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by

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1. Introduction

In 1973 *Economica* devoted its February issue to celebrating Sir John Hicks. On that occasion, Hicks's famous monograph *Value and Capital* (1939) was hailed as "the book that transformed economic theory" (p. 1). Nowadays *Value and Capital* (henceforth *VC*) is viewed as the starting point of the so-called neo-Walrasian research programme. From at least the early 1960s Hicks himself endorsed that qualification and recognized his own Walrasian affiliation at the time when he was writing *VC* (1960, p. 250, fn. 5; 1976, pp. 290, 296; 1983b, p. 85).

Yet, on re-reading VC, such parentage does not stand out so clearly. *VC* is subdivided into four parts, the first two devoted to 'statics' and the last two to 'dynamics'. One should expect that, at least in the 'static' parts of *VC*, Walras should be at the centre of the stage; but this is not really so. It is true that, soon after the beginning of Part II – General Equilibrium, Hicks (1939, p. 61) declares: "I shall follow Walrasian methods in considerable parts of this book". Yet, perusing the one hundred and ten pages of the 'static' parts, one immediately realizes that the pride of place goes to Marshall and Pareto; Walras is hardly mentioned, except when it is strictly necessary. And even when his name is mentioned, as it happens at the pages 57-61, where Hicks evokes the allegedly Walrasian equation-counting method for 'proving' equilibrium existence and alludes to Walras' Law, the reference is far from favourable: at p. 61, in particular, the "Walrasian system" is blamed for its "sterility", which is ascribed to the fact that "Walras did not go on to work out the laws of change for his system of General Equilibrium."

If one then moves to the 'dynamic' parts of *VC*, the situation is understandably even worse. In the two fundamental chapters introducing Part III, respectively called "The Method of Analysis" and "Equilibrium and Disequilibrium", there is room for Böhm-Bawerk, J.B. Clark, Cassel, Keynes, Knight, Pigou, and Wicksell, but not for Walras or any other economist of the "Lausanne School" (this being the name occasionally used by Hicks in those years to designate Walras, Pareto and their followers). Walras is only mentioned once, together with Edgeworth, in the two-page long Note to Chapter IX on "The Formation of Prices" (1939, pp. 127-9), which is almost entirely devoted to Marshall's theory of barter and temporary equilibrium.

Further, it is at least curious that the *VC* model, nowadays universally regarded as the point of departure of the neo-Walrasian programme, should have been called with a name, 'temporary equilibrium model', that is directly drawn from Marshall's, rather than Walras's, conceptual system.

As a matter of fact, the controversial relationship between Hicks and the Walrasian approach is not only exemplified by some passages to be found in *VC*, but is also instanced by the contents of almost all of Hicks's writings in the 1930s. For instance, in his 1933 paper in German, now re-published in English translation as (1980b), not only does Hicks criticize Pareto for his ambiguity about the equilibrium notion and his lack of any proper theory of capital, but he also attacks Walras for his confusion about the meaning of equilibrium, a confusion that is regarded as even more serious than Pareto's, and, as a consequence, for his mistaken theories of capital and money (1980b, pp. 29, and 33, fn. 13). Strong criticisms are raised against Walras's conception of equilibrium and

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his theory of capital also in Hicks's 1934 *Econometrica* article specifically devoted to Walras; but what is perhaps even more striking in this paper is the way-out suggested by Hicks in order to escape from the fallacies beleaguering Walras's theory: for he recommends a stationary interpretation of both the Walrasian equilibrium notion and Walras's theory of capital. Quite paradoxically, however, a few months later Hicks abandons the stationary equilibrium interpretation (1935b, pp. 68) and reproaches Pareto, among others, for allegedly interpreting the equilibrium concept in a stationary sense (1936a, p. 85).

The intricate relationship between Hicks and the "Lausanne School" in the 1930s, culminating in the ambiguous connotations of *VC*, has been indirectly acknowledged by Hicks himself, who has come back over and over again to those years, on the one hand to offer an inside reconstruction of his own intellectual history during that decade (1963, 1973, 1979a, 1980a, 1981b, 1982b, 1982c, 1982d, 1983b), and, on the other, to reinterpret, qualify, amend, and even disown and repudiate the *VC* model and the underlying method of analysis (1956, 1965, 1976, 1985).

The aim of this paper is to clarify the theoretical reasons behind the winding path followed by Hicks over the 1930s, especially as far as the Walrasian conception of equilibrium and equilibration is concerned: in fact, while Hicks's haziness about his theoretical affiliation in both *VC* and his later writings has been thoroughly investigated in the literature (see, in particular, De Vroey (1999b), (2006)), the roots of such ambiguity in his previous intellectual history have still to be discussed in detail. In Sections 2 and 3 we shall examine Hicks's ideas on the Walrasian approach and related matters in the early 1930s, focusing especially on Hicks's 1933 paper in German and his 1934 article on Walras: Section 2 will chiefly deal with the exchange model, while Section 3 will be mainly devoted to the models with production and capital formation. Section 4 will summarize the questions that are left open by the 1934 article. In Section 5 we shall analyse the evolution of Hicks's thought on Walrasian equilibrium and equilibration during the years of the gestation of *VC*, focusing especially on Hicks's two important 1935 papers (1935a and 1935b) and on his two reviews of Keynes's *General Theory* (1936b and 1937a). Section 6 will discuss the *VC* temporary equilibrium model. Section 7 concludes.

2. Hicks on Walrasian equilibrium and equilibration (1930-1934): exchange

Hicks's reflections on equilibrium date from his first theoretical paper on Edgeworth and Marshall (1930) and are central to all his other works of the early 1930s: the paper on uncertainty and profit (1931), the book on the theory of wages (1932), the paper on equilibrium and the cycle (1933), as well as the joint paper with Allen on consumer and demand theory (Hicks and Allen, 1934a and 1934b). Yet, since the focus of the present paper is on Walrasian equilibrium, it is convenient to start from the 1934 article on Léon Walras (called the 'Walras' paper in the following), where Hicks systematically confronts our main issue, going back to the previous writings when necessary.

The 'Walras' paper was commissioned to Hicks by the editors of *Econometrica* on the occasion of Walras's centenary and was meant to provide a general presentation of that author's ideas to the benefit of the readers of that journal. According to Hicks's later opinion, he did not do a good job, for in the paper "there [was] too much about the relation of Walras to Marshall, who naturally bulked large in the circles in which [Hicks] was moving" (1983b, p. 85). As a matter of fact, the place of Marshall in the paper is absolutely out of proportion, as can be seen, e.g., from the following passage:

> Indeed, the modern reader of Walras' *Eléments d'Economie Politique Pure* is struck by its affinity, not with the work of Jevons or Menger, but with that of Marshall. For a quite considerable part of the way Walras and Marshall go together; and when they separate, it is a difference of interest, rather than of technique, that divides them. (Hicks 1934, p. 338).

In the 1980s Hicks ascribed his juvenile overvaluation of the affinities between Walras and Marshall to the influence of the scientific environment from which he was then surrounded. This is certainly true, but it is not all the truth: for, in the whole of the 'Walras' paper, one perceives a systematic tendency on the part of Hicks to blur the differences between Walras and Marshall, a tendency that will persist over the years and will have momentous effects in *VC*, as will be seen in Section 6 below. In 1934, the most important instance of such tendency can be found in a passage where Hicks maintains that both Walras and Marshall, starting from the same "conception of perfect competition", traceable to "Cournot's analysis [of] Unlimited Competition", are able to build a theory of individual competitive behaviour that is at the basis of their "theory of exchange value generally":

> In the hands of Walras, this conception of perfect competition was converted into a special technique of using prices as economic parameters. Although this technique was used by Marshall as well, its very consistent employment is highly characteristic of Walras's work. (Hicks 1934, pp. 339-40)

Yet Marshall, unlike Walras, never assumed price-taking behaviour and never used "prices as economic parameters"[1]. Many years later, Hicks will recognise his mistake (Hicks 1983a, p. 88, fn. 7). In the 1930s, however, the blurring of the differences between the two economists was there to stay and to play an important role in Hicks's story.

According to Hicks, it is with the determination of the equilibrium conditions in the multiple exchange problem that, "for the first time, we have a characteristically Walrasian doctrine. What is it worth?" (1934, p. 341) Hicks's answer runs as follows:

> The types of equations used by Walras in determining exchange equilibrium are two; those which express the dependence of the amounts demanded and supplied by particular individuals on the system of market prices, and those which express the equality of demand and supply in particular markets. These two classes stand on very different footings. So far as the first class is concerned, they have become the essential foundation for the whole branch of economics to which they refer. [...]
> The second class, which expresses the equation of supply and demand in the different markets [...] has proved much more open to criticism. For it is on this class that the meaning of Walras' system of general equilibrium depends, and by far the most important divergence between Walras and Marshall turns on this point. (Hicks 1934, p. 341)

What Hicks has in mind in the last part of the last sentence may appear mysterious at first sight. But the meaning is clarified by the content of a footnote appended to the quotation, where the reader is referred to "Edgeworth's review of Walras in *Nature* (1889) and his controversy with Bortkiewicz in the *Revue d'Economie Politique* (1890-91). Also his comment in *Papers*, II, 311."

What the references in this footnote make clear is that the issue Hicks is concerned with is that of the establishment of equilibrium, that is, of the process by which an equilibrium state is arrived at in the exchange model: for the chief objection raised by Edgeworth against Walras's theory in his 1889 review of the second edition of the *Eléments*, an objection reiterated with almost identical words thirty six years later in the passage of Edgeworth's *Papers* (1925b) referred to by Hicks, concerns precisely Walras's view of the equilibration process, as expressed by his celebrated *tâtonnement* construct. That construct also represents the main bone of contention in the 1890-91 controversy between Bortkiewicz (1890), acting as Walras's spokesman in this contingency, and Edgeworth himself (1891a)[2].

[1] See Donzelli (2008, Section 4).
[2] See Donzelli (2009).

After recalling the Walrasian rule of price adjustment underlying *tâtonnement* in exchange ("if demands and supplies are not equal, prices will be changed until equilibrium is reached"), Hicks continues as follows:

> What, however, Walras does not make really clear is whether any exchanges do or do not actually take place at the prices originally proposed, when those prices are not equilibrium prices. If there is no actual exchange until the equilibrium prices are reached by bidding, then Walras' argument is beyond reproach on the score of logical consistency, though it may be called unrealistic. (The market then proceeds under Edgeworth's principle of "recontract", or provisional contract.) But if such exchanges do take place, then, in general, the final equilibrium prices will be affected by them. (Hicks 1934, p. 342)

To this passage a long footnote is appended, where Hicks quotes a well-known passage belonging to Lesson 5 of the *Eléments*, right at the beginning of the part on exchange, where Walras expresses his view that the markets which are best organized from the competitive standpoint are those in which transactions are centralized and prices are openly announced by professional agents (Walras 1988, p. 70[3]). This passage, however, has nothing to do with Hicks's dilemma and can consequently do nothing to dispel his doubts. Hence, not surprisingly, the footnote ends with the sentence: "This remains ambiguous" (1934, p. 342, f. 11).

Yet, all, or at least most, of Walras's ambiguities censured by Hicks would have been removed if the latter, instead of quoting the irrelevant passage on organised markets, had quoted another famous passage, immediately following the one referred to by Hicks, where Walras explicitly solves the dilemma in favour of the first alternative: for, in the context of a specific example concerning the market for a particular kind of securities (perpetuities) traded on the Paris Stock Exchange, he makes an explicit assumption to the effect that no trade is allowed to take place ("Théoriquement, l'échange doit être suspendu" or "Suspension de l'échange") up until equilibrium is reached (1988, p. 89, **2**-**4**).

One can hardly believe that this passage may have escaped Hicks's attention[4]. It is true that the few words expressing the 'no trade out of equilibrium assumption' were not present in the first edition of the *Eléments* and were inserted only in the second. But, since Hicks was then using the 4th edition, as he himself informs us (1976, p. 296, fn. 16), his neglect of the 'no trade out of equilibrium assumption' cannot be explained with its belated introduction. A more likely explanation is that Hicks was led to "forget" the few clarifying words added by Walras in the second edition of the *Eléments* because of his desire to stress the general dilemma underlying any equilibration process of the Walrasian type, as well as, perhaps, Walras's own ambiguities in this respect.

