

The History of Macroeconomics Viewed Against the Background of the Marshall-Walras Divide

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Abstract

Analysing the recent history of macroeconomics, my paper claims that the new classical revolution should be viewed as a substitution of Walrasian macroeconomics to the earlier prevailing Marshallian macroeconomics. The first part of the paper is concerned with two conceptual prerequisites, the relationship between macroeconomics and general equilibrium, the meaning of the “Keynesian” modifier. In a second part, the different facets of the Marshall-Walras divide are expounded. My general claim is substantiated in the third paper of the paper. Herein, I claim that the IS-LM model is a simplified *Marshallian* general equilibrium model while real business cycle models belong to the Walrasian research programme. Finally, I express my scepticism as to the possibility of a New Neoclassical Synthesis.

KEYWORDS: History of macroeconomics, Neoclassical Synthesis, New Neoclassical Synthesis, Keynesianism, Monetarism

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1. Introduction

The demise of the line of thought embodied in the IS-LM model and its replacement by stochastic intertemporal equilibrium models is probably the most salient change that occurred in macroeconomics during the last quarter of the twentieth century.¹ My aim in this paper is to analyse it against the background of what I call the Marshall-Walras divide. According to the latter, the contributions of Marshall and Walras to economic theory are less complementary than usually believed. Instead, the Marshallian and the Walrasian approaches are considered alternative research programmes. The underlying reason is their being based on radical different trade technology assumptions. This divide, I will claim, has a direct bearing on the unfolding of macroeconomics: the change that has taken place about two decades ago is the substitution of Walrasian macroeconomics for Marshallian macroeconomics.²

Before addressing this claim a twofold clarification task must be undertaken. First, I need to make clear what I mean by the terms “general equilibrium model” and “macroeconomics”, “Keynesian approach”. Second, I must spell out how I understand the Marshall-Walras divide. Only after having dealt with these preliminaries, will I be able to expound my claim in a non-equivocal way.

2 Terminology

2.1 General equilibrium models and macroeconomics

Usually a general equilibrium model is associated with Walrasian or neo-Walrasian theory, that is, Walras’ own theory as expounded in his *Elements of Pure Economics* (1954) or the Arrow-Debreu model.³ The implication is that there exists only one way of doing general equilibrium analysis, the Walrasian. Instead, I will suggest a broader conception resulting from positing that general equilibrium analysis exists as soon as two criteria are met. First, the

¹ Several interesting papers on this subject have been published recently: Blanchard (2000), Blinder ([1988] 1997), Drèze (2001), Danthine (1997), Hairault (1999), Lipsey (2000), Snowden and Vane (1996) and Woodford (1999).

² This claim was voiced by some authors who studied what at the time was called “Monetarism Mark I” and “Monetarism Mark II”, associated respectively with Friedman’s and Lucas’s Phillips curve models (Friedman 1968, Lucas 1972). For example, Hoover noted that “Friedman, as one important monetarist, differs from the new classicals on a fundamental point of methodology: he is a Marshallian; they are Walrasians” [(1984) 1990, 528].

³ Henceforth the term Walrasian will be understood in a broad sense as encompassing neo-Walrasian models (the Arrow-Debreu model and its followers) or even models that are often misleadingly called non-Walrasian such as the Barro-Grossman model.

object of analysis must be an economy as whole rather than an isolated sub-section of the economy. Second, the analysis must deal with a formalised or mathematical model.⁴

The point to be underlined, because it goes against the grain, is that the Walrasian approach has no monopoly over general equilibrium theory. It is true that most general equilibrium models belong to the Walrasian approach but this actual hegemony cannot be viewed as matter of principle. Alternative programmes cannot be excluded.

My next step is to see how the categories of general equilibrium model and macroeconomic model fit together. What exactly is macroeconomics? In their recent accounts of the unfolding of macroeconomics, written at the occasion of the millennium, Blanchard and Woodford concur in stating that macroeconomics consists of the study of fluctuations (Blanchard 2000: 2; Woodford: 1999: 1). It is of course true that present macroeconomics deals with fluctuations, yet it should not be inferred that any study of fluctuations is macroeconomics. Moreover, if this definitional stance is taken, it turns out that the IS-LM model does not belong to macroeconomics!

The term macroeconomics is usually ascribed to Ragnar Frisch, who coined it in his essay “Propagation Problems and Impulse Problems in Dynamic Economics” (1933: 172). To him, its concern was “the whole economic system in its entirety” while microeconomics was defined as concerned with individual behaviour. Unfortunately, this cogent conception has not been followed, and the meaning of macroeconomics has become messy. What is now called macroeconomics deals with a whole economy, yet not every analysis bearing on such an object is usually considered macroeconomics. Take Walrasian theory. Beyond doubt, it studies the economy as a whole, yet it is ranked as microeconomics. In contrast, why count real business cycles models, which claim a Walrasian heritage, as macroeconomics ?

To solve this puzzle, we should return to Frisch’s viewpoint and consider that macroeconomics belongs to the domain of general equilibrium.⁵ In effect, it meets the two criteria posited above. However, it should be viewed as a sub-category of general equilibrium analysis rather than being equated with it. In other words, different types of general equilibrium analysis must be separated. My suggestion is to draw a distinction between complex and simplified general equilibrium models.

Macroeconomics then deals with simplified general equilibrium models. They comprise a small number of variables. They study a few markets, named after those markets that are deemed to be the most important real-world markets. They deal at once with aggregates, by reasoning in terms of representative firms or agents. They embed a few institutions, such as

⁴ The rationale for this second condition is that, in its absence, I should always need to draw a distinction between earlier general equilibrium theories, reasoning in prose, and formalised general equilibrium models.

⁵ This viewpoint was already expressed by Patinkin (1965). It is also to be found in Woodford (1999: 8), Farmer (1993 : 1) and Young and Zilberfarb (2000: 1).

the government and the central bank. They are geared towards addressing policy issues, such as the level of employment, national output, inflation, government deficit, effects of changes in money supply, etc. Finally, they claim to be empirically relevant and to give results which can be tested against reality. In contrast, Walras' model or the Arrow-Debreu model involve a great number of agents and commodities. They are geared towards more abstract purposes, like demonstrating the logical existence of equilibrium, stability, etc. Therefore, what is usually "general equilibrium analysis" can be recast as "complex general equilibrium analysis".

If macroeconomics belongs to the general equilibrium line of research, it is concerned with mathematical models. Accepting that reasoning in prose is not a model *strictu sensu*, the arising of macroeconomics should be ascribed not to Keynes' *General Theory* but to the subsequent models that tried to translate Keynes' blurred message into a precise model. Here the IS-LM model stands out. Thus, if my definitional stance is taken, macroeconomics started with the IS-LM model.⁶

To summarise, general equilibrium analysis can be sub-divided along two lines. First, a distinction must be drawn between Walrasian general equilibrium, the Walrasian modifier being understood in its broadest sense, and what could be called "Non-Walrasian" general equilibrium models had this modifier not be pre-empted by models that actually belong to the Walrasian line. The contending alternative line that I will consider is general equilibrium in the Marshallian approach.⁷ The second divide relates to the division between complex and simplified general equilibrium models. By crossing the two criteria, one gets the following box-diagram.

⁶ Several passages of Laidler's book on the Keynesian revolution (1999) hint at my viewpoint, although this is not its main thrust. Let me quote one of them. « Nevertheless, the impression of continuity in the development of macroeconomics which one might get from the very selective survey of reactions to the *General Theory* presented in the foregoing chapters would be misleadingly incomplete. A break did occur around 1936, and it did center on the *General Theory*. The break in question, however, arose not directly from that book itself, but from the IS-LM model, which a number of younger commentators found in it and used to expound not just what they took to be its main message, but that of an earlier classical tradition too. » (1999: 303). Other passages are on p. 318 and 324.

⁷ As well-known, Marshall is an author who can be pulled into opposed directions. There is Marshall the evolutionary-institutionalist economist, often associated with Book IV of the *Principles* and Marshall the value theorist, associated with its Book V. Although the first of these two Marshalls has become more popular than the second, my interest here lies with the latter. Thus, when speaking of Marshallian theory I have in mind the reconstruction of his "great fifth book on 'General Relations of Demand, Supply, and Value'", to borrow Hicks' characterisation (1946: 119).

