

SHARED IDEAS AMID MUTUAL INCOMPREHENSION: KALECKI AND CAMBRIDGE

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‘Cambridge is a very isolated place ...’ (Johnson 1977)

1. Introduction: The End of the Cambridge Project¹

Discussions about the relationship between Michał Kalecki and John Maynard Keynes have rightly focussed upon the compatibility of the ideas of the two men. Interpretations of Keynes have not always found Kalecki to be complementary to Keynes. Joan Robinson famously did (Robinson 1964). But her close associate in Cambridge, Richard Kahn did not (see, for example, Kahn 1972). Both Kahn and Robinson had worked closely with Keynes and Kalecki. Kalecki’s collaboration with Joan Robinson and Richard Kahn occurred during 1939, when Robinson and Kahn supervised Kalecki’s research. This paper focuses on what the fate of that collaboration reveals about the methodological preconceptions of Keynes and Kalecki.

At the end of 1939, Kalecki found that the project on industrial pricing, which he was working at Cambridge, funded by the National Institute for Economic and Social Research, was terminated. Kalecki moved, with his post funded by the NIESR, to Oxford. Kalecki’s relationship with Keynes does not appear to have suffered from the set-back of the former’s removal to Oxford. Keynes continued to support Kalecki’s research through the National Institute of Economic and Social Research. Nevertheless some clash of ideas

¹ This paper could not have been written without the pioneering work of Malcolm Sawyer in his *The Economics of Michał Kalecki* (Sawyer 1985) and his generous discussions with me on the meaning and significance of Kalecki’s analysis.

had occurred and it is useful to consider what those ideas were and why Kalecki was unable to submit his research to a standard Cambridge method of reasoning, or 'mode of thought', in Sheila Dow's much more vivid phrase. Such a consideration lies at the heart of the relationship between Kalecki and Keynes and their many arguments over method. Yet it is largely missing from the replete literature on the relation between the theories of the two great men, drawn mainly from Joan Robinson's claim of independent discovery of common theoretical positions (Robinson 1964) and Kalecki's own claim to priority in that discovery: In a letter to T.C. Chang dated 17 February 1955, Kalecki wrote of a lecture that he gave in Cambridge in which 'they made a point of it to stress in the introduction my discovery of General Theory before Keynes.' (PAN III 319/30). (At the end of his life Kalecki was even more convinced of the priority of his 'discovery', referring to his own papers published 'in Polish before Keynes' *General Theory* appeared and containing, I believe, its essentials'. Kalecki 1971, p. vii.)

A recent paper in the journal *History of Political Economy* on Keynes's methodological differences with Kalecki concludes correctly that Keynes's analytical methods were foreign to (Kalecki)' but does not really explain why (De Vecchi 2008). Another paper, by Nahid Aslanbeigui and Guy Oakes, suggests that the whole Cambridge project, on the effects of the 1930-1935 depression on prices, costs, production, employment, incomes and foreign trade, was merely an elaborate ruse to remove Dennis Robertson from Cambridge as a way of obtaining the hegemony of Keynesian ideas over Economics in that university (Aslanbeigui and Oakes 2002). Aslanbeigui's and Oakes' case is tenuous at best. It does not explain why Robertson was the victim, and not Arthur Pigou or Maurice Dobb, or even Piero Sraffa, none of whom were Keynesians, and confuses the style of Cambridge for its substance: Just because nothing can happen in Cambridge without a conspiracy does not mean that everything that happens in Cambridge is a conspiracy. There were other factors at work. Even if Aslanbeigui and Oakes suggest that Kalecki was dispensable after Robertson had resigned from Cambridge, Keynes certainly did not cease to support Kalecki after the project had collapsed. Robertson, in any case, returned to Cambridge as Professor of Political Economy in 1943, when Pigou retired.