For Walras had certainly been very undecided and unclear about the interpretation of his *tâtonnement*: as far as the exchange model is concerned, Walras's original view had probably been a very realistic one, allowing transactions to take place out of equilibrium; as far as the models with production are concerned (i.e., the exchange and production model and the model with capital formation), from the very beginning Walras had envisaged a *tâtonnement* process where observable exchange and production activities are allowed to take place out of equilibrium[5]. While postponing

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[3] Since 1988 a comparative edition of Walras's *Eléments* is available, where the texts of the various editions of the *Eléments* (1st 1874-77, 2nd 1889, 3rd 1896, and 4th 1900) are collated and compared. When quoting from or referring to the *Eléments*, we shall use the comparative 1988 edition. All references will have the following form: (Walras 1988, page number(s), edition number(s) in bold). When no edition number is specified, as in the reference above, this must be taken to mean that the text remained unchanged over all the editions.

[4] In effect, it did not escape the attention of Kaldor, who, at about the same time as Hicks, put forward an interpretation of Walras's *tâtonnement* in exchange where due account is paid to the 'no trade out of equilibrium assumption' (Kaldor 1934, p. 126).

[5] See Donzelli (2007).

an examination of the models with production to the next Section, we discuss now the issues related with the equilibration process in the exchange model.

As is well-known, in 1883 Bertrand criticised a version of the exchange model contained in the second edition, then recently published, of Walras's *Théorie de la richesse sociale* (Walras 1993). Bertrand's contention was that, owing to the actual carrying out of observable transactions during the equilibration process (this being Bertrand's interpretation of *tâtonnement* in exchange), the data of the economy would change during the process, so that no prediction could be made of the equilibrium eventually reached and the model would turn out to be indeterminate.

Walras reacted to this attack at first in 1885, by explicitly introducing a 'no trade out of equilibrium assumption' in an obscure paper on an entirely different subject (Walras 1885, p. 312, fn. 1), and then in 1889, by inserting the above mentioned words in the relevant passage of the second edition of the *Eléments*. Yet, apart from that insertion, the whole structure of Walras's reasoning, as far as the exchange model is concerned, remained unaffected. So that in 1934 Hicks, though formally incorrect, had after all some substantive justification in signalling the existence of a problem that Walras had simply swept under the carpet with his 'no trade out of equilibrium assumption'[6].

After describing the dilemma confronting Walras as far as the equilibration issue is concerned, in his 'Walras' paper Hicks went on to explain how Marshall had tried to face the same issue from a different perspective:

> Marshall's way out of this dilemma was to concentrate on a particular market, where he could show that if the marginal utility of one of the commodities exchanged could be treated as constant, then the final rate of interchange would be independent of the path followed to reach it. But this solution – which is, after all, only a very particular solution – is usually not available in the case of General Equilibrium. (Hicks 1934, pp. 342-3)

Hicks meant here to refer to Marshall's theory of barter and its generalisation to the theory of market or temporary equilibrium, respectively discussed in Appendix F. Barter and Chapter 2, Book 5 of Marshall's *Principles of Economics* (1961a, pp. 791-3, 331-6). Marshall's theory of barter had also been the subject of a controversy between Edgeworth, (1891b) and (1891c), and Berry (1891), acting in this contingency as Marshall's spokesman[7]. In 1934 Hicks was well aware of this debate, for it had been at the centre of his 1930 paper on the indeterminateness of barter. Moreover, in his article with Allen on the theory of demand (Hicks and Allen, 1934a and 1934b), Hicks had also arrived at restating Marshall's assumption of a constant marginal utility of either one of the commodities exchanged (in the "theory of barter") or money (in what Marshall's calls the "theory of buying and selling", i.e., the usual Marshallian theory of a "particular market" where an ordinary commodity is traded for money) in terms of the newly worked out conceptual apparatus, based on marginal rates of substitution and income elasticities of demand: specifically, he had been able to show that the Marshallian property of a constant marginal utility of either one of the traded commodities or money is equivalent, in the new terminology, to the property of a zero income-

[6] It is worth noting that Keynes, to whom Hicks had sent a copy of the 'Walras' paper, wrote back to Hicks that, in his opinion, Walras had certainly had in mind the first alternative: Keynes's conjecture was not based on a superior knowledge of Walras's writings, but on his empirical acquaintance with the method followed at the Paris Stock Exchange for arriving at equilibrium prices, a method coinciding with Hicks's first alternative, and on his persuasion that Walras couldn't but be aware of what was going on at the Paris Stock Exchange (Hicks 1976, p. 296, fn. 16). Even if we now know that Walras's reason for explicitly introducing the 'no trade out of equilibrium assumption' was theoretical, rather than empirical, we must also add that Keynes was right in his conjecture: for Walras had indeed been aware of the rules governing the price adjustment process at the Paris stock Exchange, including the 'no trade out of equilibrium' rule, as he had made clear in an applied paper published in 1880 (Walras 1880, pp. 408, 432). Hicks will duly take account of Keynes's remark in *VC*, changing his interpretation of Walras's passage quoted in the 'Walras' paper accordingly (Hicks 1939, p. 128, fn. 1).

[7] See also Marshall (1961b, pp. 791-8).

elasticity of demand (a zero income-effect) for either the other commodity involved in the barter or the commodity proper exchanged against money (Hicks and Allen 1934a, pp. 64-5).

In the passage from the 'Walras' paper quoted above Hicks appears to rule out the possibility of extending Marshall's special solution of the equilibration issue from a "particular market" to the multiple-exchange case characteristic of "General Equilibrium". Yet, the results reached just a few months before in the Hicks and Allen paper had apparently been more favourable to such extension: for, "if the marginal utility of one commodity out of many is constant, the income-elasticities of *all* the rest will be zero"[8]; and this would seem to suggest that Marshall's solution is not necessarily confined to his partial equilibrium framework. However, since the possibility of extending Marshall's solution to a "General Equilibrium" multi-commodity context will play a paramount role in Hicks's construction of his *VC* temporary equilibrium model, we put provisionally aside this issue, postponing its discussion to Section 6 below.

Up to this point in the 'Walras' paper, in examining the equilibration issue with exclusive reference to the exchange model, Hicks had explored three alternative 'solutions', all of which had apparently turned out to be unsatisfactory. The first alternative, according to which the equilibration process is a purely virtual process in 'logical' time with no observable counterpart, is "logically consistent", but "unrealistic"; this 'solution' would be exemplified by either Edgeworth's recontracting process, if interpreted as a purely mentalistic process, as suggested by Hicks in the quoted passage, or Walras's *tâtonnement* process in exchange, under the 'no trade out of equilibrium assumption' (but, as we have seen, Hicks was apparently unaware of its explicit adoption by Walras); since, in this case, the purely mentalistic equilibration process would not require any instant of 'real' time (i.e., of the time set over which the economy is supposed to evolve) to carry its effects through, the equilibrium eventually arrived at may be regarded as 'instantaneously' reached at that instant of 'real' time to which the data characterising the economy, hence the equilibrium, are associated. The second alternative, according to which the equilibration process is an observable process in 'real' time with actual trades out of equilibrium, is "realistic", but either logically inconsistent, if combined with the assumption that the equilibration process should neither change the data of the economy nor affect the equilibrium eventually arrived at, or, as supposed by Hicks, necessarily producing an undetermined outcome; this 'solution' would be instanced by Walras's *tâtonnement* process, if it lacked the 'no trade out of equilibrium assumption', as surmised by, e.g., Bertrand (1883). Finally, the third alternative, according to which the equilibration process is an observable process in 'real' time, with actual trades taking place under Marshall's assumption of a constant marginal utility of either a money-like commodity or money proper, is not only "realistic", but also such that the final rate of exchange, in a two-commodity barter economy, or the final money price, in a Marshallian "particular market", would be left unaffected by the trading process; yet the possibility of extending Marshall's solution to a multi-commodity economy is doubtful.

In view of this, it is certainly not surprising that Hicks, in concluding his preliminary review of the alternatives put forward by the economists of the "Lausanne School" (or, for that matter, by Marshall himself) in order to solve the equilibration problem in a multi-commodity exchange model, should express the following negative opinion:

> Neither Walras nor Pareto faced up to this difficulty; when we do so, it is impossible to avoid the conclusion that the "Lausanne equations" are of rather less significance then they imagined. (Hicks 1934, pp. 343)

But is it really true, as Hicks would let us believe, that neither Walras nor Pareto "faced up to the difficulties" inherent in the analysis of the equilibration process in a general equilibrium framework?

[8] Hicks and Allen (1934a, p. 18; italics added). See also the footnote appended to the quoted sentence.

3. Hicks on Walrasian equilibrium and equilibration (1930-1934): production and capital

As already recalled, Walras had explicitly introduced the 'no trade out of equilibrium assumption' in the mid-1880s to get rid of the indeterminacy problem in the exchange model, but he had not at the same time changed the assumptions concerning the nature of the *tâtonnement* in his models with production. As a result, in the second and third editions of the *Eléments*, Walras had kept to the assumption, already made in the first one, that in the models with production the quantity adjustment process is a process in 'real' time, involving exchange and production activities aimed at transforming capital services into products. The process repeats itself over a sequence of rounds or periods, under unchanging technical conditions (summarised by a single-output, fixed-coefficients technology) and a constant provision of capital services; the process brings about a progressive adjustment, in accordance with the Walrasian rule of quantity adjustment which stipulates that the change in each industry's output be a sign-preserving function of that industry's profits. The process goes on until the production equilibrium conditions (zero profits in all industries) are eventually satisfied.

Up to this point, Walras's analysis of the production process in the second and third editions of the *Eléments* is common to his two equilibrium models with production. From this point onwards, however, one must carefully distinguish between the exchange and production model, on the one hand, and the model with capital formation, on the other: for in the first model production consists in transforming capital services (by definition non durables) into non-durable consumer goods; in the second, instead, the outcome of the production process consists not only in consumer goods, but also in capital goods proper (by definition durables). Hence, while in the exchange and production model there are only flow-commodities (i.e., services and non-durable consumer goods), in the model with capital formation stocks (i.e., capital goods proper) are present too. Therefore, the economy described by the exchange and production model is a pure-flow economy; that described by the capital formation model is an economy where both flows and stocks are jointly considered.

In a pure-flow economy there are no linkages between time periods: in each period the data are purely exogenous, since no stocks can possibly be carried over from the past and no intertemporal activities can possibly be performed by the agents participating in the economy. Hence, in this case, the invariance of the data over 'real' time can be legitimately assumed. As a consequence, the equilibrium eventually arrived at has the nature of a stationary equilibrium, identically repeating itself over time under unchanging conditions.

When stocks are present, however, the data characterizing the economy in each period are not longer purely exogenous: for at least part of the data (namely, the produced capital goods) are the result of the production activities carried out in the past. No assumption of data invariance in 'real' time can be legitimately made in this case. Hence, the capital formation model of the second and third editions of the *Eléments* necessarily is an inconsistent model, for it employs a stationary equilibrium concept which runs foul of the endogenous source of change in the data represented by the production of capital goods.

In the end, one can conclude that in the second and third editions of the *Eléments* Walras ends up by finding himself in a blind alley. As a matter of fact, each one of the three formalised equilibrium models discussed in those two editions turns out to be characterised by a different equilibrium notion: the exchange model is associated with an instantaneous equilibrium concept, the exchange and production model with a consistent stationary equilibrium concept, and finally the capital formation model with an inconsistent stationary equilibrium concept.

To escape this predicament, in the fourth edition of the *Eléments* Walras eventually resolved to make an assumption, called "hypothèse des *bons*", which rules out all observable trade and production activities during the equilibration process: the agents are supposed just to exchange "*bons*", without carrying out any actual action. In this way the *tâtonnement* processes at work in all the three models are converted into purely virtual processes, taking place in 'logical' time and driving the economy towards an overall instantaneous equilibrium.

Under this assumption, the time structure of the analysis can be specified as follows:

> Au moyen de l'hypothèse des *bons*, on peut distinguer nettement, surtout si on les suppose successives, les trois phases suivantes :
> 1° La phase des *tâtonnements préliminaires* en vue de l'établissement de l'équilibre en principe ;
> 2° La phase *statique* de l'établissement effective *ab ovo* de l'équilibre […] pendant la période de temps considérée, aux conditions convenues, sans changements dans les données du problème ;
> 3° Une phase *dynamique* de trouble continuel de l'équilibre par des changements dans ces données et de rétablissement continuel de l'équilibre ainsi troublé.
> En conséquence de ces définitions, il doit être bien entendu que les capitaux neufs […] qui seront livrés pendant la seconde phase […] ne fonctionneront que dans la troisième phase, constituant ainsi un premier changement dans les données du problème. (Walras, 1988, pp. 447, 449, **4**)

As can be seen, the economy envisaged by Walras is, in modern language, a sequential economy, where markets open at specified instants of 'real' time. Such instants are distributed over time at intervals ("périodes") of arbitrary length, which for simplicity can be taken to be always the same. At each specified instant an equilibrium is instantaneously reached by means of a purely virtual *tâtonnement* process in 'logical' time ("*tâtonnements préliminaires*"). The equilibrium contracts made at the initial instant of each period are actually carried out during the period ("phase *statique*"), without any change in the data. The data can only change, for both exogenous and endogenous reasons, at the timeless juncture between any two periods, i.e., at the initial instant of the second one ("phase *dynamique*"). The endogenous source of change in the data specifically mentioned by Walras consists in the production of new capital goods proper.

