WORKING WITH ARCHIVES: CAMBRIDGE ECONOMICS THROUGH THE MAGNIFYING GLASS

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La investigación en economía empleando archivos documentales permite interpretaciones más claras sobre las ideas y desarrollo de las mismas a través del tiempo; el contexto de escritura en relación con interlocutores y antagonistas; y la naturaleza de los problemas abordados. En este documento se presentan ejemplos de trabajo con borradores, correspondencia, tablas de contenido, notas, y material relacionado, de cuatro economistas del "Grupo de Cambridge": Piero Sraffa, Richard Kahn, Joan Robinson y John Maynard Keynes. En cada caso se describen los hallazgos e importancia de la investigación realizada en los archivos.

Palabras clave: pensamiento económico, historia económica, P. Sraffa, R. Kahn, J. Robinson, J.M. Keynes, heurística.
JEL: B10, B20, B31, B24.

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Research in economics using documentary archives provides clearer interpretations about the ideas and their development throughout time, in the context of the writing process in relation to the interlocutors and antagonists, and the nature of the problems addressed. This document presents examples of works with tough drafts, correspondence, tables of contents, notes, and related material of four economists of the "Cambridge Group": Piero Sraffa, Richard Kahn, Joan Robinson, and John Maynard Keynes. In each case the findings and importance of the research done in the files are described.

Keywords: economic thought, economics history, P. Sraffa, R. Kahn, J. Robinson, J.M. Keynes, heuristics.
JEL: B10, B20, B31, B24.

Marcuzzo, M.C. (2012). Travail avec les archives : économie de Cambridge à travers la loupe. *Cuadernos de Economía*, 31(58), 5-21.

La recherche en économie à l'aide des fichiers documentaires permet de plus claires interprétations sur les idées et leur développement à travers du temps, le contexte de l'écriture par rapport aux partenaires et antagonistes, et la nature des problèmes abordés. Cet article présente des exemples de travaux avec brouillons, correspondance, tables des matières, notes et de matériel connexe, de quatre économistes du « Groupe de Cambridge » : Piero Sraffa, Richard Kahn, Joan Robinson et John Maynard Keynes. Dans chaque cas on décrit les résultats et l'importance de la recherche dans les archives.

Mots clés : pensée économique, histoire économique, P. Sraffa, R. Kahn, J. Robinson, J.M. Keynes, heuristique.
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PREMISE

The revival of scholarly interest on Keynes in particular and Cambridge economists in general has certainly been helped by the archival work which has been pursued in the last 25 years on the papers of Marshall, Keynes, Sraffa, Harrod, J. Robinson, Kahn and Kaldor and to a lesser extent of Robertson, Dobb, Kalecki, Meade, Stone and Goodwin. As to the other major protagonists of Cambridge economics, unfortunately there are no extant papers of Pigou and EAG Robinson's papers and notwithstanding the work done by his biographer they still need to be fully exploited. The shameful situation of the Hicks papers (still unavailable) have prevented to enlarge the scope of the investigation to an important interlocutor; on the contrary the availability of Hayek's papers, which have been thoroughly researched, have allowed us to grasp his complex relation with Keynes and Cambridge.

I want to show in which respect working with the archives has given us a far better understanding as the scope and method of Cambridge economics than reliance on published material alone. Archival research has allowed interpreters to have clearer insights into the development of ideas through time, their context in relation to interlocutors and opponents, and on the nature the problems addressed.

Ideas like any old artefacts come to us covered with incrustations due to the passage of time; therefore they have to be restored to their original form before any serious study of them is attempted. The drafts, correspondence, table of contents and related material are essential to this purpose.

THE CAMBRIDGE "GROUP"

The archives have revealed to us that the group of people of economists renowned as the "Cambridge school" or the "Cambridge Keynesians" (Pasinetti, 2007) should be best defined as a "group" rather than a "school", to denote not adhesion to a common body of doctrine but rather the idea of both cohesion and sharing (Marcuzzo and Rosselli, 2005). The Cambridge tradition of economics thus appears as a heritage to draw upon as an alternative to neoclassical economics, more of a legacy than a fully-fledged system of thought. Moreover, the label "Keynesians" is somewhat misleading: while the centrality of Keynes' ideas is beyond dispute, there is the parallel line of thought provided by Sraffa's revival of classical political economy, which J. Robinson was greatly influenced by in the later part of her life, and which some would consider as relevant to identification of the "Cambridge tradition of economics" as the line stemming from Keynes.

