

Backhouse and Boianovsky on “disequilibrium theory”

**A review article of Transforming modern macroeconomics.
Exploring disequilibrium microfoundations, 1956-2003**

M. De Vroey

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**Institut de Recherches Économiques et Sociales
de l'Université catholique de Louvain**



Backhouse and Boianovsky on 'disequilibrium theory'. A review article of
*Transforming modern macroeconomics. Exploring disequilibrium
microfoundations, 1956-2003*

Michel De Vroey *

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Abstract

In this review article of Backhouse and Boianovsky's book, *Transforming modern macroeconomics. Exploring disequilibrium microfoundations, 1956-2003*, I make the following points:

- (a) Backhouse and Boianovsky's too broad understanding of the disequilibrium approach results in their bringing together theories that should be kept separate. While the disequilibrium label fits the works of Patinkin, Clower and Leijonhuvud, it betrays the project of Barro and Grossman, Drèze and Benassy who strived at producing an equilibrium theory.
- (b) I put in question their claim that an inner link exists between disequilibrium and fixed price equilibrium theories and imperfect competition modeling.
- (c) I try to identify the deep nature of the controversy in which these authors were involved.
- (d) I put forward a few conjectures about the reason why fixed price modeling petered out.

Keywords: disequilibrium theory, non-Walrasian equilibrium models, Keynesian macroeconomics

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* IRES, University of Louvain. michel.devroey@uclouvain.be

Enough time has elapsed since the emergence of modern macroeconomics to warrant the existence of studies aiming at recounting its history. At present such works remain scarce. The book under review is thus a welcome new contribution. As can be drawn from the Preface, Backhouse and Boianovsky's decision to reexamine this episode in the history of macroeconomics originated in their dissatisfaction with the scant attention that disequilibrium theory received in influential surveys of the history of macroeconomics such as Blanchard's and Woodford's. On the contrary, to them, disequilibrium macroeconomics has played a central role in the history of macroeconomics:

We came to see that disequilibrium macroeconomics was far more central to the transformation of macroeconomics that has taken place since the 1950s than we had realized (p. XI)

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Though rather short (it comprises about 190 pages of text), Backhouse and Boianovsky's book covers the history of macroeconomics over a long time span (1956-2003). Its main subject matter is what they call 'disequilibrium macroeconomics' (a terminology that I shall question), but there are many incursions into neighboring themes. The book, which proceeds chronologically, is composed of nine chapters, in addition to the introduction and conclusion. Each of them is divided into sections, which are usually four or five pages long. A great idea, Backhouse and Boianovsky start the book with a *Dramatis Personae*, describing the contributions of the different economists the reader will encounter along her reading. The first two chapters are of an introductory nature. Chapter 1 sets the scene. Among others things, the authors comment upon some modern accounts of disequilibrium theory, they provide citations information, draw a chart summarizing the relationship between the main economists involved and make some remarks on terminology (eventually warning the reader that she will have to count on her own forces to decipher the meaning of the concepts used). Chapter 2 accomplishes the feat of retracing the history of macroeconomics from Keynes to Woodford in fifteen pages. With Chapter 3, we enter into the heart of the matter. After a brief discussion of Hicks's, Modigliani's and Lange's interpretation of the *General Theory*, Backhouse and Boianovsky set themselves to study Patinkin's argumentation in *Money, Interest and Prices*, which is often considered as the fountainhead of disequilibrium theory. To my surprise, their appreciation of his work is rather lukewarm. In their judgment, "he failed to realize the full significance of what he was doing" (p. 41). The chapter ends with a nice section on the neoclassical synthesis. Chapter 4 is devoted to a study of the views of Clower and Leijonhufvud, the other two towering figures of the disequilibrium approach. Clower is highly praised for his 1965 'Keynesian Counter-Revolution' article, in which he introduced the 'dual-decision hypothesis'. Backhouse and Boianovsky hail it for having been the source of all further developments in disequilibrium theory. They also write a fine account of Leijonhufvud's famous book, *On Keynesian Economics and the Economics of Keynes*. The chapter ends with a section on Clower's and Leijonhufvud's Marshallian affiliation.

Chapter 5, 6 and 7 deal with the following generation of models. Chapter 5 broaches a set of themes as is made clear by a mere list of its section titles: "Solow, Stiglitz and the dynamics of income distribution", "Barro and Grossman and the search for microfoundations", "Benassy's model of three regimes", "Developing the fixprice model", "From new paradigm to specialist technique", and "Rationalizing price stickiness". Disequilibrium and Lucas macroeconomics were the two main rival contenders for a reorientation of the field. After having characterized the former in the previous chapters, in Chapter 6 Backhouse and Boianovsky turn to a depiction of the latter. Again, its content is well conveyed by listing the section titles: "Edmund Phelps and the Phelps volume", "From Phelps to new classical macroeconomics"; "Conception of equilibrium"; "Barro and Grossman" (who recanted the approach which they had contributed to creating). In Chapter 7, again, they change sight, zeroing in on the theme of imperfect competition, on the grounds that "non-market clearing and imperfect competition go together" (p. 109). The sections in this chapter are: "Arrow, non-tâtonnement and imperfect competition", "General equilibrium with price rigidities", "Benassy and macroeconomic disequilibrium", and "Imperfect competition as a theory of price adjustment".