In the light of the above definitions and assumptions, the evolution of the economy can be described (or explained) by means of a chronologically ordered sequence of instantaneous equilibria, each corresponding to the data prevailing at the instant to which the equilibrium is associated. Walras (1988, p. 447, **4**) designates this descriptive (or explanatory) procedure by the expression "équilibre *variable* ou *mobile*". No assumption of stationarity of either the data or the sequence of instantaneous equilibria is necessary or possible in this theoretical framework; and, in effect, no such assumption is made by Walras. Moreover, no special limitation as to the nature of the commodities that can be traded or produced in the economy is required in this context: in particular, both stocks and flows can be the object of economic activity.

Pareto had reached very similar conclusions even before Walras. As a matter of fact, in his *Cours d'économie politique* (1896-97) Pareto had very closely followed in Walras's steps, structuring his theoretical system by means of the same tripartite classification of nested models (exchange, exchange and production, capital formation) as that adopted by Walras; moreover, to each one of the three models Pareto had associated a specific *tâtonnement* process along the lines suggested by his predecessor. Yet, already in 1896 he had arrived at interpreting the *tâtonnement* construct and the equilibrium concept in a way that would be fully endorsed by Walras only in 1900, when he would finally adopt the "hypothèse des *bons*" in the fourth edition of the *Eléments*. Specifically, Pareto had suggested subdividing the continuous flow of time into discrete periods, thereby interpreting time variable as a discontinuous variable. To the initial instant of each period a "static" equilibrium could then be associated. The evolution of the economy over time could then be described, as in Walras (1900), by means of a chronologically ordered sequence of such equilibria. This method of analysis had been called by Pareto the "method of successive equilibria" (Pareto 1897, p. 10).

In his later treatise on economic theory, the celebrated *Manuel d'économie politique* (1909), Pareto developed a much more abstract and general theory of individual choice and economic

equilibrium. In this new framework, the *tâtonnement* construct, as well as Walras's distinction between "revenues" (flows) and "capitaux" (stocks), all disappeared from sight; any implicit or explicit reference to time, attained by Walras through these two devices, evaporated. Yet the "method of successive equilibria", viewed as the only tool on which economic theory could rely at the time to describe (or explain) the evolution of the economy, survived in the *Manuel* as well (1909, pp. 147-8, 337-8).

After this necessary digression on the evolution of Walras's and Pareto's views on equilibrium and equilibration, with special reference to the models with production and capital formation, let's go back to Hicks's 'Walras' paper. In the section of the paper devoted to a critical examination of Walras's theory of capital, Hicks goes back to a remark already made a few pages before, reminding the reader of Walras's "confusion about the exact meaning of equilibrium": in Hicks's opinion, in fact, it is precisely such a "confusion" to be blamed for Walras's inability to comprehend Wicksell's critique of Walras's own capital theory – an issue to which we shall return towards the end of the present Section. It is worth noting that to the just quoted sentence Hicks appends a footnote, where he asserts that "the confusion [...] gets palpably worse in the later part of Walras's work. See, for example, the rather pathetic passage on pp. 214-215 of the *Eléments*" (Hicks 1934, p. 346, fn. 19). But the cited passage is exactly the one, quoted in full above, where Walras discusses the consequences of assuming the "hypothèse des *bons*" on the time structure of the analysis, hence on the interpretation of the *tâtonnement* construct and the equilibrium concept. It is worth stressing, in this respect, that the passage hailed by Hicks (1934) as "rather pathetic" is contained in Lesson 28, one the very few lessons added *ex novo* to the forth edition (1900) of the *Eléments*.

As we shall see in Section 6 below, the interpretation of *tâtonnement* and equilibrium put forward by Walras in the above-quoted passage will play a significant role, though only an implicit one, in the construction of Hicks's *VC* temporary equilibrium model in 1939. Hence, it is natural to enquire more deeply into the reasons underlying such a severe judgement as that passed by Hicks on that passage in his 'Walras' paper.

One reason is probably the following. The assumption newly introduced by Walras in the fourth edition of the *Eléments*, namely, the "hypothèse de *bons*", magnifies those elements of artificiality that were already present in the *tâtonnement* construct from the very beginning, one such element being, for instance, the assumption that prices or quantities are publicly announced at random at the start of the process ("prix" or "quantités criés au hazard"), and subsequently changed by loosely specified entities according to the Walrasian rules of price or quantity adjustment. This is the reason leading Kaldor (1934, p. 127) to qualify "Walras's assumption" about the *tâtonnement* in exchange as "slightly ridiculous".

But the main reason must be that the "hypothèse des *bons*", by making explicit the assumption of an instantaneous equilibration process and by extending it to all models and all kinds of activities taking place in the economy, including production, amplifies the "unrealism" of the Walrasian *tâtonnement* construct, an "unrealism" that, having already been denounced by Hicks in connection with the exchange model taken by itself, is now pushed to limits that are still more difficult to stand: for intuition suggests that adjustment to equilibrium takes time, particularly when production is allowed for, while logic would seem to imply that the idea of an "instantaneous process" is nothing but a contradiction in terms.

If the most important reason for Hicks's 1934 rejection of the "hypothèse des *bons*" and its consequences is the one stated in the previous paragraph, then it becomes easier to account for another passage in the 'Walras' paper that might appear unexplainable otherwise. For, immediately after having concluded that, due to the inability of Walras and Pareto to "face up to [the] difficulty" inherent in the analysis of the equilibration process, "the 'Lausanne equations' are of rather less significance then they imagined", Hicks suddenly appears to change his mind, opening the door to a possible resolution of the "difficulty", provided that a particular interpretation of the equilibration process is adopted:

> The equations of Walras are not by any means a complete solution of the problem of exchange; but they remain a significant step towards such a solution. For Walras' system of prices will be reached, either if contracts are made provisionally or (a more important case) if people come on to the market on successive "days" with the same dispositions to trade, and the there is no carry-over of stocks (or a constant carry-over) from one day to the next. *When it is understood in the last sense*, the theory of static equilibrium of exchange takes its place as a step towards the development of a complete theory. (Hicks 1934, pp. 343; italics added)

Hicks is here referring to the exchange model. This is not accidental: for, as we shall see in a moment, extending the argument to production raises some special problems. Yet, for the discussion's sake, it is preferable to start immediately from a more general framework.

Of the two interpretations of the equilibration process which are suggested by Hicks in the above passage, the first would follow from the adoption of something like the "no trade out of equilibrium assumption" or, more generally, the "hypothèse des *bons*"; in Walras's theoretical system, as we have seen, it corresponds to either the conception of the *tâtonnement* process in the exchange model since the second edition of the *Eléments* (1889) or the conception of the so-called *tâtonnements préliminaires* in all models since the fourth (1900). The second interpretation would instead follow from the assumption that the same activities repeat themselves over a sequence of periods under unchanging exogenous conditions in a pure-flow stationary economy; in Walras's theoretical system, this interpretation corresponds to the conception of the *tâtonnement* process in the exchange and production model in the second and third editions of the *Eléments* (1889 and 1896).

Hicks never explicitly recognises that the two alternative interpretations he suggests in the 'Walras' paper precisely correspond to alternative conceptions of the *tâtonnement* process associated with specific equilibrium models put forward by Walras in the same or in different editions of the *Eléments*; in particular, he tends to present his second interpretation, the stationary one, as if it were something altogether new, unthought of before, and all the same instrumental in solving the long-standing "difficulty" with the equilibration process. Nor does Hicks explicitly acknowledge, in 1934, that his two alternative interpretations almost exactly correspond, also in the phrasing, to the two alternative interpretations of the recontracting process put forward by Edgeworth in a long series of writings: a purely mentalistic interpretation, based on the idea of 'provisional and revocable contracts', which leads to a notion of instantaneous equilibrium (or, more generally, solution) at a moment of time; and an effective interpretation, based on the idea of 'enforceable and irrevocable' one-period contracts, reiterated over a sequence of periods under unchanging exogenous conditions, which leads instead to a notion of stationary equilibrium (or solution) analogous to the stationary equilibrium of a Walrasian pure-flow economy[9].

Hicks had already adumbrated the distinction between the two alternative interpretations of the equilibration process, albeit somewhat confusedly, in his 1933 paper on "Equilibrium and the

[9] The first interpretation, the better known of the two, is the prevailing one in Edgeworth (1881, pp. 16-20), (1891b, p. 235, fn. 1), and (1891c, p. 317). Both interpretations are presented as equally valid alternatives in (1904, p. 40). The second interpretation is the prevailing one in (1925c, p. 313). In the 'Walras' paper Hicks only alludes to the first interpretation, when he associates "Edgeworth's principle of 'recontract'" with the idea of "provisional contracts" (1934, p. 342). But he is certainly aware of the existence of the second interpretation, which is in effect hinted at in his paper in German published in the previous year (Hicks, 1933, p. 30, fn. 3). To our knowledge, Berry is the first to openly suggest the possibility of interpreting Edgeworth's recontracting as a stationary process in 'real' time, taking place over a sequence of periods in each of which effective one-period contracts are stipulated. Berry's suggestion is made during his controversy with Edgeworth over Marshall's theory of barter (Berry 1891, p. 552; see also Berry's letter to Edgeworth, published in Marshall 1961b, pp. 793-4). On the same occasion, Berry also suggests to identify the unit period in the stationary sequence with the "week"; his suggestion will prove useful to Hicks, at least from a terminological point of view, first in 1935, when he will resume that term to designate the unit period in a sequence that is however non-stationary, and then a few years later in *VC* (see Sections 5 and 6 below).

cycle" (1933, pp. 29-30). There Hicks's problem was to make more precise the "deliberately ambiguous definition" of equilibrium put forward by Pareto in the *Manuel*. In the 1933 paper, as in the 'Walras' paper of the following year, Hicks had openly expressed his preference for the second alternative, presupposing a "continuing" or "ongoing market" and leading to a stationary equilibrium notion, over the first, which leads instead to an instantaneous equilibrium notion: in 1933 the "second interpretation of Pareto is more to the point" (p. 30), while in 1934 it represents, as we have seen, "a more important case".

Thus, in the end, in his 1933 and 1934 papers, in analysing the alternative interpretations of the equilibration process compatible with general equilibrium theory of either the Paretian or Walrasian variety, Hicks was led by his preference for the "ongoing market" interpretation over the instantaneous adjustment one to unearth that traditional notion of stationary equilibrium that he had already employed in *Theory of Wages* (1932, pp. 6-7) in an essentially Pigouvian partial equilibrium framework, with some sprinklings of Walras and Böhm-Bawerk (Hicks 1963, p. 314; 1973, p. 136).

In a general equilibrium framework, however, a problem arises that in the partial equilibrium analysis of a consumer good market, or of the labour market for that matter, can be swept under the carpet by means of the Marshallian *ceteris paribus* clause: the problem of capital.

In the 1933 paper, where Pareto's theory is the point of departure of the analysis, Hicks had maintained that in the *Manuel* there are clues indicating that

> in essentials Pareto accepted the theory of capital and interest due to Böhm-Bawerk [...]. But this notion was never fully worked out and incorporated into his system – though the incorporation would not have presented any particular difficulty once it had been decided that equilibrium was to be interpreted in the second sense – that of the ongoing market. (Hicks 1933, p. 30)

Therefore, having arrived at the conclusion that, for the purpose of analysing the equilibration process, the stationary framework of an "ongoing market" should be preferred to the alternative framework focusing on one single instant of 'real' time, Hicks apparently discovered (in 1933) that the stationary framework is also to be preferred for the purpose of incorporating into general equilibrium theory the phenomena of time-taking production and capital. Yet, if the "ongoing market" framework is more favourable to the development of a theory where time and capital have an important role to play, at the same time it imposes upon the theory a "most significant limitation": for

> the extended equations would only [be] applicable to the conditions of a Stationary Equilibrium – the equilibrium of an economy in which there is no net saving. [...] The Lausanne equations become no more than an exact formulation of what Marshall called the 'famous fiction' of the Stationary State. As such, they are not a description of reality. At most, they are a tool for its analysis. (Hicks 1933, p. 31)

In spite of the unrealism of such an extended theory, "a precise formulation of the conditions of Stationary Equilibrium is a useful achievement". Hence, in 1933 Hicks set out to provide a sketch of the equations determining such a Stationary Equilibrium, under the assumptions of stationary expectations and zero net saving: the first assumption is the most natural under stationary conditions and can therefore be justified under the same circumstances; the second, which finds expression in a "capital equation" of an aggregate type, "has often caused trouble", but is nevertheless required by the structure of the extended Stationary Equilibrium model (Hicks 1933, p. 31).