	Complex general equilibrium models	Simplified general equilibrium models (i.e. macroeconomics)
The Walrasian approach	_____	_____
The Marshallian approach	_____	_____

The very story to be told in part 4 of my paper is that of the gradual filling in of the diagram. The first research programme to be implemented corresponded to the upper-left box, concomitant to the publication of Walras' *Elements of Pure Economics* (first edition in two instalments in 1874-7). The second step was the invention of macroeconomics with Hicks' IS-LM model later recast by Modigliani (1944).⁸ This model, I will claim, belongs to the Marshallian approach. The third step, the establishment of Walrasian macroeconomics has been a more protracted phenomenon. Patinkin can be considered the first modern author having constructed a simplified Walrasian general equilibrium model in his *Money, Interest and Prices* (1965; first edition 1956). It was later revived by Barro and Grossman in their disequilibrium model. Although Patinkin's book exerted an importance influence, the model it contained was never viewed as an alternative to IS-LM base camp for macroeconomics. Nor did Patinkin campaigned to this effect. As far as the disequilibrium *à la* Barro and Grossman is concerned, its vogue has been short-lived. The age of Walrasian macroeconomics had to wait the emergence of real business cycle models, themselves the offspring of Lucas' Neutrality of Money paper (1972). Finally, I associate the creation of Marshallian complex general equilibrium theory with imperfectly competitive general equilibrium models. In sum, while Marshallian macroeconomics predates Walrasian macroeconomics, the contrary is true for what concerns complex general equilibrium modelisation.

⁸ Cf. De Vroey (2000).

	Complex general equilibrium models	Simplified general equilibrium models (i.e. macroeconomics)
The Walrasian approach	<i>The Walrasian model (1874-77)</i>	- <i>First attempts: Patinkin (1956), Barro and Grossman (1971)</i> - <i>Full-scale emergence: real Business Cycle Models</i>
The Marshallian approach	<i>Imperfectly competitive general equilibrium models (e.g. Hart 1979)</i>	<i>The IS-LM model: Hicks (1937), Modigliani (1944)</i>

2.2 Elucidating the “Keynesian” modifier

In his 1988 paper criticising the new classical approach, Blinder draws the following distinction between normative and positive Keynesianism.

The division of Keynesian economics into positive and normative components is central to understanding both the academic debate and its relevance to policy. Positive Keynesianism is a matter of scientific judgement. A positive Keynesian believes that both monetary and fiscal policy can change aggregate demand, that fluctuations in aggregate demand have real effects, and that prices and wages do not move rapidly to clear markets. No policy prescriptions follow from these beliefs alone. ... Normative Keynesians add both value judgement and political judgement to the preceding list. A normative Keynesian believes that government should use its leverage over aggregate demand to reduce the amplitude of business cycles. He or she is probably far more interested in filling in cyclical troughs than in shaving off peaks. These normative propositions are based on judgements that (a) macroeconomic fluctuations significantly reduce social welfare, (b) the government is knowledgeable and capable enough to improve upon free-market outcomes, and (c) unemployment is a more important problem than inflation (Blinder 1988: 112-3)

Blinder’s insight can be cast differently by drawing a distinction between a theoretical framework or apparatus, on the one hand, and the “policy cause” which it may serve, on the other. The “Keynesian apparatus” term then refers to what Blinder calls “positive Keynesianism”, the “Keynesian policy cause” label to his “normative Keynesianism”.

Most commentators will agree that the IS-LM model is the Keynesian theoretical framework *par excellence* – for sure, it has been the most influential translation of Keynes’s in prose reasoning into a model. But the ‘Keynesian’ modifier can also be used in reference to what

motivated Keynes to write the *General Theory*. In this line, it can be viewed as a catchword for regrouping authors who think that, for all its virtues, the market economy can exhibit market failures, which state interventions, in particular demand stimulation, are able to remedy upon. The *laissez-faire* policy cause, which pleads against such interventions, can then be viewed as the non-Keynesian or anti-Keynesian policy cause.

In an interview with Foley, Leontief remarked that in his opinion Keynes developed his theory essentially as an instrument to support his policy advice (1998: 122). Put crudely, my viewpoint is that this observation extends beyond Keynes' person and applies to Keynesians and anti-Keynesians (in the policy cause sense) alike. Moreover, I think that there is nothing to be blamed about such an attitude.

To further compound the matter, no one-to-one relationship exists between the Keynesian framework and the Keynesian policy cause or between the non-Keynesian framework and the anti-Keynesian policy cause. While the launching of the Keynesian apparatus was motivated by the will to support the Keynesian policy cause, authors who were anti-Keynesian from a policy perspective – Milton Friedman being the emblematic example – were able to subvert it and use it as a weapon for the *laissez-faire* cause. Thus, Friedman, although being anti-Keynesian from the policy cause perspective, must be considered a Keynesian author methodologically speaking – i.e., in so far as the modifier refers to the theoretical apparatus dimension. Likewise, while the non-Keynesian conceptual apparatus, to be associated with new classical economics, was initiated by authors wanting to defend the anti-Keynesian policy cause, this does not exclude the possibility of it becoming subverted in its turn. This is the very task that New Keynesian economists set themselves forth. Against my distinction, they are non-Keynesian from the theoretical framework viewpoint yet Keynesian from the policy cause perspective.

3 The Marshall-Walras divide

The standard view about the Marshallian and the Walrasian approaches, the former being narrowed down to Book V of the *Principles*, is that they are complementary.⁹ They share the same subjective theory of value grounding market supply and demand functions in agents' optimising behaviour. Moreover, a division of labour seems to be existing between them, with Marshallian theory focusing on the study of isolated parts of the economy and Walrasian theory on the study of the economy as a whole. The latter then appears to be the generalisation of the former.¹⁰

⁹ It is e.g. expressed in Hicks ([1934] 1983: 86).

¹⁰ Marshall's only foray in the study of general equilibrium was in his Mathematical Notes XIV and XXI (on this, see Dimand (1990). Why did he not try harder to develop a theory as to the functioning of the economy as a

Following Clower and Leijonhufvud (Clower and Leijonhufvud 1975, Leijonhufvud 1999), I dissent from this view. I share their belief that there is room for a Marshallian general equilibrium approach that is distinct from the Walrasian. Moreover, like them, I am of the opinion that the possible emergence of alternatives to Walrasian general equilibrium theory is a matter of trade technology, i.e. the institutional set-up enabling equilibrium outcomes to be arrived at.

If it were accepted that the exclusive issue economic theory needs to address is that of the logical existence of equilibrium, then the matter would be sealed: no need for an alternative theoretical construction exists. Walras paved the way to solving it and Arrow and Debreu finalised the job by constructing the benchmark general equilibrium model. To all intents and purposes most economists have no qualm with such a restricted approach. Yet, economic theory cannot limit itself to the issues of the logical existence and stability of equilibrium. Trade technology must be taken in earnest.

3.1 The Walrasian trade technology

One of Walras' strokes of genius was to have started his analysis with considering an entire economy at once, rather than a section of it. A Walrasian economy constitutes a single centralised market, encompassing every agent and every good and service. The auctioneer, though not explicitly introduced by Walras in the *Elements of Pure Economics*, is the cornerstone of the Walrasian trade technology (De Vroey 1998). His main task consists of using the price system in order to make individual trade offers compatible. To this end, he announces price vectors to which agents react by expressing the optimal quantity they offer to trade. Prices are changed until general equilibrium is reached – i.e. whenever every market excess demand is nil or negative, the price being equal to zero in the latter case. Most general equilibrium theorists who recognise the auctioneer's role only grudgingly and openly declare their disliking. In contrast, I think that it is integral to the neo-Walrasian research programme. To say it bluntly and at the risk of raising the eyebrows of important commentators of Walras, such as Walker (1996), as well as of many present-day Walrasian theorists – yet not Lucas (see Lucas 1986) – the Walrasian approach cannot dispense with the auctioneer, because it is the only working trade technology assumption on stock.

This being granted, the need for making explicit the whereabouts of the auctioneer-led *tâtonnement* process cannot be side-stepped. On top of what I already stated, other characteristics are worth mentioning. The formation of equilibrium proceeds in one stroke, involving all goods and agents. At each trade round everything occurs simultaneously. Actual

whole? Two possible answers come to the mind. First, he might have considered that this subject-matter was not concrete enough. Second, he might have been aware that the tools for such a study were unavailable.

transactions remain suspended until the equilibrium price vector is arrived at. Although there is less consensus on this point, I am of the opinion that a Walrasian economy is basically non-monetary, in spite of the existence of a *numéraire*-good.¹¹ Another point that needs to be clarified is the information structure. In the canonical Walrasian or neo-Walrasian model, agents are supposed to hold perfect information over the prices announced by the auctioneer, the quality of goods and services, and the states of the world. However, due to the presence of the auctioneer, they are not supposed to be knowledgeable over other agents' demand and supply functions, nor over the factors underpinning them. All this still makes for a quite heroic information assumption yet, as will be seen, it is less heroic that the informational assumption underpinning Marshallian theory. Finally, consider the communication structure of an auctioneer-led system. The auctioneer economy is a set of bilateral relationships between the auctioneer and isolated individual agents. Before the attainment of equilibrium, agents' exclusive social link is with the auctioneer. They do not interact and communicate between them. Nor, as stated, are they cognisant about excess demand functions and the data that underpin them. An important implication ensues. Whenever a given agent makes a trading offer by responding to the prices announced by the auctioneer, he ignores how many other agents are making an offer similar to his. This means that an agent can be in a monopolistic position whilst being totally unaware and hence incapable of taking advantage of it! In other words, the tâtonnement set-up itself guarantees the "perfectness" of competition, whatever the possible monopolistic factors that may be present in the economy. Agents cannot influence prices because the institutional device is such as to preclude it.¹² The auctioneer hypothesis and imperfect competition, it turns out, are incompatible bedfellows.