Chapter 8 is entitled 'Macroeconomics and microeconomics'. Such a title suggests a theoretical or conceptual discussion, but the chapter rather concerns itself with providing a survey of the reception and appraisal of disequilibrium theory, sometimes engaging in second-degree surveying, i.e. surveying surveys. It also comprises sections, those on Malinvaud and Grandmont, which I would have located in earlier chapters. In Chapter 9, Backhouse and Boianovsky argue that interest in disequilibrium macroeconomics continued in the 1980s and into the 1990s when its heyday was over. They document Solow's and Patinkin's defense of it. They evoke Hahn and Solow's 1995 book, *A critical Essay on Modern Macroeconomics*, attempting to stop the ascent of Lucasian macroeconomics. They also discuss Phelps's work, standing neither with the rational expectations school nor with disequilibrium theorists. Finally, they claim that such disparate economists as Stiglitz, Diamond and Hart have a filiation with disequilibrium theory. The book ends with a short conclusion, entitled "Keynesian economics and general equilibrium theory".

ASSESSMENT

My judgment on Backhouse and Boianovsky's book is mixed. I hold its authors in high esteem. The book reflects the wide range of their knowledge of recent theory. Their account is balanced, mixing internal and external history. It is also exhaustive, covering the writings of a large number of economists involved in this history. I have appreciated the emphasis put on Younes's role, which is often overlooked. The book is well written and full of fine insights. A timely publication, it is a stimulating read.

My dissatisfaction lies elsewhere. First of all, I find the book wanting in its structure. The titles of chapters and sections are often ill chosen. Studying Benassy's work in Chapter V which is devoted to slow price adjustment is odd since Benassy's models deal with the logical existence of equilibrium and fail to consider the speed of equilibrium formation. Presenting Drèze's work in a chapter entitled "General equilibrium and imperfect competition" is also odd. Chapters sometimes look hotchpotch. Their different sections are internally consistent but there seems to be no logic in their ordering. As a result, the reader has the impression of a succession of vignettes.

My second, more important, source of frustration relates the substance of their argument. Here, my criticism is that Backhouse and Boianovsky surf over their subject instead of delving deeply into it. The remainder of my review is devoted to substantiating this general criticism.

A heterogeneous literature

In the conclusion of their book, Backhouse and Boianovsky write that the most important lesson from their study is that the economists at the core of their investigation did not constitute a homogenous group.

These economists approached disequilibrium theory in ways that were very different, and as a result, even when they developed models that resembled each other, they viewed them in different ways (p. 185).

I agree with them, having made this claim myself (De Vroey 2004, chapters 11 and 12). Unfortunately, this conclusion comes out of the blue, since almost nothing in the previous chapters had prepared the reader to it. It is merely adumbrated on p. 46, where they suggest a difference in agenda between Clower and Leijonhufvud and subsequent economists.² My frustration is twofold. First, there are semantic problems. Second, Backhouse and Boianovsky are almost silent on the nature of this existing heterogeneity.

Disentangling the two agendas evoked by Backhouse and Boianovsky implies labeling them differently. Doing this is actually easy. After having used the disequilibrium label at first, Drèze, Benassy and Malinvaud, and the others, made it clear that their models ought to be conceived of as equilibrium models — non-Walrasian equilibrium models— rather than as disequilibrium models.³ The motivation behind this terminology change was to make the label on the bottle match its content, which the word “disequilibrium” failed to do for these economists’ work. Barro and Grossman did not participate in this change, not for a

² "Their ideas [Clower and Leijonhufvud's] about what happened when markets did not clear were later developed by economists who, we show, had agendas that were very different from those of Clower and Leijonhufvud" (p. 46).

³ In their introductory chapter, Backhouse and Boianovsky write: "At this point, the inclination of the historian and the economist part company. The economists developing a theory, will opt for a consistent terminology. The historian will generally favor using the terminology that was used by those whose work he or she is discussing" (p.12). To be consistent historians, Backhouse and Boianovsky should have taken stock of this change in appellation.

substantive reason but because at the time they had lost interest in the approach and thus saw no interest in putting things right (more on this below).

Thus, a prerequisite for sustaining the heterogeneity claim is separating the disequilibrium research line, associated with the names of Patinkin, Clower and Leijonhufvud, from the non-Walrasian equilibrium line, associated with the others. Backhouse and Boianovsky fail to do this. It is a small wonder then that confusion should arise. Let me give three examples. First, on p. 7, Backhouse and Boianovsky write that disequilibrium theory "crucially was an attempt to find an alternative to a theory of general competitive equilibrium believed to be both unrealistic and logically flawed". This statement is true for Leijonhufvud and Clower, but incorrect both for Patinkin and non-Walrasian equilibrium economists. Second, they should not have written, "Drèze had not been the first to construct a disequilibrium model" (p. 112), since this was not Drèze's intention. The last example is the subtitle of the book, "Exploring disequilibrium microfoundations". This expression may be applied to Patinkin, Clower and Leijonhufvud, but not to non-Walrasian equilibrium economists.

