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Disequilibrium economics: some comments about its nature, birth and fate

A review essay on Transforming Modern Macroeconomics, The Relationship of Micro and Macroeconomics in Historical Perspective (2013) by Roger Backhouse and Mauro Boianovsky

Goulven Rubin¹

¹ EQUIPPE and University of Lille 2 Law and Health, goulven.rubin@univ-lille2.fr.
Introduction

In 2013, Roger Backhouse and Mauro Boianovsky published *Transforming Modern Macroeconomics Exploring Disequilibrium Microfoundations, 1956-2003*. This book presents the first comprehensive history of the search for disequilibrium microfoundation or disequilibrium theories. The aim of the present essay is twofold. In the first part, I will summarize their argumentation in order to capture their most important claims. In the second part, I will assess these claims and sketch an alternative view of the subject. Backhouse and Boianovsky offer a larger treatment of the subject than the one proposed by De Vroey (2004, 2009), the only true alternative. But their version of the story can be questioned on two grounds. Firstly, their presentation of the early phase of the search for disequilibrium microfoundations downplays the role of Don Patinkin. As a consequence, they tend to overemphasize the heterogeneity of approaches related to disequilibrium. This leads to a dubious explanation of the failure of these approaches to survive the 1980s. Secondly, they argue that disequilibrium theories left a deep mark on contemporary macroeconomics. This claim derives from the importance they attach to models of imperfect competition within the disequilibrium literature and to the idea that any model of imperfect competition is a disequilibrium model. Since monopolistic competition is a central feature of contemporary macroeconomics, they conclude that disequilibrium economics lives on. But this claim is not supported by a step by step demonstration. Reading their rich and important book will also be the occasion to take stock and identify the issues that remain unanswered and need to be clarified in order to complete the history of disequilibrium macroeconomics. I shall also ponder upon the possibility for such a history to inform our understanding of contemporary macroeconomics.

1. A survey of *Transforming Modern Macroeconomics*

The introduction of the book summarizes its main motivation. In macroeconomics, “microfoundations” have been equated with the elaboration of miniature general equilibrium models in which representative agents are optimizing. In this context, all markets clear and there is no room for involuntary unemployment. As a result, Keynesian models of the 1960s have been discarded for their lack of acceptable microfoundations. Practitioners of macroeconomics have often told the story of the evolution of the discipline from the primitive models of the Keynesian era to the New classical and New synthesis models of today. But they usually fail to mention the large body of works developed around the 1970s as a result of a different conception of microfoundations:

“However, the search for microfoundations, which was well under way long before the 1970s, for reasons that clearly had nothing to do with the macroeconomic challenges of that period, also involved economists who argued that Walrasian models were not suitable for analyzing the real world” (page 2).

Backhouse and Boianovsky propose to offer a “much fuller account of the search for disequilibrium microfoundations” that includes all the strategies that were followed around the 1970s. This leads to a number of claims that the book intends to demonstrate.
1. The search for microfoundations was not the result of stagflation. It began before that and mainly as an attempt to find an alternative to the Arrow-Debreu model considered as “unrealistic and logically flawed”.

2. The literature on disequilibrium went beyond fixed-price models.

(i) For this literature, adjustment towards equilibrium took time. Hence, trade was bound to occur before its attainment. So a general theory of the functioning of market out of equilibrium was needed.

(ii) This literature resulted from a rejection of the auctioneer assumption either because it was not realistic or because it was inconsistent with the assumption that transactions were without costs. The question was who set the prices.

(iii) The literature contained models with endogenous prices.

This list of claim is followed by an important “note of terminology”. This note clarifies Backhouse and Boianovsky’s definition of disequilibrium, a very encompassing one actually. They begin by saying that disequilibrium occurs when the state of rest that is reached on the market goes along with agents that are off the “conventionally defined supply and demand schedules” (page 9). So disequilibrium means non market clearing in the sense that “conventional supply and demand” do not coincide. But then they add that this outcome may arise when “competition is imperfect, or monopolistic” and when “agents have mistaken expectations or asymmetric information, or are bound by long-term or even implicit contracts”. As a result, a very large variety of models are considered as featuring “disequilibrium” and not only the fixed-price models of the 1970s.

The last section of the introduction is also important to understand what Backhouse and Boianovsky try to do. They recognize that their history is only lightly contextual and actually focuses on the internal logic of theoretical developments.

Chapter 2 offers a condensed history of macroeconomics from Keynes (1936) to Woodford (2003). Although it may be very useful to gain a general understanding of the context of the story told in the book for readers that lack any knowledge in the history of macroeconomics, it is not strictly necessary for the following chapters.

Chapter 3 on “Don Patinkin and the Neoclassical Synthesis” is the real opening chapter on the history of disequilibrium theories. It offers an interesting contextualization of the origins of disequilibrium economics. Whereas De Vroey (2009) starts with the work of Patinkin, Backhouse and Boianovsky relate his work to the ideas of Hicks (1939) and to the works of Lange (1942, 1944). So the story gains in scope. Backhouse and Boianovsky stress the fact that Patinkin was inspired by Lange’s restatement of Keynes’ chapter 19 in terms of stability analysis of general equilibrium system. Then they present the content of chapter 13 of Money, Interest and Prices (1956) but end up being quite critical with respect to Patinkin. According to them: “Patinkin’s awareness of the significance of his analysis of non-market clearing behavior appears to have grown rather slowly” (page 41). The chapter ends with a rather
standard presentation of what was called the “neoclassical synthesis” and the role of Patinkin in its establishment.

Chapter 4 presents the contributions of Clower and Leijonhufvud. Like d’Autume (2000) or Benassy (1984), Backhouse and Boianovsky consider that Clower (1965) was a key contribution. The concept of “dual decision hypothesis” was “crucial to virtually all the ensuing literature” (page 45). An important contribution of this chapter is the discussion of unpublished manuscripts coming from Duke University’s archives that shed light on the path leading to Clower’s 1965’s contribution. Though these archives have been available for a long time, it is the first work that attempt to use them in a comprehensive manner. Drawing on this material, the authors conclude that Clower developed his insights by following an independent research program centered on the dynamics of stock-flow models. They insist in particular on the fact that Patinkin did not realize the connection between the ideas of Clower and his own work. After that they present the contribution of Leijonhufvud and his collaboration with Clower in the 1970s. The main point here is that Clower and Leijonhufvud were both dissatisfied by the representation of the market offered by the Walrasian theory and were looking for something more realistic. For them, this implied coming closer to Keynes economics. But their aim was more general. And what they had in mind was a theory without any rigidity of prices. Their focus was on market process and dynamics.