3. 2 Extending the Marshallian trade technology to the economy as a whole

Imagine that Walras never wrote his *Elements*, and that only Marshall's writings were available for economists wanting to build a general equilibrium theory.¹³

Marshall gave implicitly a clue as how to proceed. Walras' decision to study an entire economy from the onset was counter-intuitive. Intuitively, one would rather take a stepwise approach consisting of studying the functioning of an isolated typical market in the first stage of the enterprise while relegating the study of the interrelationship of markets, the piecing

¹¹ This is clear for what concerns the Arrow-Debreu model. It is likewise so for Walras' *Elements* in so far as it is accepted that Walras' introduction of money in his model of circulation and money does not stand up scrutiny (cf. Bridel 1997). On the other hand, starting with Patinkin (1965), a line of authors have claimed to have successfully introduced money in Walrasian theory, yet this claim has been refuted by Hahn ([1965] 1969) (cf. Bridel 2002). Turning to present-day theory, money is present under the form of cash-in-advance in some new classical models, yet again it can be argued that its introduction is contrived.

¹² De Vroey (1998) argues that they are unable to manipulate the auctioneer.

¹³ In fact, this was the position Keynes was facing because of the wide ignorance of Walrasian theory amongst Cambridge economists.

together of the results of partial analysis, for its second stage. This two-tier general equilibrium methodology can be considered typically Marshallian in contrast to the one-shot Walrasian methodology. Its gist is well encapsulated in Hicks' following observation – a statement that Walras would not have uttered:

If a model of the whole economy is to be securely based, it must be grounded in an intelligible account of how a single market is supposed to work (Hicks, 1965: 78).¹⁴

Extrapolating from the institutional set-up upon which Marshall's partial equilibrium analysis is based, we can reconstruct the trade technology that would have underpinned his general equilibrium analysis had he been able to construct it, thereby defining the contours of a Marshallian economy to be contrasted with the Walrasian economy. A first feature is that the economy is depicted as composed of separate markets of different sub-types (factors, final goods, etc.). Each of them is an autonomous locus of formation of equilibrium. Another of its features is its monetary character, each market witnessing to an exchange between one given good and money. Contrary to what is the case for Walrasian theory, a non-monetary Marshallian model can hardly be conceived of. As far as price-setting is concerned, no auctioneer is assumed to be present. Agents are price-makers. This goes along with a stronger information assumption, a point that requires more elaboration.

Like in the Walrasian set-up, perfect information is supposedly present, yet it receives a stronger contents. In so far as the auctioneer is absent from the Marshallian scenario, the burden of the formation of equilibrium now lies on economic agents. They need to assess relevant market supply and demand functions on their own. Therefore, they must be informed about the relevant private data. Perfect information in this stronger meaning turns out to be the linchpin of the equilibrium formation process. In short, the agents participating in the market are supposed to be as omniscient about it as the outside model-builder economist.¹⁵

Once this omniscience feature is brought to the forefront, which is scarcely the case, the conclusion must be drawn that the Marshallian trade technology fares hardly better than the Walrasian in terms of realism. Both are based on a *deus ex machina*, perfect information in one case, the auctioneer in the other. Perfect information is no less a betrayal of the alleged theoretical *explanandum*, a decentralised trading system, than the auctioneer, because the hallmark of a decentralised economy is that private data will not become public. Methodologically, the underlying flaw is that two levels of knowledge, which should have

¹⁴ The following other statement is in the same vein: “What we have to do now is to generalize his [Marshall's] framework, so that it can be used for the discussion of the problems of a whole economic system” (Hicks 1965: 121).

¹⁵ Cf. De Vroey (2003).

been kept separate – the knowledge of the outside omniscient economist and that of the economic agent – have become blurred.¹⁶

The result obtained up to now is as follows. Assume a Marshallian theorist, endowed with the conceptual toolbox to be found in Book V of the *Principles* and wanting to study the economy as a whole. He would face the need to put forward some scenario as to the functioning of the economy. What I have done is to spell it out. Clearly enough the Marshallian trade technology is different from the Walrasian. Five differences stand out.

- 1) A single all-encompassing market in the Walrasian approach *versus* a juxtaposition of separate markets in the Marshallian approach;
- 2) Prices are formed by the auctioneer in the Walrasian approach while agents are price-makers in the Marshallian approach;
- 3) Money is absent from the Walrasian approach (if present, it is introduced in a contrived way) while it is an essential ingredient of the Marshallian; put differently the Marshallian economy, unlike the Walrasian, is necessarily monetary.
- 4) In the Walrasian approach perfect information does not include agents' ability to reconstruct market supply and demand function, while this requirement is needed for the formation of equilibrium in the Marshallian approach.
- 5) The Walrasian trade technology excludes imperfect competition (i.e. the possibility of monopolistic or oligopolistic behaviour) while there is no reason for this exclusion in the Marshallian approach.¹⁷

4 The unfolding of macroeconomics viewed against the background of the Marshall-Walras divide

4.1 The overall picture

I am now able to address the issue of the relevance of the Marshall-Walras divide for explaining the history of macroeconomics. My claim is that macroeconomics started as Marshallian but became Walrasian after the new classical revolution. To make my point, let me confront the state of development of general equilibrium theory, including macroeconomics as its simplified sub-type, at two points in time, first, at the heyday of the IS-LM tradition, say in the 1950s and 1960s, and second at the turn of the century. Tables 1 and

¹⁶ This is a point that Hayek underlined more than half a century ago, unfortunately with little impact. Cf. Hayek ([1937] 1948: 45).

¹⁷ Perfect competition should not be equated with price-taking behaviour. It can also be associated with price-making behaviour. Hence my criteria (2) and (5) are not identical.

2, which display a few potential lines of development of general equilibrium theory in a tree form, serve this purpose. Four nodes are separated. The first is a bifurcation between Marshallian and Walrasian general equilibrium, the second separates complex from simplified models. A further distinction, which is relevant only for the Marshallian approach, is between perfectly and imperfectly competitive models. Finally, a last node is between static and dynamic models, dynamics meaning here intertemporal.

Insert tables 1 and 2

Table 1 shows that during the first period the Marshallian and the Walrasian research programmes were in an inverse position. I shall argue presently that the IS-LM model is Marshallian. This being granted, it turns out that the Marshallian programme succeeded in constructing simplified general equilibrium models (macroeconomics) yet no Marshallian complex general equilibrium models were existing. The inverse is true for the Walrasian programme: no Walrasian macroeconomics was existing, except for Patinkin and the disequilibrium approach, yet the complex part of the Walrasian programme was alive and well.

Table 2 reflects the present situation. The difference with the earlier situation is that most of the empty slots have been filled up. The IS-LM model has been supplemented with important developments in the imperfect competition branch. For example, I take Hart 1979's model as a fine example of a complex Marshallian general equilibrium model with imperfect competition. Moreover, dynamic imperfect competition models have emerged.¹⁸ As far as the Walrasian branch is concerned, the salient feature is the breakthrough of Walrasian macroeconomics with real business cycle models.

¹⁸ See for example Benassy (2002).

4.2 The IS-LM model

The IS-LM model is often, yet offhandedly, characterised as Walrasian.¹⁹ I will show that this judgement is mistaken. But before let me consider two possible reasons that may have led to put forward such a judgement. A first one is that general equilibrium is equated with Walrasian theory. This was for example Patinkin's stance. He rightly perceived that Keynes' project amounted to study the interdependency across markets. Since he did not conceive of any alternative to the Walrasian route, he concluded that Keynes had unwittingly followed Walras' footsteps.²⁰ I have shown that such a stance is inappropriate. As soon as it is admitted that the Walrasian approach is not the exclusive way of doing general equilibrium analysis, Patinkin's conclusion falls.

