2. Robinson and Sraffa

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1. Introduction

In this chapter I follow the evolution of Joan Robinson’s appreciation of Sraffa’s “critique of economic theory” over the years. My purpose is twofold: first, to construct a narrative based also on the archival findings; second, to shed some light on certain dividing issues—as we shall see in the final section—which impinged on the fortunes of the “Cambridge school”, in search of a clue to explain its “fall from grace” (Desai, 1985). The main conclusion of this chapter is that despite her participation in and continual reference to the critique of marginalist theory in her life, Robinson never adopted the Sraffian apparatus as her own and at least in one respect she misunderstood Sraffa’s point.

The chapter is organized as follows: Section 2 considers the archival evidence of the early influence of Sraffa and finds that Robinson did not accept his rejection of Marshallian theory. Section 3 follows the evolution of her independent trajectory, noting the influence of Marx on her view of “history v. equilibrium.” Section 4 shows that Robinson’s escape from Sraffa could not endure since she needed a theory of profit as a foundation of her own contribution. But as section 5 shows Robinson demanded early on that time be built into Sraffa’s critique in a way that it was not intended. The penultimate section probes the depth of the break with Sraffa and her alignment with Keynes in his emphasis of history. A concluding section sums up.

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2. This paper draws on Marcuzzo 2001, 2001a, 2003, and Marcuzzo and Sardoni 2004. The abbreviations used in the text are : LLK: Lydia Lopokova Keynes; JMK: John Maynard Keynes; JVR: Joan Robinson; RFK: Richard Kahn; PS: Piero Sraffa. The papers of JMK, JVR and RFK are kept in King’s College Modern Archives, Cambridge while those of PS are in Trinity College, Cambridge. References are given according to their respective catalogue numbers. Permission to quote from unpublished material is gratefully acknowledged to the Provost and the Fellows of King’s College and to P. Garegnani.
3. See also Harcourt (1986, p. 97): “towards the end of their lives they were to part on what was most damaging to their shared opponents, as Joan Robinson shifted the emphasis of her attack to one of methods rather than particular results, while Sraffa, or at least, the Sraffians, concentrated on the results obtained within a particular methodology.”
2. THE ANALYTICAL OPTIMIST

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”.

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, 1951a, p. vii);

iii) E. A. G. Robinson’s account: “Joan had got to know [Kahn] as a fellow participant in Piero’s Sraffa’s very unorthodox lecture course” (Robinson, 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; his main point was that there were two distinct theories of distribution, classical and marginalist; the latter alone was the foundation of demand and supply analysis. See, for instance, the following passage:

> For Marshall wages, interest and profits, are simply shares in the product; they are co-ordinate quantities, that can be regarded as acting upon the value of the product in the same way. It is not necessary for the actual goods which compose real wages and profits to be in existence at the beginning of the process of production—the hope, or the promise of these goods is equally effective as an inducement. ... Petty and all the classics, on the contrary, take the opposite view. They don’t regard at all wages as an inducement; they regard them as a necessary means of enabling the worker to perform his work.

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 on 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.”

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4 Letter from JVR to RFK, 7 April 1933 in RFK papers 13/90/1/221-4.
5 PS papers: D 2/4 3(22-23). See Marcuzzo (2001, p. 82).
6 Garegnani locates in the winter, 1927-28: “an initial (and decisive) turning point...and led to an examination of the classical economists with consequent abandonment of the Marshallian interpretation of them that had been behind the articles 1925-26” (Garegnani, 1998, p. 152). For a full account see Garegnani (2004).
It is very interesting and original—but I wonder if his class will understand it when he lectures.”

Her first two books (Robinson, 1932, 1969a) 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 his 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.”

She attempted to bring Sraffa round to her view, defended methodologically 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 E. A. G. 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; Harcourt, 1990).

On the plane of the theory, in her major book she sought to demonstrate that, if either factor heterogeneity or factor specialization were allowed for, the supply curve for a single industry—contrary to Sraffa’s claim—could be rising. 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 was 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[creasing] R[turns] there are as long there are some on which I can use an ingenious analysis of monopoly under I[creasing] R[turns]”

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,

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9 Letter from JVR to PS, 1931 in PS papers: D1/86/2.
but it does not in the least help to increase their number.”