Linked to Backhouse and Boianovsky's too broad understanding of the notion of disequilibrium is another semantic mistake, which is to consider that 'disequilibrium' and 'non-Walrasian' are synonyms, (pp. 30, 153, 180). In a sense, it is obvious that Leijonhufvud, for example, is non-Walrasian. However, negative characterizations are treacherous. Defining oneself as non-Christian covers many possible standpoints on religion. In the case at hand, there are degrees of 'non-Walrasianism'. Clower and Leijonhufvud are radically non-Walrasian; Barro and Grossman and the others are only slightly non-Walrasian. In other words, the former are anti-Walrasian, as can clearly be seen from Leijonhufvud (1968), Clower ([1975] 1984) and Clower and Leijonhufvud ([1975] 1984), while the latter are not. Barro and Grossman and the others adhere to the neo-Walrasian program, but want to modify it minimally in order to reach Keynesian conclusions. They are non-Walrasian because their models reach an equilibrium allocation which differs from the Arrow-Debreu allocation, but they basically belong to the same research program, contrary to what terminology suggests. To avoid ambiguity, it is preferable to use fix-price equilibrium (I shall use the non-Walrasian equilibrium and fix-price equilibrium in alternance).

Shifting from semantics to substance, Backhouse and Boianovsky do a fine job of characterizing the mission fix-price equilibrium economists set for themselves:

What united this group of economists was their commitment to general equilibrium theory: for all of them, rigorous theorizing meant starting from the framework of general equilibrium theory and developing it to deal with macroeconomic issues. ... In the 1970s, these general equilibrium

theorists developed general equilibrium models that explained Keynesian phenomena — that provided microfoundations for Keynesian macroeconomics (pp. 120-1).⁴

Nonetheless, I wonder whether Backhouse and Boianovsky would agree with my own view about the distinctive contribution of non-Walrasian equilibrium models, namely that these models were able to demonstrate that the existence of fixed prices does not rule out the possibility of a general equilibrium allocation (a non-Walrasian allocation yet differing from the baseline Walrasian allocation). The aim of fix-price economists was not to demonstrate that, because of fixed prices, the economy is in disequilibrium. Rather, they wanted to restore the notion of equilibrium, showing that it was unnecessary to get rid of it in order to have a Keynesian result.

Backhouse and Boianovsky may well assert the existence of important differences among the economists they study, but they shed little light on their nature and causes. My own take can be summarized in a box diagram. The first of its two criteria is whether the result put forward arises during the equilibration process (finishing when equilibrium is reached) or whether it has an end-state existence, in which case it is assumed that equilibrium arises instantaneously. The second is the Marshall-Walras divide. It stems from the idea, expounded in detail in De Vroey (2012), that, contrary to the general belief, the Marshallian and the Walrasian approaches are alternative rather than complementary research programs.

Table 1. Differentiating the economists under study

	Marshallian approach	Walrasian approach
Process occurrence (slow adjustment towards equilibrium)	– Clower and Leijonhufvud	– Patinkin
End-state occurrence (instantaneity being assumed)		– Barro & Grossman, Drèze, Malinvaud – Benassy

The table calls for the following comments. To begin with, it must be noticed that all the models conceived by the economists mentioned in the table are general equilibrium models, concerned with the functioning of the entire economy. The box also shows that the most opposed modeling strategies are those of Clower and Leijonhufvud, on the one hand, and those of fix-price equilibrium economists, Benassy being an exception. Clower and Leijonhufvud wanted to engage in Marshallian general equilibrium centered on processes

⁴ I had a hard time understanding that by "this group of economists", Backhouse and Boianovsky meant non-Walrasian equilibrium economists.

rather than on the existence of equilibrium. As already stated, the others accepted the main tenets of the Walrasian research program.

Benassy's case is special. While the other non-Walrasian equilibrium economists just paid lip service to Clower and Leijonhufvud, he took their precept of getting rid of the auctioneer in earnest. He also wanted Clower's dual-decision hypothesis to be the starting point of his model. Without vindicating it, he ended up adopting the Marshallian explanation of the formation of market-period equilibrium, i.e. assuming that agents have the ability to conjecture market conditions correctly. If every agent can do this, no exchanges at a price-quantity mix different from market equilibrium will occur.⁵ That is, the model builder's omniscience about the fictive market constructed is extended to the agents inhabiting the model— a heroic assumption, no less a *deus ex machina* than the auctioneer assumption. Keynes implicitly generalized this assumption to the entire economy, an even more heroic move. Benassy followed suit while further assuming that the excess demand functions which the agents are supposedly knowledgeable about are the effective rather than the notional ones. As a result, classifying Benassy's models requires subtlety. As far as explaining how equilibrium is reached, he is Marshallian since he replaces the auctioneer hypothesis with the agents' omniscience assumption. However, as far as theorizing style and methodological principles is concerned, Benassy is Walrasian.