A second reason is that Hicks, the initiator of the IS-LM tradition ([1937] 1967), is considered a Walrasian economist – at least, he supposedly was when writing his « Mr. Keynes and the 'Classics' ». This was also the period during which he was working on *Value and Capital* (1946 ; first edition 1939), which is often credited for having revived Walrasian theory. Hence the conclusion that IS-LM must be Walrasian as well. The snag, however, is that *Value and Capital* is less Walrasian than it usually claimed. In my opinion, Hicks was never more than a half-hearted Walrasian. He may well have considered that Walrasian theory opened a window on new horizons ([1979] 1983: 358), yet he always read Walras through Marshallian glasses. Contrary to subsequent Walrasians, he did not want to limit his analysis to the study of the logical existence of equilibrium. Following Marshall's lead, he confined mathematical developments to the appendices of his book. Nor was he ready to adopt the auctioneer hypothesis. Marshall was to him a better reference for studying the working of markets than Walras. Finally, as seen, his general equilibrium methodology was different from the Walrasian. All in all, Hicks' mistake lies in his failure to have envisaged that Marshall and Walras were in a relationship of discontinuity. Unfortunately, most of the profession followed suit.

To assess whether the IS-LM model belongs to the Marshallian or the Walrasian approach, we must see how it fares with respect to the criteria adopted above to differentiate the Marshallian and the Walrasian trade technologies – the structure of the economy, the price-

¹⁹ See e.g. Vercelli (2000). This view is also taken for granted in several of the contributions to the Young-Zilberfarb volume on IS-LM (2000).

²⁰ « Thus a basic contribution of the *General Theory* is that it is in effect the first practical application of the Walrasian theory of general equilibrium : 'practical' not in the sense of empirical ... but in the sense of reducing Walras' formal model of n simultaneous equations in n unknowns to a manageable model form which implications for the real world could be drawn » (Patinkin 1987 : 27). « The analysis of the *General Theory* is essentially that of general equilibrium. The voice is that of Marshall, but the hands are those of Walras. And in his IS-LM interpretation of the *General Theory*, Hicks quite rightly and quite effectively concentrated on the hands » (*ditto* 1987 : 35).

making process, the role of money, the information assumption and the admissibility of imperfect competition.²¹

As far as the first criterion is concerned, the matter is clear. The economy that the IS-LM model analyses is composed of markets that function separately, each of them being an autonomous locus of equilibrium. Likewise, for what concerns the second criterion, no auctioneer is supposedly present. The monetary character of an IS-LM economy is also undeniable. Turning to the information assumption, it is true that accounts of the IS-LM model scarcely evoke the possibility that it might rest on the assumption that agents are omniscient. But then nobody seems to have raised the issue of how equilibrium is arrived at in this model.²² The IS curve describes every combination of income and interest at which the goods market is clearing, the same is true about the LM curve with respect to the money market. Nothing is stated about how these market clearing results are arrived at.²³ What is even more perplexing is how the intersection of the IS-LM curves can be obtained. I see no other explanation than assuming agents' ability to reconstruct the equilibrium values of the economy, i.e. their being omniscient!²⁴

Two supplementary factors point to a Marshallian belonging. First, it has often been remarked that the IS-LM model has little room for expectations.²⁵ To me, this lack is due to the stationary equilibrium conception, underpinning the Marshallian approach and centred on the distinction between market prices and normal prices with normal equilibrium serving as a centre of gravitation.²⁶ This conception of equilibrium is based on a central yet unnoticed assumption, namely that the economic data remain constant over the period of analysis, except for shocks that are supposed to be accidental, reversible and unpredictable. Now if shocks cannot be predicted (although once they have occurred, agents are supposed to be able to assess their effect and length correctly), and if, moreover, their impact is supposedly well circumscribed, so that all the other data of economy remain unchanged, why on earth would

²¹ As the last criterion is irrelevant here, this leaves us with the first four.

²² This issue was dodged because it was believed, certainly by Hicks, that Marshall's haggling and bargaining mechanism would do the job. In De Vroey (2003), I have claimed that this argumentation is hollow.

²³ In his study of Modigliani's 1944 IS-LM model, Rubin (2004, this volume) shows that Modigliani was concerned with the formation of equilibrium. However, the fact that in Modigliani's reasoning the adjustment took place exclusively in the money market, requiring no across markets changes, is further testimony to the Marshallian character of the IS-LM model.

²⁴ One might claim that the equilibrium can as be attained by adopting the auctioneer assumption, in which case the IS-LM could be viewed as based, at least in this respect, on the Walrasian trade technology. Upon reflection it turns out that this will not do. Either it is accepted that markets function separately, in which case an auctioneer per market would be present. Yet how will one of the local auctioneers, say the auctioneer in charge of the goods markets, be able to determine which among the possible equilibria between savings and investment will the 'true' one? To this end, he should come in touch with the other auctioneer, which makes the story too contrived. Or it assumed that a single auctioneer takes the care of the economy as a whole. But then idea of separate markets ceases to make sense.

²⁵ See e.g. King (1993).

²⁶ Cf. De Vroey (1999).

one bother about expectations? In order for these to become important, economic data must be constantly changing over time. Turning to the second factor, different enrichments were brought to the initial IS-LM model, pertaining to consumption, portfolio choice, investment and the labour market. All of them consisted of analysing one sector of the economy viewed in isolation from the others, thereby witnessing to the Marshallian two-step general equilibrium methodology evoked above.

The conclusion to be drawn is that the IS-LM model is a simplified Marshallian general equilibrium model. This conclusion is in accordance with the implicit view taken by most defenders of IS-LM macroeconomists. In spite of their unawareness of the Marshall-Walras divide, they firmly believe that their approach is poles apart from Walrasian microeconomics.²⁷

4.3 Patinkin's Macroeconomics

According to Leijonhufvud, the development of economic theory can be thought of as forming a decision tree.

It is useful to think of our subject as forming a decision tree. Major economists force their contemporaries to face choices – choices of what to ask, what to assume, what to regard as evidence and what methods and models to employ – and persuade the profession or some faction of it to follow the choice they make. The path that any particular school has followed traces a sequence of such decisions. Many of the choices faced in such a sequence were not anticipated by the founder to which we trace the development in question, by were created by subsequent contributors; some of the decisions made we may judge to have been ‘wrong’ in hindsight (Leijonhufvud 1994: 148).

These remarks are apposite for reflecting on the place of Patinkin's *Money, Interest and Prices*, (first edition 1956, second edition 1965) in the history of macroeconomics. This book, which aimed at integrating value analysis and money theory, comprised a simplified general equilibrium model with two variants, a classical and a Keynesian, that can be put on the same footing as the IS-LM model. Arguably, it is superior to it in several respects – its composing markets are better defined, it has stronger micro-foundations, its adjustment process involves all the commodities forming the economy.²⁸ When Patinkin's model is gauged against my five criteria for separating the Marshallian from the Walrasian trade technology, its Walrasian belonging stands out, except for money. Yet Patinkin hardly pushed it that way. Not that he

²⁷ See e.g. Lipsey (2000: 69).

²⁸ Cf. Rubin (2004, this volume). Small wonder then that it was hailed by Lucas in the introduction to this volume.

was unaware of the Walrasian character of his model. The contrary is true. But he missed the un-Walrasian nature of the IS-LM model and hence the deep-seated contrast between the two models! Although his book exerted an important influence, it was considered as a contribution to monetary theory rather than to macroeconomics. As a result, the fact that it might constitute an alternative to the IS-LM was disregarded. Patinkin's insights were re-discovered a few decades later by the authors of the so-called disequilibrium approach (Barro and Grossman 1971, 1976). Yet, its success was short-lived, many of its practitioners ending up admitting to the validity of the criticism levelled against it – that its cornerstone, the idea of price rigidity, was ill-grounded..

To conclude, Walrasian macroeconomics could have emerged as an alternative to the IS-LM way of modelling, first in the 1950s with Patinkin's model, and later with the disequilibrium line of research, yet this possibility failed to be realised.

4.4 2.3 The neoclassical synthesis

Several authors, in particular Goodfriend and King (1997), have claimed that the unfolding of macroeconomics has led to the emergence of a New Neoclassical Synthesis close in spirit to the neoclassical synthesis that prevailed in the 1960s. Hence the need to clarify the contents of the “old” synthesis.