The core of chapter 5 surveys the main macro models that were inspired by Patinkin, Clower and Leijonhufvud’s contributions. Yet, the first section is devoted to the particular contribution of Solow and Stiglitz (1968) considered has another starting point of this literature. The two authors developed a model showing the dynamics of a two market economy in which prices and wages were parametric. The aim was to clarify the debates between the two Cambridge. But the model presented an economy out of equilibrium and featured a number of assumptions and properties that would be found in fixed-price models of the 1970s. A short side adjustment rule was made explicit and various regimes were displayed for different values of the real wage. But this model was totally ad hoc. No micro-foundations were offered. That was the contribution of Barro and Grossman (1971) who enlarged the model to the money market and clarified the decisions that firms and households faced out of equilibrium thanks to the insights of Clower and Patinkin. Backhouse and Boianovsky insist on the fact that Patinkin, Clower or Solow and Stiglitz did not consider Barro and Grossman’s model a faithful expression of their respective visions. Clower and Leijonhufvud were particularly critical. Patinkin was grateful to Barro and Grossman for having made him realize the connection between his insights and the contribution of Clower and for having stated his priority over Clower and Leijonhufvud. A section is devoted to a model that Jean-Pascal Bénassy elaborated during his PhD at Berkeley and that refined the Barro and Grossman model and the analysis of its properties. Backhouse and Boianovsky also spend a section on the model of Muelbauer and Portes (1978). They show how this class of model was applied to a variety of issues like consumption behaviors, international trade or planed economies becoming what they call a “specialist technique”. Finally, they argue that a number of contributions in the 1970s were developed to explain why prices could remain rigid. They
mention the works of Stiglitz on asymmetric information, the models of staggered prices and wages developed by Phelps, Taylor or Fisher and the literature on implicit contracts.

Chapter 6 is essentially an aside to the main thread of the book. It presents the birth and the evolution of the Lucas’ research program until Barro and Grossman repudiated their work on disequilibrium around 1979. This contextualization is interesting in itself, in particular for the non specialists, but apart from the last section on Barro and Grossman is it not essential to the book.

Chapter 7 supports one of the main claims of the book. After a brief presentation of the history of Walrasian theory from Value and Capital on, it argues that dissatisfaction towards the Walrasian model would have been an independent source of disequilibrium theories:

“The Walrasian model, as interpreted by Arrow and Debreu, was fundamentally flawed as a general account of the economy”.

Or,

“They were economists who started from the assumption that Arrow-Debreu general equilibrium model had flaws that needed attention, something that was evident even before it received its canonical formulation in Debreu’s The Theory of Value (1959)” (page 121).

The source here would have been a paper by Arrow published in 1959 titled “Towards a theory of price adjustment” pointing to the lack of integration of microeconomics and macroeconomics and of general equilibrium and imperfect competition and transaction costs. This inspired the work of Negishi (1960, 1962) on imperfect competition and of Hahn and Negishi (1961) on the formation of equilibrium when trade was permitted before equilibrium was reached. According to Backhouse and Boianovsky the two threads were related by a paper published in 1961 by Negishi in which he tried to model non-tâtonnement as a process of bargaining between agents. But they also note that in Arrow and Hahn (1971), the discussion of Keynesian economics focused on the integration of money and made no reference to imperfect competition.

This “general equilibrium” (in the microeconomic meaning) approach of disequilibrium is also found in the work of Jacques Drèze. The latter still talks about “general equilibrium macroeconomics” today. This resulted from his study of general equilibrium models with incomplete markets and the proof that their equilibria were inefficient and also from the study of pricing rules for public utilities. He first realized that a multiplier could emerge in a microeconomic context. In 1971, he wrote a paper defining an equilibrium with price rigidities. Price rigidities were interpreted as a form of income insurance where the market offered no substitute. Jean-Michel Grandmont helped him realize the connection between his paper and the macroeconomic literature on fixed-price equilibria that he was pioneering himself. Backhouse and Boianovsky also discuss the work of Yves Younès (1970) who developed the first disequilibrium model at the CEPREMAP before working in association with Grandmont and Malinvaud. After that they turn to the works of Jean-Pascal Bénassy. The latter wrote a PhD thesis at Berkeley under the supervision of Debreu and Bent Hansen.
and tried to use the ideas of Clower and Leijonhufvud to build a model comprising Walrasian and Keynesian equilibria. Backhouse and Boianovsky insist on the last model he built in which he introduced imperfect competition and endogeneous price setting along with quantity constraints and disequilibrium. For them, his long run goal was a “theory of how prices were established out of equilibrium” (page 116). They pursue with Frank Hahn’s work on conjectural equilibria in the 1970s. Hahn explicitly endorsed the problem raised by Arrow or the idea of a link between disequilibrium analysis and imperfect competition. He attempted to go beyond the fixed-price equilibria but had to recognize that agent’s conjectures had to remain given. Grandmont and Laroque (1976, 1977) are also shown to have defended the use of imperfect competition in order to explain price setting without having recourse to the auctioneer: “We shall argue that monopolistic competition must be a central feature of the Keynesian model” (Grandmont and Laroque, 1976: 53). Finally they mention a paper by Hart but the latter recognize that his model was closer to Pigou that to Keynes.

Chapter 8 documents the fact that if the terms “microeconomic foundations of macroeconomics” came from Sidney Weintraub (1956) they would have been popularized by Phelps’ Volume on “The Microeconomic Foundations of Employment and Inflation Theory”. But the notion would have evolved during the Seventies. To show that the notion was not stabilized in the first half of the decade, Backhouse and Boianovsky quote a survey by Stanley Fisher (1975) in which the term microfoundations is used about monetary theories but not about disequilibrium theory, though the latter was discussed. They also tell the story of two conferences on the topic that took place in 1974 and 1975. The first one, organized by the Institute for Advanced Studies in Vienna on the topic “Equilibrium and Disequilibrium in Economic Theory”, showed participants agreeing that general equilibrium models were at the core of economics which implied methodological individualism. The second conference, inspired by John Hicks, offered a more catholic approach and displayed a broader vision of microfoundations. Erich Streissler, for instance, had challenged the need to assume optimizing agents in macroeconomics.