The principles common to both Keynes and Kalecki were, according to Joan Robinson their common interlocutor, ‘that the rate of saving is governed by the rate of investment, that the level of prices is governed by the level of money wage rates, and that the level of interest rates is governed by the supply and demand for money’ (Robinson 1966). Any notion of a common monetary and financial analysis may be dismissed despite serious claims that Post-Keynesian monetary principles may be found in Kalecki (Sawyer 2001). Even his most sympathetic follower Joseph Steindl was moved to conclude that ‘generally speaking you do not find in Kalecki very much about ... finance, debt, credit crises. I don’t think Kalecki denied the importance of these factors in any way, but for him they were secondary to, and in a sense derived from, the events in the ‘real’ sphere of production, investment, overcapacity, and so on.’ (Steindl 1989, pp. 312-3). Any notion that both emphasised the importance of aggregate demand as a determinant of output and employment may also be dismissed. By the 1930s such ideas were hardly original, and could be found, for example, in Ralph Hawtrey’s well-known *Good and Bad Trade*, which had been published in 1913.

In his most serious examination of the *General Theory* Kalecki himself identified ‘the proposition that investment determines the global volume of production’ as a principle which he had ‘proved in a similar way to Keynes’s in my *Essays on the Business Cycle Theory*.’ (Kalecki 1936). However, even among Post-Keynesians, such a proposition is hardly common ground. The serious question for Keynesian economics that arises from any comparison of Kalecki with Keynes is why, if the two men had made common discoveries, did their closest collaboration in Cambridge in 1939 end in such apparent incomprehension? Part of the enigma arises because the question has always been approached from the point of view of Keynes’s originality in Cambridge economics, an intellectual project to which Joan Robinson recruited Kalecki, who joined willingly because he agreed with essential elements of Keynes’s ideas and had few other professional options. As she put it: ‘The interesting thing is that two thinkers, from completely different political and intellectual starting points, should come to the same conclusion. For us in Cambridge it was a great comfort.’ (Robinson 1964, p. 337).

The question of the degree to which Kalecki had ‘anticipated’ Keynes, or shared common ground with him, is a matter of textual exegesis whose starting point has to be what Keynes really meant. The true meaning of Keynes’s theory is not an issue on which his partisans themselves agree. Keynes himself sanctioned a wider discussion of his core ideas in his 1937 article in the *Quarterly Journal of Economics* in which he wrote ‘I am more attached to the comparatively simple fundamental ideas which underlie my theory than to the particular forms in which I have embodied them, and I have no desire that the latter should be crystallised at the present stage of the debate.’ (Keynes 1937, p.111). His most enthusiastic follower George Shackle endorsed this transcendentalist view of Keynes’s ideas by referring to ‘Keynes’s ultimate meaning’ (Shackle 1967, p. 129). In his last book on Alan Coddington documented the varieties of ostensibly core Keynesian ideas (Coddington 1983). Specifying the nature and significance of Keynesian ideas is therefore a major task that cannot satisfactorily be concluded in an essay towards an intellectual biography of Kalecki. In relation to the failure of his project in Cambridge, the question ‘did Kalecki anticipate Keynes?’ should really be ‘if Keynes and Kalecki held key ideas in common, why was the Cambridge project closed down?’

Answers to this may be found in the incompatible personal chemistry of the two men, the one an urbane, ‘moderately conservative’ upper class Englishman, the other a Polish Jew of pronounced left-wing and Marxist sympathies who was as socially awkward as he was confident in his views. The financially-insecure Kalecki nevertheless yearned for acknowledgement from Keynes of the former’s priority in discovering the principles behind the *General Theory* – another misjudgement since Keynes was least inclined to intellectual modesty. (Tadeusz Kowalik points out to me that even in Keynes’s life-time Joan Robinson promised to raise the issue of Kalecki’s priority with Keynes, but procrastinated. Kalecki told Kowalik that he eventually went himself to raise the matter with Keynes. But Keynes, like Joan Robinson later, merely treated this as confirmation of the ‘scientific’ character of his ‘discovery’.)

Nevertheless, given their own commitment to ‘scientific’ economic research, and to the new ideas which they espoused, personal temperament could not have been a sufficient

reason for the Cambridge project's failure. If the Keynesian Revolution was the major scientific breakthrough that Keynes's supporters in Cambridge, led by Richard Kahn and Joan Robinson, claimed that it was, why did Kalecki, who was playing a major part in that revolution, leave the University through which passed the front line of Keynes's war with 'the Classics'?