After providing this concise and altogether unsatisfactory outline of the model, however, Hicks did not dwell upon it. For, in 1933, he also believed that the extended Stationary Equilibrium model can be replaced, in its only function as a tool of analysis, by a better model: the extended general

equilibrium model with Perfect Foresight[10]. In outlining this alternative model, Hicks directly drew his inspiration from the writings of Knight (1921), which had already represented an important point of departure for Hicks's theoretical investigations in the field of risk and uncertainty (1931), and especially from Hayek (1928), where a semi-formalised model of perfect foresight instantaneous equilibrium had already been discussed in some detail. But Hicks could also gain some insight from the almost contemporary works of a few Swedish economists, particularly Lindahl (1929) and (1930) and Myrdal (1932), who were then exploring similar paths. As Hicks himself tells us (1982b, p. 7), he was informed by Hayek himself in 1932 of the existence of Hayek's 1928 paper in German. We also know that, when writing his 1933 paper in German, Hicks had not yet read the Swedish economists' works, nor had he been able to talk with them yet; but their ideas were, so to speak, in the air.

So Hicks took the idea of 'period analysis' from the Swedish economists' tool-box: in a way very similar to the one suggested by Walras after the adoption of the "hypothèse des *bons*", time is subdivided into periods, changes in the data take place at the junctures between periods, while endogenous variables are assumed constant (at their equilibrium levels) within each period. Hicks's endogenous variables are prices, in the competitive tradition. Therefore, each unit period is taken to be "so short that the movement of prices *within it* can be neglected" (Hicks 1933, p. 32): this is an assumption that will survive up until *VC*. Since the stationarity assumption is dropped, the assumption of stationary expectations is no longer justified. Perfect Foresight can however be invoked to make expectations and equilibrium determinate at one and the same time: a Perfect Foresight equilibrium is defined as a system of current and expected prices such that, at those prices, all markets (current and future) clear. "Disequilibrium is the Disappointment of Expectations" (Hicks 1933, p. 32).

The Perfect Foresight equilibrium model, as the Stationary Equilibrium model, is barely outlined: no analytical discussion, let alone a formal proof of any statement or proposition, is really put forward. Yet, the simple sketch provided is enough for Hicks to arrive at a very definite conclusion:

> Such a 'dynamic equilibrium' is obviously still far from being a description of reality. It does nevertheless serve as a model of a *perfectly working* economic system, which is much more usable as a standard of comparison than is the model of Stationary Equilibrium. (Hicks 1933 p. 32; Hicks's italics)

Somewhat paradoxically, however, the sharp conclusion reached in 1933 concerning the relative merits of Perfect Foresight and Stationary Equilibrium is completely reversed just one year later: in the 'Walras' paper, in fact, Perfect Foresight Equilibrium vanishes, while Stationary Equilibrium gains once again the centre of the scene. This preference reversal can be explained as follows.

Let's come back to the Section of the 1934 paper devoted to Walras's theory of capital. Therein, after stressing the highly controversial character of this part of Walras's overall theory, Hicks recalls one of Wicksell's chief objections against it: since Walras's theory "determines the rate of interest on the market for new capital", it is "apparently inapplicable to stationary conditions" (Hicks 1934, p. 446)[11]. But this can be regarded as a compelling criticism only if, contrary to the

[10] On this see also Hicks's much later recollections in (1973, pp. 137-8), (1979a, p. 359), and (1982b, pp. 6-7).

[11] It should be noted that neither Wicksell, nor Hicks (in the 'Walras' paper) are able to identify the ultimate reason why Walras, in order to determine the rate of interest, is forced to assume that the economy under investigation be "progressive", so that there can exist positive net saving to be exchanged against newly produced capital goods proper, whose prices can then be determined on the corresponding markets, eventually allowing the rate of interest to be determined. From a logical point of view, the requirement of a positive net saving, characterising a "progressive" economy, is as disturbing as the symmetrical requirement of a zero net saving which characterises a stationary economy: both requirements, in fact, impose upon the agents of the economy a particular type of saving behaviour, whereas the agents should be free to choose whatever sort of saving behaviour they prefer according to their preferences. In fact, Walras is forced to assume the "progressiveness" of the economy because of another tacit

conclusion reached by Hicks in the 1933 paper, compatibility with Stationary Equilibrium is viewed as a prerequisite for producing a good theory of capital. So, in the end, why does Hicks change his mind, in so short a stretch of time, about the relevance of the stationarity condition?

There are two main reasons underlying Hicks's change of view. The first is implicit in the whole trend of Hicks's reasoning in the preceding Sections of the 'Walras' paper: there Hicks had defended the stationary interpretation of the equilibration process against the instantaneous one for only the first interpretation allows the theorist to account for the 'real' time required for the adjustment towards equilibrium; therefore, a good theory of capital must satisfy the stationarity condition too, for only a Stationary Equilibrium is empirically justifiable. The second reason, instead, is made explicit by Hicks at precisely this point of his argument. It has to do with the supposedly greater plausibility of the assumption of stationary price expectations, a supposition that, by itself, is enough to explain the reversal of Hicks's preferences concerning Stationary and Perfect Foresight Equilibrium:

> Further, as Walras would have realized if it had not been for his confusion about the exact meaning of equilibrium, it is only in a stationary state that we can get any sensible sort of equilibrium so long as people expect the prices of products to remain unchanged in the future (as Walras tacitly assumed they do). (Hicks 1934, p. 346)[12]

So, a good capital theory has to be consistent with the stationarity assumption. Walras's original theory is not consistent with Stationary Equilibrium, and consequently cannot be a good theory. However, Walras's oversight can be remedied. A "slight extension" of Walras's original theory of capital, implying that the "new capital goods become not only net additions to the capital stock, but also replacements", is enough to immunise the theory from Wicksell's criticism, for in that case "the capital market does not disappear in the stationary state". In view of this, Hicks can conclude his discussion by asserting that, "once the amendment is made, Walras' theory of capital becomes as good as Wicksell's, and better than Böhm-Bawerk's" (Hicks 1934, pp. 346-7).

Yet, in reaching this conclusion, Hicks unfortunately forgets that the stationarity assumption, legitimate in a pure-flow economy, becomes illegitimate in an economy with stocks and any other sort of intertemporal linkages: and capital, not only for Wicksell and Böhm-Bawerk, but also for Walras, certainly represents a linkage between present and future.

4. Eight unsettled questions (1934)

Looking back at the scientific path travelled by Hicks during the early 1930s, one cannot but be impressed by the great number of questions confronted, the many diverse fields explored, and the important results reached. However, the position taken by Hicks in those years on several controversial issues does not display that steadiness and conclusiveness that might be hoped for: as has been seen, on many crucial topics, concerning in particular equilibrium, equilibration, expectations, and capital, Hicks changes his mind quite often and sometimes unexpectedly, thereby rendering the rational reconstruction of the course travelled by him over the 1930-1934 period a very complicated task. In order to make the reconstruction of the subsequent half-decade easier, it is convenient to summarise at this point the most contentious issues remaining unsettled at the end of 1934, also keeping in mind that that year represents a turning point in Hicks's intellectual history.

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assumption, inadvertently introduced in the analysis, which constrains the agents' behaviour in a different way: for he tacitly assumes the already existing capital goods to be untradeable. If they were tradable, there would be no need to assume "progressiveness" (or, for that matter, stationarity) of the economy in order to provide some equation with which to determine the rate of interest.

[12] A similar point, concerning the relative plausibility of the assumptions underlying Stationary and Perfect Foresight Equilibrium, particularly as regards expectation formation, will be made by Hicks almost thirty years after the 'Walras' paper, in the "Commentary" added to the second edition of the *Theory of Wages* (1963, Section III, p. 308).

Even if the unsolved questions are obviously interconnected, it is expedient to group them under the following eight headings.

1. General vs. partial analysis
   As the 'Walras' paper clearly shows, Hicks is undecided about which of the two great traditions, the Walrasian and the Marshallian, should be privileged. Walrasian general equilibrium theory should be praised for its generality, but it runs the risk of falling into abstractness and sterility. The strengths of Marshallian partial equilibrium analysis are its concreteness and fruitfulness; its weakness lies in its specificity. Hicks implicitly asks the following question: How and to what extent can the two traditions be combined?
2. Equilibration, equilibrium, and disequilibrium: Walras-Pareto vs. Marshall
   Hicks stresses the following dilemma: an observable disequilibrium theory in a general equilibrium framework, such as the Walrasian exchange model, leads to indeterminacy; an observable disequilibrium theory in a Marshallian partial equilibrium framework can be made consistent with a determinate outcome, at the cost of making some special assumptions. Hicks leaves the following question to be answered: How and to what extent the special assumptions employed in a partial equilibrium framework can be exported to a general equilibrium framework without losing their effectiveness?
3. Equilibrium, equilibration, disequilibrium: unobservable vs. observable disequilibrium in a general equilibrium framework
   Hicks perceives that, in general equilibrium analysis (of the Walrasian, Paretian or Edgeworthian type), there exist two alternative interpretations of the equilibration process, both of which consistent with the premises of the theory and compatible with determinacy: the first, unobservable disequilibrium in 'logical' time, leads to an instantaneous equilibrium notion; the second, observable disequilibrium in 'real' time (restricted however to an "isolated period" economy, of the pure-flow or similar type), leads to a consistent stationary equilibrium notion. Hicks is dissatisfied with both interpretations: the first is unsatisfactory for its unrealism, the second for its narrowness. Yet, in 1934, he does not exactly know how to overcome the ascertained limitations. He only tries to broaden the set of phenomena to which stationary equilibrium theory can be applied (see below, points 5 and 7). The question that Hicks implicitly poses is the following: How can the empirical strength of the observable equilibration process supporting a consistent stationary equilibrium be combined with the generality and flexibility of instantaneous equilibrium?
4. Instantaneous equilibrium
   Hicks dislikes this notion, due to the unrealism of the underlying equilibration process. Yet, perceiving its generality and potentialities, he makes use of it in special contexts, even if he refrains from justifying its employment on empirical grounds. The best instance of Hicks's attitude towards instantaneous equilibrium is his use of the Perfect Foresight equilibrium concept in the 1933 paper, where it is essentially justified as a quasi-normative standard of reference with no claim to realism (1933, p. 32; see also 1963, p. 308). The temporary equilibrium concept makes just a flimsy appearance in the 1930 paper (1930, p. p. 228). Hicks implicitly poses the following question: How and to what extent can the normative uses of the instantaneous equilibrium notion be reconciled with its possible uses in positive theory?
5. Stationary equilibrium
   Hicks likes the kind of realistic equilibration process supporting this equilibrium notion, but finds the pure-flow economy, to which the concept can be consistently applied, excessively restrictive. Therefore he repeatedly tries to employ the stationary equilibrium concept also to discuss issues connected with stocks and capital (in 1932, 1933, and 1934). However, Hicks's attitude towards stationary equilibrium models with capital formation is winding. The questions that Hicks tacitly poses are: How can the stationary equilibrium notion be rescued from its limitations? Should it be rescued?

6. Expectations
   Already since 1931, when he publishes his paper on uncertainty and profits, Hicks is aware that in principle expectations should be represented by means of probability distributions. Yet he generally keeps to the more traditional representation of expectations as point expectations. Moreover, in line with the competitive tradition, he typically identifies expectations with price expectations. Stationary point price expectations are then justified when the economy is assumed to be stationary, and consequently the equilibrium is stationary as well; they are unwarranted otherwise. Correct point price expectations are the hallmark of competitive perfect foresight equilibrium, which should be used as a standard of reference. Hicks implicitly raises the following questions: How are expectations formed? Should they be regarded as exogenous or endogenous?
7. Capital
   Hicks distinguishes three main theories of capital: the Austrian theory, meaning by that the theory of Böhm-Bawerk and Wicksell; Walras's original theory; Walras's amended theory. There are many clues suggesting that Hicks's preferences go to the first theory, but it is also clear that he feels unable to formalise it and uncertain as to its soundness. The discussion of Walras's theory, both in its original version and in the amended one, is wholly unsatisfactory. In 1934 the problem of capital is viewed by Hicks as strictly connected with the problem of stationary equilibrium. So Hicks's unsettled questions concerning capital are essentially the same as those concerning the stationary equilibrium notion.
8. Production and demand theory
   In the early 1930s Hicks provides important contributions to the theory of production (in the 1932 book) and fundamental ones to the theory of demand (in the 1934 paper with Allen). Such contributions are somewhat uncoordinated with the other breakthroughs that Hicks is simultaneously accomplishing in the fields listed above, as well as in others. In this respect, the fundamental question that Hicks ought to answer is the following: How and to what extent can the results already obtained in the fields of individual and market demand and production be merged with the results already achieved or yet to be achieved in the other fields of interest?