The term ‘neoclassical synthesis’ is often traced back to Samuelson's 1955 edition of his *Economics* textbook, yet what he writes there is hardly instructive.²⁹ For a more substantive explanation, we may turn to Howitt's or Blanchard's entries in the *New Palgrave Dictionary* or to Woodford (1999). All of them hold the same viewpoint well summarised in the following quotation from Howitt:

Since it was widely believed that wages were less than fully flexible in the short run, it seemed natural to see Keynesian theory as applying to short run fluctuations and general equilibrium theory as applying to long-run questions in which adjustment problems could safely be ignored. This view came to be known as the ‘neoclassical synthesis. (Howitt, 1987 : 274).³⁰

The problem lies in what should be understood by the term “synthesis”. Strictly speaking, a synthesis refers to the process by which two theoretical analytical frameworks that at a certain

²⁹ “In recent years 90 percent of American economists have stopped being ‘Keynesian economists’ or ‘anti-Keynesian economists’. Instead they have worked towards a synthesis of whatever is valuable in older economics and in modern theories of income determination. The result might be called neo-classical synthesis and is accepted in its broad outlines by all but about 5 per cent of extreme left wing and right wing writers “ (Samuelson 1955 : 212).

³⁰ See also Blanchard (1987: 634-5).

stage were considered as antagonistic eventually turn out to be congruent. Hence a merger between them proves possible.³¹

The neoclassical synthesis as understood by Howitt *et alii* does not stand up to this strict understanding of the notion of synthesis. Actually, their conception expresses a Keynesian viewpoint, which “Classicists” have no reason to endorse. The classical model has as much to say about the short period as the Keynesian – actually it has a rival theory to propose about it. Therefore, classicists should have refused the territorial partition that Keynesians were proposing.³² In short, the so-called neoclassical synthesis was a meta-theoretical compromise between two approaches that did not want to enter into an open intellectual fight rather than a synthesis in the strict sense of the term.

4.5 Monetarism

Monetarism is associated with the work of Milton Friedman and his criticism of Keynesian activist policy. At a certain juncture, it was believed that monetarism could become a new paradigm, rival to the Keynesian.³³ At present, a different view prevails. For example, Woodford thinks that monetarism has won the day in bringing monetary policy and expectations to the forefront, yet he considers that, from a methodological viewpoint, it has been absorbed within the Keynesian paradigm (Woodford 1999: 18). Likewise, Snowdon and Vane write that “monetarist influences were absorbed *within* the existing framework leading to a Keynesian-monetarist synthesis” (Snowdon and Vane 1996: 386). Finally, Lipsey (2000) views the difference between monetarists and Keynesian as being mainly rhetorical (Lipsey 2000: 68).

Although making this point would require an article on its own, Friedman’s contribution should not be viewed as a rejection of Keynes’ methodology. Let me just evoke three elements of justification. First, Keynes and Friedman shared the same style of theorising and a common belonging to the Marshallian connection.³⁴ Friedman praised Keynes for being “a true Marshallian in method” and for adopting the Marshallian instead of the Walrasian framework (1974: 18)!³⁵ Second, when request to put his claim in a broader theoretical

³¹ The identity of the involved streams should made clear. Thus one should speak of the synthesis between theory A and B. In this respect the neoclassical synthesis appellation is wanting.

³² The point that it is logically inconsistent to be a “Keynesian” in the short period and a “new classicist” in the long period, was already made by Lucas in his *Tobin and Monetarism* review article (1981).

³³ Cf. Johnson (1971).

³⁴ On this see Hirsch and de Marchi (1990) chapter 9, Dostaler (1998) and Hammond (1992).

³⁵ The following excerpt from his "Marshallian Demand Curve" illustrates: “Of course, it would be an overstatement to characterise all modern economic theory as 'Walrasian' in this sense. For example, Keynes' theory of employment, whatever its merits or demerits on other grounds, is Marshallian in method. It is a general equilibrium theory containing important empirical content and constructed to facilitate meaningful predictions” (Friedman [1949] 1953: 92).

perspective the model on which Friedman fell back was the IS-LM model (Friedman 1974). Third, as claimed in De Vroey (2001), Friedman's conception of expectation in his expectations-augmented Phillips Curve model must be characterised as Marshallian rather than as Walrasian.

The distinction between Keynesianism as a theoretical apparatus and Keynesianism as a defence of policy activism must come into play at this juncture. Friedman's aim was to reverse Keynesian policy conclusions. Yet he did hardly feel it necessary to overthrow the Keynesian theoretical apparatus, contrary to what Lucas and Sargent had in mind ten years later (1978). In short, Friedman should be considered as Keynesian from the methodological viewpoint and as anti-Keynesian from the policy viewpoint. In Blinder's words:

The long and to some extent continuing battle between Keynesians and monetarists, you will note, has been primarily fought over the *normative issues* Thus, by my definition, most monetarists are positive Keynesians, but not normative Keynesians. ...The briefer, but more intense, debate between Keynesians and new classicals had, by contrast, been fought primarily over the tenets of positive Keynesianism (Blinder : 112-3).

I have argued that what was called the neoclassical synthesis should not be considered a real synthesis. In contrast, it can be claimed that such an integration has taken place for what concerns the relationship between monetarism and the IS-LM model.³⁶ No deep conceptual opposition divides the IS-LM framework and monetarism. The irony of this state of affairs should, however, not go unnoticed. On the one hand, the reconciliation between Keynesian and monetarists had to wait for the emergence of a common foe, the new classical paradigm. On the other, its counter-part has been a split between "old" and "new" Chicago, the former being Marshallian, the latter Walrasian.³⁷ In this light, the terminology that was used at the time, separating "Monetarism I" (*à la* Friedman) from "Monetarism II" (*à la* Lucas) reveals itself inappropriate. The use of the same substantive suggests that these two models belong to a same programme. If my interpretation is accepted, the contrary is true, Friedman's work belonging to the Marshallian approach while Lucas' is Walrasian.

³⁶ The well-known Laidler-Tobin debate in the *Economic Journal* (Laidler 1981, Tobin 1981) illustrates.

³⁷ Snowden and Vane's interviews of Friedman and Lucas are enlightening in this respect. Question [to Lucas]: *You acknowledge that Friedman has had a great influence on you, yet his methodological approach is completely different to your own approach to macroeconomics. Why did his methodological approach not appeal to you?* Answer: I like mathematics and general equilibrium theory. Friedman didn't.... Question: *His methodological approach seems more in keeping with Keynes and Marshall.* Answer: He describes himself as Marshallian, although I don't know quite what it means. Whatever it is, it's not what I think of myself (1998, 132). Question [to Friedman]: *Kevin Hoover has drawn a methodological distinction between your work as Marshallian and that of Robert Lucas as Walrasian. Is that distinction valid?* Answer: There is a great deal to that. On the whole I believe that is probably true. I have always distinguished between the Marshallian approach and the Walrasian approach. I have always been personally a Marshallian (1997: 202).

4.6 The unnoticed emergence of Marshallian general equilibrium theory

Has the Marshallian general equilibrium line ever been pursued? The quick answer to this question is “No”. Evidently, no well-established Marshallian general equilibrium theory has seen the day. Yet this should not be the last word of it. First of all, attention should be given to programmes that exist only in an inchoate way. The first name to be mentioned here is that of Keynes. He had a reason for making the step of studying the economy as a whole that Marshall did not have, his interest in mass unemployment and his insight that, although the problem manifested itself in the labour market, its origin was to be looked for in other parts of the economy. Such an insight required to study the interdependency across apparently unrelated markets (the labour market and the money market) and thus to engage into general equilibrium analysis. Therefore the *General Theory* (1936) can be viewed as an attempt at building a Marshallian general equilibrium theory. Other authors who set forth the task of building a Marshallian general equilibrium theory are Clower and Leijonhufvud (Cf. Clower and Leijonhufvud 1975). They had in mind a radically different way of studying the coordination of economic activities, abandoning the study of the logical existence of equilibrium to concentrate on economic processes and institutions. They also suggested abandoning optimising behaviour. Yet, for all its appeal, Clower and Leijonhufvud’s programme has had trouble to taking off.³⁸

Moreover and somewhat oddly, we must consider the possibility that Marshallian general equilibrium theory exists yet only in disguise. What needs to be envisaged is that models, which are presented as amending the canonical Walrasian model, are in fact built on a radically different trade technology. This might occur with neither their builders nor their users being aware of it, due to their focusing on problems of logical existence of equilibrium and their neglecting the trade technology dimension. I have here in mind imperfect competition general equilibrium models.³⁹

Authors developing such models have rightly noted that the equilibrium they obtain are non-Walrasian, differing from the equilibrium that would be arrived at if perfect competition had been prevailing. However, the question to be raised is that of the trade technology upon which their models are based. Is it Walrasian – one can get a non-Walrasian equilibrium outcome within a Walrasian trade technology, to wit Drèze (1975) – or non-Walrasian in the radical sense, pointed out above in section 2.1, of a trade technology that is radically distinct from the Walrasian. Unfortunately, imperfect competition theorists have hardly reflected on this issue.