Backhouse and Boianovsky on Patinkin and Clower

While Backhouse and Boianovsky's judgments about the authors they study are usually balanced, it is not the case in their discussion of Patinkin and Clower. I find their appreciation of Patinkin too lukewarm and that of Clower too dithyrambic.

In their section on Patinkin in Chapter 3, they observe that Patinkin was hardly aware of the significance of his analysis. Witness to this, they claim that terms such as 'disequilibrium' or 'non-market clearing' "are conspicuous by their absence" (p. 41). This is correct for the expression 'non-market clearing' — it was hardly in usage at the time — but not for 'disequilibrium'.⁶ Backhouse and Boianovsky also take the fact that during the 1960s and the 1970s Patinkin worked on other topics as a justification for their observation. To this I oppose that, throughout his life, he fiercely fought to defend his disequilibrium explanation of unemployment. Backhouse and Boianovsky themselves, a few pages later (p. 50), point out

⁵ The way in which Backhouse and Boianovsky capture Benassy's reasoning looks mysterious to me as they write: " His [Benassy's] reasoning not that agents knew all other agent's demands, but that their demand would be based on information, and that this information would reflect the demand expressed by other agents" (114). XXXI do not see what information other than the shape and size of effective demand and supply they could have in mind. I also disagree with their statement that Benassy "had to assume a type of tâtonnement for he could not explain how the economy got into equilibrium" (pp. 114-5).

⁶ The two following passages invalidate their assertion: "... the coexistence of involuntary unemployment and flexible money wages precludes the existence of equilibrium" (p. 315). "It is within the foregoing framework of dynamic disequilibrium that we must study the problem of involuntary unemployment" (p. 323).

that as early as 1959, just three years after the publication of *Money, Interest and Prices*, Patinkin criticized Hicks for his failure to think in disequilibrium terms.

I would have preferred it if Backhouse and Boianovsky had entered in a substantive discussion of Patinkin's argumentation instead of concerning themselves with Patinkin's states of mind. They have a section entitled "Patinkin, inconsistency and unemployment", which suggests that they find Patinkin's analysis inconsistent, but no justification is provided.⁷

My interpretation of Clower's Counter-Revolution paper (Clower [1965] 1984) differs from Backhouse and Boianovsky's. First of all, I view it as an exception with respect to Clower's previous and subsequent writings.⁸ Indeed, in this article, he departed from his recurrent anti-Walrasian standpoint, to tread on the enemy's home turf, i.e. adopting the Walrasian language. Introducing the dual-decision hypothesis was his way of waging this battle. With hindsight, it is clear that it was bound to be lost. The dual-decision hypothesis makes perfect sense in a Marshallian framework, wherein production is assumed to take place before trade, but it does not fit Walrasian theory where production takes place only after the equilibrium price vector has been determined. It is thus no surprise that, with the exception of Benassy, fix-price theorists did not incorporate this hypothesis in their models. Clower himself soon came to the view that he had taken the wrong route. Siding with Leijonhufvud, he went on fostering a 'Marshall process-oriented general equilibrium' research program (Clower and Leijonhufvud [1975] 1984). In his Afterword in his collected papers volume (Walker 1984), Clower disowned Benassy for having adopted the dual-decision hypothesis and, more generally, the whole group of fix-price economists for having followed the trail he had opened.

Backhouse and Boianovsky write that "virtually all the literature on Keynesian economics" (I guess that they mean the non-Walrasian equilibrium literature) stems from Clower's result according to which the existence of rationing in the labor market makes the notional demand for goods give way to effective demand. This is only half of the story, the other half being Patinkin's symmetric proposition that rationing on the goods market triggers a replacement of the notional demand for labor with effective demand. The two propositions should be put on equal footing, and Patinkin given as much credit as Clower for the invention of the spillover effect.

⁷ I have argued that the flaw in Patinkin's reasoning lies in his lack of consideration for the existence of an income effect arising when false trading is present, to the effect that the economy fails to converge towards the equilibrium that would have prevailed had false trading been absent (De Vroey 2004, Chapter 11).

⁸ Backhouse and Boianovsky implicitly recognize this as they write, "Clower never thought in terms of Walrasian general equilibrium" (p. 63). They are right; they just forget that there was an exception, his 1965 article.