With the benefit of hindsight, we know in advance which is the end point that Hicks will finally reach at the conclusion of his five-year journey from 1935 to 1939: the finishing line is nothing but *Value and Capital*. Yet, since the interpretations of *VC* are still nowadays highly controversial, it may prove illuminating to try and understand how exactly Hicks arrives at his final destination (which is of course final only with respect to the 1930s). Moreover, even if at the beginning some of the final ingredients are already present in embryonic form, from a theoretical point of view the starting point is still very far from the end point. Hence, not only is the distance to be covered long, but also the questions to be answered are many and diverse: as a matter of fact, in order to fulfil his task, Hicks should be able to answer all the questions listed under the eight headings above. It should be clear, however, that not all the issues are tackled and solved by him at one and the same time; further, not all the solutions suggested over time are conclusive and immutable: as in the early 1930s, so in the mid- and late 1930s too there are some afterthoughts and changes of mind, though in a lesser degree than before; finally, not all the solutions eventually arrived at are equally convincing: *VC*, after all, is only a step in Hicks's intellectual history, albeit an important one.

Before embarking upon a more detailed discussion of the individual issues, it is still convenient to distinguish the questions that are confronted in the period from mid-1934 to 1939, leaving some evidence of the progress made in published papers and documents, from the questions whose solution is disclosed only when *VC* is finally made available.

The questions grouped under headings 2 and 3, that is, the questions concerning the equilibration issue, as well as the question under 8, that is, the question concerning the possible amalgamation of the various parts of Hicks's theoretical endeavour, are left to be spelled out in *VC*. This does not mean, of course, that they are not jointly analysed with the others, but only that their

resolution requires the previous systematisation of the whole architecture of the theoretical system to be put forward in *VC*. The question under heading 1, as we shall see, is repeatedly confronted over the intervening years, and some traces are made public in the papers published in that period; but, also in this case, a full solution of the underlying methodological issue can only be provided when the picture is complete.

The questions under the headings from 4 to 7, on the contrary, are systematically tackled over the five years from 1935 to 1939, and the advances progressively made find their way to the press. There are a few papers playing a crucial role in this regard: the first is a paper published in 1935 with the title "Wages and Interest: The Dynamic Problem", but soon relabelled by Hicks the 'Bread' paper, in view of the assumed nature of the only consumer good supposed to be produced in the economy therein described; then there are two papers related with the appearance of Keynes's *General Theory*, namely, the timely review of Keynes's book, published in *The Economic Journal* in 1936, and the celebrated IS-LM paper, published in *Econometrica* in 1937. The focus of the 'Bread' paper is formally on capital (under 7), but actually on the two alternative equilibrium notions (under 4 and 5). The focus of the two *General Theory* papers is on expectations (under 6) and the two alternative equilibrium notions (under 4 and 5). Finally, as already said, the grand methodological issue concerning the alternative between the Walrasian and the Marshallian tradition insinuates itself everywhere, but is not really solved before the appearance of *VC*.

5. Value and Capital: The Long Gestation (1935-1939)

Let us start from the 'Bread' paper (Hicks 1935b), whose central model is in many respects a small-scale anticipation of the *VC* temporary equilibrium model. However, it should be immediately added that, on top of the difference of scale, which is not irrelevant, there are many other dissimilarities between the two models that deserve mentioning. Money and all the monetary complications are essentially disregarded in the 'Bread' paper, except for a few hints in the last section. With the exception of this last section, the model discussed in the paper is a three-commodity general equilibrium system, with two flow-commodities (labour services and Bread) and one asset (loans). All prices "are reckoned in terms of 'Bread'. The rate of interest is a 'bread' rate of interest". Capital, called Equipment, "is not exchangeable" (Hicks 1935b, p. 69)[13] .

In the 'Walras' paper, as will be recalled, Hicks had concluded that an amended version of Walras's theory of capital, satisfying the stationarity assumption and an aggregate capital equation, presumably of the type suggested by J.B. Clark (1899) or Cassel (1932), would represent the best point of departure for economic dynamics. Yet, in the 'Bread' paper, Hicks's stance on this issue is altogether different: quite evidently, the reasons underlying his previous support for a stationary equilibrium approach had been fading away in the meanwhile, and this had induced him to change his mind on the capital issue as well.

To begin with, Hicks recalls that

> most modern capital theories fall into one of two classes. On the one hand, there is the 'timeless' type of theory, which treats capital as a factor of production like any other. Such a theory is that of J.B. Clark. On the other hand, there is the 'period of production' theory of Böhm-Bawerk and Wicksell. [...] They are both stationary theories, built upon the hypothesis of a stationary state, quite satisfactory under that hypothesis, but incapable of extension to meet other hypotheses, and consequently incapable of application. In a

[13] The non-exchangeability of existing Capital makes the model more similar to Walras's original model with capital formation, where only newly produced capital goods are exchangeable, than to the *VC* model, where all capital goods are exchangeable. In this respect, the slightly earlier paper "A Suggestion for Simplifying the Theory of Money" (Hicks 1935a), later nicknamed as the 'Simplifying' paper, comes closer to the spirit of the future *VC* model: for it paves to way to a full incorporation of money, bonds, and all sorts of assets into a general equilibrium framework of the type that will be discussed in detail in *VC*.

stationary state they are both correct. [...] But once we leave the stationary conditions, these convenient equalities disappear, and theories based on them cease to be applicable. (Hicks 1935b, pp. 67-8)

To this passage, the following somewhat cryptic sentence is appended:

To found a theory upon an assumed equality, which is not a real equality, is a most dangerous thing to do [...]. (Hicks 1935b, p. 68)

This sentence may be interpreted as asserting that, while the aggregate capital equation is assumed to satisfy an appropriate stationarity condition, depending on the specific approach followed, such stationarity condition is imposed from outside the model, rather than being the endogenous outcome of the individuals' choices, as it should be in a consistent general equilibrium model. If this interpretation is correct, this is the first passage where Hicks proves to be aware that the stationarity assumption is illegitimate outside the model of a pure-flow economy.

In any case, at the time when the 'Bread' paper was written, Hicks's acknowledgement that the "hypothesis of a stationary state" has its "dangerous" sides proved instrumental in favouring his departure from the stationary equilibrium approach in capital theory. Besides that, a second important reason for abandoning the stationarity assumption was provided by the theoretical developments that had been taking place in Swedish economics since 1929, by virtue of the pioneering works of Lindahl, (1929) and (1930), and Myrdal (1932). Hicks had read Myrdal (1932) at the beginning of 1934, enthusiastically reviewing it in *Economica* (Hicks, 1934b)[14]. Moreover, in the summer of 1934 Hicks had had the opportunity to discuss directly with Lindahl his 1929 and 1930 papers in Swedish, an experience that would repeat itself in 1935.

Starting from Wicksell's monetary theory and his analysis of the so-called "cumulative processes" (Wicksell 1898, and 1901-6, vol. 2), Lindahl had developed a method of analysis that would later become known as 'period analysis': the method, of which, as already recalled, Hicks had already made a rudimentary use in his 1933 paper, consists in subdividing time into a sequence of periods, in studying the equilibrium conditions of each individual period, and, possibly, in extending the analysis to the entire sequence. In his 1929 paper Lindahl, after critically discussing the stationarity assumption, had put forward two alternative models of instantaneous equilibrium, under the alternative assumptions of perfect and imperfect foresight, respectively. These equilibrium models had then been employed in the 1930 paper to discuss the Wicksellian problem of the "cumulative process". Similarly Myrdal, after criticising the method of assuming a stationary state as the starting point of the analysis, had put forward, with many a proviso (Myrdal 1932, p. 44), an instantaneous equilibrium model with given expectations, to be used as the foundation of his 'period analysis'.

In the 'Bread' paper Hicks, after dropping the stationarity assumption, quite deliberately chose "to treat the continuous variable time as if it were discontinuous" (1935b, p. 68, fn. 2). In the paper the single periods into which time is subdivided are called "weeks". The market is only open on the first day of the week, i.e., on Mondays. Perfect competition is assumed in all markets (for Bread, labour, and loans, respectively) and a competitive equilibrium is supposed to be reached on each Monday: at the equilibrium prices (wages and rate of interest), demand equals supply in each market. Walras' Law is explicitly proved and exploited to allow the theorist, in determining the equilibrium conditions of the model, to get rid of one of the markets and of the corresponding market-clearing equation. Availing himself of this opportunity, in the 'Bread' paper Hicks chose to disregard the market for loans, therein identified with "that elusive thing, the 'capital market'" (1935b, p. 77).

[14] Myrdal's essay, originally published in German in 1932, was translated into English and published a few years later under the title *Monetary Equilibrium* (Myrdal 1939). But, by that time, Hicks had long been acquainted with Myrdal's work through the German version of his essay.

As can be seen, the inspiration of the model is clearly Walrasian. And yet, the choice to restrict the analysis to just three markets is certainly due to Hicks's desire to combine Walrasian rigour with Marshallian concreteness. The model is in effect a macromodel with some Walrasian microfoundations: the device of assuming representative agents (one labourer, one entrepreneur, one rentier) is exploited, but the treatment is unsatisfactory. In fact, in the 'Bread' paper, Hicks contented himself with discussing the probable effects of changes in current and possibly expected prices on demands and supplies; but, in spite of Hicks's assertion to the contrary (1935b, p. 78, fn. 11), he was unable to reach any definite conclusion by means of his analysis, due to the insufficient specification of the model. In particular, a quite detailed discussion of the representative entrepreneur's choices of intertemporal production plans under alternative assumptions about changes in prices and price expectations is actually developed in the paper. But, since the model is not really closed, all this discussion does not go beyond the casuistic analysis of the representative entrepreneur's choices, without providing any explanation of market results[15].

As can be seen from the above account, the 'Bread' model anticipates many aspects of the *VC* temporary equilibrium model. In particular, in the 1935 paper one can already find the systematic use of 'period analysis', the distinction Monday-week, the assumption that a competitive Walrasian equilibrium is established on each Monday, the idea that the dynamics of the economy concerned can be described by means of a chronologically ordered sequence of equilibria associated with successive Mondays. Yet, at the end of the story, in assessing the results achieved by using the machinery of the model, Hicks is forced to acknowledge that the potential inherent in 'period analysis' has not been really exploited, particularly in so far as the dynamic aspect is concerned. For

> in all our investigations we have never got beyond our first Monday. There is no reason why theory should be becalmed at that point; [...] we ought to go on to see what happens on Monday week. However, time must go on in its own order, and Monday week will have to be another story. (1935b, p. 79)

Hicks raises here the problem of the relationship between static analysis, which aims at determining the equilibrium prevailing on each Monday together with its properties, and dynamic analysis, which aims instead at stringing together all such equilibria in a sequence, whose properties ought then to be examined by means of a distinctive piece of theory. As far as the 'Bread' model is concerned, however, it is clear that the static part of the analysis tends to absorb all the energies of the researcher, killing the dynamic part before it can even start to do its job. A similar problem also arises in connection with the method of analysis adopted by Keynes in his *General Theory*. To better understand similarities and differences between the approach that Hicks was trying to build in the second half of the 1930s and the approach that Keynes was striving to work out at about the same time, let us now turn to an examination of Hicks's reactions to the appearance of Keynes's *General Theory* at the beginning of 1936.

As is well-known, Hicks put forward his assessment of Keynes's book on two different occasions: the first opportunity arose when Hicks was asked (by Keynes himself) to contribute a review-article to *The Economic Journal* immediately after the publication of the book (Hicks 1936b); the second chance was instead offered by the presentation of the famous IS-LM paper at a meeting in Oxford in September 1936 (Hicks 1937a). While the 1937 article is of course of paramount importance for the future development of Keynesian economics, and of macroeconomics in general, the 1936 review is instead more relevant to the understanding of the impact of Keynes's theory on Hicks's own ideas and on the systematisation of those issues that were still open at the time in his methodological and theoretical stance.

[15] The inadequate specification of the 'Bread' model can also help explain the contradictory results obtained by Hicks in using the same model to test some conclusions he had reached a few years before in *The Theory of Wages* (1932). On this, see also Hicks's comments in (1982d).