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 equals price and price equals marginal revenue; and ii) in the long-run, on the assumption that a normal profit can be defined at which average cost equals average revenue. If competition is not perfect, there is no supply curve for the firm and very special assumptions are needed to derive the industry 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:

> 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, 1969a, 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 either. We conclude, then, that Robinson could not join Sraffa in his fundamental rejection of Marshallian theory; she rather looked for an apparatus which could be made consistent with Marshall, employing *ad hoc* assumptions to smooth the way. In this effort, Robinson began to develop a second, independent strand of Cantabrigian theory, one that would ultimately owe much more to Keynes than to Sraffa.

3. **Beyond the short period**

Although she openly disowned her *Economics of Imperfect Competition* only in 1953, as after 1934 she sidelined these issues, throwing herself entirely into the Keynesian Revolution.\(^{11}\) This she began by assisting Keynes in the transition from the *Treatise* to the *General Theory* and then set about “elaborating, popularizing and defending” its message.\(^{12}\)

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\(^{11}\)“The assumptions which were adequate (or which I hoped were adequate) for dealing with such questions [comparison of a price and output of a commodity under conditions of monopoly and of competition, demand and costs being given] are by no means a suitable basis for an analysis of the problems of prices, production and distribution which present themselves in reality.” (Robinson, 1960a, p. 22).

\(^{12}\)Preface to the German translation of the *Accumulation of Capital*; PS papers: I 101/5-6.
Beyond the short period

In doing so she relied on the apparatus she had devised with Richard Kahn and whimsically christened the “Trumpington Street School”.$^{13}$ One key concept was the elasticity of substitution between factors, defined as the proportionate change in the ratio of the quantities of factors employed divided by the proportionate change in the ratio of their prices (See Robinson, 1969a).

In her first attempt to extend the General Theory to the long period article she employed the elasticity of substitution to show that in the long period, the amount of employment is the result of “the contrary pulls of increased total output and increased output per head” (Robinson, 1937, p. 87) as measured by the elasticity of substitution of labour for capital and the elasticity of demand for output as a whole. The elasticity of substitution measures the change in the distribution of income, while the elasticity of demand for output as a whole measures the effects of the distribution of income on the propensity to save and therefore on the multiplier.

In the same article she also analyzed the effects of inventions on the distribution of income, i.e. whether innovation reduces the share of labor (reducing the equilibrium level of income by increasing thriftiness) or increase it (increasing the equilibrium level of income by decreasing thriftiness). She developed her analysis of inventions (Robinson, 1937, p. 95n) on the basis of a classification structured on the distinction between neutral, capital-saving and capital using technical change.$^{14}$ Harrod challenged Robinson’s definitions and Keynes sided with Harrod (Keynes, 1973, p. 174).$^{15}$ Keynes criticized her “elasticity substitution method” as ambiguous, without the provision of a precise measure of the volume of capital. In fact, the “elasticity of substitution method” is based on the assumption that the proportions between capital and labor is always such that the ratio between their marginal productivities reflects the ratio between their prices. It implies that the quantity of capital can be measured unambiguously, so that there is no difficulty in measuring its marginal productivity.

In an article she sent to Keynes to defend her position against Harrod she boldly claimed:

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$^{13}$From the address where Joan and Austin Robinson lived in Cambridge and where Richard Kahn was a frequent visitor. The “Trumpington Street School” was in fact how Kahn, Austin and Joan Robinson signed the 1932 Manifesto arguing in favour of the “method of supply and demand” (Keynes, 1979b, p. 43), to prove Keynes’s proposition that variation in investment had the same sign as variation in output. See Marcuzzo (2002).

$^{14}$In equilibrium, neutral technical change leaves capital per unit of product and the relative shares of labor and capital in a given output unchanged, while capital-saving and capital-using inventions reduce/increase capital per unit of product and reduce/increase the relative shares of capital. The reason is that capital-saving technical progress increases efficiency in producing capital goods more than in producing final goods, while the opposite occurs in the case of capital using innovation.

$^{15}$This occasioned an exchange between Harrod and Robinson which went on between May and June 1937 (Besomi, 2001).
For our present purpose capital must be conceived in physical terms, that is, as a stock of capital goods, and it is most conveniently measured in terms of cost units. Two stocks of capital are said to be equal if they cost the same sum to produce at a given date, in a given state of knowledge (Robinson, 1938, p. 139).