Much of the criticism that Kalecki's research met in Cambridge was methodological. This provides not only the rationale for his departure, but also the clue to the differences that he had already had with Keynes over his publications, and which he was to have after leaving Cambridge. While the textual exegetists have pored over the two men's work in search of similarities and complementarities, the fact remains that their differences were rooted in different methodological approaches that went beyond Kalecki's class approach, and Keynes's more obvious preference for explaining economic phenomena in terms of individual choices. At the interface between Cambridge (including its most original thinker after Marshall, Keynes) and Kalecki was a much greater, even impersonal, incompatibility between the economic tradition in Cambridge that had formed Keynes, and the continental European economics from which Kalecki had emerged. Keynes's own failure to understand Kalecki is indicated in a remark that Keynes made, in parenthesis, in a letter to Kahn dated the 30 April 1938 referring to the Polish economist's 'appalling method of exposition'. 'His mathematics seems to be largely devoted to covering up the premises and making it extremely difficult to bring one's intuition to bear. If only he would state his premises in the most illuminating possible manner and be perfunctory over his mathematics, instead of the other way around, one would have a better idea of what he is driving at'. Keynes's way of dealing with this, he wrote, was 'to disentangle painfully exactly what the assumptions amount to and then consider whether the conclusion appears to be correct, not bothering much about the proof which, in spite of the appearance to the contrary, obviously contains endless loopholes for introducing fresh assumptions' (Kahn Papers RFK/13/57/366). The 'appalling method of exposition' was that of Kalecki's *Econometrica* article on 'The Determinants of Distribution of National Income'; 'one's intuitions' were obviously Marshallian!

It is argued here that Kalecki's rendezvous in Cambridge was a part of a limited interchange between continental European and Cambridge economic theory. His arrival in Cambridge may be called the third emergence, or impact, of continental European economic theory, in an intellectual community made introverted by its dispersal around a federal university, and its common struggles to establish and maintain Economics teaching at that university. (Some of this introversion is apparent in Robert Skidelsky's generous chapter on what he called 'Cambridge Civilisation' (Skidelsky 1983). A somewhat more bilious account is provided by the otherwise judicious and scholarly Terence Hutchison in an extended essay on 'The Philosophy and Politics of the Cambridge School' (Hutchison 1981)). Earlier, Harry Johnson had painted a picture of Cambridge economics that took exception, in similar terms to Hutchison, to the Marxian influence at Cambridge. However, Johnson emphasised explicitly the influence of Kalecki's ideas, through their advocacy by Joan Robinson, in the Cambridge version of Keynesian economics (Johnson 1977).

2: The Cambridge Research Project

From its very beginnings the Cambridge Project was beset by methodological issues. In the first place, and contrary to much of the subsequent reporting of the project's work, it was not a project of the University of Cambridge, or of its Faculty of Economics, but of the National Institute of Economic and Social Research (NIESR) that was located in Cambridge because that was where Keynes was hoping to establish a permanent economic research unit that was to be the Department of Applied Economics. This, rather than the machinations of a Keynesian faction in the Faculty of Economics, explains the peculiar administration of the project. The finance for the project was from the NIESR, managed by a Supervisory Committee consisting of Arthur Bowley and Lionel Robbins (both of whom were then working at the London School of Economics), Noel Hall of the NIESR, who was also Professor of Political Economy at University College, London, as well as Keynes and Austin Robinson.

The finance was to support a Cambridge research group that consisted of Keynes and Austin Robinson (Chairman and Secretary respectively), Richard Kahn, Joan Robinson, Piero Sraffa, David Champernowne (the University Lecturer in Statistics), and Kalecki, who was described as working for the group 'as their Statistician'. Kalecki in fact was the only member of the group who was actually engaged in project research, assisted by two research students Brian Tew and Yu-Nan Hsu. Hence, out of the initial annual budget of £600 allocated to the project, £350 was a salary paid to Kalecki. An initial report on the work of the project referred to the 'functions' of the other members of the group as 'primarily critical and supervisory.' The actual title of the project was *The Cambridge Research Scheme of the National Institute for Economic and Social Research* and its purpose was to study 'the process of Economic Change in the United Kingdom since 1928' (Keynes Papers, King's College Cambridge N15/1/77).