Two sections are devoted to Roy Weintraub’s intellectual path as an illustration of the way the general view of the relation between macro and micro was settled in the course of the 1970s. A detailed history of the writing of his 1977 survey which introduced for the first time the exact terms “microfoundations of macroeconomics” is told. The conclusion is that Weintraub reformulated the microfoundation of macroeconomics issue as “the problem of how to design a suitable general equilibrium model” (page 136).

The History of “The Theory of Unemployment Reconsidered”, Malinvaud’s survey is presented. Backhouse and Boinovsky insist on Malinvaud’s choice to discard the notion of disequilibrium in favour of the idea that general equilibrium did not imply market-clearing. He was offering new concepts of equilibrium with rigid wages and rigid prices. This was presented as a short cut where a dynamic analysis would have been too complex. Hence, he departed notably from the program of Clower and Leijonhufvud. Then Malinvaud showed how shocks could move the economy in such a way that it fluctuated between a regime of Keynesian unemployment and a regime of inflation. But a second form of unemployment, named classical, was also possible. It was the consequence of shocks on wage or the price of
oil as experienced in the 1970s. Malinvaud also put aside imperfect competition and departed from Benassy’s approach.

What is interesting in this survey of surveys and of reactions to surveys is the way it allows the reader to see the enfolding debates of the times. But one also wonders where the chapter is going to take him. What is it that the authors are trying to argue or show us?

A number of critical reviews of Malinvaud are summarized which tend to explain how the whole approach was finally discarded. The idea which seems to be in the mind of the authors, though not very clearly articulated at this stage, is that the sophisticated motivations of enlarging the Arrow-Debreu was lost to many commentators who saw in Malinvaud’s simplified model a limited framework. The targeted simplifications were obviously the fixed prices when contemporary works tried to explain these and showed that prices could under-react but also over-react. Another simplification deemed unacceptable was the static character of the model, the fact that agents lived in a one period world meaning that expectations were simply left out of the picture.

The presentation of a survey by Drazen (1980) published in *Econometrica* also suggests that a basic problem was the way Malinvaud’s model convinced too many observers that disequilibrium theories were about the consequences of price rigidities whereas much more was at stake. The crucial issue, for Backhouse and Boianovsky, was the attempt to understand how real markets functioned, how prices were set and modified and what happened when exchange were held before reaching market clearing. According to them, the visibility of Malinvaud’s « elegant » survey eclipsed the original aims of disequilibrium theorists (« to find a more general, logically consistent and more realistic theory of market processes than was offered by Walrasian theory ») and the models with endogenous prices and imperfect competition that had been devised along the way.

Chapter 9 intends to show that the disequilibrium line of research did not die out in the early 1980s:

“The common perception that disequilibrium macroeconomics died at the end of the 1970s, killed by its failure to explain macroeconomic event is incorrect” (page 180).

This claim is based on the assimilation of disequilibrium theories and imperfect competition. To explore the behavior of the market out of equilibrium one had to explain who set prices. The solution was to turn towards imperfect competition. This also allowed to endogenize price rigidities. This line of research was pursued in the 1980s and came to be incorporated in DSGE models in the 1990s. According to Backhouse and Boianovksy, for instance, imperfection competition, hence disequilibrium, was a basic component of the models in *Interest and Prices*, Woodford crowning achievement in 2003.

This basic claim is based on a broad review of research programs and works developed from the 1980s to the years 2000. First, Backhouse and Boianovsky note that Solow and Patinkin went on defending disequilibrium economics though they did not push further their preceding contributions. They also analyze the content of a book published by Hahn and Solow in 1995
conceived as a proposed alternative to the new classical economics. They note that in this book “Hahn and Solow were neither rejecting nor accepting the notion of disequilibrium […] they were simply arguing in a different way” (page 160). But they were trying to show the possibility of involuntary unemployment and that “might involve situations that could be described as disequilibrium, but there was no necessity for this” (same page). After that, Backhouse and Boianovsky turn their attention to Phelps development of his theory of structural slumps. Though Phelps’ theory was about equilibrium unemployment and its evolution, they show some links between his approach and the disequilibrium school. Phelps mentioned Malinvaud (1980) as a precursor of his works. He also organized a conference about “Expectation Formation and Economic Disequilibrium” in 1981 but the authors recognize that the term “disequilibrium” was “rarely used” in the book that came out of this event. After that they study the birth of new Keynesian economics. They mention again the work by Neary and Stiglitz (1983) mixing rational expectation and fixed-price equilibria but they conclude with a reference to the New Keynesian manifesto of Greenwald and Stiglitz (1987) which noted that the fixed-price assumption “fails to square with evidence” and “cries out for theoretical justification” (page 167). Then they stress the fact that Mankiw and Romer introduction to their 1991 volume failed to make any mention of disequilibrium theories, a point highlighted by a review of Huw Dixon (1992). A very brief mention is then made to the work of Franklin Fisher on Disequilibrium Foundations of Equilibrium Economics published in 1983 who tried to push further the theory of non-tâtonnement process. This is followed, in the same section by a presentation of the works of Diamond. According to the authors:

“Diamond analyzed the problem of disequilibrium through modeling the technology through which trade took place, showing that, even if people were rational, market disequilibrium might emerge.” (page 171)

Then they refer to the work of Howitt and the first agent based models as the offspring of Clower and Leijonhufvud research program. Finally, in their last sections, they tell the story of the introduction of imperfection competition in the DSGE models coming from the works of Lucas and Kydland and Prescott. Interestingly, they note that two important contributions in this field, that one would normally identify as New Keynesian, one of Rotemberg (1982) and the other by Svensson (1986), referred to the disequilibrium literature. Basically, incorporating monopolistic competition could account for price rigidities that were exogenous in Barro and Grossman. They also insist on the views of Bénassy (2002) who saw a continuity between DSGE model with monopolistic competition and calvo pricing and the non-market clearing theories of the 1970s. A presentation of Woodford (2003) as the illustration of Goodfriend and King (1997) “New Keynesian Synthesis” closes the chapter.

