doing. A psychological pre-condition for accepting this sort of method is the ability to sustain a certain amount of ambiguity. For Pigou, a closure which at some unspecified time would break down, as the 'short period merges into the long period', was not an ambiguity but incoherence; he chose internal consistency at the expense of reality.

Concepts of the long period –a dividing line between Keynes and Pigou

The origin of Pigou's choice lies not only in his anxiety to avoid contradiction, but also in an attachment to the conception of the classical stationary state, where replacement investment keeps capital intact (in the case of Pigou 1937, the assumption is stronger: with no depreciation, there is zero *gross* investment and saving). Keynes, as is I think well known by now, rejected the idea of this long period equilibrium as the central tendency of the economy and deviations from it as temporary aberrations mainly due to monetary (or interest rate) dislocation and, because temporary, not really a fit subject for economic theory.[19]

In his lecture in Cambridge of 14 November 1932 Keynes distinguished two senses of 'long period':' a position towards which forces spring up to influence the short-period position whenever the latter has diverged form it (or, alternatively, the long period is a

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[18] See Chick (2002) for the application of this idea to the theory of investment and Chick and Dow (2006) for the need to compromise when linking micro and macroeconomics.

[19] This conception survives in contemporary economics amongst the neoRicardians. Amongst Post-Keynesians, to my great regret, the otherwise insightful Colin Rogers has accepted this idea, to the detriment, in my view, of his theoretical progress, foreshadowed in Rogers (1988?).

stable position towards which short-period positions tend to move). There is a third position from the point of view of production' (Rymes: 73). This third position is, I suppose, the one in which capital is adjusted fully to production of goods according to the demand for them.

Pigou's long period was thus the third type, which in Pigou (1937) he also applied to the short period, while Keynes, in the 'frozen land' paragraph, adheres to the second type.

However defined, the long period was not, according to Keynes's conception, the appropriate framework for the analysis of employment – and it was Pigou's *Theory of Unemployment* (1933) that started this debate between Pigou and Keynes. Employment decisions are always taken with respect to the capital stock already on hand, even while new capital might be coming on stream. The new capital may affect employment 'later', but not until capital affects supply conditions. So not only is the frozen land a non-existent territory, it is a singularly inappropriate framework in which to discuss employment or unemployment. But unless there is investment, or some other exogenous variable to change income, how can employment change?

Accumulation *defined* the subject of the long period for Marshall, though not for Pigou. *The stationary state is an equilibrium situation*, the outcome or end-point of a process of accumulation, in which all capital earns equal profit, because it is perfectly adjusted to demand, and there is no force for change. To analyse in the framework of a stationary state is to do one variety of what Chick and Caserta (1997) called Equilibrium Theory, using capital letters to distinguish this procedure from creating a theory or model which *has* an equilibrium but where analysis is not confined to that situation. Marshall sought to analyse the process, Pigou the end-point. Pigou was doing Equilibrium Theory.

We have seen that one of the important theoretical differences between Pigou and Keynes, time preference, is directly due to the different methodological foundations of their theories. As always, it is missing the point to concentrate on theoretical differences when methodological differences are, at bottom, responsible for those differences.

Keynes's invitation

Keynes took up Kahn's offer in his letter of 22 October: 'After all we could all of us write replies to Pigou if you wanted them'. Recall his reply (25 October):

> The most useful opportunity for the rest of you will be after the Professor's reply in March. My present intention is not to say any more myself, but to leave to you any further stages.

How did 'the children' respond? - and one can include Shove, Sraffa and Austin Robinson along with Joan Robinson and Kahn, as all had deplored Pigou's article. With absolute silence. Now as anyone knows, when children are playing out of sight, but not out of earshot, it is when they fall quiet that one really needs to worry what they are doing.

Ambrosi points to three areas where the debate really needed further development: the connection between changes in money wages and in real wages; time preference, and the appropriate construction of the short (and long) periods. We only consider the last.

Previous work of 'the children'

Joan Robinson

As Ambrosi points out, while the economics education in Cambridge was founded on Marshall, Pigou's influence was very strong. And we have seen something of his attachment to equilibrium theorising and to the stationary state. It may be, also, that the classical evaluation of the short period as temporary and therefore not amenable to theorising led Joan Robinson (hereafter JR) to 'extend' Keynes's theory of employment to the long period to protect Keynes from this charge.[20] (This is Kregel's interpretation, 1996.) Her paper, 'The long-period theory of employment', was published in the same year as the *General Theory* and quickly reprinted in the collection *Essays in the Theory of Employment* (1937), just as the debate about Pigou (1937) was going on. In this work the rate of interest is such as to keep net investment and saving equal to zero. Another of the *Essays*, 'The concept of zero saving', elaborates this role of the rate of interest and addresses the question of a negative rate. 'Diagrammatic illustrations' deals with representations of labour demand and supply in both the long and the short period.

The essays in that volume were 'written while Keynes's *General Theory* was going through the press' (*CEP* IV: 174) – they would have to have been, even given the much quicker publication time of those days. If she saw the 'frozen land' complaint, she would certainly not want it published. Perhaps Kahn was protecting her from this embarrassment.

In 1933, James Meade, who was by now at Oxford** but had been a participant in the Circus, had published *The Rate of Interest in a Progressive State*. His opening method was slightly different from Pigou's. Income was kept constant but investment could vary, compensated by an opposite change in consumption (or v.v.):

> A neutral monetary system is one which simply interprets the decisions of individuals, of companies or of the government without, by its own action or inaction, making the effects of such decisions different from what they would have been in a non-monetary economy. A neutral money system is then, on the assumption of a constant population, one which maintains Final Incomes constant (p.11).