The very top-heavy structures established to manage the project were in large part due to the requirements of the NIESR whose Director Noel Hall wanted to avoid pressure to use the Institute's funds for 'private and personal investigations'. However, by the late 1930s, empirical investigations of the business cycle were widespread. (The best-known of these studies, and still largely underestimated among economists, was Schumpeter's massive *Business Cycles: A Theoretical, Historical and Statistical Analysis of the Capitalist Process* was published in 1939). In a letter to Keynes, dated the 22 September 1938, Hall had expressed his reservations concerning an earlier version of the research proposal presented by Austin Robinson, which Hall thought was 'very much too wide' and therefore likely to lead to 'overlapping and duplication' of similar work at the Institute and in universities (Keynes Papers, King's College Cambridge N15/1/17). The initial year of study was subsequently changed to 1924, and the scope of the study was narrowed to examining the relation of prices to costs in different industries; consumption and foreign trade; the relationship between foreign investment and exports; and sources of saving, bringing up to date the estimates given in the *Liberal Industrial Inquiry* (1928, p. 109).

At Cambridge Kalecki settled down to gathering data on industrial production by industry and the share of 'prime costs' (labour and raw materials) in the total output of the coal, cotton, steel tobacco, ship-building and electricity supply industries. The result was a series of papers that have not hitherto been published but are deposited in the Keynes and Kahn Papers in the Archive Centre of King's College Cambridge. The papers are largely concerned with statistical methods to obtain consistent data series. In addition to the list given by Jerzy Osiatyński in the Kalecki *Collected Works*, derived from documents obtained from Richard Stone (Osiatyński 1991, p. 525) there is a substantial paper, *Prime Costs and Proceeds etc. in Shipbuilding*, co-authored with G.A. Bauer. This developed an ingenious method for calculating an average construction period in shipbuilding, from which a continuous series for shipbuilding construction is derived ((Keynes Papers, King's College Cambridge N15/1/128).

The Kahn Papers reveal the extent of dissatisfaction of the Cambridge economists with Kalecki's research. Keynes couldn't understand what the purpose of gathering the data and estimating gaps in it, and he worried over the aggregation of firms into industries (Kahn Papers RFK/5/1/142). He and Joan Robinson objected to Kalecki's work on the 'degree of monopoly'. Although this is not mentioned in the papers that Kalecki prepared in Cambridge, it is clear from other papers he published at the time (Kalecki 1939, pp. 23-41) that the degree of monopoly was going to feature in his explanation of different rates of profit in different industries. Kahn argued that the role of surplus capacity, associated with monopoly, was 'exaggerated'. Kahn composed all this in an extensive letter to Kalecki (Kahn Papers RFK/5/1/159-162). Kalecki replied with a six-page memorandum that did not mention the degree of monopoly. Point by point he dealt with the chief accusations that his estimates were 'manipulated'; that he did not use indices; that his choice of industries was unclear (he had in fact chosen the industries in agreement with Richard Stone and his wife). In a letter to Kahn, dated the 9 June 1939, Kalecki indicated his intention to leave Cambridge in order to write his own 'theoretical interpretation of the results'. (Kahn Papers, RFK/5/1/146&147). He stayed in Cambridge until the end of 1939, when the Department of Applied economics was ready to be set up.

3. The confrontation of methodological traditions

The confrontation over statistical method was in fact a confrontation over theory, reflecting the difficulty of incorporating Kalecki's 'degree of monopoly' analysis within a Marshallian framework. This much is clear from the excellent exposition of the dispute by Peter Kriesler (Kriesler 1987, pp. 107-111. See also Halevi 1978 and Sawyer 1985, chapter 2). Kalecki's view was derived from the European discussions that followed the publication of Hilferding's *Finance Capital*, which first put forward an analysis of a capitalist economy divided into two sectors: one consisting of industries dominated by monopolies; and the other a competitive sector. However, Kalecki's was not the first confrontation between European economic theory and the tradition established by Marshall in Cambridge.