The pivotal figures of the Cambridge approach are Keynes, Kahn, J. Robinson and Sraffa, who shared in the physical space and lifestyle of Cambridge to an exceptional degree. The bond between them was intellectual partnership, a recognised common ground, dialogue and acceptance of criticism, although Sraffa, while exerting strong influence himself, was far less subject to the influence of the others.

Keynes gave form and finish to his ideas by submitting them to the others, his own contribution to the work of the others remaining far more modest. For him dialogue yielded the desired results only if it ran along the lines he traced out and apart from the occasional comments and consultation, it was hard to draw him out on other grounds, like the theory of value or imperfect competition. Indeed the fascination of his proselytising art and flair for creating consensus made him the central figure of Cambridge economics.

The relationship between J. Robinson and Kahn epitomises the kind of intellectual collaboration that was typical of Cambridge. In the first place it was a sharing of times and spaces, which also entailed a sharing of knowledge and the habit of exchanging ideas and mingling together. From the post-war period until the end of 1970s both had fundamental roles in shaping the Cambridge that attracted students and scholars in great numbers from all over the world.

Sraffa was involved in all the intellectually important happenings at Cambridge, but found no company along his solitary path in quest of an alternative economic theory. Although it was Keynes who drove drew him to Cambridge and both Kahn and Robinson attended his lectures, the impact of his criticism of the Marshallian theory and his efforts to gain acceptance for an alternative approach were surprisingly ineffectual. His suggestions -such as imperfect competition- developed in directions departing far from the approach which had inspired them.

THE CAMBRIDGE TRADITION

The archives have given us better access to the method, "style" and content of the type of economics associated with the Cambridge tradition[2] at the very core of which Keynes, Kahn, J. Robinson and Sraffa stand.

First of all, there is the framework of analysis provided by Marshall, the founder-father of Cambridge economics to whom institutionalization of the subject at the University was due. Here the archival work done by Tiziano Raffaelli, Peter Groenewegen and Simon Cook has been of paramount importance in framing Marshall's ideas in context.

Be it in the form of criticism, or of refinements or of extension, the approach taken by Marshall to the multifarious aspects of economic life was taken by this group as point of reference. While Marshall praised market mechanisms, albeit with many qualifications and footnotes to the contrary, the path opened in their own ways by Keynes and Sraffa was to expose the shortcomings of both the trust in markets and the faith in market theory inherited by Marshall.

[2] "The Cambridge tradition of the equality of intellects, arrival at the truth through discourse and the careful nurturing of the minds of the young, encouraging without patronizing and guiding without compelling" (Sen, 2003, p. 54).

Secondly, there was the challenge to provide alternative explanations of and remedies to these failures (Marcuzzo, 2012). While Sraffa pursed the goal of exposing Marshall's inconsistencies arising from his method of representing the equilibrating forces of the market with a pricing mechanism of goods and factors of production based on marginal magnitudes, Keynes was more concerned with the inconsistency of expecting full employment of resources in the aggregate from individuals maximizing either utility or profit.

Kahn followed Keynes in accepting the Marshallian basic postulate of individual behaviour and questioning the implication that there was always a level of effective demand sufficient to sustain full employment and, like Keynes, looked for institutions and schemes to intervene in the market in the public interest.

While remaining a fierce Keynesian fighting against the "bastard" (as she called them) progeny throughout her life, Joan Robinson was seduced by Sraffa's arguments favouring the classical (and Marx's) political economy as better equipped to explain capitalism, and took a more radical stance than the others in politics as well in academic debates.