Keynes was not persuaded. In correspondence he made clear to her that “a great deal of difficulty arises” in her use of cost units to measure capital.\(^\text{16}\) She retorted that as far the measurement of capital was concerned she was not “any worse than the others”, adding that “Piero is devoting his life to the question, and we cannot expect an answer quickly.”\(^\text{17}\)

A year earlier Robinson had had an exchange with Sraff on the issue of the measurement of capital and she had been warned: “If one measures labour and land by heads or acres the result has a definite meaning subject to a margin of error: the margin is wide, but it is a question of degree. On the other hand if you measure capital in tons the result is purely and simply nonsense.”\(^\text{18}\)

For the second time Robinson was trying to escape Sraff’s criticism since he seemed unable to provide an alternative to what her “analytical optimism” would naturally lead her to. A new factor came in when, in 1940, she began to read Marx.\(^\text{19}\) Later she claimed that she “began to read Capital, just as one reads any book, to see what was in it” (Robinson, 1966, p. vi). M. Dobb was one of her tutors, but Kalecki was the main influence.\(^\text{20}\) She wrote that Piero Sraff used to tease her, saying that she “treated Marx as a little-known forerunner of Kalecki” (Robinson, 1966, p. vi).

By December 1941 “Marx on Unemployment” had already appeared in the *Economic Journal*, but her most substantial work on the subject, the

\(^{16}\)Letter from JMK to JVR, 27 September 1937 in JMK papers: CO/8/228-31.

\(^{17}\)Letter from JVR to JMK, 28 September, 1937 in JMK papers: CO/8/232-5. See Marcuzzo and Sardoni (2004.)

\(^{18}\)Letter from PS to JVR, 27 October 1936 in JVR papers: vii/431/14-5; see Rosselli (2004).

\(^{19}\)In fact she had already expressed in print her opinion on Marx in reviewing a book by J. Strachey in 1936, with no direct knowledge of the original texts. Among her papers, there is an unpublished note in which she recounts the episode: “In 1930 I published a review of a book by John Strachey, (brother of the more famous Lytton) who had set up as a popularizer of Marx – *The Nature of Capitalist Crisis*– I accused him of presenting the labour theory of value in terms of Say’s Law, ignoring Keynes and treating Hayek as the representative of academic economics. He replied that it was absurd for someone who had never read Marx to talk about him. We each felt that the other had made a fair point. He began to read Keynes and I read Marx.” (JVR papers: i/10.1). When writing her Strachey review, she was obviously anxious not to make a faux pas while he *General Theory* was not yet published, as we can infer from a letter by Keynes to her: “The point about Marx and Ricardo is, I assume, common knowledge; and I don’t think there’s anything in my book you need wait for before reviewing Strachey” (JMK to JVR, 15 September 1935, JMK papers: GTE/1/280-1; see Marcuzzo, 2001a).

\(^{20}\)See the exchange of letters between January and May 1941 in JVR papers: vii.
Essay on Marxian Economics, came out in 1942. The lesson drawn from the study of Marx in those years was later summed up by Robinson with the sentence: “For me, the main message of Marx was the need to think in terms of history, not of equilibrium” (Robinson, 1973b, p. x). The influence of Marx appears very clearly in the review of Harrod’s Towards a Dynamic Economics, published in 1949 in the Economic Journal, where she maintained that: “When you turn to the General Theory in the long period you have to start with Marx’s schema for expanded reproduction” (Robinson, 1973a, p. 253).

Thus, by the end of the 1940s, for Robinson developing a long-run analysis of accumulation meant developing an analysis “which has freed itself from the need to assume conditions of static equilibrium” (Robinson, 1980a, p. 105). It is noteworthy that she was looking into the classical tradition, in Marx rather than in Marshall, for a thread to follow. She had evidently concluded there was little in Marshall that would be of use to her in this project. My question is: what made her change her mind?