The first emergence of continental European economics in Cambridge came with the arrival there in the mid-1920s of Piero Sraffa. Sraffa sent a shot over the bow of Cambridge economics by showing that Marshall's elaborate scheme of partial equilibrium in perfect competition was incompatible with increasing, or even constant, returns to scale, in a paper which his friend Keynes published in the *Economic Journal* (Sraffa 1926). Thereafter he withdrew from teaching and quietly nurtured his critique of Marshallian economics until the publication of his major reconstruction of Ricardian economics in 1960. While he participated significantly in many of the key discussions between Keynes, Joan Robinson, Kahn and Kalecki 'it remains a puzzle that the two escape routes from Marshallian orthodoxy – the one associated with Sraffa and imperfect competition, the other with Keynes and effective demand – never converged in Keynes's lifetime, though leading disciples like Kahn and Joan Robinson were heavily involved both 'revolutions' (Skidelsky 1992 p. 290. James Tobin had expressed a similar view in Tobin 1981. Even earlier, Joan Robinson admitted that the two 'revolutions' had converged in Kalecki's work: '... the two streams of thought were combined by Michał Kalecki' Robinson 1958, p. 241).

The second emergence of continental European economics at Cambridge came with the arrival, at the beginning of 1931, of Friedrich Hayek at the London School of Economics. He came at the behest of Lionel Robbins, who was then an admirer of Austrian economic theory. Hayek became the chief exponent in Britain of continental European economic ideas. But his first lectures, published as *Prices and Production* were poorly received in Cambridge. His monetary analysis and capital theory were ferociously criticised by Keynes, Sraffa and subsequently by Hayek's former student Kaldor (Keynes, 1931, Sraffa 1932, Kaldor 1942). Hayek's own aversion to state intervention ensured that, as the Great Depression corroded established economic theory and policy, Hayek and his supporters excluded themselves from mainstream economic discussions until the 1980s. By the 1930s, and especially after the publication of his *General Theory* Keynes, with only fragmentary notions of what was being discussed among economists on the continent, but with editorial control over the *Economic Journal*, was setting the agenda for economic theory in Cambridge.

When Kalecki arrived in Cambridge, he brought with him a version of continental European analysis that was methodologically and philosophically incompatible with the Marshallian tradition. At the root of his difficulties in Cambridge lay the different approaches that had been adopted in Britain and continental Europe in answer to the most fundamental question of the scope and significance of economic analysis.

The question had been raised in the third quarter of the nineteenth century by John Stuart Mill, Auguste Comte, and Karl Marx. All of them had concluded that the economy is an abstraction from the way in which societies organise production and distribution. There were therefore no universal economic 'laws' that were not conditional upon some associated social arrangements. This view argued that economic models which claimed to represent any real situation, were always going to be under-determined because any given real situation would have, among its determinants, social, historic and cultural, as well as economic factors. In other words, economic analysis could not give rise to unambiguous conclusions, because these would always depend upon social, historic and cultural influences upon economic activity. Out of this came the notorious German

methodenstreit, which pitched historicism against deductive analysis. But, by the early twentieth century, the discussion had moved on in Europe and in Britain, to uncover those elements of economic analysis that could be combined into determinate models, self-determinate sub-systems of evolving social systems.