I.e. the banking system sustains Say's Law. JR, in her review (1934), sees this as a throwback to the classical notion of the 'real' equilibrium and purely monetary disturbances. So JR had accepted that part of Keynes's message. She describes Meade as inhabiting a 'half-way house between the short and the long period' (p. 285) but his own argument (Ch IV) 'shows that in the kind of long-period equilibrium appropriate to the theory of value no investment is taking place and it is clearly necessary …to explore the still more remote territory of the stationary state' (*ibid*.).

[20] It is surprising, actually, how much of Keynes's message survives in her treatment.

JR's evaluation (1933) of the *Treatise on Money* (1930) is notable for her saying for the first time that Keynes didn't understand what changes he had wrought,[21] but for our purposes the relevant passage is this:

> [I]t was only with disequilibrium positions that Mr. Keynes was consciously concerned when he wrote the *Treatise*. He failed to notice that he had incidentally evolved a new theory of the long-period analysis of output.(JR 1933: 56)

The tone suggests approval. Note that this was published as the lead article in the first issue of 'the children's magazine'. It couldn't have escaped the notice of any of the followers of Keynes – or of Keynes himself.

Kahn

Kahn also had an interest in the frozen land. JR tells us that when the *Treatise* caused puzzlement, he had recast its main argument in terms of a reinvented form of Marx's simple reproduction, where there is neither saving nor investment. This work apparently circulated widely but was not published[22] (Robinson 1953: 252; GMA: 249). Both Kahn and Champernowne are thanked by Pigou for their criticisms of *The Economics of Stationary States* (Pigou 1935: v) and Champernowne for 'finding two fallacies' in his reply to Keynes and Kaldor (what became Pigou 1938) (letter to Keynes, 23 December, XIV: 266).

But Kahn also wrote *The Economics of the Short Period* as his fellowship dissertation. It was submitted in 1929, a year after he took his economics degree. (It was not published until 1983 in Italian and 1989 in English.) Much of what we now take for granted in the short period is worked out there – and there is much we have, unfortunately, forgotten. Surely he understood that employment was a short-period problem. His definition of the short period is based on the characteristics of fixed and working capital in the real world: '…the life of fixed capital is considerably greater that the period of production, greater, that is, that the life of working capital' (Kahn 1989: xiii). This is a definition quite close to Marshall's and quite unlike the abstract conception of Pigou. And he connects the length of the short period in real time with producers' expectations of whether a change in demand is permanent of temporary. But why did a person with such expertise in the short period and such a realist methodological stance want Keynes to suppress his dissatisfaction with a theory of employment based in the frozen land of Pigou's long period? It is a mystery.[23]

Kaldor

Kaldor had not written a great deal before this time – on the benefits or otherwise of technical progress, the determinateness of equilibrium, the equilibrium of the firm, market imperfection and excess capacity – nothing that was particularly relevant to

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[21] She later famously said of the *General Theory* that the Circus found it difficult to get Keynes to see 'what the point of his revolution really was'.**

[22] Readers should not rely on Ambrosi's excerpts (p. 248) and interpretation to understand the spirit and meaning of this amusing piece.

[23] If one accepts Marcuzzo's evaluation of the cooperation between Kahn and Robinson as amounting to their being joint originators of short-period analysis (1996), then JR's attachment to long-period analysis is even more puzzling.

the problem raised by Pigou (1937). He was happy to accept time preference in the long-period context, and he was not unsympathetic to Pigou's frozen land. Indeed, Robertson (of all people) wrote to Keynes (17 Oct 1937; XIV: 254), 'Kaldor, in my view, underestimates (p. 8 [of Kaldor's draft]) the difference made to the situation by the removal of the assumption of nil investment.' Perhaps Kaldor's methodological foundations were not strong. Can the following be correct?:

> So far we have assumed, in accordance with Professor Pigou's model, that investments are actually zero in short-period equilibrium. But it is easily shown that the argument is equally applicable to the more general case where investment is assumed to be constant and positive. (Kaldor 1937: 751; GMA: 238)

Few phrases arouse my suspicion more than 'it is easily shown that…' – with good cause in this case, as it turns out.

> I really do not think it makes much difference to the argument, with which of the short-run models one works, and I think my argument holds just as much for the Keynesian short-run [sic]…. as for the original Pigou model. This, however, is not easy to show…(letter from Kaldor to Robertson, 23 October 1937, as quoted by Young 1987: 111)

Another agenda?

Ambrosi looks at this background (and what in particular JR had done and went on to do – he devotes the whole of Part IV to this). He concludes that 'the children', especially JR, were so imbued with Pigou's methodology that it would have been far too difficult psychologically for them to make a clean break.

> [F]or Keynes himself the debate [was about] Pigou's…fundamental methodological approach. But none of Keynes' disciples seconded him in this view. In particular, they were not prepared to see a paradigmatic dividing line between Keynesian and Pigovian short-period analysis.' (GMA: 245; he refers to Kaldor's letter, quoted just above, in evidence.)

Ambrosi's important conclusion may lead us to reassess more radically than has been done before the position of 'the children' in the development of Keynes's economics. Bridel and Ingrao (2005) conclude that Pigou 'forced Keynesian analysis into the theoretical scaffolding of long-term macroeconomic equilibrium built on Marshallian foundations. …[He] denied Keynes's claim that he had got off the beaten track of classical theory.' (p. 165). This is a debatable evaluation: early 'Keynesians' did not move in that direction. It took the gradual co-option of IS-LM to the classical cause to do that (Barens 1997, Young and Zilberfarb 2000). But if Pigou did succeed, he did so, according to Ambrosi, with more than a little help from *Keynes's* friends.

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