The IS-LM article, as is well-known, is a reconstruction of a part of the analytical core of the *General Theory* along (loosely) Walrasian lines, a reconstruction that Hicks was able to provide in so short a time because he could exploit much of the work already done in view of his 'Bread' paper[16]. In the 1937 paper, the most evident influence of the Walrasian methodology, as employed in the 'Bread' paper, is represented by Hicks's reliance on Walras' Law in order to reduce the equilibrium conditions of a three-market macromodel (money, bonds, and goods) to a two-equation system or a two-dimensional diagram (the IS-LM diagram), thereby also getting rid at one stroke of the so-called "loanable funds" controversy. Keynes, who expressed overall appreciation about Hicks's reconstruction, was unable to understand this point[17], thereby confirming his distance from the analytics (though not from the broad perspective and general aims) of the Walrasian approach[18].

For the present purposes, however, it is the 1936 review to be the more relevant. Here Hicks starts by pointing out that the *General Theory* "is sometimes presented as a theory of 'output in general'; sometimes as a theory of 'shifting equilibrium'" (Hicks 1936b, p. 84). These features identify two of the most revolutionary traits of the *General Theory* from Hicks's own point of view.

As to the first point, Hicks's remarks on the method of the *General Theory* are as follows:

> It is a theory of output in general *vis-à-vis* Marshall, who took into account many of the sort of complications which concern Mr Keynes, but took them into account only with reference to the single industry. [...] The technique of this work is, on the whole, conservative: more conservative than in the *Treatise*. It is the technique of Marshall, but it is applied to problems never tackled by Marshall and his contemporaries. (Hicks 1936b, pp. 85, 99)

From Keynes's bold generalisation of Marshall's theory to a large body of issues that Marshall had disregarded, combined with his preservation of Marshall's technique and pragmatic attitude, Hicks drew an encouragement to do the same with respect to Walras and, at the same time, to make a further effort to overcome the Marshall-Walras opposition.

As to the second point, Hicks's comments are as follows:

> It is a theory of shifting equilibrium vis-à-vis the static or stationary theories of general equilibrium, such as those of Ricardo, Böhm-Bawerk or Pareto.

While the inclusion of Ricardo in the set of economists to rebuke is essentially due to Keynes's definition of "classics", the names of Böhm-Bawerk and Pareto allude instead to the sort of value and capital theory on which Hicks had been working over the previous few years.

But then, in 1936, the novel approach admirably developed by Keynes in his *General Theory* provided Hicks with a further argument to add to the already remarkable list of reasons in favour of the abandonment of the stationarity assumption, an assumption which however continued to be fashionable among Cambridge economists[19] and many other quarters:

> Ordinary (static) economic theory, so the old argument went, explains to us the working of the economic system in 'normal' conditions. Booms and slumps, however, are deviations from this norm, and are thus to be explained by some disturbing cause. [...]
> The present theory [i.e., Keynes's *General Theory*] breaks away from the whole of this range of ideas. It is no longer allowed that ordinary economic theory can give a correct analysis of even normal conditions; the things it leaves out of account are too important. The

[16] On the mixed origins and uncertain lineage of Hicks's 1937 article, see Dimand (2007). However, on the alleged Walrasian parentage of the IS-LM model, see also Hicks's much later comments in (1980a, p. 320).
[17] On this see the Keynes-Hicks exchange of correspondence in (Keynes 1973, pp. 79-83).
[18] On this see De Vroey (1999a, p. 122, fn. 14).
[19] See, e.g., Pigou's 1935 book *The Economics of Stationary States*, reviewed by Hicks (1936a) at a time when he was also reviewing Keynes's *General Theory*.

changing, progressing, fluctuating economy has to be studied on its own, and cannot usefully be referred to the norm of a static state. (Hicks 1936b, p. 85)

One might suspect that dropping the "norm" may cause the theory to become indeterminate. Yet, this is not so, if Keynes's most important innovation, "the method of expectations", is accepted. For

from the standpoint of pure theory, the use of the method of expectations is perhaps the most revolutionary thing about the book [...]. The point of the method is that it reintroduces determinateness into a process of change. (Hicks 1936b, pp. 86-7)

According to Hicks, the method of expectations works as follows:

If we assume given, not only the tastes and resources ordinarily assumed given in static theory, but also people's anticipations of the future, it is possible to regard demands and supplies as determined by these tastes, resources and anticipations, and prices are determined by demands and supplies. Once the missing element – anticipations – is added, equilibrium analysis can be used, not only in the remote stationary conditions to which many economists have found themselves driven back, but even in the real world, even in the real world in 'disequilibrium'. (Hicks 1936b, p. 86)

This is a quite precise description of what would become, in a couple of years, the *VC* temporary equilibrium model, rather than of Keynes's *General Theory* model. It is worth noting, however, that the embryonic *VC* model already detectable in Hicks's 1936 review-article is presented as the outcome of Keynes's most revolutionary innovation, the "method of expectations".

The use of Keynes's method allows the theoretician to determine the equilibrium at a certain "date" (the "week" of the 'Bread' paper or, a few years later, of *VC*). There is of course the problem of stringing together the equilibria referring to successive "dates": it is the problem of the so-called "continuation" theory, a problem to which Hicks would come back over and over again for the rest of his life (1956, pp. 223-5; 1963, p. 315; 1965, p. 65; 1976, p. 290; 1976, p. 290; 1985, pp. 69-70). In this respect, however, the stance taken by Hicks in 1936 is a very cautious one:

It is, indeed, not impossible to say something about further effects; for we can deduce what the stocks of goods will be at the end of the period if the decisions are carried out, and this gives us a basis for the analysis of a second period. But it is probable that the change in actual production during the first period will influence the expectations ruling at the end of that period; and there is no means of telling what that influence will be. (Hicks 1935b, p. 87)

The first part of this passage is purely Walrasian theory (after the adoption of the "hypothèse des *bons*"). But the second part alludes to a source of dynamic indeterminacy (the unpredictable influence of current performance on end-of-period expectations) that had certainly not been contemplated by Walras, given his tacit assumption of stationary (or static, as they are currently called) expectations.

6. Hicks's Temporary Equilibrium Model: Walras *Retrouvé*?

As we have seen in the previous Section, during the long period of gestation of *VC* Hicks confronted most of the issues that had been left unsettled at the end of 1934, achieving some provisional results that, immediately made available through publication in scientific journals, would eventually be systematized and incorporated in *VC*. Yet, two important fields to which Hicks

had devoted much of his time and effort in the early 1930s, the field of consumer and producer choice and the equilibration issue, remained essentially frozen during the *VC* gestation period: Hicks was evidently working on these themes as well[20], but he was waiting for the appropriate framework where to fit his results. That framework was finally provided by *Value and Capital*.

As regards the theory of individual choice, as well as the related theories of demand and production, Hicks's chief problem in the mid-1930s was to find a way to coordinate the results achieved in this area in the first half of the 1930s with the results already obtained or hopefully to be obtained in other fields, such as the theory of money and capital, traditionally unrelated with the previous ones[21]. The opportunity to systematize all these apparently disparate subjects was offered by the process of writing up *Value and Capital*. In outlining the overall design of the book, Hicks took two related methodological steps that, independently of their intrinsic validity, which is indeed questionable and has in effect been repeatedly questioned over time, allowed him to cope with his still unsettled questions.

The first of the two steps consisted in sharply dividing Statics from Dynamics. At the very beginning of the dynamic part of *VC*, Hicks put forward the following blunt definitions:

> I call Economic Statics those parts of economic theory where we do not trouble about dating; Economic Dynamics those parts where every quantity must be dated. (Hicks 1939, p. 115)

The second step consisted in remorselessly subdividing the subject matter of economics into two neatly separate parts, each of which was then allocated to Statics or Dynamics, apparently with no overlapping between the two. To Statics Hicks allocated all of traditional choice, demand and production theory, that is, all those issues on which he had been working in the early 1930s, obtaining the results at first collected in his 1932 book and in the joint 1934 paper with Allen, and later summarised in the 1937 book in French. All the remaining parts of economics, instead, were allocated to Dynamics.

By taking a closer look at the static part of *VC*, however, one realizes that, apart from individual choice theory (both producer and consumer), also an important portion of market equilibrium theory is included in Statics. Yet, since in Statics no quantity is dated, all activities that are essentially related with time must be excluded from it. Therefore,

> [Statics] abstracts from capital and interest, saving and investment, and all that complex of activities which, in an earlier chapter, I called 'Speculation'. (Hicks 1939, p. 100)

To this list of excluded activities, which appears towards the end of the static part of *VC*, a further list is added at the beginning of the dynamic part, which includes "trade fluctuations" and "money" (1939, p. 116). What remains after all these exclusions essentially coincides with the activities allowed for in the consistent models of stationary equilibrium discussed in the previous Sections of the present paper, that is, all the activities and only those activities that are permitted to take place in a pure-flow economy.

So, in the end, a statical equilibrium model, in the *VC* sense, is nothing but a consistent stationary equilibrium model of a pure-flow economy, where, in accordance with Hicks's conventions and definitions, all the variables have been deprived of any time dimension. In the

[20] As far as the field of consumer and producer choice is concerned, a significant trace of Hicks's ongoing process of revision and development of the ideas originally put forward in the first half of the 1930s can be found in the book *Théorie mathématique de la valeur*, published in French in 1937 (Hicks 1937b), which somewhat anticipates the static part of *VC*.

[21] What we chiefly have in mind are the results reached by Hicks in his two 1935 papers: the 'Simplifying' paper, on the theory of money and portfolio choice (Hicks 1935a), and the 'Bread' paper, on the theory of capital, interest, prices, and price-expectations (Hicks 1935b).

traditional stationary equilibrium models of the consistent variety (such as Walras's exchange and production model in the second and third editions of the *Eléments*, Wicksell's model of a-capitalistic production, or the so-called Walras-Cassel production model) all the variables used to have a time dimension; yet, since the models belonging to this class can only deal with pure flows, all the variables were in effect referred to the same time period. The time dimension used to play a role in traditional stationary equilibrium models because those models were typically associated with an equilibration process, which requires the variables to be dated. According to the new distinction between Statics and Dynamics proposed by Hicks in *VC*, however, the analysis of the equilibration process becomes one of the issues to be studied by Economic Dynamics. Hence a consistent stationary equilibrium model of a pure-flow economy can be legitimately transformed into a timeless static model in the sense of Hicks.

Yet, even if a Hicksian static model is timeless, there are two ways of dating its variables which leave the formal structure of the model unaffected. If all the variables are assigned the same date, we obtain the model of a 'Spot Economy', that is, an economy where all transactions are for immediate delivery. On the contrary, a model where all variables are dated, but the dates are not all the same, can be interpreted as a 'Futures Economy', where markets are currently open for all present and future dates (Hicks 1939, p. 136). The 'Spot Economy' and the 'Futures Economy' are extreme cases of some theoretical interest[22]. They are dynamic models, according to Hicks's definitions of Statics and Dynamics, since in such models the variables are dated. But the very fact that such models are formally identical with a static model makes it apparent that Hicks's definitions in *VC* are not as clear-cut as he probably had expected or hoped for.

As we have seen, the analysis of the equilibration process, traditionally regarded (by Hicks himself before *VC*) as belonging to Statics, becomes a part of Dynamics in the *VC* theoretical framework: this is unavoidable, in the light of Hicks's new definitions, since "the adjustments needed to bring about equilibrium take time" (Hicks 1939, p. 116). So, let us now proceed to Dynamics, specifically to the second big issue still in need of clarification, namely, the equilibration issue.

In the 'Walras' paper, as we have seen in Sections 2 and 3 above, Hicks had eventually identified four alternative equilibration mechanisms through which market equilibrium can be supposed to be established: three of these mechanisms can be shown to be consistent with both the logic of general equilibrium theory and the determinacy requirement, while the fourth is not. Let us recall the main results. The inconsistent mechanism is best exemplified by the equilibration device that would be at work in a Walrasian exchange model where the "no trade out of equilibrium assumption" were not to hold. Of the three consistent processes, the first is instanced by the Walrasian virtual *tâtonnement* process in 'logical' time or, for that matter, by the similarly interpreted Edgeworth recontracting process; both processes are supposed to drive the economy towards an instantaneous equilibrium. The second is exemplified by the Walrasian observable *tâtonnement* process in 'real' time taking place in a pure-flow stationary economy or, for that matter, by the similarly interpreted Edgeworthian recontracting process; both processes are supposed to drive the pure-flow economy towards a stationary equilibrium. Finally, the third is represented by Marshall's observable equilibration process in 'real' time, as applied to a barter system or to the daily market for a consumer good or service under suitable assumptions about the traders' utility functions; this process would drive the system or the market towards a final equilibrium which is determinate, as far as the rate of exchange between the two commodities or the money price of the consumer good or service are concerned.