4. The Cambridge peculiarity

In Britain the major economic systems-builder who brought academic economics out of the grasp of social theory was Alfred Marshall. His ingenious solution to the problem of economic under-determination was established at Cambridge, and continues to hold sway throughout the English-speaking world and, increasingly, throughout mainstream economics. Marshall's solution was to divide up economic activity into discrete systems which he postulated operated in periods that allowed them to determine particular outcomes. The periods roughly coincide with observed time-periods. The basic period is a market 'day' in which the prices that bring the supply of and demand for commodities into equilibrium are fixed. During that period, firms supplying goods, and individuals buying them, are only influenced by prices, and therefore can come to an agreement as to the prices that will satisfy them all. During that period too, productive capabilities do not change. Given those prices, firms then determine how much to supply. In a separate 'short term' period, firms decide where production is most profitable, and expand into the most profitable markets, eliminating excess profits. In yet a third, 'long-term' period, firms decide on what scale to produce. (Marshall 1920, Book V). This, and the associated assumption that all these decisions could be made in a state of perfect competition, was the point that aroused Sraffa's criticism of Marshallian analysis (see above).

One of the difficulties of Marshall's solution to the problem of economic under-determination is that it results in an over-determined system capable of multiple equilibria. It is only useful on its own terms if the equilibrium in each period is arrived at in one period at a time. Once all things are allowed to change then it is possible to have different general equilibria in the whole system according to, for example, the different scales of production or investment in the system. Moreover, Sraffa's critique had the important methodological implication that developments in one period, such as the

increasing returns long-term, may subvert the mechanisms that are assumed to bring equilibrium in other periods. Therefore it may not be possible to move through successive periods, establishing successive equilibria to the satisfaction of the analyst, in the way postulated by Marshall. Nevertheless, the Marshallian method of getting determinate solutions for particular economic sub-systems defined by their periods was a way of dealing with the complexity of an economy that was otherwise under-determined. Hence, when Keynes came to consider the complexity of an economy as a whole, in his *General Theory*, it was natural to use this Marshallian method: (See e.g., Keynes's definitions of income and saving in chapter 6 of the *General Theory*, Keynes 1936). As Axel Leijonhufvud noted: 'Sequential period analysis is simpler in that it substitutes step-functions for more complicated time-paths of the variables.' (Leijonhufvud 1968, p. 36).

In one of her most insightful comments on the economics derived from her teacher Alfred Marshall, Joan Robinson was later to distinguish 'periods' in which all other factors are held constant, but one economic sub-system adjusts to equilibrium, from the more common time periods over which economic activity occurs, by calling the first 'logical' time and the second 'historical' time. She concluded:

'There is much to be learned from *a priori* comparisons of equilibrium positions, but they must be kept in their logical place. They cannot be applied to actual situations... In a model depicting equilibrium positions there is no causation. It consists of a closed circle of simultaneous equations. The value of each element is entailed by the values of the rest. At any moment in logical time the past is determined as much as the future. In an historical model, causal relations have to be specified...' (Robinson 1962, pp. 25-26). As a result, she might have added, analysis in logical time may be driven by intuitions derived from empirical observation. But such analysis has no empirical content, if only because actual economic events occur in historical time. Kalecki's empirical study of industrial prices, indeed any empirical study, could not fail to challenge the methodological preconceptions of Cambridge economics.

5. The German economic determination

By the time he came to be aware of it, continental European economic thought, which Kalecki had absorbed in the course of his economic journalism and researches at the Institute for Research in Business Cycles and Prices in Warsaw, had resolved the problem of economic under-determination in a very different way. Instead of dividing up economic decisions into discrete determinate periods, analytical economics in continental Europe identified two key systems of economic variables that were determinate. The first of these was the circular flow of income i.e., the income flows created in the process of production, as firms' expenditure which returns to them as sales revenue when those who have received incomes spend them. This economic relation may have been rooted in a social process of production, but its outcome in an identity between aggregate output, income and expenditure is obviously a logical and determinate system. Moreover, since flows occur over time, the circular flow of income offers a neat way of linking up economic activity in successive periods, as opposed to a unique equilibrium that, once obtained, disappears from history.