Linking 'disequilibrium theory' and imperfect competition theory

I am amazed by the important place which Backhouse and Boianovsky give to imperfect competition theory in their book. Disequilibrium theorists all reasoned assuming perfect competition, following Keynes. The same is true for fix-price equilibrium economists with a few exceptions, in particular Benassy. He proposed an early model of rationing with imperfect competition, which he referred to in all his surveys of the approach (e.g. Benassy 1982). Still, my impression is that this model constituted a one-shot achievement. The systematic introduction of imperfectly competitive models in macroeconomics had to wait for the emergence of new Keynesian modeling, a decade or so later. In contrast, Backhouse and Boianovsky seem to think that the ascent of 'disequilibrium theory' and imperfect competition modeling are two faces of the same process (p. 109). The following excerpt summarizes the reasoning behind their viewpoint:

Adopting a general equilibrium framework requires analysis of individual agents in a way that macroeconomics modeling does not, for individuals are modeled explicitly. This focuses attention on individual decision making and leads naturally to the problem of price setting. However, if agents have the power to set prices, market cannot be perfectly competitive, which leads naturally into a discussion of imperfect, or monopolistic competition (p. 105).

It is worth pondering this quotation sentence by sentence. The first one expresses the view that the microfoundations perspective is normally absent from macroeconomics. Such exclusion cannot be declared inherent to macroeconomics. Had Backhouse and Boianovsky written 'Keynesian macroeconomics usually did not start from individual decision', it would have been fine. As for the second sentence, it may comprise a *non sequitur*. Why should attention to individual decision naturally lead to the issue of price setting? This is not the case in a Walrasian framework, in which individual decisions are made taking prices as parameters. Price setting is thus an issue which is different from individual decision making, the auctioneer hypothesis taking care of this process (quite poorly). Things are different in a Marshallian set up. Here, agents are price quantity makers (rather than price makers as is often written); they announce price quantity mixes to potential traders. For the Marshallian case (i.e., in this discussion, Benassy's model), Backhouse and Boianovsky's second sentence is acceptable. But then, a problem arises with the third sentence in which Backhouse and Boianovsky declare that price setting and perfect competition are mutually exclusive. I see no reason for this proposition, except to define imperfect competition as price setting. Benassy's models, other than his imperfect competition model, are the best counter-example: they combine perfect competition (as defined by Marshall) and price quantity making agents.⁹

To conclude on this point, contrary to what Backhouse and Boianovsky claim, there is no need, either logical or historical, to establish an inner link between disequilibrium theory

⁹ Backhouse and Boianovsky reiterate their argument on p. 183. "[In the 1970s] disequilibrium theory became entwined not only with theories of imperfect competition, without which it was hard to, but also with arguments about information". Oddly enough, in the next paragraph, they write what seems to me to be the opposite statement: "[disequilibrium] economists continued, for the most part, to assume perfect competition".

and non-Walrasian equilibrium theory, on the one hand, and imperfect competition theory, on the other.

Involuntary unemployment and Walrasian theory: incompatible bedfellows

Backhouse and Boianovsky do not question the view that fix-price economists succeeded in demonstrating the possibility of non-market clearing (and hence of involuntary unemployment). Involuntary unemployment refers to a situation in which some agents are unable to make any element of their optimizing plan come through — in other words, they endure an individual disequilibrium experiment. On reflection, wanting to integrate such an outcome in Walrasian general equilibrium analysis is an odd goal. As stated by McKenzie, "The general equilibrium implies that all subsets of agents are in equilibrium and in particular that all individual agents are in equilibrium" (McKenzie 1987, p. 498). Less obvious, the same incongruity arises when it comes to integrating involuntary unemployment in the Marshallian research program (De Vroey 2007). No unemployment result, not to mention involuntary unemployment, can be produced in either of these frameworks. The reason lies in their trade technology assumptions, the auctioneer hypothesis in the first case, and agents sharing the model-builder's omniscience in the second.¹⁰ Claiming to have a theory of involuntary unemployment with all agents in individual equilibrium thus amounts to wanting to have one's cake and eating it! Once this point is accepted, the conclusion must be drawn that the fix-price economists' alleged demonstration of involuntary unemployment relies on a trick. I regard it as a semantic trick which runs as follows.

Involuntary unemployment is associated with non-market clearing. As long as long as economic reasoning remains simple, market clearing can be defined in two ways: (a) equality between supply and demand at the end of a given market period, and (b) generalized individual equilibrium. However, the arising of new developments compels to decide which of these is the most significant one. My contention is that it is the second. On the contrary, to all intents and purposes, non-Walrasian equilibrium models adopt the first definition with one difference. The hallmark of these models is the presence of a split between *notional* supply and demand and *effective* supply and demand. The latter, not the former, is at work in the formation of market period equilibrium. In other words, general equilibrium is attained when effective supply and demand match for all commodities. As to notional magnitudes, they enter the picture only as milestones, describing what the equilibrium allocation would be had prices been flexible. One need only define market clearing as the matching of notional supply and demand to draw the conclusion that non-market clearing has been proven (and hence involuntary unemployment).

¹⁰ As argued in Batyra and De Vroey (2012), a cogent unemployment theory had to wait for the ascent of search models, which are based on other trade technology assumptions.