In sum, the papers not only shed light on the life and work of each individual author, but presents them as a group within the physical and metaphorical *place* which was Cambridge (Marcuzzo, Naldi, Rosselli, and Sanfilippo, 2007). By allowing us to look at them pair-wise, as it were, we are able to focus on their collaboration in various intellectual achievements. The two Cambridge Revolutions (imperfect competition and effective demand) are just two examples of their common challenge to the idea of smooth working of the markets and common pursuit to policies of intervention.

In what follows I will give four examples, drawn from work I and other people (Annalisa Rosselli, Nerio Naldi, Heinz Kurz, Anna Carabelli, Toshiaki Hirai, Brad Bateman) have done to show the importance of archival work in understanding Cambridge economics. The catalogues of the papers of the major Cambridge figures, Keynes, Sraffa, Kahn and Joan Robinson are available on line and their content has been reviewed in the literature. I will just give a few reference.

A very interesting account of Keynes's papers can be found in Cox (1995), the archivist of King's college who prepared the catalogue of them. The content of Sraffa's papers have been described by Pasinetti (2001), Kurz (1998), Smith (1998, 2012); the papers of Kahn and Joan Robinson by Marcuzzo and Rosselli (2008).

SRAFFA AND "MARGINISM"[3]

My first example, working with notes and related matters, is Sraffa's arguments against the marginal method in his unpublished writings.[4]

[3]I draw on Marcuzzo and Rosselli (2011).

[4]References are to the catalogue of Sraffa's papers conserved at Trinity College, Cambridge.

The use made of marginal method in economics (or 'marginism' as he often called it) which is the method upon which Marshall's demand and supply functions of goods and factors are constructed was a specific target of Sraffa's criticism, a constant element in his thought. His papers show his unending quest for measurable and observable entities in economics in his unpublished papers, and the reasons why he believed that marginal method did not satisfy his criteria of scientific method. Sraffa was in quest of a measurement criterion valid in theoretical analysis that should prove univocal, in contrast with the approach in statistical investigation, which requires some arbitrary choices as in the case of selecting weights in constructing index numbers.

In his 1929 notes, which were written he was working on the equations that would become *Production of Commodities by Means of Commodities*, we find the claim that in economic theory there are three types of magnitudes. First are those that are observable and measurable in production and consumption processes (acres of land, tons of grain, etc.).

Secondly, there are the purely subjective magnitudes (such as sacrifice or utility), which are not observable and cannot be measured. This is not to say that for Sraffa there are no subjective or purely mental forces at work in the real world of economics (such as expectations, beliefs, or motivations), but only that these 'magnitudes' are neither observable nor measurable.

According to his notes of Winter 1927-8, in this distinction between the two types of magnitudes lies the difference between classical political economy and modern economics; the former dealt with:

> 'material things' that have existed in the past. Modern economics deals with the second class, i.e. hopes for the future, such as utility, abstinence, disutility, etc.; these things, it must be noticed, refer only to the foresight of future acts(D3/12/10/61 (1)).

Finally, there are the third type of magnitudes which are features of controlled experiments only. All marginal magnitudes, together with the demand and supply curves which are built upon them, belong to this class. They share the nature of being a sequence of mutually exclusive alternatives which do not exist *at the same time*. For example, we cannot observe the same piece of land simultaneously cultivated by n and $n + 1$ units of factor, as we cannot observe two alternative quantities supplied (or demanded) in the same instant. As Sraffa noted in October 1929:

> [...] [demand and supply curves, marginal productivities, which form the basis of Marshall's theory, (or, rather, Pareto's)] do not exist at any one moment, nor during any period of the recurrent steady process of production and consumption. They are alternatives, only one of which can exist in anyone position of equilibrium, all the others being thereby excluded [...] Therefore they cannot be found by merely observing the process or state of things, and measuring the quantities seen. They can only be found by experiments[...].

> But the experiments have an entirely different significance: they actually produce facts which would otherwise not happen at all; if the experimenter did not step in first to produce them, and then to ascertain them, they would remain in the state of 'unknown possibilities', which amount to the deepest inexistence (D3/12/13/3).