³⁸ Their programme has recently received a new impulse in the works of a group of economists centred around David Colander (Colander 1996). Oddly, enough, these authors call their approach “Post Walrasian”, while in terms of methods it is poles apart from Walras. “Neo-Marshallian” would have been a better brand, reference being then made to “Marshall the evolutionist”.

³⁹ Pioneering works in this line were Negishi (1961) and Gabszewicz and Vial (1972). Cf. De Vroey (2003).

The Walrasian perfect competition model has served as the benchmark for several types of developments, the latter proceeding usually through the introduction of imperfections. Missing markets, externalities, information imperfections, price rigidities are all lines that have been taken. All of them have proven compatible with the auctioneer trade technology. Would imperfect competition then be just another of such departures? No! All imperfections cannot be put on the same footing. My above remark on the working of a auctioneer economy, stating that Walrasian models necessarily are perfectly competitive models points to the contrary. Now, if general equilibrium models exists that cannot be considered as belonging to the Walrasian approach, this means that another way of doing general equilibrium is present. Hence my claim that imperfectly competitive general equilibrium models belong to the Marshallian rather than to the Walrasian approach. They are based on the Marshallian representation of the working of the economy. Take Hart's (1979, 1982), Blanchard and Kyotaki's (1987), Benassy's (2002) models and look at them against the grid of my opposition between the two types of trade technology. They all feature the Marshallian traits instead of the Walrasian ones. In short, while most models that are called "non-Walrasian" should better be called "quasi-Walrasian", imperfect competition models are really "non-Walrasian".

As soon as it is accepted that trade technology matters, the view that Walrasian perfect competition theory is the base camp for imperfectly competitive general equilibrium models must be abandoned.⁴⁰ Admittedly, it looks odd to claim that these models are a departure from the Marshallian perfectly competitive general equilibrium model if the latter is non-existing. The way out is to consider that the builders of these models have constructed a Marshallian perfectly competitive general equilibrium result inadvertently, as *in passim*. It is to be found in those passages of their papers where they substitute the assumption of a great number of agents to that of a monopolistic or of oligopolistic agents. Then, one falls back on a Walrasian equilibrium, i.e. the array of prices and quantities that the auctioneer would have made emerge. However here these equilibrium values result from the Marshallian trade technology rather than from the Walrasian.

4.7 The new classical revolution

Like everybody, I consider that Lucas' work, especially his *Expectations and the Neutrality of Money* paper ([1972] 1981), caused a radical breach. Interpreting it as having started a Kuhnian scientific revolution seems to be no exaggeration. His motivation for writing this article was to strengthen Friedman's policy ineffectiveness claim by giving it stronger micro-foundations and casting it in an explicit general equilibrium framework. Yet the end result

⁴⁰ Cf. Pignol (1999).

was a methodological revolution blazing the trail for real business cycle theory and dynamic macroeconomics (Sargent 1996). The shift from IS-LM to real business cycle models is twofold: a move from a static to a dynamic approach, on the one hand, a shift from the Marshallian to the Walrasian side of the tree, on the other.

Some commentators draw a divide between new classical and real business cycle models. I rather consider them as representing two stages in the development of the same paradigm, centred on stochastic dynamic equilibrium models. However, they differ in that Lucas' model is a parable with no claim to direct empirical confrontation while real business cycle models claim to explain the real world.⁴¹

The novelty of the new classical approach (defined in a broad way in order to encompass both Lucas' initial models and real business cycle models) can be accounted for in several ways. Mine is as follows. Its distinctive feature lies in its having extended the scope of relevance of value theory (i.e. the theory of equilibrium price) to a domain that before was believed to be beyond its grasp.⁴² Thereby it bridged a gulf that had marked economic theory for more than a century, its split in two distinct branches, value or price theory, on the one hand, and business cycle theory, on the other. Evolving at a high level of abstraction and concentrating on the issue logical existence of equilibrium, the first branch was based on trade technology assumptions resulting in the ever existence of market clearing. The second consisted on qualitative, descriptive accounts of the evolution of economies over time with only vague references to equilibrium, what allowed it to integrate phenomena such as market rationing that had no room in value theory.⁴³ Underlying this evolution is the abandonment of what was the initial motivation of macroeconomics, i.e. bringing to the fore some malfunctioning of the market system – after all, it was born in the aftermath of the Great Depression!⁴⁴

⁴¹ In Woodford's terms, "The real business cycle literature offered a new methodology, both for theoretical analysis and for empirical testing. ... It showed how such models [of the Lucas type] could be made *quantitative*, emphasising the assignment of realistic numerical parameters values and the computation of numerical solutions to the equations of the model, rather than being content with merely qualitative conclusions derived from more general assumptions. The "equilibrium business cycle models" of Lucas had really only been parables; they could not be regarded as literal descriptions of an economy, even allowing for the sort of idealisation that all models of reality have.... Real business cycle models are instead quantitative models, that are intended to be taken seriously as literal depictions of the economy, even if many details are abstracted from. The literature emphasises the numerical predictions of the models, when parameter values are assigned on the basis of measurement of the relevant aspects of an actual economy" (Woodford 1999 : 25-26).

⁴² Critics might say it has brought the imperialism of value theory to a new height.

⁴³ A tempting way of putting the issue is to state that price theory was concerned with equilibrium and business cycle theory with disequilibrium. Yet this is misleading in so far as equilibrium and disequilibrium are part and parcel. The truth is rather that these linked categories played only a marginal role in business cycle theory. The latter, it was believed, was too complex a field of inquiry to be accounted for with the tools of price theory.

⁴⁴ As aptly perceived by Hahn and Solow, "The irony is that macroeconomics began as the study of large-scale economic pathologies: prolonged depressions, mass unemployment, persistent inflation, etc. This focus was not invented by Keynes (although the depression of the 1930s did not pass without notice). After all, most of Haberler's classic *Prosperity and Depression* is about ideas that were in circulation before *The General Theory*. Now, at last, macroeconomic theory has as its central conception a model in which such pathologies are, strictly speaking, unmentionable. There is no legal way to talk about them" (Hahn and Solow 1995: 2-3).

Real business cycle theoreticians insist that their models belong to the Walrasian general equilibrium approach. This is not to be denied. Yet a sharp contrast exists between Walrasian models as they were existing before the rise of the new classical paradigm, pertaining to an economy comprising a large number of agents and goods, and the new models based on the assumption of a representative agent and the existence of a single good traded over time. This evolution should be interpreted as a return to square one of Walras' construction dealing with a the two-good exchange economy.⁴⁵ But then, this is exactly what macroeconomics is all about, to work with simple rather than complex models! Hence my claim that real business cycle models mark the start of Walrasian macroeconomics or, more precisely, its predominance, in view of the existence of earlier attempts.

This being stated, it may be wondered whether Walras himself would have endorsed the move made by real business cycle theorists. When he started to write his *Elements of Pure Economics* (1954, English translation) his intention was to construct a theory able to account for the working of the competitive process in decentralised economies. However, he gradually became aware of a conflict between the requirements of demonstrating the logical existence of equilibrium states and any realistic account of the competitive process. Facing such a dilemma, Walras opted for rigor over realism, and consequently refrained from claiming any straightforward empirical relevance for his model, thereby foregoing his initial ambition.⁴⁶ The methodological implication was far-reaching. In particular, Walras came to believe that the validity of his theory was not a matter of empirical relevance. The following extract, drawn from an unpublished note written by Walras and criticising Pareto, is enlightening:

Pareto believes that the aim of science is to ever come closer to reality through successive approximations. I, for one, believe that the eventual aim of science is to bring reality close to a certain ideal. This is why I formulate this ideal (Walras 2000: 567, own translation).⁴⁷

Subsequent neo-Walrasian economists have o all intents and purposes followed suit.⁴⁸ Authors such as Arrow, Debreu and Hahn have repeatedly insisted on the point that neo-Walrasian general equilibrium theory is an abstract construction the strength of which lies in

⁴⁵ For a more detailed analysis, see De Vroey (2002).

⁴⁶ As put by Jaffé, “The *Elements* was intended to be and is, in all but the name, a realistic utopia, i.e. a delineation of a state of affairs nowhere to be found in the actual world, independent of time and place, ideally perfect in certain respects, and yet composed of realistic psychological and material ingredients” ([1980] 1983: 345).

⁴⁷ Walras' note is undated. Bridel's conjecture is that it was written between 1896 and 1906. A similar testimony is Walras' annotation on p. 17 of his copy of Cournot's *Principes de la théorie de la richesse*, held at the Centre Walras-Pareto of the Université de Lausanne, where he wrote “pure theory requires no confirmation from reality” (“la théorie pure n'attend aucune confirmation de la réalité”), quoted in Baranzani. and Bridel (2003).