To me, this is a mistake. To justify my standpoint, I invite the reader to ponder upon 'rationing' as occurring in an imperfect competition context. As was well perceived by Hart, an economist Backhouse and Boianovsky briefly study, such models can tackle underemployment but not unemployment (Hart 1982, p. 114). The optimal decision of a monopolistic firm is to fix the price-quantity mix where, graphically, the quantity corresponds to the intersection of marginal revenue and marginal cost and the price is set at the level corresponding to this quantity on the demand curve. This position is off the supply curve. Thus, according to the supply and demand matching criterion, this is a case of non-market clearing. This makes little sense. Were such a characterization accepted, it should be admitted that market clearing is associated with inefficiency and efficiency with non-market clearing!

Backhouse and Boianovsky have no qualms with the non-Walrasian economists' definitional practice. This hardly makes them an exception. Most macroeconomists do the same — even Lucas and Sargent in their famous "After Keynesian Macroeconomics" paper fell prey to it.¹¹ It is not that, faced with the choice between the two meanings of market clearing I have distinguished above, economists explicitly choose the supply and demand matching definition, rejecting the generalized individual equilibrium one. It is rather that they are unaware that in some circumstances (not only the case examined here, but also, for example, when it comes to new Keynesian models), such a choice needs to be made. I surmise that, were economists invited to make such a choice explicitly, they would join my side.

I need to underline that my point bears on the integration of involuntary unemployment understood as a case of individual disequilibrium. It does not imply that it is impossible to reach a Keynesian result — a demand activation policy conclusion — in the frame of Walrasian theory. Diamond's transaction externalities 1982 model, which Backhouse and Boianovsky also discuss, is an example of this alternative research line. In this model, from which unemployment *tout court* is absent, generalized optimizing behavior and sub-optimality, to be remedied by demand activation, coexist.

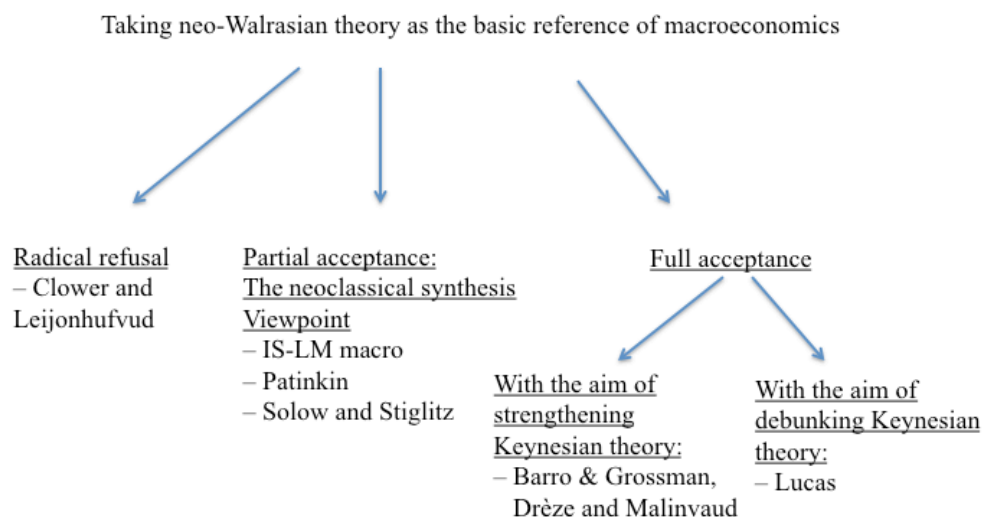
The tasks ahead

As noted at the beginning of this review, the aim of Backhouse and Boianovsky's book was to establish that 'disequilibrium' macroeconomics played a central role in the transformation of macroeconomics. For all the interest of this ambitious program, I doubt whether their book achieves what they set out to do. Much territory has been covered; many diverse streams presented, yet no clear conclusion emerges. Two crucial questions in particular remain unanswered: (a) what exactly was the controversy which disequilibrium and

¹¹ Introducing the 'equilibrium discipline' notion, Lucas and Sargent write that, to be valid, economic models must rest on two postulates: (a) agents act in their own self-interest and their behavior is optimal; and (b) markets clear (Lucas and Sargent, [1979] 1994, p. 15). This phrasing amounts to assuming that market clearing means something different from generalized individual equilibrium, presumably, the matching of supply and demand.

fix-price economists took part in?, and (b) what explains the demise of the non-Walrasian equilibrium paradigm? My own answers are as follows.

Until the 1970s, with the exception of Patinkin (though he was not considered a real macroeconomist), Walrasian general equilibrium theory and macroeconomics were regarded as two separate territories, their practitioners belonging to non-connected scientific communities. Keynesian macroeconomics was more or less generalized Marshallian partial equilibrium analysis. Pragmatism and a loose attention given to the microfoundations requirement were among the traits which it had inherited from Marshall. In the 1970s, the scene changed. The contention arose that Walrasian principles might have their say in macroeconomics. This view originated from both non-Walrasian equilibrium economists and the founders of 'new classical macroeconomics', in particular Lucas. The possibility that Walrasian general equilibrium analysis might be useful for macroeconomics makes sense because both deal with the workings of an entire economy and the interactions of its composing elements. Macroeconomics can be viewed as simplified general equilibrium analysis with two additional dimensions, an applied, empirical perspective and a policymaking stake. Thus, disequilibrium and non-Walrasian equilibrium theories participated in a broader debate about how macroeconomics should position itself with respect to Walrasian (or neo-Walrasian) theory. The chart below summarizes the four main standpoints taken on this issue.