Thus Sraffa not only questioned the legitimacy of using magnitudes in economic theory, such as marginal magnitudes, whose existence has to be brought about by experiment, but also the feasibility of the experiment required. He wrote in Oct 1929:

> These experiments cannot be carried out (and never have been, as a matter of fact) for various reasons: 1) the practical difficulties, 2) the lack of definition of the conditions to be required, which are always summed up in the absurd 'other things being equal'(D3/12/13/3).

These observations have a general application to social sciences, but Sraffa's contribution consists in questioning the validity of reasoning at the margin specifically in economics.

In the case of marginal productivity theory, by means of the "experiment" measurement is made of the product obtained with $n + 1$ units as compared with that obtained in the preceding instance with n units, but what we measure is only the difference between two products. Can it really be claimed that the variation in production brought about by the "experiment" measures the marginal productivity, or in other words, the contribution to production attributable solely to the $n + 1th$ unit of the factor? It might be, perhaps, if the *ceteris paribus* condition held, but it is precisely this condition that Sraffa considered highly questionable. How can we "disentangle" the contribution of the additional unit? What we observe is only the difference between two products and not the marginal product (or the marginal utility) of the marginal unit.

Moreover Sraffa held that change in economic realities hardly ever manifested itself in the form of infinitesimal variations in magnitudes that leave the overall structure unchanged. His criticism of the continuity hypothesis does not concern the hypothesis of infinite divisibility of the factors, but the 'logical' possibility of continuity, or in other words the possibility that by continually varying the quantity of a factor, both the product and the proportions between the factors go through continual variation.

However, study of variations does find a place in Sraffa, since he does not rule out scientific analysis of observable 'margins' –the elements, that is, that Ricardo takes into account in determining extensive rent.

Over the years Sraffa refined and honed his critique as the focus of his analysis shifted towards construction of his own theory, which did not require marginal magnitudes to determine prices. *Production of Commodities* demonstrates the non-necessity of marginal analysis by constructing a counter-example. To Sraffa's mind, however, this was only the 'prelude' to the critique proper, as the subtitle to his work clearly indicates. The fully formulated critique, based on the arguments reconstructed here, was to follow. One of the many drafts of the Preface, bearing the date of 14 December 1957, makes so much quite clear:

> The sketch now submitted to the public has grown out of a projected critique of the marginal theory of production and distribution, to which it was to serve as foundation. It has however seemed better to publish this material by itself so that it may be judged on its own merits. The critique may, or may not, follow later(D3/12/46/29).

Thus we have, it seems, ample confirmation that the arguments against marginism were formulated above all in the unpublished writings, and that the part of his critique that Sraffa chose to publish in explicit form was very small. All these writings have remained unpublished, raising the question of why exactly Sraffa was not satisfied with them, but casting no doubts on the strength of his convictions nor of the importance of archival findings.[5]

KAHN ON THE SHORT PERIOD[6]

My second example –everybody's dream in working in Archives, i.e. the discovery of an unpublished manuscript- is Richard Kahn's draft of a book on the short period, based on his Fellowship dissertation of 1929.

The main point of Kahn's Fellowship dissertation, "The economics of the short period" which remained unpublished for many years[7], was to prove that *in the short period* an equilibrium at less than full capacity may arise, in the event that the fall in demand is not expected to last, when the market is imperfect.

From Marshall's definition of short period - as the situation in which machinery and the organization of production are assumed to be constant –Kahn drew a further implication. The possibility of considering them as constant from the point of view of the short period arises from the fact that in both cases the decision to alter them is the same and depends on whether demand conditions are or are not considered "normal". Accordingly, depending whether changes in demand are believed by entrepreneurs to be transitory or permanent, as compared with the level considered as normal, the decisions to modify the plant or the organization will or will not be taken. Although short period cannot be "shorter" than the length of the productive process or longer than the time necessary to modify productive capacity, the time necessary to modify productive capacity depends not only on technological factors, but also on prevailing conditions –depression or boom– which mould expectations regarding the return to "normal" conditions of demand.