The conclusion of the book is important for it clarifies the claims of the authors.

1. The origins of “the search for non-Walrasian or disequilibrium microfoundations” lie in the works of Hicks, Lange, Modigliani and Patinkin starting in 1930s and the 1940s.

2. Clower is the crucial figure in the search for a better theoretical basis for Keynesian economics because his dual decision hypothesis showed more clearly than Patinkin had done how trading out of equilibrium cause supply and demand functions to be different.
3. There were two sources to the disequilibrium theories of the 1970s. In addition to the Patinkin-Clower line, one should not forget a line of research resulting from problems raised by the Arrow-Debreu model and put forward by Arrow, Hahn and Negishi.

4. “In contrast to the claims sometimes made, the advocates of disequilibrium theory did produce theories of why prices were inflexible”, using in particular theories of imperfect competition and more informal “arguments about information”.

5. In spite of this, disequilibrium theories were assimilated to fixed-price models. This was due to the excessive complexity of the more advanced models, the absence of models in the case of Clower and Leijonhufvud and the success of the simple fixed-price models of Barro and Grossman and Malinvaud.

6. Another reason why the ambitious version of disequilibrium theories was disregarded was the attitude of macroeconomists towards imperfect competition. Either they considered that Keynesian economics had to assume perfect competition or they considered that imperfect competition was the result of exogenous institutional barriers. But Arrow had something different in mind: imperfect competition was necessary to explain the functioning of market out of equilibrium.

7. Whereas disequilibrium theories proposed a more general approach to the problem of how market worked, they came to be seen as a detour and a special case on the road to New Keynesian economics.

8. “Perhaps the most important lesson from this account of the search for non-Walrasian foundations for macroeconomics is that it was never homogeneous”. And this diversity kept on increasing after the 1970s.

9. “The search for non-Walrasian microfoundations left a deep mark on macroeconomics” and “the story we tell in this book is central to the transformation of macroeconomics that took place during the 1970s”.

10. In certain respects, the evolution of macroeconomics after the 1970s and the triumph of New Classicals and New Keynesian economics marked a regression compared to disequilibrium theories (Lucas and his disciples assumed away the question of price setting and New Keynesian reduced imperfect competition to institutional barriers).

2. General comments

Since my main area of expertise is the relationship between the works of Lange, Hicks, Patinkin and Clower, I will take the time to examine how these relations are presented by Backhouse and Boianovsky. My views in this field will lead me to question some of the broader claims put forward in their book.

2.1 From Patinkin to Clower

The insistence on the influence of Hicks (1939) and Lange (1938, 1942 and 1945) at the origin of disequilibrium economics is a strong point of the book. Yet, the meticulous
presentation of their contributions tends to blur the lines between them and Patinkin. Furthermore, some critical aspects of Patinkin’s PhD thesis are ignored. As a result, Backhouse and Boianovsky fail to bring out the real significance of his contribution and tend to downplay his importance.

**Hicks and Lange**

The crucial idea on the way to disequilibrium theories was that Walrasian general equilibrium theory was necessary in order to give solid foundations to Keynes’s theorys. Lange and Hicks were the one who introduced and spread this notion.

This approach was not part of Hicks’ famous IS-LM article. It was first suggested by Lange (1938). Backhouse and Boianovsky rightly note that he “compared Keynes with Walras” (page 33) and that he “used his diagrammatic apparatus to sort out historical issues pertaining to the relation between Keynes’s, Walras’s and Malthus’s respective ideas” (page 37). But much more was at stake. A careful reading of his paper shows that he used the reference to Pareto and Walras in order to make sense of Keynes’ aggregate relations. For him, Keynes’s system was a simplified Walrasian model. This was important for Lange because it meant that Keynes had proven that even a perfectly competitive system could be subject to underaccumulation and underemployment. In other words, Lange suggested that to appreciate fully the scope of Keynes’ result, one had to see his model as an aggregate version of Walras’ theory. This assimilation was possible because Lange interpreted involuntary unemployment as a labor market clearing state with a horizontal labor supply curve. Hicks’s contribution to this line of thought was quite distinct and appeared in *Value and Capital* (1939). In the introduction of his book, Hicks explained that though Keynes had “got in first”, the drawback of his “superb intuition” was that in reading the *General Theory* it was difficult to “separate out those things which are the fruit of pure logic [...] from those things which are the fruit of Mr Keynes’s own point of view on social questions” (1939: 4). In other words, he proposed to use the “pure logical analysis of capitalism” (1939: 7) primarily inspired by Pareto and Walras to clear up things left unclear by Keynes. The result appeared in the last chapters of the book and is well explained by Backhouse and Boianovsky. Keynes’ chapter 19 on the effects of wage variations was translated in the language of stability analysis. Under certain circumstances, the temporary equilibrium of a market system became totally unstable. In contrast with Lange (1938), Hicks did not try to clarify the relation between Keynes’ aggregate model and a Walrasian model. He proposed instead to assess Keynes’s main claims directly within a general equilibrium framework. This approach involved disequilibrium analysis in so far as Keynes’ was interpreted as showing that under certain conditions, the economy could not get back to the temporary general equilibrium. Stability could be restored by the introduction of an exogenous wage floor but this at the cost of persistent excess supply on the labor market. In the first case, Hicks assumed, like Marshall, that trade taking place during the process of adjustment would have no consequence on its results, they could be ignored. In the second case, Hicks did not write down his model.

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2 On IS-LM and Walrasian general equilibrium theory see Rubin (2013).
3 On the way Hicks dissociated himself from Keynes views see Rubin (2011).
and ignored the possible modifications of its microfoundations required by the introduction of unemployment.