4. The stumbling block

According to her later reconstruction, when she had been trying to extend the Keynesian analysis to the long period, the stumbling block in the dynamic analysis consisted in “the lack of an adequate conception of the rate of profit” (Robinson, 1980a, p. 107). On the basis of short-run analysis, which Keynes was persuaded to adopt in the aftermath of the discussions within the Circus, there is no need for the notion of a normal rate of profit. She wrote:

If, as Kahn argued, there is a supply curve of output as a whole (given money wage rates) in a short period situation with fixed total productive capacity, then, corresponding to any given state of demand, there is a particular amount of employment, level of prices and flow of gross profits. There is no one level of profits that is more “normal” than any other (Robinson, 1978, p. xiii).

On the contrary, to determine the rate of profit it is necessary to define the value of the stock of capital, but at the time, she observed, “no one seemed able to do” (Robinson, 1978, p. xvi). She remarked that she...

...had innumerable discussions with Piero Sraffa but they always consisted in his heading me off from errors; he would never say anything positive. Thus it was not till I found the “corn economy” in his Introduction to Ricardo’s Principles

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21In her 1951 “Introduction” to R. Luxemburg’s Accumulation of Capital the need to return to the classical approach is clearly stated: “Academic economists have recently returned from the elaboration of static equilibrium to the classical research for a dynamic model of a developing economy” (Robinson, 1960b, p. 59).
that I saw a gleam of light on the question of the rate of profit on capital (Robinson, 1978, p. xvii).

What exactly did the “corn economy” reveal to Robinson? This is a question that has puzzled some commentators, to answer which it may be useful to present Sraffa’s reconstruction of Ricardo’s argument.

The corn-ratio theory, which Sraffa identified as “the rational foundation of [Ricardo’s] principle of the determining role of the profits of agriculture” makes it possible to determine profits “by the difference between product and capital advanced”. Since in agriculture, by assumption, corn is both the capital (the subsistence necessary for workers) and the product, the determination of the difference between total product and capital advanced “is done directly between quantities of corn without any question of valuation” (Sraffa, 1951, p. xxxi).

In the Principles Ricardo substituted the labour theory of value for the corn-ratio theory, allowing for the determination of the rate of profits on similar lines, “by the ratio of the total labour of the country to the labour required to produce the necessaries for that labour” (Sraffa, 1951, p. xxxii). However, as Sraffa has argued, a problem arises with Ricardo’s theory of profit because any change in distribution between wages and profits alters the relative values of commodities, including those produced with the same quantity of labour; thus unless a measure of value is found which would be invariant to changes in distribution, it becomes impossible “to measure changes in the magnitude of aggregates of commodities of different kind or, what is even more important, to ascertain its constancy” (Sraffa, 1951, p. xlix).

Ricardo was not able to resolve the problem of the invariant measure of value, but thanks to his corn-ratio theory he was successful, albeit “at the cost of considerable simplification” in rendering distribution independent of value (Sraffa, 1951, p. xxxii).

5. MISUNDERSTANDINGS

By the time Sraffa’s Introduction to Ricardo’s Principles came out, Robinson was ready for it. As we have seen since 1940 she had been studying Marx and

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22Sraffa appears to have sent a copy of the first proofs of his Introduction to the Principles, marked 15 November 1950, to Sergio Steve and Nicholas Kaldor, but not to Joan Robinson. In the Draft of the General Preface written in early December, the names of Joan Robinson and Richard Kahn appear as deleted in the acknowledgements. However, in the page-proofs, marked 7 March 1951, Sraffa first re-inserted the names and then deleted them a second time. According to his Diary entry of 20 March 1951 he had reached the decision after having consulted her: “Phoned Joan. (Says better not mention in Preface)” (PS papers: E 23). See Gehrke (2003).

23See for instance Gilibert, 1996, p. 123): “[The corn-economy model] is highly suggestive, and certainly shows extremely clearly the working of a surplus theory of profits, as contrasted with the usual equilibrium theory. However, it is difficult to see how it could lead to the arguments used by Robinson to question the possibility of finding suitable unit for measuring capital, this being made of a heterogeneous collection of goods.”
even earlier she had praised Kalecki’s approach to effective demand, which had been suggested by Marx’s Reproduction Schemes. The first evidence that Marshall’s equilibrium arguments were becoming increasingly irrelevant to her project is a collection of three short essays, *On Re-reading Marx*, of which she said they were “written in a hilarious mood after reading Piero Sraffa’s *Introduction* to Ricardo’s *Principles* which caused me to see that the concept of the rate of profit on capital is essentially the same in Ricardo, Marx, Marshall and Keynes” (Robinson, 1973a, p. 247).