[5]Garegnani (2005) provides another example of the usefulness of archival evidence to date Sraffa's "turning point" in his understanding of classical political economy.

[6]I draw on Marcuzzo (1996).

[7]Published in Italian in 1983 and in English in 1989.

In the following two or three years –when the 'multiplier' was also developed– Kahn presented his thoughts on short-period economics in a book bearing the same title as the dissertation which remained unfinished and is still unpublished.[8]

The nature of the short period is seen not as a conceptual experiment but as a question of fact. namely that the life of fixed capital is considerably greater than the period of production (Kahn 1932: ch. 2, p. 2; 1989: xiii). If there were a complete range of continuous variation in the lives of the different means of production. the notion of short period could not be employed. But, in reality, as far as the range of variation is concerned:

> Between raw materials. on the one hand. and productive plant, on the other hand, there is a desolate and sparsely populated area. As a general rule, the life of physical capital is illustrated either by the mayfly or by the elephant (Kahn, 1989, p. xiii).

One aspect of the rationale for an "economics of the short period" is therefore, rooted in the nature of the production process, which gives meaning to a time interval where productive capacity is given and only its utilization varies. In fact, there are changes that occur rapidly (such as output and employment) and others that occur only slowly (such as alterations in fixed plant) (Kahn, 1989).

The other aspect characterizing the short period is rooted in expectations of changes in demand relative to the level perceived as "normal". The level of demand which individuals take as 'normal' is the benchmark against which observed variations are evaluated and expectations about its future course are formed.

The two aspects are combined to explain why in the short period productive capacity is not altered. This is so because a change in the conditions of demand is not perceived as permanent. In fact, the 'ideal' short period is defined as a situation where "any change that occurs is not expected to be permanent" (Kahn, 1989, p. 10). In a depression, however, short period equilibrium implies expectations that demand will return to its normal level, since suspending production or reducing the productive capacity to zero would require the belief that demand continued to be permanently low. In a boom, by contrast, short-period equilibrium implies that expectations are such that an increase in production is preferred to building up capacity until the increase in demand is perceived as "permanent".

Since what matters are expectations about the normal values of certain variables, in particular the level of demand, it follows that the short period need *not* be a 'short' time interval or only a temporary state before the long period forces work out their effects. It is rather a position which is maintained as long as the set of decisions, depending upon the expected values of selected variables, does not change (Dardi, 1996).

[8]One draft is extant, with annotations and related material, amounting to roughly 300 pages. Of the planned eleven chapters, according to the index, chapters 1, 3 and 4 remained unwritten, while 7, 9 and 10 are seemingly unfinished. The draft was most certainly written in the last quarter of 1932. References are to Kahn's papers preserved in the Modern Archives of King's College, Cambridge.

When the demand for an industry's output alters, according to Kahn, there are changes where responsiveness is immediate and changes where responsiveness is slow, but there is no continuous range of variation between 'responsiveness' and 'irresponsiveness'.

Changes that occur rapidly (output and employment) "do not very much depend on what has occurred in the past or what is expected to occur in the future" (Kahn, 1989, p. 12). They are reversible changes as opposed to changes 'which by definition have not time to occur in the short period [and which] depend on what has occurred in the past and what is expected to occur in the future' (Kahn, 1989, p. 12).

In conclusion, according to Kahn, we have causes which have different effects according to whether they are *perceived* by economic agents as permanent or persistent on the basis of what is believed to be the normal value of a given variable. However, if what matters is the divergence between the expected and the 'normal' values of selected variables, the crucial question becomes whether one still needs a theory to explain how these normal values are determined or whether we can take them as being characterized simply by their repetitiveness.

The archival findings have helped to trace the building blocks of what a few years later Robertson, was to refer to, in a letter to Keynes, as "your and Kahn's s[hort] p(eriod] method" (Keynes, 1979, p. 17).

JOAN ROBINSON'S IMPERFECT COMPETITION[9]

My third example, using published and unpublished correspondence[10], is about J. Robinson's imperfect competition approach in her 1933 book.