The first standpoint, of which Clower and Leijonhufvud were the custodians, asserts that the Walrasian paradigm with its auctioneer-led trade technology is totally unfit to address the issues that are on the agenda of macroeconomics. They positively endorsed the Marshallian approach while trying to make it more attentive to the study of adjustment

processes. The second position constitutes an adhesion to the neoclassical synthesis viewpoint. Let me remind the reader that the latter states that Keynesian theory is the valid theory to adopt when studying the short period during which markets are supposedly in disequilibrium, while Walrasian theory is apt to come to grips with the study of the economy when it has reached equilibrium, which is understood as a state of rest. Thus these economists are not against Walrasian theory. They just want it to remain in its own place, leaving the domain of the short period to Keynesian macroeconomics.¹² Looking at the composition of these first two standpoints, again we observe the split between Clower and Leijonhufvud, on the one hand, and Patinkin, on the other, in spite of their common emphasis on the need for a disequilibrium approach. The third standpoint abandons the neoclassical synthesis viewpoint, fully adopting the Walrasian language and method. Two variants of this standpoint exist. The first is held by economists motivated by the desire to strengthen Keynesian theory, which is the vision of non-Walrasian equilibrium economists. The second variant is Lucas's, according to which Walras and Keynes are incompatible bedfellows, and the research line to be abandoned is the Keynesian one. Again, Benassy is absent from this classification since his research strategy spans two of the four standpoints as he has one foot in the Marshallian approach (his trade technology assumption) and the other one in the Walrasian approach.¹³

In the 1970s, there were thus four contending streams (leaving aside smaller contenders such as post-Keynesian theory): (a) standard Keynesian macroeconomics based on the IS-LM model; (b) disequilibrium theory *à la* Clower/Leijonhufvud, a radically anti-Walrasian standpoint; (c) non-Walrasian equilibrium modeling proposed by economists taking Walrasian theory as their starting point yet wanting to modify it so as to reach Keynesian policy conclusions; and (d) new classical macroeconomics, led by Lucas, the standpoint of economists who saw no reason to merge Keynes and Walras. Only the last two streams were cutting edge. Keynesian macroeconomics, which had dominated the scene for the past two decades, was on the defensive after the criticism of the Phillips curve by Phelps and Friedman, the challenge raised by the stagflation episode, and the 'Lucas critique'. Though they were held in high respect by the profession, Clower and Leijonhufvud were confined to the righters-of-wrongs role. Focusing on the need to address the issue of coordination failures was fine. But, if no tools were available, what else could be done than draw blueprints of what the ideal yet inaccessible theory should be? ¹⁴

¹² Patinkin tried to substantiate the view that Keynesian disequilibrium states gravitate towards the Walrasian equilibrium, but he failed. For their part, new Keynesian economists such as Solow and Stiglitz admit that this gravitation has not been demonstrated, but still defend the need for macroeconomics to be composed of different models tailored for different purposes (cf. De Vroey and Duarte 2013).

¹³ The same can be stated about DSGE modeling with the further feature that in these models imperfect competition has fully come to the forefront.

¹⁴ In the 1990s, however, things began to change with the ascent of agent-based computational economics, a result of the computer revolution.

It is clear that fix-price equilibrium macroeconomics lost out to Lucasian macroeconomics — it being admitted that in our field, the victory/defeat terminology is misleading; in a way or another, the 'victorious paradigm' always end up, if not dethroned, at least endogenously transformed. In their 1978 article, Muellbauer and Portes wrote that non-Walrasian equilibrium modeling was likely to serve as an alternative framework to IS-LM macroeconomics (1978.p. 788). This did not happen. It is not that IS-LM won over non-Walrasian equilibrium modeling.¹⁵ It is rather that both were dethroned by Lucasian macroeconomics. After a quick and promising start, especially in Europe, non-Walrasian equilibrium modeling lost its momentum. Many of the young researchers, who started their career following this line, shifted to the Lucasian program or moved to other areas of research. Although Backhouse and Boianovsky mention elements which played a part in this issue (the change in equilibrium concept, or Barro's recantation), they do not engage in a systematic investigation of the reasons explaining the Lucasian 'victory'.