Hicks’s approach caught the attention of Lange who pushed it to its extreme in his 1945 monograph on *Price Flexibility and Employment*. There he developed what one could call a disequilibrium analysis of Walrasian type. What he proposed was to analyze the various parameters conditioning the stability of general equilibrium system with $n$ commodities, money and bonds. For a certain constellation of parameters, the system would prove unstable. In more Keynesian language, once an excess supply had appeared on a factor market, the economy could never return to full employment. What economists had to do was to examine the actual situation of the economy and see if they could modify its parameters in order to stabilize the system. This approach was about disequilibrium since it focused on states of the economy in which supplies and demand differed. But it was Walrasian because the possibility and the implications of rationing were totally ignored by Lange. In Lange’s perspective, a depression was a tâtonnement process lasting a decade. Though he did not discuss the issue, this implied that agents did not trade, produce or consume for years…

Before discussing Patinkin’s contribution, let us note a little awkwardness around the notion of “inconsistency”. The authors suggest that the notion that Keynes’ theory of unemployment could be interpreted in terms of inconsistency, defended in Patinkin’s PhD thesis, came from Lange’s lectures (page 36). But they offer no evidence of this. Patinkin called “inconsistency” a lack of equilibrium solution in the Walrasian model. But Lange (1944) always assumed the existence of a full employment solution. He only questioned its stability. So, the notion of “inconsistency” in the meaning of Patinkin was clearly absent from Lange’s writings. This term was probably an invention of Patinkin to name an insight coming from Klein and Samuelson (Rubin, 2012).

**Patinkin**

Backhouse and Boianovsky note very briefly that Patinkin did not “accept Lange’s claim that there was no essential difference between equilibrium and disequilibrium interpretation of involuntary unemployment” (page 38). This point should have been discussed more thoroughly for it marks the crucial move opening the whole field of disequilibrium economics as it came to be understood in the 1960s and the 1970s. Disequilibrium economics was about economic systems featuring rationing or agents that could not realize their notional or Walrasian plans. But how could one integrate such individual disequilibria using the language of microeconomic theory? This was the key issue. As we have seen above, this issue was ignored by Hicks (1939) and by Lange (1938, 1945) either. Patinkin was the first to raise it in his PhD thesis in 1947, a document to which Backhouse and Boianovsky pay insufficient attention. His starting point was Lange’s definition of involuntary unemployment as the result of a horizontal supply curve of labor. According to Patinkin, this definition could not account for the involuntariness of unemployment. Since the labor market was cleared, workers realized their plans. Unemployment was an excess supply of labor. But it was more than that. It was a situation in which workers were “off their supply curves of labor”. They could not obtain their desired level of employment and income but they did obtain certain levels of employment and income. And these levels acted like “additional restraints” in their
optimizing plans. In other words, to account for involuntary unemployment one had to modify to standard theory of choice:

“Though the existence of widespread involuntary unemployment is historically irrefutable, economic theory has yet to deal adequately with it.” (Patinkin, 1947: 79)

Patinkin actually realized as soon as 1947 that a theory of involuntary unemployment required a general theory of involuntary action that would distinguish “normal” plans and “involuntary” plans, plans resulting from “additional restraints”:

“In general, for any given set of prices and income, the amount purchased, as given by $g_i$, will differ from that given by $h_i$. So even though the consumer may be on his $g_i$ function, he is not fulfilling his desires; for the desires of the individual are defined as being presented by his $h_i$ functions.” (1947: 118)

As shown in Rubin (2012), Patinkin’s PhD thesis contained the basic ideas defining the research program of disequilibrium theoreticians in the 1970s. From his teacher, Lange, Patinkin inherited the idea that the Keynesian aggregate models could be related to Walrasian models. But unlike Lange, he realized that to derive a Keynesian framework from a Walrasian model, the latter had to be deeply modified and this at two levels: the level of individual choices and the level of adjustment rules. As I explained elsewhere, the result of Patinkin’s highly ambitious efforts were disappointing, to say the least, and his project was not understood by the members of the Cowles Commission. For this reason, he was infinitely more prudent and modest in chapter 13 of *Money, Interest and Prices*, where he presented his disequilibrium interpretation of the Keynesian theory. But the core ideas were still there. Backhouse and Boianovsky ignore the notion of “additional restraint” though it is the basis of all latter developments: “An individual subjected to any additional restraints will be said to be acting involuntarily” (1989: 314). In 1947, Patinkin failed to give an operational content to this very general notion. This is precisely what he managed to do in 1956 in the case of firms. In a situation of excess supply on the goods market, firms would have to take account of the outlet constraint imposed by aggregate demand for goods. As a result, they would have to modify their labor demand even though the real wage remained the same. The result was a distinction between “desired” and “actual” demand and supply curves. Furthermore, Patinkin’s analysis of the interaction between the labor market and the goods market based on the “spill over effect” laid down the basis of general disequilibrium analysis. Two blind spots remained though. Strangely enough, firstly, and in spite of his definition of involuntary unemployment, Patinkin did not notice that his analysis of firms’ behavior in disequilibrium also applied to households and questioned the standard theory of the consumer. Secondly, Patinkin abandoned his efforts to model mathematically the behavior of agents in disequilibrium and adopted an excessively prudent attitude. But in spite of this, Patinkin made the decisive contribution. He defined the basic issues and concepts that would be recombined and developed in latter works.

This leads me to the concluding remarks of Backhouse and Boianovsky about Patinkin. They suggest that he did not realize the significance of his own ideas. The words that he used to announce his chapters on unemployment are offered as proof of this:
“Terms such as ‘disequilibrium’ or ‘non-market clearing’, which might show that he was aware that his theory of involuntary unemployment might be but part of a more general theory of how markets worked, are conspicuous by their absence. Failure to realize the full significance of what he was doing would be consistent with the fact that during the 1960s and the 1970s, he chose to work on other topics […]” (Backhouse and Boianovsky, page 41)