Now, according to Hicks (1939), of the three consistent mechanisms the first, namely, the Walras-Edgeworth virtual mechanism implying that equilibrium is actually reached before any

[22] Disguised under the mask of Hicks's 'Spot Economy' one can easily detect the old pure-flow economy traditionally analysed by the stationary equilibrium approach of the consistent variety. On the contrary, Hicks's 'Futures Economy' can legitimately be regarded as a forerunner of the so-called complete-market, Arrow-Debreu economy, at least when the latter is interpreted in the way suggested by Debreu (1959).

observable activities are allowed to take place, concretely applies to "highly exceptional" markets only. Hence Walras's and Edgeworth's virtual "solution (if it may be called one) [is] not very convincing" and must be put aside (Hicks 1939, p. 128). So one is left with the remaining two processes: the equilibration process under stationary conditions, which is called the "method of the Austrians" in *VC*, and the "method of Marshall" (Hicks 1939, p. 117).

Let us consider the first "method" first. As already explained, stationary equilibration processes of the consistent variety apply only to pure-flow economies. Yet, at the time of writing *VC*, Hicks was well-aware that the economists who had embraced the stationarity assumption over the previous decades – not only the Austrians, who for Hicks (1939, p. 117, fn. 1) were essentially Böhm-Bawerk and Wicksell, but also many others, among whom J.B. Clark, Cassel and Pigou are explicitly mentioned in this connection – had tried to use, and effectively used, this "method" to deal with capital as well.

Now, in the case of J.B. Clark and Cassel, their endeavour had led, according to Hicks, to "great errors" (p. 116, fn. 1). As to the Austrians, in *VC* Hicks is not so explicit, contenting himself with saying that the stationary-state approach had had "a baneful influence on the minds of economists", encouraging the neglect of all those time-related phenomena that are "supremely important" in economic dynamics (Hicks 1939, p. 119). Yet, it is precisely the very last paragraph of *VC* to dispel any possible doubt as to Hicks's ultimate opinion, in 1939, about the stationary-state approach:

> We began our study of dynamic economics by rejecting the concept of a stationary state as an analytical tool. We rejected it then, because it seemed to be no more than a special case, which offered no facility for generalization. We have come in the end to doubt whether it is even conceivable as a special case [...]. (Hicks 1939, p. 302)

So, in the end, Hicks is only left with

> the method of Marshall; though since, in the relevant part of Marshall's work, [...] he is concerned with the determination of the value of one commodity only, considered as much as possible in isolation, while we are concerned with the determination of the whole system of values, we cannot follow him in all respects. (Hicks 1939, pp. 119-20)

In view of the above exclusions and qualifications, therefore, as far as the equilibration issue is concerned, Hicks's stated aim in *VC* turns out to be the following: to extend Marshall's "method" to a multi-market setting, amending it as required by the larger context to which it is meant to apply. Yet, in spite of Hicks's claims, it can be easily shown that, in generalizing Marshall's "framework, so that it can be used for the discussion of the problems of a whole economic system" (1939, p. 122), Hicks ends up by jettisoning most of Marshall's conceptual framework.

As is well-known, in Book V of his *Principles*, Marshall had adopted a tripartite classification of time periods according to their 'length' (Day, Short Period, Long Period), to which a similar classification of equilibrium concepts was associated (temporary or market-day equilibrium, short-run normal equilibrium, long-run normal equilibrium). In Marshall's temporary equilibrium model the problem at issue was to study the exchange process involving a given stock of a certain commodity during a specified day; in the two normal equilibrium models, instead, Marshall's problem was to study the functioning of an "ongoing market", extending over a "short" or "long" sequence of days, in each of which a flow of a certain commodity was supposed to be produced and exchanged under unvarying market conditions (more or less restrictively specified according to the 'length' of the period).

According to Hicks (1939, p. 122), "these categories are suitable enough for Marshall's isolated market, but they hardly fit the analysis of the whole system". Hence, in *VC*, Marshall's Short Period and Long Period were dropped without any qualms, together with the associated normal equilibrium concepts – concepts that many economists, no doubt including Marshall himself, would have

regarded as the most characteristic concepts in the Marshallian system. After such unceremonious cuts, of Marshall's original tripartite classification only the Day and the associated temporary equilibrium concept were provisionally able to survive.

But this is not all. For, unlike Marshall, Hicks also wanted production to be allowed to take place during the Day in his *VC* temporary equilibrium model. In view of this, Hicks eventually decided to change the name of his shortest period, calling it "a Week, to distinguish it from Marshall's Day" (1939, p. 122)[23]. However, the Week was not to remain, after all, the shortest indivisible period in VC: for Hicks also assumed that markets could open and contracts be made only in the first day of the week; so that, for the purposes of bargaining, making contracts, and setting prices, as opposed to the mere carrying out of economic activities already fixed up in previously settled contracts, it was Monday to become the shortest unit period in *VC*.

Now, according to Hicks (1939, p. 120), in his temporary equilibrium model Marshall had put forward an "ingenious argument" by means of which, in spite of the equilibration process proceeding by trial and error, through bilateral exchanges taking place at prices different from the temporary equilibrium one, he had been able to show that the price at which the market would finish up at the end of the Day, that is, the temporary equilibrium price, was all the same perfectly determinate. In *VC* Hicks, after labelling as 'false' prices those prices (or, better, rates of exchange), that are implicit in the transactions carried out during a Marshallian equilibration process before the final equilibrium price is attained, suggested that Marshall's "ingenious argument" could survive basically unaltered in a general equilibrium framework (1939, pp. 120, 128). And then, in a Note on the "Formation of Prices" appended to Chapter IX of *VC*, the first chapter of the dynamic part of that book, he set out to prove his claim or, better, his conjecture (1939, pp. 127-9).

We shall come back to this in a while. For the time being, if we provisionally take for granted, for the discussion's sake, that Marshall's argument actually generalizes to "a whole economic system", we can conclude that all markets smoothly proceed by trail and error, by "higgling and bargaining", as Marshall used to say, through transactions at 'false' prices, to a determinate equilibrium position on each given Monday, a position which remains thereafter unaltered over the following week. The data of the economy, including the agents' expectations, change at the junctures between successive weeks, for both endogenous and exogenous reasons. The evolution of the economy over time, namely, its dynamics, can be described by means of a chronologically ordered sequence of temporary equilibria.

As can be immediately verified, the time structure of Hicks's temporary equilibrium model is tremendously similar to the time structure of Walras's general equilibrium theory after the adoption of the "hypothèse des *bons*", as stated in the passage of the fourth edition of the *Eléments* quoted at the beginning of Section 3 above: in fact, the equilibration process supposed to take place during Walras's "phase des *tâtonnements préliminaires*" is similar to the equilibration process occurring during the market hours on Hicks's 'Monday'; Walras's "phase statique" is analogous to Hicks's 'week'; finally, Hicks's change in the data at the juncture between successive weeks reminds us of Walras's "phase dynamique" of trouble and change in the data at the end of each "phase statique".

The similarity between the two approaches stands out even more clearly if one considers the following passage by Hicks (1939, p. 127):

> By using the week, we become able to treat a process of change as consisting of a series of temporary equilibria; this enables us still to use equilibrium analysis in the dynamic field.

In reading this sentence, the following question spontaneously arises: Which is the difference, if any, between Hicks's temporary equilibrium method, as described above, and Pareto's "method of successive equilibria" or Walras's method of "mobile equilibrium"?

[23] As will be recalled, the "week" had already made its appearance in (Hicks, 1935b) and, much before, in Berry (1891).

At first sight, the difference might appear to be flimsy or altogether inexistent. And yet, in 1934, Hicks had qualified as "rather pathetic" the passage by Walras, referred to above, on the time structure of the analysis after the adoption of the "hypothèse des *bons*". In the attempt to find some justification for Hicks's puzzling behaviour, one is led to look at the relevant passages more closely.

At a closer inspection, the chief point of difference between the two approaches appears to be the following: in Hicks's *VC* model the equilibration process underlying the temporary equilibrium concept is conceived as a 'real' time, observable disequilibrium process, in the spirit of Marshall's "higgling and bargaining" of the market, whereas in the fourth edition of Walras's *Eléments* the equilibration process supporting the Walrasian equilibrium concept (after the adoption of the "hypothèse des *bons*") is explicitly viewed as an unobservable process in 'logical' time. After all, this is precisely the point that Hicks himself decided to stress when, after explaining why he could "not employ Marshall's tripartite classification" as such in the *VC* model, he immediately added that he would nevertheless endeavour "to keep the truth it embodies (the time taken in adjustment) clearly in mind" (1939, p. 122).

In view of this, it now becomes important to answer the question that has been raised and immediately postponed above: Is Hicks's generalization of Marshall's "ingenious argument" really legitimate? The way in which this question is answered will allow us to reach a definite conclusion on whether or not there exist any significant difference, at least as far as the equilibration process and the time structure of the analysis are concerned, between Walras's instantaneous equilibrium concept in the fourth edition of the *Eléments* and Hicks's temporary equilibrium concept in *VC*.

Hicks's argument in the Note on "The Formation of Prices" appended to Chapter IX of *VC* runs as follows. In Marshall's partial equilibrium analysis, trading at 'false' prices gives rise to income effects, which are then sterilised by Marshall's assumption about the constancy of the marginal utility of money[24]; the latter assumption, in turn, can be empirically justified for all commodities on which the consumer's expenditure is small with respect to his total income. Hicks then proceeds as follows:

> It remains true in the general case, just as in Marshall's special case, that gains and losses due to false trading only give rise to income effects – effects, that is, which are the same in kind as the income effects which may have to be considered even when we suppose equilibrium prices to be fixed straight away. (Hicks 1939, p. 129)

Now, so Hicks's argument proceeds, in standard static theory of the type discussed in the first part of *VC*,

> [we] have seen again and again that a certain degree of indeterminateness is nearly always imparted by income effects to the laws of economic theory. All that happens as a result of false trading is that this indeterminateness is somewhat intensified. (Hicks 1939, p. 129)

But static theory has long since got accustomed to the sort of indeterminacy brought about by income effects and, by the end of the 1930s, it is well prepared to deal with it: the analysis of static equilibrium is by no means impaired by the recognition of the existence of income effects. "Just as in statics", therefore, also in the analysis of the equilibration process we may expect that trading at 'false' prices, with its ensuing income effects, ought not to disturb the quick convergence to a determinate equilibrium, particularly since "we may reasonably suppose that the transactions which take place at 'very false' prices are limited in volume". (Hicks 1939, p. 129)

As can be seen, the very conclusion eventually reached by Hicks is very weak in itself, for no robust theoretical argument would concede that the convergence to a determinate equilibrium ultimately depends on the "supposition", however "reasonable" it may be, "that the transactions

[24] See above, Section 2, particularly fn. 8.

which take place at 'very false' prices are limited in volume". Yet, this is not the most disturbing aspect of Hicks's overall argument: for much more troubling is the fact that not only are the premises on which Hicks's conclusion rests shaky, but also the alleged 'proof' of the desired result is faulty.

To see this, let us start by explaining how the idea of 'generalising' Marshall's "ingenious argument" to a general equilibrium setting ought to be interpreted. First of all, it should be noted that, if one were willing to confine one's attention to a pure-exchange economy and to assume that all the traders participating in such economy are characterised by additively separable utility functions which are quasi-linear in money (or a money commodity), then no formal 'generalisation' of Marshall's temporary equilibrium model would in effect be required in order to make it applicable to a multi-market economy: for, in such a case, Marshall's assumptions, by themselves, would be enough to provide a fairly 'general' theory of the functioning of all the markets existing in the economy, however numerous they might be. It would of course be true that, under such conditions, with no need to make any special *ceteris paribus* assumption, the functioning of each individual market, on which one individual consumer good is traded for money, might or even should be studied in isolation, all income effects would be cancelled, and finally - what is hardly mentioned in this context - all inter-commodity substitution effects would disappear too. But the economy would still remain a multi-market economy, if it were so from the beginning.

Hence, when Hicks thinks of a possible 'generalisation' of the arguments employed by Marshall in his temporary equilibrium model, he is not simply suggesting to extend the analysis to a multi-commodity economy, but he wants the proposed extension to apply to an economy characterised by truly interconnected markets, where income and inter-commodity substitution effects are allowed to occur and production is permitted to take place. In order to reach this goal, therefore, Hicks drops Marshall's assumption of additively separable and quasi-linear (in money) utility functions, refrains from making any special *ceteris paribus* assumption, and studies the effects of out-of-equilibrium transactions ("false trading") on the working of a multi-commodity economy with interconnected markets - which, however, is tacitly assumed to simply be a pure-exchange economy in the Note on "The Formation of Prices".

This is Hicks's "general case", with reference to which he makes essentially three statements: 1) "that gains and losses due to false trading only give rise to income effects"; 2) that these "income effects [...] are the same in kind as the income effects which may have to be considered even when we suppose equilibrium prices to be fixed straight away", i.e., the same in kind as those arising in static analysis; 3) that "[a]ll that happens as a result of false trading is that [the] indeterminateness [already due to static income effects] is somewhat intensified". But, while the first statement is at best grossly misleading, the second and the third are positively wrong.