We do not have archival findings on Robinson's attendance of the lectures on *Advanced Theory of Value* which Sraffa gave in Cambridge between 1928 and 1931, but indirect evidence only, viz.: i) a letter to Kahn where she mentioned it ("I owe in fact far more to Piero's lectures and private conversations than I owe to any of Gerald [Shove] outside his published works"; JVR to RFK, 7 April 1933, RFK papers 13/90/1); ii) a reference in one of her published work ("When I returned to Cambridge in 1929 and began teaching, Mr. Sraffa's lectures were penetrating our insularity", (Robinson 1951: vii)); iii) EAG Robinson's account ("Joan had got to know [Kahn] as a fellow participant in Piero's Sraffa's very unorthodox lecture course)" (Robinson, EAG, 1994, p. 7).

In Sraffa's lectures the outline of the research project –albeit only the destruens part– that would occupy him for the following 30 years was clearly laid down;

[9] I draw on Marcuzzo (2005).

[10] J.V. Robinson's papers are preserved in King's College, Cambridge. Reference are given to the catalogue.

his main point being that there were two distinct theories of distribution -classical and marginalist; the latter alone was the foundation of demand and supply analysis. Although both his 1925 and 1926 articles are often referred to, the scope of Sraffa's lectures is much wider, reflecting the extensive work in which Sraffa had been engaged, probably since the Summer of 1927, which would become the core of *Production of Commodities*.

It is clear that Sraffa's lectures had no impact on Robinson, nor or any other pupils, for that matter, thus fulfilling Keynes's prophecy, contained in a letter to Lydia: "On Saturday I had a long talk with Sraffa about his work. It is very interesting and original - but I wonder if his class will understand it when he lectures" (JMK to LLK, 28 Nov. 1927; JMK papers: PP/45/190/3/268-9).

Her first two books (Robinson 1932, [1933] 1969) were a response to Sraffa's 1926 article alone, and in the extant correspondence with him, from 1931 to 1933, during the making of the *Economics of Imperfect Competition*, she made no attempt to address those wider issues which she must have been exposed to, attending the *Lectures*.

The letters exchanged between Robinson and Sraffa during that period deal with Sraffa's critique of the Marshallian value theory; we get a glimpse of the gulf existing between their positions in the comment by Sraffa accompanying the corrected proofs of the *Economics of Imperfect Competition*: "I have avoided raising 'broad issues' –it would be of no use to you at this stage, or indeed at any stage" (PS to JVR, 12 January 1933, JVR papers: vii/431/8).

She attempted to bring Sraffa round to her view –defended on the plane of methodology in her 1932 pamphlet– that there are cases "which made Sraffa's critique of Marshallian theory less forcible" (Rosselli, 2001). In that pamphlet, while Sraffa was cast as a "fundamental pessimist", she would label herself, together with Kahn and EAG Robinson, as an "analytical optimistic" –one who will make hypotheses known to be heroic, in order to be able to give formal treatment to an economic problem (Robinson, 1932).

On the plane of the theory, she sought to demonstrate that, if either factors heterogeneity or factor specialization were allowed for, the supply curve for a single industry –contrary to Sraffa's claim– could be raising. In the 1926 article Sraffa's point was that factor supply, although fixed in the system as a whole, may be considered infinitely elastic for an industry, since increasing costs are confined to the rare case of an industry in which there is a specialized factor employed exclusively by that industry. Robinson set out to find cases in which an industry uses a specialized factor and provided a classification of these cases, her argument being that since a priori intermediate cases between perfectly elastic and perfectly inelastic supply of a factor cannot be ruled out, and since they may effectively be found in reality, there was no reason why they should be dismissed as irrelevant.

Her approach is neatly summed up in the following passage from her letter to Sraffa:

> I am not trying to defend Marshall and his knife handles. I do not mind how few the cases of I[ncreasing] R[eturns] there are as long there are some on which I can use our ingenious analysis of monopoly under I[ncreasing] R[eturns] (JVR to PS, [1931], Sraffa papers: D1/86/2).