Here, Backhouse and Boianovsky rejoin the usual perception of Patinkin’s contribution in chapter 13. Patinkin’s thinking would have remained quite fuzzy on disequilibrium economics explaining why the decisive impetus was given by Clower and his more lucid contribution. The problem is typical of the difficulties encountered by the historian of thought trying to assess the knowledge of a thinker of the past. How to avoid the retrospective bias which leads the historian to endow the author with a mastery of his thinking that was only acquired by later generations? The only thing we can do his read attentively all the writings at our disposal and take account of contextual evidence. According to me, in the case of Patinkin, the main consequence of this exercise is precisely to dispel the usual views of chapter 13. The words “disequilibrium” and “non-market clearing” may not be present in the introduction of *Money, Interest and Prices*. But they were used time and again in key texts. To my knowledge, Patinkin was actually the first Keynesian economist to write that Keynesian theory was about disequilibrium. This was actually the conclusion of an article published in 1948 that attracted a lot of attention at the time:

“In other words, what Keynesian economics claims is that the economic system may be in a position of underemployment disequilibrium (in the sense that wages, prices, and the amount of unemployment are continuously changing over time) for long, or even indefinite, periods of time.” (1948: 563)

And this conclusion was the main message of chapter 13 repeated on pages 315, 323, 327, 328 and on page 337-8 of chapter 14 each time in the form of striking slogans. For instance:

“The argument of this chapter can now be summarized in the following terms: Equilibrium means full employment, or, equivalently, unemployment means disequilibrium” (328).

or

“Thus Keynesian economics is the economics of unemployment disequilibrium” (337-8).

Did Patinkin realize that what was at stake was a “more general theory of how market worked” (page 41)? He surely believed that the issue was of a general nature. The words he used in his introduction to define the subject, quoted by Backhouse and Boianovsky themselves, were “the monetary theory of an economy with involuntary unemployment”. But this theory included full employment as a special case. This theory was the general theory towards which economists had to work their way, the “climax” of his book just like chapter 19 was the “climax” of the General Theory according to Patinkin. Did Patinkin realize that

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this theory was something different from the Walrasian theory? Here is the main difficulty. I think that my work on Patinkin’s thesis shows clearly that he was plainly aware that his analysis implied behaviors incompatible with standard microeconomics. In 1947 he had attempted to devise an alternative micro. I think that his failure led him to a very conservative stance in later years. One had to find a way to reconcile his analysis with standard micro. But the task was daunting and Patinkin proved aware of it. This is why he wrote footnote 9 in chapter 13 admitting the inconsistency between the behavior of firms in his analysis and the standard theory. Does this show that his understanding of the issues was insufficient? Did he abandon the subject after 1956 because he thought it was secondary? Certainly not. I rather believe that he faced conceptual difficulties and theoretical choices so hard that he could not find a solution. But these difficulties were not resolved by Clower or his French disciples and they would actually resurface in the 1970s. 

Clower

Backhouse and Boianovsky rightly emphasize the importance of Clower’s 1965 article on the “Keynesian Counter-Revolution. Clower had a stroke of genius. He realized what a whole generation of Keynesian economists had failed to realize! He saw that the Keynesian consumption function could not be derived from the standard consumer theory. In a Walrasian general equilibrium context, one could not assume that income was given like in introductory textbooks. A different theory was needed to explain how income could appear as an independent variable in worker’s consumption function. His answer, the existence of trade out of equilibrium on the labor market was only the obverse side of Patinkin’s spill over effect. But Clower managed to formulate the issue in general terms and, above all, he did what Patinkin had refused to do. He offered a mathematical transcription of his insight. Finally, he resorted to warlike rhetoric and proclaimed his heterodoxy in a way that could not fail to attract the attention of the younger generation at the end of the 1960s. Backhouse and Boinovsky suggest that Clower arrived at this contribution independently of Patinkin and that it was the natural outcome of a research program developed in the 1950s. Both claims can be questioned on the face of evidence. In other words, the authors have not examined the available material sufficiently in depth. First of all, Clower’s research programs in the 1950s did not lead naturally to the dual decision hypothesis because they lacked the crucial idea at the center of Patinkin’s thinking on Keynesian economics: the importance of involuntary unemployment considered as a situation of market disequilibrium qua individual disequilibrium. A disciple of Hicks and Lange, like Patinkin, Clower expressed the need to clarify the microfoundations of the Keynesian theory as early as 1952 in his PhD thesis. For instance:

\[\text{\footnote{Patinkin feared that a formalization of firm’s behavior like the one that would be put forward by Barro and Grossman was too arbitrary. See the discussion on this point with Nissan Liviathan reported in Boianovsky (2006: 229-240). This arbitrariness was certainly a weakness of fixed-price equilibrium theories of the 1970s: arbitrariness concerning the behavior of prices, arbitrariness concerning the rationing schemes and arbitrariness concerning the behaviors of agents.}}\]

\[\text{\footnote{What follows owes a lot to discussions with Romain Plassard. For a systematic argumentation of the following points see Plassard (2014).}}\]
“From a formal point of view, is The General Theory a special case of general equilibrium theory? Once again, there are essential differences between the two levels of analysis, differences which may not be reconcilable until the foundations of general equilibrium theory are broadened.” (Clower, 1952, page 5).

But at the same time, Clower dismissed the problem raised by Patinkin concerning the definition of involuntary unemployment as irrelevant (Clower, 1952: 66). In other words, he adopted the attitude of benign neglect of Lange (1945). A horizontal supply of labor curve and a labor market that cleared was enough to speak of fluctuation in employment, the only important subject. Like Patinkin, Clower wanted to reconstruct the theory of Keynes starting from the Walrasian general equilibrium theory. Unlike him though, he approached the matter from the perspective of the debate on loanable funds versus liquidity preference theory. What Keynes had introduced was a new theory of the rate of interest based on the role of stocks of bonds and money, a theory showing how speculation could generation instability. In order to incorporate this insight into the “traditional theory of prices”, Clower set out to “broaden” it. This led to the elaboration of stock-flow models. These models featured market clearing all the way. But they were supposed to show how the economy would fluctuate around a stationary equilibrium due to speculative behaviors. For various reasons, the research program petered out. But Clower developed a second research program.

According to Backhouse and Boianovsky “Clower was searching for an alternative to the tâtonnement process” (page 49). It is correct to say that Clower considered situations of disequilibrium in which agents set prices and traded out of equilibrium in a series of papers between 1954 and 1959 and even in a book published in 1957. But the focus was not on disequilibrium there. Clower tried to devise a general theory of the market allowing a unification of all forms of competitions from monopoly to perfect competition. To do so he reasoned in partial equilibrium. This means that he completely ignored the spill over effects and, more generally, the consequences of disequilibrium trading. Income effects, the heart of the matter, were neglected in a typically Hickso-Marshallian mode. Actually, the dynamic analysis did not go very far much of the time and the most important parts of the discussion often turned around the properties of equilibria with full market-clearing.