As to the first statement, one should consider that, in Marshall's temporary equilibrium model, when each individual market can be examined in isolation, the income effects arising from trading at 'false' prices are indeed sterilised, due to the constant marginal utility of money assumption; but the determinacy of both the temporary equilibrium price and the quantity traded of the commodity concerned, as well as its independence of the path followed by the sequence of 'false' prices obtaining over the equilibration process, do not simply depend on assuming away all income effects, but also on the fact that, in Marshall's temporary equilibrium model of an individual market, demand and supply functions can be unambiguously defined, together with the limits within which 'false' prices can range over the equilibration process, on the only knowledge of the traders' utility functions, without any assumption whatsoever of price-taking behaviour on the part of the traders being either necessary or possible[25]. When we come to Hicks's "general case", however, and allow for the possible occurrence of actual transactions out of equilibrium, we are completely at a loss, for we are unable to specify not only the 'false' prices at which the transactions might take place, but also the 'false' amounts of the commodities that might be actually traded: Marshall's

[25] On this, see Donzelli (2008, Section 4).

theory cannot be of any help, here, due to the assumed generality of the utility functions and the ensuing interconnectedness of markets; but Walras's theory cannot be of any avail, too, for Walrasian demand and supply functions can only allow the theorists to predict desired transactions at prices taken as given by the traders, being completely powerless when it comes to predict actual transactions occurring at otherwise unspecified and unspecifiable 'false' prices[26]. Reducing all this mess to the fact "that gains and losses due to false trading only give rise to income effects", as Hicks appears to suggest, is misleading, to say the least.

In the last analysis, Hicks's confusion in this respect can probably be traced to his unwarranted belief, documented in Section 2 above, that Marshall's idea of competition and competitive behaviour is after all very similar to Walras's idea of competition and price-taking behaviour, what is certainly not true. In the small world of Marshall's temporary equilibrium model, observable and irreversible disequilibrium behaviour can be allowed for, without hampering in the least the determinateness of the final equilibrium (as far as price and quantity traded of the commodity under investigation are concerned). On the contrary, in the large world of interconnected markets characteristic of both Walras's general equilibrium theory and Hicks's *VC* temporary equilibrium model, allowing for observable and irreversible disequilibrium behaviour would cause the whole theoretical structure to fall to pieces, due to the occurrence of transactions on which the theory has nothing to say, so that no prediction could be made concerning the transactions actually taking place, or the path possibly traced over time by the trading process, or, finally, the ultimate outcome (if any) of the process.

Having discussed at length the ambiguities surrounding Hicks's first statement concerning the possible extension of Marshall's "ingenious argument" to Hicks's "general case", it is now easy to explain why Hicks's second and third statements are mistaken. The income effects studied by standard static theory have to do with the effects of notional changes in relative prices on Walrasian individual, hence aggregate, demands of price-taking consumers. All these effects are perfectly determinate from a theoretical point of view, provided that consumers' utility functions are specified. What is *a priori* uncertain is the sign of the income effect; but such uncertainty can be immediately dispelled, as soon as consumers' preferences are known. Similarly, the "indeterminateness [...] imparted by income effects to the laws of economic theory", e.g., to the uncompensated law of demand, is really such only *a priori*: for, if the appropriate data were known, no indeterminateness would be left. Altogether different is the nature of the income effects caused by 'false trading', as well as the indeterminateness to which they give rise. Such effects have nothing to do with notional changes in relative prices: they are simply part (perhaps, the smallest part) of the disruptive effects produced on the functioning of a system of truly interdependent markets by the actual carrying out of disequilibrium trades. Since no theory is available to explain such effects, they must remain obscure, even if the (Walrasian) data of the economy were perfectly known. For the same reason, the indeterminateness they impart to the laws of economic theory, specifically to the laws of equilibrium establishment, is not bound to disappear, whatever amount of (Walrasian) data might be at hand. What is missing is not the data, rather the theory explaining what might occur out of equilibrium.

To sum up on the equilibration issue, Hicks's attempt at making his approach more 'realistic' in a Marshallian spirit ends up in a complete failure. In the 'Walras' paper (1934), as recalled above, Hicks had doubted of the possibility of generalising the Marshallian equilibration mechanism to a multi-market context; five years later, in *Value and Capital*, he eventually persuaded himself not only that there is a way in which such a generalisation can be carried out and made acceptable, but also that he had been able to discover that way; but unfortunately his conjecture was wrong. So that, in the end, if one were asked by which sort of equilibration process Hicks's temporary equilibrium concept can be supported, one ought to conclude that any such process could not significantly differ

[26] Many years after *VC*, Hicks himself (1965, p. 77) would appear to recognise that "[i]f we abandon the demand-supply equation, [we do] not have any way of determining prices. [...] The determination of prices is taken right outside the model."

from the virtual process in 'logical' time, advocated by Walras in the fourth edition of the *Eléments* to support his instantaneous equilibrium concept. Hicks himself, as we shall see in a moment, came very close to admitting this in *VC*.

We have now reached a point where we can draw some conclusions as to the relationship between the *VC* temporary equilibrium model, the final upshot of the winding path followed by Hicks's system of thought over the 1930s, and the Walrasian and Paretian tradition of the previous decades.

Over the 1930s, as has been seen, Hicks had been able to make important progress over Walras and Pareto in a number of diverse fields, bringing about theoretical innovations and analytical improvements that caused general equilibrium theory at the end of the 1930s to be something altogether different from what it had been at the beginning. Apart from achieving remarkable results in the theory of choice and equilibrium strictly speaking, Hicks had made advances in the more specialised fields of uncertainty, money and financial assets, expectations, intertemporal relations and planning, indirectly contributing, in this way, to the theory of capital too.

All these results were incorporated into the *VC* temporary equilibrium model, which is therefore by far richer than Walras's original model in all these respects. In particular, the following two points should be stressed: 1) while in his *VC* temporary equilibrium model Hicks made a very conscious use of generalised competitive point expectation functions, making expectations of future prices depend on current (and occasionally past) prices according to functions that can in principle take any analytical form, Walras had only tacitly introduced stationary price expectations into his analysis, making of them quite a parsimonious use; 2) while in his *VC* temporary equilibrium model Hicks systematically employed the notion of intertemporal plan, thereby making it possible to distinguish between equilibrium at a point of time and equilibrium over time and paving the way for the possible discussion of intertemporal disequilibrium, Walras had confined his attention to currently open markets only, thereby telescoping all future planning into the agents' current choices among currently available capital goods.

All these differences are no doubt very significant. And yet, as regards the interpretation of both the equilibration process and the equilibrium concept *stricto sensu*, there are no major distinctions to be drawn between the results eventually arrived at by Walras in 1900, after a quarter of a century of revisions and amendments, and those reached by Hicks in 1939, after ten years of reflections and turnabouts: in both cases the equilibration process is essentially virtual and the equilibrium concept is "essentially instantaneous"[27]. The only difference is that, in Hicks's case, the virtual character of the process and the instantaneous nature of the equilibrium concept are disguised under a mask of superficial Marshallianism, with all its pretended, but apparently reassuring, insistence on the "time taken in adjustment", as well as on the alleged observability and irreversibility of disequilibrium behaviour.

Hicks himself, on the other hand, seemed occasionally to be aware, already in 1939, of the true nature of his constructs and of the real implications of his assumptions. In fact, at the beginning of Chapter X of *VC*, less than two pages after that Note on "The Formation of Prices" that had been designed precisely to account for the time-consuming, observable disequilibrium process that ought to precede the establishment of the temporary equilibrium on any given Monday, we can find the following, disarming statement:

[27] In the Additional Note C, a Note on "Professor Samuelson's Dynamic Theory" appended to the second edition of *Value and Capital*, which was published seven years after the first (Hicks 1946, p. 337), Hicks decided to use exactly the expression "essentially instantaneous" to describe the nature of the process of adjustment underlying the temporary equilibrium concept employed in the first edition of *VC*. This should be compared with the expression used by Hicks in the first edition of his *Theory of Wages* (1932), where the equilibration processes are said to be "by no means instantaneous". On this see also De Vroey (1999b, p. 38).

> Since we shall not pay much attention to the process of equilibration which must precede the formation of the equilibrium prices, our method seems to imply that we conceive of the economic system as being always in equilibrium. (Hicks 1939, p. 131)

In the years and decades following the publication of *Value and Capital*, Hicks's awareness of the ultimate implications of the temporary equilibrium method, which is only embryonic in 1939, grew stronger and stronger, leading him eventually to disavow it:

> The fundamental weakness of the Temporary Equilibrium method is the assumption, which it is obliged to make, that the market is in equilibrium – actual demand equals desired demand, actual supply equals desired supply – even in the *very* short period, which is what its single period must be taken to be. This assumption comes down from Marshall, but even in a very competitive economy, such very short-run equilibration is hard to swallow [...]. (Hicks 1965, p. 76)

And yet, even in 1965, at a time when Hicks was fully prepared to repudiate his foremost intellectual offspring of the 1930s, the Temporary Equilibrium method, he was not yet ready to disown the alleged Marshallian parentage of the *VC* temporary equilibrium model, as can be seen from the above-quoted passage. Nor was he willing to give up the comforting fiction of a 'real' time equilibration process where observable disequilibrium transactions take place at 'false' prices, as can be inferred from the following passage, where the structure of the *VC* temporary equilibrium model is being once again reconsidered:

> Monday's trading proceeds until prices are established that equate demands and supplies, for goods and services to be delivered during the 'week'. It is not supposed that equilibrium prices are established at once; there may be a good deal of 'false' trading before they are established. (Hicks 1965, p. 66)

7. Concluding remarks

After many explorations in different directions during the early 1930s, Hicks ended up in 1934 by advocating an interpretation of Walras's equilibrium and capital theory along stationary lines, an interpretation that does not correspond to the view endorsed by the last Walras and by Pareto. This was due, in part, to the influence of the scientific environment surrounding Hicks in the years of his formation and, for another part, to Walras's sheer confusion and Pareto's excessive conciseness and insufficient analysis. But it also revealed the existence of inner difficulties in the general equilibrium approach, particularly as far as the equilibration process is concerned.

In the following years, however, Hicks embarked upon a systematic revision of his inherited or acquired ideas in a number of fields, first of all in the general theoretical field of equilibrium and equilibration, a revision that followed a path very similar to that covered by Walras himself in the last quarter of the Nineteenth century. In the second half of the 1930s, during the long gestation of *Value and Capital*, Hicks's ideas on equilibrium and capital progressively changed and matured, to eventually culminate, with the publication of *VC* in 1939, in the rediscovery of a method of analysis and an equilibrium concept, Hicks's temporary equilibrium, that are substantially similar to the method of analysis and equilibrium concept ("mobile equilibrium" and "method of successive equilibria") put forward by the last Walras and by Pareto.

Yet this direct link with the Walrasian tradition was not overtly recognised by Hicks in *VC*. In particular, the essentially Walrasian character of the equilibration process supporting Hicks's temporary equilibrium concept was carefully disguised under Marshallian garments in Hicks's 1939 great book. This fact delayed Hicks's own recognition of the limits of the *VC* approach, which

would start to be cautiously questioned by him only in 1956 and, more openly, since 1965[28]. Yet, in view of the enormous diffusion of *VC* and the centrality of its role in the development of the neo-Walrasian programme, Hicks's ambiguities as to the ascendance of *VC* have also had two less private consequences.

From a historiographical point of view, Hicks's ambiguities have concurred to spread the legend that the *VC* temporary equilibrium concept was an entirely new concept, essentially due to Hicks himself and a few other economists at work in the late 1920s and in the early and mid-1930s. It was only since the late 1970s, with the works of Diewert (1977) and Morishima (1977), later followed by Witteloostuijn and Maks, (1988) and (1990), and others, that the origin of the concept was eventually traced back to Walras.

From a theoretical point of view, a fuller awareness of the tormented history of the temporary equilibrium concept, and of the reasons explaining such a winding path from Walras's time onwards, would have helped to build the neo-Walrasian programme on partially different and perhaps sounder foundations.

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[28] See Hicks (1956, p. 223-5), (1965, Ch. pp. 70-5), (1976, pp. 290, 296), (1980a, p. 320), and (1985, pp. 73-80), where one can find explicit critiques of the *VC* temporary equilibrium method. It might be claimed, however, that well before 1956 Hicks's dissatisfaction with that method had already been revealed, though only implicitly, by his decision to adopt an altogether different method of analysis in his theoretical investigations of the late 1940s and early 1950s, particularly in his 1950 book on trade cycle theory (Hicks 1950).

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