Sraffa's reaction was, as usual, much to the point:

> Your subdivision of the 'specialized factor' cases into groups is an interesting piece of analysis, but it does not in the least help to increase their number (PS to JVR, 31 May 1931, JVR papers: vii/431/1-3; Rosselli, 2004).

Against Sraffa, who had questioned the validity of deriving the supply curve for an individual commodity from the equilibrium of a firm in a given industry, in her Economics of Imperfect Competition Robinson argued that providing perfect competition and the independence of demand from costs are assumed, a supply curve can be derived: i) in the short run and quasi-long run, on the basis that for all firms marginal cost = price and price = equal marginal revenue; and ii) in the long-run, on the assumption that a normal profit can be defined at which average cost=average revenue. If competition is not perfect, very special assumptions are needed to derive the supply curve, namely that changes in the demand curve of the industry do not affect the individual demand curves facing each firm. She concluded that:

> It is [...] false to suggest, as some writers appear to do, that there is a mysterious difference between the mechanism by which supply price is determined when it is rising and when it is falling. The essential distinctions are not between rising and falling supply price, but between perfect competition and imperfect competition, and between an analysis in which time factors are admitted and an analysis in which they are ignored (Robinson, 1969, p. 129).

Sraffa had questioned the Marshallian assumption of perfect competition in the presence of increasing returns and the asserted independence of demand and supply schedules. Robinson did not take these points on board as implicating the abandonment of the Marshallian theory; she rather looked for an apparatus which could be made consistent with *ad hoc* –assumptions.

The 1931-33 exchange has helped us to highlight the dividing line between JVR's and Sraffa's approach which would have not surface into the published writings.

KEYNES ON THE CONTINUITY BETWEEN THE *TREATISE* AND THE *GENERAL THEORY*[11]

My fourth and final example is working with drafts, lecture notes table of contents and correspondence, is Keynes's journey from the Treatise to the *General Theory*. Throughout the process which led Keynes from the former to the latter book, he

[11] I draw on Marcuzzo (2003).

repeatedly claimed that the Treatise analysis was compatible with that of the *General Theory* and that he had made the new argument only "much more accurate and instructive" (Keynes, 1973a, p. 77).

In fact, the transition from the *Treatise* analysis, as presented in the Fundamental Equations and that of the *General Theory*, as incorporated in the principle of effective demand, required the introduction of new concepts and a change in definitions, which eventually made the latter approach quite distinct from the former. However, Keynes wanted his readers to believe that "under the surface [...] the essential ideas are the same" (Skidelsky, 1992, p. 442), and presented his new book as a "natural evolution" in his line of thought (Keynes, 1973a, p. xxii).

Working with archival material has allowed us to have a better grasp of the question. For this purpose the 5 years spanning from the publication of the *Treatise* to that of the *General Theory*, can be divided into three time-legs, which I have marked as Stage I, II and III. The first dates from comments and criticism on the *Treatise* (autumn 1930) to the early material for the new book and lectures (spring 1932). Stage II spans from the Easter Term 1932 lectures, which were attended by members of the "Circus", to the summer 1933, when the writing of the new book was well under way. The final stage runs from the 1933 Michaelmas Term lectures and the contemporary fragments of versions of the *General Theory*, when the principle of effective demand was clearly expounded, to the final touches to the proofs in December 1935.

In Stage I Keynes inherited a framework of analysis based on the Fundamental Equations, in which profits were the "main spring of change", through variations in the price levels of consumption and investment goods. As a consequence of much criticism within the "Circus" and by Robertson, Hawtrey and Pigou, with the main focus on the supposed independence in determination of the two price levels and the neglect of output as opposed to price adjustment, Keynes was led to forge new tools. In the Harris Foundation lectures we find the first "hints" (Moggridge, 1973, p. 79) of a move towards a different approach. During the summer 1931 and until he resumed lecturing in May 1932, he searched for the conditions for an equilibrium of output to occur, at less than full employment. His solution rested on what he believed to be only a "generalisation" of the old argument, but which was instead a switch of focus: from investment-relative-to-saving to expenditure-relative-to-income.