The origins of this analysis went back to Quesnay's *tableaux économiques*, but it had found its way into Austrian economic theory in large part through the discussions that followed the publication in 1885 of Volume II of Marx's *Capital*. It should be remembered that the 'capital' whose circulation Marx analysed in this volume consisted of the total costs of production (in aggregate total national income), that are placed into circulation in the economy by capitalist production. Thus in his first exposé of history of economic thought and economic methodology Josef Schumpeter identified the Physiocrats' circular flow of income, as a methodological cornerstone of economics that showed:

'... how each economic period becomes the basis for the subsequent one, not only in a technical sense but also in the sense that it produces exactly such results as will induce and enable the members of the economic community to repeat the same process in the same form in the next economic period; how economic production comes about as a social process As long as economic periods were viewed merely as a technical phenomenon, and the fact of the economic cycle through which they move had not been recognised, the connecting link of economic causality and an insight into the inner

necessity and the general character of economics was missing. It was possible to consider the individual acts of exchange, the phenomenon of money, the question of protective tariffs as economic problems, but it was impossible to view with clarity the total process which unfolds itself in a particular economic period.' (Schumpeter 1912, pp. 43-44).

For Schumpeter, the other key system of determinate relations was of prices. Here he believed that the ultimate breakthrough had been achieved by Léon Walras, with all quantities supplied and demand, brought into general equilibrium by a unique system of prices. Schumpeter concluded that, along with the notion that marginal products determine the shares of 'various factors of production', '...the theory of price ... really forms the basis for the formation of incomes.' (Schumpeter 1912, p. 197). Although this view fits uneasily with the circular flow of income determination of aggregate incomes, Schumpeter does not seem to have considered the approaches to be incompatible.

The circular flow of income eventually found its way into English-language economics as the identities between income output and expenditure that are used in national income accounts. However, such accounts belong to applied economics and the circular flow of income was therefore not a part of English economic theory. Nevertheless, the idea that income depends on expenditure is the foundation of Keynes's paradox of thrift and arguably one of the key innovations in his thought that followed the publication of his *Treatise on Money*. As for Kalecki, the circular flow of income was embedded in his ideas from at least his early investigations of aggregate income and expenditure. The circular flow became the foundation of the national income statistics for Poland in 1929 that Kalecki and his colleague Ludwik Landau published in 1934 (Landau and Kalecki 1934). Following his move from Cambridge, Kalecki embraced even more strongly the circular flow of income as a fundamental principle integrating economic phenomena. Using it he came to his theory of profits, on which basis he elaborated his analysis of capitalist dynamics.

6. Kalecki's economic determination

Even before Kalecki's arrival in Britain he had become aware of the methodological problems of trying to isolate economic variables and combine them into determinate systems. His excursion into this methodological territory was 'Three Systems' a rather obscure paper whose lack of immediate theoretical or practical consequence meant that it had to wait until the 1990s to see publication in English (Kalecki 1934). The paper is an exercise in showing how economic variables may be combined into determinate systems under different assumptions. His first system is a two sector (consumption and investment) barter economy with a given level of investment (corresponding to Keynes's short period) in which it is easy to show that, with flexible prices, the economy comes to a stable equilibrium at full employment. In the second system money is introduced, with an elastic supply in accordance with the demand for money. As a result the economy tends to either inflation or deflation, depending on the initial state of either over-full employment, or under-employment. However, if the interest rate is allowed to rise with inflation and fall with deflation, then a stable equilibrium at full employment may be eventually reached. In his third system investment is allowed to vary autonomously, i.e., it is not fully regulated by the rate of interest. This then leads to fluctuations in economic activity until the volume and structure of the capital stock is constant.

Kalecki's conclusion underlines his methodological rather than analytical concerns: '... we have only examined the formation of equilibrium ... within the already existing capital equipment. ... Investment activity ... (will result in) a continual movement through a series of equilibria ... until the final equilibrium is attained. ... If we consider the time of construction of new investment goods ... it may also turn out that ... the position of final equilibrium will never be attained... these are proper business fluctuations.' (Kalecki 1934, pp. 218-219).