So what happened to Clower? Well he experienced a decisive turnabout between 1960 and 1962. In 1960, Clower came back on the question of the relation between Keynes and the Classics. Like he had done during all the preceding decade, he maintained that both theories were perfectly compatible. But at the same time he built a Keynesian model in which consumers decided their level of consumption on the basis of a level of employment imposed by firms. In 1962, he had realized that this was not compatible with the standard theory of the consumer and claimed that Keynes and the Classics were totally incompatible. At this stage he focused on unemployment defined as an excess supply and portrayed Keynesian economics as disequilibrium economics. How can one explain such a change? It happens that, starting in 1958, Clower began interacting intensively with Patinkin and reading Money, Interest and Prices. In 1960, Clower invited Patinkin to Northwestern University where he stayed some months as a Visiting Professor. Actually, as I explained elsewhere (Rubin, 2005), the discussion between the two economists started about a paper in which Patinkin examined

7 For evidence taken from the correspondence between the two economists see Rubin (2005).
the validity of Walras’ law in a Keynesian context. This led him to write down a budget
costant in which, just like in Clower (1960a, 1965), the level of employment was imposed
by firms. What I showed was that nearly all the ingredients of Clower contributions were
already in Patinkin writings. For instance, in 1965, Clower explained that Keynesian
economics required the rejection of Walras’ law. A careful reading of chapter 13 of Money,
Interest and Prices shows that Walras’ law is violated in Patinkin’s dynamical analysis
exactly like in Clower (1965), though Patinkin (1958, 1965) offered arguments to restore its
validity. Backhouse and Boinovsky present a paper published by Clower in 1960 as the result
of the work he realized in 1950s about disequilibrium dynamics. Actually, this paper was a
simplified version of a conference on the relation between Keynes and the Classics he gave at
Northwestern University after the departure of Patinkin. The last sentences of the paper are
nearly a paraphrase of Patinkin:

“Nothing could be simpler in principle than a shift from stational to dynamical habit of thought;
neither, could anything be more difficult in practice. The fruits of the Keynesian Revolution
have been, and are being, gathered primarily by a new generation of economists, a generation
that has finally accustomed itself to thinking in terms of points and planes instead of curves
and crosses.” (Clower, 1960)

“Thus our first task in studying involuntary unemployment is to free ourselves of the mental
habit –long ingrained by the methods of static analysis –of seeing only the points on the
demand or supply curve.” (Patinkin, 1956)

In contrast with Backhouse and Boianovsky, I believe that Clower owes a lot to Patinkin’s
writings. Clower’s importance came from the fact that he saw what Patinkin had not seen
concerning the Keynesian theory of the consumer. On top of this, Clower managed to break
with the conservative attitude of Patinkin. This may be due to his successive failures at
“broadening” the traditional theory of prices during the 1950s. The idea that Keynesian
economics was about the dynamics of a flexible price system was not original. As we have
seen above this was a recurring slogan of Patinkin. The originality in Clower was his interest
for instability. And this probably explains why he insisted on rejecting Walras law.8

2.2 Disequilibrium economics: a current of though inscribed in the DNA of the
neoclassical synthesis

According to Backhouse and Boianovsky, the history of disequilibrium theories is the history
of two different lines of research, one springing from Keynesian macroeconomists and the
other springing from internal questions within the Walrasian research program. And beyond
these two branches, a wide variety of approaches were developed. Failure of micro and macro
and of the different approaches to communicate and excessive success of a narrow and
excessively simplified class of model would explain in large part the decline of disequilibrium
macroeconomics.

8 But more on this point below.
A different story is possible. Beyond the indisputable heterogeneity of the views supported by the promoters of disequilibrium economics, a strong continuity can be shown to exist in the developments that began at the end of the 1930s and flourished in the 1970s. According to me, disequilibrium theories were the embodiment of a research program contained in the DNA of the Neoclassical Synthesis as early as the 1940s. The mainstream of economics established in the 1950s and 1960s resulted from a mix up of ingredients realized in the 1940s mainly around Harvard and the Cowles Commission in Chicago. This combination could lead up and did lead up to various approaches. A potential research program was illustrated by the work of Patinkin around 1947 at the Cowles Commission (Rubin, 2012). As seen above, the resulting PhD dissertation contained all the insights and the questions defining the research program taken up at the CEPREMAP and Louvain-la-Neuve in the 1970s. Patinkin’s limited grasp of general equilibrium theory impeded him to go very far on this way and the depth of his insights were lost. The important lesson is that the mains aspects of the problem were already discussed at the Cowles Commission in the 1940s. But this program needed two more decades of maturation before it could flourish. Robert Clower really played an important role in this process. But this role consisted in clarifying and marketing questions and concepts already present in the works of Patinkin. And his contribution was not independent. Clower read carefully Patinkin and interacted intensively with him just before he wrote his 1965 paper. There was strong links of continuity from Patinkin to Clower.

Moreover, my guess is that the research developed by Arrow, Hahn and Negishi were not completely independent from the contributions of Patinkin and the problems raised by the foundations of Keynesians macroeconomics. In a recent paper, Wade Hands (2012) argues that the Walrasian research program was deeply influenced by Keynesian economics:

“What Keynesian and Walrasian economics evolved into –what they became- when they stabilized into textbook macro and (advanced) textbook micro during the 1950s and 1960s, was, at least in part, a result of the fact that they were joined together in, and co-evolved within the context of, the neoclassical synthesis” (Wade Hands, 2012, p. 120).