In Stage II lecturing on April-May 1932 Keynes presented his new argument as a "generalisation" of that of the *Treatise*:

> [...] fluctuations of output and employment for a given community over the short period [...] depend almost entirely on the amount of current investment. This goes beyond the contention of my Treatise, where it was meant to depend on the amount of investment relatively to saving [...] This less restricted generalisation is the result of taking account of the probable effect on saving of a change in the amount of investment (Keynes, 1979, p. 41).

This result was reached on the "presumption" (Keynes, 1979, p. 41) that changes in saving, following a change in investment, rather than offset, reinforce the effects of the change in investment on profit and output. In those lectures, windfall profits are the signals which induce entrepreneurs to revise their production decisions, but whether or not entrepreneurs are making profits is now made dependent on whether disbursements (i.e. expenditure) are greater than earnings.

Certainly, unlike the *Treatise*, we have here a mechanism preventing output (and/or prices) to falling to zero or infinity: "neither prices nor output will fall forever; and they will [...] come to rest again at some position from which they will have no further tendency to depart" (Keynes, 1979, p. 57). The mechanism is provided by the assumption that expenditure always increases less or decreases more than does income (Keynes, 1979, p. 38). However, saving is not yet fully integrated as dependent variable in the output adjusting mechanism.

Early in 1933, the changed political climate prompted Keynes to write 4 articles for "The Times" (published between the 13 and 16 March 1933) to give his new approach an airing and to relaunch a plan for public spending. These articles were subsequently published as a pamphlet, *The Means to Prosperity*. A further contribution came with the article *The Multiplier* (where the term later to become familiar made its first public appearance) published in "*The New Statesman*" of 1 April 1933).

Thus, a visible leap forward from the *Treatise* was accomplished in Stage II, with the crucial discovery of the income-expenditure approach, which provided the framework where the multiplier could be fully accommodated.

In Stage III Keynes reinterpreted his former approach based on the Fundamental Equations in the light of the latter, based on Effective Demand. By this way he claimed to have established compatibility between the two. The "expected increase of investment relatively to saving" as defined in the *Treatise* had become "a criterion of an increase in effective demand" (Keynes, 1973a, p. 78). So he felt confident to present the escape from his "old ideas" as continuity in his line of thought, granting that the exposition in the *Treatise* was "of course, very confusing and incomplete in the light of the further developments here set forth" (Keynes, 1973a, p. 78).

Throughout the writing of the *General Theory*, Keynes was at pains to make the new approach compatible with the *Treatise*. He managed to present his former approach as compatible with the latter by: a) re-interpreting profits of the *Treatise* "as determining the current expectation of profit"; b) presenting a change in the excess investment over saving of the Treatise as "a criterion" of an increase in effective demand. However, he must have had doubts that his attempted reconciliation was entirely successful, since he wrote in the Preface to the General Theory: "what in my own mind is a natural evolution in a line of thought which I have been pursuing for several years, may sometimes strike the reader as a confusing change of view" (Keynes, 1973a, p. xxii).

The archival evidence has allowed us to trace the twist and turns of his change of view.

SOME CONCLUSIONS

The scope for the history of economic thought is to review existing records and texts in order to provide evidence for interpretations and to explain developments of ideas. Unfortunately, the evidence is rarely unambiguous and interpretations are often constrained by the evidence available. Archives supplement and provide clues to the published role. Their role in filling the gaps in our knowledge of the personal and intellectual lives of the economists concerned in thus unquestionable. No significant biography can be written without spending long hours on documentary research, but what is their value in increasing our grasp of the *theories* of the authors concerned? How are we to answer the critics who view these activities as a sort of antique collecting?

There are, we would suggest, two legitimate answers. Firstly, theories should always be referred to their context. By context we mean the set of questions which framed them, the intellectual interlocutors to whom they were addressed and "the state of the art" at the time of their conception. Papers and correspondence afford insight into the motivations behind the choices of a particular set of questions, assumptions or tools. These are not always explicitly stated in the published version where the solutions discarded and definitions abandoned are left out.

Archives, as it were, allow us to travel the road towards rather than to visit the final destination.

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