In other words, there is no actual determinate equilibrium in a capitalist economy, but a series of constantly changing variables. This is reiterated in Kalecki's remarks about Keynes's analysis in the *General Theory* of movements between one short period and another. (Kalecki's comments were published in Polish and hence possibly unknown to the Cambridge milieu in which he worked in 1939):

'Let us suppose that, in the original situation expected profitability was higher than the rate of interest and that investment increases. This generates such a rise in the prices of investment goods that the expected profitability, calculated on the basis of these new prices and of the expected incomes in the *initial situation*, is equal to the rate of interest. Now we must take into account the fact that the growth of investment not only generates an increase in the price of investment goods but also, according to ... Keynes's theory ... stimulates a general recovery, producing a rise in prices and output in all sectors. However, because, as Keynes holds in another part of his book, 'the facts of the existing situation enter, in a sense disproportionately, into the formation of our long-term expectations', the expectations will become more optimistic and a difference between the marginal efficiency of investment and the rate of interest will arise again. 'Equilibrium', then, is not reached, and the growth of investment will still persist (we are dealing here, as may easily be seen, with a cumulative Wicksellian process).' (Kalecki 1936, pp. 230-231).

Kalecki was here clearly analysing movements in what Joan Robinson had called 'historical' time, as opposed to the 'logical' time in which Keynes had couched his *General Theory*. But Cambridge remained wedded to the Marshallian tradition of treating sub-systems of variables in logical time. For Cambridge, 'dynamic' analysis in 'historical' time meant shifting from one closed sub-system of variables (with factors such as the capital stock assumed constant or irrelevant) to another closed sub-system in which the capital stock was allowed to vary, but other factors such as relative prices and competition were taken as constant (notably in Joan Robinson's 'extension of Keynes's short-period analysis to long-run development, Robinson 1956, p. vi). This provides the clue to the methodological enigma that Keynes, Joan Robinson, Kahn, and all those Cambridge sympathizers of Kalecki found in his work. The notion of a circular flow of income integrating prices and economic decisions was as foreign to economists brought up to believe that the complexity of an economy could be made tractable or calculable by having individuals make economic decisions simultaneously in different periods, as the German economic literature was in Cambridge. Kalecki's refusal to fit price theory into Marshallian methodology condemned his Cambridge research.

7. Summary and Conclusions

At the end of 1939 the research project which Keynes had set up to provide employment for Kalecki and establish applied economic research in Cambridge, was discontinued. Kalecki moved from the heart of the Keynesian revolution to Oxford, where a congenial theoretical void appeared with the absence from Oxford of Keynes's chief supporters there, J.R. Hicks and Roy Harrod. Keynes continued to support Kalecki's work. But Kalecki was no longer part of the Cambridge circle that had played such an important part in the development of Keynes's ideas.

Kalecki's move from Cambridge was the result of more than just an incompatibility of personalities. The move came about as a result of a much more fundamental incompatibility of a sub-culture in Cambridge economics that resisted external influence and engaged with foreign ideas in order to find confirmation of its approach to economic analysis through the methodology of Alfred Marshall. That resistance is apparent from an examination of the impact of three engagements with the economic methodology that dominated central European economics in the inter-War period. The first engagement, with the arrival in Cambridge of Piero Sraffa, resulted in Sraffa's diffidence towards the economic theory discussed around him, or even with him, in the case of Keynes's work, and Sraffa's concentration on his Ricardian value project. The second engagement was the clash with Austrian theory, advanced by Hayek at the London School of Economics. The Austrians' policy quietism in the face of the Great Depression facilitated a successful disengagement by Keynes's Cambridge supporters.

The third engagement came with the arrival in Cambridge of Michał Kalecki. With shared elements of theory and policy it was easy at first to overlook the methodological incompatibility between the dynamic business cycle framework within which Kalecki had always worked, and Marshallian periodic equilibria. When Kalecki put forward changes in income and expenditure, rather than stylised changes in supply and demand, to explain shifts in prices and employment this proved too much for his Cambridge supporters. Cambridge was and remains resolutely hostile to business cycle analysis.

References²

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² References to papers in the Kalecki archives are given as PAN followed by the file and page number. The Keynes and Kahn Papers in the Archive Centre of King's College Cambridge are referred to as by their class mark (e.g., N15 or RFK) followed by their file and page number.

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