In fact, far from being in a hilarious mood, Robinson was in the midst of one of her manic–depressive crisis, for which she was hospitalized for several weeks, when she wrote these essays. She explained to Kahn, who tried to persuade her not to publish them: “I want to have the family joke about Piero. I cannot pinch 20 years of his life’s work without acknowledgement and acknowledgement in a joke is the only way I can do it.”

In these essays she made two main points; the first is that, unlike the short period, in the long period capital changes and the question “what is the quantity of capital” cannot be avoided; the second is the inadequacy of supply and demand analysis in dealing with the passage of time—which is irreversible—as if it were a movement in space limited to only one direction.

The second piece of evidence of the change in her approach is her famous 1953 article, which is widely regarded as the starting point of the capital theory controversies. Since the question of Robinson’s priority in the criticism of neoclassical capital theory was raised, it may be helpful to quote what she wrote to Kahn in 1975: “It is true that I anticipated Piero’s publication but only because I had more or less worked it out for myself from his *Introduction to Ricardo’s Principles*—the corn economy.”

My next question is: what exactly did she work out?

In this article, the starting point is that, since in the long period we need to explain why a particular technique is chosen and the rate at which the stock of capital is altered, we must know how capital is measured. The neoclassical explanation is based on the postulate that the prices of factors of production are such that all their available quantities are employed. At a given wage rate that technique is chosen which maximizes the rate of profit; then the total amount of capital and the chosen technique determine the level of employment. However, she wrote:

...the condition that the given amount of capital employs the given amount of labour...entails a particular rate of profit.

But the value of the stock of concrete capital goods is affected by this rate of profit and the amount of “capital”

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24 In a 1948 article she had written: “What divides Marx’s theory from others is not at all the question of relative prices of commodities but the question of the total supply of capital and the rate of profit on capital as a whole” (Robinson, 1951a, p. 139).


that we started with cannot be defined independently of it (Robinson, 1978a, pp. 87-8).

Thus she pointed up the neoclassical failure to distinguish between changes in the conditions of producing a given output, when the quantity of capital is altered, from changes in the value of that capital, due to variations in wages and profits. The implication is that “different factor ratios cannot be used to analyse changes in the factor ratio taking place through time”, because in time the value of the quantity of capital may change as a consequence of a change in distribution and we will not be comparing the same quantities. She concluded that “it is impossible to discuss changes (as opposed to differences) in neo-classical terms” (Robinson, 1978a, p. 89).

Her approach is reminiscent of Sraffa’s interpretation of Ricardo’s failure to distinguish between differences in the value of a given output due to conditions of production and changes in the value of a given output due to variation in distribution. However, Sraffa refers to “the two points of view of difference and of change” (Sraffa, 1951, p. xlix) to clarify two different effects on the relative value of two commodities of different proportions or durabilities of capital employed in their production: “First, that of occasioning a difference in the relative values of two commodities which are produced by equal quantities of labour. Second, that of the effect which a rise of wages has in producing a change in their relative value” (Sraffa, 1951, p. xlviii).

Thus in the Introduction Sraffa takes the question of measurement of the quantity of capital to pertain only to the question of measuring “the magnitude of aggregate of commodities”. Since the prices of commodities depend on the rate of profit, there seems to be a change in the quantity of output to be distributed whenever there is a change in its value due to a change either in wages or in profits. Sraffa does not take it to pertain to the question of comparing two different aggregate of commodities at two different points in time, as Robinson seems to interpret it.

Robinson interpreted the “points of view of difference and of change” as pertaining to the question of comparison between two equilibria, with different amounts of capital; since variations in the quantity of capital cannot be ascertained independently from changes in the rate of profit, she drew the conclusions that equilibrium positions could only be compared as differences and never described as changes from one to another.