⁴⁸ This assertion is true only for mathematical Walrasian economists. Other economists, who also claim a Walrasian affiliation, like neo-Austrians or like Walker (1996), pull Walras towards the stationary equilibrium perspective.

the ability to posit issues in a rigorous way. Its main interest with respect to reality, they argue, is to provide a negative benchmark. In short, they admit to a no bridge between their theoretical construct and real-world market economies.

Real business cycle models mark a radical change in this respect by adopting the Friedman's methodological standpoint according to which models ought to be evaluated in terms of their predictive capacity.⁴⁹ The validity of their models, they argue, ought to be assessed by gauging their capacity to mimic real-world time-series.

The oddity, however, is that the gist of Friedman's criticism of Walrasian economics was that one could not jointly take into account the total set of interdependency across agents and goods and have a tractable model allowing for predictions. The preference of many economists for the Marshallian approach was probably based on their readiness to trade rigorous general equilibrium perspective for empirical measurability. In other words, Friedman and lucid Walrasian authors at least concurred on one point, that one should not turn to the general equilibrium methodology whenever aiming at explaining concrete phenomena. Why is it that what was true for the earlier Walrasian models – that higher rigor had to be paid for by an admission to a lack of empirical relevance of the model – is no longer so for even more rudimentary Walrasian models? Now, outwardly, there is no longer a price to be paid for engaging in Walrasian theory: one can have the cake (Walrasian rigor) and eating it (engaging empirical work) at the same time!

4.8 A New Neoclassical Synthesis?

Initially, dynamic macroeconomics was entirely non-Keynesian. This was true for both the conceptual framework and the policy cause. Not surprisingly, the new classical attack on Keynesian economics stirred up a revival of Keynesian thought, known as “New Keynesian economics”. This label does not cover a unified theoretical programme. For example, efficiency wage model are partial equilibrium models (or at least started this way), coordination failure models can be Walrasian yet are not necessarily so, imperfect competition models are Marshallian, etc. Moreover, the “New Keynesian” label is ambiguous in that the Keynesian modifier should be understood exclusively in reference to the policy cause dimension. It is inappropriate as far the theoretical framework is concerned.

⁴⁹ Although Lucas' Neutrality of Money model still belongs to the “old” Walrasian tradition, since it has no empirical counter-part, Lucas nonetheless endorses the real business cycle viewpoint. In their interview, Snowden and Vane asked him whether he “would agree that the appropriate criterion for establishing the fruitfulness of a theory is the degree of empirical corroboration attained by its predictions?” Lucas's answer was “Something like that. Yes” and to the next question – “You are Friedmanite on that issue of methodology?” – his answer was affirmative. (1998: 132)

Without commenting New Keynesian models any further, I want now to address the issue of the New Synthesis. The particular branch of New Keynesian models that is of concern here is imperfectly competitive general equilibrium models, which above I have characterised as unwittingly belonging to the Marshallian research programme. Their hallmark is to combine imperfect competition and price and wage stickiness in order to refute the Friedman-Lucas ineffectiveness of monetary policy claim.⁵⁰ The idea of a merger between these models and real business cycle models has been wholeheartedly endorsed by New Keynesians as the following excerpt from Mankiw's interview by Snowdon and Vane illustrates:

I'm delighted that some of the people who previously worked closely with the real business cycle models are now trying to incorporate monetary effects in those models. That provides a hope that somewhere down the line the new Keynesian models and the real business cycle models are going to merge to some grand synthesis that incorporates the strengths of both approaches (Snowdon and Vane 1994: 340).

Many New Keynesians share Mankiw's viewpoint. Blanchard is amongst them. Rejecting any picturing of the evolution of macroeconomics in terms of scientific revolution, he claims that the passage from the IS-LM model to real business cycle models must be viewed as a smooth evolutionary process.

On the surface, the history of macroeconomics appears as a series of battles, revolutions and counterrevolutions.... But this would be the wrong image. The right one is of a surprisingly steady accumulation of knowledge (Blanchard 2000: 2).

To him, the clash between "New Keynesians" and New Classicists" in the 1970s was short-lived, and based on a misunderstanding.

In the early 1980s, macroeconomic research seemed divided in two camps, with sharp ideological and methodological differences. Real business cycle theorists argued that fluctuations could be explained in a fully competitive model, with technological shocks. New Keynesians argued that imperfections were of the essence. Real business cycle theorists used fully specified general equilibrium models based on equilibrium and optimisation under uncertainty. New Keynesians used small models capturing what they saw as the essence of their arguments, without the paraphernalia of fully specified models. Today, the ideological divide is gone. Not in the sense that underlying ideological differences are gone, but in the sense that trying to organise recent contributions along ideological lines would not work well. As I argued earlier, most macroeconomic research today focuses on the macroeconomic implications of some

⁵⁰ See for example Blanchard and Kyotaki ([1987] 1991).

imperfection or another. At the frontier of macro-economic research, the field is surprisingly a-ideological (Blanchard 2000: 39).⁵¹

Interestingly enough, the same viewpoint is held by some economists coming from what was earlier viewed as the opposite camp. In particular, Goodfriend and King (1997) claim that in the last ten years « macroeconomics is moving toward a New Neoclassical Synthesis » (1997 : 231). Like Blanchard and Woodford, they praise the real business cycle programme for its micro-foundations and its allowing a comparison of alternative policies on the basis of measures of utility benefits or costs. Like them, they argue that it must be enriched with the considerations of imperfections, such as imperfect competition and sticky prices, the very purpose of new neoclassical synthesis models. Under this heading, they put models that « range from the flexible, small models of academic research to the new rational-expectations policy model of the Federal Reserve Board » (1997 : 232). These models, which Goodfriend and King view as close in spirit to the neoclassical synthesis that prevailed in the 1960s, combine Keynesian elements – the above mentioned imperfections – and real business cycle elements – intertemporal optimisation, rational expectations, and their integration in stochastic dynamic model (1997 : 232). These two components, they claim, are compatible because of their shared reliance on microeconomics (1997 : 256). As far as policy is concerned, they concur with New Keynesians in believing that “ aggregate demand must be managed by monetary policy in order to deliver efficient macroeconomics outcomes” (Goodfriend and King 1997: 255-6).⁵²

Has the so-called new synthesis the attributes of a real theoretical merger? At the present juncture, this remains an open question. A positive answer would be appealing because it would reveal that the reigns of the IS-LM paradigm and that of the new classical paradigm have witnessed a similar evolution. On the score of the theoretical dimension, a two-step evolution would have taken place, where two frameworks that used to be considered rivals turn out to be compatible. On the score of the policy cause dimension, a shift would have occurred from a situation where the policy cause and the theoretical framework seemed part and parcel to a more open state of affairs in which the same conceptual framework proves able to justify either the Keynesian or the *laissez-faire* policy cause.

True, a rallying around the real business cycle methodology has occurred. New Keynesian authors have admitted that macroeconomics should study the dynamic evolution of the economy in a stochastic context, rational expectations and intertemporal substitution being furthermore considered *sine qua non* ingredients of the analysis. Likewise, most of them have

⁵¹ Contrary to Blanchard, Woodford (1999) finds it useful to account for the history of macroeconomics in terms of a succession of revolutions and counter-revolutions. However, he shares Blanchard's and Mankiw's opinion that the future of macroeconomics lies in incorporating Keynesian features into real business cycle models (Woodford 1999 : 29).

⁵² Hairault makes the same point (Hairault 1999: 616).

abandoned the aim of demonstrating involuntary unemployment. On the other hand, two arguments point against the achievement of the proposed synthesis. First, will the *quid pro quo* that they new Keynesians are offering to new classicists – the rallying to the their methodology against the introduction of the assumptions of imperfect competition and sticky prices –be accepted? In other words, would prominent real business cycle theoreticians – say Lucas or Prescott – be ready to sign any manifesto as to a merger between the two approaches along the terms proposed by Goodfriend and King? I, for one, doubt it. “Chicago” (and for that matter, Minnesota) may well have shifted from Marshall to Walras yet, as well documented by Leeson (2000), its resistance to imperfect competition is deep-rooted. Second, if my above analysis is accepted, the perfectly competitive and imperfectly competitive models must be viewed as rooted in incompatible trade technologies, the Walrasian and the Marshallian. Therefore a merger between them is hard to envisage. The picture which then emerges is rather one of two rival macroeconomic paradigms, a Marshallian (i.e. imperfect competition) and a Walrasian (i.e. perfect competition), existing side by side.