What I surmise is that two elements played a joint part. The first is that after the pioneering articles succeeded in setting out a new framework, no precise view existed about what to do next. The second is the existence of an alternative program; the demise of non-Walrasian equilibrium models ought to be understood in light of the ascent of Lucasian macroeconomics. It must be underlined that the rivalry that ended up in this demise was one between siblings. Methodologically, Lucas and non-Walrasian equilibrium economists were very close. They belonged to the same neo-Walrasian paradigm, used the same language, and abided by the same standards.¹⁶ Their dissent concerned the direction to be taken and, beyond this, ideology, i.e. the policy conclusions to be upheld. While new classicists wanted macroeconomics to be purely Walrasian — the consequence of which was that, at least in a first stage, policy conclusions were non-interventionist — non-Walrasian equilibrium economists wanted it to corroborate Keynes's insights about the workings of the economy.

One appeal of the Lucasian program was its new conception of equilibrium. Backhouse and Boianovsky discuss it briefly. They also comment on Lucas's main methodological paper, "Methods and Problems in Business Cycle Theory". Nonetheless, they fail to convey what, to me, seems to be the central point, the potentiality of a transformation of the field which this innovation brought along. Traditional dynamic analysis was concerned with the adjustment towards a fixed state of rest (stationary equilibrium), a perspective which was unable to come to grips with economic change in any strong sense. Non-Walrasian

¹⁵ Actually, it turned out that connections between the non-Walrasian equilibrium model and the IS-LM model were quite easy to make. This was a mixed blessing since it meant that the contribution of the new models was just to supplement traditional Keynesianism.

¹⁶ Backhouse and Boianovsky seem to have missed this communality of views. For instance, they write that "for Lucas, it did not even make sense to talk of a market that was not in equilibrium". (p. 186), suggesting that for disequilibrium and non-Walrasian equilibrium theorists such talk makes perfect sense. It may be the case for the former but not for the latter once market clearing is correctly understood.

models were mainly static but, when they dealt with dynamics, it was in the old way. By contrast, Lucas's conception of equilibrium, which was inherited from Hicks and Arrow-Debreu, removed the paralyzing effect of the previous conception of equilibrium. New horizons then opened up, in particular the possibility of constructing an equilibrium theory of business fluctuations. The fact that new demanding mathematical tools had to be used was another appealing aspect. In short, Lucas's program looked more promising than the non-Walrasian equilibrium program.¹⁷

However, the crucial turning point occurred when Kydland and Prescott took over from Lucas, inaugurating the RBC variant of the Lucasian program. As aptly underlined by Woodford, they transformed Lucasian qualitative modeling into quantitative modeling.¹⁸ Adding the 'replication discipline' to the 'equilibrium discipline', Kydland and Prescott were able to impose the idea that there is no salvation in macroeconomics outside of applied work — an idea which was dear to Keynesian macroeconomists but had somewhat been set aside when a Walrasian perspective came to the forefront. In addition, to the econometricians' outcry, they were also able to force the widespread acceptance of a very specific empirical assessment procedure, the calibration technique. This technique implied that the search for a baseline model of business fluctuations was over as the stochastic extension of the Solow model was deemed to be 'established theory'. What remained to be done was applying it to different contexts and solving the puzzles that its application might reveal. A game changer, RBC modeling stabilized the Lucasian revolution into a well-defined research program providing the research bread and butter for scores of economists for more than a decade. Against such a strong bandwagon, the non-Walrasian equilibrium economists had two options: jump on or be left behind.

¹⁷ This factor was probably more important than the lack of exhaustion of the mutually benefitting trade argument, which Barro evoked at the time to justify abandoning the approach which he had contributed to creating (Barro 1979). The following extract from Barro's 2005 interview with the Federal Reserve Bank of Minnesota magazine, *The Region* testifies to this: "In the early 1970s, I believe, you told Gary Becker that you thought macroeconomics was stagnating and you thought you might go into microeconomics. ... Barro: I was clearly a very bad predictor in the early 1970s of how macroeconomics would evolve. I think I was particularly influenced at that time by Keynesian macroeconomics, which I had been working on, and I think I was accurate in seeing this field as not having great potential. But as you've suggested, shortly after I said that macroeconomics was not promising, there was an amazing sequence of exciting developments. It started with the rational expectations work — applied initially to monetary models with some version of a Phillips curve. And then there was the real business cycle analysis that Prescott particularly pioneered. Then you had the very exciting work on economic growth, with growth viewed more and more as a core part of macroeconomics. Then there was the pretty much distinct empirical work on economic growth. So, this was a whole sequence of, you could say, four different exciting developments in macroeconomics after I said it was stagnating" (2005, p. 12).

¹⁸ "[The real business cycle literature] It showed how such models [of the Lucas type] could be made *quantitative*, emphasizing 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" (Woodford 1999, p. 25-26).

CONCLUSION

After all these harsh remarks, I want to conclude on a positive note by saying that I find that Backhouse and Boianovsky's book is a good, intelligent and stimulating introduction to the history of macroeconomics. As such, it will surely find a readership. Taking over the baton and tackling the issues which they have left open remains to be done in the future.

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Institut de Recherches Économiques et Sociales
Université catholique de Louvain

Place Montesquieu, 3
1348 Louvain-la-Neuve, Belgique