6. Dividing issues

There is some archival evidence that Sraffa perceived Robinson’s interpretation of his message as misleading in some respects. We have for instance a letter in which she refers to his objection to an issue raised in her Accumulation of Capital and a letter in which she openly declares her debt to him:27

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27“Now I see the point. We are a case of pot and kettle. The effects of differences in the rate of profit on value of capital in terms of a unit of surplus depend on: A. Differences
Dear Piero: all the work that I have been doing the last 10 years has been much influenced by you—both our conversations in the old days and by your Preface. When I went off my head I thought that the idea that I had seen in a blinding flash was yours, because it came to me in terms of Ricardo’s corn economy; but it was connected with TIME and it now appears is very much alien to your point of view (though to me it seems to fit perfectly well). Since, quite apart from worldly success, I have a lot of fun. I have a very deep feeling of gratitude to you. The fact that you reject it doesn’t affect the case at all. Yours, Joan

For many years she championed his approach and was indeed one of the most belligerent players in the capital controversy. However, she always defended the branch of neoclassical school that derived from Marshall, because unlike the branch that derives from Walras, it maintains “a sense of time” (Robinson, 1965, p. 101). This became the ground of disagreement with some of her allies in the battle against neoclassical economics. One point in particular became central in the discussion, i.e. the maintenance of a concept of a long-run equilibrium in the context of historical analysis:

I concede that we might imagine that we could detect the ghost of a long-run rate of profit that would correspond to the momentary actual situation if it were permanent (though it is necessarily in the course of changing)... But there are so many indeterminacies in such a calculation that probably anyone who attempted it would only get an answer corresponding to his preconceptions, whatever they might be. Meanwhile, it is the expectation of profits, at any moment, that is the operative force. [...] The very description of equilibrium implies that correct decisions were made in the past (Robinson, 1980b, p. 128).

Thus, it is the critique of the concept of equilibrium itself, and not only of neoclassical equilibrium, which she sees as the legacy from Keynes. The main line of attack on the neoclassical theory lies for her in the distinction between time patterns of different commodities; B. Differences in labour embodied. In my big book I played down A and put all the emphasis in B, which caused you to dismiss it as absurd” (JVR to PS, August 1967, PS papers: G7).


Garegnani retorted that the assumption of irreversibility in time is implicit only in the method of supply and demand analysis, whereby the tendency towards equilibrium is described as movements along those curves, while the same assumption is not made when comparing two long run equilibrium positions determined by a “classical” theory of prices and distribution. (Garegnani, 1979) Others have disputed his claim, because of the unwarranted assumption of the “gravitation” of market prices towards natural prices (For a review of the issue, see Schefold, 1997, pp. 386-97).
historical time and logical time (Robinson, 1979b, p. xiv), not between two alternative explanations of prices and distribution.

In this respect she found that Sraffa’s language in Production of Commodities is limited, because “there is no causation and no change” [and] “the argument is conducted strictly in terms of comparisons of logically possible positions” (Robinson, 1980d, p. 132). She felt it to be more promising to rely on Keynes who, “at the opposite extreme to Sraffa, discusses only events” (Robinson, 1980c, p. 139) and discusses them “in terms of processes taking place in actual history” (Robinson, 1979b, p. xiv).

7. Conclusion

It seems to me that the two strands in Cambridge economics, one stemming from Keynes and one from Sraffa, were never fully integrated in the work of Joan Robinson. And indeed the question is open as to whether in fact they can and should be integrated (See Pasinetti, 2003). Robinson attempted at least three times—with imperfect competition, the theory of employment in the long period and with a Post-Keynesian analysis—to integrate Sraffa’s points within a Marshallian and Keynesian framework.30 Twice she discarded her previous attempts as unsatisfactory, and finally fully endorsed Sraffa’s criticism of neoclassical capital theory. She unsuccessfully tried to convince Kahn that the gulf between Keynes and Sraffa could be bridged: “Cannot we agree on Piero’s prices for the long run and on Keynes’ prices for the short run and leave it at that?”31 Similarly, she unsuccessfully tried to convince the “Sraffians” that there was no incompatibility with Keynes’ theory. She never drew the inference, that the others were drawing, that this integration simply cannot be done. Thus, we may perhaps conclude that notwithstanding her bleak moments, she remained at heart an analytical optimist.

30“The classical tradition, revived by Sraffa, which flows from Ricardo through Marx, diluted by Marshall and enriched by the analysis of effective demand of Keynes and Kalecki” (Robinson, 1973c, p. xii).
31Letter from JVR to RFK, 25 May 1961 in RFK papers: 13/90.