5 Concluding remarks

The starting point of this paper is my insight that any valid critical analysis of economic theories must take into account the trade technology assumptions upon which they are based. Interesting conclusions ensue. First, it turns out that the Marshallian and the Walrasian approaches ought to be viewed as alternative research programmes because they rest on radically different trade technologies. Second, the Marshall/Walras divide provides an interesting angle of attack for reflecting on the history of macroeconomics, allowing for an original interpretation of its recent unfolding. To wit, the new classical revolution, i.e. the replacement of the IS-LM paradigm by stochastic dynamic models, can be interpreted as a shift from Marshallian to Walrasian macroeconomics. Moreover, against the background of the Marshall/Walras divide, the possibility of constructing a real theoretical synthesis between New Keynesians and New Classicists looks doubtful.

Table 1. General equilibrium and macroeconomics at the heyday of the IS-LM tradition

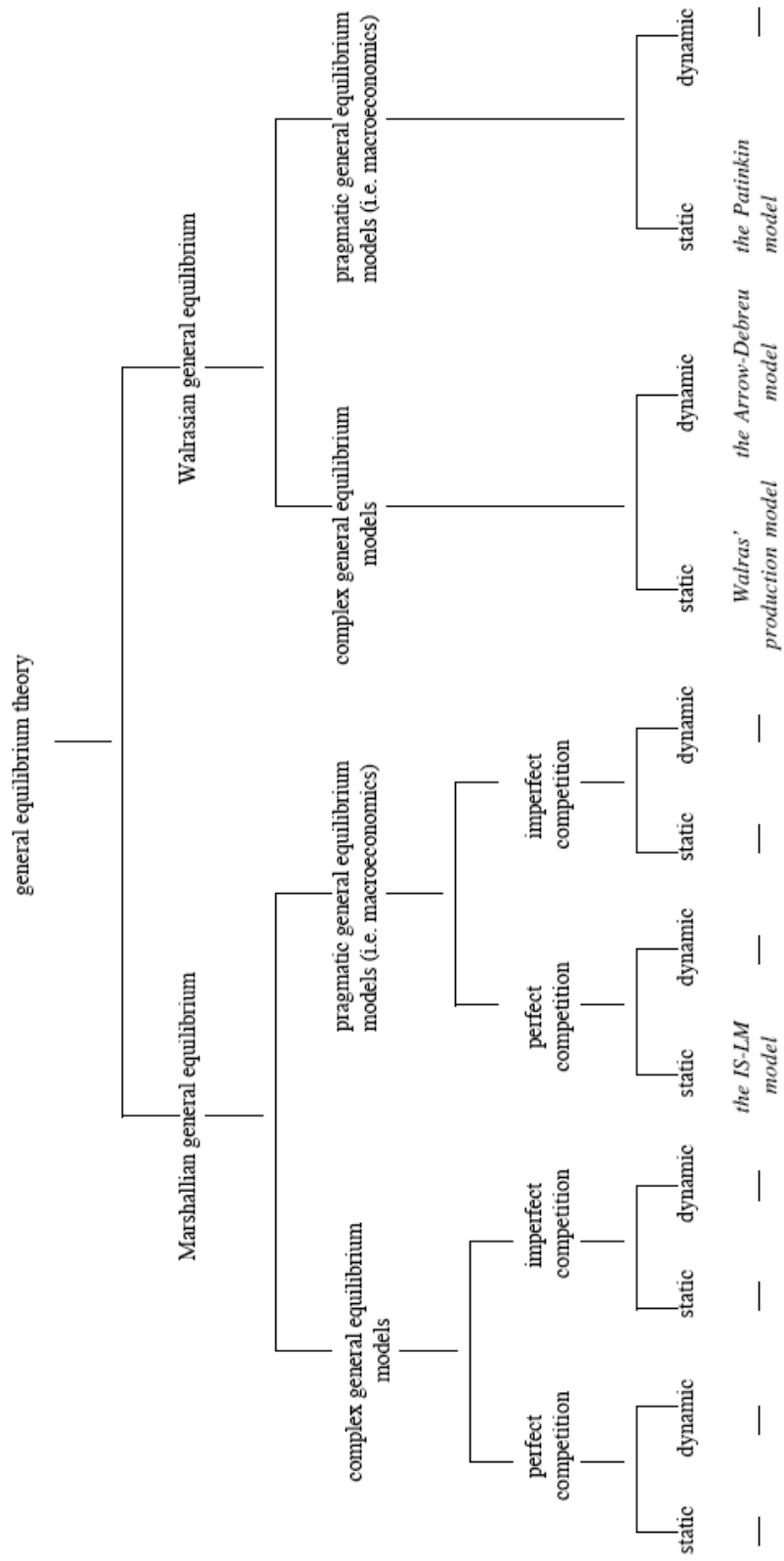
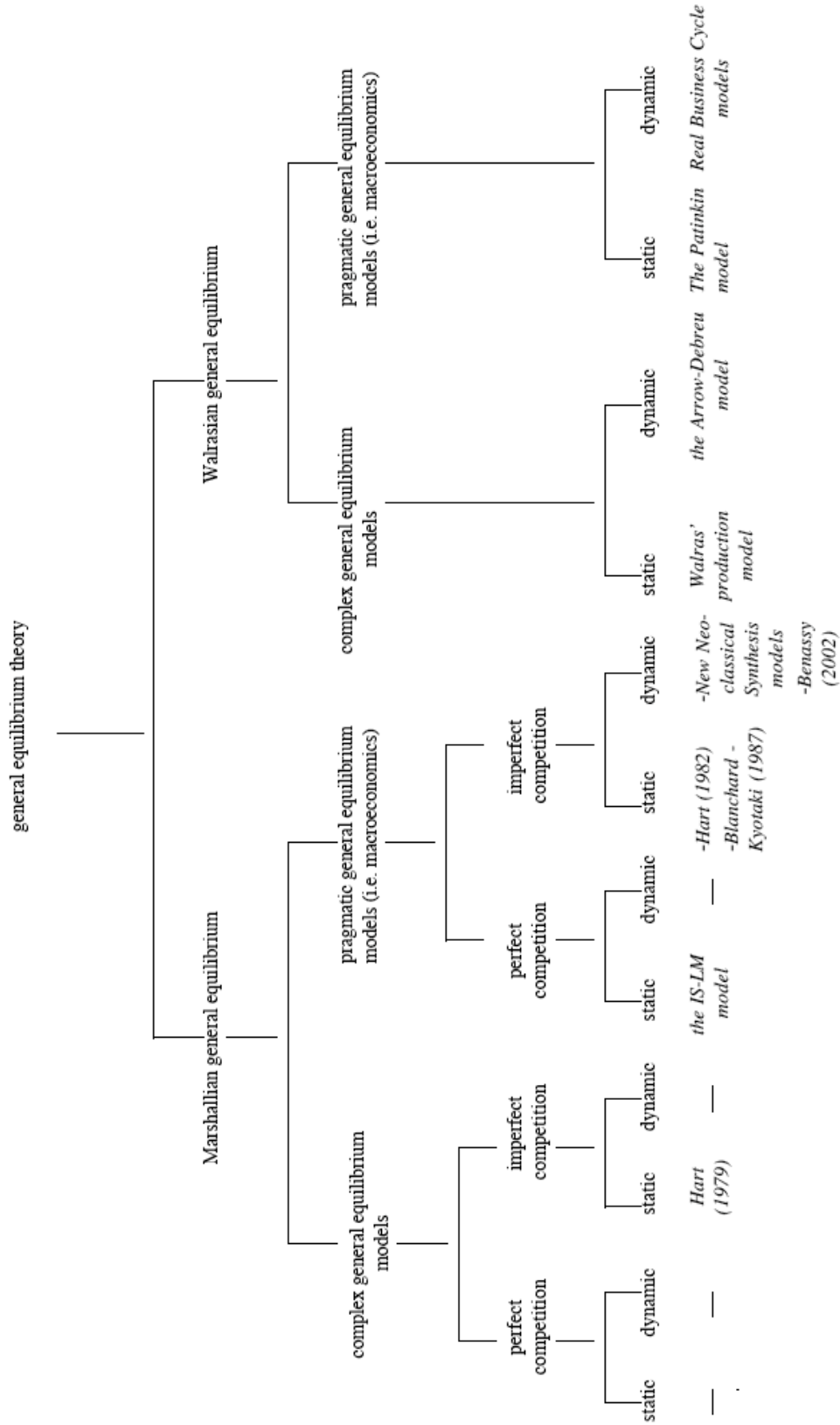


Table 2. General equilibrium and macroeconomics at the turn of the XXth century



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