I have little evidence to offer beyond those presented by Wade Hands but they are compelling. First, Arrow was a member of the Cowles Commission when Patinkin wrote his PhD and he was among those who discussed his work. Second, Hahn wrote his thesis under the supervision of Kaldor and its subject was income distribution in the context of a Keynesian model. Third, Negishi discussed with Clower around 1960 when the latter turned his attention to the relation between Keynes and the Classics (Negishi, 1960). Negishi and Hahn showed in 1962 that if Walras’ law held, a non-tâtonnement process should be stable. The same year, Negishi (1962), in a survey on stability in the Walrasian context, insisted on Walras’ law as a necessary condition. Clower liked to think that in Keynesian models, adjustment processes did not lead to full employment. They were unstable. And it does not seem absurd to conjecture that his vehement rejection of Walras law in 1962 was an answer to the work of Hahn and Negishi. Note also that Hahn and Negishi (1962, p. 465) referred to “spillover effects” a concept coming from Patinkin (1956).
So, from the 1940 to the 1970s, the idea of a combination of the Keynesian theory and the Walrasian theory was in the air. This idea informed the efforts of a number of economists, some of whom could be considered as microeconomists, while others could be considered as macroeconomists. These two communities did not ignore each other contrary to what Backhouse and Boianovsky or even De Vroey (2009, 69) suggest. Microeconomists certainly read Patinkin and Clower. Actually, the main protagonists of this story shared an intellectual background constituted by the works of Hicks (1939), Lange (1942, 1944) and Samuelson (1947). From this respect, the models developed in the 1970s realized a potentiality of the Neoclassical Synthesis.

From this respect also, the heterogeneity of disequilibrium theories should not be overemphasized. Around Paris and Brussels, the consensus on method and models was certainly strong enough. This means that the failure of this research program to survive the 1980s is probably due to reasons other than heterogeneity. This leads me to my last point. A very important subject for future research would be the relation between fixed-price aggregative models and complex general equilibrium models within the disequilibrium research program. My guess is that Hahn or Bénassy failed to develop a convincing and simple enough framework to compete with the macromodels of the New Classicals. And this was due to theoretical difficulties inherent to their research program (Rubin, 2005), difficulties that blocked the way towards a dynamic theory of market processes out of equilibrium. But Backhouse and Boianovsky barely touch upon this subject.

2.3 Concluding remarks

Backhouse and Boianovsky’s history of disequilibrium theories is now the most comprehensive study of this episode in the history of macroeconomics. Michel De Vroey (2004, 2009) has dedicated very useful book chapters to this subject. But his focus was narrower. Seeing his role of historian as the role of an art critic, De Vroey only discussed what he considered to be the most representative contributions and offered uncompromising assessment for each of them. What is lacking in his work is, first of all, a study of the long term dynamics of this research field. Second, his story omitted important pieces of work like the work of Franklin Fisher, Hahn or Negishi. Backhouse and Boianovsky complete the story in all these areas. They offer a panoramic view studying the genesis of disequilibrium theories from Hicks and Lange to the 1970s. Furthermore, the emphasis put on the search for a better theory of markets springing from Arrow and Negishi is a strong idea of the book and one that was certainly too neglected by De Vroey. More may have been at stake than offering foundations to Keynesian ideas in the work of Hahn or Clower. Clearly, for economists like them, building an acceptable version of the Keynesian theory required, as a prerequisite, a deep understanding of the functioning of a market economy. This meant going beyond the Arrow-Debreu model to integrate incomplete markets, money, imperfect competition and trading out of equilibrium. This dimension of the search for disequilibrium microfoundation was clearly eclipsed by the success of Malinvaud (1977) and fixed-price models.

9 The difference came from the fact that microeconomists had assimilated Arrow and Debreu (1954) and shared their axiomatic approach while Clower and Patinkin were treading in Hicks’ and Samuelson’s footpaths.
But is it sufficient to state that interesting lines of research were developed and then unjustly disregarded because of “their complexity”? One would like to know why this research program was abandoned. And “complexity” is a bit short as an explanation. Economists like Hahn or Fisher may have encountered insuperable obstacles or problems to which they had no answer. Or was it incapacity to pass from complex models to simple aggregate frameworks necessary to discuss policy issues and analyze the data. But Backhouse and Boianovsky never go far enough in their discussion of the various models to answer this kind of issue. In this respect the rational reconstructions proposed by De Vroey perform better. It seems like Backhouse and Boianovsky consciously avoid assessing the theories they examine. I suspect that a methodological principle is at work here. Assessing past theories would always run the risk of falling prey to retrospective bias and offering logical classification would distort the complex facts facing the historian. But this methodological creed weakens the conclusions put forward by the two authors. Rational-historical reconstruction should be possible. One can attempt to capture as carefully as possible the intentions and the views of economists in the distant past and at the same time assess their theories at the analytical level in order to make sense of history.

The idea that disequilibrium theories “left a deep mark on macroeconomics” is also a seducing claim. A common history of macroeconomics in the 1980s, a history that may come from Solow, tells that New Keynesian economics resulted from an attempt to justify the fixed-price assumption of Malinvaud style models. Backhouse and Boianovsky show that many New Keynesian economists went on referring to disequilibrium. And it is quite obvious that theories and schools of thought overlap and interact in complex manner. But to me, the general claim is insufficiently clarified and demonstrated by the book. What exactly are the marks left by disequilibrium macro on contemporary macroeconomics? Could it be the use of monopolistic competition à la Dixon and Stiglitz? But then, Backhouse and Boianovsky do not explain how their contribution was related to disequilibrium theories. Can we consider Phelps as developing a kind of disequilibrium theory in the 1980s? Earlier in the book, he was presented as pioneering a line of research well distinct from the line of Benassy and Drèze. But in spite of these questions, Backhouse and Boianovsky make an important point here and open an exciting avenue for future research. On this score, their work needs to be pushed further.

That the abandonment of disequilibrium implied a regression in some respect is also a point that I found interesting. For sure, the market is a black box in the models of Lucas or Prescott. Disequilibrium theoreticians were trying to open the black box and from there to built a new macroeconomics. But on this point again, deeper analysis will be required to show that disequilibrium theories are still a resource for contemporary macroeconomics.

Conclusion

Backhouse and Boianovsky’s book present the first comprehensive history of the search for disequilibrium microfoundation. It is a pathbreaking contribution opening many avenues of research. The price paid for the scope of the book is a lack of depth in the treatment of various questions, a problem that weakens the interesting conclusions of their study.
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